Johann Lauer

Philosophy of Political Science.

Limits and possibilities of political science research: the axiological, epistemic, methodological and ontological foundations of political science

In memoriam

Klaus Gustav Heinrich von Beyme

(1934-2021)

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Preface

The most important results of this monograph: innovations in content and methodology

In my view, a *pluralist habitus* coupled with a *reductionist agenda* dominates discussions in the *philosophy of science*. In this monograph, I attempt to implement a *pluralist approach* on several levels.

Firstly, I establish a philosophical pluralism that recognizes *three complementary philosophical traditions*. I call them the *descriptive-interpretive*, the *explanatory-prognostic* and the *practical* tradition. There is a minority of scientist that advocates the complementarity of two traditions, namely the *naturalistic* (positivistic, Galilean) and the *constructivist* (interpretative, Aristotelian). Political scientists who see themselves as social scientists usually pursue a reductionist agenda and argue for *a naturalistic revolution*, especially for a *causal reductionism*. Humanist-oriented political scientists, on the other hand, call for an *interpretive-constructivist turn* (counter-revolution).

Secondly, *linguistic reductionism*, i.e. an Anglophile monoculture, is another feature of current academic discourse. I try to overcome this reductionism through a *multilingual approach*. English as the *lingua franca* of science is not questioned, as my monograph is published in both English and German. However, a multilingual approach increases the *reliability* of the results and ensures that *new perspectives* bring fresh air into the discussion. The practical tradition is justified here with many arguments that come from German philosophers (Hans Albert, Walter Ernst Otto Dubislav, Rainer Enskat, Otfried Höffe, Klaus Kornwachs, Hans Poser, Wolfgang Wieland) but have not yet played a role in the discussion of philosophy of science. Based on the practical tradition, a plea is also made here for a *practical political science* that sees itself as *complementary* to *empirical political science*.

I am also trying to overcome a third reductionism: Edutainment leads to an *infantilization* of scientific discourse. Therefore, no effort, no pleasure, *per aspera ad astra*: no edutainment, no fictional narratives, only logical-analytical (rational) analyses and hermeneutic-multilingual interpretations ad fontes. I also reject *revolutions* or *turns* (counter-revolutions). *Evolution* through the evolutionary further development of existing traditions, through the development or further development of new *research programs* or individual new methods within the respective traditions is the preferred method of choice.

Content innovations include a more nuanced view of the *non-epistemic norms* and values that shape science. Even more important is the re-evaluation of *epistemic norms* and values in science. In my view, a highly complex epistemology and methodology, based on a variety of general, abstract or universal, as well as concrete, specific or local conditions or criteria, enables the legitimation of scientific knowledge and the drawing of boundaries between *science* and *non-science*. These *epistemic or constitutive norms* and values guarantee *scientificity*.

As a final innovation in terms of content, I would like to mention the first *curriculum* for the *philosophy of political science*, which contains exercises for bachelor students on the one hand and for master students on the other. The accompanying *charts*, like maps, make it possible to present the *logical geography of philosophy of science* using political science as an example, as well as to *reduce the complexity* of complex and specific debates and topics.

Abstract philosophy of political science: logical geography and theses on the further evolution of the subdiscipline

Political philosophy has been one of the best-established subdisciplines in political science for over two millennia. The same cannot be said of its subdiscipline, *philosophy of political science*. The axiological, epistemic, methodological, and ontological foundations of political science, the subject matter of the subdiscipline, were already studied in antiquity, especially in Aristotle's Organon. Today, the philosophy of political science is probably the most *neglected* subdiscipline of political science, although there has been a *philosophical awakening* within the discipline over the past three decades.

The first main goal of the book is to present a *logical geography* of the philosophy of science using the example of political science. The book therefore offers an *overview* of the philosophical (axiological, epistemic, methodological and ontological) foundations of political science research at *ten levels*. Both the *state of research*, in particular the most controversial issues of the *science war* (*Methodenstreit*), and the *genesis* of the debates are made visible. The *limits* and *possibilities* of political science research are also identified.

In line with the *current state of research*, the second main goal is to present important proposals for the *evolutionary further development* of the subdiscipline "philosophy of political science". For the further evolutionary development of the subdiscipline, I would like to present my *own theses* on the following topics:

First, the *state of research* in the philosophy of (political) science, the need for a *further development* of the debate, and the *methodological approaches* are presented, with the help of which a reception of, reflection on and further development of philosophical traditions with a systematic intent can succeed. Second, six *axiological questions*, in this case *non-epistemic norms* and *values*, are identified that are posed to science from outside, by citizens, politics, scientists and society. Third, there is a wealth of *general* as well as *concrete conditions* with the help of which scientific knowledge can be legitimated and a *demarcation line* can be drawn between *science* and *non-science*. These *epistemic* or *constitutive norms* and values guarantee *scientificity*. Fourth, I will show that the science war (*Methodenstreit*) can be overcome by demonstrating the *complementarity* of three philosophical traditions (descriptive-interpretative, explanatory-prognostic, and practical). Fifth, I argue for an advancement of *political philosophy/political theory* into a *practical political science* in which practical (normative, pragmatic and technical) knowledge can be rationally justified with a practical methodology. And finally, the sixth point is a plea for the *institutionalization of the subdiscipline philosophy of political science*, with a corresponding institutional anchoring in the science system, e.g. in curricula, chairs and journals.

The third major objective is to establish a *curriculum* for the subdiscipline "philosophy of political science" for the first time. The *syllabus* includes a *basic* (undergraduate) and an *advanced* (graduate) *seminar* that are developed for *beginners* and *advanced prospects*. This should enable two things: firstly, these courses serve as a guiding thread, a *user's manual* for this book. Secondly, they contain *didactic course templates* for two different target groups: *beginners* and *advanced prospects*.

The *charts* in the book enable a quick *overview* of the most important topics and a *reduction in complexity* in the case of complex and special discussions and subject areas.

Target groups

The book is aimed at *practitioners* and *scientists* who want to discover the *limits* and *possibilities* of science and how to draw a *demarcation line* between *science* and *non-science*. The slogan "Follow the science" revealed the *total scientification* of all areas of life, including politics. Precisely for this reason, a critical and prominent examination of the *philosophical foundations* of science and how to guarantee *scientificity* should be the main topic of scientific, political and social debates.

The book is aimed at anyone who is interested in an overview of the most important issues and the current state of research in the subdiscipline *philosophy of political science*. I think that this book is very suitable for *beginners*, i.e. all undergraduate students of political science. Also, for anyone studying *social sciences* or *humanities*. Regardless of whether someone wants to pursue a career as a scientist or simply wants to use the results of political science for later practical work in administration, as a politician or as an engaged citizen, because this book elaborates the limits and possibilities of political science.

The book keeps its finger on the pulse of the current state of research. Six important suggestions are made for the further development of the subdiscipline. My suggestions are very far-reaching and have only partially been published elsewhere. Therefore, secondly, it is of particular interest to both advanced students and professionals working on the further development of the subdiscipline.

The current trend towards *edutainment* contradicts my approach. While the zeitgeist demands a simplification of complex content, I maintain the need for thorough discussion, especially since I have been working on this book for several decades. *No effort, no pleasure*, or as Seneca said: *per aspera ad astra.*

Acknowledgements

The search for scientific answers to political and practical questions was the reason why I studied philosophy, political science and medieval and modern history at the University of Heidelberg. Since then, these questions have been at the center of my work. Over time, it has become clear that the philosophical foundations of these questions are now dealt with in the philosophy of political science. This book offers a comprehensive overview of my answers and questions to these questions. The aim of this book is to discuss the philosophical foundations of political science. Parts of this book have already been published in my dissertation and in various articles.

Harold Kincaid and Jonathon Wayne Moses read early drafts of this book and made valuable suggestions. They deserve special thanks for this, especially since both are among the most experienced writers on the topics discussed here at the beginning of the 21st century. Harold Kincaid has written extensively in the field and is co-editor of the first introduction to the philosophy of political science (Kincaid/Van Bouwel 2023). Jonathon Wayne Moses, together with Torbjørn Lindstrøm Knutsen, has written one of the most successful and repeatedly published books on the subject (Moses/Knutsen 2019 [2007]), which does not focus solely on political science.

I have received inspiration for this work not only from the authors listed in the bibliography, but above all from members of the various institutes of the University of Heidelberg. I would like to thank the professors and lecturers who, in their seminars, lectures or papers, have given me particularly valuable suggestions and advice on the philosophical questions discussed here. Quotations and the bibliography can only indicate direct, not indirect, influences.

The encouragement I received at the *Institute of Political Science* deserves special mention. First of all, I would like to thank my doctoral supervisor Klaus Gustav Heinrich von Beyme for his consistently critical but also excellent human supervision of my dissertation. Special thanks also go to my colleagues in the doctoral colloquium for the constructive discussions and the critical remarks and comments. I would like to thank Michael Haus, the second reviewer of my dissertation, for further remarks and critical comments and for the opportunity to present the work at his colloquium. I would also like to thank the participants in the colloquium, especially Julian Tobias Klar. I also benefited from events at the Institute of Political Science with Wolfgang Merkel, Frank R. Pfetsch and Manfred Gustav Schmidt.

The *Philosophy Seminar* had a great influence on me and was also crucial for the creation of this work, especially since I studied philosophy as my main subject up to the Magister Artium. The intellectual climate of the seminar was shaped by Hans-

Georg Gadamer in the second half of the 20th century and was characterized above all by openness and interest in controversial dialogue with all philosophical currents. Gadamer's joy in controversy was impressive, especially with students. However, *openness* was not confused with *postmodern arbitrariness* in this seminar. Great importance was placed on the methodological foundations that make rational debate possible in the first place. I acquired the methodological foundations in interpretation courses and logic seminars (propositional and predicate logic, modal logic, time logic, deontic logic) with the following lecturers: Knut Eming, Michael Hampe, Bertram Kienzle, Harald Pilot, Hans-Peter Schütt. Only the foundations I acquired here enabled me to deal with questions of the philosophy of science at an adequate level. I would particularly like to highlight the works and lectures of Rainer Enskat, Erhard Scheibe, Wolfgang Wieland and Reiner Wiehl. Reiner Wiehl supervised my master's thesis "Philosophical Problems. The distinction between factual and linguistic problems in Wittgenstein's late philosophy". I contacted other philosophers by email, Otfried Höffe, Klaus Kornwachs and Hans Poser, who replied promptly. The latter two even made at that time unpublished manuscripts available to me.

I also dealt with methodological questions at events in the *History Department* with Detlef Junker and Dietmar Otto Ernst Rothermund. At the *Law Department* I attended lectures on the philosophy of law by Winfried Brugger and Friedrich Müller. Although I did not attend any events at the Max Weber *Institute for Sociology*, one cannot avoid dealing with the methodological work of Maximilian Carl Emil Weber when dealing with these topics. At this institute, Weber's work is intensively examined, whether it is reconstructed and further developed as a "Weberian research program" (Wolfgang Schluchter) or a "Weber paradigm" (Thomas Schwinn). A further step was to look at the philosophical work of the sociologist Gerhard Wagner, who was also contacted by e-mail.

Dealing with philosophical questions often left me with my head above the clouds. My wife Heidemarie and our two daughters Anita-Maria and Sonja Agnetha, who were both born during my studies, made sure that I kept my feet on the ground. Both, with my head above the clouds and with both feet on the ground, is not only an unusual way of moving, but also led to some belly landings that had to be overcome. So last but not least, I would like to thank my family for their patience and support. I was also reminded that there is a beautiful life outside of philosophy and political science. My greatest thanks go to my parents, Johann Lauer and Agnetha Lauer, née Hihn, who always supported me unconditionally with all the means at their disposal. I dedicate this monograph in grateful memory to my highly esteemed academic teacher Klaus Gustav Heinrich von Beyme.

Heidelberg/Leimen, winter 2025 Johann Lauer

1 Introduction: Philosophy of Political Science

1.1 Starting points: state of research in the philosophy of science and the philosophy of political science

1.1.1 State of research in the philosophy of science

The 19th century witnessed the emancipation of individual sciences from philosophy. But this process came to an end in the first half of the 20th century. At the same time, a new, special discipline was established within philosophy: philosophy of science (Humphreys 2016, Lohse/Reydon 2017, Okasha 2016 [2002], Rosenberg/McIntyre 2020 [2000]).

The *general philosophy of science* analyzes the rational limits and possibilities of the sciences or of the science system. The fundamentals of science and scientific research are at the center of all its investigations. It discusses the importance of empiricism, methodology, practical relevance, rationality and values. Its central questions relate to the foundations as well as the limits and possibilities of scientific research. In short, it is about guaranteeing *scientificity.* In my opinion, four *areas* and thus *complexes of questions* can be *ideally* identified:

1. *Ontology*: What *objects* can and should be scientifically investigated? What kinds of problems, what entities, characteristics, phenomena, relations or structures should be addressed within science?
2. *Axiology*: What *tasks* and *objectives* do scientists pursue? To what extent can and should values be studied?
3. *Epistemology*: What is science and what can it do? What results, forms of knowledge or theories can it legitimize? What are the epistemic limits of scientific research? According to what values should scientific research proceed?
4. *Methodology*: Which methodologies can guarantee *scientificity*?

At first, these questions were explained using the example of physics. Gradually, more disciplines joined in, including the social sciences (Cartwright 2014, Kincaid/Ross 2009, Kincaid 2012, Lohse/Reydon 2017). And now the 20th-century differentiation of philosophy of science has finally reached political science, one of the last standouts.

1.1.2 State of research in the philosophy of political science: philosophical awakening within political science

The special discipline *philosophy of political science* is still in its infancy. *The Routledge Companion to Philosophy of Social Science* (McIntyre/Rosenberg 2017) has a chapter on every social science (economics, history, political science, psychology and sociology/anthropology); the chapter on political science is titled "*Why is there no* *philosophy of political science?*" (Verbeek/McIntyre 2017). It states that there is no special discipline of "philosophy of political science" with a corresponding institutional anchoring in the science system, e.g., curricula, chairs, or journals.

There are, however, a variety of contributions that deal with the limits and possibilities of political science research as well as its scientific foundations and thus with fundamental questions of the subject. Indeed, the philosophical foundations of political science were already studied in antiquity, especially in the Aristotelian Organon. Since the emergence of modern political science at the beginning of the twentieth century, the arguments about the foundations of the discipline have split along ideological fronts. Unfortunately, even today one can legitimately speak of a *science war* or *Methodenstreit* (Lauer 2017).

On one side of this war are the naturalists, (neo- or post-) positivists or scientistic scientists, who base their view on the goals and methodologies of the natural sciences (Box-Steffensmeier/Brady/Collier 2010 [2008], Moses/Knutsen 2019 [2007], Kincaid/Ross 2009, Kincaid 2012, Dowding 2016, Kincaid/Van Bouwel 2023). On the other stand the hermeneutics, constructivists or interpretivists, who orient themselves on the goals and methodologies of the humanities (Bevir/Rhodes 2016, Creswell 2013 [1998], Flick/von Kardorff/Steinke 2015 [2000], Yanow/Schwartz-Shea 2014 [2006]). Georg Hendrik von Wright introduced a different terminology for these disputes, which largely coincides with the one mentioned above: he speaks of Galilean and Aristotelian traditions (von Wright (1971). As I have shown elsewhere (Lauer 2017), we can trace this ideological confrontation back to the axiological, epistemic, methodological and ontological foundations of the discipline. There are assumptions or prerequisites about the limits and possibilities of political science research that justify the formation of schools within the discipline of political science.

In addition to the contribution by Verbeek and McIntyre (2017), there are other introductory articles on the philosophy of political science. Catherine Herfeld (2017) presents three topics that the philosophy of political science should address: the methodological area, the traditional area of the philosophy of science and the area of normative questions. Ginaluca Pozzoni (2020) advocates a "philosophy of the social sciences in a unitary fashion", starting from the assumption that "the only legitimate definition of 'philosophy of political science' is 'the philosophical study of whatever happens to conventionally fall within the scope of political science at a given moment'" (Pozzoni 2020: 290).

Jonathon Wayne Moses (2020) speaks of a methodological awakening within political science:

> This methodological awakening can be seen in the three figures below, which are derived from a Web of Science topic search of their core collection (from 1945) for two terms (´ontolog*´and ´epistemolog*´), conducted on 25 July 2018. This search was limited to

> articles in the fields of ´political science´ and ´international relations´, and resulted in 674 hits for `ontolog*´ and 689 hits for ´epistemolog*´ (Moses 2020: 453).

There has been enormous growth since the 1990s, and even exponential growth in some periods of the 21st century (see the three figures in Moses 2020: 454-455). In addition to these articles, Moses quite rightly points to the many very influential books on these topics, which are also discussed in more detail in this work.

In my opinion, one can generally speak of a *philosophical awakening in political science*, as interest in the axiological, epistemic, methodological, and ontological foundations of the discipline has increased significantly over the past three decades. Firstly, it is not only the fields of ontology and epistemology that benefit from this growth, but also, as will be shown here, those of axiology and methodology. Second, at the beginning of the 21st century, the science war again expanded to the philosophical level, after having been restricted to the methodological level for a while (Lauer 2017; section 2.2, chapters 3-6).

In addition to these introductory articles, there are three main books that deal with the subdiscipline "philosophy of political science". In the following, I will only briefly point out the differences to my book.

Recently, Harold Kincaid and Jeroen Van Bouwel published the most comprehensive introduction to the subdiscipline: The Oxford Handbook of Philosophy of Political Science (Kincaid/Van Bouwel 2023). In it, only the naturalistic (Galilean, positivistic, scientistic) tradition or way of knowing is addressed, as in the other Oxford Handbooks dealing with related topics (Box-Steffensmeier/Brady/Collier 2010 [2008], Kincaid/Ross 2009, Kincaid 2012).

The other two books are published by Palgrave/Macmillan. In his book "The Philosophy and Methods of Political Science", Keith Dowding (2016) deals with the philosophical foundations of the *empirical* mainstream, which corresponds to the naturalistic tradition mentioned above. Jonathon Wayne Moses and Torbjørn Lindstrøm Knutsen (2019 [2007]) discuss the issues raised here in great detail in their book "Ways of Knowing. Competing Methodologies in Social and Political Research", which has had several editions since 2007.

What is special about these is that they advocate not only a methodical but also a methodological pluralism: the complementarity of the constructivist and naturalistic ways of knowing is convincingly presented. One goal is to build methodological bridges (Moses/Knutsen 2019 [2007]: 299) between constructivism and naturalism.

One of the main goals of my book, which I first presented in other works (Lauer 2017, 2021a), is to show that we need *three traditions*: a *descriptive-interpretive* tradition, an *explanatory-prognostic* tradition, and a *practical* tradition. I show that practical investigations cannot be understood as *applied science* and cannot be classified in either the naturalistic or constructivist tradition. The fundamental differences between these three traditions are comprehensively elaborated on ten levels in this book (overview chart 2, section 9.4.2).

My most important suggestion for the innovative further development of the subdiscipline is to show that a *practical political science* is possible as a *complement* to *empirical-analytical political science.* The basis of practical political science is the practical tradition mentioned above. I don't believe in scientific revolutions and scientific turns; innovative further development is the appropriate progress strategy (section 2.6). This is the first comprehensive difference to the three books above.

My book is only about the philosophical foundations of political science and not about the specific methods or methodological approaches that political scientists use to explain or change the world. It is therefore not a *method book* that offers a practical introduction to the way political science research can or should be carried out using individual methods. This marks a second difference, because the three other books not only deal with the philosophical foundations, but also, in some cases, with concrete political science methods.

The third difference is that in the ninth chapter I design a *curriculum* for the subdiscipline. The aim of this *syllabus* is to support political scientists and students in understanding (bachelor’s seminar) and exploring (master’s seminar) the limits and possibilities of political science. As mentioned above, it is not a methodological book that offers practical instructions for studying political phenomena, collecting and analyzing data with a view to conducting sound and meaningful political science research.

In the following, the questions and aims of my book will be explained in more detail.

1.2 Questions: philosophical foundations that ensure the scientificity of political science research as a subject area of the philosophy of political science

The book is aimed at *practitioners* and *scientists* who want to discover the *limits* and *possibilities* of science and how to draw a *demarcation line* between *science* and *non-science.* “Follow the science” revealed the *total scientification* of all areas of life, including politics. Precisely for this reason, a critical and prominent examination of the *philosophical foundations* of science and how to guarantee *scientificity* should be the main topic of scientific, political and social debates.

Due to the necessary specialization of political science, the empirical orientation as well as the concentration on empirical and practical theories, i.e. especially on the empirical and practical content, important philosophical prerequisites are lost. However, these philosophical foundations determine the scientific nature of political science research and have a decisive impact on the content. The focus of the philosophy of political science should therefore lie on the following areas: *axiology*, *epistemology*, *methodology* and *ontology*. Its central questions relate to the foundations as well as the limits and possibilities of political science research. In short, it should serve to guarantee *scientificity* in political science. For systematic reasons, the following sets of questions should be addressed:

1. *Ontological* foundations: What is the *subject area* of political science as part of the science system? What kinds of *problems*, what *entities*, *properties*, *phenomena*, *relations* or *structures* are addressed within political science?
2. *Axiological* foundations: What *tasks* and *goals* can and should political scientists pursue? To what extent can and should political *values* be researched? What value should scientific *results* have for *state* and *society*?
3. *Epistemic* foundations: What basic *epistemic assumptions* do political scientists make? What *results*, political *knowledge*, or political *theories* can political science generate? How can political scientists *justify* knowledge? What *forms of knowledge* can they generate? What *values* must political science research satisfy? What are the *epistemic limits* of (political) science research and knowledge generation? What *ideals* does science strive for?
4. *Methodological* foundations: Which political science methodologies can guarantee *scientificity*? What are the *limits* and *possibilities* of these methodologies? Which political science methodologies are used?

The great variety of questions as well as the complex and diverse answers within the philosophy of science are a positive surprise. Of the negative aspects, the above-mentioned *science war* (*Methodenstreit*) is especially striking. Therefore, I will also pursue the following questions:

5. *Science war*: *How* does this science war come about? How can it be *overcome*? What role can an *institutionalization* of the subdiscipline philosophy of political science play in the future?

1.3 Objectives of the book

Aristotle and Plato, as the founders of political science, also addressed the *philosophical foundations* of the discipline. Since then, the number of contributions to these questions has become almost unmanageable. Now the foundations of the discipline are dealt with in the subdiscipline philosophy of political science.

The first main aim of the book is to provide a *logical geography* of the *philosophical foundations* (axiological, epistemological, methodological and ontological) of the philosophy of science using the example of political science. The most controversial issues of the methodological *science war* (*Methodenstreit*) and the *genesis* of the debates are highlighted (section 1.3.1). The book moves at the pulse of the *current state of* *research*, so the second main goal is to present important proposals for the *evolutionary further development* of the subdiscipline philosophy of political science (section 1.3.2). The third major objective is to establish the first *curriculum* for this subdiscipline (section 1.3.3).

1.3.1 Logical geography of the philosophy of political science: overview of the philosophical foundations as well as limits and possibilities of political science research

The first major purpose of this monograph is to present a *logical geography* of the philosophy of political science, or an overview of the axiological, epistemic, methodological, and ontological foundations of political science research at *ten vertical* and *three horizontal levels* (1st and 2nd chart, sections 9.4.1 and 9.4.2). It also explains how *scientificity* can be guaranteed in political science. The *limits* and *possibilities* of political science research will also be described.

Firstly, the *systematic intent* is to record the *state of research*, especially the most *controversial* topics, within the philosophy of political science. Secondly, the *genesis* of the debates should be rendered visible by capturing the themes in the historical texts by authors who discussed the questions first or formulated fundamental developments.

1.3.2 State of research in the philosophy of (political) science, the need for a new framework for the debate and methodological approaches to critical reception of, reflection on, and further development of philosophical tradition with a systematic intent

The book moves at the pulse of the *current state of research*, and therefore the second main objective is to capture the state of research in the philosophy of (political) science, to present the need for a *new framework* for the debate and the *methodological approaches* with the help of which a reception of, reflection on, and *further evolutionary development* of existing philosophical traditions can succeed with a systematic intent.

Six *own theses* on current issues are justified, which should enable an *innovative* and *evolutionary further development* of the subdiscipline:

- Evolution instead of revolution, ideal-typical, multilingual *ad fontes* reconstruction with a systematic intent (A).
- External influences on science: non-epistemic norms and values within science (B).
- Epistemic norms or values constitute or enable science: demarcation line between science and non-science (C).

- Overcoming the science war (*Methodenstreit*). Complementarity between three philosophical traditions (descriptive, explanative-prognostic and practical) as a way out (D).
- Further development of political philosophy/political theory into a practical political science (E).
- Plea for an institutionalization of the subdiscipline philosophy of political science (F).

A. Evolution instead of revolution, ideal-typical, multilingual *ad fontes* reconstruction with a systematic intention

We live in *exciting times:* digitization, globalization, climate change etc. undoubtedly brings new, incremental and disruptive innovations. Content and methodological innovations are the result. At the same time, we also live in *turbulent times.* Due to the laws of the attention economy, it is almost impossible to reach one's fellow human beings unless one makes a mountain out of every molehill: an idea becomes a paradigm, an innovation a revolution, a tool a methodology. In short, the *Zeitgeist* thirsts for revolutions. My motto is: combining tradition and progress. Therefore, I advocate *evolution* instead of *revolution* (section 2.6).

This work presents *methodological approaches* with the help of which a reception of, reflection on, and further development of philosophical traditions can succeed with a systematic intent. The following methodological approaches will be explained: *ad fontes reconstruction* (section 2.3), determination of *ideal-type issues* (axiological, epistemological, methodological, and ontological) within the philosophy of science (section 2.5), *evolution* through critical reception of, and innovative development of traditions rather than revolution (section 2.6), and a *multilingual* approach (section 2.4).

A *multilingual ad fontes* reconstruction of the philosophical foundations of the subject is particularly necessary because an evolution and further development of these traditions is the better approach. Arguments will be presented to show that the revolutionary alternative (revolutions or turns) favored by many is not nearly as powerful. Therefore, the following terms will be in focus: evolution, research programs, ideal types, innovations, multilingual *ad fontes* reconstruction, and further development, and not paradigm or revolution (chapter 2).

B. External influences on science: non-epistemic norms or values within science

Axiological questions, in this case non-epistemic norms and values, are identified that are posed to science from outside, by society, politics, citizens and scientists (10th and

11th chart, sections 9.4.10 and 9.4.11). Six sets of questions are distinguished and discussed separately in order to avoid unnecessary misunderstandings (chapter 4):

1. *Value relationship*: The scientist's value system, the scientist's relationship to his research object
2. The value or *political* and *public relevance* of science
3. *Epistemic values for science*, or criteria for better methodologies that ensure the authority of science
4. Values as an *object of science* (values in the object area)
5. *Value base*: non-epistemic norms and values that influence scientific results
6. The problem of *value judgments in the narrower sense*: Value-free empirical science is possible, but empirical justification of norms is impossible, while practical justifications are possible.

C. Epistemic norms or values constitute scientificity: enabling science and demarcation between science and non-science

Popper believed that one criterion is sufficient to distinguish between knowledge and pseudo-knowledge. In my opinion, an extremely complex methodology based on a *plethora* of general, abstract or universal as well as concrete, special or local *conditions* makes it possible to legitimate scientific knowledge and to draw a *line of demarcation* between *science* and *non-science*. These epistemic or constitutive norms and values guarantee *scientificity* (chapter 5).

Scientifically founded *knowledge* consists of empirical and practical *theories*, axiological, epistemic, methodological and ontological *foundations* as well as scientific *methodologies*, and has a *hypothetical* character (8th, 9th and 10th charts, section 9.4.8, 9.4.9, and 9.4.10).

D. Overcoming the science war (*Methodenstreit*). Complementarity between three philosophical traditions (descriptive-interpretative, explanatory-predictive and practical) as a way out

Von Wright (1971) speaks of the *Aristotelian* and *Galilean traditions*, Moses and Knutsen (2019 [2007]) distinguish between *constructivists* and *naturalists*. This book also presents and examines two opponents in detail: the *scientistic scientists* of the explanative-prognostic or the Platonic-Galilean tradition, who are oriented to the natural sciences (3th chart, section 9.4.3); and the *perestroikans*, who belong to the Aristotelian tradition and are oriented towards the humanities (4th chart, section 9.4.4).

Paraphrasing a well-known bon mot of Whitehead,[1] one might ask: is the general characterization of the philosophical traditions just a footnote to Plato and Aristotle?

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[1] The safest general characterization of the European philosophical tradition is that it consists of a series of footnotes to Plato (Whitehead 1978 [1929]: 39).

The differentiation into Platonic-Galilean and Aristotelian tradition would suggest this (von Wright 1971). First, it must be noted that these traditions cannot be played off against one another, since each has its justification and cannot be reduced to any other. Secondly, dynamism and innovation is possible on both the Platonic and the Aristotelian basis.

How can the religious war be overcome? Neither *revolutions*, as the scientistic scientists think, nor *turns*, as the interpretivists demand, will help: evolution and innovation instead of revolution, innovative further development instead of revolutions or turns is required (section 2.6). The existing methodologies must first be explained, made explicit, specified and reconstructed: the heritage must first be worked out[2] before it can be reformed and innovatively developed further. There is no Archimedean point, just a variety of proven methodologies and perspectives. This can best be realized if the complexity of the tasks is adequately discussed at several levels. Maintaining methodological traditions does not conflict with innovations and further developments, i.e., it does not prevent scientific dynamism. Both are necessary, hence my motto "combining tradition and progress".

The *science war* can be overcome by demonstrating the *complementarity* of three philosophical traditions. Therefore, I will identify the fundamental differences between *descriptive-interpretative, explanative-prognostic* and *practical* (normative, pragmatic and technical) *traditions* on ten levels and then show their complementarity (2nd chart, section 9.4.2, and section 2.2,).

E. The advancement of political philosophy/political theory into a practical political science

The *emancipation* of political science from philosophy has not yet been fully achieved, especially in the subdiscipline of political philosophy/political theory. The emancipation of each individual science is associated with a division of labor, i.e. differentiation and specialization. This includes in particular a separation between theory (content) and methodology (form). In empirical political science, this differentiation has long since taken place, as can be seen above all in the large number of empirical (descriptive-interpretative, explanative and prognostic) methodological books that are published independently of empirical theories. In political philosophy/political theory, practical-normative theories and methodologies are usually dealt with together. There are hardly any practical (normative, pragmatic and technical) methodological books.

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[2] What you have inherited from your fathers,
Acquire it in order to possess it (my translation).
Was du ererbt von deinen Vätern hast,
Erwirb es, um es zu besitzen (von Goethe 1978 [1808]: 171 [682-683]).

A *practical* (normative, pragmatic, and technical) *methodology*, as presented and justified here, can contribute to the further development of *political philosophy/political theory* into a *practical political science* and to the *rational justification* of practical (normative, pragmatic, and technical) knowledge (2nd, 3rd, 8th and 12th charts, section 9.4.2, 9.4.3, 9.4.8, and 9.4.12).

F. Plea for the institutionalization of the subdiscipline philosophy of political science

Finally, this book presents a plea for the *institutionalization* of the subdiscipline philosophy of political science, with a corresponding institutional anchoring in the system of science, e.g. in curricula, chairs and journals. It presents the most important arguments that speak for the importance of the subdiscipline.

Philosophy of political science should, in my opinion, have at least two major tasks: first, to identify the scientific limits and possibilities of the discipline as well as to further develop the philosophical foundations of political science. And second, to prevent a politicization and moralization of political science (chapter 7).

1.3.3 Curriculum for the subdiscipline philosophy of political science: bachelor's and master's seminars and extensive charts

The third major objective is to establish the first *curriculum* for the subdiscipline *philosophy of political science*, and to show what should be considered. A *basic* (undergraduate) and an *advanced* (graduate) *seminar* are developed for *beginners* and *advanced prospects*. The *charts* in the book provide a quick *overview* of the most important topics and a *reduction in complexity* of special discussions and subject areas (chapter 9).

1.4 Book structure

The *introduction* is intended to provide a general lead-in to the topic and, above all, an overview of the most important questions in this monograph. The objectives, the structure and the procedure are also presented. The *second chapter* explains the methodological approaches that are used in this work for the reception of, reflection on, and further development of philosophical traditions with a systematic intent: *ad fontes* reconstruction, determination of ideal-typical questions within the philosophy of science, evolution by means of innovative further development of the traditions instead of revolution, and a multilingual approach. Furthermore, it provides an overview of the opponents in the *science war* (*Methodenstreit*) and their most important points of contention.

The *third chapter* deals with ontological questions, using the example of political science. This is done with the aim of working out the *ontological* foundations that are of particular importance within political science. The *axiological* foundations of the discipline are discussed in the *fourth chapter.* The *non-epistemic norms and values* that scientists, society, politics and other institutions as well as political and social groups bring to science are presented. The separation between *non-epistemic* and *epistemic values* can only be made ideally. For purely pragmatic reasons, for example, I treat the normative specifications that scientists want to follow in determining causalities in this chapter, even though these can also be regarded as epistemic values and therefore could have been treated in the next two chapters.

The question of *scientificity* is the focus of the fifth and sixth chapters. These present the *epistemic* or *constituting norms and values* that make *science possible*, and draw a *line of demarcation* between rational, scientific knowledge and other forms of knowledge or between *science* and *non-science.* The *fifth chapter* on *epistemology* contains the general, abstract, or universal conditions of knowledge. The concrete, local, or special conditions of knowledge are mainly discussed in the *sixth chapter* on *methodology.* The focus of philosophers is on the universal conditions, while the focus of individual disciplines is on the special conditions. I will pay special attention to identifying the *fundamental differences* between three philosophical traditions – *descriptive-interpretative, explanative-prognostic* and *practical* (normative, pragmatic and technical) – on ten levels as well as their *complementarity.*

The *seventh chapter* is a plea for the institutionalization of the subdiscipline philosophy of political science, with a corresponding institutional anchoring in the system of science, e.g. in curricula, chairs and journals. The most important arguments that speak for the importance of the subdiscipline philosophy of political science are presented.

The *eighth chapter* summarizes important conclusions on the axiological, epistemic, methodological, and ontological *foundations of political science* as well as the *limits* and *possibilities* of political science research. In particular, it focuses on *my own suggestions* for the further development of the subdiscipline philosophy of political science.

At the end of the book, the *ninth and final chapter* develops a *curriculum* for the subdiscipline philosophy of political science consisting of an undergraduate (bachelor's) and a graduate (master's) seminar as well as extensive charts. This *syllabus* aims to provide students with a structured and in-depth exploration of the philosophical dimensions of political science, equipping them with the necessary knowledge and critical thinking skills to engage meaningfully in this interdisciplinary field.

1.5 *Per aspera ad astra*: No edutainment, no fictional Narrative, only logico-analytical (rational) analysis and hermeneutic multilingual *ad fontes* interpretations

Having explained in detail what you will find in this book, it is now time to say what you definitely will not find. In short, I don't believe in superficial knowledge or fleeting trends, in the creation morsels or tidbits of knowledge for small talk. No edutainment, no fiction, only logico-analytical (rational) analysis and return to primary sources through careful hermeneutic multilingual ad fontes interpretation. Only through rigorous scholarly inquiry can we truly achieve meaningful understanding.

Edutainment is a popular method of knowledge transfer in which educational content is presented in an entertaining and engaging way to make learning more interesting and enjoyable. The goal of edutainment is to present learning content through entertaining storytelling in a form that maintains learners' attention and interest, making learning more effective and motivating. Many books, especially introductions to philosophy and the humanities in general, are characterized by witty narration, paraphrasing and storytelling. Emotional images and metaphors help, as does varied language that avoids repetition and makes extensive use of synonyms.

Every instrument always has two sides, a positive and a negative. On the one hand, edutainment can significantly increase motivation. On the other hand, even the wittiest narratives are far too imprecise and can therefore have a very negative impact on both the development and controversies within a discipline. Inaccurate narratives have had a significant negative impact on developments within the philosophy of science. The methodological science war (*Methodenstreit*) in particular owes its emergence and continued existence to such inaccurate narratives. As this book also shows, it requires enormous effort and time to firstly point out the pseudo-philosophical problems that have arisen for over 100 years, secondly to identify the important questions of the subdiscipline, and thirdly to formulate precise answers to these problems.

Narratives that are based in myth have shaped scientific discussion for centuries. This problem will become even more serious in the future. Thanks to generative artificial intelligence, so-called hallucinations are generated that are coherent and syntactically correct, but still wrong. I discuss the limits and possibilities of generative AI elsewhere: Philosophy of generative artificial intelligence. Theoretical limitations and possibilities, practical benefits and threats of large language models (Lauer forthcoming).

In other words, we are dealing here with an epistemological sophism or with the Gettier problem (section 5.2.1). These are false arguments or narratives because they are based on false assumptions or misunderstandings.

Not only must the coherence of the narratives be correct, but also the premises on which they are based. In addition, both the narrative analyses and their premises should be evidence-based (section 9.3.6). To ensure this, this book firstly aims at an *ad fontes* reconstruction (section 2.3). Secondly, own narratives are created using a *multilingual approach* (section 2.4). Thirdly, this book is written in the spirit of *analytical* philosophy (see acknowledgements), in which logical-analytical rigor and not rhetorical momentum determines the analysis. I am not afraid to be *pedantic* when I think it necessary. In my opinion, Maximilian Carl Emil Weber coined the motto of analytical philosophy before it even came into being:

> Personally, I believe that no means in this world is 'pedantic' if it serves to avoid confusion (Weber 1973d [1917]: 510 [472]; my translation).[3]

The logical-analytical approach is not about denying emotionality or subjectivity. Science is an endeavor that enables and strives for an intersubjective and rational view also of emotionality or subjectivity. This also applies to values (chapter 4).

One drawback of the logical-analytical approach is that reading is sometimes perceived as a burdensome task. There are numerous challenging passages to be mastered, characterized by a complex structure. The approach is also characterized by a certain attention to detail, which is expressed in a detailed presentation of arguments and a strong orientation towards original sources. This requires extensive paraphrasing and the integration of many quotations into the text (for further details, see section 2.3 Ad fontes reconstruction with a systematic intention and section 2.4 Multilingual approach: the central importance of multilingualism for science).

The *zeitgeist* demands *edutainment*, and I find that difficult. I have been working on this book for over three decades, and I am not prepared to turn a complex text into an infantile text for everyday use. However, it is possible to enjoy the process. While pleasure and enjoyment can still be derived from this approach, they often only arise after a challenging journey. Similarly, in extreme mountaineering, joy is typically only experienced after a strenuous climb that requires intense concentration. The joy of exploring a philosophical star set in is only achieved after overcoming the hardships of rough paths. The book demands that the reader be prepared to concentrate on it. No effort, no pleasure, or as Seneca wrote: *per aspera ad astra.*[4]

The following analogy serves to illustrate a point that has been previously made. To gain an overview of the Alps, one can drive through them on the motorway. However, an Alpine explorer should not only ignore the hardships of the plains, but must also

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[3] Persönlich bin ich der Ansicht, dass kein Mittel der Welt ‚pedantisch' ist, um nicht zur Vermeidung von Konfusionen am Platze zu sein (Weber 1973d [1917]: 510 [472]).

[4] "Per aspera ad astra" is a very common Latin phrase that goes back to Seneca, the translation of which is: Over rough paths/Through hardship one reaches the stars.

set off through the Alps to explore them. Only by doing this can one speak of serious exploration of the Alps.

The ninth chapter includes ten bachelor's courses and seven master's courses, and offers guidance to explore seventeen 'philosophical stars'. The pleasure of exploration is not restricted to the initial experience; each exploration brings new insights and therefore new joys. A Socratic habitus, which involves a constant questioning of certainties, enables the generation of new insights and the associated new joys.

I wish readers a busy, stimulating and profitable reading not only of this book, but also in their possible re-reading of the books referred to in this book.

1.6 Literature, self-citations and citations

The citations in this book and in the references have several *special* features:

- ➢ The citation first indicates the year of the edition used, and the original year of publication in brackets. For classic works, the original year of publication (not the year of publication of the first German translation) is always stated in brackets after the year of publication of the edition used.
- ➢ The page number refers to information on the used edition. Exceptions to this are due to edition conditions. In classics such as works by Aristotle, Plato, Kant or Weber, the page numbers of basic editions are also recorded in brackets. A special citation is also appropriate for the works of Wittgenstein. When citing the Philosophical Investigations, the paragraph is indicated instead of the page number, while only the number of the sentence is given when citing the Tractatus logico-philosophicus.
- ➢ The indication of the edition is omitted in the text, whereas it is indicated in the bibliography. For online articles, the URL as well as the date of last access of the website are given.
- ➢ As a rule, the all first names of authors are mentioned in the references, provided they could be determined.

I have also been publishing scientific papers on the Internet for years, and a number of projects are constantly being developed as “works in progress”. I use extensive material from these projects.

Self-citations are avoided because they would be very reader-unfriendly. The necessary URL information would make the text illegible, as the projects that are being developed are published in HTML format and not in PDF format. These projects have not yet been published in print.

I have translated into English the quotes from German-language books for which there are no English translations.

2. The state of research in the philosophy of (political) science: the need for a further development of the debate as well as methodological approaches to the critical reception of, reflection on and further development of the philosophical tradition with a systematic intent

- What approach can succeed in achieving the goal of a philosophy of political science with systematic intent?
- Which methodological approaches can be used to achieve an adequate overview of this complex of topics?
- Why is evolution by means of innovative further development of traditions more useful than scientific revolutions?
- What are the most important concepts, deficits and points of contention in the science war (*Methodenstreit*)?

The following topics are addressed in this chapter:

- Starting points: sprawling philosophical discourses and an unmanageable variety of scientific tools (section 2.1)
- Concepts, opponents and points of contention in the science war (section 2.2)
- *Ad fontes* reconstruction with a systematic intention (section 2.3)
- Multilingual approach: central importance of multilingualism for science (section 2.3)
- Ideal types: determination of ideal-type questions within the philosophy of science (section 2.4)
- Evolution through innovative further development of traditions instead of revolution (section 2.6).

2.1 Starting points: sprawling philosophical discourses and an unmanageable variety of scientific tools

There is currently, at the beginning of the 21st century, an unmanageable variety of scientific tools (concepts, sentences, theories, logics, ways of argumentation, methods and methodical approaches) as well as discourses on philosophical foundations and prerequisites of scientific work both within the philosophy of science and within the social sciences, not least within political science.

Several different approaches, but also attitudes, can be observed within political science at the beginning of the 21st century in this context:

- Ignorance of methodological questions, “just do it” pragmatism (section 2.2.1)

- Methodological hard work (drudgery) in normal mode (section 2.2.2)
- Methodological wars of faith in revolutionary mode (section 2.2.3)
- Pluralistic habitus and reductionist practices (section 2.2.4).

2.1.1 Ignorance of methodological questions, “just do it” pragmatism

Since the emergence of modern social sciences, methodological questions have been regarded by many empirical scientists as a necessary evil, if not as superfluous. An attitude characterized as pragmatic (“just do it” pragmatism) coupled with an ignorance of methodological questions determines action, probably of the vast majority of political scientists:

> Their motto is, once again: “just do it” (Barry 1970, v; Dryzek 2005) (quoted from Goodin 2011b [2009]: 29).
>
> But the vast majority of political scientists whose main concern lies elsewhere are generally nonplussed. They do obeisance to the reigning “big thing” in their opening paragraphs, but then they get down to business in pretty much the same way they would have done under any alternate regime (Goodin 2011b [2009]: 30-31).

Those who want to give this attitude a higher consecration can, albeit without justification, even quote a classic like Weber:

> All of this gives rise to the wish that the current fashion, that every beginner’s work has to be adorned with epistemological investigations, should very soon die out[5] (Weber 1973b [1903-1906]: 127; my translation).

This comment referred in a footnote to the work of Dr. Bierman. Weber is a classic of the social sciences not least because he made lasting contributions to the methodology of the social sciences and also implemented the methodological specifications in his empirical work. Therefore, one should not overestimate these critical remarks and should pay close attention to their context.

2.1.2 Methodological hard work (drudgery) in normal mode within different methodological traditions and schools

Methodological drudgery is mainly practiced within different methodological traditions, or in schools within these traditions. Without this work, the enormous methodological progress of the last century would not have been possible.

The explanative-prognostic or the *Platonic-Galilean* tradition, which cultivates methodological reductionism and is very homogeneous, is usually presented first. This is followed by the Aristotelian tradition, which represents a methodological pluralism and is very heterogeneous. From this tradition, only one school, the perestroikans, will

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[5] [D]ies alles läßt den Wunsch entstehen, es möge die heutige Mode, daß jede Anfängerarbeit mit erkenntnistheoretischen Untersuchungen geziert werden muß, recht bald wieder aussterben (Weber 1973b [1903-1906]: 127).

be discussed in this book in detail. The distinction between the Platonic-Galilean and the Aristotelian tradition, or, to put it another way, between methodological reductionism and methodological pluralism, ties in with the work of von Wright (1971).

“The Oxford Handbook of Political Methodology” (Box-Steffensmeier/Brady/Collier, 2010a [2008], hereafter cited only as “"Political Methodology”) provides an outstanding overview of the naturalistic, positivistic, or scientistic methodology of the discipline (for the most recent overview, see Kincaid/Van Bouwel 2023), which is oriented toward the natural sciences, sees itself as part of the social sciences, and works primarily with experiments, logical-mathematical modes of argumentation, quantitative and qualitative-mathematical (qualitative-positivist) methods, and models (especially rational choice models).

Criticism of this scientistic, explanative-prognostic or the Platonic-Galilean tradition already arose in the 19th century among scholars who leaned on the cultural and human sciences (*Geistes- and Kulturwissenschaften*), favoring linguistic, hermeneutic, (post-) structuralist, or interpretive modes of argumentation, as well as qualitative-interpretive methods and methodological approaches (Dilthey 1922 [1883], Rothacker 1926, Rickert 1910 [1896], Windelband 1900 [1894], Gadamer 2010 [1960], Garfinkel (1967), Glaser/Strauss 1967, Foucault 1971 [1966] and 1995 [1969], Geertz 1983 [1973]), Giddens (1984 [1976]), Bodammer 1987, Denzin/Lincoln 1994, Creswell 2013 [1998], Flick/von Kardorff/Steinke 2015 [2000], Blatter/Janning/Wagemann 2007, Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a).

At the start of the 21st century, a new methodological school within the *Aristotelian* tradition is rebelling against the naturalistic methodology of the scientistic scientists. This school sees itself as phronetic political or social science (Flyvbjerg 2001 and 2006, Schram/Caterino 2006) or as real social science (Flyvbjerg/Landman/Schram 2012a). Phronetic scholars adopt arguments from the Aristotelian tradition and refer to it in their humanistic values, but they see themselves as part of the perestroika movement and argue for an independent, real social science. A first overview of the diversity of the perestroika movement is provided by the volume “Perestroika. The Raucous Rebellion in Political Science” (Monroe 2005). An evaluation was also provided in this volume, followed by another evaluation in 2015 in the journal *Perspectives on Politics* (Gunnell 2015a and 2015b, Farr 2015, Laitin 2015, Monroe 2015, and Schram 2015).

It must be emphasized that the hard work takes place almost exclusively within the two traditions. As a rule, the representatives of these two traditions do not engage in any constructive debate with representatives of the other tradition, but conduct an unproductive, scientifically religious war.

2.1.3 Science war in revolution mode. The opponents in the *Methodenstreit* within political science at the beginning of the 21st century: scientistic scientists versus phronetic perestroikans

Methodological disputes within political science can take on questionable traits. Von Beyme speaks of "a religious war with Manichaean features of hardening of the front"[6] (von Beyme 2000 [1972]: 142, my translation). He was referring to the methodological debates during the so-called *behavioral revolution* in the 1950s and 1960s and the *rational choice revolution* since the 1970s. Unfortunately, religious wars with Manichaean features have not disappeared at the beginning of the 21st century. So, says Goodin, a representative of the scientistic establishment. In his "State of the Discipline, the Discipline of the State" he also states that disputes within political science often take on a "Manichean, Good versus Evil form" (Goodin 2011b [2009]: 10).

The scientistic scientists (3rd chart, section 9.4.3) usually simply deny the scientific nature of their opponents' investigations:

> However, American scientists have in many cases reduced all competing approaches [meaning all approaches except the rational choice approach] to the status of inferior journalism[7] (von Beyme 2000 [1972]: 148; my translation).

This subdivision is also likely to be overstated for the economy, but it essentially captures a division of the subject that has existed in this way also within political science, especially in the U.S., since the emergence of political science and continues to exist today. While the history of economics also tends to be pursued in the history departments, scientistic political scientists attempt to relegate the history of ideas, political philosophy and generally all *theorists* who *only* work with linguistic-interpretative argumentation and qualitative-interpretative methods to the humanities and claim the status of scientist only for those who search for causal relations or causal mechanisms.

> Political theory is an interdisciplinary endeavour whose centre of gravity lies at the humanities end of the happily still undisciplined discipline of political science (Dryzek /Honig /Philips 2009: 62; see ibid *political scientist versus political theorist* S. 63).

The rejection and devaluation of other axiological, epistemological and ontological basic assumptions as well as methodological procedures is one of the strategies of the scientistic scientists in this science war. This is accompanied by a rejection, or at least

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[6] Glaubenskrieg mit manichäischen Zügen der Frontverhärtung (von Beyme 2000 [1972]: 142).

[7] Amerikanische Wissenschaftler haben aber vielfach alle konkurrierenden Ansätze [gemeint sind hier alle Ansätze außer dem Rational-Choice-Ansatz] auf den Status eines inferioren Journalismus herabgedrückt (von Beyme 2000 [1972]: 148).

a devaluation, of linguistic-hermeneutic or linguistic-interpretative argumentation, methods and methodological approaches.

> Political science is the only social science in which the history of past theories plays a special role. ‘Dogma history’ in economics, on the other hand, has become a minor subject for ‘fairy tale exams’ – alongside the hard business of quantifying economics[8] (von Beyme 2002: 14).

This division is probably overdrawn for economics as well, but it strikes at the core of a division of the subject that has existed in this way within political science, especially in the U.S., since the emergence of political science and still exists today. While economic history tends to be pursued in the history departments, scientistic political scientists also try to relegate the history of ideas, political philosophy and generally all “theorists” who “only” work with linguistic-interpretative argumentation and qualitative-interpretative methods to the humanities and to claim the status of a scientist only for those who work with quantitative methods and search for causalities or want to identify some.

For their part, the opponents of the scientistic scientists (the human and cultural scientists, constructivists, anti-positivists, hermeneuticists, interpretivists, structuralists or post-structuralists, phronetic perestroikans, see 4th chart, section 9.4.4) react with exaggerated and sometimes unjustified criticisms. While the scientistic scientists often do not take their opponents seriously or ignore them altogether, the interpretivists always start by building up a naturalistic or positivistic straw man, which can then be easily refuted. Not only do they reject the basic positivist and naturalist assumptions for exploring the political and social world, but they also find the logical-mathematical tools to be inadequate for the study of political science. These are essentially the main arguments from which the call for a *turn* in the human and social sciences is derived and the need for an anti-positivist political science is postulated.

Furthermore, the interpretivists point to the alleged lack of public relevance (Flyvbjerg, 2001), sterile methodological orientation (Green/Shapiro 1994) or unworldly self-centeredness in the form of methodological scholasticism (Mead 2010) of the discipline. It is not surprising that Flyvbjerg (2006: 56), one of the representatives of the perestroika movement speaks of a “science war” (Flyvbjerg 2001: 1).

The formation of a front, which degenerated into a war of faith, has been going on since the 19th century because of the orientation of some scientists within the social sciences to the natural sciences and the resistance of others to such efforts.

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[8] Die Politikwissenschaft ist die einzige Sozialwissenschaft, in der die Geschichte vergangener Theorien eine besondere Rolle spielt. ‚Dogmengeschichte‘ in der Ökonomie wurde dagegen zum Nebenfach für ‚Märchenklausuren‘ – neben dem harten Geschäft der quantifizierenden Ökonomie (von Beyme 2002: 14).

The *Kuhn narrative*, i.e. the use of Kuhn's (1976 [1962]) philosophy of science to explain progress within science, acts like a fire accelerator because it is used by both sides, not just to prove one's own superiority to the opponent, but also to present the opponent as obsolete. This makes a meaningful debate between scientists with different basic scientific assumptions impossible from the outset.

It is amazing that scientists who, insofar as they act as *normal* scientists, use the endogenous values of the scientific community (*communis opinio doctorum*) and the necessary tools in an exemplary manner, turn into *warriors of faith* when they act as revolutionary standard-bearers who know only one goal – to finish off all methodological opponents by all available means.

In my opinion, a money or *power orientation* (power and money driveness) can also be identified in the revolution mode. In other words, one factor is material resources, recognition, publication, career opportunities etc. This is not least the case because the existential, socio-economic situation of the overwhelming majority of political scientists has been very precarious since the emergence of political science in the U.S.

Maximilian Carl Emil Weber already wrote at the beginning of the 20th century that the situation of many scholars and scientists in Germany was the same as in the U.S.: "as precarious as any 'proletarian' existence"[9] (Weber 1973e [1919]: 584 [526]; my translation).

Aristotle, on the other hand, did not personally have to struggle with such existential or economic problems. It is easier to proceed *sine ira et studio* in such conditions.

> If social science were viewed less as a prizefight between competing theoretical perspectives, only one of which many prevail, and more as a joint venture in which explanations condition and augment one another, the partisan impulses that give rise to methodologically deficient research might be held in check (Green/Shapiro 1994: 204).

Unfortunately, this appeal from the last century has so far remained a pious wish, like the religious war between scientistic scientists at the beginning of the 21st century on the one hand (King/Keohane/Verba 1994, Brady/Collier 2010 [2004], Box-Steffensmeier/Brady/Collier, 2010a [2008], Kincaid/Van Bouwel 2023, Humphreys 2016) and the phronetic perestroicans (Flyvbjerg 2001, Schram/Caterino 2006, Flyvbjerg/Landman/Schram 2012a), the interpretivists (Rosenthal 2014 [2005], Kleemann/Krähnke/Matuschek 2009, Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a, Münch 2016) or qualitative researchers (Denzin/Lincoln 1994, Creswell 2013 [1998], Flick/von Kardorff/Steinke 2015 [2000], Blatter/Janning/Wagemann 2007) on the other side proves.

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[9] ähnlich prekär wie jede ‚proletaroide' Existenz (Weber 1973e [1919]: 584 [526])

Due to these disputes in revolution mode, there are many misunderstandings that make it almost obligatory to talk past each other. Since the first methodological disputes took place in the German-speaking world in the 19th century, the German word *Methodenstreit* has also found its way into the English language and is still used today: "harking back to the *methodenstreiten*" (Hawkesworth 2006: 152). Other Anglo-Saxon authors also use the German word (Caterino/Schram 2006: 11).

2.1.4 Pluralistic habitus and reductionist practices

Pluralism is a value that hardly any political scientist questions today, even if, like the scientistic and phronetic reductionists, they practice exactly the opposite. In short: the opponents in this methodological war of faith or science constantly display a pluralistic habitus, even if they pursue or implement a reductionist agenda on a daily basis.

The tendency towards a camp mentality is rejected – but only beyond the camp boundaries. Regrets are repeatedly expressed about debates taking place in an either/or mode. This applies not only to the two great debates that the neo-Marxists (Caucus for a new Political Science) started in the 1960s and 1970s and the perestroikans at the beginning of the 21st century, but also to many other debates:

> [T]here is a remarkable penchant for representing the options in "either-or" fashion. Behavioralist or traditionalist, structure or agency, ideas or interests, realist or idealist, rationalist or interpretivist: you simply have to choose, so we are constantly told. On all those dimensions and many others as well, the only proper response is to refuse to choose. Respond insistently, "Both!" (Goodin 2011b [2009]: 10).

Goodin also concedes that the perestroikans also propagate a pluralistic ideology:

> This is also the official ideology, if not always practice, of the Perestroika movement (Goodin 2011b [2009]: 10, footnote 19 with reference to Schram 2003: 837).

However, the eleven-volume series "The Oxford Handbook of Political Science" (Goodin 2011a [2009]), which Goodin supervised as general editor, does not always live up to this pluralistic claim. In particular, the volume "Political Methodology" (Box-Steffensmeier/Brady/Collier, 2010a [2008]), which is most relevant for this study, practices methodological reductionism by presenting only the logical-mathematical methodology in detail. The linguistic-interpretative methodology for interpreting contexts of meaning is completely ignored. Only methodologies that are important for causal reasoning (causal relation or causal mechanism) have found their way into this volume, which aims to provide an overview of the entire methodology of political science. While qualitative-mathematical methods are explained, qualitative-interpretative methods are not, although they exist in abundance and are also frequently used (Denzin/Lincoln 1994, Flick/von Kardorff/Steinke 2015 [2000], Blatter/Janning/Wagemann 2007, Creswell 2013 [1998], Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a).

Flyvbjerg (2001) also justifies his plea for a phronetic, real social science not least by pointing out the difference in principle between natural and social sciences. Furthermore, he does not tire of emphasizing the practical meaninglessness or irrelevance of quantitative methodology, exposing the limits of deductive modes of argumentation, and stressing the superiority of inductive modes of argumentation. Yet at the end of the first part of his book, he suddenly finds the exact opposite:

> To amputate one side in these pairs of phenomena into a dualistic "either-or" is to amputate our understanding. Rather than the "either-or", we should develop a non-dualistic and pluralistic "both-and". Hence, we should not criticize rules, logic, signs, and rationality in themselves. We should criticize only the dominance of these phenomena to the exclusion of others in modern society and in social science. Conversely, it would be equally problematic if rules, logic, signs, and rationality where marginalized by the concrete, by difference, and by the particular. This latter problem, however, is presently far less pressing than the former (Flyvbjerg 2001: 49).

One wonders then why the plea for a phronetic turn or (counter-) revolution, which is supposed to make a real social science possible in the first place. Additional considerations would have done it, as Toulmin (2003 [1958], 2001), to which Flyvbjerg himself refers, has already done.

2.2 Concepts, opponents and points of contention in the science war

- What are the most important concepts, deficits and points of contention in the science war?
- Who are the opponents at the beginning of the 21st century, and which different axiological, epistemic, methodological and ontological presuppositions or approaches do they prefer?

The point here, then, is to explain the philosophical underpinnings and associated controversies, or, as Mark Bevir aptly puts it in the title, to point out the meta-methodological underpinnings, the "underbrush" of any science ("Meta-methodology: Clearing the Underbrush", Bevir 2010 [2008]).

The cultivation of different traditions in different schools even in the discipline of political science proceeds on the basis of axiological, epistemic, methodological, and ontological preferences, as shown not least by the so-called science war (Lauer 2017). Therefore, these philosophical foundations of political science are at the center of the present analysis.

This subsection first explains the main points of contention that characterize this controversy (section 2.2.1). This is followed by an explication of the main concepts, interpretations, and problem formulations (section 2.2.2). The third part contains a comprehensive overview of the main positions of the opponents. The focus is on the Kuhn narrative, which is used in different versions by both parties to reduce complexity (section 2.2.3). This is only a brief overview; the main controversies are dealt with in detail in the main part of the work (chapters three to six).

2.2.1 Emergence of the Science War (*Methodenstreit*)

The *Methodenstreit* began in the late 19th and early 20th century at the philosophical level (axiological, epistemic, methodological, and ontological), especially in Germany (Dilthey 1922 [1883], Rothacker 1926, Rickert 1910 [1896], Windelband 1900 [1894], Weber 1973b [1903-1906], Weber 1973c [1904], Weber 1973g [1906], Weber 1973d [1917], Weber 1973e [1919]).

In the second half of the 20th century, it shifted mainly to the methodical level in a narrower sense, between quantitative and qualitative methods. Scientists rarely indicated in the titles of their methodological books that quantitative-mathematical methods were involved (Box-Steffensmeier/Brady/Collier 2010a [2008], see section 6.9). The qualitative researchers, in contrast, almost always demonstratively set themselves apart from the other side. The titles of their books indicated that they were manuals for qualitative-interpretative research (Flick/von Kardorff/Steinke 2015 [2000], Flick 2008 [2002], Denzin/Lincoln 1994, Creswell 2013 [1998], Blatter/Janning/Wagemann 2007).

At the turn of this century, the science war was shifted by the interpretivists and the perestroikans from the method level to the philosophical level. Therefore, the word “interpretative” is frequently found in the titles of methodological handbooks (Kleemann/Krähnke/Matuschek 2009, Rosenthal 2014 [2005], Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a). Among the phronetic perestroikans one finds a title like “Real Social Science” (Flyvbjerg/Landman/Schram 2012a).

This development came about for two reasons. Firstly, was the realization that quantitative and qualitative-interpretative methods can be used for causal analysis as well as for *meaning* or *sense making*. Secondly, methodological innovations since the 1970s contributed to the establishment of a *qualitative-mathematical research methodology*, as I elaborate here in section 6.9.

Moses and Knutsen make this point primarily about Qualitative Comparative Analysis (QCA), pointing in particular to the work of Ragin and the website www.compasss.org, thus closing the gap between small-N studies and large-N studies. From this they conclude that one can hardly speak of a quantitative and qualitative divide anymore:

> These developments have made it more difficult to refer to a quantitative/qualitative divide in social science (Moses/Knutsen 2012 [2007]: 97).

Also important, in my opinion, are in particular the highly influential handbooks by, as Mr. Perestroika would call them, the East Coast Brahmins of Harvard (King/Keohane/Verba 1994) and the West Coast Brahmins of Berkeley (Brady/Collier

2010 [2004]), which advocate a decidedly causal reductionism and dub the new research methodology *qualitative*, although this research methodology has no linguistic-interpretive or hermeneutic background but is based first on *alethic modal logic* and second on mathematics, specifically *set theory* (section 6.9).

Authors of all traditions, not only scientistic scientists, now quite rightly resist reducing the science war to quantitative and qualitative methods: Flyvbjerg (2006: 56 ff.), for example, also objects to the rejection of quantitative methods, arguing instead for a balance between quantitative and qualitative-interpretative methods.

In my opinion, instead of a qualitative and qualitative schism, it is more accurate to speak of a science war between scientistic scientists on the one hand and interpretivists, including phronetic perestroikans, on the other. The former operate knowledge legitimation for world recognition and world change by searching for invisible causalities by means of a logical-mathematical research methodology. The latter look for world descriptions of (visible) phenomena, interpretation of symbols (text, image, audio and video), especially by means of language, as well as a hermeneutic-interpretative methodology.

2.2.2 Values as points of contention: scientific authority (scientificity) and relevance of political science research

Two overarching questions, which on closer examination consist of a plethora of individual questions and, as I will show, exhibit enormous complexity, justify the division into two camps along philosophical lines and are therefore at the forefront of the science war: The first concerns the methodology that guarantees the scientific authority or scientificity of political science results (section A). The other set of questions is about the relevance of political science or social science research, first, for society and, second, for the science system itself (section B).

A. Methodology and scientific authority (scientificity) or epistemic values

The first set of questions relates to the scientific nature and thus the authority of the subject (these questions are dealt with in detail in chapters five (Epistemology) and six (Methodology)). Which methods, or rather which methodologies, above all, can guarantee the scientific nature (scientificity), but also the unity, of the discipline?

The scientistic scientists attach great importance to methodology, because only then can scientific authority or scientificity be guaranteed. A central goal in the founding of political science was to work on political issues with the authority of a scientist (Goodin 2011b [2009]).

The interpretivists as well as the phronetic perestroikans see in this above all a turning away from concrete political problems and a self-referential scientific system that investigates irrelevant methodological questions in the scientific ivory tower. The critical keywords are: method- versus problem-driven research (Green/Shapiro 1994, Shapiro 2005, Schram 2003 and 2005) and scholasticism (Mead 2010). For the perestroikans, the next point, the practical relevance, is the main focus of interest. Methodological questions are disparaged.

B. Relevance of political science research or non-epistemic values

Above all, two non-epistemic values are the subject of the dispute. On the one hand, it is about the relevance of political science research to day-to-day political business; and secondly, about the methodological innovative power of the discipline. While the relevance of a subject in the public is based in particular on the *results*, i.e., the *content of the subject* or the knowledge that it creates, the reputation of a discipline within the scientific system is based on its *methodological innovation power*. Axiological questions are dealt with in detail in chapter four, Axiology.

a. Political and public relevance of political science

The relationship between political science and practical politics is an important complex of questions; more precisely, the issue is the relevance of political science research for daily political business. How can practicable solutions for existing political problems, especially for disadvantages facing groups and people, be generated? How can one guarantee that the suggestions made in the subject will be heard, especially in times of scientification? How can the independence of the discipline be guaranteed with respect to clients (state, civil society, economy)?

With regard to practical research, the scientistic establishment prefers an *applied*, technical methodology, more precisely a normative rational choice theory (Hardin 2011 [2009]), whereas the phronetic perestroikans want to revolutionize the social sciences through an *applied phronesis* (Flyvbjerg/Landman/Schram 2012) and contribute to making them more publicly relevant again. The titles “Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again” (Flyvbjerg 2001) and “Real Social Science. Applied Phronesis” (Flyvbjerg/Landman/Schram 2012) reveal the program.

The central goal of the perestroikans is to profile political science in public debate:

> Working to ensure that political science gets to play its part in the broader field of political struggle is still an effort well worth repeating (Schram 2015: 428).

b. Methodological relevance within the science system

The relevance of a discipline in the interplay of all sciences is given by the fact that methodologies developed in political science have been adopted from other disciplines. In other words: the methodological innovative strength of a discipline mainly determines the reputation and relevance of the discipline within the scientific system.

> Since political science brought neither its own theory nor its own method and since its limits were controversial, its efforts to create partial theories, in view of the excessive

> theoretical claims that have always been the dark side of the Humboldt University, made little impression on the neighboring disciplines[10] (von Beyme 2016: 51; my translation).

This judgment was formulated with regard to the situation of political science in Germany after the Second World War; it should also be applicable to the development of American political science. In particular, the methodology of the discipline was adopted from other sciences. In the U.S., the issue of empirical research had and still has two opponents: on the one hand, the mainstream (which, as we shall see, is actually the minority of the scientistic establishment), which prefers and supports causal thinking and a logical-mathematical research methodology, and generally an orientation towards the natural sciences as well as an orientation towards economics; and on the other hand, at the beginning of the 21st century, the so-called perestroikans, with a linguistic-interpretative research methodology and a greater orientation towards the humanities.

If methodological disputes are now dismissed as "controversy", as quite a few in political science do, then the discipline is in trouble, because the scientific system is a meritocratic system where aristocratic claims do not hold water. What counts is the *reconstruction, further development of existing methodologies or the innovation of new processes that advance science*, and not just any outdated claims. It is therefore only logical that all political scientists have given up the "attitude of the philosopher king"[11] (Wildemann 1967: 21, quoted from von Beyme 2016: 49; my translation).

With regard to the relevance of the discipline, scientistic scientists and perestroikans have different emphases, which, as I will show (chapter 3-6), do not contradict each other. On the contrary, these can be pursued in a *complementary* fashion when the exaggerations on both sides are put aside.

2.2.3 The opponents in the science war

Now the two opponents in the science war within (Anglo-Saxon) political science shall be briefly introduced, followed by a detailed discussion (chapters three to six). On one side stand the causal reductionists, naturalists, (neo-) positivists, scientific scientists, and the disciplined political scientists who are oriented towards the natural sciences and advocate a methodological, especially causal and empirical reductionism (section A). For the other side, a limitation must be made within the Aristotelian tradition. Only those perestroikans are considered who are oriented in particular to the human and

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[10] Da die Politikwissenschaft weder eine eigene Theorie noch eine eigene Methode mitbrachte und ihre Grenzen umstritten waren, hat ihr Bemühen um Partialtheorien angesichts des überzogenen Theorieanspruches, der die Schattenseite der Humboldtschen Universität schon immer gewesen ist, auf die Nachbardisziplinen wenig Eindruck gemacht (von Beyme 2016: 51).

[11] Attitüde des Philosophenkönigtums (Wildemann 1967: 21, zitiert nach von Beyme 2016: 49)

cultural sciences (humanities), accept descriptions of meanings and contexts of meaning in addition to causal explanations, and prefer a decidedly problem-oriented rather than method-oriented approach. In this way, they hope to solve the public relevance problem (section B).

The science war between these two opponents is mainly conducted in the field of philosophical foundations, has existed since the emergence of the social sciences in the 19th century, has lasted to this day, and has repeatedly come to a head every few decades.

A. The explanative-prognostic or the or the Platonic-Galilean tradition within political science: methodological, causal and empirical reductionism, logical-mathematical research methodology

a. The philosophical foundations of the explanative-prognostic or the Platonic-Galilean tradition

The philosophical or epistemological foundations of the explanative-prognostic or the Platonic-Galilean tradition were laid by the following thinkers in the 17th century: Galileo Galilei (1564-1641), Francis Bacon (1561-1626), René Descartes (1596-1650), Thomas Hobbes (1588-1679) and John Locke (1632-1704). Some important foundations go back to Plato (von Wright 1971: 2; see also note 5, p. 170).

Francis Bacon replaced the Aristotelian Organon with his *Novum Organon* (Bacon 1990 [1620]); in contrast to Bacon, Aristotle pursued a pluralistic rather than a reductionist methodology. Galileo introduced the *experiment* as a scientific tool, Descartes praised mathematics (*more geometrico*, Descartes 2001 [1637] and Descartes 1994 [1641]) both as the most important scientific tool and as a model: he was the first to justify the mathematical ideal of exactness or *mathematics* as a methodological model within the sciences.

Epistemological reductionists search for an Archimedean point, more precisely for an *absolute* foundation for knowledge or for science. The search for *conditions of knowledge* (Wieland 1999b [1982], Lehrer 1990, Enskat 2005, Lauer 2017) following Plato's dialogue *Theaetetus* (Plato 1983c [4th century BC]) has been extremely influential to this day.

Descartes' *ego cogito, ergo sum*, and in my opinion even Popper's criterion of demarcation between science and pseudoscience, reflects the search for an unshakable foundation (*fundamentum inconcussum*, Descartes 1994 [1641]). Although Popper criticizes *certism* (Spinner 1974 and 1978) and thus the search for a foundation, he speaks of the fact that the problem of demarcation is "the fundamental problem of epistemology" (Popper 2010 [1979]: 422), to which his *fallibilism* would have a solution. Since logical-mathematical models (Braun/Saam 2015) are also used in the social sciences, and since a *rational choice revolution* (Goodin 2011b [2009]: 13) has even been diagnosed since the 1970s, the term explanative-prognostic or the *Platonic-Galilean tradition* is justified in my opinion.

These are the philosophical foundations that enabled the stupendous rise of the natural sciences, especially physics. At least since the 19th century, physics has been regarded by many as the exemplary discipline for all methodological considerations. This has had enormous implications for the social sciences that emerged in this century. Many social scientists argued for an orientation toward the natural sciences, especially physics. Statistics, especially correlation and regression analyses, the foundations of which were laid in the 19th century, are held in high esteem; language and especially hermeneutics are placed under general suspicion. The effects on political science, which only emerged in the 20th century, are still enormous today.

The group of political scientists within the United States, which I believe belongs to this tradition, values being "scientists", or more precisely "disciplined political scientists". Their opponents call them "(neo-) positivists", "scientistic scientists", or "naturalists". They are scientistic scientists who practice art for the sake of art, a "model Platonism" (Albert 1967c [1965]) or "hypermethodologism" (Bevir 2010 [2008]: 69), and their research is "method-driven" (Shapiro 2005). They are even accused of being frozen in "scholasticism" (Mead 2010). These are some of the prejudices listed against them.

What is the disciplinarity and exactness within political science? Goodin, the general editor of the eleven-volume series "The Oxford Handbook of Political Science", states the following in his summary of the series with the telling title "The State of the Discipline, the Discipline of the State":

> Still even that small sample suffices – to my mind, at least – to illustrate both the unity and the diversity of contemporary political science [...].
> What made all this progress possible, I submit, is not any loosening of the discipline of political science. Rather, that progress is attributable to the strength of the discipline's discipline [...].
> The discipline is a pluralist one, but the plurality is contained within and disciplined by a discipline (Goodin 2011b [2009]: 32).

Although Goodin does not say so explicitly, in my view the 10th volume of this series, "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]), guarantees "the discipline's discipline", or, in other words, political science methodology legitimizes the discipline's scientificity. This volume was produced by key contributors to the discipline and contains an outstanding overview of the methodology of causal and empirical reductionism or causal thinking consisting of deductive and inductive or epagogic modes of reasoning, quantitative-mathematical and qualitative-mathematical methods, and empirical and normative methodological approaches within political science at the beginning of the 21st century. This volume is (another assertion on my part) the updated version of Bacon's *Novum Organon* (1990 [1620]), not only for political science, but for all the social sciences. This is true even though the name "Francis Bacon" does not even appear in the handbook's index of persons. Therefore, the contributions to this volume can be considered representative of the current methodological beliefs of the explanative-prognostic or the Platonic-Galilean tradition, and thus of the scientistic scientists.

I prefer the concept "scientistic scientists", because it places the relevance of methodology at the center. The designation "positivists", which dominates in English-language books, is very misleading. Primarily by terminology, theses are imputed to these researchers that are clearly positions of logical empiricism. Such positions, however, are only held by a few scientists today. The term "naturalism" also leads to misunderstandings, because it implies that scientists do not examine the role of actors from the outset or that they assume that political systems are given by nature and are not constructed.

Now some general remarks on the explanative-prognostic or the Platonic-Galilean tradition. *Causality*, or better causal relation and causal mechanism, which is generally believed to be invisible, i.e. *not directly observable* but only ascertainable by *causal inference*, is the ontological presupposition *par excellence*. Causality is seen as that which holds the world together at its core[12], or the "cement of the universe" (Mackie 1974). Those who identify causal relations or causal mechanisms can recognize and change the world. Both are possible only because, as Bacon states in an aphorism (Bacon 1990 [1620]: 80, 3rd aphorism, subvolume 1), there is an *equivalence* between *causality* and *action*. Only on this condition can one transform *cognition* (theory) into *action* (practice), i.e. into *social technology*, by "inversion of causal propositions" (Weber 1973d [1917]: 529 [491]) or by "inversion of the fundamental explanatory scheme" (Popper 1984 [1972]: 367). Thus, instructions or advice can be formulated quasi incidentally as part of an arguably *applied* (not practical) *political science*. The ethical-normative as well as the pragmatic dimensions are not addressed at all, as has been done in practical philosophy or political philosophy since antiquity. Only a halved, *instrumental reason* (Horkheimer 1967 [1947]) is at work here.

Within this tradition, only causal relations and mechanisms between events are sought. Causality is the only relation that counts, while other relations or contexts are of no interest. Hence my concept of *causal reductionism* is justified.

While both 19th century positivism and Marxism were still searching for social laws (sociophysics), since the 20th century postpositivists within political science have been focusing on causal *regularities* and *generalizations* and, above all, something that the phronetic perestroikans and the interpretivists overlook, on concrete, causal

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[12] That I recognize what holds the world / Together at its core.
Daß ich erkenne, was die Welt / Im Innersten zusammenhält
(von Goethe, 1978 [1808]: 162 [382-383]).

mechanisms. This has mainly been the case since the 1970s, after qualitative-mathematical methods were introduced (section 6.9) and further developed. From the phenomenal world, the causal reductionists are only interested in data-set observations (DSOs) and causal-process observations (CPOs). Both are needed for causal inferences, the DSOs to determine causal regularities at the *macro level* and the CPOs to identify causal processes at the *micro level* (Brady/Collier 2010 [2004]).

The project of a *sociophysics* or a social physics, prominently advocated in the 19th century and forgotten or shelved in the 20th century, is also back on the agenda, albeit not yet in political science but in sociology (Wagner 2012).

Thus, in the focus of the explanative-prognostic or the Platonic-Galilean tradition within political science are *empirical causal analyses*, which since the 1950s have been produced with the help of *quantitative tools* (concepts, methods and methodological approaches) and *deductive* and *inductive modes of argumentation*. Since the 1970s, logical-mathematical model analyses have been added, in political science mainly *rational-choice models*, since the 1970s *qualitative-mathematical tools*, and since the 1990s *experiments* (but in political science, in contrast to sociology, hardly any *simulations*).

Methodological individualism, another fundamental characteristic of this tradition, goes back to Hobbes (1996 [1651]), but especially to Weber (Weber 1980 [1922]) and von Hayek (2004 [1943]), and is preferred to *holism* in the explanative-prognostic tradition (for a very influential critique of holism, see Popper 1980a [1944], 1980b [1944] as well as 2003 [1957]).

Since *liberal* (Locke 1989 [1690]) and *utilitarian categories* (Mill 1998 [1861]) were added, this *Great Revolution* within methodology can be called liberal. Rudolph speaks of “Lockean liberalism’s universalism” (Rudolph 2005b). Because political scientists in this tradition also apply liberal categories to developing countries, she diagnoses an “imperialism of categories” (Rudolph 2005a).

Liberalism, universalism as normative assumptions on the one hand and causal and empirical reductionism, methodological individualism and model analyses as axiological, epistemic, methodological and ontological assumptions on the other hand are the *hidden assumptions* that usually flow into empirical research without being reflected. Researchers, if they do not explicitly mention and consider these assumptions, try to pretend to objectivity with a logical-mathematical research methodology, but this objectivity does not exist on closer examination. To paraphrase Habermas (1968c), it is the epistemological interests (liberalism, universalism), or rather the philosophical assumptions of the liberal establishment, that can counteract the objectivity of the results (but do not necessarily have to influence them, as Habermas claims), unless they are addressed and thus neutralized (section 3.1).

b. Five research programs within the explanative-prognostic or the Platonic-Galilean tradition instead of five “revolutions” within one “Great Revolution”

The term “methodological reductionism” is appropriate because in the explanative-prognostic tradition only the methodology with the help of which one can identify invisible causalities is considered; other methodologies are ignored. However, a methodological rigidity or scholasticism (Mead 2010) within this tradition cannot be determined; on the contrary, several *methodological research programs* within this part of the discipline have been established by this tradition; Goodin speaks of several *revolutions* within political science.

According to Goodin (2011b [2009]: 13), there were *three revolutions* within American political science. The *first revolution* took place at the beginning of the 20th century and led to the establishment of political science as a science, oriented toward the natural sciences, especially through the introduction of causal and empirical thinking, deductive and inductive modes of argumentation, and the separation between *is* and *ought*. In addition, there was the introduction of professional and systematic procedures and the establishment of the subject at American universities at the beginning of the 20th century.

In the 1950s, the *second, behavioral revolution* took place, methodologically in particular with the introduction of quantitative tools (quantitative-mathematical terms as well as such methods and methodical approaches):

> Behavioralism as a method-conscious individualistic approach with exact methods has become the rallying movement in America for all directions working with quantitative methods[13] (von Beyme 2000 [1972]: 111; my translation).

The preferences for methodological individualism and the pejorative rejection of any kind of holism also stretch back to this period.

The *third* revolution, the *rational choice revolution*, finally took place from the 1970s onwards and introduced work with logical-mathematical models. Political science absorbed developments in economics, and so immediately concentrated on a very specific model, namely the rational choice model. However, sociology also works with other models (Braun/Saam 2015).

In my opinion, as I will yet demonstrate, there are two more “revolutions”, better rather methodological innovations, which led to the introduction of the following methodological *research programs*: firstly, the *qualitative-mathematical research program*, through the introduction of qualitative-mathematical methods from the

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[13] Der Behavioralismus als eine methodenbewußte individualistische Vorgehensweise mit exakten Methoden ist in Amerika zur Sammelbewegung für alle Richtungen geworden, die mit quantitativen Methoden arbeiten (von Beyme 2000 [1972]: 111).

1970s, and secondly, the *experimental research program*, through the introduction of experiments (in contrast to sociology, *simulations* are rarely used in political science) from the 1990s. Here it is particularly clear that it is better to speak of *innovations* than of *revolutions* (more in section 6.9). None of the innovations listed above have been *superseded*, but all of them cumulatively form, in my opinion, the methodology of the explanative-prognostic tradition. These five methodological research programs, not "revolutions", all occurred within a liberal "Great Revolution", explanative-prognostic or the Platonic-Galilean tradition, the foundation of which, as briefly described above, was laid primarily in the 17th century and which was enforced in American political science from the beginning of the 20th century by the disciplined political scientists, the scientistic establishment (overview in chart 6, section 9.4.6).

c. Spread of causal thinking within political science: establishment instead of mainstream

Quantitative analyses show that it is not the mainstream, i.e. the majority of political scientists, who use quantitative methods, as claimed by the perestroikans (and not just by them). Since the 1950s, only a minority of political scientists have used these methods. Therefore, one can legitimately use the term "establishment". Nevertheless, there are far more than a few "East Coast Brahmins" (Mr. Perestroika 2005 [2000]: 9) who favor causal thinking using quantitative mathematical research methodology and, according to the perestroikans, also dominate the American Political Science Association (APSA) (Monroe 2005):

> However monolithic the US discipline may seem from a distance, those working within it know full well that it is internally highly diverse. From a distance, the US discipline may seem to be dominated by some hegemonic practice – "behavioralism" in the previous generation or "rational choice" in the present one. But in fact, those supposedly "hegemonic" practices are actually practiced to any high degree by only perhaps 5 % of the US discipline, even in many top departments (Goodin et al. 2007, 9) (Goodin 2011b [2009]: 13).

The figure of only 5% of quantitative researchers in the U.S., especially behavioralists and rational choice theorists, working almost exclusively with quantitative or qualitative mathematical methods is very surprising, as is the small number of articles in which causal reasoning is assumed or has been demonstrated. In the APSR (American Political Science Review), although the number increased to about 35% and has been constant since the 1970s, it was still below 20% for all articles in JSTOR (Journal Storage) in the 1990s (Box-Steffensmeier/Brady/Collier 2010b [2008]: 4; see also Brady/Collier/Box-Steffensmeier 2011 [2009]: 1006, 1022, and 1025). If one then considers that quantitative researchers prefer to publish their results in the form of articles, while qualitative-interpretative researchers tend to publish their results in book form (book publications were not analyzed in the paper cited above), one can see that quantitative-mathematical research is far from constituting the majority of political science publications. Based on the following research, it is justified and even warranted to speak of the “establishment” instead of the “mainstream”.

Furthermore, there is a gap between causal and linguistic-interpretative studies, with the latter only increasing in the 1980s. This, *nota bene*, is only the case within the most important American publications:

> There is clearly a “causal dimension” which applies to about one-third of the articles and an “interpretative” dimension which applies to roughly 6 percent of the articles. Although we expected this two-dimensional structure, we were somewhat surprised to find that the word “explanation” was almost entirely connected with “causal or causality” and with “hypothesis”. And we were surprised that the two dimensions were completely distinctive since they are essentially uncorrelated at 0.077. Moreover, in a separate analysis, we found that whereas the increase in “causal thinking” occurred around 1960 or maybe even 1950 in political science (see Figure 48.1), the rise in the use of the term “narrative” and “interpretative” came in 1980” (Brady/Collier/Box-Steffensmeier 2011 [2009]: 1036).

d. Pluralism within the explanative-prognostic or the Platonic-Galilean tradition: diversity of methods, but no pluralism of methodologies

Political scientists within the explanative-prognostic tradition see themselves as adherents of pluralism, in two main respects: they represent a democratic and liberal pluralism within political theory on the one hand and, on the other hand, within methodology. On closer examination, however, the latter is based on the fact that different methods and methodical approaches are used for the empirical identification of invisible causalities. Only the empirical provability of causal relations or mechanism is in the focus. Interpretive and meaningful contexts as well as a phenomenology of the visible do not play a role. Explanations of *causal relation and causal mechanism* and not *descriptions of contexts* of *meaning or appearances* are the goal.

As Farr rightly points out, this is a *variant* of pluralism in its own right; there are indeed a variety of methods and modes of argumentation:

> The behavioral revolution blazed on to the science with its own variant of methodological pluralism, too. [...]
> Behavioral observations, experimentation, statistical methods, game theory, and systems analyses were all “in”, without much skepticism that they did not all “fit” together very well and that no political scientist could embrace them all (Farr 2015: 416).

But the common thread is always causal reductionism, although this is not explicitly stated here either. It is, in my view, justified to speak of *methodological reductionism*, not only because of empirical and causal reductionism, but also because only logical-mathematical modes of argumentation, quantitative-mathematical as well as qualitative-mathematical methods are applied (section 6.9). This is true even though the Oxford series aims to provide an overview of the entire discipline of political science:

> The Oxford Handbooks of Political Science is a ten-volume set of reference books offering authoritative and engaging critical overviews (Goodin 2011a [2009]: ii).

What is more, the general editor also acknowledges the diversity of political science research (Goodin 2011b [2009]: 32).

Although *qualitative methods* are also presented in this handbook, they have only their name in common with the methods used by interpretivists and perestroikans (section 6.9). Within qualitative research methodology, the focus is on identifying contexts of meaning and interpretations of symbols (sense making, meaning making). This is done with the help of qualitative-interpretive tools (concepts, methods and methodological approaches), such as those discussed in many handbooks on qualitative or interpretive research (Flick/von Kardorff/Steinke 2015 [2000], Blatter/Janning/Wagemann 2007, Denzin/Lincoln 1994, Creswell 2013 [1998], Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a).

In the handbooks "Designing Social Inquiry. Scientific Inference in Qualitative Research" (King/Keohane/Verba 1994) or "Rethinking Social Inquiry. Diverse Tools, Shared Standards" (Brady/Collier 2010 [2004]), very influential especially in the English-speaking world, the presented qualitative methodology is solely for determining *causal relations or causal mechanisms*; since this research is oriented toward quantitative research, I call it *qualitative-mathematical research* or methods. Interpretivists also speak of *qualitative-positivist* methods (Schwartz-Shea 2014 [2006]: 143, footnote 6). Qualitative-interpretivists researchers demarcate themselves, sometimes exaggeratedly, from quantitative-mathematical research methodology and they use a hermeneutic-interpretative methodology (Flick/von Kardorff/Steinke 2015 [2000], Denzin/Lincoln 1994, Creswell 2013 [1998], Yanow/Schwartz-Shea 2014 [2006], and Bevir/Rhodes 2016a).

In contrast, the *content* generated by "undisciplined theorists" is also discussed in detail in the corresponding manuals of the Oxford series. Only the methodology with which these theorists work falls by the wayside. It is, unfortunately, *completely ignored.* In the 10th volume of "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]) only the logical-mathematical research methodology is discussed in detail. This is, in my opinion, the biggest omission of this series.

e. The scientistic narrative: axiological, epistemological, methodological and ontological assumptions of the explanative-prognostic or the Platonic-Galilean tradition

In the 3rd chart (section 9.4.3), a *reduction of the complexity* of the scientistic narrative was undertaken by citing the main axiological, epistemic, methodological, normative and ontological assumptions of the explanative-prognostic tradition that shape this tradition at the beginning of the 21st century. These are the main assumptions of scholars who practice political science as a social science, align themselves with the natural sciences, form the scientistic establishment, have carried out a liberal-scientistic "revolution" in the social sciences, and quite rightly defend their great achievements, but unfortunately in a revolutionary tone. Even when scientists formulate their own position, important premises are missing. Apparently, it is always assumed that these assumptions are implicit and known by all. Therefore, the *scientistic narrative* is summarized *ad fontes* in this chart elaborated from major sources, especially *handbooks* (Salmon 1989, Salmon 1992, King/Keohane/Verba 1994, Brady/Collier 2010 [2004], Box-Steffensmeier/Brady/Collier 2010a [2008], Moses/Knutsen 2012 [2007]) and supplemented with assumptions from classicists (Bacon 1990 [1620], Popper 1984 [1972], Weber 1973d [1917]).

Two limitations must be taken into account in this very general overview. First, it is a *static snapshot*; neither the *dynamics* of development nor even the *historical* development in all its ramifications can be documented. Secondly, these are *ideal-typical assumptions* for all scientistic scientists. In practice, however, there are different opinions about different assumptions, sometimes even contradictory opinions. Even within the explanative-prognostic tradition there is a *multitude of controversies* in the philosophy of science (Kincaid/Van Bouwel 2023, Humphreys 2016).

A thorough examination of these assumptions, as well as their limitations and possibilities, will be undertaken here in chapters three through six. The purpose of this chapter is first to present an overview of the scientistic narrative. I will then return to the same narrative in the next subsection, but as the phronetic perestroikans reconstruct it.

B. The phronetic perestroikans within the Aristotelian tradition: qualitative-interpretive research methodology and applied phronesis

a. The Aristotelian tradition using the example of the phronetic perestroikans

And now for the happily undisciplined theorists within political science who criticize the liberal establishment. Criticism of the explanative-prognostic or the Platonic-Galilean tradition has existed since the emergence of the social sciences in the 19th century, with recourse to the Aristotelian tradition and with reference to the human and cultural sciences (*Geistes- und Kulturwissenschaften*), which, according to these representatives, differ in principle from the natural sciences (Dilthey 1922 [1883], Rothacker 1926, Rickert 1910 [1896], Bodammer 1987). In the U.S., the human or cultural sciences are known as “Humanities”. Those who work in this field are denied the status of scientist. Those in the social sciences who are located in the explanative-prognostic or the Platonic-Galilean tradition wish to reserve the term political scientists exclusively for themselves; the others are deemed at most political theorists. Pejoratively, the term *Feuilletonist* (columnist, feature writer) is also used by scientistic scientists in Germany. Conversely, the interpretivists call the scientists rationalistic, insensitive “fly-leg counters” (*Fliegenbeinzähler*) who, in the spirit of the natural sciences, want to explore social reality with crude measurement methods.

Methodological reductionism, or the liberal establishment, is very homogeneous both methodologically (causal reductionism, quantitative-metric tools, rational choice models, experiments) and normatively (favoring liberalism and utilitarianism), while the critics are very heterogeneous both methodologically and normatively:

> [T]he constructivist camp covers much territory, and as a consequence it may house a more heterogeneous group of fellow travelers than the naturalist camp (Moses/Knutsen 2019 [2007]: 196).

An overview of the state of research at the beginning of the 21st century on neopositivist methodology, as described above, is summarized in "Political Methodology" (Box-Steffensmeier/Brady/Collier, 2010a [2008]). In the Aristotelian tradition, which advocates a pluralistic methodology, things are quite different because of the heterogeneity of positions. Both the descriptive and the practical approaches as well as the axiological and normative positions are heterogeneous (Flick/von Kardorff/Steinke 2015 [2000], Denzin/Lincoln 1994, Creswell 2013 [1998], Yanow/Schwartz-Shea 2014 [2006] and Bevir/Rhodes 2016a). The volume "The Argumentative Turn in Policy Analysis and Planning" by Frank Fischer and John Forester (Fischer/Forester 1993a) is also methodologically important. The *argumentative turn* is also part of the inventory of an interpretive science.

For decades, causal analyses were performed exclusively using quantitative methods based on statistics and probability theory. Quantitative research is still associated with causal analyses today, especially in Europe (Schmitz/Schubert 2006a), while questions of interpretation and meaning are associated with qualitative research. Since the 1970s, causal analyses have also been carried out under the label "qualitative research", especially in the U.S. An overview of everything that runs under this label can be found in the four-volume compendium "Qualitative Research in Political Science. Backgrounds, Pathways and Directions in Qualitative Methodology. Volume I" (Blatter/Haverland/van Hulst 2016a), "Qualitative Research in Political Science. Causal Regularities, Cross-Case Comparisons, Configurations. Volume II" (Blatter/Haverland/van Hulst 2016b), "Qualitative Research in Political Science. Mechanism, Temporality and Within-Case Analysis. Volume III" (Blatter/Haverland/van Hulst 2016c) and "Qualitative Research in Political Science. Interpretive and Constructivist Approaches. Volume IV" (Blatter/Haverland/van Hulst 2016d). This four-volume compendium contains the 62 most important articles or chapters, ranked by editor, from widely used methodology books on this subject since the 1970s.

The first volume deals with the fundamentals of scientific theory. The second and third volumes deal with causal case analyses and consider qualitative-mathematical or qualitative-positivistic methods for identifying causalities and causal processes at the micro level. They contain excerpts from books and articles that contributed to the launch of a qualitative mathematical research program (section 6.9). Tellingly, the subtitle is “The Revolt in the US. Alternatives to the Statistical Template” (Blatter/Haverland/van Hulst 2016a: 1-101). The last volume discusses the European tradition of interpretative and constructivist research: “Building on and Defending European Traditions: From Understanding to Practices and Interpretation” (Blatter/Haverland/van Hulst 2016a: 103-166).

The perestroikan movement at the beginning of the 21st century also belongs to the latter, very heterogeneous tradition (Mr. Perestroika 2005 [2000], Schram 2003, Monroe 2005). In addition to the scientistic scientists, it is the focus of my considerations, since the dispute over methods will be described using the American situation as an example. It is also important that, like my outline of a practical political science (Lauer 1997), it is a methodological critique from a practical perspective. This is one of the reasons why my choice among the interpretivists fell on the phronetic perestroikans.

In 2000, an e-mail was sent to academics that formulated an extremely brief but very polemical methodological and organizational critique of the “mainstream”, meaning, as shown above, the establishment within American political science. The mail is anonymous, signed by a “Mr. Perestroika” (Mr. Perestroika 2005 [2000]); the author(s) remain(s) unknown to this day. The corresponding perestroika program is described by Schram in an essay, “Return to Politics. Perestroika and Postparadigmatic Political Science” (Schram 2003). In it, Schram refers, among other things, to a volume by Flyvbjerg, in which he had already called for a revolution: “Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again” (Flyvbjerg 2001). Together with other authors, both advocate a revolution towards a “Real Social Science. Applied Phronesis”. This is the title of another volume in which this methodology is presented using examples (Flyvbjerg/Landman/Schram 2012a, Schram/Caterino 2006). This movement differs terminologically from the interpretivists: they not only want a *turn* to a descriptive-interpretative political science, but to a phronetic and genuine (real) political science or social science, although they share many philosophical assumptions with the interpretivists and also expressly refer to interpretive research.

Schram summarizes the creative use of various philosophical positions from Aristotle to Foucault as follows:

> Flyvbjerg’s book is such a breath of fresh air; he creatively uses Aristotle, Nietzsche, Foucault, Bourdieu, and others to make many of the same points as Toulmin, but in his own distinctive way. He fuses an Aristotelian concern for phronesis with a Marxist concern for praxis, adding a Foucauldian critique of Habermas’s preoccupation with consensus to demonstrate that a phronetic social science that can offer a praxis worth pursuing is one that would work within any contextualized setting to challenge power, especially as it is articulated in discourse. Flyvbjerg’s phronetic social science would be open to using a project of research methods to help people challenge power more effectively (Schram 2006: 27).

The heterogeneity of the critics is not only due to the methodology, but also due to the goals that scientific work must achieve: how empirical knowledge of the world is determined, and how world changes are justified.

b. World recognition, world interpretation, especially the recognition of political reality based on tension points

The opponents of the naturalists or scientistic scientists, regardless of whether they are called “constructivists” or “interpretivists”, i.e. all qualitative researchers who feel they belong to the humanities, define themselves in contrast to the social scientists who are oriented towards the natural sciences:

> One of the most commonly held family features in the constructivist camp is a deep skepticism of the naturalist approach to social science. This takes aim at the core ontological, epistemological and methodological claims of the naturalist tradition. As this skepticism is broadly shared, residents of the constructivist camp might be construed as a collective self by virtue of their common opposition to a naturalist order (Moses/Knutsen 2019 [2007]: 197).

While the methodological reductionists only want to identify causal relations between events, causal analyses play a rather marginal role for the interpretivists. Their focus is primarily on connections of *sense making* (meaning making, context of meaning), about descriptions, about understanding, and less about explanations; Structures, meanings and networks are at stake. *Descriptions* should capture *meanings* and *contexts* of meaning. There is an emphasis on text analysis, with images, photos, audio and videos treated like text. *Text analyses* are primarily carried out using reconstructive methods, abductive and inductive argumentation methods, and qualitative-interpretative tools (concepts, methods and methodological approaches) (Flick/von Kardorff/Steinke 2015 [2000], Creswell 2013 [1998], Yanow/Schwartz-Shea 2014 [2006] and Bevir/Rhodes 2016a).

There are also *descriptions of visible phenomena* (appearances); only when one can describe the visible, the events, can one then proceed to explain the invisible (causality). Von Wright speaks of the fact that one must first *understand* something before one can *explain* it (von Wright 1971). Phenomenological descriptions and causal explanations are *complementary* to each other.

In addition, within this tradition the focus of investigations is not only on the political phenomena but also on the *researcher* himself, and thus above all on epistemological questions:

> Thus the focus of their inquiry is just as often the inquirer (and her context) as it is the particular object of inquiry – because it is here that the roots of these patterns lie buried (Moses/Knutsen 2019 [2007]: 200).

Many of these questions of the pluralistic (Aristotelian) tradition are not in the focus of the scientific interest of the perestroikans. At least in the first publications, they did not even bother with world recognition but wanted to start a revolutionary world change right away. Only the last volume (Flyvbjerg/Landman/Schram 2012a) includes a search for *tension points* that then need to be changed. Identifying these tension points should therefore become the central goal of empirical research for the perestroika researchers.

c. Changing the world, specifically changing political reality by means of applied phronesis

The heterogeneity is also reflected in the political content. The entire breadth of the political spectrum is represented; in particular left and conservative, but also liberal positions can be found within the Aristotelian tradition. Here too, as with the scientistic scientists, methodology and political philosophy often go hand in hand.

Criticism of methodological reductionism came first from left-wing movements such as the Frankfurt School. The Caucus for a New Political Science within American political science in the 1960s was also shaped primarily by neo-Marxism. This was followed by criticism from hermeneutics and phenomenologists within the human and cultural sciences, from the philosophy of language, from American pragmatism, from French (post-) structuralism, and finally from the perestroika movement at the beginning of the 21st century.

In addition to the preference for linguistic-interpretative research methodology, the Aristotelian tradition is united by the belief in the priority of practice. Everyone emphasizes priority of practice over theory. As I will show, the explanative-prognostic or the Platonic-Galilean tradition is partly wrongly accused of a lack of practicability and useless theorizing. The perestroikans already point out this alleged shortcoming in the titles of their works: “Making Social Science Matter” (Flyvbjerg 2001), “Return to Politics” (Schram 2005), “Real Social Science. Applied Phronesis” (Flyvbjerg/ Landman/ Schram 2012a).

But they do not limit their criticism to the quantitative methodology; they also focus on the ideological-normative as well as the axiological, epistemic, methodological and ontological basic positions represented by the liberal establishment. Gunnell summarizes the position of the perestroikans as follows:

> Although the critics were philosophically and ideologically diverse, they coalesced around their mutual opposition to the basic values embedded in American political science such as liberalism. individualism, interest-group pluralism, scientificism and unity of science, the logical disjunction between fact and value, and pragmatic relativism, which were all, in various respects and degrees, elements of what had become a general vision of historical progress in both politics and political inquiry (Gunnell 2015a: 410-411).

Here, too, only the most important differences are listed, and only in a very reduced form. In order to clear up the whole range of differences and misunderstandings, I will explain both positions again in detail in chapters three to six.

d. The perestroikans’ view of the explanative-prognostic or the Platonic-Galilean tradition

The methodological efforts of the explanative-prognostic tradition are summarized in very abridged form and misleadingly by the perestroikans and are reproduced as follows. Knowledge is generated in a cumulative process, where knowledge consists of explanations, predictions and context-free theories:

> In addition to the characteristics mentioned, there are also the pursuit of truth and objectivity, universal generalizations and falsifiable causal hypotheses, all of which are determined using large-N studies. The causal knowledge thus generated is then made available to society for practical change based on context-independent theories (Flyvbjerg 2001: 26).

In addition to the characteristics mentioned above, there are also the pursuit of truth and objectivity, universal generalizations and falsifiable causal hypotheses, all of which are determined using large-N studies. The causal knowledge thus generated is then made available to society for practical change:

> From the vantage point of many Perestroikans, the dominant paradigm in the field operates to the following hierarchy of assumptions: (1) political science exists to help promote understanding of the truth about politics; (2) political science research contributes to this quest by adding to the accumulation of an expanding base of objective knowledge about politics; (3) the growth of this knowledge base is contingent upon building of theory that offers explanations of politics; (4) the building of theory is depending on universal generalizations regarding the behavior of political actors; (5) the development of a growing body of generalizations occurs by testing falsifiable, causal hypotheses that demonstrate their success in making predictions; (6) the accumulation of a growing body of predictions about political behavior comes from the study of variables in samples involving large numbers of cases; (7) this growing body of objective, causal knowledge can be put in service of society, particularly by influencing public policy makers and the stewards of the state (Schram 2003: 836, Flyvbjerg/Landman/Schram 2012a).

These summaries of the explanative-prognostic or the Platonic-Galilean tradition by Flyvbjerg, Landman and Schram, certainly intended as ideal (similar views can be found in many other authors as well), do not provide an adequate reduction of complexity. On the contrary, they contribute significantly to the many misunderstandings, because central aspects of naturalistic or scientistic methodology do not come into focus at all, and if they do, they are often simply wrong or outdated. Not least for this reason, the methodological differences will be elaborated on ten levels in the chapter three to six. In the third chart, I have compiled the liberal-scientistic narrative and its axiological, epistemic, methodological, and ontological assumptions (3rd chart, 9.4.3).

These paraphrase is a positivist or naturalistic straw man. Similar narratives are also first put forth by other interpretivists to show the inadequacy of the positivist tradition and justify a shift or *turn* towards an interpretive paradigm (Bevir/Blakely 2016, Yanow/Schwartz-Shea 2014b [2006]) or contrast the naturalistic with the constructivist paradigm (Moses/Knutsen 2019 [2007]).

In the following I will briefly (details in sections 6.2 and 6.3) go into assumptions that distort the scientistic position and point to a fundamental difference that does not in fact exist in this way.

Naturalists differ from constructivists (Moses/Knutsen 2019 [2007]) in that the former start from the ontological assumption that external reality is given and the latter that it is constructed by actors. The behavioralists had indeed banned actors from their analyses, but the rational choice revolution reintroduced them:

> “Bringing men back” was a credit to the rational choice approach[14] (von Beyme 2000 [1972]: 145; my translation).

Scientistic scientists’ epistemic assumptions would include the search for universal truths. The hypothetical nature of truth and its if-then structure are among the implicit assumptions that are seldom explicitly addressed. The search for “the” truth is an idea that has hardly been represented, at least since critical rationalism. Popper even speaks of conjectures in the title of an epistemological book, but does not renounce the ideal of truth and speaks of an approximation to truth (Popper 1972, section 3.2).

The *correspondence theory* or the image theory of truth (Wittgenstein 1984b [1922]) is also rejected by many scientistic scientists, not least because of the theory-loaded nature of the observation and the impossibility of assuming the position of an independent observer. The *coherence theory* of truth is likely to have the most supporters in both traditions (Skirbekk 1977, Gloy 2004), especially since Habermas, one of the most important proponents of the *consensus theory* of truth, now stands by the coherence theory (Habermas 2009a [1999]: 400).

Flyvbjerg reproaches the scientistic scientists for orienting themselves towards an outdated idea of science, an idea stuck in the 19th century. Similarly, one can accuse him, the perestroikans and many interpretivists of not having noticed the epistemological developments of the explanative-prognostic or the Platonic-Galilean tradition since logical empiricism and the philosophy of the ideal language. This is also reflected in the term “positivism”, which I do not use because scientistic scientists do not seek universal truths, nor do they all follow the correspondence theory of truth. In particular, scientists who work with (rational choice) models primarily use the coherence theory of truth, even if this is rarely stated explicitly.

Scientists who prefer model thinking should also be familiar with constructivist positions. In the positivism controversy, Popper rightly complained that his critical rationalism was confused with logical positivism. Unfortunately, this can still be seen

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[14] `Bringing men back‘ war ein Verdienst des Rational Choice-Ansatzes (von Beyme 2000 [1972]: 145).

today. An overview of the scientistic narrative can be found above (chart 3, section 9.4.3).

e. The phronetic narrative of the perestroikans

The most important ontological assumptions of the perestroikans are that reality is constructed and that there are so-called tension points. The exploration of the following epistemic goals is required: the description of phenomena and the use or naming of symbols (texts, images, audios and videos). The framing and the description of language rules, interpretation schemes, interpretation patterns, ways of life and, above all, power structures are also important. Perestroikans are concerned with both local knowledge and the contextuality of knowledge.

They reject the *quantitative-qualitative schism*; both quantitative and qualitative tools can be used. Furthermore, a variety of methods are propagated in this context.

The focus of the epistemological topics of the phronetic perestroikans is clearly on the *axiology*. Problems with a *practical relevance* as well as problem-oriented research in general are recommended in order to achieve better relevance and practical orientation. All this can be achieved with an *applied phronesis*. This is intended to change unjust power relations.

The phronetics artificially build up four opposites to the scientistic scientists. First, it is claimed that scientists strive for universal truths. This is not true, because the hypothetical character of knowledge is also unquestionable for scientistic scientists (section 6.2).

Second, the interpretivists, like the phronetics, emphasize the spatial and temporal *contextuality* of knowledge. The scientistic scientists emphasize the *if-then deep structure* of knowledge; the “if” part can refer not only to local and temporal contexts, but also to an unlimited number of conditions. The *ceteris paribus* clause applies implicitly, so it must be taken into account for every scientific claim (section 5.4.7).

The next two opposites are only partial. The third is that while the perestroikans advocate *radical constructivism*, the scientistic scientists’ external reality can be either *socially* constructed or *natural*. Finally, regarding the fourth opposite, many scientistic scientists have long been familiar with the problems of the correspondence theory of truth. Scientistic scientists who prefer to work with models (rational choice theorists) use the *coherence theory* of truth just as the interpretivists do (section 3.3).

For an overview of the phronetic narrative and its axiological, epistemic, methodological, and ontological assumptions, see the fourth chart above (section 9.4.4).

2.3 *Ad fontes* reconstruction with a systematic intention

Paraphrasing usually leads to an inaccurate representation of an opponent's position. Quickly, the opposing argument becomes a straw man. This is a real source of misunderstanding and has contributed decisively to the methodological controversy and to the science war. Accurate logical and linguistic analyses, as developed in the context of the *linguistic turn* in philosophy, enable an adequate analysis and contribute to a factual discussion.

First, it will be discussed in the following why an *ad fontes* reconstruction with systematic intent is necessary on the basis of method books and philosophical classics (section 2.3.1). Furthermore, a more appropriate conceptualization will be presented, which can be used to better reconstruct the development of political methodology. Therefore, I will then discuss the explication of concepts and introduce them by means of concepts (methodology versus method) that are important for the analysis (section 2.3.2).

2.3.1 *Ad fontes* reconstruction

The Latin term *ad fontes* means "To the sources". It was the motto of the *humanists* and *reformers* in Europe in the early modern period. While the humanists called for a return to the original texts of the Greek philosophers, the religious reformers propagated a return to the Hebrew and Greek Bible texts. Both groups felt that the Latin sources used no longer conveyed the original messages due to translations and paraphrases.

The *ad fontes reconstruction* was therefore established as a hermeneutic tradition in the humanities by Desiderius Erasmus of Rotterdam (1466-1536), Philipp Melanchthon (1497-1560) and Martin Luther (1483-1546) from the 16th century onwards. The aim was to legitimize scientific narratives with the help of original sources. Paradoxically, this hermeneutic tradition, which has since been firmly established in the human and cultural studies, is ignored by some scholars who prefer linguistic-interpretative to logical-mathematical methodology.

As I have shown in the previous section (2.2), many misunderstandings arise. The disregard for opponents arises from the fact that one does not deal with their books, or only very superficially. These misunderstandings can only be overcome by asking all parties to deal with the sources (especially methodological manuals and classics of the corresponding philosophical tradition) of their opponents, i.e. to heed the motto *ad fontes*.

I take this to heart and therefore work above all with many *quotations*, so that everyone can immediately see whether my interpretation is covered by the sources. A *systematic reconstruction* can succeed by striving for a differentiated and *comprehensive* coverage of the positions in the methodological dispute. This is made possible by identifying the positions on *ten vertical* and *three horizontal levels* (2nd chart, section 9.4.2).

The most important works for the philosophy of political science were written in the following languages Ancient Greek, Latin, French, German and English. Unfortunately, I do not speak Ancient Greek, so there are no original quotations from these works in this book. The Aristotelian and Platonic sources are treated on the basis of the accepted interpretations and translations. Latin and French sources are quoted both in the original language and in English translation.

In my opinion, there are proven English translations and interpretations for the ancient Greek, Latin and French works discussed in this book. Unfortunately, for many German-speaking authors, such as Hans Albert, Wilhelm Dilthey, Rainer Enskat, Michael Esfeld, Otfried Höffe, Klaus Kornwachs, Hans Poser, Heinrich John Rickert, Erich Rothacker, Christian Sachse, Wolfgang Wieland or Wilhelm Windelband, there are no corresponding English translations of all their writings. For this reason, many of the quotations from these German philosophers mentioned in this book are translated into English for the first time. Only quotations from the works of Immanuel Kant did not have to be translated, as I was able to rely on established English translations.

2.3.2 Explication of concepts

In the sciences, there is usually not an essentialist, but rather a nominalist understanding in dealing with concepts, i.e. the meaning of concepts is not fixed forever but is very constructive and can therefore only be determined by actual use. A constructivist or nominalist understanding should also take the historical development of terminology into account to prevent confusion. Poser (2012 [2001]) points out that many important concepts in science are not clearly defined or are applied vaguely, for example the terms “explanation”, “observation”, “natural law”, “society” or “epoch”.

There is a risk of getting stuck in a colloquial meaning or of failing to do justice to the underlying facts through arbitrary attempts at definition. In order to follow the current state of research when creating the concept and to keep further developments open, a procedure such as that proposed by Carnap for concept explication is recommended:

> The task of **explication** consists in transforming a given more or less inexact concept into an exact one or, rather, in replacing the first by the second. We call the given concept (or the term used for it) the **explicandum**, and the exact concept proposed to take the place of the first (or the term proposed for it) the **explicatum**. The explicandum may belong to everyday language or to a previous stage in the development of scientific language. The explicatum must be given by explicit rules for its use, for example, by a definition which incorporates it into a well-constructed system of scientific either logicomathematical or empirical concepts (Carnap 1963 [1950]: 3; emphasis adopted).

The replacement of the *explicans* (*explicandum*) by the *explicate* (*explicatum, explanans*) is only successful if the explicate satisfies four sufficiency conditions (Poser 2012 [2001]: 44):

- *Similarity*: There must be a similarity between the *explicate* and the *explicans*. In the majority of cases, the explicate should be able to take the place of the explicans.
- *Regularity*: The rules for the use of the explicate must be specified precisely, exceptions must be excluded if possible.
- *Fertility*: The new term is said to be fruitful for science, i.e. it should allow as many general statements as possible.
- *Simplicity*: The new term must be as simple as possible.

This *method of explication* of the concept level is also used in this book. In doing so, colloquial concepts should not be defined more precisely, but rather existing specialist concepts in (politics) science should be redefined (e.g. “method”, “political theory” or “politics”) or new concepts (e.g. action maxims, action strategies, action instruments) introduced into the discussion (section 6.4.3). In my opinion, the pattern that Carnap used for concept explication applies analogously not only to the conceptual level, but also to all levels and thus to all questions of epistemology and all scientific tools. The main task of any *methodology* is to explicate, specify, reconstruct or further develop. Poser aptly summarized the advantages of the *explication* over the *definition* as follows:

> Whereas the introduction of a definition makes a preliminary decision that cannot be further questioned and which therefore anticipates the result − think of the fishing net − in the sense of a circle of justification, explication firstly avoids a prior dogmatism, and secondly, while explication does make a determination, in contrast to definition, its overhaul ability is always guaranteed: the explicate can be revised if necessary. As will be shown, the revision itself is not unconditional, but can be criticized and revised again and again[15] (Poser 2012 [2001]: 26; my translation).

In the following, I will discuss the pairs of terms "methodology versus method" (A) and "method controversy versus methodology controversy" (B) and their meaning, which play a role in this work.

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[15] Während nämlich der Einstieg mit einer Definition eine Vorentscheidung trifft, die nicht weiter in Frage gestellt werden kann und die demzufolge das Resultat − man denke an das Fischernetz − im Sinne eines Begründungszirkels vorwegnimmt, wird bei der Explikation erstens ein vorgängiger Dogmatismus vermieden, zweitens wird mit der Explikation zwar eine Festsetzung getroffen, aber im Gegensatz zur Definition bleibt deren Überholbarkeit stets sichergestellt: Das Explikat kann, wenn es sich als erforderlich erweist, revidiert werden. Die Revision ist dabei, wie sich zeigen wird, selbst nicht voraussetzungslos, allerdings ist auch sie immer wieder kritisierbar und revidierbar (Poser 2012 [2001]: 26).

A. Methodology versus method

The concept “method” is used in the narrower sense: methods are only tools that enable the scientific generation of facts (e.g. correlation and regression analyses, content analyses, participatory observation, discourse analyses, technology assessment). In addition to the method level, there are nine other philosophical levels. In my opinion, scientific analyses can and must be evaluated on ten methodological levels. The first three levels form the axiological, epistemic and ontological foundations. As a rule, scientists implicitly adopt positions developed by philosophers in particular. Another seven levels are the various scientific tools: concepts, sentences, theories, logics, modes of argumentation, methods and methodological approaches (1st chart and 2nd chart, section 9.4.1 and 9.4.2).

Methodology is the broader concept and refers to the totality of all methodological discussions or what is often understood under methods in a broader sense. Methodology is about the tool used to generate knowledge and enable distinguishing science from other forms of knowledge.

The difference between knowledge (science), e.g. scientific policy consulting, on the one hand, and other forms of knowledge, e.g. subjective ideologies, utopias, slogans or wishes, on the other hand, lies not in the content – which may even be the same – but in the reasoning or procedure of justification. More precisely, it lies in the fact that scientific results are generated with the help of a scientific methodology. A science-based policy consulting justifies proposals for regulation or reform of a political system by means of scientific tools that satisfy scientific principles. Scientific tools offer the means with whose help empirical (descriptive-interpretative, explanatory and prognostic) statements and systems of statements as well as practical (normative, pragmatic and technical) norms and rules as well as normalization and regulation systems can be hypothetically justified. The philosophical foundations determine the tasks, limits, criteria and characteristics of the knowledge generated (Lauer 2017).

B. Method dispute versus methodology dispute

The concept *method dispute* (*Methodenstreit*) is used in the debate in a narrower and a broader sense. In the narrower sense, it refers primarily to the dispute on the *method level* between supporters of quantitative and qualitative-interpretive methods. In a broader sense, however, it is a *methodological* dispute which is not only about methods, but also about scientific tools in general, as well as about basic beliefs or presuppositions in the philosophy of science in general (axiological, epistemological, methodological and ontological foundations of science).

At the beginning of the *Methodenstreit* at the end of the 19th and beginning of the 20th century, the focus was primarily on philosophical (axiological, epistemic, methodological and ontological) questions (Dilthey 1922 [1883], Rothacker 1926, Rickert 1910 [1896]), Windelband 1900 [1894], Weber 1973b [1903-1906], Weber 1973c [1904], Weber 1973c [1906], Weber 1973d [1917], Weber 1973e [1919]). In political science, the dispute quickly shifted to the *methodical level*, namely to the rejection or preference of quantitative or qualitative methods (Flick/von Kardorff/Steinke), 2015 [2000], Blatter/Janning/Wagemann 2007, Denzin/Lincoln 1994, Creswell 2015 [1998]). Meanwhile, the philosophical conflicts are once again in the focus (Moses/Knutsen 2019 [2007], Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a).

Because of these interrelationships, it is better to speak of a dispute over methodology rather than a dispute over method. This is especially true since the term *Methodenstreit* also leads to misunderstandings, because the focus is only on a contrast between quantitative-mathematical and qualitative-interpretive methods, for example, and the many other philosophical differences are only indirectly addressed, or the other nine methodological levels insufficiently considered.

In political science, as in other disciplines, the focus is primarily on methods and methodical approaches (form) on the one hand and theories (content) on the other; all other scientific tools and philosophical foundations are often neglected. In my opinion, it makes sense to differentiate between ten levels in order to avoid confusion and avoidance (1st chart and 2nd chart, section 9.4.1 and 9.4.2).

2.4 Multilingual approach: central importance of multilingualism for science

Language and mathematics are the most important tools of scientists. If one considers language alone as an instrument of science, then two peculiarities stand out: first, there are a huge number of languages; and second, scientists all over the world tend to publish only in English, today's *lingua franca.* In addition, over two millennia, important contributions to the philosophy of science have been made in different languages. An adequate discussion of these contributions that avoids linguistic misunderstandings therefore requires a multilingual approach.

In the following, I will show that multilingualism, first, *enhances the performance* of language as an instrument of science and, second, that multilingualism contributes to the *reliability* of scientific results because the results are simultaneously reproduced in another language. Third, *multilingualism* is the basic prerequisite for *multiculturalism.* Multiculturalism enriches and broadens horizons through new perspectives. Therefore, it is worthwhile to publish in at least one more language besides English. The distinction between three fundamentally different traditions is made with the help of arguments based on the works of classical authors such as Aristoteles, Immanuel Kant and Maximilian Carl Emil Weber, but also on some German-speaking authors who are little known in the English-speaking world. The quotations from the works of German-speaking authors such as Hans Albert, Walter Ernst Otto Dubislav, Rainer Enskat, Otfried Höffe, Klaus Kornwachs, Hans Poser, Wolfgang Wieland, etc., which I use to justify my theses, are, to my knowledge, translated into English for the first time and have not previously been taken into account in the philosophy of science. Publication in English is indisputably essential today.

These theses are developed using the example of a problem from the philosophy of knowledge. A discussion of these concepts in both languages leads first to the avoidance of bogus problems and second to better results (section 2.4.1). Second, these theses will be elaborated using the concept of science as an example (section 2.4.2). Finally, I briefly deal with automatic translation systems (section 2.4.3).

2.4.1 Knowing that (*Wissen*) versus knowing how (*Können*)

The distinction made by Ryle (2009 [1949]) between "knowing that" and "knowing how" is still very influential. This distinction is adopted literally in German, although the German terms "*Wissen*" (knowledge) and "*Können*" (capability) are much more appropriate.

In Ryle's distinction, practical knowledge and practical capabilities or skill are equated. However, this is merely based on ambiguous statements in English, as Kurt Erich Maria Baier, Ryle's translator into German, rightly points out. The use of the English words "knowing how" and "knowing that" does not result in a more precise explanation, but in linguistic confusion. The German translation shows that these terms are extremely imprecise and therefore do not in any way lead to a clarification of the logical geography of knowledge, as Ryle (2009 [1949]) believed.

Baier writes in a comment:

> The translator could not find an equivalent German counterpart for the phrase "knowing how – knowing that" used in the English title of this chapter. Ryle wants to say that "being able to do something" means the same as "knowing how to do it". In German, however, you cannot use any of the two expressions similar to the English "knowing how". The first of these expressions, "knowing how to do something" ["*Wissen, wie man etwas macht*"] does not mean the same as "being able to do something" ["*etwas machen können*"]. Someone may know how to change a car tire (and is even able to describe or show it to someone else), but without being able to do so himself, perhaps because he is not strong or skillful enough or because he has bad eyesight. "Knowing how…" ["*Wissen wie…*"] is a form of theoretical knowledge, not the same as the English "knowing how to do…". The second similar German expression "*Er weiß zu…*" ["He knows how…"] is also inappropriate because it cannot generally be used in place of "*können*" ["can"]. You can possibly say of someone: "He knows how to flatter you", but you will hardly want to ask the question of whether someone can drive with the words: "Does he know how to drive?" ["*Weiß er zu chauffieren?*" In German, one does not use the expression "*Wissen*/know" in such contexts, but "*Können*/can", so "can you drive", not "do you know how to drive". The inquiry is not about a knowledge of driving but about a practical capability to drive.]
>
> The translator therefore had to be content with reproducing the English couple "knowing how – knowing that" with the German couple *Können – Wissen*, which, unlike the English couple, does not provide linguistic confirmation of Ryle's thesis that "being able to do

> something” is a kind of practical knowledge[16] (note from Kurt Baier, the translator of Ryle’s work, in Ryle 1969 [1949]: 26; I made the translations, and I also inserted the comments in brackets).

Baier provides a successful example of how linguistic analysis can be used to solve philosophical problems, by tracing them back to linguistic confusions (Wittgenstein 1984 [1953], Lauer 2017).

Baier’s comments led me to a fundamental rethinking of the importance of multilingualism. The practical consequence was that I chose to work bilingually, in this case too when discussing different forms of knowledge. The distinctions between the various forms of knowledge were worked out by discussing them in two languages, German and English, at the same time. The final formulations had to be convincing in both languages, so they underwent a number of changes.

The results are presented in the form of charts (Chart 10: Knowledge (theory) versus practice (action) and Chart 11: Knowledge versus skill, section 9.4.10, 9.4.11 and 5.2.4). The multilingual approach has proven positive in the treatment of all concepts and has therefore also been incorporated into all other charts.

2.4.2 The concept *science*

The concepts *knowledge* (*Wissen*) and *science* (*Wissenschaft*) belong together in German. In other languages, except in Latin, where the word *scientia* means both science and knowledge, this is not as obvious as in German. Thus, in English there are two words, *science* and *knowledge*, in French *la science* and *connaissances* .or „*savoir*“

In the German-speaking world, there are not only *natural* and *social sciences*, but also *human* and *cultural sciences* (*Geistes- und Kulturwissenschaften*). The methodological

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[16] Für das im englischen Titel dieses Kapitels verwendete Ausdruckspaar ‚Knowing how – knowing that‘ konnte der Übersetzer kein gleichbedeutendes deutsches Gegenstück finden. Ryle will hier sagen ‚being able to do something‘ bedeute dasselbe wie ‚knowing how to do it‘. Im Deutschen kann man das aber durch keinen der beiden dem englischen ‚knowing how‘ ähnlichen Ausdrücke wiedergeben. Der erste dieser Ausdrücke, ‚Wissen, wie man etwas macht‘, heißt nicht dasselbe wie ‚etwas machen können‘. Denn es kann einer wohl wissen, wie man einen Autoreifen wechselt (so daß er es einem anderen sogar beschreiben oder zeigen kann), ohne es jedoch selber zu können, vielleicht weil er nicht stark oder geschickt genug ist oder weil er schlechte Augen hat. ‚Wissen wie …‘ ist eine Form des theoretischen Wissens, also nicht dasselbe wie das englische ‚Knowing how to do …‘. Der zweite ähnliche deutsche Ausdruck ‚Er weiß zu …‘ ist auch unpassend, weil er nicht allgemein an Stelle von ‚können‘ anwendbar ist. Man kann zwar unter Umständen von jemandem sagen: ‚Er weiß zu schmeicheln‘, aber man wird kaum die Frage, ob einer chauffieren kann, mit den Worten: ‚Weiß er zu chauffieren?‘ stellen wollen. Der Übersetzer mußte sich daher damit begnügen, das englische Paar ‚Knowing how – knowing that‘ mit dem deutschen Paar ‚Können – Wissen‘ wiederzugeben, das nicht wie das englische Paar sprachliche Bestätigung für Ryles These liefert, das Können sei eine Art des praktischen Wissens (Anmerkung von Kurt Baier, des Übersetzers von Ryles Werk, in Ryle 1969 [1949]: 26).

controversy has also led to a situation in the English-speaking world where the term *humanities* dominates and the term *human and cultural sciences* is rarely used. The scientistic scientists also use the term humanities to simply deny the scientific nature of these disciplines. Undisciplined but cheerful theorists work here: “happily still undisciplined” (Dryzek/Honig/Philips 2009: 62). One aim of this book is to show that there are three methodological traditions. Therefore, the human and cultural sciences also work with rational methodologies.

2.4.3 Multilingualism and automatic translation systems (ATS)

Today’s lingua franca is clearly English, so every scientist should publish his or her texts in English. But as shown above, it is worth reproducing the results in other languages.

A translation can be facilitated by today’s technical possibilities. A number of automatic translation systems (ATS) are available on the internet, such as Google Translator (translate.google.de) or DeepL (deepl.com). I have been working with these two for years. A text can be entered quickly, and within seconds one obtains an answer. But that is only the beginning of the work. The results are rarely satisfactory; often one even has to change the source text to obtain reasonably good results. For complex issues, I even use both translation services, comparing the results before the final version is created. Through its technical means, generative artificial intelligence enormously expands the possibilities for using language. I discuss the limits and possibilities of generative AI elsewhere: Philosophy of generative artificial intelligence. Theoretical limitations and possibilities, practical benefits and threats of large language models (Lauer forthcoming).

2.5 Ideal types: identifying ideal-type questions within the philosophy of science

Science is characterized by specialization; therefore, the *reduction of complexity* is generally at the beginning of every scientific work. Every scientist has to resort to *Ockham’s razor*. This *principle of parsimony* was first formulated by William of Ockham (1288-1347) and in this case calls for the economic use of scientific theoretical assumptions. Unfortunately, there is now no sure method to separate important from unimportant factors. In the reduction of complexity, Occam’s razor is used so vigorously that sometimes only straw men remain rather than veritable opponents. In the following, I will explain why a participatory philosophy of science can help to reduce complexity (section 2.5.1), and then how a participatory philosophy of science could succeed using the example of political science (section 2.5.2).

The philosophy of political science is about the basics of scientific research using the example of the discipline. Tasks and limits of (political) scientific research can be better determined by distinguishing four main topics: Axiology, Epistemology, Methodology and Ontology (section 2.5.3). Furthermore, I show the necessity to treat axiological, epistemological, methodological and ontological questions ideally on ten vertical and three horizontal levels (section 2.5.4).

2.5.1 Participatory philosophy of science

> *Philosophy is like a lame man who cannot move anything without its supports, the *sciences. And the *sciences are like workers in the dark if they do not use the light of *philosophy to see the ways that connect them to life[17] (Lorenzen 1974: 130; my translation).

Even if Lorenzen exaggerates, the importance of philosophical analyses, which are usually formal, is vastly underestimated, while substantive contributions by philosophers are usually overestimated. This is analogously true within the sciences.

Lorenzen firstly overestimates the possibilities of philosophy. Secondly, he usually operates in a *philosophy-of mode* and not in a *philosophy-with mode*:

> In this mode [*philosophy-of mode*] philosophers remain with their work largely outside the single sciences. Hansson contrasts this with "philosophy with...", by which he means a mode of philosophizing in close cooperation with specialist scientists from a single science. In this mode, philosophers of science do not pursue their subject as outsiders, as observers of the individual sciences, but are themselves active participants in the theory development process of the individual sciences. In this perspective, the goal of philosophy of science is not only to understand what science is and, if possible, to methodologically improve the operation of science. Rather, it also aims to make a substantive contribution to the questions posed by individual sciences[18] (Reydon/Hoyningen-Huene 2011: 136; my translation; see Hansson 2008: 472-483).

These considerations are presented under the subtitle "Participatory Philosophy of Science" by Reydon and Hoyningen-Huene. The second position, philosophy-with mode, is thus summarized:

> Other authors have a more ambitious goal: In their opinion, philosophy of science should also strive to produce scientific knowledge. Here, philosophy of science becomes an interdisciplinary enterprise and the philosopher of science becomes a researcher who

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[17] Die *Philosophie ist wie ein Lahmer, der ohne seine Stützen, die *Wissenschaften, nichts bewegen kann. Und die *Wissenschaften sind wie Arbeiter im Dunkeln, wenn sie nicht das Licht der *Philosophie benutzen, um die Wege zu sehen, die sie mit dem Leben verbinden (Lorenzen 1974: 130).

[18] In diesem Modus [Philosophie-von-Modus] bleiben Philosophen mit ihrer Arbeit weitgehend außerhalb der Einzelwissenschaften. Dem stellt Hansson die ‚Philosophie mit ...' gegenüber, womit er einen Modus des Philosophierens in enger Zusammenarbeit mit Fachwissenschaftlern aus einer Einzelwissenschaft meint. In diesem Modus betreiben Wissenschaftsphilosophen ihr Fach nicht als Außenstehende, als Beobachter der Einzelwissenschaften, sondern sind selbst aktive Teilnehmer im Theorieentwicklungsprozess der Einzelwissenschaften. In dieser Perspektive ist das Ziel der Wissenschaftsphilosophie nicht nur zu verstehen, was Wissenschaft ist und nach Möglichkeit den Wissenschaftsbetrieb methodologisch zu verbessern. Vielmehr soll auch ein inhaltlicher Beitrag zu den einzelwissenschaftlichen Fragestellungen geliefert werden (Reydon/Hoyningen-Huene 2011: 136).

> himself participates in the knowledge production process of the individual sciences and continues this process in the areas where the individual sciences themselves do not appear[19] (Reydon/Hoyningen-Huene 2011: 136; see also 140-141; my translation).

Bunge also calls for close cooperation between philosophy and individual science:

> Now a philosophy of *x* [x stands for any individual science] should match *x* rather than be at variance with *x*, for only then will it be able to (*a*) give an adequate (true) description of *x*, (*b*) suggest fruitful avenues for the conduct of inquiry in x, and (*c*) participate competently and effectively in philosophical controversies in or about *x*. We call these the *conditions of adequacy and fertility* [...]. But what does "match" mean in this context? Loosely speaking, a philosophy P*x* of *x* matches *x* if P*x* shares the "spirit" or "attitude" of *x*, deals with philosophical issues raised be the actual practice of *x*, and makes use of scientific findings to construct and check its own hypotheses (Bunge 1996: 10).

Logical positivism and Erlanger constructivism can be regarded as schools of philosophy of science working in the philosophy-of mode. The vast majority of 20th century philosophers of science, on the other hand, have always explained their reasoning using a single science as an example, with physics dominating (Popper 2005 [1934], Kuhn 1976 [1962], Feyerabend 1986 [1975], Lakatos 1982 [1978], Scheibe). In the second half of the 20th century, there was a turn to biology (Salmon 1989, Vollmer 2002 [1975]), to medicine (Wieland 1986), in some cases to the social sciences (Topitsch 1967, Adorno 1976 [1969], Acham 1983, Bodammer 1987, Salmon 1992, Braun/Saam 2015), and recently also to the technical sciences (Poser 2008a, Kornwachs 2012). In the following, I will briefly describe how I understand, have pursued, and would like to continue to pursue a participatory science methodology using the example of political science.

2.5.2 Participatory science methodology using the example of political science

My critique of the mainstream of science, especially political science, meaning both scientistic scientists and interpretivists, has arisen from the fact that a practical (normative, pragmatic and technical) discourse that satisfies current logical-analytical standards of argumentation is not possible with either a scientistic or an interpretivists methodology, because this requires a practical (normative, pragmatic and technical) rather than a reductionist methodology. In particular, using the example of the European Union and social security, scientific tools (concepts and methodological approaches) have been explained, explicated, specified, reconstructed, redeveloped or further developed, true to my motto, *combining tradition and progress*. Thus,

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[19] Andere Autoren haben ein ambitionierteres Ziel: Ihrer Meinung nach sollte die Wissenschaftsphilosophie ebenfalls anstreben, wissenschaftliches Wissen zu produzieren. Die Wissenschaftsphilosophie wird hier zu einem interdisziplinären Unternehmen und der Wissenschaftsphilosoph zu einem Forscher, der selbst am Wissensproduktionsprozess der Einzelwissenschaften teilnimmt und diesen Prozess in den Bereichen weiter fortsetzt, wo die Einzelwissenschaften selbst nicht auftreten (Reydon/Hoyningen-Huene 2011: 136).

philosophy of science as participatory scientific methodology is practiced on the example of concrete questions within political science (Lauer 1993 and 1998).

In my opinion, methodological questions can only be dealt with adequately within a *participatory scientific methodology*. This requires that one first deals with philosophical questions (Lauer 2013); secondly, I follow up on the concrete axiological, conceptual, epistemic, methodological and ontological considerations within a subject. I do this using here the example of political science. Third, I apply the developed methodology to concrete, paradigmatic examples; I have worked this out most extensively using the example of social security.

2.5.3 Foundations of scientific research: axiology, epistemology, methodology and ontology as well as tasks and limits of (political) scientific research

- ➢ What effects do axiology, epistemology and ontology have on methodology?
- ➢ Which axiological, epistemic and ontological assumptions can partly be identified as implicit assumptions within the methodology?

Epistemology, methodology, and ontology form the "holy trinity" of science, treated especially within philosophy or philosophy of science:

> "[M]ethodology" often appears as one member in a trio from the philosophy of science, the two others being "ontology" and "epistemology". These are the three musketeers of metaphysics (Moses/Knutsen 2019 [2007]: 4).

For the individual sciences, the methodology is the *favorite child*, epistemology, is still somehow accepted as a *necessary evil*. Ontology is clearly the *stepchild*, especially in the 20th century, of many (neo-) positivist and naturalistic philosophers or scientists in the individual sciences and is readily dismissed by these as *obsolete*; since the 1990s, this has also been changing in political science, albeit very slowly (Hay 2011 [2009]).

Moses and Knutsen analyze epistemological, methodological, and ontological issues in terms of their relevance to political science research, viewing them primarily through an epistemic lens. Thus, the title of their book, "Ways of Knowing. Competing Methodologies in Social and Political Research" (Moses/Knutsen 2019 [2007]). This is clearly noticeable in the structure and the discussion in the book. Moses and Knutsen attempt to present both positions; they speak of naturalism versus constructivism objectively, i.e., analyzing both the possibilities and limitations of both methodologies. Their second goal is to build methodological bridges (Moses/Knutsen 2012 [2007]: 299) between naturalism and constructivism.

Axiological issues are just as relentlessly debated in the science war. As a result, they are quite rightly listed alongside epistemic, methodological, and ontological issues in some methodology books (Creswell 2013 [1998]: 21). The main issue here is whether there is a separation between *is* and *ought*. Scientistic scientists affirm this with reference to Weber (1973c [1904] and 1973e [1919]), while interpretivists and perestroikans deny it without offering any viable justifications. Furthermore, there is also a controversial discussion about to what extent a value loading of scientific research is given or whether values as epistemic interests (Habermas 1968c) influence research. In general, axiological questions are concerned with how to deal with questions of value and what role practical questions should play (Kincaid/Dupré/Wylie 2007).

The scientistic scientists, who also go by other names such as social/political scientists, naturalists, positivists or neo-positivists, concentrate primarily on the method level in the narrower sense (1st chart, section 9.4.1) and tend to deal with fundamentals of scientific theory offhandedly. These questions, which were in the foreground in the second half of the 19th and first half of the 20th century, have been pushed into the background. Disputes on the method level have come to the fore, more precisely between the representatives of quantitative and those of qualitative methods.

The importance of philosophical questions is re-emphasized not only by the phronetic perestroikans, but also by the interpretivists:

> Treating methods as self-standing "tools" apart from the presupposed ontological and epistemological positions that inform and shape them denies their significance and denies them their character (Yanow/Schwartz-Shea 2014a [2006]: 425; see also Bevir 2010 [2008], Moses/Knutsen 2019 [2007], Bevir/Rhodes 2016, Bevir/Blakely 2016).

2.5.4 The need to deal with axiological, epistemic, methodological and ontological questions ideally on ten vertical and three horizontal levels

All axiological, epistemic, methodological, and ontological issues are discussed in this book on ten vertical levels (1st chart, section 9.4.1) and on three horizontal levels (2nd chart, section 9.4.2).

> An attempt is made to separate the levels of abstraction more than is the case in most American books, in which arguments from the philosophical, theoretical, methodological and research-technical levels often stand side by side. It should not be concealed that this attempt occasionally has something artificial about it[20] (von Beyme 2000 [1972]: 7; my translation).

Following von Beyme's approach, the aim here is to separate the different philosophical levels of scientific discourse. The intention is to show the complexity and diversity of scientific discourses and their methodology, in particular by

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[20] Es wird versucht, die *Abstraktionsebenen* stärker zu sondern, als dies in den meisten amerikanischen Darstellungen der Fall ist, bei denen Argumente der meta-theoretischen, theoretischen, methodologischen und forschungstechnischen Ebene häufig unvermittelt nebeneinander stehen. Es sei nicht verschwiegen, daß dieser Versuch gelegentlich auch etwas Künstliches an sich hat (von Beyme 2000 [1972]: 7).

identifying the implicit assumptions and presuppositions, which become apparent above all in the choice of scientific tools as well as in the presupposed philosophical foundations (axiological, epistemological, methodological and ontological basic convictions). The distinction into different levels of scientific theory certainly has something artificial about it and can only be made in an ideal-typical way. Nevertheless, it is made not just in the hope that it is of didactic interest, but also because it is central for scientific analysis as well as for the evaluation of scientific results. The distinction into ten vertical and three horizontal levels forms, so to speak, a mental overview, an orientation or a topography of the scientific methodology (*logical geography*, Ryle 2009 [1949], *orientation in thought* or *topography of reason*, Kant 1977 [1786]). The ten vertical levels also form the outline according to which almost all of the following chapters and charts are structured.

In this book, all ten levels of scientific discourse are systematically presented (1st chart, section 9.4.1). In addition to this vertical structure, there is also a horizontal structure. This shows, first, that there are structural differences between empirical-interpretative (descriptive-interpretative), empirical-scientistic (explanatory and prognostic) and practical methodologies and, second, that these methodologies are complementary to each other (2nd chart, section 9.4.2).

I have elaborated three different methodological traditions, between which a methodological *incommensurability* can be identified, i.e. each methodology has different axiological, epistemic, methodological and ontological prerequisites or procedures. On the one hand, there is a fundamental difference between an empirical (descriptive-interpretative, explanative and prognostic) methodology (chart 6, section 9.4.6) and a practical (normative, pragmatic and technical) methodology (chart 7, section 9.4.7). Within the empirical methodology, there are fundamental differences between an interpretative (descriptive-interpretative) (chart 3, section 9.4.3) and a scientistic (empirical-explanatory and prognostic) methodology (chart 4, section 9.4.4, an overview of all three methodologies can be found in chart 2, section 9.4.2).

However, it does not make sense to speak of different paradigms within political science, because this does not imply a *general incommensurability. Methodological* incommensurability does not lead to *irreconcilable discontinuities* that prevent meaningful cooperation. Exactly this should be the case, however, if one speaks of different paradigms, as would be necessary according to the Kuhn narrative. On the contrary, a practical methodology is absolutely *dependent* on the results that are generated with an empirical (descriptive-interpretative, explanative and prognostic) methodology. Therefore, all three methodologies are needed to generate political knowledge: firstly, the descriptive methodology, so that visible political phenomena can be described, then the empirical-explanative and prognostic methodology, so that the invisible causal relations of these phenomena can be explained and predictions about the future made. Political norms and regulations can only be justified with a genuinely practical (normative, pragmatic and technical) methodology. A practical political science that wishes to justify realistic proposals for regulation and not unworldly dystopias or utopias is dependent on the knowledge that has been developed with the help of descriptive and empirical-explanative methodologies (chart 9, section 9.4.9).

2.6 Evolution through innovative further development of traditions instead of revolution

We live in *exciting times:* digitization, globalization, climate change etc. undoubtedly brings new, incremental and disruptive innovations. Content and methodological innovations are the result. At the same time, we also live in *turbulent times.* Due to the laws of the attention economy, it is almost impossible to reach one's fellow human beings unless one makes a mountain out of every molehill: an idea becomes a paradigm, an innovation a revolution, a tool a methodology.

In short, the *Zeitgeist* thirsts for revolutions. Unfortunately, no utopia is in sight; instead the consequences are infantilization and polarization of discourse. This revolutionary impetus also overshadows the philosophy of science. What's more, one of the most important philosophers of science, Thomas Samuel Kuhn, made a primarily indirect contribution to this.

Various *concepts for the reconstruction of scientific progress* are used to describe the various scientific methodologies, the related innovations and the progress made with them. These concepts also serve to differentiate between different schools or philosophical traditions. I will first discuss the most widely used concepts, *revolution* or *Kuhn narrative*, and show why this is unsuitable for describing methodological developments in political science (section 2.6.1). Then I will further develop the concepts *research programs* and *traditions* and show that these concepts are more appropriate and sufficient to represent the methodological developments since the Aristotelian Organon (section 2.6.2). In other words, the concepts of von Wright (1971) and Imre Lakatos (1982 [1978]), which I further developed, are more suitable than those of Kuhn (1976 [1962]), Schäfer (1993), Mittelstraß (1992) or Blumenberg (1975) to trace the methodological developments.

Maintaining methodological traditions does not conflict with innovations and further developments, i.e. it does not prevent scientific dynamism. Both are necessary, hence my motto: *Combining tradition and progress.* True to this motto, methodologies should, in this book, be explained, made explicit, specified, reconstructed, newly developed or further developed.

2.6.1 Revolution or Kuhn narrative: paradigm, incommensurability, (Copernican) revolution or (Copernican) turn, Bacon project and Leonardo world

The terminology introduced into the debate by Kuhn (1976 [1962]) (paradigm, incommensurability, (Copernican) revolution, normal science) is used most often. The Copernican Turn (Blumenberg 1975), the Bacon Project (Schäfer 1993) and Leonardo World (Mittelstraß 1992) point in roughly the same direction. What these descriptions have in common is that a scientific revolution can be identified in modern times that is fundamentally different from the path within science that has been taken since antiquity, and that there have also been irreconcilable discontinuities, incommensurabilities and thus a *scientific revolution.* There is a *general incommensurability* between the old and the new *paradigm*, which makes a meaningful discussion impossible. After the revolution, scientific progress is driven by "normal" work. This is what I call the *Kuhn narrative*, which is used in different versions not only by the opponents in the *Methodenstreit.*

The concept "paradigm" is used in a very inflationary manner by everyone in political science, with very different meanings and often misleadingly. This is not least due to the vagueness of the term:

> Part of the reason for its success is, I regretfully conclude, that it can be too nearly all things to all people. For that excessive plasticity, no aspect of the book is so much responsible as its the introduction of the term "paradigm", a word that figures more often than any other (Kuhn 1977: 293).

Mastermann has worked out at least *22* different meanings (Kuhn 1977: 389; details on this in Hoyningen-Huene 1988, especially Chapter 4: The Concept of Paradigms, p. 133 ff.).

In spite of this problem, Kuhnian terminology is also very popular in political science, although it was actually developed to describe developments in physics, more precisely to describe and explain the (Copernican) revolution from the Ptolemaic to the Copernican world view.

The concept "paradigm" is used to indicate the incomparability between epochs, theories or methodologies. Two paradigms differ in that there is a general and not just a methodological incommensurability between them, according to one of the most common interpretations. The *general incommensurability* causes a paradigm shift in the case of scientific revolutions and leads to a breakdown of scientific communication due to irreconcilable discontinuities. This is very seldom the case. Within the methodology it cannot be proven, because methodological innovations have never been adopted by all scientists, for example within political science, and *old* methodologies are still used. One wants to express this already in the name and speaks of post-positivism (Münch 2016). It is therefore much more appropriate, following

Imre Lakatos (1978), to speak of various *methodological research programs* rather than paradigms.

The term "revolution" is used not least when innovations are described within the methodology, which are often presented as *revolutionary*. It is suggested that the old methodology is now obsolete, that a new one has taken its place (the title of Bacon's main work is "Novum Organum" (Bacon 1990 [1620]), thus becoming a model for many others) and that within science not only progress and thus significant innovation has been achieved, but "revolutionary" innovations have been made at once. The term "revolution" is used to point to fundamental, revolutionary turns within the development of science, whereby one speaks of "Copernican revolutions and turns" also beyond the field of physics. The term "turn" is used to bring about a new revolution, because one considers the current situation to be inappropriate. One can also speak of *counter-revolutionaries*, especially since many, e.g. Hennis (1963), like the phronetic perestroikans (Flyvbjerg/Landman/Schram 2012a), go back to the work of Aristotle and even want to reverse the modern revolution.

The scientistic scientists use the term "revolution" and believe that the current reductionist paradigm, in this case the reductionist methodology, the foundations of which were laid in the 17th century and which were implemented in political science in the 20th century, is correct and therefore only "normal" work is required for the scientists. This is the version of the *Kuhn narrative* from a scientistic point of view.

The interpretivists (hermeneutics, phenomenologists, structuralists, perestroikans) are of the opinion that the current positivist-scientistic paradigm is an aberration, at least for the human, cultural and social sciences, and that a turnaround is therefore absolutely necessary, according to the *interpretative Kuhn narrative*. The scientistic scientists prefer the word "revolution", the interpretivists and perestroikans the word "turn".

Schäfer (1993) speaks of a "Bacon project" that is a basic feature of modernity and differs from antiquity. In antiquity, the knowledge of nature was an end in itself, whereas since modern times the knowledge of nature has been regarded as a means to increase the general well-being of mankind. Nature research should enable the development of techniques and thus provide man with the power through which he can free himself from dependence on nature and from material need. Bacon (1561-1626) is regarded by Schäfer as a propagandist of the new objectives of natural research.

Mittelstraß also offers a terminology with which one can grasp the modern revolution. He speaks of the "Leonardo world" (Mittelstraß 1992). Leonardo da Vinci (1452-1519) to him represents industrial modernism. Scientific and technical rationality and its results then decisively shape the ideas of social progress. In terms of the history of science, the origins of today's industrial society should be identified. Both characterize the modern age: humanity and progress on the one hand and inhumanity and destruction on the other.

Kuhn's terminology has become established in all sciences, including political science. Goodin (2011b [2009]: 13) speaks of several revolutions within American political science. These so-called revolutions are essentially the introduction of new methodologies, so I will write of the introduction of methodological research programs. These have not completely supplanted existing research programs, nor are there any exchanges between researchers using different research methodologies. On the contrary, these methods are complementary and are sometimes used by one and the same scientist. In short, *complementarity*, but above all a juxtaposition and seldom a coexistence of methodologies, rather than incommensurability characterizes the methodological field. Therefore, terms such as "paradigm", "revolution" and "turn" are misleading, even if there is confusion and misunderstanding between the various research schools. The words "revolution" (behavioral revolution, rational choice revolution) and "turn" (linguistic, cultural, interpretative or practical turn) suggest that there has been a fundamental upheaval, the old has been shelved and only new methodologies are being used.

This interpretation by Kuhn does not do justice to his thinking, especially his later thinking, as is recorded in the two collections of essays (Kuhn 1977 and 2000, see Kitcher 2016). Here scientific progress is no longer reconstructed as revolution but as evolution: Scientific development is

> a process driven from behind, not pulled from ahead – as evolution from, rather than evolution toward (Kuhn 2000: 96).

2.6.2 Evolution or tradition and research programs

The division of the Galilean and Aristotelian traditions into two different methodological traditions, which goes back to von Wright (1971), is better suited to discussing the methodological developments. In the following I will use this terminology in a slightly different form:

> What I call here the Galilean tradition has an ancestry going back beyond Aristotle to Plato (von Wright 1971: 2, cf. note 5, p. 170).

Therefore, the term *Platonic-Galilean tradition* seems more appropriate to me. It is important not to confuse the Platonic-Galilean tradition with *Neoplatonism*, which focuses primarily on the content of Plato's work. Platonic thinking within *scientism* can be demonstrated above all on the level of knowledge and the level of methodological approaches when it comes to the conditions and criteria of knowledge and model thinking.

There is *no general incommensurability* between these two methodological traditions, only a *methodological* incommensurability. These traditions are therefore not mutually exclusive, even if obviously contradictory positions are not compatible, but they can be thought of and applied as *complementary*. For example, one must first *understand* events before one can subsequently *explain* them (von Wright 1971).

Both traditions contain several methodological research programs that have also evolved over time. *Tradition* and not *research program* is in my opinion therefore the broader term. The distinction between two traditions is justified because the explanative-prognostic or the Platonic-Galilean tradition is very homogeneous and also differs from the Aristotelian tradition on seven out of ten levels.

There are also other ways of designating different schools in the social sciences. For example, a distinction is made between *quantitative* and *qualitative research*. This distinction is mainly used by researchers oriented to the human and cultural sciences (Flick/von Kardorff/Steinke), 2015 [2000], Blatter/Janning/Wagemann 2007, Denzin/Lincoln 1994, Creswell 2013 [1998]). Scholars who take their cue from the natural sciences and focus on epistemic distinctions prefer to distinguish between *naturalism* and *constructivism* and speak of “Ways of Knowing. Competing Methodologies in Social and Political Research” (Moses/Knutsen 2019 [2007]), the title of a widely acclaimed book. Here, too, the concept of *tradition* is explicitly used (Moses/Knutsen 2019 [2007]: 6).

Neither approach convinces me, the first primarily because it shifts the argument to the methodological level, the second because it describes what constructivism is with an interpretive lens. Scientistic scientists, too, may very well deal with constructions, as I will demonstrate. Furthermore, the central goal of the constructivist tradition, understanding meaning, is simply ignored by scientistic scholars, who pretend that causal reductionism also prevails in the Aristotelian tradition just as it does in the explanative-prognostic or the explanative-prognostic or the Platonic-Galilean tradition.

3 Ontology and Political Science

- What is the *subject area* of political science as a subfield of the scientific system?
- What kind of problems, entities, properties, phenomena, relations or structures are addressed within political science?

The chapter begins with introductory remarks on ontology, metaphysics, and political science. First, the importance of ontological questions for political science is pointed out (section 3.1). After that, several ontological problem complexes are treated briefly: Naturalism and anti-naturalism: invisible causalities and visible phenomena, causal reductionism, and empirical reductionism (section 3.2), methodological individualism versus methodological holism (section 3.3), micro- and macro-level (section 3.4). Mainly ontological questions are discussed that prominently affect the discipline as well as are controversially discussed in the science war.

The four areas of axiology, epistemology, methodology and ontology can only be distinguished ideally. An exact separation is therefore not possible. In the chapter on axiology (section 4.2), this book discusses the tasks and thus also the subject area that political science should have.

3.1 Introductory remarks on ontology, metaphysics, and political science

In the course of the 20th century, ontology, metaphysics as well as sociophysics have been sorted out by mainstream science as well as within philosophy. At the turn of the century, from the 20th to the 21st century, this thread is now being picked up again. Surprisingly, this was also done by scientists who were shaped by the very traditions that thought one could dispense with these questions without scientific loss. More than that, discussing them is detrimental to science. In short: *Ontological questions*, i.e. questions about the subject area of political science, what should be recognized or changed, cannot be ignored.

Already at the end of the 20th century a rediscovery of ontology begins. Even *Popper*, a proven critic of all metaphysics and ontology, developed in his late phase in collaboration with Eccles a three-layer model, whereby he did not use levels of reality like Hartmann (1964 [1940] and 1949 [1942/1949]) but speaks of three worlds. According to Popper, world 1 is the world of physical things, world 2 the world of subjective consciousness.

> Among the inmates of my `third world´ are, more especially, *theoretical systems*; but inmates just as important are *problems* and *problem situations*. And I will argue that the most important inmates of this world are *critical arguments*, and what may be called - in analogy to a physical state or to a state of consciousness – *the state of a discussion* or the

> *state of critical argument*; and, of course, the contents of journals, books, and libraries (Popper 1972: 107).

Poser, a proven philosopher of technology and science (Poser 2008a, 2008b, 2012 [2001] and 2016), is not only aware of the importance of ontological questions for a philosophy of technology, but also draws on Hartmann's ontological levels of reality (*Schichtenmodell*, Hartmann 1964 [1940] and 1949 [1942/1949]). Only then could onto–logical questions relating to technical *artifacts* be dealt with adequately (Poser 2016). Hartmann's model postulates *four levels of reality*: inorganic, living, soul and spirit. In addition, there is *ideal being*: mathematical structures, entities, ethical and aesthetic values.

More relevant to the social sciences is the model of *sociophysics*, or *social physics*, which was first developed in the 19th century by social scientists who took their cue from physics. The project of a sociophysics tries to combine a causal reductionism with a holism. In this way one could also preserve the *unity of the sciences.*

Wagner offers an outline of a sociology as social physics that is capable

> to determine the object of sociology (ontological dimension) in a way that corresponds to the causal and nomological completeness of the physical domain. It is also able to determine the method of sociology (epistemic dimension) in a way that corresponds to the explanatory completeness of the physical domain[21] (Wagner 2012: 87; my translation).

This sociophysics is based on a reductionist ontology using causal relations and a holistic methodology as advocated by Esfeld and Sachse, to whose work Wagner expressly refers:

> The result, holism expanded into a comprehensive metaphysics of causal-functional structures, which leads to a conservative, functional reduction, is, we hope, a position that does justice to both - the unity of nature and the natural sciences as well as their diversity[22] (Esfeld/Sachse 2010: 11; my translation).

Here, too, causality is the element intended to guarantee the unity of nature and the natural sciences. Causality is therefore what holds both the world and the natural sciences together at its core (section 4.2).

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[21] den Gegenstand der Soziologie (ontologische Dimension) in einer Weise zu bestimmen, die der kausalen und nomologischen Vollständigkeit des physikalischen Bereichs entspricht. Sie ist auch in der Lage, die Methode der Soziologie (epistemische Dimension) in einer der explanatorischen Vollständigkeit des physikalischen Bereichs entsprechenden Weise zu bestimmen (Wagner 2012: 87).

[22] Das Ergebnis, der Holismus ausgebaut zu einer umfassenden Metaphysik kausal-funktionaler Strukturen, die in eine konservative, funktionale Reduktion mündet, ist, so hoffen wir, eine Position, die beidem gerecht wird – der Einheit der Natur und der Naturwissenschaften ebenso wie ihrer Vielfalt (Esfeld/Sachse 2010: 11).

On the one hand, ontological questions, for example regarding the *subject area* of political science, seem to be relatively simple and clear: it is primarily a question of delimiting the subject area of political science from other disciplines. This question can be answered easily, at least from a general point of view: The subject area of political science is *politics* or *political reality*.

Perhaps also because of this simplicity, ontological questions are neglected within political science, which is particularly empirically oriented. This has partly changed, and ontological questions are coming back into focus:

> Political scientists, for the most part, have tended to leave ontological questions to philosophers and to those social scientists less encumbered by substantive empirical concerns. Yet as the discipline has become more reflexive and perhaps rather less confident than once it was at the ease with which it might claim a scientific license for the knowledge it generates, so ontological concern have increasingly come to the fore (Hay 2011 [2009]: 460).

On the other hand, there are also more complex ontological questions, which as prerequisites influence axiology, epistemology and methodology and thus also the content of scientific investigations. The importance of ontological questions is underestimated because ontological assumptions are often accepted without a thorough discussion. Unfortunately, the many problems that accompany or are connected with such requirements are not pointed out.

Hay, in his contribution to the Oxford Series, rightly points out that ontological assumptions are so important because, first, they are the source of differences:

> [O]ften unacknowledged ontological choices underpin major theoretical disputes within political analysis. Second, whilst such disagreements are likely to be manifest in epistemological and methodological choices, these are merely epiphenomena of more ultimately determinate ontological assumptions (Hay 2011 [2009]: 461).

Second, ontological questions precede epistemological and methodological questions; The following ranking was created by Hay (2011 [2009]: 466):

A. *Ontology*: „[w]hat's out there to know about?"

B. *Epistemology*: „[w]hat can we (hope to) know about it?"

C. *Methodology*: „[h]ow can we go about acquiring that knowledge?"

As can be seen, only the "holy trinity" or the "three musketeers of metaphysics" count for Hay as well as for Moses and Knutsen (Moses/Knutsen 2019 [2007]: 4, see although Moses 2020). Questions of value are just as important, which is why in this study, in addition to epistemic, methodological and ontological questions, axiological questions are also examined in chapter four separate, as is also done in some handbooks (Creswell 2013 [1998]: 21).

In his paper Hay (2011 [2009]) deals with three ontological complexes of questions that he believes are important for political science:

A. *Individual-Group Relationship*

B. *Structure-Agency Relationship*

C. *Ideational-Material Relationship*

Political ontology is in my opinion within the science war significant primarily because it provides an exogenous critique of the causalists' or naturalists' favorite child, rational choice theory or rational choice methodology. The critique is exogenous because it focuses on assumptions that were not substantiated of this methodology:

> The rise of political ontology has increasingly led to a series of challenges to naturalism (a belief in the possibility of a unity of method between the natural and social sciences) and to naturalistic political science more specifically. The above paragraphs provide but one example. As they suggest, rational choice theory can deliver a naturalist science of politics only by virtue of the implausible (ontological) assumptions it makes about the universally instrumental, self-serving, and utility-maximizing character of human conduct (Hay 2011 [2009]: 472-473, section 6.4.1, especially the critique of the "Imperialism of Categories" by Susanne Hoeber Rudolph (2005a)).

In concluding the introductory remarks, I would like to point out a very recent and ongoing controversy. It is about the limits and possibilities of a social ontology. The philosopher Richard Lauer presented a “pragmatic approach to naturalized social ontology” (Lauer, Richard 2019 and 2021, cf. Little 2009). The point is not whether at all, but how a social ontology can generate important knowledge for science. Lohse (2020) and Kincaid (2021) submitted critical comments and suggestions for improvement.

In the following, I will concentrate on three subject areas that have had a very strong impact on the science war. Furthermore, for the sake of completeness, I would also like to point out ontological controversies that, in my opinion, are not so central to political science, but are extremely important in the philosophy of science. There is, first and foremost, arguments about scientific realism (Lyons 2016).

3.2 Naturalism and anti-naturalism

> *Naturalism* is the assumption that explanation in political science should be formal, ahistorical, and invariant like those often found in the natural sciences. The philosophic roots of naturalism are found in the Vienna Circle, logical positivism, British empiricism, and early analytic philosophy (Ayer 1952: 58; Carnap et al. 1929: 331; Neurath 1931: 48). By contrast *anti-naturalism* is the view that human beliefs and action are expressive meanings, making political inquiry incompatible with the naturalist quest for formal, ahistorical, and invariant explanations. The philosophic roots of anti-naturalism are found in German Romanticism, phenomenology, idealism, and post-Wittgensteinian analytic philosophy (Collingwood 1946: 285-8; Dilthey 1976; Husserl 1936; Winch 1958) (Bevir/Blakely 2016: 319.

Bevir and Blakely speak of specifically philosophical aspects of the conflict between naturalism and anti-naturalism. In the following I will discuss the ontological aspects of this conflict, first the distinction between invisible causalities and visible phenomena (section 3.2.1), then causal reductionism (section 3.2.2) as well as empirical reductionism (section 3.2.3). I discuss what requirements explanations should meet in the chapter on axiology (section 4.2). The epistemic aspects are discussed in the fifth chapter on epistemology.

3.2.1 Invisible causalities and visible phenomena

The scientistic scientists emphasize that causality is invisible and that one can identify causalities only with the help of causal inferences. For this purpose, one needs various scientific tools: logic and mathematics, experimental, quantitative, qualitative-mathematical research programs (3rd chart, section 9.4.3, section 4.2.2).

The interpretivists, like the phronetic perestroikans, are primarily interested in visible phenomena and use a different methodology (4th chart, section 9.4.4 and 4.2.3): language, mainly interpretative and qualitative-classificatory tools (concepts, methods and methodological approaches, e.g. qualitative content analysis, discourse analysis, hermeneutics).

This ontological distinction has led me to distinguish on the horizontal level between an empirical-interpretative (descriptive-interpretative) and an empirical-scientistic (explanative and prognostic) methodology. As can be seen (2nd chart, section 9.4.2), there are fundamental differences between these two methodologies on ten vertical levels.

3.2.2 Causal reductionism

Causal reductionism is a recognized ontological presupposition within the explanative-prognostic or the Platonic-Galilean tradition. Tradition, which has been postulated since the days of Francis Bacon, as I will show in detail (section 4.2, 3th chart, section 9.4.3). But ontological presupposition is rarely discussed discursively. Thus, even the methodological handbook Political Methodology (Box-Steffensmeier/Brady/Collier 2010a [2008]) only discusses the methodologies with which causality can be proven, but *not* why one should only search for causalities.

There is no question that the subject area of political science also includes political language. Quantitative-mathematical methods (section 6.9) can also help to describe and understand meanings and contexts. But without a linguistic-interpretative methodology as a supplement to causal analyzes there is no adequate recognition of the world, without a practical (normative, pragmatic and technical) methodology there is no practical knowledge with the help of which a rationally justified world change would be possible.

The Perestroikans point out the importance of language and linguistic-interpretative methodologies. On the other hand, scientistic scientist do *not* address the extent to which there could be other than causal relations.

3.2.3 Empirical reductionism

Empiricism (empirical anchoring) and *rationality* (rational formalization) are the overriding principles that a scientific discourse must satisfy (chapter 5). Is *empiricism alone a conditio sine qua non* of all scientific results or the instance against which all scientific results must be measured? Authors who affirm these questions are *empirical reductionists*. Most empirically oriented scientists are among them. Empiricism is usually exaggerated within the explanative-prognostic or the Platonic-Galilean tradition of scientists who see themselves as "*empirical*" researchers or speak of an *empirical political science*.

On the other hand, there are scientists who recognize that there are e.g. ontological questions for which an empirical examination or decision is in principle not possible because they can neither be falsified nor empirically posed in a meaningful way at all:

> Quite simply, perspectives on the question of structure and agency, or any other ontological issue for that matter, cannot be falsified – for they make no necessary empirical claim. It is for precisely this reason that logical positivists (like Popper) reject as meaningless ontological claims such as those upon which realism and structuration theory are premised [...].
>
> In particular, social ontologies cannot be brought in to resolve substantive empirical disputes (Hay 2011 [2009]: 469).

An empiricism (sometimes referred to as "*hyperfactualism*"), anchored in logical empiricism and critical rationalism, is an ontological premise. This is at least criticized in an article in the Oxford Handbook, by the only author who can be called an interpretivist because of his other contributions (Bevir/Rhodes 2016, Bevir/Blakely 2016), without this having any discernible impact on the other contributions:

> To be harsher still, therefore, political scientists are in danger of becoming dull technicians, capable of applying the techniques that they learn from statisticians and economics, but lacking any appreciation of the philosophical issues entailed in decisions about when we should use these techniques, the degree of rigor we should want from them, and how we should explain the data they generate. Many political scientists have long worried about hyperfactualism – the collection of data without proper theoretical reflection. Today we might also worry about hypermethodologism – the application of methodological techniques without proper philosophical reflection (Bevir 2010 [2008]: 68-69).

In his contribution, Bevir criticizes the lack of consideration of meta-methodological questions, in particular naive empiricism and realism, and points to the criticism of Quine and Wittgenstein in particular, and he also advocates a *meaning holism*. I also go into this (section 3.3.3). In addition, there is a wealth of other philosophical questions and problems that are necessary to avoid sterile *hypermethodologism*, which are discussed here in detail (chapter 3-6).

3.3 Methodological individualism versus methodological holism

Both empiricism and the radical form of it, empirical reductionism, resort to methodological individualism, whereby theories are to be either empirically confirmed

(3.3.1 Empirical confirmation or empirical refutation) or empirically falsified (3.3.2 Fallibilism). Methodological holism is directed against both approaches (3.3.3 The Duhem-Quine thesis).

3.3.1 Empirical confirmation or empirical refutation: Instantias Crucis (cross cases or decision experiments)

Locke is considered the founder of empiricism because of his work "An Essay Concerning Human Understanding" (Locke 1975 [1690]). Galileo Galilei is often regarded as the founder of experimental research. However, Francis Bacon was the first to claim that theories can be confirmed empirically using decision experiments or cross cases (*Experimenta/Instantias Crucis*).

> These are the so-called experimenta crucis, which mark a theoretical crossroads and are intended to enable a clear determination of causes (Bacon 1620, II. § 36). Two alternatives are designed and one of them is refuted by experience. Then the other has been proven correct[23] (Carrier 2006: 25; my translation).
>
> In the fourteenth place among *privileged instances* we place *crucial instances*; we take the term from the *signposts* which are erected at forks in the road to indicate and mark where the different roads go. We have also chosen to call them *decisive instances* and *instances of verdicts*, and in some cases *oracular* and *commanding instances*. This is how they work[24] (Bacon 2000 [1620]: 159).

3.3.2 Fallibilism

Popper (2005 [1934]) also advocates empirical reductionism; according to him, theories should not be confirmed but, if possible, falsified by experience. Popper is also the best known critic of holism in the 20th century (Popper 1980a [1944], 1980b [1944] and 2003 [1957]). Since Critical Rationalism is very widespread within political science (von Beyme 2016: 47), this naturally also applies to scientistic scientists.

3.3.3 The Duhem-Quine thesis or the holistic objections to an empirical reductionism

Duhem (1978 [1906]) first denied that such *experimenta crucis* could exist:

> Duhem's analysis of the relationship between experiment, law, and theory inevitably leads to a 'holistic' view of science. This means the following: The experimental verification of a certain hypothesis is only possible by making use of a whole group of further laws - ultimately the entire theory. Should the experiment turn out negative, the contradiction is not directed against this single hypothesis, but against the entire theoretical structure that

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[23] Dies sind die sogenannten Experimenta crucis, die eine theoretische Wegscheide markieren und eine eindeutige Ermittlung von Ursachen ermöglichen sollen (Bacon 1620, II. § 36). Dabei werden zwei Alternativen entworfen und eine von diesen durch die Erfahrung widerlegt. Dann ist die andere als richtig erwiesen (Carrier 2006: 25).

[24] Inter Praerogativas Instantiarum, ponemus loco decimo quarto Instantias Crucis; translato vocabulo a Crucibus, quae erectae in biviis indicant et signant viarum separationes. Has etiam Instantias Decisorias et Judiciales, et in casibus nonnullis Instantias Oraculi et Mandati, appellare consuevimus (Bacon 1990 [1620]: 438, 36. aphorism, volume 2).

> had to be called upon to test it. At most, the whole of a physical theory must be called false. However, no experiment can show where the error is in the system. So the hope tied to the experimentum crucis since Bacon's day of being able to eliminate alternative hypotheses in this way has to be abandoned: a decision-making experiment between competing hypotheses is impossible[25] (Schäfer 1978: XXVI-XXVII; my translation).

Willard Van Orman *Quine* generalizes the connection established by Duhem (1978 [1906]) for physics to science as a whole: "The unit of empirical significance is the whole of science" (Quine 1961 [1953]: 42), because the "dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all" is flawed because "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (Quine 1979 [1953]: 4) and "[t]aken collectively, science has the double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one" (Quine 1979 [1953]: 42).

3.4 Micro-and macro-level

The distinction between the individual and the general, the part and the whole, or the micro and macro levels can be found in all methodological traditions, even if the focus is on different topics.

For example, scientistic scientists look for probabilistic laws or causal regularities at the macro level, and for causal processes or causal mechanisms at the micro level.

The interpretivists describe phenomena or the use of symbols (text, images, audio and video) at the micro level and enable their *naming*. At the macro level, language rules, interpretation schemes, ways of life, structures and interpretation patterns are described and *framing* is carried out. The phronetic perestroikans want to discover power structures or *tension points* on both levels and help those affected to overcome them (chart 6, section 9.4.6).

Firstly, it is important to investigate which level one has to start with (methodological individualism or methodological holism). Second, whether the hiatus between the

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[25] Aus Duhems Analyse des Verhältnisses von Experiment, Gesetz und Theorie ergibt sich zwangsläufig eine ‚holistische' Auffassung der Wissenschaft. Das will folgendes besagen: Die experimentelle Überprüfung einer bestimmten Hypothese ist nur dadurch möglich, daß von einer ganzen Gruppe weiterer Gesetze – letztlich der gesamten Theorie – Gebrauch gemacht wird. Sollte das Experiment negativ ausfallen, richtet sich mithin der Widerspruch nicht gegen diese einzelne Hypothese, sondern gegen das gesamte theoretische Gefüge, das bei der Überprüfung in Anspruch genommen werden mußte. Allenfalls das Ganze einer physikalischen Theorie muß falsch genannt werden. Kein Experiment kann jedoch zeigen, an welcher Stelle des Systems der Fehler steckt. Also ist die an das experimentum crucis seit Bacons Tagen gebundene Hoffnung, auf diese Weise alternative Hypothesen eliminieren zu können, preiszugeben: ein Entscheidungs-experiment zwischen konkurrierenden Hypothesen ist unmöglich (Schäfer 1978: XXVI-XXVII).

levels can be overcome in principle (section 3.3.3). Third, how one can or must proceed in doing so.

The scientistic scientists, especially the Critical Rationalists underestimate the possibilities of induction and overestimate those of deduction. Meanwhile, the principled limitations of both *induction* and *deduction* are seen and one uses primarily quantitative methods at the macro level and qualitative mathematical methods at the micro level. The perestroicans prefer exclusively bottom up (epagogical) methods and procedures especially *abduction* (details in section 4.2, 6.8 und 6.9).

4 Axiology and political science

- What are the *axiological foundations* of political science?
- What *value* should scientific results have for the *state* and *society*?
- What is the relationship between *is* and *ought*?
- Which *tasks* and *goals* should be pursued within political science?
- Can and should science contribute to *world change* or *problem solving*?
- Do we need an *applied* (normative rational choice theory or applied phronesis) or a *genuinely practical political* science?

When it comes to axiological questions, one often finds very extreme positions within the scientific community. Some scientistic scientists completely reject questions of value within the sciences with reference to Weber and the value-free nature of science. Others e.g. the perestroikans point out the value ladenness of all scientific knowledge and that this cannot be prevented. Furthermore, value issues play a role at all levels of scientific research. Thus, fundamental differences between an empirical and a practical methodology can be identified on ten levels (2nd chart, section 9.4.2).

This chapter deals with two axiological complexes or topics: First, the axiological foundations of (political) scientific research (4.1), second, the tasks or goals of (political) scientific research (4.2).

4.1 Axiological foundations of (political) scientific research

In the following, the path from the Aristotelian to the explanative-prognostic or the Platonic-Galilean tradition is traced. The focus is specifically on the path from a practical philosophy to an applied (not practical!) social science. Both the empirically oriented political scientists and the perestroikans advocate an applied political science, the one wanting to implement this by means of normative rational choice analysis, the other by means of applied phronesis.

The transition from *practical philosophy* to *applied social science* can best be traced by starting with the methodological works of Weber, a classic of the social sciences. Weber's primary interest was in empirical investigations, so his methodological works also dealt intensively with the possibilities and limitations of empirical science. In the process, important philosophical foundations are formulated, which are still valid today within empirical, especially scientistic, political science as basic methodological principles.

The following problem complexes are discussed separately:

- State of research: value issues within science as well as genuinely practical methodology at the beginning of the 21st century. Analytical, empirical and practical judgments (4.1.1).
- Is-ought relation, value freedom and value judgments within empirical sciences (4.1.2).
- Applied social sciences: inversion of causal propositions or transformation of cognition (theory) into action (practice) (4.1.3).
- Applied methodology or purely technical methodology within the explanatory-prognostic tradition: normative rational choice theory (4.1.4).
- Applied methodology of the perestroicans: applied phronesis (4.1.5).

4.1.1 State of research: value issues within science as well as genuinely practical methodology at the beginning of the 21st century

The possibilities and limits of practical methodologies are particularly in focus due to my scientific interests. The search for scientific answers to political-practical questions forms the core of my work. Empirical (descriptive, explanatory and prognostic) answers I take note of, but my claim is to formulate practical (normative, pragmatic, technical) answers with practical tools (concepts, propositions, theories, logics, modes of argumentation, methods and methodological approaches). In doing so, it is necessary to use existing scientific tools, to develop some further, and to discover and justify new ones.

The possibilities and limitations of two practical methodologies will therefore be presented and critically evaluated. Both the practical methodology of the scientistic scientists (normative rational choice theory) and that of the phronetic perestroikans (applied phronesis) will be evaluated. Within the linguistic-interpretive methodology, unfortunately, little attention is paid to practical methodology, although there is a strong call for practical, problem-driven research (Green/Shapiro 1994, Shapiro 2005, Schram 2003 and 2005). Phronetic perestroicans, as one of several interpretive schools, not only call for problem-oriented research, but have also formulated a methodological approach to how problem-oriented research should be concretely implemented with applied phronesis.

The aim of modern science is to enrich human life (Bacon 1990 [1620]: 173, 81st aphorism, volume 1). Because of the equivalence between causality and action (Bacon 1990 [1620]: 80, 3rd aphorism, volume 1), one can transform cognition (theory) into action (practice), i.e., social technology, by "inversions of causal propositions" (Weber 1973d [1917]: 529 [491]) or by " inversion of the fundamental explanatory scheme"

(Popper 1984 [1972]: 367). Practical problems could thus be solved by inverting causal propositions, because true knowledge and effective action are equivalent (Bacon 1990 [1620]: 286, 4th aphorism, volume 2).

The scientistic establishment believes that normative rational choice theory not only offers an adequate practical methodology, but also that it is currently the best practical-normative methodology (Hardin 2011 [2009]).

The phronetic perestroikans doubt the problem-orientation of scientism and want to revolutionize political science with applied phronesis and help it regain more public relevance: "Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again" (Flyvbjerg 2001), "Making Political Science Matter" (Schram/Caterino 2006), and "Real Social Science. Applied Phronesis" (Flyvbjerg/Landman/Schram 2012a) are the programmatic titles.

Weber distinguishes between an empirical and a practical social science (Weber 2011 [1904]). He refers to the distinction between theoretical and practical philosophy that has existed since antiquity only indirectly in a footnote in which he approvingly emphasizes the work of the logicians of *Neukantianism* (Weber 2011 [1904]: 60). Weber's methodological writings are primarily concerned with the possibilities and limits of an empirical science. The limits he elaborates for empirical science are generally accepted by scientistic scientists to this day.

The perestroikans as well as most interpretivists, on the other hand, reject a separation between *is* and *ought* and even go a step further by claiming that the axiological values of the researcher and the researched almost necessarily influence research as epistemic interests (Habermas 1968c, Kincaid 2023, Van Bouwel 2023).

Other important goals of this thesis concerning value issues are, first, to present in detail the axiological disputes and their influence on scientific results. Second, the criticism of philosophers of technology (Bunge 1967b, Kornwachs 2008 and 2012, Poser 2001) of an inversion of causal propositions, which Weber and Popper still considered unproblematic, is explained. Third, it cites the need for a practical (normative, pragmatic, and technical) methodology that differs on ten levels from an empirical (descriptive, explanatory, and prognostic) methodology.

4.1.2 Analytical, empirical and practical judgments

Both the Aristotelian and the explanative-prognostic or the Platonic-Galilean traditions assume a principled distinction between *is* and *ought*, on the basis of which a separation is made between theoretical and practical philosophy. Therefore, within the social sciences, a distinction would then have to be made between a theoretical and a practical social science.

An important, if not the most important, social science journal at the turn of the 19th and 20th centuries was the Archive for Social Science and Social Policy (*Archiv für Sozialwissenschaft und Sozialpolitik).* Weber recorded the difference between *empirical social science (empirische Sozialwissenschaft)* on the one hand and *practical social policy* or *practical social science* (*Sozialpolitik* or *praktische Sozialwissenschaft*) on the other in one of his most famous and still widely read articles: „`Objectivity´ in Social Science and Social Policy" (Weber 2011 [1904]). This article was written by Weber when he became editor of the above journal alongside Werner Sombart and Edgar Jaffé. Equally important for the questions relevant here are the article „The meaning of the 'value-freedom' of the sociological and economic sciences" (*Der Sinn der „Wertfreiheit" der soziologischen und ökonomischen Wissenschaften*, Weber 1973d [1917]) and the speech „Science as a profession" (*Wissenschaft als Beruf*, Weber 1973e [1919]).

Weber's orientation toward the spirit of Neo-Kantianism, and specifically the German Southwest School of philosophy (Wilhelm Windelband, Georg Simmel and Heinrich Rickert), is noted in a footnote:

> Those who know the work of the modern logicians – I cite only Windelband, Simmel, and for our purposes particularly Heinrich Rickert – will immediately notice that everything of importance in this essay is bound up with their work. (Weber 2011 [1904]: 50)

This also recognizes the fundamental difference, for which Neo-Kantianism was known, between theoretical and practical discussions/philosophy, i.e. that there is a fundamental difference between *is* and *ought.*

Much more influential are the methodological distinctions made by Weber, which continue to characterize the axiological arguments even today. Weber unconditionally adopts the distinction between is and ought, since he is guided by the spirit of neo-Kantianism. However, he does not use the Aristotelian or Kantian terminology, preferring the terms „empirical discipline" (*empirische Fachdisziplin*) and „empirical science" (*empirische Wissenschaft and Erfahrungswissenschaft*, Weber 2011 [1904]: 52, 54 and 55) on the one hand and „practical social science" (*praktische Sozialwissenschaft*, Weber 2011 [1904]: 56) and „social policy" (*Sozialpolitik*, Weber 2011 [1904]: 60 and 67) on the other (the corresponding passages in the German-language article can be found here Weber 1973c [1904]: 149, 151, 152, 153 and 157).

Furthermore, he speaks on the one hand of an *empirical is* (*empirisches Sein*) and on the other hand of a *(normatively) correct sense* ((*normativ*) *richtigen Sinn*, Weber 1973e [1917]: 532 [494]).

The first terminology has prevailed in the social sciences, no one speaks, for example, of theoretical political science, but only of empirical or, meanwhile more and more rarely, of empirical-analytical political science.

A practical political science, for example, is rejected not least by the scientistic establishment with reference to Weber and the freedom of value judgment in science. Wilhelm Hennis (1963) wanted to establish a practical political science by recourse to Aristotelian topics, but was unable to achieve any effect with it. Some of the perestroikans are now trying again to establish a problem-driven political science by recourse to Aristotelian methodology (*applied phronesis*, Flyvbjerg 2001, Flyvbjerg/ Landman/Schram 2012a).

Scientific tools can only be used to justify facts, ideals are not part of them; according to Weber, the former is dealt with within social science, the latter within social policy or philosophical disciplines. The scientific nature of the value discourses is sometimes denied:

> In the pages of this journal, especially in the discussion of legislation, there will inevitably be found social *policy*, i.e., the statement of ideas, in addition to social *science*, i.e., the analysis of facts. But we do not by any means intend to present such discussion as „science" and we will guard as best as we can against allowing these two to be confused which each other[26] (Weber 2011 [1904]: 60).

The last quotation could be interpreted in the direction that normative discussions do not belong to science. Weber, unlike Popper, is not a causal, empirical, and methodological reductionist. According to Weber, both causal-explanatory, sense-understanding (sense or meaning making, *Sinnverstehen*) as well as normative discourses are scientifically not only possible but also necessary.

Weber's programmatic article is primarily concerned with general understanding (*Gemeinverständlichkeit*) and not with a systematic investigation (*systematische Untersuchung*, Weber 1973c [1904]: 146). Probably because of this, he does not use the Aristotelian or Kantian terminology, although he follows their tradition *expressis verbis*. This leads him to break with tradition but only in purely conceptual terms. Thus, he establishes new conceptualizations within the newly emerging social sciences, which are still influential today.

Weber is also quoted extensively because he is often perceived as a reductionist. The inaccuracies are due to the fact that Weber try of his contributions and not with a systematic investigation (Weber 2011 [1904]: 49, *Gemeinverständlichkeit*, Weber 1973c [1904]: 146).

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[26] Es wird also in den Spalten der Zeitschrift – speziell bei der Besprechung von Gesetzen – neben der Sozial*wissenschaft* – der denkenden Ordnung der Tatsachen – unvermeidlich auch die Sozial*politik* – die Darlegung von Idealen – zu Worte kommen. Aber: wir denken nicht daran, derartige Auseinandersetzungen für ‚Wissenschaft' auszugeben, und werden uns nach besten Kräften hüten, sie damit vermischen und verwechseln zu lassen (Weber 1973c [1904]: 157, vgl. 165).

Empiricism (is, *Sein*) and values (*ought*, *Sollen*) are heterogeneous levels and must also be treated separately:

> These are problems of the philosophy of values, not of the methodology of the empirical disciplines. The only thing that matters is that the validity of a practical imperative as a norm, on the one hand, and the validity of an empirical statement of fact, on the other, lie on absolutely heterogeneous levels of the problem, and that the specific dignity of *each* of the two is damaged if one fails to recognize this and tries to force the two spheres together[27] (Weber 1973d [1917]: 501 [463]; my translation).

Weber demands from scientists a clear separation between firstly *logical-analytical discussions*, secondly *empirical analyses*, and thirdly *practical evaluations*, between

> what of his respective remarks is either purely logically deduced or purely empirical fact-finding and what is practical valuation. To do this, however, seems to me directly an imperative of intellectual rectitude, once one admits the strangeness of the spheres; in this case it is the absolute minimum of what is to be demanded[28] (Weber 1973d [1917]: 490-491 [452-453]; my translation).

In his methodological writings, Weber's main concern is the possibilities and limitations of an *empirical* science. The limits of empirical sciences that he identified have generally been accepted by the scientistic scientists until today.

Thus, Dasgupta writes in his paper on "Facts and Values in modern Economics":

> The case study is designed to illustrate the thesis of this paper, that professional debates among contemporary economists on even such ethically loaded concerns as poverty and distributive justice have been about facts, not ethical values (Dasgupta 2009: 585).

In this context, it is important to point out that Weber also distinguishes at the methodological level between ideas in the sense of *ideal types* and ideas in the sense of *ideals*:

> [t]o avoid serious and foolish blunders requires a sharp, precise distinction between the logically *comparative* analysis of reality by ideal-*types* in the logically sense and the *value-judgement* of reality *on the basis of ideals* (Weber 2011 [1904]: 98).

Ideal-types are suitable as analytical tools for empirical investigations, *ideals* are practical (normative or pragmatic) norms or rules and are suitable for practical investigations.

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[27] Das sind Probleme der Wertphilosophie, nicht der Methodik der empirischen Disziplinen. Worauf allein es ankommt, ist: daß einerseits die Geltung eines praktischen Imperativs als Norm und andrerseits die Wahrheitsgeltung einer empirischen Tatsachenfeststellung in absolut heterogenen Ebenen der Problematik liegen und daß der spezifischen Dignität *jeder* von beiden Abbruch getan wird, wenn man dies verkennt und beide Sphären zusammen–zuzwingen sucht (Weber 1973d [1917]: 501 [463]).

[28] was von seinen jeweiligen Ausführungen entweder rein logisch erschlossen oder rein empirische Tatsachenfeststellung und was praktische Wertung ist. Dies zu tun allerdings scheint mir direkt ein Gebot der intellektuellen Rechtschaffenheit, wenn man einmal die Fremdheit der Sphären zugibt; in diesem Falle ist es das absolute Minimum des zu Fordernden (Weber 1973d [1917]: 490-491 [452-453]).

While the distinction between is (*factuality*) and ought (*normativity*) is recognized by the scientistic scientists, not only the perestroikans vehemently reject this separation with reference to American pragmatism and the Frankfurt School. The separation between *is* and *ought*, or between *facts* and *values*, is rejected by all antipositivists or interpretivists, not just the perestroikans:

> But even more than this, the debate may well constitute a 'myth' of academic practice, serving to deflect collective attention away from an area of incommensurable values about which there is no consensus – that 'knowledge' is always and deeply 'political', tied to the humanity of its producers (the interpretative position), rather than able, somehow, to escape the bounds of the physical, social, and historical embeddedness of those producers (the methodological positivist position). Where researchers stand on this metaphysical issue is often indicate of the gestalt with which they approach their research and their lives (Yanow/Schwartz-Shea 2014a [2006]: 425).

The rejection of this separation is a blanket argumentation that does not take into account the manifold relations between values and statements. Value ladenness is assumed to be an inescapable fact. In the following, exactly this diversity shall be shown.

4.1.3 Is-ought relation, value freedom and value judgments within empirical sciences

What relationships are there between norms, regulations and value judgments on the one hand and factual statements and factual judgments on the other? Several possible positions of values can be examined within the framework of scientific discussions. Hans Albert differentiates between three sets of questions:

Value basis ("to what extent social science statements must be based on values of some kind"), *values in the object domain* ("to what extent these sciences must make values of some kind the object of their statements"), and the actual *value judgment problem* ("to what extent social science statements themselves must have the character of value judgments") (all quotes from Albert 1967b [1965]: 189;[29] my translation).

Kevin Elliott (2017) offers a current overview of the values debate and distinguishes six avenues for value influences, connected to six questions which form the chapter headings of his book (chapter 1 is the introduction):

> 2. What should we study?

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[29] inwieweit sozialwissenschaftlichen Aussagen Wertungen irgendwelcher Art zugrunde liegen müssen, ...
inwieweit diese Wissenschaften Wertungen irgendwelcher Art zum Gegenstand ihrer Aussagen machen müssen, ...
inwieweit sozialwissenschaftliche Aussagen selbst den Charakter von Werturteilen haben müssen (Albert 1967b [1965]: 189).

> 3. How should we study it?
>
> 4. What are we trying to accomplish?
>
> 5. What if we are uncertain?
>
> 6. How should we talk about It?
>
> 7. How can we engage with these values? (a critical evaluation of this can be found in Van Bouwel 2023).

For decades now, the dispute over value judgments has been characterized by people talking past each other in a way that is rarely found in such a serious form in science (Lauer 2017, Kincaid 2023). Not least for this reason, as described above, one speaks with good reason of a *science war* (*Methodenstreit*). This is mainly due to the fact that different questions are mixed up. Therefore, in the following, I will differentiate between six sets of questions and discuss them separately:

- *Value relationship*: value beliefs of the scientist, relationship of the scientist to his research object (A).
- The *value* or political and public *relevance of science* (B).
- *Value basis*: non-epistemic norms and values that influence scientific results (E).
- *Values for science* or *epistemic value*, better methodologies that ensure the scientificity, or the authority of science (C).
- *Values* as the *object of science*, values in the object domain (D).
- The *value judgment problem in* a narrower sense: value-free empirical science is possible, but empirical justification of norms is impossible, while practical justifications are possible (F).

A. *Value relationship*: value beliefs of the scientist, relationship of the scientist to his research object

This set of questions is about norms and values as well as evaluative statements of the researcher about the object of his investigation, which determine his choice of problem:

> That science wants to achieve 1. 'valuable', i.e. logically and factually correct and 2. 'valuable', i.e. important results in the sense of scientific interest, that the selection of the material already contains an 'evaluation' – such Things have, in all seriousness, surfaced as 'objections' despite everything said about them[30] (Weber 1973d [1917]: 499 [461]; my translation).

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[30] Daß die Wissenschaft 1. ‚wertvolle', d.h. logisch und sachlich gewertet richtige und 2. ‚wertvolle', d.h. im Sinne des wissenschaftlichen Interesses wichtige Resultate zu erzielen wünscht, daß ferner schon die Auswahl des Stoffes eine ‚Wertung' enthält, – solche Dinge

Here Weber addresses not only the personal values of researchers and their motivation for choosing material, but the value of science to society, e.g., in that researchers produce valuable and important results (see next section).

Enthusiasm, passion or *vocation* (Wolin 1969) and thus also the personal norms and values of the researcher for certain questions and research objects usually do not form a fundamental problem for an objective and value-judgment-free science or can be neutralized.

Weber, as well as all researchers in the explanative-prognostic or the Platonic-Galilean tradition, are of the opinion that methodologically or technically good scientific work can be produced without one's own evaluations determining the results of the work from the outset (Dasgupta 2009). This is still *not* shared by many perestroikans. This brings me to the next point.

B. The *value* or political and public *relevance of science*

The relevance or value of science refers to the function of science for certain goals of interest of an extra-scientific nature, whether these goals are brought to science by the state or by social actors. Weber does not object to such extra-scientific desires, but he is strict about the fact that science cannot provide definitive answers, but can only elaborate a variety of possibilities:

> The sciences, normative and empirical, can only render an invaluable service to political actors and contending parties, namely, to tell them: 1. there are conceivable such and such different 'ultimate' positions on this practical problem; - 2. such and such are the facts which you must reckon with in your choice between these positions[31] (Weber 1973d [1917]: 499 [461]; my translation; see Dasgupta 2009).

In this subsection, the main point is to show the complexity of the problem of value and the relation between is and ought. This point, relevance of scientific results, plays a central role in the current science war. The call for a move away from a methodological orientation towards a problem orientation (method-driven versus problem-driven, Shapiro 2005) or away from scholasticism towards more relevant research (Mead 2010, Héritier 2016) will be problematized in another subsection (section 4.2.4). It should only be noted here that Weber could not see any contradiction between methodologically stringent science and the relevance of science, but on the contrary the stringency of scientific procedures accounts for the actual importance of

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sind trotz alles darüber Gesagten allen Ernstes als ‚Einwände‘ aufgetaucht (Weber 1973d [1917]: 499 [461]).

[31] Die Wissenschaften, normative und empirische, können den politisch Handelnden und den streitenden Parteien nur einen unschätzbaren Dienst leisten, nämlich ihnen zu sagen: 1. es sind die und die verschiedenen ‚letzten‘ Stellungnahmen zu diesem praktischen Problem denkbar; – 2. so und so liegen die Tatsachen, mit denen ihr bei eurer Wahl zwischen diesen Stellungnahmen zu rechnen habt (Weber 1973d [1917]: 499 [461]).

science for society. He categorically rejected the task of gurus who think they can definitively answer practical questions or questions of meaning:

> Under these inner conditions, what is the meaning of science as a profession, since all these earlier illusions: 'path to true being', 'path to true art', 'path to true nature', 'path to true God', 'path to true happiness' have sunk? Tolstoy gave the simplest answer with the words: 'It is meaningless because it answers the question that is important for us alone: 'What should we do? How shall we live?' gives no answer.' The fact that science does not provide this answer is absolutely undeniable[32] (Weber 1973e [1919]: 598 [540]; my translation).

A scientist must be neither a prophet nor a demagogue:

> But politics does not belong there on the part of the lecturer. Especially not when he deals with politics scientifically, and then least of all. Because practical-political opinion and scientific analysis of political entities and party position are two different things. [...] One can demand from him only intellectual honesty: to see that the ascertainment of facts, the ascertainment of mathematical or logical facts or the inner structure of cultural goods on the one hand, and on the other hand the answering of the question about the value of culture and its individual contents and about how one should act within the cultural community and the political associations, - that these are both completely heterogeneous problems. If he then further asks why he should not deal with both in the lecture hall, the answer is: because the prophet and the demagogue do not belong on the lectern of a lecture hall. The prophet as well as the demagogue is told: 'Go out into the streets and speak publicly'. There, that means where criticism is possible[33] (Weber 1973e [1919]: 601-602 [543-544]; my translation).

Weber is primarily concerned here with righteousness, the ability to differentiate, and knowledge of the limits of the scientist, which are related to the limits of methodology. I think it goes too far to reject practical-normative questions within science from this passage and some other over-pointed passages, so I have already pointed out the importance of normative questions within a „practical social science” (*praktische*

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[32] Was ist unter diesen inneren Voraussetzungen der Sinn der Wissenschaft als Beruf, da alle diese früheren Illusionen: ‚Weg zum wahren Sein‘, ‚Weg zur wahren Kunst‘, ‚Weg zur wahren Natur‘, ‚Weg zum wahren Gott‘, ‚Weg zum wahren Glück‘ versunken sind? Die einfachste Antwort hat Tolstoj gegeben mit den Worten: ‚Sie ist sinnlos, weil sie auf die allein für uns wichtige Frage: ‚Was sollen wir tun? Wie sollen wir leben?‘ keine Antwort gibt.‘ Die Tatsache, daß sie diese Antwort nicht gibt, ist schlechthin unbestreitbar (Weber 1973e [1919]: 598 [540]).

[33] Aber Politik gehört allerdings auch nicht dahin von Seiten des Dozenten. Gerade dann nicht, wenn er sich wissenschaftlich mit Politik befaßt, und dann am allerwenigsten. Denn praktisch-politische Stellungnahme und wissenschaftliche Analyse politischer Gebilde und Parteistellung ist zweierlei. [...] Verlangen kann man von ihm nur die intellektuelle Rechtschaffenheit: einzusehen, daß Tatsachenfeststellung, Feststellung mathematischer oder logischer Sachverhalte oder inneren Struktur von Kulturgütern einerseits, und andererseits die Beantwortung der Frage nach dem Wert der Kultur und ihrer einzelnen Inhalte und danach wie man innerhalb der Kulturgemeinschaft und der politischen Verbände handeln solle, – daß dies beides ganz und gar heterogene Probleme sind. Fragt er dann weiter, warum er nicht beide im Hörsaal behandeln solle, so ist darauf zu antworten: weil der Prophet und der Demagoge nicht auf das Katheder eines Hörsaals gehören. Dem Propheten wie dem Demagogen ist gesagt: ‚Gehe hinaus auf die Gassen und rede öffentlich‘. Da, heißt das, wo Kritik möglich ist (Weber 1973e [1919]: 601-602 [543-544]).

Sozialwissenschaft, Weber 2011 [1904]: 56) as well as „social policy" (*Sozialpolitik*, Weber 2011 [1904]: 60). In addition, as I will show (section 4.1.3 F), there is the importance of the „philosophical disciplines" (Weber 1973d [1917]: 508 [470]), whose task it is according to Weber to deal rationally with values.

C. *Value basis*: non-epistemic norms and values that influence scientific results

The greatest disagreement on questions of value between representatives of the explanative-prognostic or the Platonic-Galilean tradition and some representatives of the Aristotelian tradition, as well as between researchers committed to American pragmatism or the Frankfurt School, is on questions of value basis.

Representatives of the explanative-prognostic or the Platonic-Galilean tradition believe that objective and value-free science is possible and that values and norms do not necessarily influence either the determination of facts or the interpretation of data.

The perestroikans dispute this with reference not only to the Frankfurt School and American pragmatism, but also to Stephen Edelston Toulmin, whose book Schram (2003) considers one of the most important philosophical foundations of the perestroikans. Therefore, I quote from this book in the following:

> Even now it takes a sophisticated analysis to convince many behavioral scientists that their theories rest on value assumptions which, if not always explicit, are nonetheless unavoidable. (This is especially hard when the scientists are skilled in such formal, abstract methods of analysis as neoclassical equilibrium theory in economics, and rational choice theory in political science (Toulmin 2001: 205).

The following general assertions are problematic: norms and values or the value relationship of the researcher to his object as well as interests that guide knowledge (Habermas 1968b) necessarily influence scientific results and cannot be methodologically neutralized.

While this the scientistic scientists in my opinion rightly deny both theses and recognize methodological possibilities to neutralize normative influences. Perestroikans are not the only ones to claim that both are unavoidable (Elliott 2017).

D. *Values for science* or *epistemic value*, better methodologies that ensure the scientificity, or the authority of science

First, there are *endogenous* or *epistemic* values that provide the scientific criteria and methodologies approved by the research community. These methodologies are constantly evolving, due to their complexity they are classified and discussed here on ten methodological levels (chapters 5 and 6). Following Searle (1971 [1969], see 6.5.3), one can speak of *constitutive* (non-imperative) norms of science.

Second, there are *exogenous* or *non-epistemic* values, which are those circumstances in which the optimal flourishing of science is given. These are, for example, questions about how, with the help of optimal research policies, science can best flourish. Furthermore, all regulative (prescriptive, imperativistic) norms and rules with which the "*enterprise*" or the "*game*" of science is confronted by society and politics, as well as which tasks and goals scientists themselves consider desirable and feasible. These non-epistemic values alone are discussed in detail in this chapter on axiology.

Here, too, researchers from the explanative-prognostic or the Platonic-Galilean tradition, contrary to many criticisms to the contrary, have no problems with values or goals, certainly not with concrete regulations, just as little as with the next complex of questions.

E. Values as the object of science (values in the object domain)

Values are material and ideal goods as well as norms that are ethically or aesthetically significant and can be the object of scientific investigations. It is self-evident that political norms and values can be the object of empirical research. A political science that does not practice this is irrelevant. One cannot make this accusation, which has been levelled at scientistic scientists by both normative-ontological and critical, Neo-Marxist scholars. When standardizations and regulations become the object of empirical investigation, they lose their normative character; we are then dealing with an *is* (factuality) and not an *ought* (normativity):

> When the normatively valid becomes the object of empirical investigation, it loses, as an object, the norm character: it is treated as 'is' (*seiend*) not as 'valid' (gültig)[34] (Weber 1973e [1917]: 531 [493]; my translation).

Here Weber makes a distinction that is also undisputed later in logic. According to Georg Henrik von Wright (1963: 105), the distinction between *norms* on the one hand and *statements about norms* on the other goes back to Ingemar Hedenius. With the help of *deontic logic* one can examine or standardize the formal relationships of an empirical discourse, with *norm logic* (*Normenlogik*), on the other hand, the *practical-normative discourse.*

Von Wright has shown in several articles (the most important ones were edited by Hans Poser, see von Wright 1977a) that there is, for example, a fundamental difference between the proposition or the descriptive statement “it is forbidden to kill” and the norm or the normative proposition ”you should not kill”.

According to him, one must first distinguish between an „is-ought” (*Sein-Sollen*) consisting of *truth-apt statements* including statements about norms, which is analyzed with the help of *deontic modal logic.*

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[34] Wenn das normativ Gültige Objekt empirischer Untersuchung wird, so verliert es, als Objekt, den Norm-Charakter: es wird als ‚seiend‘, nicht als ‚gültig‘ behandelt (Weber 1973e [1917]: 531 [493]).

Second, there is an „act-ought" (*Tun-Sollen*) consisting of *non-truth-apt norms* or *rules*, which is analyzed with the help of *norm logic*. An „is-ought" relates the deontic operators to „action propositions“ (more precisely, action statements), on facts or states of affairs, and a „act-ought" to „action verbs“ or to actions (von Wright 1977g [1974]: 120, details in section 6.7).

F. The *value judgment problem* in a narrower sense: *value-free empirical science* is possible, but empirical justification of norms is impossible

Do we need a practical social science that formulates value judgments about its subject area, social reality? Or as Hans Albert put it:

> to what extent social science statements themselves must have the character of value judgments? (Albert 1967b[35] [1965]: 189; my translation).

This dispute is about the question of the self-understanding of the humanities, human or social sciences. The problem of evaluative science itself amounts to answering a normative question, namely the question of the task of science. Scientists within the Aristotelian tradition hold that a rational justification of norms and rules is possible. The scientists of the explanative-prognostic or the Platonic-Galilean tradition claim, not least by reference to Weber, that a justification of norms and values is not possible with *empirical* social-scientific methods; only the justification of social-technological rules is affirmed. Many social scientists, especially those with an empirical orientation, generalize the first thesis with reference to Weber to the effect that no scientific tools can be used to justify norms and values.

> An empirical science cannot tell anyone what he *should* do - but rather what he *can* do - and under certain circumstances - what he wishes to do (Weber 2011 [1904]: 54).
>
> Our journal as a representative of an empirical specialized discipline must, as we wish to show shortly, reject this view in principle. It must do so because, in our opinion, it can never be the task of an empirical science to provide binding norms and ideals from which directives for immediate practical activity can be derived (Weber 2011 [1904]: 52, cf. p. 55).

With reference to these passages in particular, Weber is quite wrongly brought into the field as someone who rejects normative discourses within the sciences. If one considers the context of the article as well as the concrete quotations, it is here *expressis verbis* only about the limits of an "empirical science" and not about limits of the sciences *per se*. Weber, as shown above, uses for a practical or normative science the terms „practical social science" (*praktische Sozialwissenschaft*, Weber 2011 [1904]: 56) as well as „social policy" (*Sozialpolitik*, Weber 2011 [1904]: 60).

Here it is important to emphasize that an empirical science (Weber uses two different German terms: *empirische Wissenschaft,* empirical science, and *Erfahrungswissenschaft,* experiential science) cannot undertake the justification of values. However, Weber

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[35] inwieweit sozialwissenschaftliche Aussagen selbst den Charakter von Werturteilen haben müssen? (Albert 1967b [1965]: 189).

also sees that practical questions can also be discussed within the sciences, and speaks of “philosophical disciplines” (Philosophische Disziplinen, Weber 1973d [1917]: 508 [470]). It is possible, for example, to examine the internal coherence of standardization and regulation:

> This criticism [scientific treatment of value judgments], can of course have only a dialectical character, i.e., it can be no more than a formal logical judgment of historically given value-judgments and ideas, a testing of the ideals according to the postulate of the inner *consistency* of the desired end (Weber 2011 [1904]: 54).

Weber himself formulates several possible related questions of a practical social science:

> The meaning of discussions about practical valuations (of the participants in the discussion themselves) can thus only be:
>
> a) The elaboration of the last, inwardly 'consistent' axioms of value, from which the opposing opinions proceed. [...]
>
> b) The deduction of the 'consequences' for the evaluative opinion, which would follow from certain value axioms, if they, and only they, were taken as a basis for the practical evaluation of factual circumstances. [...]
>
> c) The determination of the factual consequences which the practical realization of a certain practically becoming opinion on a problem would have to have: 1. as a result of being bound to certain unavoidable means, - 2. as a result of the inevitability of certain side-effects which are not directly intended. This purely empirical statement [...]
>
> d) represent new axioms of value and consequent postulates, which the representative of a practical postulate did not consider and on which he consequently did not take a stand, although the implementation of his own postulate collides with those of others either 1. in principle or 2. as a result of the practical consequences, i.e.: sensible or practical. In case 1 the further discussion is about problems of type a, in case 2 of type c[36] (Weber 1973d [1917]: 510-511 [472-473]; my translation).

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[36] Der Sinn von Diskussionen über praktische Wertungen (der an der Diskussion Beteiligten selbst) kann also nur sein:

a) Die Herausarbeitung der letzten, innerlich ‚konsequenten‘ Wertaxiome, von denen die einander entgegengesetzten Meinungen ausgehen. […]

b) Die Deduktion der ‚Konsequenzen‘ für die wertende Stellungnahme, welche aus bestimmten Wertaxiomen folgen würden, wenn man sie, und nur sie, der praktischen Bewertung von faktischen Sachverhalten zugrunde legte. […]

c) Die Feststellung der faktischen Folgen, welche die praktische Durchführung einer bestimmten praktisch werdenden Stellungnahme zu einem Problem haben müßte: 1. infolge der Gebundenheit an bestimmte unvermeidliche Mittel, – 2. infolge der Unvermeidlichkeit bestimmter, nicht direkt gewollter Nebenerfolge. Diese rein empirische Feststellung […]

d) *neue* Wertaxiome und daraus folgende Postulate vertreten, welche der Vertreter eines praktischen Postulats nicht beachtet und zu denen er infolgedessen nicht Stellung genommen hatte, obwohl die Durchführung seines eigenen Postulats mit jenen anderen entweder 1. prinzipiell oder 2. infolge der praktischen Konsequenzen, also: sinnhaft oder praktisch, Philosophical disciplines can discuss the *meaning* of valuations as well as delimit their meaningful *spheres of validity*. Empirical sciences, on the other hand, can only determine the *means* of enforcing ends and point out possible *consequences* and *side-effects*:

> Furthermore, *philosophical* disciplines can use their means of thinking to determine the 'sense' of valuations, i.e. their ultimate sensible structure and their *meaningful* consequences, to assign them their 'place' within the totality of the 'ultimate' values possible at all and to delimit their sensible spheres of validity. Even such simple questions as: to what extent an end should justify the unavoidable means, as well as the other one: to what extent the unwanted side-effects should be accepted, as well as the third one, how conflicts between several in concreto conflicting, wanted or intended ends are to be settled, are completely a matter of choice or compromise. There is no (rational or empirical) scientific procedure of any kind which could give a decision here. Least of all our *strictly empirical science* [my emphasis] can presume to spare the individual this choice, and therefore it should not appear to be able to do so[37] (Weber 1973d [1917]: 508 [470]; my translation).

The *impossibility*, *nota bene*, refers only to justifying norms and values by means of empirical social science (Weber 1973c [1904], Weber 1973d [1917], Acham 1983: 230 ff., Albert 1967b [1965], Albert 1971, Stegmüller 1979b: 177 ff., Krobath 2009: 193 ff.), but *not a general impossibility of a rational or scientific, practical* (normative, pragmatic or technical) *discourse*. On the contrary, such discourses are firstly desirable and necessary as well as methodologically feasible, since there is no normative force of the factual, rather all demands require legitimation and thus a practical discourse:

> There is, however, no normative force of the factual. Tendencies and developments in society can never as such make duties binding or justify actions. The facticity of demands, even if they are raised by the modern idol society, can never in itself establish legitimacy of norms. For demands, without exception, are themselves in need of standardization and legitimation, no matter by what authority they may be raised[38] (Wieland 1986: 136; my translation).

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kollidiert. Im Fall 1 handelt es sich bei der weiteren Erörterung um Probleme des Typus a, im Falle 2 des Typus c (Weber 1973d [1917]: 510-511 [472-473]).

[37] *Philosophische* Disziplinen können darüber hinaus mit ihren Denkmitteln den ‚Sinn' der Wertungen, also ihre letzte sinnhafte Struktur und ihre *sinnhaften* Konsequenzen ermitteln, ihnen also den ‚Ort' innerhalb der Gesamtheit der überhaupt möglichen ‚letzten' Werte anweisen und ihre sinnhaften Geltungssphären abgrenzen. Schon so einfache Fragen aber, wie die: inwieweit ein Zweck die unvermeidlichen Mittel heiligen solle, wie auch die andere: inwieweit die nicht gewollten Nebenerfolge in Kauf genommen werden sollen, wie vollends die dritte, wie Konflikte zwischen mehreren in concreto kollidierenden, gewollten oder gesollten Zwecken zu schlichten seien, sind ganz und gar Sache der Wahl oder des Kompromisses. Es gibt keinerlei (rationales oder empirisches) wissenschaftliches Verfahren irgendwelcher Art, welches hier eine Entscheidung geben könnte. Am allerwenigsten kann diese Wahl *unsere streng empirische Wissenschaft* [meine Hervorhebung] dem Einzelnen zu ersparen sich anmaßen, und sie sollte daher auch nicht den Anschein erwecken, es zu können (Weber 1973d [1917]: 508 [470]).

[38] Eine normative Kraft des Faktischen gibt es jedoch nicht. Tendenzen und Entwicklungen in der Gesellschaft können als solche niemals Pflichten verbindlich machen oder Handlungen

As can be seen, Weber deals with the problem of value in a very differentiated way. However, his reflections on the limits of empirical science are particularly influential. This is the case because mainly those political scientists who are in the explanative-prognostic or the Platonic-Galilean tradition still see themselves as empirical (social) scientists today.

The possibilities of a *practical social science* (*praktische Sozialwissenschaft*) or *social policy* (Sozialpolitik), which Weber certainly sees and considers justified, are later not pursued, especially by political scientists who explicitly refer to Weber, just as it happens with his considerations about understanding meaning (*Sinnverstehen*). Creation of meaning (*Sinnstiftung*), interpretation of meaning (*Sinndeutung*) or understanding of meaning (*Sinnverstehen*) are pursued today primarily by interpretivists. Causal analyses alone are the focus of scientistic scholars. Thus normative (ethical-moral) questions within a normative level value discourse, e.g. whether solidarity within a society is *right* or *wrong* or what is *just* or *unjust*, are not dealt with at all (Dasgupta 2009). This also applies to pragmatic questions within a pragmatic level discourse, e.g., which strategies or objectives for implementing solidarity are *wise* or *unwise*, *desirable* or *undesirable*. Only technical questions within a technical level means discourse are addressed, e.g. how to avoid or remedy poverty in a very specific life situation which are *effective/ineffective* (*section 5.2.5*).

The value problem is usually treated in a very differentiated way by Weber and the naturalists, while the critique of the perestroikans is very sweeping and mixes different issues.

In the following subsection, we will now discuss how causal analyses, though not practical, could at least be used to conduct technical discourses on means in the view of the empirically oriented political scientists.

4.1.4 Applied social sciences: inversion of causal propositions or transformation of cognition (theory) into action (practice)

The following subsection deals with world change (practice) and thus with an applied political science, as the scientistic scientists have it in mind, as well as with the problem-oriented approach of the perestroikans.

The reduction of practical (normative, pragmatic and technical) to technical means discourses within the explanative-prognostic or the Platonic-Galilean tradition is only possible under two ontological assumptions, which are unfortunately accepted within the methodological literature, but rarely if ever addressed. On the one hand it is about

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rechtfertigen. Die Faktizität von Forderungen, auch wenn sie vom modernen Götzen Gesellschaft erhoben werden, kann für sich allein niemals Legitimität von Normen begründen. Denn Forderungen sind ohne Ausnahme, von welcher Instanz sie auch erhoben werden mögen, selbst der Normierung und der Legitimierung bedürftig (Wieland 1986: 136).

the *equivalence between causality and action* (A) and on the other hand about *inversion of causal statements* or the inversion of the fundamental explanatory scheme (B). Application means the *inversion* of causal statements, empirically determined causalities in the form of if-then statements are converted into technical rules (C). These relationships will now be described in more detail.

A. Equivalence between causality and action

The philosophical foundations of causal reductionism were first formulated in the 17th century, so it is necessary to look back at the emergence of Galilean thought in the 17th century, specifically at Francis Bacon's epistemological insights.

Bacon claimed that the Aristotelian Organon, or the tools presented there, are obsolete. Therefore, he proposed new tools and wrote a „Novum Organum“ (Bacon 1990 [1620]). In the form of aphorisms, he described his position and, above all, outlined a methodological program that future scientists should actually prove and thus fill with life.

Bacon not only propagated the *idea of progress*, but also implemented it on a large scale for the first time. It is still very popular: a researcher puts forward theses and is sure that future researchers or generations of researchers will be able to provide the evidence at some point without any problems. Bacon does not provide any proof for his theses either, but he nevertheless formulates important prerequisites for causal thinking.

The subtitle of his „*Novum Organum*“ (Bacon 1990 [1620]) is: „*Aphorismi de Interpretatione Naturae et Regno Homini*“. The word „*Interpretatione*“ cannot be translated in today's very narrow understanding of hermeneutic-linguistic interpretation, but includes besides interpretation also explanation and evaluation (see Latin dictionary: online-latin-dictionary.com), in short it is about cognition or understanding of nature and the world (*Interpretatione Naturae*) on the one hand and human control of nature or the world (*Regno Homini*) on the other hand.

Causality can only form the basis of both world knowledge and world change if the two are closely linked, or more precisely, if there is *equivalence* between *causality* and *action*. This equivalence is presupposed and, unfortunately, not addressed at all today in political science or other social sciences. Nor is it done in the above-mentioned volume Political Methodology (Box-Steffensmeier/Brady/Collier 2010a [2008]). Bacon was aware of this connection:

> Human knowledge and human power come to the same thing, because ignorance of cause frustrates effect. For Nature is conquered only by obedience; and that which in thought is a cause, is like a rule in practice[39] (Bacon 2000 [1620]: 33, 3rd aphorism, see aphorism 129).

Farrington's translation is more accurate, in my opinion:

> The twin goals, human science and human power, come in the end together. (Translation by B. Farrington, quoted from Krohn 1990: XVII).

Here Bacon establishes a new relationship between natural causality and the rule of action:

> The central aspect is the reorganization of the relation between the concepts of natural causality and the rule of action. Bacon establishes the equivalence that the recognition of a cause-effect relation in nature can serve as a rule for the production of an effect (a3) and, conversely, that the production of an effect by a rule makes possible the specification of a causality (b4)[40] (Krohn 1990: XVI; my translation).

This *equivalence* is a

> transmission rule from knowledge about nature to the rule of action in nature (Kornwachs 2013: 42; my translation).

Mario Bunge calls this a *pragmatic syllogism* (Bunge 1967b: 132-139). Importantly, both the pragmatic syllogism and the analogy are not formally valid. Logically, equivalence is a biconditional relation: A if and only if B, which formulates once a *necessary condition* (if A, then B) and at the same time a *sufficient condition* (if B, then A) (in the 12th chart, section 9.4.12 the logical relations are clearly stated).

In the causalist terminology of today, one would formulate as follows: *Theory and practice are the same or coincide in the same; statements that are true in theory are efficient in practice.*

Matthias Kortmann and Klaus Schubert speak, on the one hand, of „causal statements“ (*kausalen Aussagen*) that are generated with the help of empirical research and then transformed by inversion into „purpose-oriented statements“ (z*weckorientierte um-zu-Aussagen* Kortmann/Schubert 2006: 48). This indicates the procedure of how to transform *empirically generated knowledge* into *socio-technological regulations.* This will now be addressed in the next section.

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[39] Scientia et potentia humana in idem coincidunt, quia ignoratio causae destituit effectum. Natura enim non nisi parendo vincitur; et quod in contemplatione instar causae est, id in operatione instar regulae est (Bacon 1990 [1620]: 80, 3rd aphorism, 1 volume).

[40] Der zentrale Aspekt ist die Neuordnung der Beziehung zwischen den Begriffen der Naturkausalität und der Handlungsregel. Bacon stellt die Äquivalenz auf, dass die Erkenntnis eines Ursache-Wirkungs-Zusammenhangs in der Natur als Regel der Hervorbringung einer Wirkung dienen kann (a3) und umgekehrt, dass die Hervorbringung eines Effektes durch eine Regel die Angabe einer Kausalität ermöglicht (b4) (Krohn 1990: XVI).

B. Inversions of causal propositions or inversion of the fundamental explanatory scheme

Weber is strictly against an

> inadmissible reinterpretation of facts of the sphere of being [Seinsspäre] into norms of the sphere of value [Wertungssphäre][41] (Weber 1973d [1917]: 539 [501; my translation]).

Legitimacy issues within value discourses and pragmatic target discourses cannot be discussed by means of an empirical methodology. However, according to him, by "simple inversions of causal propositions" one can justify technical means, Weber speaks of "means" (*Mittel*) or of "measure" (*Maßregel*), the modern term is social-technological, technical rules or regulations within a discourse of means, which are necessary for success-oriented (purposive-rational) action:

> It remains the case that economic theory can say absolutely nothing other than: that for the given technical purpose x the measure y is the only suitable means or the means alongside y1, y2, that in the latter case between y, y1, y2 such and such differences and modes of action and - if applicable - the rationality exist, that their application and thus the achievement of the purpose x requires the 'side effects' z, z1, z2 to be accepted. That everything is a simple inversion of causal propositions and as well as „valuations" can be linked to them, they are exclusively those of the degree of rationality of an imagined action. The evaluations are unequivocal if and only if the economic purpose and the social structure conditions are fixed and only one can choose between several economic means, and if these differ only in relation to the security, speed and quantitative yield of the success, but function completely identically in every other respect that may be important for human interests[42] (Weber 1973d [1917]: 529 [491], cf. also p. 517 [479], p. 538 [500] and p. 526 [488]; my translation).

Only technical means discourses with a given *end* are possible within an empirical experiential science or empirical science:

> Only where an absolutely definite purpose is asked for the appropriate means is it a really *empirically* [my emphasis] decisive question. The proposition: x is the only means for y, is

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[41] unzulässige Umdeutung von Tatsachen der Seinssphäre in Normen der Wertungssphäre (Weber 1973d [1917]: 539 [501]).

[42] Es bleibt eben dabei: daß die ökonomische Theorie absolut gar nichts andres aussagen kann als: daß für den gegebenen technischen Zweck x die Maßregel y das allein oder das neben y1, y2 geeignete Mittel sei, daß im letzteren Fall zwischen y, y1, y2 die und die Unterschiede und Wirkungsweise und – gegebenenfalls – der Rationalität bestehen, daß ihre Anwendung und also die Erreichung des Zweckes x die ‚Nebenfolgen' z, z1, z2 mit in den Kauf zu nehmen gebietet. Dass alles sind einfache Umkehrungen von Kausalsätzen und sowie sich daran ‚Wertungen' knüpfen lassen, sind sie ausschließlich solche des Rationalitätsgrades einer vorgestellten Handlung. Die Wertungen sind dann und nur dann eindeutig, wenn der ökonomische Zweck und die sozialen Struktur-Bedingungen fest gegeben sind und nur zwischen mehreren ökonomischen Mitteln zu wählen ist, und wenn diese überdies ausschließlich in Bezug auf die Sicherheit, Schnelligkeit und quantitative Ergiebigkeit des Erfolges verschieden, in jeder anderen für menschliche Interessen möglicherweise wichtigen Hinsicht aber völlig identisch funktionieren (Weber 1973d [1917]: 529 [491], vgl. auch S. 517 [479], S. 538 [500] sowie S. 526 [488]).

> in fact the mere inversion of the sentence: x follows y[43] (Weber 1973d [1917]: 517 [479], see p. 529 [491], 538 [500] and 526 [488]); my translation).

When it comes to causal analyzes (*nota bene* only there!), Weber follows the principles formulated by Bacon and is of the opinion that technical regulations can be provided by inverting causal propositions. Just like later Karl Raimund Popper:

> This make it clear how, from a logical point of view, both the derivation of predictions and the technical application of scientific may be regarded as mere *inversions* [my emphasis] of the basic schema of scientific explanation (Popper 1972: 353).

For political science with regard to the importance of practice in particular, Adrienne Héritier has formulated this as follows:

> In other words, if theory guided hypotheses are not logically consistent, the causal relations derived from them would be flawed. If in turn, policy recommendations would derived from latter, the policy recommendations would be detrimental rather than beneficial (Héritier 2016: 23).

Today, due to the development of logic and philosophy of language, one can formulate much more thoroughly and differentiated what Weber and Popper meant by that. The first proposition is a *causal and empirical statement* of the form *if x then y*. The inverse is a *(technical) rule* of the form *if you want to achieve y, then do x*. There is only a *pragmatic*, but *no logical, relationship* between law-like statements or propositions, e.g., if A, then B, and associated (technical) rules or instructions, e.g., B by A, if you want to achieve B, then try A (Kornwachs 2008: 139 and Kornwachs 2012: 64 ff.). There is a difference

> between statements A and B and the associated action A or a real state B that is brought about by action A[44] (Kornwachs 2012: 65; my translation).

Kornwachs adopts this notation from Mario Bunge (1967b).

> The pragmatic syllogism is a result of the pragmatic interpretation of a deductive-nomological explanation and its linking to a normative sentence, e.g. that B is desired. Bunge sometimes calls this expression a technological rule[45] (Kornwachs 2012, 67; my translation; see Bunge 1967, 132-139).

Mario Bunge's subchapter is called „Technological Rule“ (Bunge 1967b: 132-139). Bunge uses the expressions „nomological statement“ and „nomopragmatic statement.“

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[43] Nur wo bei einem absolut eindeutig gegebenen Zweck nach dem dafür geeigneten Mittel gefragt wird, handelt es sich um eine wirklich empirisch entscheidende Frage. Der Satz: x ist das einzige Mittel für y, ist in der Tat die bloße Umkehrung des Satzes: auf x folgt y. (Weber 1973d [1917]: 517 [479], vgl. auch S. 529 [491], 538 [500] sowie 526 [488]).

[44] zwischen den Aussagen **A** und **B** und der zugehörigen Handlung **A** oder eines realen Zustands **B**, der durch die Handlung **A** ins Werk gesetzt wird (Kornwachs 2012: 65).

[45] Der pragmatische Syllogismus ist ein Ergebnis der pragmatischen Interpretation einer deduktiv-nomologischen Erklärung und deren Verknüpfung mit einem normativen Satz, z.B., dass B gewünscht werde. Bunge nennt diesen Ausdruck zuweilen technologische Regel (Kornwachs 2012: 67).

The principal criticism of this, which surprisingly comes especially from the philosophy of technology, is formulated in much more detail in the next section (12th chart, section 9.4.12).

Why do *causalists* not recognize that the equivalence described above is necessary or must be presupposed, if one carries out inversions of causal propositions?

The answer is that all those who follow Weber and Popper, such as Hans Albert, *wrongly assume* that the inversion of causal propositions is a *tautological transformation* that needs no additional premises:

> To transform a theoretical into a technological system requires certain logical operations. Since it is a tautological transformation of the system in question, you do not need additional premises. The informational content of a technological system is in no way beyond its theoretical foundation[46] (Albert 1967b [1965], 192; my translation).

This assumption is also *untenable* for still other reasons, as stated above.

Bacon is right, without the assumption of equivalence between causality and action, this transformation is not possible. Only under this assumption can one *transform cognition* (theory) *into action* (practice), i.e., into *social technology*, by "inversions of causal propositions" (Weber 1973d [1917]: 529 [491] or by "inversions of the basic schema of scientific explanation" (Popper 1972: 353).

Without addressing the *hidden assumptions* of the explanative-prognostic or the Platonic-Galilean tradition, one can neither clarify the misunderstandings in the science war nor understand the *difference* between *applied* and *practical science*. Therefore, the three most important hidden assumptions are listed again here:

1. *causality* as an invisible and hidden force that holds the world together at its core,
2. *equivalence* between causality and action,
3. *inversions* of causal propositions or inversion of the fundamental explanatory scheme are tautological transformations.

In addition, a *visible* component is the preference for a *causal* and *empirical* approach and a *logical-mathematical research methodology*.

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[46] Um ein theoretisches in ein technologisches System zu transformieren, bedarf es bestimmter logischer Operationen. Da es sich um eine tautologische Transformation des betreffenden Systems handelt, benötigt man keine zusätzlichen Prämissen. Der Informationsgehalt eines technologischen Systems geht in keiner Weise über den seiner theoretischen Grundlage hinaus (Albert 1967b [1965]: 192).

C. Applied instead of practical social sciences

However, the equivalence between causality and action and thus an equivalence between statements and rules also leads to the division into empirical (theoretical) and applied sciences. Within the former, causalities are determined, the *applied* sciences only have to invert them. Thus, instructions or advice can be formulated quasi incidentally as part of a well-applied (*not practical!*) political science. This is possible because with the determination of causalities the world is firstly recognized and secondly can be changed.

Thereby, the ethical-normative as well as pragmatic dimensions are not addressed at all, as it has been done in practical philosophy since antiquity (section 5.2.3). Only a "halved", "instrumental reason" (Horkheimer 1967 [1947]) is at work here. Only a *technical ought* or social-technological rules can be scientifically justified, following the explanative-prognostic or the Platonic-Galilean tradition. Neither a pragmatic nor a normative ought can even be aspired to. Normative as well as pragmatic questions are explicitly excluded. Normative and pragmatic discourses are a concern of the scientistic sciences only insofar as the discussion of means is concerned. This is also clearly recognized by the adepts:

> One must not confuse the relevance of a technological system with a legitimacy for its practical application[47] (Albert 1967b [1965]: 193; my translation, see Dasgupta 2009).

The criticism of the Frankfurt School is still shared, so Bo Rothstein speaks of this causal and empiricist orientation producing only "technically competent barbarians" (Rothstein 2005). Now this overshoots the mark because there are also visible assumptions, e.g. liberal and utilitarian principles, which contain the normative values for which means are then formulated, for instance, by means of the normative rational choice approach. The normative (liberal and utilitarian) values that are actually presupposed by the rational choice approach cannot, however, be legitimized by the same rational choice approach (section 6.10).

With the help of "inversions of causal propositions" (Weber 1973d [1917]: 529), "inversions of the basic schema of scientific explanation" (Popper 1972: 353) or the pragmatic syllogism (Bunge 1967b: 134), applied rules are inferred from empirical statements by means of analogy (*nota bene*: with a formally invalid mode of argumentation). Thus, results of *empirical* sciences are transformed into *results* for *applied sciences* (12. Chart, section 9.4.12).

Crucially, according to the scientistic view, this means that a *genuinely practical methodology is no longer necessary.* This methodological reductionism, as I will demonstrate in more detail in the next section, is still assumed today within the

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[47] Man darf die Relevanz eines technologischen Systems nicht mit einer Legitimation für seine praktische Anwendung verwechseln (Albert 1967b [1965]: 193).

explanative-prognostic or the Platonic-Galilean tradition. When one speaks of applied sciences, this does not propagate an independent methodology; on the contrary, the adjective "empirical" often still emphatically indicates that one uses the same methodology as empirical sciences. The difference lies in the fact that one deals with questions that can be quickly implemented in practice.

The influence of Weber and Popper on the application of cognition (theory) into action (practice), i.e. transformation of empirical knowledge into applied practice, is not only enormous, but meanwhile so self-evident that inversions of causal propositions or inversions of the fundamental explanatory scheme today belong to the implicit and unspoken presuppositions (hidden and tacit assumptions), which are very rarely stated as such or even addressed.

Popper, in contrast to Weber, also examined the practical aspects more closely and published on the requirements of an applied social technology (Popper 1980b [1944], 1980b [1944] and 2003 [1957]).

The transformation of causalities is not as trivial as it seems, since it is supposed to be a matter of *tautological* transformations. The formulation of social-technological regulations then requires some "imaginative effort", as Hans Albert also admits:

> What logically appears as a tautological transformation of theoretical statements into relevant technological statements is thus practically in many cases a considerable feat of imagination. The reason for this fact, which seems strange in itself, lies in the fact that even the conception of certain logical connections, possibilities of deduction and consequences is not mechanizable in important cases[48] (Albert 1967b [1965]: 197; my translation).

Neither the perestroikans nor their opponents discuss these connections in the books mentioned above (in fact, these connections should have appeared in the contribution "Normative Methodology" by Russel Hardin (2011 [2009]), see next section).

In the following, two quotations from textbooks for political scientists are which, as exceptions, confirm the above rule, since they at least indicate that a transformation is being made, even if neither the justification for it nor the how is addressed:

> Purely empirical contextual knowledge can also serve the realization of values. If/then-statements that grasp contexts can be 'normatively loaded' and thus be transformed into practically useful instructions for action: If, within the framework of a normative argument, one of the components of an empirically true if/then statement is asserted as ought, then the information content of that if/then statement can be used to realize an

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[48] Was sich logisch als eine tautologische Transformation theoretischer in relevante technologische Aussagen darstellt, ist also praktisch vielfach eine beachtliche Phantasieleistung. Die Begründung für diesen an sich seltsam anmutenden Tatbestand liegt darin, daß auch die Auffassung bestimmter logischer Zusammenhänge, Ableitungsmöglichkeiten und Konsequenzen in wichtigen Fällen nicht mechanisierbar ist (Albert 1967b [1965]: 197).

> ought by formulating the other component as a command[49] (Patzelt 1986: 204; my translation).
>
> The development of action rules is a component of policy field analysis. Like empirical research, it initially aims at the development of causal if-then statements; however, these are 'normatively loaded' (Patzelt 1986: 204, [see the previous quotation]) and thereby establish a relation to political practice. This is done by formulating the if-constituent of the causal statement as an instruction for action, while the then-constituent is expressed as an aspirational state. Thus, the causal statements are transformed into purpose-oriented statements (Schubert 1995: 283 ff.)[50] (Kortmann/Schubert 2006: 47-48; my translation, see 2nd chart, p. 52).

4.1.5 Applied methodology or purely technical methodology within the explanatory-prognostic tradition: normative rational choice theory

After discussing the hidden epistemological presuppositions of applied (political) science, the following section examines Russel Hardin's (2011 [2009]) article “Normative Methodology” to see how practical knowledge can be generated within the explanative-prognostic or the Platonic-Galilean tradition. This is the only contribution within the volume “Political Methodology” (Box-Steffensmeier/Brady/Collier, 2010a [2008]) that deals with practical-normative methodology. Here, only the goals will be described; the rational choice approach will be discussed in detail later (section 6.10).

Also, in the “normative methodology” of the explanative-prognostic or the platonic-galilean tradition causal reductionism in the form of a *normative rational-choice theory* claims the sole dominance. This theory is virtually put forward as the only usable normative methodology and theory (both are equated with it, see section 6.10), all other normative theories are even dismissed as esoteric and irrelevant:

> Two of the methods, shared-value and contractarian arguments, threaten to be narrowed down to use by academic moral theorists with little resonance beyond that narrow community. Any method that becomes as esoteric as much of contemporary moral theory

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[49] Der Wertverwirklichung kann auch rein empirisches *Zusammenhangswissen* dienen. Zusammenhänge erfassende Wenn/Dann-Aussagen lassen sich nämlich ‚normativ aufladen‘ und dadurch in praktisch nützliche Handlungsanweisungen umsetzen: Wird im Rahmen eines normativen Arguments *eine* der Komponenten einer empirisch wahren Wenn/Dann-Aussage als *gesollt* behauptet, so läßt sich der Informationsgehalt jener Wenn/Dann-Aussage zur Verwirklichung des Gesollten nutzen, indem man die *andere* Komponente als *Gebot* formuliert (Patzelt 1986: 204).

[50] Die Entwicklung von Handlungsanweisungen ist ein Bestandteil der Politikfeldanalyse. Wie die empirische Forschung zielt diese zunächst auf die Entwicklung von kausalen wenn-dann-Aussagen; diese werden jedoch ‚normativ aufgeladen‘ (Patzelt 1986: 204) und stellen dadurch einen Bezug zur politischen Praxis her. Dieses geschieht dadurch, dass der Wenn-Bestandteil der kausalen Aussage als Handlungsanweisung formuliert, während der Dann-Bestandteil als erstrebender Zustand ausgedrückt wird. Somit werden die kausalen Aussagen im Ergebnis also zu zweckorientierten um-zu-Aussagen umgeformt (Schubert 1995: 283 ff.) (Kortmann/Schubert 2006: 47-48, siehe 2. Schaubild, S. 52).

> has become is apt to be ignored and even dismissed by the overwhelming majority of social theorists as irrelevant.
> […]
> Over the past four or five decades, rational-choice normative theory, the third major branch of contemporary normative methodology [conflict theories and contractualism are the other two], has become a vast program that increasingly leaves the other two branches behind in its scope and sheer quantity of work. This development is made more readily possible by the clarity and systematic structure of game theory and game-theoretic rational choice. Game theory and rational choice methodology are very well laid out and easily put to use. Perhaps at least partially because of that fact, rational choice methods are taking over normative theorizing and theories (Hardin 2011 [2009]: 99).

Quantitative analyses supporting the assessment of the prevalence of normative rational choice theory are not provided in this article, nor are external works referenced. It is important to note that in the methods volume "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]) this approach is put forward as the only promising one, and other approaches are simply ignored. Second, it is much more important whether this normative rational choice theory can also provide an adequate normative (practical) methodology.

What are the goals of this approach, which is also supposed to be a methodology? The rational choice approach serves to explain the world empirically by means of if-then statements, but also to change the world by means of social technological rules. Empirical political scientists determine the invisible causalities, and the social technological consequences can then be created within an applied political science by transforming causal propositions as explained in the upper subsection.

What is important for this book now is *not* how to use game theory to formulate social technological regulations. According to Hardin, this is relatively straightforward:

> Game theory and rational choice methodology are very well laid out and easily put to use (Hardin 2011 [2009]: 99, section 6.10).

Of importance is the character of a practical discourse that works with the help of this methodological approach: The normative rational choice approach is a *technical discourse of means* (9th chart, section 9.4.9, and 10th chart, section 9.4.10), which rejects from the outset any legitimating goal and cannot afford it at all. All values, norms and goals, they are usually liberal and utilitarian value concepts, cannot be justified, but are simply assumed and treated as assumptions, just like other ontological and epistemological (gnosiological) concepts. In other words, it is only about *technical means discourses*, any *legitimatory* intention is simply relegated to the underlying ontological (individualism, self-interest) and axiological assumptions (pragmatism, and utilitarianism). These assumptions cannot now be justified by the rational choice approach.

4.1.6 Applied methodology of the perestroicans: applied phronesis, practical wisdom, practical reason

Flyvbjerg and the phronetic perestroikans want to establish a *better* and, above all, a *relevant* alternative to current social and political science by resorting to Aristotelian phronesis and including questions of power: a phronetic or real social science. Thus, the scientistic view, which is oriented towards the natural sciences, is dismissed as inauthentic:

> MSSM [MSSM meaning the book by Flyvbjerg (2001)] reinterpreted the Aristotelian concept of phronesis to include issues of power and explained that building on this new version of phronesis is the best bet for the relevance of the social sciences in society. [...]
>
> The book provided a thorough analysis of how its alternative social science is dedicated to enhancing a socially relevant form of knowledge, that is, 'phronesis' (practical wisdom on how to address and act on social problems in a particular context) (Flyvbjerg/Landman/Schram 2012a: 1)

The causalities that scientistic scientists can determine are certainly also socially relevant. The justified criticism of the Frankfurt School or the conservative, normative-ontological theory is that one not only has to generate technical, applied knowledge, but that it should also be the task of the sciences to legitimize this knowledge.

Can the real or phronetic social scientists achieve this goal? Even with the applied phronesis it is only enough for the generation of technical knowledge. A pragmatic target discourse can also *not* be conducted. The general goal is:

> [T]o make the world a better place. (Flyvbjerg/Landman/Schram 2012b: 11)

This goal was similarly formulated by Bacon as well as the pioneers of American political science (section 2.2, 4.2).

Who now knows what is better, and above all who justifies this and how is this justified? The real social scientist knows what is better; unfortunately, where and how he determines this knowledge is not addressed: A practical science, however, must not only answer this question, but also justify this. Here the perestroikans actually offer once the moral attitudes of the researching scientists or the groups concerned:

> [W]here 'better' is defined by the values of phronetic researchers and their reference groups. (Flyvbjerg/Landman/Schram 2012c: 290)

Three pages later, this is already rendered obsolete and replaced by a context-dependent common opinion. At the same time, universalism is rejected; socialization and one's own history are supposed to offer an effective remedy against relativism and nihilism:

> [T]he normative basis for applied phronesis, and for problematizing tension points, is the attitude among those who problematize and act, and this attitude is not based on idiosyncratic moral or personal preferences, but on a context-dependent common world view and interests among a reference group, well aware that different groups typically

> have different world views and different interests, and that there exists no general principle by which all differences can be resolved, no view from nowhere. For phronetic social scientists, the socially and historically conditioned context, and not fictive universals, constitutes the most effective bulwark against relativism and nihilism and is the best basis for action. Our sociality and history is the only foundation we have, the only solid ground under our feet (Flyvbjerg/Landman/Schram 2012c: 293).

Unfortunately, they do not elaborate on how applied phronesis can achieve these goals and what other scientific tools are needed to do so. However, the perestroikans are convinced that a social science oriented towards natural science cannot achieve these goals:

> Intelligent social action requires phronesis, to which the social sciences can best contribute and the natural sciences cannot with their emphasis on 'epistemé' (universal truth) and 'techné' (technical know-how) (Flyvbjerg/Landman/Schram 2012b: 1).

Only priests seek and proclaim universal truths; neither natural scientists nor the social scientists as Flyvbjerg criticizes have any such claims at all. Scientistic scientists seek knowledge that can be rationally justified. The knowledge they find is hypothetical in character for reasons of principle; the if-then structure of scientific knowledge is one of the usually implicit presuppositions of any scientific inquiry (section 5.4.7 and chapter 6).

The goal of phronetical perestroicans is to use applied phronesis to create a problem-oriented methodology (Flyvbjerg/Landman/Schram 2012c: 285). Where applied means applying knowledge that has emerged from context:

> In phronetic social science, 'applied' means thinking about practice and action with a point of departure not in top-down, decontextualized theory and rules, but in 'bottom-up' contextual and action-oriented knowledge, teased out from the context and actions under study by asking and answering the value-rational questions that stand at the core of phronetic social science (Schram 1995) (Flyvbjerg/Landman/Schram 2012c: 286).

Phronetic scientists strive for a revolutionary philosophy or *philosophy of engagement*, with the help of which one can change unjust conditions. Thereby, the applied phronesis consists not only in the practical knowledge of how to transform unjust states. First, one must discover the tension points. Second, one should have a revolutionary *ability* or *skill* to transform these tension points:

> What ist applied is not theory, but a philosophy of engagement that recognizes that phronesis is a skill and that having phronesis is iteratively dependent on practising phronesis (Flyvbjerg/Landman/Schram 2012c: 286).

In doing so, they act together with the affected groups, giving them the knowledge and practical capability or skills to assert their concerns:

> In each case, the students and instructor rely on phronetics in the sense of working with affected communities to achieve empowerment (Flyvbjerg/Landman/Schram 2012a: 10).

Practical knowledge and practical capability are not differentiated:

> This practical wisdom seems to have three aspects: it is content, a quality of persons and a form of action. As content, phronesis is a resource – a stock of experiential knowledge. As a quality of persons, it is what enables acquisition and appropriate use of that knowledge – a capacity. And as action, phronesis necessarily involve doing something – a practice in which experiential knowledge is both used and gained. 'Having phronesis' is iteratively dependent on 'practising phronesis' (Flyvbjerg/Landman/Schram 2012a: 4).

A more detailed criticism can be found in the next section (chapter 4.2).

So scientistic scientists are in this view *scholars* and *revolutionaries* in one person. This does not do justice to modern specialization, which differentiates knowledge and capability, nor to the complexity of the subject. Political scientists are already very challenged today to develop even just a skill in the form of knowledge for individual areas of their subject, i.e. they are busy establishing knowledge in a special area. Participation and commitment as a citizen are certainly also appropriate in a democracy, but cannot be demanded as a task for scientists. There is a difference between scientists and politicians (section 5.2.4, 10th chart, section 9.4.10).

4.2 Tasks and objectives of (political) scientific research

- Which *tasks* and *objectives* should be pursued within political science?
- Can and should science contribute to *world change* or problem solving?

The separation between non-epistemic and epistemic values can only be made ideally. In the process, there are also overlaps. This becomes apparent in this subsection. Tasks and goals of (political) scientific research in the narrower sense can be called non-epistemic values, since these values are brought to science not only by scientists but also from outside.

In the following I will also explain concretely how the self-imposed tasks, e.g. the determination of causalities, should be implemented in a very concrete way. However, these are epistemic values in a narrower sense, because they determine how causalities are to be investigated and which procedure is to be applied. In this subsection as well as in chapters five and six, where further epistemic values are discussed, it becomes apparent that epistemic values have firstly evolved over the centuries, and that this process is still continuing.

First, the general tasks and goals that scientists strive for are presented (4.2.1). Then the scientistic, positivist and naturalist view is explained (4.2.2) then the scientist who orients himself towards the cultural or human sciences (humanities) and sees himself as an anti-positivist or alternative (4.2.3). The end of this section discusses the perestroikans and their phronetic political science, which have emerged as the latest revolutionary alternative to the scientistic view (4.2.4).

4.2.1 General tasks and goals of scientific research: knowledge generation for world cognition and world change

Knowledge generation for world cognition and world change has been the most important goal of the sciences since antiquity, with a primacy of practice. The primacy of practice is not questioned in the explanative-prognostic or the Platonic-Galilean tradition, as not only the perestroikans *falsely* claim. The opposite is the case:

> The true and legitimate goal of science is to endow human life with new discoveries and resources[51] (Bacon 2000 [1620]: 66).

Even more: The practical benefits of the sciences are regarded as even greater than those of the practically active politicians, since they are supposed to benefit mankind without disadvantages and that still for all times, while the benefits of the politicians work only for a certain time in certain places and these must be enforced even by force:

> For the benefits of the discoveries may extend to the whole human race, political benefits only to specific areas; and political benefits last no more than a few years, the benefits of discoveries for virtually all time. The improvement of a political condition usually entails violence and disturbance; but discoveries make men happy, and bring benefit without hurt or sorrow to anyone[52] (Bacon 2000 [1620]: 99, 129. aphorism).

The practical self-interest of scientists and their patrons was thus summed up by Bacon:

> Despite his strangeness to mathematics, Bacon struck a chord with the spirit of science that followed him [...]
> [T]he mind that conquers superstition is said to rule over nature unenchanted. [...]
> What people want to learn from nature is how to use it in order to completely dominate it and people[53] (Horkheimer/Adorno 2010 [1947]: 14; my translation).

Ever since Francis Bacon, science has found it very difficult to use knowledge as an end in itself. Knowledge is thus, contrary to the assertion of the phronetic perestroikans (Green/Shapiro 1994, Shapiro 2005, Schram 2003 and 2005), always regarded as knowledge in the service of humanity and thus, like the perestroikans

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[51] Meta autem scientiarum vera et legitima non alia est, quam ut dotetur vita humana novis inventis et copiis (Bacon 1990 [1620]: 172, 81. aphorism, volume 1).

[52] Etinem inventorum beneficia ad universum genus humanum pertinere possunt, civilia ad certas tantummodo hominem sedes: haec etiam non ultra paucas aetates durant, illia quasi perpetuis temporibus. Atque status emendatio in civilibus non sine vi et perturbatione plerumque procedit: at inventa beant, et beneficium deferunt absque alicujus injuria aut tristitia (Bacon 1990 [1620]: 268, 129. aphorisme, volume 1).

[53] Trotz seiner Fremdheit zur Mathematik hat Bacon die Gesinnung der Wissenschaft, die auf ihn folgte, gut getroffen. [...]
[D]er Verstand, der den Aberglauben besiegt, soll über die entzauberte Natur gebieten. [...] Was die Menschen von der Natur lernen wollen, ist, sie anzuwenden, um sie und die Menschen vollends zu beherrschen (Horkheimer/Adorno 2010 [1947]: 14).

assert, problem-oriented or problem-driven. With the concept of interests that guide knowledge Jürgen Habermas (1968b) even assigns an interest to every science, the natural sciences in mastering nature, the humanities and cultural studies in orientation and understanding, and the critical sciences in enlightenment and emancipation.

In the spirit of Bacon, Karl Raimund Popper formulated the goal of scientific research in the 20th century as follows:

> The task of science is partly theoretical - *explanation* - and partly practical - *prediction and technical application*. I shall try to show that these two aims are, in a way, two different aspects of one and the same activity (Popper 1972: 349, emphasis in the original).

World knowledge as a means to change the world is also seen by the scientistic establishment as the most important goal of political science since its emergence in the United States at the beginning of the 20th century:

> The founding idea of American political science was one of the discipline 'as a source of knowledge with practical significance' (Gunnell 2006, 485) (quoted from Goodin 2011a [2009]: 7).

Both goals are still pursued today by both the liberal establishment and mainstream political science.

The relevance of practice was even emphasized by the two „revolutions" considered very formal and technical, the behavioralist revolution and the rational choice revolution. A central concern of the behavioralist approach was:

> Instead of 'pure research', political science should conduct applied research to solve specific political problems and innovative programs[54] (von Beyme 2000 [1972]: 113).

This applies all the more to the rational choice approach, where the focus is on *maximizing utility* or reducing the complexity of the reality, above all with a view to *practical utilization of knowledge* (Coleman 1990, Braun 1999, von Beyme 2000 [1972]: 122-150).

Therefore, the criticism of the perestroikans in this regard is not appropriate, because within the explanative-prognostic or the Platonic-Galilean tradition it is not only a matter of ascertaining the truth (section 5.3). Schram draws on Jacqueline Stevens' distinction between „science as use" and „science as truth" (quoted from Schram 2003: 850). He refers to American pragmatism, but, like many perestroikans, does not recognize that for scientistic scientists, just as in pragmatism, knowledge and action are two sides of the same coin, American pragmatism argues in a similar way:

> Charles S. Pierce's pragmatic maxim was that there is *an inseparable connection between rational knowledge and rational purpose* [emphasis in the original]. Terms should be understood like tools based on their functionality. Metaphysical questions about the

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[54] Die Politikwissenschaft soll statt ‚reiner Forschung' *angewandte Forschung* zur Lösung bestimmter politischer Probleme und innovatorischer Programme treiben (von Beyme 2000 [1972]: 113).

> ultimate things should give way to practical problems[55] (Ruffing 2005: 246; my translation, cf. James 1907, Popper 1972: 349 argues similarly).

Even if the *general goals*, world cognition and world change, are the same for all traditions, there are differences in the *concrete* goals that both traditions pursue or that both believe they can achieve.

What is the difference between the Aristotelian and the Platonic-Galilean tradition or to what extent are different goals pursued? To anticipate the answer: First of all, there is the difference between causal thinking and understanding (*Verstehen*) of meaning or sense making (*Sinnverstehen).* Second, the difference between practical and applied sciences (including political science). In the case of scientistic scientists who orientate themselves towards the natural sciences (causal thinking, quantitative and qualitative-mathematical methodology), an exaggeration of explanations goes hand in hand with a disdain for understanding (descriptions of meanings, and contexts of meaning using a qualitative-interpretative methodology). Interpretative methods (hermeneutics, phenomenology, qualitative-interpretative methods), which are primarily descriptive-interpretative, are neglected.

4.2.2 Knowledge generation or world cognition as an explanation of the world using logic and mathematics. Determination of invisible causalities within scientistic (political) sciences

A. Definition and meaning of causality. Distinction from correlation, co-occurrence and collocation

The proponents of causal reductionism claim that causality plays the decisive and exclusive role in both world knowledge (cognition) and world change (action). With Johann Wolfgang von Goethe[56] one could say: causality is what holds the world together at its core. Therefore, like Goethe's Faust causal reductionist are solely in search only for what holds the world together at its core.

Within the Anglo-Saxon discussion, this poetic image plays no role, following the pragmatic tradition one finds a pragmatic metaphor of an Anglo-Saxon philosopher (John Leslie Mackie was Australian) namely the image of causality as cement of the universe, „The Cement of the Universe, a Study of Causation” (Mackie 1974), the programmatic title of his much-cited book.

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[55] Die pragmatische Maxime von Charles S. Pierce lautete, dass es *eine untrennbare Verbindung zwischen rationaler Erkenntnis und rationalem Zweck* gebe [Hervorhebung im Original]. Begriffe seien wie Werkzeuge über ihre Funktionalität zu verstehen. Metaphysische Fragen nach den letzten Dingen sollten lebenspraktischen Problemstellungen weichen (Ruffing 2005: 246).

[56] That I recognize what holds the world / Together at its core.
Daß ich erkenne, was die Welt / Im Innersten zusammenhält
(von Goethe, 1978 [1808]: 162 [382-383]).

In this case, the poetic metaphor is *more apt* and useful, since causality, according to the naturalistic view, is an *invisible* and *hidden force* or *relation* that holds the world together at its core. Cement, on the other hand, is a *visible* material.

While phenomena or appearances are *visible* (the Greek word „*phainómenon*" means visible, appearance), causality is *invisible*, so appearances can be *described*, but causalities can only be *explained*:

> Taking 'description' in the narrower sense which includes only description of appearances, the realist can say that explanatory knowledge is knowledge of the underlying mechanisms – causal or otherwise – that produce the phenomena we want to explain. To explain is to expose the internal workings, to lay bare the hidden mechanisms, to open the black boxes nature presents to us (Salmon 1989: 134).

This is not only seen by philosophers of science, but is also taught in the corresponding textbooks on political science:

> Obviously, we do not thereby mean that one direct *observes* causation. Rather, this involves *inference*, not *direct observation* (Seawright/Collier 2010 [2004]: 318, emphasis in the original).

It is only secondarily about observations, but primarily about inferences.

What's more, naturalistic scientists see the goal of science solely as generating deductions, inferences or conclusions („*inference*" is the magic word) and thus identifying the invisible causalities suspected behind the phenomena or appearances:

> **The goal is inference** [emphasis in the original]. Scientific research is designed to make descriptive-interpretative or explanatory *inferences* on the bases of empirical information about the world. Careful descriptions of specific phenomena are often indispensable to scientific research, but the accumulation of facts alone is not sufficient. […]
> [B]ut our particular definition of science requires the additional step of attempting to infer beyond the immediate data to something broader that is not directly observed. That something may involve *descriptive-interpretative inference* – using observations from the world to learn about other unobserved facts. Or that something may involve *causal inference* – learning about causal effects from the data observed. […]
> [T]he key distinguishing mark of scientific research is the goal of making inferences that go beyond the particular observations collected (King/Keohane/Verba 1994: 7-8, all emphasis in the original).

As I will show, the distinction between „description" and „*descriptive inference*" on the one hand and „explanation" and „*causal inference*" on the other hand causes confusion. The word „inference" is used to show that it is an inference to something invisible. In the terminology of King, Keohane, and Verba, this is a descriptive inference of a causal mechanism or process and not a phenomenological description or a *description of appearances*, as Salmon (1989: 134) writes. This leads to confusion.

It would be better to speak only of *explanations* that explain invisible *causal regularities* at the macro level and *causal mechanisms* or causal processes at the micro level. The term *causal inference* could be used to identify causalities at all levels. Furthermore, descriptions should only be used when describing visible phenomena (section 6.9).

Causal thinking is widespread today, especially within the scientistic establishment of political science (Box-Steffensmeier/Brady/Collier 2010a [2008]: 4, cf. also Brady/ Collier/Box-Steffensmeier 2011 [2009]: 1006, 1022, and 1025, and Goodin 2011b [2009]: 13). Even more: It is supposed to form the center of explaining, even supposedly of understanding:

> Causality is at the center of explanation and understanding (Brady 2011 [2009]: 1054).

Henry E. Brady formulates the most widely held view of causality within political science at the beginning of the 21st century as follows:

> Causal statements explain events, allow predictions about the future, and make it possible to take actions to affect the future (Brady 2011 [2009]: 1054).

Causal statements should thus enable three different goals: First, to explain events; second, to make predictions about future developments; and third, to enable rules that can be used to justify or enable actions that change the world.

As a result, only causal statements provide scientifically based knowledge of the world by means of explanations and forecasts. At the same time, as the other side of the coin, so to speak, causal statements through "inversions of causal statements" (Weber 1973d [1917]: 529 [491] or through "inversion of the fundamental explanatory scheme" (Popper 1984 [1972]: 367) allow a world change and thus action (section 4.1.4).

Why *exclusively causality* can provide world knowledge and change is not answered by King, Keohane, and Verba (1994), Brady (2011 [2009]), or other authors in „Political Methodology“ (Box-Steffensmeier/Brady/Collier 2010a [2008]).

The orientation of political science and its methodology to the natural sciences is so obvious not only for these authors that they do not even mention the methodological naturalism and causal reductionism represented by it as a starting point.

For Merilee H. Salmon[57] the search for causalities in the social world is the most important question of a naturalistic philosophy of social science or a naturalistic methodology:

> [T]his chapter looks at the so-called social sciences with particular attention to whether we can investigate human behavior in the way scientists study the rest of the natural world. Because scientific studies are so centrally concerned with causal relationships, a question closely related to our main theme is how to understand *causation* [emphasis not in the original] in the social world. [...]
> Neither side [what is meant is individualism on the one hand, collectivism or holism on the other] in the dispute (actually, there are many different versions of both sides) denies

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[57] Merilee H. Salmon is the wife of Wesley C. Salmon, only this quote is from Mrs. Salmon, all other quotes are cited from Mr. Salmon's works.

> the obvious causal interplay between individuals and societies. Nevertheless, individualists and collectivists disagree about the ultimate causes of human behavior (Salmon 1992: 404).

In short, science, at least within the explanative-prognostic or the Platonic-Galilean tradition, is all about identifying causality; the predicate „scientific“ is granted only to causal studies. The meaning of causality is rarely formulated *expressis verbis*. That *causal reductionism* is involved, however, can be further demonstrated by quoting from a very influential political science methodology book:

> At its core, real explanation is always based on causal inference. We regard arguments in the literature about ‘noncausal explanation’ as confusing terminology; *in virtually all cases, these arguments are really about causal explanation* [emphasis not in the original] or are internally inconsistent (King/Keohane/Verba 1994: 75).

Explanation is exclusively associated with causal inference and non-causal explanation is even dismissed as confusing terminology. Yet the most discussed explanatory model of the 20th century, the deductive-nomological model, went far beyond this. Causal explanations were only one of several possible explanations. Since causality is the only relation on which science should focus, and all other relations are unimportant, I think the term *causal reductionism* is more accurate and therefore more appropriate.

Correlation (Latin: *correlatio*) refers to an interrelationship of two events that often occur simultaneously. In particular, scientists conducting research at the macro level look for correlations primarily in the hope that they can then be identified as causalities. If one finds causal regularities at the macro level, one could then deductively infer individual causalities at the micro level with the help of the deductive-nomological model (DN model), according to the deceptive hope spread especially by critical rationalism, as remains to be demonstrated (chapter 6.8).

Linguists speak of *cooccurrence* (Latin: *coocurrentia*) when *two lexical units* (e.g. words) appear together. Researchers oriented to the natural sciences look for correlations, while scientists oriented to the cultural sciences (humanities) look primarily for cooccurrences.

The search for cooccurrences is undertaken with regard to the fact that two terms are also dependent on each other if they frequently occur together. Thus, a *collocation* (Latin: *collocatio*) is present, if e.g. a grammatical or semantic dependence of two terms frequently occurring together is proven.

B. From correlation to causality

After the goals and the definition of causal thinking have been clarified, the following is about showing which methodological approaches can be used to determine causalities. While methods primarily ensure a scientific determination of data and facts, methodological approaches within a subject provide strategies for generating scientific theories (section 6.9 and 6.10).

Causal thinking requires different, very complex methodological approaches to identify causalities. These methodological approaches are based on corresponding *philosophical theories of causality*, which were developed primarily by philosophers of science. In this case, the methodological approaches presented by Brady (2011 [2009]) ultimately form the core of *four causal theories* (Salmon 1989), which have been operationalized in such a way that the procedure for finding causalities can be specified. The main purpose of these theories is to establish the properties of causality and criteria that can be used to distinguish valid from invalid causal explanations.

Brady (2011 [2009]) has presented four methodological approaches, *all* of which are necessary to identify causalities or explain events causally:

I. regularity or neo-Humean approach
II. counterfactual approach
III. manipulative approach
IV. mechanism and capacities approach

> A really good causal inference should satisfy requirements of all four approaches. Causal inference will be stronger to the extent that they are based upon finding all the following: (1) Constant conjunction of causes and effects required by the neo-Humean approach. (2) No effect when the cause is absent in the most similar world to where the cause is present as requirement by the counterfactual approach. (3) An effect after a cause is manipulated. (4) Activities and processes linking causes and effects required by the mechanism approach (Brady 2011 [2009]: 1055).

I. Regularity or neo-Humean approach to determination of causal regularities (correlations)

The regularity or neo-Humean approach goes back to David Hume (1999 [1748]), the counterfactual approach to a counterfactual theory of causality (Hume 1999 [1748], Mill (1968 [1843]), Weber 1973c [1906]), and in particular Lewis 2001 [1973]), the manipulative approach to a corresponding theory of causality (Woodward 2013), and the mechanism and capacity approach to scientific theory considerations of causal processes (Dowe 2008).

The *regularity approach* is used to establish a connection or conjunction between two events, technically speaking between two variables and their temporal occurrence. This is to prove a regular and *constant correlation between two events*:

> The regularity approach relies upon the constant conjunction of events and temporal precedence to identify causes and effects. Its primary tool is essentially the 'Method of Concomitant Variation' proposed by John Stuart Mill in which the causes of a phenomenon are sought in other phenomena which vary in a similar manner (Brady 2011 [2009]: 1083).

Quantitative-metric tools (concepts and methods, especially correlation and regression analyses) as well as deductive reasoning argumentation allow the identification of correlations, probabilistic laws or regularities. However, these tools *do not allow to* *distinguish false from true correlations* or probabilities. To exclude *coincidental correlations*, between which there are or can be no causalities, further methodological approaches are needed, namely both the *counterfactual* and the *manipulative* approach.

II. Counterfactual approach for the determination of causal regularities (correlations)

The *counterfactual approach* also goes back to David Hume (2007 [1739/1740), but also to John Stuart Mill (1973 [1843]) as well as to Maximilian Carl Emil Weber, namely his discussion with Eduard Meyer (Weber 1973g [1906]), but especially to the work of David Lewis (2001 [1973] and 1986) and his conception of *possible worlds*. The logical tools are found in particular in *modal logic* (Hughes/Cresswell 1978 [1968]).

With the help of experiments (thought experiments, but also laboratory and field experiments) and/or simulations, one can imagine *possible worlds*, but also artificially create worlds within experiments or simulations in which, for example, the cause does not appear, and then see what happens and what the world looks like.

> The counterfactual approach relies upon elaborations of the 'Method of Difference' to find causes by comparing instances where the phenomenon occurs and instances where it does not occur to see in what circumstances the situations differ. The counterfactual approach suggests searching for surrogates for the closest possible worlds where the putative cause does not occur to see how they differ from the situation where the cause did occur (Brady 2011 [2009]: 1083).

Causality has a *symmetrical property* between cause and effect, i.e., both cause and effect must always be present, and an *asymmetrical property*; the latter is that a cause produces an effect but not vice versa (Brady 2011 [2009]: 1083).

The regulative and counterfactual approaches can be used to identify correlations and probabilities, and thus *confirm* the *simultaneous presence* of two variables, but they *cannot identify the cause* (independent, explanatory variable) or the effect (dependent, explained variable), i.e., they can be used to identify the *symmetric* but not the *asymmetric* property of causality.

III. Manipulative approach for the determination of causal regularities

The manipulative approach, which also makes use of experiments and/or simulations, is primarily intended to find out the *direction* of causality or the *arrow of time* and thus identify one variable as a *cause* and the other variable as an *effect*:

> In an experiment there is a readily available piece of information that we have overlooked so far because it is not mentioned in the counterfactual approach. The factor that has been manipulated can determine the direction of causality and help to rule out spurious correlation. The manipulated factor must be the cause (Brady 2011 [2009]: 1076).

C. First digression: from correlation to causality or from regularity at the macro level to causal process at the micro level. Pairing problem and cause-effect mechanism

With the first three approaches, firstly, correlations on the macro level were determined, secondly, coincidental correlations were excluded or only correct ones were identified. Thirdly, the temporal question was solved, which event precedes, thus cause and effect could be identified. But did they find causalities? No,

> since correlations do not explain phenomena, but themselves require explanation[58] (von Beyme 2000 [1972]: 175; my translation).

One has, in other words, in these three approaches, identified causal regularities or patterns at the macro level and thus only the *nomological property* of causality (Salmon 1989).

These approaches are macro-level studies that cannot answer the following questions: How can a specific correlation at the micro level be identified? How does the causal mechanism work? The first question is about solving the *pairing problem* at the micro level, with the micro level simply forming each case. The second question aims at the *ontic conception* of causality (Salmon 1989: 129 und 182). In other words, causality also has an ontological property as well as being nomological.

Regulative, counterfactual, and manipulative theories of causation, methodically generated using correlation methods, experiments, and simulations, can demonstrate the nomological aspect of causality and even partially answer the why question. However, the question of how is an ontological question and should above all explain how a cause produces an effect. Macroanalysis consisting of hypothesis-testing methods is therefore not enough to move from correlation to causality; microanalysis is required.

The *deductive-nomological model* (DN model) of explanation was considered during the 19th and 20th century as the adequate approach to infer from the macro to the micro level or to subsume the individual case under a general regularity (law). Why this is not possible or, more precisely, only possible in a deterministic world, I show in another chapter (section 6.8).

Georg Henrik von Wright (1971) distinguishes *two main types of causa*l *explanation*, which logically differ from each other as follows:

- First, there are explanations by means of *sufficient conditions*, which is the first main type of causal explanations, thus explaining *why questions (warum-Fragen).* This would include, if Brady's four approaches are covered by this

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[58] da Korrelationen Erscheinungen nicht erklären, sondern selbst der Erklärung bedürfen (von Beyme 2000 [1972]: 175).

terminology, all explanations that are given on the basis of the regulative, counterfactual and manipulative approaches by means of correlation analyses, experiments and simulations on the macro level determine regularities and thus the *nomological* property of causality.

- The second main type of causal explanations (more precisely causal mechanisms or causal processes) contains explanations with the help of *necessary conditions*, thus the *ontic* property of causality can be recognized with the help of how-questions. Among these, von Wright also includes *quasiteleological explanations*, which are primarily at home in the biological sciences, have a *teleological terminology*, and *can* be reduced to causal explanations.

This distinction has not caught on; the analyses of Wesley C. Salmon, among others (see „Four Decades of Scientific Explanation“ Salmon 1989), have been more influential. These analyses make a similar distinction, but are less interested in logical aspects or modes of argumentation as von Wright's analyses, and more interested in a theory of causality. Salmon refers to Carnap's Logical Foundations of Probability (Carnap 1963 [1950]) and distinguishes between two different forms of explanations:

> Let us identify explanation1 with causal/mechanistic explanation. It could fairly be said, I believe, that mechanistic explanations tell us how the world works. These explanations are local sense [...]
> they explain particular phenomena in terms of collections of particular causal processes and interactions [...].
> Explanation2 then becomes explanation by unification. Explanation in this sense is, as Friedman emphasized, global; it relates to the structure of the whole universe (Salmon 1989: 184).

These two forms of explanations are not incompatible with each other, but complement each other. This is the case because they deal with different questions, one time the why-question, another time the how-question:

> These two ways of regarding explanation are *not incompatible* with another; each one offers a reasonable way of construing explanations. Indeed, they may be taken as representing two different, but compatible, aspects of scientific explanation (Salmon 1989: 183).

The scientists who work with a naturalistic methodology want to explain the world with the help of causal analyses. The orientation to physics gave way to the orientation to biology in the 20th century. In the process, the micro-level in particular came increasingly into focus and the DN model of explanation became obsolete, i.e., the pairing problem at the micro-level cannot be solved by subsumption (section 6.8). For this reason alone, independent micro-level studies are needed. This is then compounded by another problem.

Since the 1970s, it has been obvious that, in addition to correlation analyses at the macro level, causal cause-and-effect mechanisms must also be explored at the micro level, because an *ontic* explanatory approach is needed in addition to the nomological one, because only in this way is the *path from correlation to causality successful*:

> [T]he sharply falling barometric reading is a satisfactory basis for predicting a storm, but contributes in no way to the explanation of the storm. The reason is, of course, the lack of a direct causal connection. For the ontic conception, therefore, mere subsumption under a law is not sufficient for explanation. There must be, in addition, a suitable causal relation between the Explanans and the Explanandum (Salmon 1989: 129-130).

In other words, causality has an ontological property in addition to the nomological one. Regulative, counterfactual, and manipulative theories of causality, methodologically generated using correlation methods, experiments, and simulations, can only prove the nomological aspect of causality and answer the *why question*. The *how-question*, however, is an ontological question and is primarily intended to explain how a cause produces an effect:

> Now, to explain a fact is to exhibit its underlying mechanism(s) [...].
> In all cases we explain facts by invoking some *mechanism* or other, perceptible or hidden, known or suspected (Bunge 1996: 137).

These are not necessarily mechanical processes, mechanisms in the narrower sense:

> We now understand that mechanism need not be mechanical: they may be physical, chemical, biological (in particular, psychological), social, or mixed. They may be natural or artificial: causal or stochastic or a combination of the two; pervasive or idiosyncratic, and so on. The only condition for mechanism hypotheses to be taken seriously in modern science or technology is that it be concrete (rather than immaterial), lawful (rather than miraculous), and scrutable (rather than occult) (Bunge 1996: 138).

So, first of all, one must be able to state exactly how the cause-effect mechanism or the causal process works. Only then one can speak of causality or has established causalities. This brings me to the fourth approach, the *mechanism and capacity approach*, and the *qualitative-mathematical tools* necessary for it (King/Keohane/Verba 1994 and Brady/Collier 2010 [2004], section 6.9.1). The qualitative-mathematical methods allow on the micro level to solve the *pairing problem* as well as the *problem of causal complexity* and to answer how-questions.

IV. Mechanism and capacities approach for the identification of causal processes

In order to answer the question of how a cause generates an effect, the causality theory was further developed in the second half of the 20th century, focusing on the causal mechanism or the causal process. With this approach one wants to explain how the cause generates an effect: Salmon speaks of

> [O]ntic conception of scientific explanation [...].
> As this approach had developed by the close of the fourth decade, it became the causal/mechanical view that is advocated by – among others – Humphreys, Railton, and me [...]
> this version of the ontic conception has developed into a view that makes explanatory knowledge into knowledge of the hidden mechanisms by which nature works. It goes

> beyond phenomenal descriptive-interpretative knowledge into knowledge of things that are not open to immediate inspection. Explanatory knowledge opens us the black boxes of nature to reveal their inner workings. It exhibits the way in which the things we want to explain come about. This way of understanding the world differs fundamentally from that achieved by way of the unification approach. Whereas the unification approach is 'top-down', the causal/mechanical is 'bottom-up' (Salmon 1989: 182-183).

Before I discuss a qualitative mathematical method to identify causal mechanisms, I will first present a qualitative mathematical method that is used to solve the pairing problem as well as the causal complexity problem.

In the following, I illuminate how scientists in the 21st century solve this so-called *pairing problem* (*multicollinearity*) at the micro level. To put it more simply, one has to prove methodically a correlation also on the micro level or, in other words, to solve a *concrete pairing* problem. On the macro level only regularities are proved and no *concrete correlation* concerning an individual case.

For this to succeed, the causal complexity that is often present must be disentangled, with complexity existing in several respects (Wagemann 2015: 441): *multicollinearity* exists when several conditions appear in parallel rather than in isolation. *Equifinality* exists when an event can be reached by different, alternative, and equivalent paths. *Multifinality* means that an independent variable can cause different outcomes. *Asymmetric causality* is when the explanation of a phenomenon does not automatically explain the absence of the phenomenon, for example, that

> Negative decisions cannot be automatically explained by the absence of the conditions for positive decisions[59] (Wagemann 2015: 442; my translation).

The pairing problem and the problem of causal complexity can be solved using QCA (*Qualitative Comparative Analysis*, Wagemann 2015). This *qualitative-mathematical method*, not to be confused with the *qualitative-interpretative methods* (section 6.9), has been developed since the 1970s. With this method, one can prove that a regularity established at the macro level is at work in a particular case at the micro level.

Andrew Bennett explains how *process tracing* can be used to identify causality at the micro level, even when complex causal structures are involved (Bennett 2010 [2004], 2010 [2008], and Starke 2015):

> [P]rocess tracing is a powerful means of discriminating among rival explanations of historical cases when these explanations involve numerous variables (Bennett 2010 [2004]: 219).

The process tracing can make use of various tests:

> [P]rocess tracing involves several different kinds of empirical tests, focusing on evidence with different kinds of probative value. Van Evera (1997: 31-32) has distinguished four such

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[59] Negativentscheidungen nicht automatisch durch das Fehlen der Bedingungen von Positiventscheidungen erklärt werden können (Wagemann 2015: 442).

> tests that contribute in distinct ways to confirming and eliminating potential explanations (Bennett 2010 [2004]: 210).

The identification of causality at the micro level is what scientists want to achieve through new observances. A central goal of the volume "Rethinking Social Inquiry. Diverse Tools, Shared Standards" (Brady/Collier 2010 [2004]) is to use *causal-process observations* (CPOs) at the micro-level to identify a concrete correlation between two events or a concrete causal process. These CPOs are meant to complement data-set observations (DSO) needed in correlation and regression analyses at the macro level. For example, in a paper in this volume, Brady explains that a causal regularity determined by regression analysis does not exist in a concrete case, proving the need for single-case analyses at the micro level (Brady 2010 [2004]). The distinction between data-set observation (DSO) and causal-process observation (CPO) is discussed here in detail elsewhere (section 6.9).

We have still not arrived at the causality, but only a concrete correlation has been proven. How important the ontic dimension of causality is can also be seen from the fact that, for fundamental reasons, one cannot solve two other problems in addition to the pairing problem with the help of statistical and comparative methods. Brady does not discuss these issues. It is *Galton's problem*, the second problem I call *Mill's problem*, both concern statistical as well as comparative methods.

Science is characterized by *specialization*, so *reducing complexity* is generally the beginning of any scientific work. Every scientist has to apply *Occam’s razor*. Unfortunately, there is no safe way to separate important from unimportant factors. A central goal in methodological handbooks is to determine how to filter out unimportant factors (King/Keohane/Verba 1994). Also, the evolution of this approach by Brady and Collier (2010 [2004]) is to develop guidance on how to separate important (DSOs and CSOs) from unimportant information.

But in spite of all caution, there is always the danger that one overlooks or ignores a third factor. This already describes the *Galton problem*:

> [A]ffected by some unidentified (underlying or lurking) third factor (in other words, Galton’s Problem) (Moses/Knutsen 2019 [2007]: 103; see also p. 88).

The next main problem is the *Mill problem.* Statistical and comparative methods cannot indicate necessity between the variables under study:

> Mill believed that the main problem with this method is its inability to establish any *necessary link* [own emphasis] between cause and effect (Moses/Knutsen 2019 [2007]: 103).

The path from correlation to causality can therefore not be taken with the help of *macro-analyse*s consisting of hypothesis-testing procedures alone, but requires *micro-analyses*. With macro-analyses, one can at most clarify the *why-question* and thus the *nomological* property (smoking leads to lung cancer), but not the *how-question* or the *ontic* property: How does the biological causal mechanism work concretely or how can smoking cause lung cancer?

At the macro level, quantitative-mathematical methods are used at the micro level qualitative-mathematical methods (not to be confused with qualitative-interpretive methods, see section 6.9), e.g., process analysis (Bennett 2010 [2004] and 2010 [2008], Starke 2015) and Qualitative Comparative Analysis (QCA), which are applied in case studies or small-N studies to identify specific cause-effect mechanisms.

The four methodological approaches for determining causality will now be described using an example. With the *regularity approach*, one can find out, for example, that there is a correlation, regularity, or probabilistic law between smoking and lung cancer. Using the *counterfactual approach*, one can show that it is not a random correlation, and the *manipulative approach* allows to identify smoking as a cause (condition) for lung cancer (effect), more precisely it shows what temporal occurrence between these two variables consists. Since by *definition* cause precedes effect, cause and effect can also be identified.

However, it has not yet been explained at all how or which mechanism is at work. Only when this has been achieved is a causal explanation complete. Furthermore, the complexity of the relevant causal relations is far from clear. Further causal analyzes can demonstrate that pollution other than smoking also leads to lung cancer (*equifinality*) and that some people do not develop lung cancer despite heavy smoking and others who do not smoke develop lung cancer (*asymmetric causality*). In other words, different effects can have a common cause (*equifinality*) and vice versa, a cause in combination with other conditions can produce different effects (*multicollinearity, conjunctural causality*). This brings us to the pairing problem. If someone dies from lung cancer who smoked, then the question remains as to what caused the death: smoking or other environmental influences (*multicollinearity*).

The methodologists who use qualitative-mathematical methods (King/ Keohane/Verba 1994, Brady/Collier 2010 [2004]) represent a naturalistic methodology in pure culture. They have made the swing of philosophy of science from physics to biology, even bringing, when explaining the type of qualitative-mathematical methods, examples from medicine and biology (so also Freedman 2010 [2004]). It is precisely this further development among the scientistic scientists that the perestroikans and the overwhelming majority of the interpretivists have not noticed.

The existence of diverse qualitative-mathematical methods for determining causalities at the micro level within individual case studies (case studies) and Small-N studies shows that scientistic scientists also pursue not only *nomothetic* but also *idiographic* investigations, which is exactly what the perestroikans deny (section 6.8).

D. Second digression: Correlation instead of causality

In the *internet age*, the logical-mathematical methodology plays a crucial role. Companies that shape the digital internet economy in our *knowledge society* work with algorithms and correlations in particular. With the help of mathematical models, *algorithmization* and *discretization* - the extraction of a finite amount of discrete data from continuous and infinite information - one wants to determine knowledge from a large flood of data (data deluge or *big data*). It is inevitable that causal thinking should play a prominent role.

In the following, a proposal for the revolutionary advancement of scientific methodologies and theories will be discussed, which proposes a replacement of causality with correlation (correlation supersedes causation, according to Chris Anderson's thesis), therefore these questions are central: Back to correlation? Is correlation alone enough to generate knowledge from big data?

Chris Anderson, the former editor-in-chief of the magazine "Wired" (the leading scene magazine of all internet prophets), considers the methodology of science to be outdated and demands that scientists should orientate themselves on Google. Google not only has a lot of data that form the raw material of the information age, but could also better transform big data into knowledge.

Both scientific theories and scientific methodology are outdated, correlations within the collected flood of data should replace causal analyses: "The End of Theory. The Data Deluge makes the Scientific Method obsolete" (Anderson 2008) is the programmatic title of his essay.

Anderson thinks that causalities can be replaced by correlations. Due to the sheer mass of data, one can do without both semantic and causal analyses:

> Petabytes allow us to say: Correlation is enough [...].
> We can throw the numbers into the biggest computing clusters the world has ever seen and let statistical algorithms find patterns where science cannot [...].
> The new availability of huge amounts of data, along with the statistical tools to crunch these numbers, offers a whole new way of understanding the world. Correlation supersedes causation, and science can advance even without coherent models, unified theories, or really any mechanistic explanation at all (Anderson 2008).

Anderson doesn't assassinate *theory*, even though the title (The End of Theory) suggests it. Theories are also generated from the flood of data in the reductionist way proposed by Anderson, using correlation, but not with the help of causal analyses, but with the help of correlations.

The scientific methodology of generating theories by means of hypotheses and experiments is rejected:

> The Data Deluge makes the Scientific Method obsolete

so the subtitle of the essay.

> Google's founding philosophy is that we don't know why this page is better than that one: If the statistics of incoming links say it is, that's good enough. No semantic or causal analysis is required (Anderson 2008).

The better explanations are provided by the *users* and not by Google. Anderson overlooks the fact that while Google is not doing semantic or causal analysis, users do. Each reader carries out these analyzes and/or confirms the analyzes with a link to them. Google only evaluates the meta links quantitatively; Google make no discussion of the content.

As shown above in the approaches to identifying causality, correlation analyzes are far from sufficient. It is not only important to find out which variables correlate with each other, but also which is the cause and which is the effect. Furthermore, whether it is a regularity in a specific case and if so, which one. Using the example above: Did smoking or environmental pollution lead to lung cancer in a specific case? How does the biological mechanism for this work? These are questions that cannot be answered with a large amount of data alone.

> The new availability of huge amounts of data, along with the statistical tools to crunch these numbers, offers a whole new way of understanding the world. Correlation supersedes causation, and science can advance even without coherent models, unified theories, or really any mechanistic explanation at all (Anderson 2008).

Anyone who can only state correlations and no causality does not understand the cause-effect mechanism and cannot answer the question of how the biological mechanism works at all. He can say that smoking and lung cancer are correlated, but not whether the correlation is causal rather than accidental. Furthermore, one cannot even decide the why question in a specific individual case because the pairing problem cannot be solved.

Correlation analyzes are particularly necessary in the regulatory approach, but cannot contribute sufficiently to explaining reality. If it comes up or if any have been developed within the company at all, Google supplies the "better analytical tools", whereby correlation analyzes have been standard in all sciences for decades. It is clear who should copy from whom, especially since the Google researchers do not learn their most important craft at Google, but rather at universities. Google only provides the technical and economic means to apply the methods learned in universities.

In other words, Google lets users do the most important and crucial analysis. Google itself only presents the results of other people's work:

> Out with every theory of human behavior, from linguistics to sociology. Forget taxonomy, ontology, and psychology. Who knows why people do what they do? The point is they do it, and we can track and measure it with unprecedented fidelity. With enough data, the numbers speak for themselves (Anderson 2008).

Here, too, Google not only relies on the large number, but also on the fact that the majority of users judge the results well based on their personal competence.

> That's why Google can translate languages without actually 'knowing' them (given equal corpus data, Google can translate Klingon into Farsi as easily as it can translate French into German). And why it can match ads to content without any knowledge or assumptions about the ads or the content (Anderson 2008).

The quality of the Google translations does not exactly speak for the excellent quality of the correlation analyses. On the contrary, the translations show that further semantic analyzes are indeed necessary.

A revolution from causality back to correlation would mean scientific regression rather than progress. The increase in the quantity of data cannot lead to a new quality in correlation analyses, more precisely the path from correlation to causality can neither be determined using the three correlation approaches explained above (regulatory, counterfactual and manipulative) nor deductively using the deductive-nomological model. Therefore, correlations cannot replace causalities. The ontic property of causality, i.e. the exact explanation of the causal cause-effect mechanism (causal mechanism), cannot be determined using correlation analyses.

In addition to causal analyses, meaningful knowledge of the world also requires research into sense and meaning making. The latter is now dealt with in the next section.

4.2.3 Knowledge generation or world cognition as world interpretation or world description by means of language: identifying of (visible) phenomena within interpretative human and cultural studies (Humanities)

In the following I will describe the research program of the interpretivists: World cognition as a world description or world interpretation using a linguistic-interpretative research methodology (A). After that, I will show that these two forms of world cognition (interpretivistic and scientistic) are not incompatible methodologies, but that they can, and even must, be pursued in a complementary way (B).

A. World cognition as world interpretation or world description of (visible) phenomena by means of language: interpretation and understanding (sense and meaning making) within the human and cultural sciences (humanities): hermeneutic, phenomenological and structural research of meanings and contexts of meaning

While some (causalists, scientists, or (neo-)positivists) seek only causal explanations, others criticize this. Within the Aristotelian Tradition, teleological relations (by Wright (1971) are also examined. In the explanative-prognostic or the Platonic-Galilean tradition, one thinks that teleological relations can be reduced to causal ones.

Far more important became the juxtaposition of causal analyses by means of logical-mathematical and quantitative research methodology on the one hand and the exploration of meanings and contexts of meaning by means of a linguistic-interpretative, hermeneutic or phenomenological, qualitative research methodology on the other. While researchers who prefer the first approach are oriented towards the natural sciences, the second approach was developed within the humanities (a comparison can be found in the 2nd, 6th and 7th charts, section 9.4.2, 9.4.6, 9.4.7, and section 2.2).

In the U.S., this is also evident in the names of the departments: *social sciences* on the one hand and *Humanities*. Political scientists, who feel they belong to the social sciences, orient themselves to the natural sciences by searching for causalities by means of a logical-mathematical research methodology. An overview of their methodology can be found, as stated above, in the volume "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]).

Hans Albert points to hypothesis-oriented versus concept-oriented approaches, citing another difference between those oriented toward the natural sciences and those oriented toward the humanities:

> Scientific research in this field, insofar as it is to have theoretical relevance, is generally not hypothesis-oriented, as in the natural sciences, but concept-oriented[60] (Albert 1967c [1965]: 419; my translation).

Both methodologies have limitations: insofar as there is an immunization against experience (facts), Albert speaks of the dangers of a "*model Platonism*" in the case of the former, and of the dangers of a "*conceptual realism*" in the case of the latter (Albert 1967c [1965]: 420, details on model Platonism in section 6.10).

Even the theorists, who are defamed by some as undisciplined political theorists, can fall back on a very differentiated and elaborate methodology. They are oriented above all to hermeneutics and phenomenology, and thus not to the natural sciences, but to the humanities (Flick/von Kardorff/Steinke 2015 [2000], Flick 2008 [2002], Denzin/Lincoln 1994, Creswell 2013 [1998], Blatter/Janning/Wagemann 2007, Kleemann/Krähnke/Matuschek 2009, Bevir/Rhodes 2016, Yanow/Schwartz-Shea 2014 [2006]).

Some qualitative-interpretative researchers distinguish *three research perspectives* and thus point to phenomenology, ethnomethodology, hermeneutics, structuralism, symbolic interactionism, constructivism, cultural studies, gender studies and evolutionary research:

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[60] Die wissenschaftliche Forschung in diesem Bereich ist, soweit sie theoretische Relevanz haben soll, im Allgemeinen nicht, wie in den Naturwissenschaften, hypothesenorientiert, sondern begriffsorientiert (Albert 1967c [1965]: 419).

> Approaches to subjective perspectives [...]
> description of processes of creating social situations [...]
> [h]ermeneutic analysis of deeper structures[61] (Flick/von Kardoff/Steinke 2015 [2000]: 19; my translation).

John W. Creswell lists the following interpretative frameworks of qualitative research: Postpositivism, Social Constructivism, Transformative Frameworks, Postmodern Perspectives, Pragmatism, Feminist Theory, Critical Theory and Critical Race Theory (CRT), Queer Theory, Disability Theory (Creswell 2013 [1998]: 22 ff.). This could be used to justify five qualitative approaches: Narrative Research, Phenomenology, Grounded Theory, Ethnography, Case Study.

The aim of (qualitative)-interpretative research is primarily to work out interpretations (*Deutungen*) and contexts of meaning (*Sinnzusammenhänge*) by means of a linguistic-interpretative research methodology. Another primarily political-practical goal is to formulate *problem-oriented*, value-oriented demands, whereby, in contrast to the scientistic scientists, no distinction is made between *is* and *ought*, and therefore practical approaches and methods are very rarely formulated. The phronetic perestroikans do this with applied phronesis, so I have also chosen this as a paradigmatic example for the Aristotelian tradition.

While hermeneutics assume that the meaning of texts can be determined, deconstructivists assume that there is a

> never fully illuminable web of cross-references and meanings[62] (Ruffing 2005: 237; my translation).

Jacques Derrida's deconstructivism can be distinguished from Gadamer's hermeneutics as follows:

> Hermeneutics is a general practice of reading or deciphering a religious, literary or philosophical text, which presupposes that the text can be read in a certain sense and that, taking into account the profundity of the text, one inevitably arrives at the meaning, content and significance of the text. I have a great deal of respect for hermeneutics, and I always think that a hermeneutic science is necessary in all fields. But deconstruction is not hermeneutics because meaning, as the final layer of the text, is always divided and multiple and cannot be joined together[63] (Derrida quoted in Ruffing 2005: 237; my translation).

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[61] Zugänge zu subjektiven Sichtweisen [...]
Beschreibung von Prozessen der Herstellung sozialer Situationen [...]
[h]ermeneutische Analyse tiefer liegender Strukturen (Flick/von Kardoff/Steinke 2015 [2000]: 19).

[62] nie völlig ausleuchtbare[s] *Gewebe* von Quer- und Sinnbezügen gibt (Ruffing 2005: 237).

[63] Die Hermeneutik ist eine allgemeine Praxis der Lektüre oder Entzifferung eines religiösen, literarischen oder philosophischen Textes, die voraussetzt, dass sich der Text in einem bestimmten Sinn lesen lässt und dass man, wenn man die Tiefgründigkeit des Textes berücksichtigt, zwangsläufig zum Sinn, zum Inhalt und zur Bedeutung des Textes gelangt. Ich habe sehr viel Achtung vor der Hermeneutik und halte eine hermeneutische Wissenschaft auf allen Gebieten immer für notwendig. Aber die Dekonstruktion ist keine Hermeneutik, weil

Derrida is also important in another context for the perestroikans as well as the interpretivists, and that is because he problematizes the importance of context:

> The sentence that for some has become, as it were, the slogan of deconstruction [...] there is no outside of the text 'il n'y a pas de hors texte', means nothing other than: There is no outside context 'il n'y a pas de hors contexte'[64] (quoted from Ruffing 2005: 236; my translation).

The perestroikans, as described, criticize the causalists for allegedly ignoring context altogether.

The results that political scientists have obtained by means of a linguistic-interpretive research methodology are included in the different volumes of the Oxford Handbook of Political Science. The methodology used to generate the linguistic-interpretive theories, on the other hand, is not explained in the volume "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]).

It is true that Robert Edward Goodin, the general editor of the eleven-volume series "The Oxford Handbook of Political Science," claims in his "State of the Discipline" that he is against an "either-or" (Goodin 2011b [2009]: 9) and that the other authors of the handbook also advocate a pluralistic methodology. The 10th volume, "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]), however, speaks a different language; this contains an outstanding overview of causal and empirical reductionism and its methodology, consisting of deductive and inductive modes of reasoning, quantitative-mathematical and qualitative-mathematical methods, and empirical and practical (normative) methodological approaches, within political science at the beginning of the 21st century. Discussions of meanings and contexts of meaning are missing, as is an overview of linguistic-interpretative tools (concepts, modes of argumentation, methods, or methodological approaches).

The criticism of the perestroikans of the methodological narrowing (concentration on the logical-mathematical research methodology) of the scientists is justified. However, this is not the place to discuss the diverse linguistic-interpretative research methodology. Of relevance to the science war discussed here is the following: The perestroikans always point to the importance of the micro level, of context, and of detailed case analysis or case studies, which the positivists supposedly ignore because they only focus on the macro level and look at the laws there (Flyvbjerg 2001: 26, Schram 2003: 836).

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der Sinn als letzte Schicht des Textes immer geteilt und vielfältig ist und sich nicht zusammenfügen lässt (Derrida zitiert nach Ruffing 2005: 237).

[64] Der Satz, der für manche gleichsam zum Slogan der Dekonstruktion geworden ist […] es gibt kein außerhalb des Textes ‚il n’y a pas de hors texte‘, heißt nichts anders als: Es gibt kein außerhalb des Kontextes ‚il n’y a pas de hors contexte‘ (zitiert nach Ruffing 2005: 236).

This criticism is now obsolete. As described above, the micro level as well as case studies, experiments and simulations play a decisive role in the mechanism and capacity approach for the determination of causalities. The extent to which decades of criticism have contributed to the transformation of the concept of causality as well as to the establishment of a qualitative-mathematical methodology cannot be discussed here. However, the limitations of quantitative research methodology have also been addressed by quantitative researchers and, therefore, qualitative ways of overcoming them have also been elaborated, explicitly committed to logical-mathematical research methodology (King/Keohane/Verba 1994 and Brady/Collier 2010 [2004]).

It is very unfortunate, however, that these methods for determining causality at the micro level have also been given the adjective "qualitative," which leads to many avoidable misunderstandings (section 6.9).

B. Complementarity between Description and Explanation

Since the 19th century, a distinction has been made between descriptions and explanations. Humanities and cultural studies primarily provide *descriptions* and thus aim to *interpret* or *understand* the world; natural sciences generate *explanations* as well as *forecasts* and aim to *explain* the world or make *predictions*. The former usually use *qualitative-interpretative methods* to generate *descriptions of phenomena*, the latter *quantitative-mathematical methods* to generate *invisible explanations*. The extent to which qualitative-interpretative methods can also be used for explanations and quantitative-mathematical methods for descriptions, or in the meantime even have to be used due to big data, is not discussed in this context (Blätte/Behnke/Schnapp/Wagemann 2018, Burdick/Drucker/Lunfield/Presmer/Schnapp 2012, Lemke/Wiedemann 2016, Lyon 2016).

In his late work, Wittgenstein (1984c [1953]) differentiated between language problems and material problems (Lauer 1987). According to Wittgenstein, material problems do not belong to philosophy; natural scientists solve such problems with the help of scientific explanations. He only wants to solve language problems or rather treat them.

In Bacon (1990 [1620]) the word "*interpretation*" is used in a very broad sense. Also, the word "*explanation*" can firstly be used in a very broad sense and then can be used interchangeably to interpret how Bacon used the word. Second, it is used in a very narrow sense, as scientistic scientists do today (section 4.1.4, A).

When it comes to the therapy of linguistic, especially philosophical problems, Wittgenstein uses *explanations* in the sense of *clarification* it generates an overview (*Übersicht*). These are not empirical problems, but linguistic problems (Wittgenstein 1984c [1953]: §§ 109, 133, 383, Wittgenstein 1984c [1922]: 4.0031, 4.003(2), 6.5 (2)). It is better to speak of *descriptions* here, since it is not about causal explanations, but about enabling an *understanding of meaning*:

> Philosophy clarifies the boundaries of meaning. It clarifies which questions and which distinctions are meaningful in principle. Philosophical explanation or clarification provides understanding by providing an overview. Therefore, philosophy is an activity of clarifying thought (Lauer 1987: 32).

Therefore, a clear distinction between descriptions (clarifications) and causal explanations can be identified in Wittgenstein:

> Philosophy, one might claim, explains by description whereas science explains by hypothesis. Philosophical explanation produces understanding by means of an Übersicht, scientific explanation produces new knowledge by constructing theories (Baker/Hacker 1980: 490).

Transferred to the controversy between interpretivists and scientistic scientists, one could say that there are fundamental differences between *descriptions* and *explanations* because of the goals and their methodological implementation:

First, a descriptive methodology enables knowledge generation or world cognition as world interpretation, world meaning or world description of (visible) phenomena by means of language.

Second, an empirical-explanative and an empirical-prognostic methodology generates knowledge of invisible causalities or world cognition as an explanation of the world using logic and mathematics.

Descriptions cannot justify, refute or replace explanations or evaluations with others. Since there are also fundamental differences between explanations and valuations, as can be seen in the second chart (section 9.4.2), a third distinction is also necessary on the horizontal level: a practical methodology to justify or legitimize questions of validity or questions of value.

Thus, a structural separation between descriptions (clarifications) and inferences (explanations) has a Wittgenstein bias. It is therefore not surprising that Georg Henrik von Wright (1971) also calls for this. Von Wright was Wittgenstein's student, Wittgenstein's successor at Cambridge, and one of three editors of Wittgenstein's estate, along with Gertrude Elizabeth Margaret Anscombe and Rush Rees.

According to von Wright ([1971), an understanding precedes an explanation, i.e. first the *visible appearance* of an event is described (phenomenology), then the *invisible aspects* are explained in the form of necessary and sufficient conditions of causality. The description of visible phenomena is usually made using qualitative-interpretative methods. The explanation of the necessary conditions, according to von Wright the clarification of the how-questions, has been carried out within political science since the 1970s using qualitative-mathematical methods. The why questions or the sufficient conditions are determined using quantitative-mathematical methods (section 6.9).

Mario Bunge, a scientistic philosopher of science *par excellence*, is opposed to hostile towards hermeneutics, phenomenologists, philosophers of language and structuralists:

> [I]t is not wise for social scientists to leave philosophy in the hands of philosophers like Husserl and Wittgenstein , who have never bothered with science in particular with social studies. And it is downright foolish to seek inspiration in the likes Heidegger and Derrida, who have written only gibberish, platitudes, or falsities (Bunge 1996: 12).

Bunge also distinguishes between descriptions and explanations:

> In particular, objective description should precede everything else, for only a (sufficiently) true description of a social situation qualifies us in advancing explanatory hypotheses, identifying social issues, and designing efficient policies or plans for tackling the latter (Bunge 1996: 135).

Explanations primarily answer why questions, while descriptions are meant to clarify where, when, from where, or to what something happened:

> Description is necessary but insufficient: we want to know why, not just what, where, when, whence, or whither (Bunge 1996: 137).

A purely descriptive-interpretative approach is not sufficient within the sciences, because here one strives for a rational understanding, and this requires an explanation in addition to an adequate and dense description:

> We want explanation, either because we want rational understanding – not some vague intuition or a metaphor, let alone a story – or because we wish to tamper with the thing in question (Bunge 1996: 138).

In other words, adequate knowledge generation for world cognition requires both: first world description and then world explanation. Due to different goals of knowledge and mostly also different research methodologies, both have to be pursued complementary to each other. Only the research methodology is *incommensurable*. There is *no general incommensurability* between these research traditions. Researchers are even dependent on the results of the respective other tradition; a discontinuity or even speechlessness is not a foregone conclusion due to these different methodologies.

The understanding of meaning (sense making) disappears within political science from the explanative-prognostic or the Platonic-Galilean tradition, mainly due to the orientation towards the natural sciences and the above-mentioned methodological research programs or "revolutions". Weber remains true to Neo-Kantianism, for him both causal thinking and understanding of meaning are two equal tasks of the sciences (Weber 1980 [1922] and 1984 [1921]).

Also, other naturalistic philosophers of science distinguish very precisely between descriptions and explanations. Only phenomena can be described and descriptive-interpretative knowledge (description of appearances) is obtained. Causal regularities as well as causal processes or cause-effect-mechanisms can be explained, Wesley C. Salmon calls this *explanatory knowledge*.

> For the proponents of the ontic conception of scientific explanation, realism provides a straightforward answer to the question of the distinction between descriptive and explanatory knowledge (Salmon 1989: 134).

It is also confusing that in political science another distinction, which is used synonymously, is made between descriptions of phenomena (*descriptive inference*) and causal conclusions about phenomena (*causal inference*):

> [T]he next section of the handbook discuss regression-like statistical methods and their extensions. These methods can be used for two quite different purposes that are sometimes seriously conflated and unfortunately confused. They can be used for descriptive inferences about phenomena, or they can be used to make causal inferences about them (King , Keohane and Verba 1994). Establishing the Humean conditions of constant conjunction and temporal precedence with regression-like methods often takes pride of place when people use these methods, but they can also be thought of as ways to describe complex data-sets by estimating parameters that tell us important things about the data (Box-Steffensmeier/Brady/Collier 2010b [2008]: 17).

The establishment of methods for investigating causal mechanisms or processes, in contrast to the quantitative methods used to determine causal regularities, leads to conceptual confusion, at least in political science. In their methodology books, the disciplined political scientists contribute to two conceptual confusions:

First conceptual confusions: The term "description" is used where "explanation" would be appropriate. Instead of saying "to explain causal mechanism", the explanation of causal processes or the "qualitative" methods for determining cause-effect mechanisms are dealt with under the heading "description and causal inference" (King/Keohane/Verba 1994, Box-Steffensmeier/Brady/Collier 2010a [2008]).

In particular, these authors, Mr. Perestroika would probably say the East Coast Brahmins from Harvard (King/Keohane/Verba 1994) and the West Coast Brahmins from Berkeley (Brady/Collier 2010 [2004]), make a very strong case for the use of a very narrow notion of description and, relatedly, of qualitative methods. A distinction is made between descriptions, which are used to gather facts, and descriptive inferences:

> [W]e distinguish description – the collection of facts – from descriptive inference (King /Keohane/Verba 1994: 34).

Descriptive inference is used to systematically divide the world into important and unimportant components:

> [M]aking descriptive inference by partition the world into systematic and nonsystematic components (King/Keohane/Verba 1994: 75).

Thus, it is a matter of dividing the visible world into systematic and non-systematic components, with only the former providing the data necessary for causal references (data-set observations (DSOs), and causal-process observations (CPOs) are introduced later by other authors (Brady/Collier 2010 [2004])), and everything else being negligible data. Even *cultural factors* as explanatory variables are neglected:

> The use of ‘culture’ as an explanatory variable in social science research is a subject of much contention but is not the subject of this book. Our only comment is that cultural explanations must meet the same tests of logic and measurement we apply to all research (King/Keohane/Verba 1994: 226).

Now this is an analogy which has to be proved.

The attempt within logical empiricism (philosophy of ideal language) to grasp the world scientifically by means of a logical-mathematical methodology alone (Frege 2008 [1963/1879], Wittgenstein 1984b [1922], Carnap 1998 [1928]) has long been exposed as a dead end, at least within philosophy.

Ethnologists, sociologists and political scientists who write case studies (area studies) outside the western world (Rudolph 2005b and 2005b) see this very differently, they have dealt with the associated developments. For example, before they can provide explanations, ethnologists must first learn the language of a tribe and, just as importantly, understand their way of life (*Lebensform*). This also applies, to a certain extent to a lesser extent, to sociologists who conduct research in non-academic milieus. It is therefore not surprising that the most important methodological considerations for qualitative linguistic research come from these sciences (Flick/von Kardorff/Steinke 2015 [2000], Schmitz/Schubert 2006a, Denzin/Lincoln 1994).

Second conceptual confusions: The use of the term "description" means that the methods that are necessary to determine cause-effect mechanisms are also labeled "qualitative". However, these are not qualitative-interpretative methods within the linguistic-interpretative research methodology, but qualitative-mathematical methods within the naturalistic research methodology, as I will show later (section 6.9).

The perestroikans refer not only to the philosophy of language, but also to structuralism:

> Deciphering the network of relationships hidden beneath the visible is the method of structuralism[65] (Ruffing 2005: 202; my translation).

Making hidden structures visible is not only a goal of structuralism, but can be seen as a general goal of science, whether one is looking for invisible causal regularities, concrete causal mechanisms of cause and effect, teleological relations or, as Michel Foucault (1971 [1966] and 1995 [1969]) for power relations. In conclusion, it can be said that the search for causalities using a logical-mathematical research methodology, and for contexts of meaning using a qualitative-interpretative research methodology have produced the most differentiated and elaborate methodologies for world cognition.

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[65] Unterhalb des Sichtbaren verborgene Beziehungsgeflechte zu entziffern, ist die Methode des Strukturalismus (Ruffing 2005: 202).

So far, I have outlined the interpretivists' goals and approaches as a whole. In the following, I will confine myself paradigmatically to the phronetic perestroikans.

4.2.4 Knowledge generation or world cognition within phronetic political science. Real Social Science - a fundamental alternative to naturalism or scientism?

The identity of the person going by the alias "Mr. Perestroika" (Mr. Perestroika, 2005 [2000]) formulated a critique of the mainstream has not yet been clarified. A heterogeneous group of scientists supports this criticism, as determined above (section 2.2.3, A, d), not against the mainstream, but above all against the scientistic establishment. An overview can be found in the volume by Kristen Renwick Monroe (2005) "Perestroika! The Raucous Rebellion in Political Science", an evaluation of the perestroika movement was published in the journal "Perspectives on Politics" in 2015 (Gunnell 2015, Monroe 2015, Laitin 2015, Farr 2015, Schram 2015).

Sanford F. Schram (2003, 2005 and 2006, Schram/Caterino 2006) not only committed himself to this movement, but also formulated the goals of this movement. Alongside Bent Flyvbjerg, Todd Landman and others, Schramm is a representative of a phronetic social science or phronetic political science. They see themselves as an alternative to the establishment (mainstream) (Flyvbjerg 2001 and Flyvbjerg/Landman/Schram 2012a). Flyvbjerg's (2001) book "Making Social Science Matter: Why Social Inquiry Fails and How it Can Succeed Again" has been cited by Laitin (2006 [2003]) and Schram (2003 and 2005), among others, as the manifesto of the perestroika movement. Flyvbjerg itself supports this assessment (Flyvbjerg 2006: 56).

In the following, this conception and its critique of the scientistic establishment will be presented.

A. Philosophical foundations of the phronetic perestroikans

The perestroikans orientate themselves towards (American) pragmatism, the (German) critical theory of the Frankfurt School, (French) (post)structuralism and (British) philosophy of language, furthermore towards the hermeneutics and phenomenologists within the humanities. They want to break away from a social science, including a political science, which is based on the natural sciences, while some would like to establish an independent phronetic political science.

First, almost all of the above-mentioned philosophical foundations were first presented within American political science within the framework of the argumentative turn of politicy analysis (Fischer/Forester 1993a, Fischer 2003). Frank Fischer summarized them briefly as follows:

> The growing interest in argumentation in policy analysis draws from both theoretical and practical perspective. On the one side, as we have already seen, its diverse theoretical influences run through British ordinary-language analysis, French poststructuralism, the

> Frankfurt school of critical social theory, and a renewed appropriation to American pragmatism. On the other hand, it is based in practical terms on a range of experiments on the part of policy analyst and planners, from stakeholder analysis and participatory research to citizen juries and consensus conferences (Fischer 2003: 182).

These philosophical positions are also the basis of the phronetic scientists within the perestroika movement, so Sanford F. Schram also points to an expansion of the philosophical and scientific-theoretical foundations of political science. A *pluralistic methodology, contextuality* and a better *practical relevance* are required:

> In its place, Perestroika would put a more pluralistic emphasis on allowing for the blossoming of more contextual, contingent, and multiple political truths that involve a greater tie between theory and practice and a greater connection between thought and action in specific settings. Perestroika lays open the possibility that political science could actually be a very different sort of discipline, one less obsessed with proving it is a ‘science’ and more connected to providing delimited, contextualized, even local knowledges that might serve people within specific settings (Schram 2003: 837).

At the beginning of the 21st century, the fact that causal reductionism is far removed from practice was criticized from many sides. Above all, it is important to the critics not only to describe political reality, but also to change it, as Bent Flyvbjerg's book Making Social Science Matter is entitled (Flyvbjerg 2001). Sounds like the 11th Feuerbach thesis formulated by Marx in 1845:

> Philosophers have only interpreted the world differently; what matters is to change it[66] (Marx/Engels, MEW 3, p. 535, 1845; my translation).

This Feuerbach thesis was also the motto of the Caucus for a New Political Science, which was formed primarily in the U.S.A. in the 1960s (Goodin 2011a [2009]: 5). This is also cited as a motto in the latest work:

> *Real* social science is when studying the world has the effect of changing it, by means of what Machiavelli calls *verita effectuale* (effective truth). Real social science that contributes to phronesis grows out of experience and, in turn, contributes to that experience. It cannot be theorized *in toto* in advance (Flyvbjerg/Landman/Schram 2012a: 4).

B. Criticism of the perestroikans on the natural scientific approach

Bent Flyvbjerg delivers a radical critique of a social science inspired by the natural sciences. He constructs a model of *epistemic* science that he believes is dominant in the natural sciences and has been uncritically adopted by the social sciences:

> By ‘epistemic’ is meant ‘well-founded’ or ‘what must be regarded as correct’. Epistemic science is science which has achieved a paradigmatic and normal-scientific level in the Kuhnian sense, and which is thereby capable of explaining and predicting in terms of context-free knowledge (Flyvbjerg 2001: 172-173).

This book and the criticism associated with it were highlighted by Sanford F. Schram (2003) and presented as a basis for an alternative to the mainstream and thus also as a

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[66] Die Philosophen haben die Welt nur verschieden interpretiert; es kommt darauf an, sie zu verändern (Marx/Engels, MEW 3, S. 535, 1845).

foundation in particular for the phronetic perestroikans and later further developed (Flyvbjerg/Landman/Schram 2012a).

Admiration for the natural sciences, not least because of the technical successes, reached a peak in the 19th century and led to the natural sciences exerting a great influence on the nascent social sciences. Many social scientists pleaded for an orientation towards these methods. There was considerable resistance to the adoption of a scientific methodology, not so much among the then still manageable number of social scientists, but all the more within the human, history and cultural sciences. Because of their subject matter, the humanities require their own methodology, according to Wilhelm Dilthey (1922 [1883]) in his very influential studies. Heinrich John Rickert (1921 [1896]) emphasized the limits of scientific naturalist concept formation and saw them as unsuitable for the historical sciences. Erich Rothacker (1926) identified an independent logic and system for the humanities.

Flyvbjerg also sees a fundamental difference between natural sciences and social sciences:

> We may thus be speaking of so fundamental a difference that the same research procedure cannot be applied in the two domains. It is this argument which is put forth by hermeneutics and phenomenology (Flyvbjerg 2001: 32).

In doing so, he does not refer to the discussion at the turn of the 19th to the 20th century, but in particular to the work of Hubert L. Dreyfus (1991), who defends the difference between humanities and natural sciences:

> If Dreyfus is right he has identified a fundamental paradox for social and political science: a social science theory of the kind which imitates the natural sciences, that is, a theory which makes possible explanation and prediction, requires that the concrete context of everyday human activity be excluded, but this very exclusion of context makes explanation and prediction impossible (Flyvbjerg 2001: 40).

Flyvbjerg, Landman and Schram (2012a) start from a social science model that does not stand up to scrutiny against reality. The articles in the volume "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]) show how a political science works or should work at the beginning of the 21st century that is oriented towards the natural sciences and can appear as science. The real or phronetic social scientists have hardly dealt with this complex methodology.

The most important objections are dealt with below; further objections are examined at the appropriate point on the corresponding methodological level (chapter 6).

A key objection concerns contextuality and the alleged impossibility of demonstrating it using scientific methods:

> We see, therefore, that context-dependence does not mean just a more complex form of determinism. It means an open-ended, contingent relation between contexts and actions and interpretations (Flyvbjerg 2001: 43).

Another property is the local reference of the phronetic knowledge:

> These are local knowledges, even tacit knowledges and skills, that cannot be thought *a priori* but that grow from the bottom up, emerging out of practice. Add a sense of praxis, seeking the ability to push for change, leaven it with an appreciation of the ineliminable presence of power, and this phronetic social science can help people involved in ongoing political struggle question the relationships of knowledge and power and thereby work to produce change (Flyvbjerg/Landman/Schram 2012b: 2).

The if-then deep structure of science (section 5.4.7) not only enables specialization, but at the same time makes it necessary and then inevitably leads to a partial decontextualization. Even perestroikans cannot escape this process. Anyone who generates local knowledge will necessarily have to at least partially ignore the regional, national, European and global context within political science or be aware that many local problems can have causes and determinants other than local ones.

Flyvbjerg criticizes the supporters of the scientific approach to the social sciences for orienting themselves towards a model of science that never existed:

> [T]he idealisation of the natural sciences has become more pronounced since Marx and Freud. This applies not only to positivism and critical rationalism, but also to areas of research not normally associated with the natural science model (Flyvbjerg 2001: 27).

By analogy, Flyvbjerg can be criticized for critiquing a model within political science that existed, if at all, among Marxists and positivists in the 19th century but has hardly been dominant in political science since the 1950s, much less at the beginning of the 21st century.

C. Tension Points

The *search for causalities* within the empirically oriented social sciences corresponds to the *search for tension points* in the real social sciences. Both are presented with a clearly reductionist claim in practical intention, since the identification of causalities just like the identification of tension points is supposed to enable both a recognition and a change of the political reality:

> These tension points are weak spots in any struggle where disagreement creates an opening for research to sway opinion and move a decision in a particular direction (Flyvbjerg/Landman/Schram 2012b: 11).

The search for tension points pursues the goal of initiating changes in political and social processes. This is intended to demonstrate the practical relevance of the social sciences:

> By exploiting these tension points, phronetic research can prove its relevance in specific settings and influence outcomes so as to improve social action and policy-making. In this way, phronetic social science can deliver on the promise of mainstream social science to speak truth to power, to inform society, improve decision-making and enhance social life (Flyvbjerg/Landman/Schram 2012b: 11).

The focus is primarily on power issues; in particular, the abuse of power is to be prevented:

> We explain the focus on tension points by the phronetic researchers to issues of power and especially researchers' commitment to challenge the abuse of power (Flyvbjerg/Landman/Schram 2012c: 289).

The aim is to discover not only tension points, but at the same time points of weakness, which allow targeted intervention in existing power relations and enable the situation to be improved:

> [P]roblematizing tension points may be compared with hitting a rock with a hammer. If you hit the rock at random it seems unbreakable, even if you hit it hard. If you hit the rock strategically at the small, near invisible fault lines that most rocks have, the rock will fracture, even if you hit it gently. Tensions points are the fault lines that phronetic researchers seek out; that is where researchers hit exiting practices to make them come apart and create space for new and better ones (Flyvbjerg/Landman/Schram 2012c: 289-290).

The reference to the search for tension points appears for the first time in the last volume (Flyvbjerg/Landman/Schram 2012), before that the critique was mainly based on the problem orientation (problem-driven, problem-based). Because of the centrality of these tension points, a more detailed elaboration would be needed, but it is not available. I have quoted all important passages on this from the introduction (Flyvbjerg/Landman/Schram 2012b) and the summary (Flyvbjerg/Landman/Schram 2012c) of the volume, but these quotations offer mainly *flowery metaphors* and cannot be compared with the methodologically detailed causal analyses of the scientistic scientists, thus perestroikans offer only metaphorical analyses and no methodological-systematic research. Furthermore, there is the suspicion that the tension points could also be causalities, thus the scientific orientation and the approach of the scientistic scientists would be introduced again through the back door.

D. Problem-oriented (problem-driven, problem-based) versus method-oriented (method-driven) research

In the U.S. in particular, in addition to the perestroikans (Flyvbjerg 2001, 2006, Schram 2003, 2006), other scholars (Shapiro 2005) argue for a problem-driven, problem-based rather than a method-oriented or theory-driven political science.

On the one hand, methodology is accorded a central, even a constitutive, importance in the explanative-prognostic or the Platonic-Galilean tradition. In short, science differs from other gnosiological undertakings in that knowledge generation and knowledge verification is a methodologically comprehensible enterprise (section 4.2.2).

On the other hand, these scientists also strive to ensure that the results they produce or the knowledge they generate are not only noticed but also taken into account in the public debate. Problem orientation is a central goal: The motto of the scientistic scientists is therefore to enrich human life with new inventions and means and to discuss problems with practical significance.

Since the development of political science at the beginning of the 20th century researchers oriented toward the natural sciences and working with mathematical methods have been confronted with the accusation that they are leading the discipline into the sidelines. This accusation came from normative ontologically oriented political scientists as well as from the followers of Critical Theory. The perestroikans renew this reproach and even call for a departure from the chosen path of a methodological orientation in contribution titles such as "Return to politics" (Schram 2005) "Making Political Science Matter" (Schram/Caterino 2006):

> Perestroika lays open the possibility that political science could actually be a very different sort of discipline, one less obsessed with proving it is a ‘science’ and more connected to providing delimited, contextualized, even local knowledges that might serve people within specific settings (Schram 2003: 837, Monroe 2005).

The perestroikans refer in particular to the work of Green and Shapiro (1994, Shapiro 2005: 83, 1st footnote) for this critique. According to Green and Shapiro, *method oriented* research proceeds as follows:

> In short, empirical research becomes theory driven rather than problem driven, designed more to save or vindicate some variant of rational choice theory rather than to account for any specific set of political phenomena (Green/Shapiro 1994: 6).

In contrast, *problem driven* research has the following attributes:

> Empirical science is problem driven when the elaboration of theories is designed to explain phenomena that arise in the world. Method-driven research occurs when a theory is elaborated without reference to what phenomena are to be explained, and the theorist subsequently searches for phenomena to which the theory in question can be applied (Green/Shapiro 1994: 194).
> As Abraham Kaplan (1964, 28) once observed, if the only tool in one´s possession is a hammer, everything in sight begins to resemble a nail (Green/Shapiro 1994: 195).

The solution is:

> Problem-driven research would replace method-driven research (Schram 2003: 837).

This distinction is intended to document the practical relevance of the new approaches and the method infatuation on the other side. Away from formalism and techniques towards important, relevant questions. *Two objections* can be raised against this:

First, this would indirectly confirm that the "happily still undisciplined" (Dryzek /Honig/Philips 2009: 62) political theorists actually work in such an undisciplined and method-distant way, virtually blinded by the problems, as the opponents claim. But this is not the case with most critics of causal reductionism, they just work with a different methodology (Flick/von Kardorff/Steinke 2015 [2000], Blatter/Janning/ Wagemann 2007, Denzin/Lincoln 1994). This is also true for the phronetic perestroicans, even if their methodology is far from being as elaborated as that of the scientistic scientists.

Secondly, the critics do not understand the meaning and consequences of causal analyses, at least not the claims associated with them: With the help of causal analyses, first, the (political) world is supposed to be recognized, i.e. mainly explained (descriptions play a smaller role), and second, one is supposed to be able to change the world through the knowledge of causalities, by transforming causalities into socio-technological rules.

The criticism of method-driven, methodologism is still misleading. Science is essentially characterized by a methodological approach. Knowledge is generated with the help of a scientific methodology, which can be verified by anyone using the same methodology; this ensures intersubjectivity, objectivity and reliability (section 5.2).

The danger that scientists lose themselves in methodological gimmicks or make mistakes due to the complexity of the methods cannot be denied. However, this in no way justifies abandoning the methodological orientation and devoting oneself to a problem-driven approach of whatever kind - because then the question arises again as to which methodology should be used to tackle the problems. Therefore, I agree with John Gunnell, who quite rightly states in his historical overview of the perestroika movement:

> The invocation of mantras such as problem-based research where far from adequate (Gunnell 2015: 409).

The criticism of specialization (Mead 2010: 453) is also misplaced because, quite simply, due to the complexity of the world as well as the current state of research, no serious research can be conducted without specialization. Of course, this can also lead to distortions, but specialization cannot therefore be turned back.

But the important problems of differentiation and specialization are not even addressed. The question here is: How can individual scientific results be put together or, to put it casually, how can the individual pieces of the puzzle be put together?

E. Rigor and Scholasticism versus Relevance

According to Lawrence M. Mead (2010), method orientation and the associated increase in stringency leads to *scholasticism*, which emphasizes ideals such as rigorous proof and transparency:

> [U]nder the norm of rigor, one ideal is *proof* – demonstrating conclusions, not simply asserting them. Hence the appeal of mathematical methods, where inferences are precise. Another ideal is *transparency*. One's conclusion should follow strictly from the data rather than from contestable judgements, so that in principle others could replicate them (Mead 2010: 460).

Stringency and overemphasis on methodology are recurring topoi, as is the lack of problem orientation. Both were already criticized in the "behavioural revolution", since the subject was thereby arbitrarily narrowed and the relevance fell by the wayside:

> Through rigid methodology, the object of what can be scientifically known has been quite arbitrarily narrowed and the relevance of what could still be researched according to these rigorous methodological requirements has been severely limited[67] (von Beyme 2000 [1972]: 117; my translation).

The perestroikans criticism of the overemphasis on methodology is not new:

> The overemphasis on methodology [here the behavioralists are meant] was caustically called by a normativist like Herbert Spiro (1971: 323ff.) the 'masturbation stage of political science'[68] (von Beyme 2000 [1972]: 117; my translation).

According to him, the scholasticism criticized by Mead (2010) has a total of four components, some of which have already been highlighted by Albert, Green and Shapiro, namely methodological orientation (*methodologism*) and immunization against experience (*nonempiricism*), in addition there are excessive specialization and a *literature focus* (Mead 2010, see also Héritier 2016).

The first three points of criticism have already been dealt with, what remains is the fourth point. A novelist does not have to follow up on a previous novel or on the work of a colleague; he can always design everything anew. The quality and relevance of science is not least because scientists are not only very fond of drafting completely new theories, but first and foremost they have to deal with the state of research and develop it further. Therefore, the accusation of "literature focus" (Mead 2010), just like the accusation of methodologism, overshoots the mark by far and is even counterproductive.

On the contrary, one of the greatest weaknesses of the science war (*Methodenstreit*) is that both opponents, scientistic scientists as well as perestroikans, do not deal with the respective state of research of the other party.

For instance, the perestroikans simply do not take note of the various methodological approaches to the determination of causalities as well as of the newly developed methods and experiments, and criticize philosophical positions and methods that have long since been abandoned or decisively changed.

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[67] Durch eine rigide Methodologie ist der Gegenstand des wissenschaftlich Erkennbaren recht willkürlich eingeengt und die Relevanz dessen, was nach diesen rigorosen methodologischen Anforderungen noch erforscht werden konnte, stark eingeschränkt worden (von Beyme 2000 [1972]: 117).

[68] Die Überbetonung der Methodologie [gemeint sind die Behavioralisten] wurde von einem Normativisten wie Herbert Spiro (1971: 323 ff.) bissig das ‚Masturbationsstadium der Politikwissenschaft' genannt (von Beyme 2000 [1972]: 117).

The situation is worse for the opponents: Disciplined scholars completely ignore the methodology of the other side or reject it *en passant* in footnotes (Goodin 2011a [2009], Box-Steffensmeier/Brady/Collier 2010a [2008]). Therefore, Flyvbjerg (2006) quite rightly criticizes that the positions of phronetic social science are not adequately represented and thus the corresponding criticism is not purposeful. His criticism is mainly directed at the criticism made by Laitin (2006 [2003]) in a review of Flyvbjerg's book.

Mead not only questions important scientific criteria, but also pleads for overcoming scholasticism by striving for a little *less rigor* but *more relevance*:

> To limit scholasticism one must step back and question the values it serves – those of *rigor* (Mead 2010: 460).

He suggests that one should get back to questions of value, i.e., pursue a political realism and keep an eye on the target group concerned:

> In contrast, nonscholastic research serves the values of *relevance*. Under that norm, one ideal is *realism* – addressing problems as they appear in the real world of politics, as against the narrower issues that academics may define. Another ideal is *audience* – to speak to all those interested in a problem rather than just the researcher (Mead 2010: 460).

At most, simultaneous attention to stringency and relevance would be acceptable:

> At its best, political science accepts a tension between rigour and relevance, serving both values to some extent (Mead 2010: 460).

Mead confuses two completely different issues here. On the one hand it is about the goals of science and on the other hand about the methodological stringency of scientific inquiry. The value of science for society is not doubted even by scientistic scientists; on the contrary, science should also be used to promote social goals. There can be no discount at all in stringency; of course, scientific questions must be answered with the best available methodology, anything else is dishonest for scientists.

In my opinion, the complex methodology for determining causalities, which the scientistic scientists within political science have taken over from the natural sciences, developed further or partly developed themselves, is in no way worthy of criticism. On the contrary, this methodological progress is to be welcomed.

One should criticize the reductionism goal, i.e. the causal and empirical reductionism or the exclusive focus on causal thinking, which is expressed by the fact that only the methodology that aims at the determination of causalities between events is treated at all in textbooks, as has been done in the methods volume of the Oxford series (Box-Steffensmeier/Brady/Collier 2010a [2008]).

In short: the problem is not the stringency, but the exclusive concentration on causalities:

> Methodical rigorists who, out of scientific-theoretical scruples, leave large areas of the relevant to speculators, paradoxically fall into boundless speculation themselves as soon

> as they enter the narrow terrain that they empirically tilled, have to leave. An attempt is made to compensate for the declining relevance of empirical results achieved with great effort by exaggerating the theoretical introduction and summary[69] (von Beyme 2000 [1972]: 120-121; my translation).

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[69] Methodische Rigoristen, die aus wissenschaftstheoretischen Skrupeln weite Gebiete des Relevanten den Spekulanten überlassen, geraten paradoxerweise selbst in uferlose Spekulation, sowie sie das schmale Terrain, das sie empirisch beackerten, verlassen müssen. Abnehmende Relevanz von mit großem Aufwand erreichten empirischen Ergebnissen wird durch Aufbauschung der theoretischen Einleitung und der Zusammenfassung zu kompensieren versucht (von Beyme 2000 [1972]: 120-121).

5 Epistemology and Political Science

- What basic epistemic *assumptions* do political scientists make?
- What *results*, political *knowledge* or political *theories* can political science generate?
- *How* can political scientists justify knowledge?
- According to what *epistemic values* should scholars conduct research?
- What *forms* of knowledge can be legitimized by political scientists?
- Which *ideals* are strived for in science?
- What *properties* should scientific propositions or sentences have?
- What are the epistemic *limits* of (political) science research and knowledge generation?

These are the most important epistemic questions of a philosophy of political science. First the epistemic perspectives (section 5.1), then the general conditions or general criteria of knowledge (section 5.2) will be dealt with. Furthermore, the epistemic values, the ideals and the properties of scientific propositions are discussed (section 5.3). Finally, the limits of (political) scientific research and knowledge generation are up for discussion (section 5.4).

5.1 Perspectives of epistemology

In the following, two different perspectives of epistemology are briefly introduced: first, an *epistemology with a cognizing subject (*section 5.1.1), and second, an *epistemology without a cognizing subject (*section 5.1.2). Within the first perspective, the limits and possibilities of the cognizing subject are discussed. The second perspective deals with the limitations and possibilities of science as a collective enterprise.

5.1.1 Epistemology with a cognizing subject: limits and possibilities of the cognizing subject

> My first thesis involves the existence of two different senses of knowledge or of thought: (1) *knowledge or thought in the subjective sense*, consisting of a state of mind or of consciousness or a disposition to behave or to react, and (2) *knowledge or thought in the objective sense*, consisting of problems, theories, and arguments as such. Knowledge in this objective sense is totally independent of anybody's claim to know; it is also independent of anybody's belief, or disposition to assent, or to assert, or to act. Knowledge in the objective sense is *knowledge without a knower*: it is *knowledge without a knowing subject.* Of *thought* in the objective sense Frege wrote: 'I understand by a thought not the subjective act of thinking but its *objective content*' (Popper 1972: 108-109. The Frege quote is from Frege 2008 [1962/1879]: 29, note 5, see Popper 2012).

An epistemology that places the knowing subject at the center, as in Lehrer (1990) and Enskat (2005), is what Popper calls a *subjective epistemology*, and he calls the authors

> '*belief philosophers*': those, who, like Descartes , Locke , Berkeley, Hume , Kant, or Russel, are interested in our subjective beliefs, and their basis or origin (Popper 1972: 107).

When the focus is on the *knowing subject*, the *knower*, it is firstly about the *context of discovery* (Reichenbach 1938: 6-7), secondly about a skill, a capability (*Können*) or the *personal anchoring of knowledge*, and thirdly about the *knowing subject* (Enskat 2005).

The Popperian devaluation of questions that concern the epistemic questions of the knowing subject, and in its wake also of many scientists, is completely exaggerated and unjustified. These questions are at the heart of everyone who feels connected to constructivism. This also applies to the phronetic perestroikans, who, however, reject the necessity or even the possibility of an epistemology without a knowing subject. However, this is a step too far and throws the baby out with the bathwater. I agree with Wolfgang Wieland that one must explore both epistemic instances:

> Certainly, all theoretical knowledge has first of all been worked out by those who know or by authorities striving for knowledge. However, it does not necessarily remain bound to such authorities[70] (Wieland 1986: 33; my translation).

5.1.2 Epistemology without a cognizing subject: limits and possibilities of science as a collective enterprise

Knowledge can also be examined independently of a person or a knowing subject and thus in the same way as is done within epistemology, where the limits and possibilities of science as a collective enterprise are also discussed.

Also, a belief philosopher like Enskat (2005) lists the *truth condition* independently of the other conditions. Discussing knowledge independently of a knowing subject does not force one to assume the existence of a third world, as Popper (1984 1972 and 2012) did. The scientific discourse is just as important to the science project as the individual scientists.

Epistemology without a knowing subject deals with general conditions and special criteria of knowledge and deals primarily with what Reichenbach (1938: 7) called the *context of justification.*

The general, abstract, or universal conditions of knowledge are usually discussed in this chapter, that is, in the epistemology chapter. The concrete, local or special conditions of knowledge are usually discussed in chapter six on methodology. The philosophers focus on the general conditions, while the focus of the individual disciplines is on the special conditions. The former determines the general limits and

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[70] Gewiß ist jedes theoretische Wissen zunächst einmal von Wissenden oder sich um Wissen bemühenden Instanzen erarbeitet worden. Es bleibt jedoch nicht notwendig an solche Instanzen gebunden (Wieland 1986: 33).

possibilities of science by means of rational considerations, the latter generate concrete knowledge by means of concrete methodologies (concepts, propositions, theories, logic, ways of argumentation, methods and methodical approaches). This applies both to the general and specific conditions of a knowing subject and to the general and specific conditions of science as a collective enterprise.

5.2 Epistemic values: general and special conditions or criteria of knowledge

Scientists claim to legitimize knowledge. Since ancient times, the philosophy of science has focused on the questions of how to justify knowledge and how to distinguish knowledge from opinions or pseudo-knowledge. In short, it is about formulating a *demarcation line* between *science* and *non-science* or between *knowledge* that satisfies scientific criteria and *other forms of knowledge*, or in other words, conditions and criteria with the help of which one can distinguish between rational knowledge and other forms of knowledge.

These conditions and criteria constitute the *epistemic norms and values* that every scientist must adhere to if he wants to claim to generate rational or scientifically grounded knowledge. They are, in effect, prescriptive guidelines from the scientific community for current and future scholars. Here only the epistemic values are discussed; the non-epistemic values are treated above in the fourth chapter on axiology and political science (for the distinction between epistemic and non-epistemic values see Kincaid 2007 and 2023, and Van Bouwel 2023).

In this context, Popper speaks of a fundamental problem in science. Scientistic scientists often speak of pseudo-knowledge. The a priori classification of knowledge that has no scientific authority as “pseudo-knowledge” or “ignorance” is not legitimate. It would be more accurate to speak of knowledge that was generated on the basis of scientific authority, and otherwise legitimate knowledge. Much more accurate and viable is the distinction between science and non-science made by Hansson (2016).

First, I will show that the search for criteria is not a modern activity but has ancient antecedents. It is the search for a demarcation line between knowledge and opinion that began with Plato’s discussion of the conditions of knowledge (section 5.2.1).

Second, I will deal with the position of the scientistic scientists. These usually want to justify the demarcation line mentioned above with the help of rationality criteria (section 5.2.2).

Third, practical knowledge should be examined more closely, so the focus here is on practical methodology within political philosophy and political science (section 5.2.3).

Fourth (section 5.2.4), I will explain a *conception of knowledge* that I have reconstructed and further developed which distinguishes between empirical (descriptive-interpretative, explanatory and prognostic) knowledge on the one hand and practical (normative, pragmatic and technical) knowledge on the other, and furthermore makes a distinction between *knowledge* and capability (*Können*) as well as between *theory* and *practice*.

Fifth, the methodological effects of this knowledge conception on scientific operations and scientific discourses will be presented using the example of political science (section 5.2.5).

The perestroikans take a skeptical stance on this issue, denying that such a line between rational knowledge and other forms of knowledge can be drawn at all. While the scientistic scientists justify scientific authority for their own results with the help of conditions or criteria, the perestroikans like to point out that their own research is problem-oriented. Finally, the perestroikans' criticism of the scientific conception of knowledge is explained (5.2.6).

5.2.1 Philosophical foundations of knowledge: general and special conditions of knowledge

A. General conditions of knowledge

The philosophy of knowledge is one of the most important areas of philosophy and has a long tradition. In Theaetetus, Plato puts three different concepts of knowledge up for discussion without finding a definitive answer. In Plato's Dialogues, *two general conditions* of knowledge (*justification* and *truth*) are formulated in some places (Plato 1983d [4th century BC]: 169-172 [*Theaetetus*: 200d-201e], Plato 1983b [4th century BC]: 38-40 [*Menon*: 97a-99a]; see Hintikka 1974 and Wieland 1999b [1982]), which are still present in any scientific definition of knowledge or in modern theories of knowledge. On the other hand, which *specific conditions* can be used to prove truth and justification is disputed.

> This [meaning *Menon* 97e ff., Plato 1983b [4th century BC]] is the classic passage for all those who understand knowledge in Plato as an opinion qualified by certain features and thus interpret it in a strictly propositional sense. But Socrates, even in the context of this discussion, does not mean to advocate such a way of distinguishing between knowledge and opinion. The degree of certainty he claims for this demarcation itself is not that of knowledge, but that of conjecture. But he wants to emphasize the fact that knowledge is different from correct opinion.
> [...]
> In this dialogue [meaning *Theaetetus*, Plato 1983d [4th century BC]] the categorical difference between knowledge and opinion is particularly strongly emphasized. One passage rejects the attempt to interpret knowledge as perception. A second passage attempts to interpret knowledge as true opinion. This approach, too, cannot stand up to closer scrutiny. A third passage seeks to understand knowledge as a justified true opinion. Socrates, however, shows that this interpretation is also untenable. By qualification or specificity of opinion one obviously never arrives at the sought knowledge. This is one of

> the lessons one can draw from the 'Theaetetus', even if the dialogue does not present this lesson in this formulation itself[71] (Wieland 1999b [1982]: 306; my translation).

In a 1963 essay of only two pages, Edmund Gettier (1987 [1963]) showed that truth and justification alone are not enough. He formulated two objections, according to which an opinion could firstly be true by chance or secondly even due to false assumptions. Thus, truth and justification are not enough to justify knowledge. This idea, also known as the *Gettier problem* in the literature, triggered a flood of publications that can only be surveyed by specialists today. Some claim that the Gettier problem is unsolvable (Zagzebski 1994, Bueno 2016).

I focus on important works (Lehrer 1990, Enskat 2005, Kornwachs 2012, Bueno 2016) in order to briefly present the objective and procedure when it comes to justifying knowledge with the help of *general conditions or criteria.*

Keith Lehrer (1990) in a "Final Analysis of Knowledge" comes to the following conclusion; or rather, according to him, knowledge must satisfy the following conditions:

> S knows that p if and only if
> (i) it is true that p,
> (ii) S accepts that p,
> (iii) S is completely justified in accepting that p, and
> (iv) S is completely justified in accepting that p in some way that does not depend on any false statement (Lehrer 1990: 18. Last condition to avoid the Gettier problem, Gettier 1987 [1963]).

Lehrer (1990) provides an analysis of knowledge only in the first chapter (The Analysis of Knowledge, Lehrer 1990: 1-19) and thus of the second, third, and fourth conditions.

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[71] Dies [gemeint ist *Menon* 97e ff., Platon 1983b [4. Jahrhundert vor Christus]] ist die klassische Stelle für alle, die bei Platon das Wissen als eine durch bestimmte Merkmale qualifizierte Meinung verstehen und es damit im strikt propositionalen Sinne deuten. Doch Sokrates will sich auch im Zusammenhang dieser Erörterung für eine solche Art der Abgrenzung zwischen Wissen und Meinung keineswegs stark machen. Der Gewißheitsgrad, den er für diese Abgrenzung selbst in Anspruch nimmt, ist nicht der des Wissens, sondern der der Vermutung. Stark machen will er sich aber dafür, daß Wissen etwas anderes ist als richtige Meinung.
[…]
In diesem Dialog [gemeint ist *Theaitetos*, Platon 1983d [4. Jahrhundert vor Christus]] wird die kategoriale Differenz zwischen Wissen und Meinung besonders stark betont. In einem Durchgang wird der Versuch, Wissen als Wahrnehmung zu deuten, zurückgewiesen. Ein zweiter Durchgang versucht, Wissen als wahre Meinung zu deuten. Auch dieser Ansatz kann einer genaueren Prüfung nicht standhalten. Ein dritter Durchgang will das Wissen als mit Begründung versehene richtige Meinung verstehen. Sokrates zeigt indessen, daß auch diese Deutung unhaltbar ist. Durch Qualifikation oder Spezifikation der Meinung gelangt man offenbar niemals zum gesuchten Wissen. Das ist eine der Lehren, die man aus dem ‚Theaitetos' ziehen kann, auch wenn der Dialog diese Lehre nicht in dieser Formulierung selbst vorträgt (Wieland 1999b [1982]: 306).

He devotes the other eight chapters to the analysis of truth and theories of truth and formulates a coherence theory of truth and thus deals with his first condition in these eight chapters.

For both Keith Lehrer and Rainer Enskat, the *knowing subject* is at the center of the theory of knowledge. Enskat (2005) formulates the following conditions for *authentic knowledge*:

> The person N.N. knows that-p if and only if:
> 1.) N.N. knows how to investigate in a promising way whether-p, or whether-not-p;
> 2.) N.N. has investigated in such a way, as one can investigate in a promising way, whether-p, or, whether-not-p, often enough himself, that is, in an authentic way, whether-p, or whether-not-p;
> 3.) N.N. has investigated often enough himself without error, whether-p, or whether-not-p;
> 4.) N.N. has often enough himself, that is, in an authentic manner, come to the conclusion that-p;
> 5.) N.N. has often enough himself recognized that-p;
> 6.) it is true that-p (Enskat 2005: 124, cf. see 76, 95, 111, 116, and 324; my translation).
>
> The conception of authentic knowledge combines the propositionalistic condition that knowledge worthy of the name must be able to be formulated, communicated and documented in true sentences with a special non-propositionalistic condition: propositional knowledge only deserves the name of knowledge, if it is innately characterized by an authenticity with which it is acquired in an authentic way by a concrete, physical person[72] (Enskat 2005: 14; my translation).

Rainer Enskat (2005) focuses in his analysis only on his first five conditions and thus on a person in the flesh, the *knower* or the *knowing subject.* The *personal anchoring* of knowledge is thus in the foreground. The words “truth” or “theories of truth” do not even appear in the extensive subject index, but the word “perception” appears quite frequently.

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[72] Die Person N.N. weiß, daß-p, dann und nur dann, wenn:
1.) N.N. weiß, wie man in erfolgsträchtiger Weise untersuchen kann, ob-p, oder ob-nicht-p;
2.) N.N. hat so, wie man in erfolgsträchtiger Weise untersuchen kann, ob-p, oder, ob-nicht-p, oft genug selbst, also in authentischer Weise, untersucht, ob-p, oder ob-nicht-p;
3.) N.N. hat oft genug selbst fehlerlos untersucht, ob-p, oder ob-nicht-p;
4.) N.N. ist oft genug selbst, also in authentischer Weise, zu dem Urteil gelangt, daß-p;
5.) N.N. hat oft genug selbst erkannt, daß-p;
6.) es ist wahr, daß-p. (Enskat 2005: 124, vgl. auch 76, 95, 111, 116 und 324)

Die Konzeption des authentischen Wissens verbindet die propositionalistische Bedingung, daß ein Wissen, das diesen Namen verdient, in wahren Sätzen muß formuliert, mitgeteilt und dokumentiert werden können, mit einer speziellen nicht-propositionalistischen Bedingung: Das propositionale Wissen verdient den Namen eines Wissens nur dann, wenn es von Haus aus auch durch eine Authentizität geprägt ist, mit der es von einer konkreten, leibhaftigen Person selbst, eben in authentischer Weise erworben wird (Enskat 2005: 14).

Klaus Kornwachs (2012: 237, see also 223-278) also bases his considerations in this regard on the theories of knowledge formulated since Plato. He distinguishes the following types of knowledge:

a. factual knowledge (report of facts),
b. prognostic knowledge (time-dependent statements),
c. explanatory knowledge (causal, deductive-nomological explanations),
d. explanatory knowledge (practical, practical syllogisms),
e. normative knowledge (goals, metamotivations),
f. logical knowledge (calculi, theorems),
g. definitional knowledge (definitions, conventions about conceptualizations), and
h. instrumental knowledge (rules of methodology).

In addition, there is another distinction, that between *explicit* and *tacit* knowledge:

> Explicit knowledge is knowledge when it can be expressed in the form of guidelines, standards, performance booklets, and protocols; implicit knowledge, on the other hand, is knowledge of skills and abilities that the subject capable of doing so cannot himself explicitly describe[73] (Kornwachs 2008: 138, Kornwachs 2012: 237 ff.; my translation).

Otávio Bueno (2016: 243) formulates four "epistemic features" for observational practices:

a. Counterfactual dependence
b. Robustness
c. Refinement
d. Tracking.

All these theories of knowledge, those of Keith Lehrer (1990), those of Rainer Enskat (2005), and those of Bueno (2016) require a general truth criterion or several general truth criteria because truth is required as a property of scientific discourses. However, this also represents a methodological reductionism, because all knowledge must be *truth-apt*. This also applies to Popper, although for him truth is only a regulative idea or, more precisely, the goal of scientific discourse is the approximation of truth. Here is only truth treated as a *condition* or *criterion* for knowledge. The next subsection deals with theories of truth and truth predicates as properties of statements (section 5.3).

Klaus Kornwachs (2012) formulates general criteria that he uses to classify into different types of knowledge. Furthermore, such criteria are also necessary for propositional knowledge. However, he does not advocate methodological

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[73] Explizit ist das Wissen, wenn es sich in Form von Richtlinien, Normen, Leistungsheften und Protokollen ausdrücken lässt, implizit hingegen, wenn es um Können und Fähigkeiten geht, die das dazu fähige Subjekt selbst nicht explizit beschreiben kann (Kornwachs 2008: 138, Kornwachs 2012: 237 ff.).

reductionism, but methodological pluralism, because according to him, technical rules are not truth-apt, but *efficient* or *inefficient* (Kornwachs 2012: 172, see section 5.3).

B. Criticism of one general criterion of truth or of several general criteria

Leaving aside the knowing subject, then, as has been seen from the epistemologies discussed above, truth remains as the sole criterion by which to distinguish between scientific knowledge and other form of knowledge. This also results in the paramount importance of the concept of truth within the sciences.

Would now a general criterion of truth be sufficient to establish knowledge? According to *Immanuel Kant*, there can be no general criterion of truth, because one always needs *specific* criteria:

> Now a general criterion of truth would be that which was valid of all cognitions without any distinction among their objects. But it is clear that since with such a criterion one abstracts from all content of cognition (relation to its object), yet truth concerns precisely this content, it would be completely impossible and absurd to ask for a mark of the truth of this content cognition, and thus it is clear that a sufficient and yet at the same time general sign of truth cannot possibly be provided (Kant 1998 [1781 and 1787]: 197 [A 58-A 59/B 83]).

This passage in Kant is quite rightly evaluated by Karen Gloy as follows:

> Here [Gloy refers to the passage in Kant just quoted] it is shown not only that but why the question about the criterion of truth is a nonsensical one, namely, because the question about the criterion of truth at all aims at the naming of a general criterion, but the proof of concrete, special true propositions always requires a specific criterion[74] (Gloy 2004: 43; my translation).

Popper also rejects a general criterion of truth:

> Although we have no criterion of truth, and no means of being even quite sure of the falsity of a theory, it is easier to find out that a theory is false than find out that it is true (Popper 1972: 318).

However, the ideal of truth is *not* rejected by him, or by the scientistic scientists to this day:

> Thus the concept of truth plays mainly the role of a regulative idea. It helps us in our search for truth that we know there is something like truth or correspondence. It does not give us a means of finding truth, or of being sure that we have found it even if we have found it. So there is no criterion of truth, and we must not ask for a criterion of truth (Popper1972: 318).

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[74] Hier [Gloy bezieht sich auf die eben zitierte Stelle bei Kant] wird nicht nur aufgezeigt, daß, sondern warum es sich bei der Frage nach dem Wahrheitskriterium um eine unsinnige handelt, nämlich, weil die Frage nach dem Wahrheitskriterium überhaupt auf die Nennung eines allgemeinen Kriteriums zielt, der Ausweis konkreter, spezieller wahrer Sätze aber stets ein spezifisches Kriterium verlangt (Gloy 2004: 43).

Thomas D. Lyons has found a similar formulation, which is also intended to represent the quintessence of scientific realism, according to which science is always in search of truth:

> The battle cry of Socratic scientific realism is the following: science seeks truth and does so rationally irrespective of whether we can justifiably believe we have achieved it (Lyons 2016: 581).

As an alternative to a truth criterion, Popper offers a demarcation criterion, which is supposed to solve the demarcation or fundamental problem of science. He then refers to the falsification principle, with the help of which one can disprove a theory (Popper 1968 [1934]).

C. General and special conditions on ten vertical and three horizontal levels

The studies of Enskat (2005), Lehrer (1990), and Kornwachs (2012), the modern theories of truth (Skirbekk 1977, Gloy 2004), and general analyses in the theory of science (Carrier 2006, Poser 2012 [2001], Bueno 2016) provide very good scientific theoretical foundations for the concept of knowledge. These are general criteria, which can indeed determine tasks, criteria, and properties of knowledge, because the philosophical foundations determine the tasks, conditions, criteria, and properties of the generated knowledge (Kant 1956 [1781 and 1787]: 101 [A 58-A 59/B 83]).

The identification of concrete and special sentences (statements, norms, rules), both for individual statements on the one hand and for individual moral or technical instructions for action as well as legal statements or norms and rules on the other hand, requires special tools, since establishing, generating, identifying or evaluating specific knowledge can only be done with the help of special scientific tools. This applies to all sentences, regardless of what predicates (true/false, right/wrong, just/unjust, wise/unwise, desirable/undesirable, or effective/ineffective) those clauses have. Further criticisms I have of Lehrer (1990) and Enskat (2005) and generally of a theory of knowledge that works with conditions following Plato are as follows:

a. One or more truth conditions alone are not sufficient to justify knowledge. For the identification of concrete knowledge all levels of scientific theory are needed; general (necessary) and special (sufficient) conditions can be found on all these levels (2nd chart, section 9.4.2).

b. Practical (normative, pragmatic and technical) discourses are simply excluded from the discourse of science due to a reductionist methodology, since practical propositions (norms and rules) are not truth-definite but have other properties (predicates) (section 5.3).

Epistemological reductionism seeks to find an Archimedean point, more precisely an *absolute foundation for knowledge or science.* Thus, following Plato's dialogue

Theaitetos (Plato 1983d), conditions of knowledge are formulated (Lehrer 1990, Enskat 2005) or strict methodical procedures (more geometrico) in Descartes (Descartes 2001 [1637], Descartes 1994 [1641], Descartes 2005 [1644]). A similar procedure is used in logical empiricism (Carnap 1998 [1928]) and in Erlangen constructivism (Kamlah/ Lorenzen 1967, Lorenzen/Schwemmer 1975, Lorenzen 1978, Lorenzen 1985). This also applies to critical rationalism (Popper 2005 [1934]), although in contrast to the other positions, only one negative criterion is established.

Otto Neurath used a metaphor to describe both the impossibility of such a reductionist approach and the complexity of all methodological tasks as follows:

> *There is no means of making finally established clean sets of protocols the starting point of science.* There is no tabula rasa. Like mariners we are who have to rebuild their ship on the open sea without ever being able to disassemble it in a dock and rebuild it from the best components. Only metaphysics can disappear completely. The imprecise 'agglomerations' are always somehow part of the ship. If the imprecision is reduced in one place, it can probably even reappear more strongly in another place[75] (Neurath 2006a [1932]: 401, Neurath 2006b [1935]; my translation).

These considerations apply to knowledge in general. Before I now differentiate between different forms of knowledge, methodological approaches that are important for practical knowledge that should be explained (section 6.2).

5.2.2 The view within the explanatory-prognostic tradition: rationality postulates and ethical norms for scientists

The epistemic perspectives mentioned above (section 5.1) also play a role when formulating epistemic values. On the one hand, values are elaborated, legitimized and discussed that are necessary for science to function adequately. This involves general conditions or criteria whose central importance is emphasized linguistically in such a way that one speaks of rationality postulates (section A). On the other hand, one can formulate ethical norms for scientists, i.e. the individual participants in science. It is about practical and political questions concerning the conduct for scientists (section B).

A. Rationality postulates or general criteria of scientific research

Empiricism and *rationality* are the overriding principles that scientific research must comply with, since scientific theories consist of a logical-mathematical formalism and

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[75] *Es gibt kein Mittel, um endgültig gesicherte saubere Protokollsätze zum Ausgangspunkt der Wissenschaften zu machen.* Es gibt keine Tabula rasa. Wie Schiffer sind wir, die ihr Schiff auf offener See umbauen müssen, ohne es jemals in einem Dock zerlegen und aus besten Bestandteilen neu errichten zu können. Nur die Metaphysik kann restlos verschwinden. Die unpräzisen ‚Ballungen' sind immer irgendwie Bestandteil des Schiffes. Wird die Unpräzision an einer Stelle verringert, kann sie wohl gar an anderer Stelle verstärkt wieder auftreten (Neurath 2006a [1932]: 401, Neurath 2006b [1935]).

an empirical interpretation of (political) reality. Science requires a rational formalization and an equally rational approach as well as empirical anchoring. Rational principles or postulates of rationality are fundamental not only in the knowledge of an is, in this case the knowledge of political reality, but also in the legitimation of an ought, here practical regulations within the political system.

The purpose of criteria is to assess the extent to which rational formalization and empirical anchoring have been successful. With the help of *postulates of rationality*, *general* criteria of scientific research are formulated so that methodological (argumentative, logical, methodical and linguistic) precision can be guaranteed.

Aristotle classified various *scientific tools* according to *tool types* in his *Organon*[76] (tool), which can be used to generate knowledge and to distinguish between scientific and non-scientific knowledge:

A. First part: The Categories (*Greek peri ton kategorion, Latin categoriae*) – categories or doctrine of the basic concepts.
B. Second part: Hermeneutics or doctrine of judgment (*peri hermeneias, de interpretatione*).
C. Third Part: First analytics or doctrine of conclusion *(analytika protera, analytica priora*).
D. Fourth Part: Second Analytics or doctrine of cognition (*analytika hystera, analytica posteriora*).
E. Fifth part: The Topics (*topoi, topica*).
F. Sixth part: On the Sophistic Refutations (*peri ton sophistikon elenchon, de sophisticis elenchis*).

Verificationism, which was developed within logical empiricism (Wittgenstein 1984b [1922], Carnap 1998 [1928], Reichenbach 1983 [1938], Stegmüller 1989), strives for an exact verification of all knowledge. It goes hand in hand with the *image or correspondence theory of truth* (section 5.3). Furthermore, it presupposes an *independent observer* who can compare the world as it is and our knowledge of it on a one-to-one basis. This position has rightly been criticized by the Constructivists with reference to Immanuel Kant. Even most naturalists have long since rejected this position and today represent a position in this regard that was developed in particular by Karl Raimund Popper. The perestroikans, like some constructivists, also dismiss the ideal of truth with the correspondence theory of truth and the concept of an independent observer, and thus immediately adopt a skeptical, anti-veritative attitude (section 5.3). They also

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[76] *Sources*: Aristoteles, 1920 [4th century BC]. Online unfortunately only in German at “My Library - zeno.org” (Permalink: http://www.zeno.org/nid/20011779470).

reject objectivity as a meaningful criterion. Neither the skeptical nor the anti-veritative attitude is justified, as I will show in this and the next section.

The most well-known and most widespread demarcation criterion in political science among scientistic scientists, with the help of which one can distinguish between scientific knowledge and other knowledge or science and non-science, is the *falsifiability* proposed by Karl Raimund Popper. This demarcation criterion is intended to separate empirical science and statements of logic and mathematics on the one hand, and metaphysics; myths or pseudoscience on the other:

> We must clearly distinguish between falsifiability and falsification. We have introduced falsifiability solely as a criterion for the empirical character of a system of statements. As to falsification, special rules must be introduced which will determine under what conditions a system is to be regarded as falsified.
> We say that a theory is falsified only if we have accepted basic statements which contradict it (cf. section 11, rule 2). This condition is necessary, but not sufficient. […]
> Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified. We shall take it as falsified only if we discover a *reproducible effect* which refutes the theory [sufficient condition; my interpretation]. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a *falsifying hypothesis* (Popper 1968 [1934]: 86-87; see chapter IV. Falsifiability, 78-92).

In his introduction to the philosophy of science, Holm Tetens does not speak of criteria, but of ideals that are striven for within science:

> In a sophisticated sense, an idea is an ideal that things in the world fulfill better or worse and by which they are measured or rated. This is always true for the idea of science[77] (Tetens 2013: 17; my translation).

According to Tetens, the ideal of science can be subdivided or, to put it another way, the following criteria can be used to evaluate scientific results:

a. Ideal of truth
b. Ideal of justification
c. Ideal of explanation and understanding
d. Ideal of intersubjectivity
e. Ideal of self-reflection

The following general criteria, conditions or ideals are recognized by scientistic scientists and by all scientists who favor a logical-mathematical research methodology:

a. Intersubjectivity (trans-subjectivity): Science seeks ways to find reasons that any reasonable and knowledgeable person can understand.

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[77] In einem anspruchsvollen Sinne ist eine Idee ein Ideal, das Dinge in der Welt besser oder schlechter erfüllen und an dem man sie misst oder bewertet. Das gilt allemal für die Idee der Wissenschaft (Tetens 2013: 17).

b. Objectivity: Subjective desires or prejudices must not enter into the work, only intersubjective reasons.

c. Reliability (auditability): The results of scientific investigations should be reproducible under the same conditions.

d. Validity: A scientific result must have argumentative weight and meet methodological-logical quality criteria. Argumentative, logical, methodological and linguistic precision are required (Druwe 1995: 21-24). A distinction is made between internal validity (credibility and authenticity) and external validity (transferability or fit).

Even the authors of the Political Methodology handbook (Box-Steffensmeier/ Brady/Collier 2010a [2008]), i.e., scientistic political scientists or the scientistic establishment, do not question these criteria, but try to meet them by looking for ways to fulfill them using scientific tools.

The list of general conditions or criteria can be extended. Kincaid suggested falsifiability, empirical accuracy, scope, coherence, fruitfulness, objectivity (Kincaid 1996: 50-51). These were suggested for scientistic social scientists. The ones I have mentioned apply to all scientific traditions.

The words "pseudo-science" or "pseudo-scientific" are used very pejoratively. First, they claim that this knowledge cannot be scientifically justified. Secondly, that it is a matter of false knowledge or even thirdly, it is nonsense knowingly and falsely passed off as knowledge.

At the beginning of the 21st century, a further development also took place within the explanatory-prognostic tradition, so that one does not classify all non-scientifically founded forms of knowledge as pseudo-knowledge or pseudo-science. Hansson (2016) distinguishes between *science* and *non-science.*

In addition to religion, there are other forms of knowledge that *cannot or cannot yet* be scientifically justified. Among these Hansson rightly counts practical, often non-propositional knowledge. This includes both the knowledge to cope with everyday practice and the fine arts:

> One of the most widespread type of non-scientific knowledge is the knowledge that we all have about how to archive things in practical life. This includes trivial knowledge, such as how to open a door and make coffee. It also includes knowledge that takes year to master and is typically part of the special competence of a profession (Hansson 2016: 489).

The four forms of knowledge that Hansson distinguishes are also worth mentioning:

> (1) Scientific factual knowledge
> (2) Nonscientific factual knowledge
> (3) Science based action knowledge
> (4) Action knowledge not based on science (Hansson 2016: 489).

The perestroikans place themselves in a long tradition of scientists who conduct qualitative or problem-oriented research and who doubt the above-mentioned criteria, especially the objectivity of scientific results, and even question the possibility of an objective approach.

Ines Steinke, who feels an affinity with qualitative-interpretative rather than quantitative research, rightly points out in her article on “Quality Criteria of Qualitative Research” (Steinke 2015 [2000]) that a total rejection of criteria, as is especially common in postmodernism, is unconvincing and does a disservice to qualitative-interpretative research.

> Nor does the tendency to increasingly conceive of qualitative research as an art doctrine (cf. Denzin 1994) or ‘research style’ (Strauss 1987, 1985) rather than as a procedure that can be formalized exempt it from the application of evaluative criteria[78] (Steinke 2015 [2000]: 322; my translation).

If one wants to construct a theory of art for exploring and verbalizing subjective experience, then one would also have to deal with the private language argument (section 5.3.8). One would have to show how a subjective methodology or subjective art theory works in concrete terms and address the objections to a private language. So far, interpretivists and perestroikans also use an intersubjective tool, namely language, to verbalize subjective utterances.

Steinke, like most interpretivists (Schwartz-Shea 2014 [2006]), disagrees with the tendency to introduce “[q]uantitative criteria for qualitative research”, such as is done by researchers who advocate the development of qualitative-mathematical methods (King/Keohane/Verba 1994 and Brady/Collier 2010 [2004], section 6.9). However, she calls for

> core criteria for evaluating qualitative research. [...]
> This is less about formulating individual criteria, as is often the case. Rather, a system of criteria that covers as many aspects of the evaluation of qualitative research as possible is necessary. This must also include ways of operationalizing the criteria that enable their concrete examination[79] (Steinke 2015 [2000]: 322-323; my translation).

Steinke rejects the criteria “objectivity”, “reliability” and “validity” for qualitative research, as these were created for quantitative research. She also distinguishes between “intersubjective verifiability” and “intersubjective comprehensibility”, the

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[78] Auch die Tendenz, qualitative Forschung zunehmend als Kunstlehre (vgl. Denzin 1994) oder ‚Forschungsstil‘ (Strauss 1987, 1985) und weniger als formalisierbare Vorgehensweise aufzufassen, entbindet nicht von der Anwendung von Bewertungskriterien (Steinke 2015 [2000]: 322).

[79] Kernkriterien zur Bewertung von qualitativer Forschung. [...]
Dabei geht es weniger darum, einzelne Kriterien zu formulieren, wie dies häufig der Fall ist. Vielmehr ist ein System von Kriterien, das möglichst viele Aspekte der Bewertung qualitativer Forschung abdeckt, notwendig. Dieses muss auch Wege der Operationalisierung der Kriterien beinhalten, die deren konkrete Prüfung ermöglichen (Steinke 2015 [2000]: 322-323).

former being relevant for quantitative research, the latter for qualitative research (Steinke 2015 [2000]: 323 and 324). The fact that one speaks of verification when using logical-mathematical tools and of comprehensibility when using linguistic-interpretive ones is, in my opinion, not decisive. What is more important here is the adjective “intersubjective” – so intersubjectivity is required in both methodologies. Furthermore, in interpretative research, in addition to intersubjectivity, the other criteria mentioned above (objectivity, reliability, and validity) are also important, as long as they are understood as generally as presented above.

It is certainly true that depending on the object of research and the research methodology used, specific criteria are also needed. In my opinion, however, it makes sense to formulate general criteria or, as Steinke writes, “core criteria” (Steinke 2015 [2000]) for all research methodologies. Which specific criteria are needed for concrete operationalization within quantitative or qualitative research should be discussed independently.

The four criteria mentioned above (intersubjectivity (trans-subjectivity), objectivity, reliability, and validity) therefore apply, in my view, to mathematical (quantitative and qualitative-mathematical), to linguistic (qualitative-interpretative) research methodology, and practical research methodology. Not in the sense that one can derive any individual criterion or justify any single statement from these postulates; they are ideals that both quantitative and qualitative researchers can use as orientation (see 5.4.4 Prima facie property of norms).

In summary, a complete rejection of criteria is not convincing; rather, what is important is that the existing “system of criteria” (Steinke 2015 [2000]) should be further developed. In particular, the specific criteria require constant improvement, and this across all research methodologies, while the general criteria will be less affected by changes or additions.

The difference between scientific knowledge, e.g. scientific policy advice, on the one hand, and other knowledge (non-science), e.g. subjective ideologies, utopias, slogans or wishes, on the other hand, can therefore not be determined with a demarcation criterion, as Popper or Lorenzen claim. Even the four general criteria or core criteria listed above are not sufficient on their own. In my view, an evaluation or a justification of knowledge requires both general and specific criteria on ten methodological levels. Therefore, in this thesis, the ten vertical and three horizontal levels form the systematic foundation according to which knowledge is methodologically evaluated and legitimized (1st and 2nd chart, section 9.4.1 and 9.4.2).

B. Ethical norms for scientists: Guide to Professional Ethics in Political Science and Data Access – Research Transparency (DA-RT)

Ethical, practical and political issues have been intensively discussed in science since the 20th century in particular. Resnik (2016: 256-257) has listed the following ethical standards, which are recognized by almost all scientists: Honesty, Openness, Care–fulness, Freedom, Due credit, Respect for colleagues, Respect for human research subjects, Animal welfare, Respect for intellectual property, Confidentiality, Legality, Stewardship, Competence and Social responsibility.

Ethical dilemmas can also arise when individual scientific norms conflict with each other or when they conflict with other ethical and social norms.

The APSA (American Political Science Association) set up a working group in 1967 to deal with the professional standards and personal responsibility of scientists. In 1968 the so-called Berstein Report, named after the chairman Marver H. Bernstein, was published under the title "Ethical Problems of Academic Political Scientists". A revised version was published in 2012, now entitled A Guide to Professional Ethics in Political Science (APSA 2012 [1968]). This ethical guide contains general criteria for the *knowing subject*, the political scientist.

The Data Access – Research Transparency (DA-RT, https://www.dartstatement.org) initiative is about formulating criteria for evidence-based social science research. The following requirement was included in the APSA ethical guidelines: "Researchers have an ethical obligation to facilitate the evaluation of their evidence-based knowledge claims through data access, production transparency, and analytic transparency so that their work can be tested or replicated" (APSA 2012 [1968]: 9). In other words, the transparency criteria formulated in DA-RT are nothing but general criteria for an *epistemology without a knowing subject,* i.e. the system of (political) science.

While scientistic scientists have no problem with the DA-RT transparency rules, there are criticisms from other scientific traditions (Van Bowel 2023: 867-870). This criticism of the scientistic conception of knowledge, or more precisely of general criteria of knowledge, is not new (section 5.2.6).

5.2.3 Methodological approaches of practical or problem-oriented social sciences: the three levels of practical analysis and the three practical (normative, pragmatic, and technical) discourses

In the contrast between the normative theories and the neo-positivist empirical-analytical theories lives on the old conflict between Aristotelian politics as a practical philosophy and the rationalist and empirical theories of modern times since Machiavelli, Bacon and

> Hobbes. The latter are primarily based on a technical-rational concept of the political[80] (von Beyme 2000 [1972]: 39; my translation).

This conflict continues in the 21st century as scientistic scientists present *normative rational choice* theory as the crowning glory or current victor in the eternal revolutionary struggle of normative theories (section 6.10). The perestroikans, on the other hand, activate *applied phronesis* from the Aristotelian tradition. This is a narrowing down that does not do justice to the Aristotelian tradition. The Aristotelian tradition offers much more important starting points for a practical (normative, pragmatic and technical) methodology. This will be briefly explained below (chart 8, 9.4.8).

In the Aristotelian tradition, practical sciences stand on an equal footing with theoretical (empirical) sciences. Within the practical sciences, one can, in my opinion, ideally distinguish three different discourses: *normative discourses of values*, *pragmatic discourses of goals*, and *technical discourses of means*. They are all concerned with the generation of *practical* (normative, pragmatic and technical) *knowledge* with the help of a *practical* (normative, pragmatic and technical) *methodology* (Lauer 2017).

The explanative-prognostic or the Platonic-Galilean tradition, however, is concerned with the determination of empirical (descriptive-interpretative, explanatory, and prognostic) knowledge. As described above (section 3.2.1), the search for causalities enables, for the first time, the recognition and, through the inversion of causalities to change political reality. Within policy field analysis as well as governance research, three levels are also distinguished, and not only by researchers belonging to the explanative-prognostic or the Platonic-Galilean tradition.

Weber is also aware of the existence of normative discourses, so he cannot be accused of reducing practical discourses to purely technical discourses:

> The hallmark of the socio-political character of a problem is precisely that it is not settled on the basis of merely technical considerations arising from fixed purposes, that there can and must be a dispute about the regulative value standards themselves, because the problem extends into the region of general cultural questions[81] (Weber 1973c [1904]: 153; my translation).

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[80] Im Gegensatz zwischen den normativen Theorien und den neupositivistischen empirisch-analytischen Theorien lebt der alte Konflikt zwischen der aristotelischen Politik als praktischer Philosophie und den rationalistischen und empirischen Theorien der Neuzeit seit Machiavelli, Bacon und Hobbes fort, die sich vornehmlich an einem technisch-rationalen Begriff des Politischen orientieren (von Beyme 2000 [1972]: 39).

[81] Das Kennzeichen des sozialpolitischen Charakters eines Problems ist es ja geradezu, dass es nicht auf Grund bloß technischer Erwägungen aus feststehenden Zwecken heraus erledigt ist, daß um die regulativen Wertmaßstäbe selbst gestritten werden kann und muß, weil das Problem in die Region der allgemeinen Kulturfragen hineinragt (Weber 1973c [1904]: 153).

In the following, these three practical-methodological levels within ethics and practical philosophy are presented and compared with the different approaches within empirical political science (2nd and 8th chart, section 9.4.2 and 9.4.8).

A. Technical means discourses

> At the *lowest level*, ways and means are evaluated for their suitability for any purpose or goal[82] (Höffe 2009 [2007]: 23; my translation).

By means of technical rationality, technical imperatives are generated within these discourses, be they *technical individual rules* or *social-technological regulations.*

Empirical policy analysis in political science has produced a different terminology within various methodological approaches (governance perspective, advocacy coalition approach, actor-centered institutionalism, institutional analysis and development framework).

The first level is the "level of an operative practice of directly dealing with problems" (Haus 2010: 109; my translation), in the governance perspective "first order governing" or "opportunity creation" (Kooiman 2003: 135 ff.); in actor-centered institutionalism this level is called "industry structure" (Mayntz/Scharpf 1995).

The lowest level of the advocacy coalition approach is referred to as "instrumental decisions" (Sabatier/Jenkins-Smith 1999: 133) or "secondary aspects of a coalition belief system". According to this approach, e.g. "policy preferences regarding desirable regulations" and "design of specific institutions" (Sabatier/Jenkins-Smith 1999: 122) are examined at this level.

Instrumental decisions are also analyzed at the lowest level of the Institutional Analysis and Development Framework, where they are called "Operational Rules-in-Use" (Ostrom/Cox/Schlager 2014: 285):

> Operational rules directly affect day-to-day decisions made by the participants in any setting (Ostrom/Cox/Schlager 2014: 284, 8th chart, section 9.4.8).

Technical imperatives are *effective* or *ineffective.*

B. Pragmatic objective discourses

A derivation of technical regulations or individual instructions for action from pragmatic objectives and purposes (strategies for action) or from ethical-moral norms (maxims for action) or even a subsumption under strategies for action or maxims for action is demanded by normative-ontological scientists, but is not possible (section 4.1.3 and section 4.2). Two further discourses are therefore required.

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[82] Auf der *untersten Stufe* bewertet man Mittel und Wege auf ihre Tauglichkeit für beliebige Absichten oder Ziele (Höffe 2009 [2007]: 23).

In the pragmatic objective discourses, pragmatic imperatives are generated with the help of pragmatic rationality, whether individual pragmatic rules or social pragmatic regulations:

> At the second level of evaluation, what is merely presupposed at the lowest level, the objective is evaluated in its turn[83] (Höffe 2009 [2007]: 24-25; my translation).

With the help of *pragmatic rationality*, *individual pragmatic rules* or *social pragmatic regulations* are justified.

This "level of structuring of problem-solving practices through 'institution building'" (Haus 2010: 109; my translation) is called "second order governing" or "institution building" (Kooiman 2003: 153 ff.) in *governance research*, while in *actor-centered institutionalism* one speaks of "governance structure" (Mayntz/Scharpf 1995: 16).

The middle level of the *advocacy coalition approach* is called "policy core belief systems" and consists of policy strategies with which central values are implemented,

> normative commitments and causal perceptions across an entire policy domain or subsystem (Sabatier/Jenkins-Smith 1999: 121, cf. 133).

The second level of the *Institutional Analysis and Development Framework* is called "Collective Choice Rules-in-Use" (Ostrom /Cox /Schlager 2014: 285):

> Collective-choice-rules affect operational activities and results through their effects in determining who is eligible and the specific rules to be used in changing operational rules (Ostrom/Cox/Schlager 2014: 284, 8th chart, section 9.4.8).

Pragmatic objectives and purposes (strategies for action) are *wise* or *unwise*.

C. Normative value discourses

The third and highest level in *practical philosophy* or *political philosophy/theory* is the normative value discourse. This is where ethical-moral rationality comes into play. Otfried Höffe distinguishes between *virtue morality* and *ethical-moral norms* on the one hand and *legal morality*, (political) justice and legal norms on the other hand (Höffe 2009 [2007]: 23-26). I adopt Höffe's classification, but sometimes use different terminology, as you can easily see in this chapter.

For example, one could justify ethical-moral norms with the categorical imperative of Kant. Legal morality is most important to political science. With the help of normative value discourses, maxims of action such as principles of justice could be formulated, as John Rawls (1979 [1971]) does. No rules can be derived from principles of justice, and certainly not concrete instructions for action, but one can evaluate political objectives (strategies for action) and political means (action instruments) in terms of the extent to which they are *just* or *unjust*.

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[83] Auf der *zweiten Bewertungsstufe* wird, was man auf der untersten Stufe bloß voraussetzt, das Ziel seinerseits bewertet (Höffe 2009 [2007]: 24-25).

Within normative objective discourses, maxims of action (normative guidelines) are justified; these then form a foil for the critical evaluation of strategies for action, instruments of action or individual instructions/tools for action – just as morality forms a foil for the evaluation of the legal system. On this level, no values, principles, etc. are (ultimately) justified from which concrete instructions for action can then be derived, as Hans Albert, for example, assumes for all normativists:

> The normativist, on the other hand, needs value premises for derivation within his system[84] (Albert 1967b [1965]: 197; my translation).

The maxims of action serve to critically question, for example, the strategies for action and instruments/tools of action concretely implemented in a political system.

At the highest level, *actor-centered institutionalism* speaks of a "design perspective" (Haus 2010: 109), the *governance perspective* knows

> third order governing, metagovernor, meta governance, who or what – ultimately – governs the governors (Kooiman 2003: 170 ff.).

The highest and most comprehensive level of the *advocacy coalition approach* exists in the "deep core belief system" consisting of normative and ontological axioms,

> basic ontological and normative beliefs, such as the relative valuation of individual freedom versus social equality (Sabatier/Jenkins-Smith 1999: 121, cf. 133).

The highest level of the *Institutional Analysis and Development Framework* is the "Constitutional Rules-in-Use" (Ostrom/Cox/Schlager 2014: 285):

> Constitutional-choice rules affect operational activities and their effects in determining who is eligible and the rules to be used in crafting the set of collective-choice rules that in turn affect the set of operational rules (Ostrom/Cox/Schlager 2014: 284, 8th chart, section 9.4.8).

Ethic or moral norms are *right* or *wrong*. Political norms are *just* or *unjust*.

D. Evaluation of the three practical levels of analysis or discourses

The subdivision reconstructed by Höffe (2009 [2007]) clearly belongs to the Aristotelian-Kantian tradition, because here practical analyses are fundamentally separated from theoretical (empirical) analyses. The situation is very different in empirical policy analysis or governance research, where many approaches reject the *separation of "is" and "ought"*.

In the explanative-prognostic or the Platonic-Galilean tradition, practical discourses, which used to consist of three separate discourses, are reduced to purely technical means discourses. By means of causal analyses, one can firstly recognize the world and secondly change the world; through the application of causalities, i.e. inversion of

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[84] Der Normativist dagegen braucht *Wertprämissen* zur Ableitung innerhalb seines System. (Albert 1967b [1965]: 197).

causal propositions, applied, technical regulations emerge. For the explanative-prognostic or the Platonic-Galilean tradition, a methodological, especially causal and empirical reductionism, as well as a logical-mathematical research methodology are sufficient to both know and change the world.

In contrast, within the Aristotelian tradition, there is methodological pluralism. The search for causalities is not rejected but is supplemented by an understanding of contexts of meaning by means of a linguistic-interpretative research methodology in particular. Added to this is a practical (normative, pragmatic and technical) methodology for changing the world, since an applied, purely technical approach (social technology), as I have shown (section 4.1.3, E), is not sufficient.

The levels of analysis developed in policy analysis and governance research lead to an appropriate differentiation. A differentiation is unfortunately neglected by some authors: it is the separation between *is* and *ought*. The assumption of an equivalence between cognition and action, which underlies causal reductionism, removes this differentiation, but it is never discussed.

Otfried Höffe (2009 [2007]) showed by reconstructing the three levels of evaluation, which mainly go back to Aristotle and Kant, that political philosophy is possible without having to resort to ontological-normative or neo-Marxist derivation schemes. Neither individual-pragmatic rules or socio-pragmatic regulations nor technical individual rules or socio-technological regulations can be derived from the ethical-moral norms or legal norms established in the third evaluation stage.

Maxims of action, which were founded in normative value discourses, and strategies for action, which were founded in pragmatic objective discourses, are taken into account when creating instruments/tools of action in technical discourses on means in that they represent the objective for the means as prescriptive norms. They are also used to evaluate instruments for action (technical regulations) and instructions for action.

John Rawls (1979 [1971]) proceeds quite differently:

> The role of the philosopher revived by John Rawls is that of the universalist nomothete, the constitution-maker, who attempts to design a universally valid order of human coexistence from an Archimedean, society- and history-neutral position. In doing so, he makes virtuoso use of the fund of categorial patterns and forms of reflection of classical modern political philosophy[85] (Kersting 2007 [1985]: 21; my translation).

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[85] Die von John Rawls wieder belebte Philosophenrolle ist die des universalistischen Nomotheten, des Verfassungsgebers, der von einem archimedischen, gesellschafts- und geschichtsjenseitigen Standort aus eine allgemeingültige Ordnung menschlichen Zusam–menlebens zu entwerfen versucht. Dabei bedient er sich virtuos aus dem Fundus der

A practical political science, as I envisage it here, thus differs decisively not only from the normative-ontological approaches and the critical-dialectical approaches, but also from John Rawls' normative theory of justice. The search for an Archimedean point from which to formulate a just political order is rejected due to the limitations of scientific research (section 5.4) and is considered unfeasible.

5.2.4 Knowledge versus capability, theory versus practice

A. Definition of knowledge

Taking into account the previous discussions on the conditions or criteria of knowledge, one can formulate the following definition of scientific knowledge:

***Scientific knowledge* consists of empirical and practical *theories,* of *philosophical foundations* as well as of *scientific tools,* and has a *hypothetical character*.**

For the identification of concrete and specific propositions (statements, norms, rules) both for individual statements on the one hand and for individual moral or technical instructions for action as well as laws, norms and rules on the other hand, scientific tools are required.

Scientific tools are needed, since only with their help can concrete knowledge be justified, generated and identified. This applies to all propositions (statements, norms or rules), regardless of what predicates (true/false, right/wrong, just/unjust, wise/unwise, desirable/undesirable or effective/ineffective) these propositions have.

B. Knowledge versus capability

The distinction made by Gilbert Ryle (2009 [1949]) between "knowing that" and "knowing how", which is often used in science, is not adopted in my investigations (10th chart, section 9.4.10) because the difference between science and practice, in particular technical science and technology, is not taken into account, i.e., practical knowledge and practical capability are equated. However, this is merely based on ambiguous statements in English, as Kurt Erich Maria Baier, Gilbert Ryle's translator, rightly points out.

The use of the English words "knowing how" and "knowing that" does not lead to more precise explanations, but to linguistic confusion. The German translation shows that these terms are extremely inaccurate and therefore by no means lead to a clarification of the logical geography of knowledge, as Ryle believed (see also section 5.2).

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kategorialen Muster und Reflexionsformen der klassischen neuzeitlichen politischen Philosophie (Kersting 2007 [1985]: 21).

I make a distinction between knowledge and capability: *Knowledge* (*knowing that*) considered as theoretical (*analytical and empirical*) knowledge is the knowledge that something is the case, consisting of statements, including statements about norms and rules. In contrast, practical knowledge is the knowledge of how to do something, consisting of statements, norms and rules. Both are *explicit*, *propositional knowledge*. I also count practical knowledge as propositional knowledge, because norms and rules are also propositions just like statements, but have different properties.

Capability is composed of dispositions, competencies, skills, abilities – how to do something. This area is often treated under the label of *implicit*, *non-propositional knowledge*. It is only a subset of know-how, that of *practical skill*. It can be distinguished from *practical knowledge*, which can be formulated explicitly in *propositional* form (norms or rules).

But this is not to deny that there is a non-propositional knowledge (in my opinion it is a *capability*) or a domain that cannot be explicated. This has been discussed since antiquity. Wolfgang Wieland, in his interpretation of Plato, placed particular emphasis on *non-propositional* knowledge.

> One can certainly not expect from the one who has non-propositional knowledge that he presents this knowledge in the form of propositions, but that he can give an account of it with the help of propositions. In any case, Socrates knows that he cannot communicate the knowledge by which he excels in the form of propositions. But he does not refer to this knowledge like to an oracle. He proves it in the fact that he never loses his orientation in dealing with sentences. Nor does he ever lose contact with the realm of sentences. But he always keeps the distance towards them, which prevents him from falling into this realm. One can hardly do justice to Plato's philosophizing if one overlooks the tension that exists between the sentences handed down in the written work and what is expressed and shown only with the help of these sentences, without, on the other hand, being intended by them as a semantic correlate in a thematic way. Any talk of a Platonic doctrine remains unclear and ambiguous if it does not take these connections into account[86] (Wieland 1999b [1982]: 324; my translation).

In practical discourses, it is primarily a matter of explicit, practical knowledge, especially about *capability* and *skill*, which is available in *propositional form*.

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[86] Man kann von demjenigen, der über nichtpropositionales Wissen verfügt, gewiß nicht erwarten, daß er dieses Wissen in Gestalt von Sätzen präsentiert, wohl aber, daß er mit Hilfe von Sätzen von ihm Rechenschaft geben kann. Sokrates weiß jedenfalls, daß er das Wissen, durch das er sich auszeichnet, nicht in Gestalt von Sätzen mitteilen kann. Doch er beruft sich auf dieses Wissen nicht wie auf ein Orakel. Er bewährt es darin, daß er im Umgang mit Sätzen niemals die Orientierung verliert. Auch verliert er niemals den Kontakt zum Bereich der Sätze. Doch er behält ihnen gegenüber immer die Distanz, die ihn davor bewahrt, diesem Bereich zu verfallen. Man wird Platons Philosophieren schwerlich gerecht, wenn man die Spannung übersieht, die zwischen den im geschriebenen Werk überlieferten Sätzen und dem besteht, was nur mit Hilfe dieser Sätze ausgedrückt und gezeigt wird, ohne daß es hingegen von ihnen als semantisches Korrelat auf thematische Weise intendiert würde. Jede Rede von einer platonischen Lehre bleibt unklar und zweideutig, wenn sie diese Zusammenhänge nicht beachtet (Wieland 1999b [1982]: 324).

Propositions within practical discourses can firstly be empirical (descriptive-interpretative, explanatory or prognostic) knowledge in the form of statements about facts, but also statements about norms, values, standardizations or regulations. Second, they can be about practical (normative, pragmatic or technical) knowledge in the form of norms and rules. Norms and rules are also propositions, but they do not reduce to statements.

Therefore, the discussion about *non-propositional knowledge*, which since the 20th century has been conducted mainly under the concept of *tacit knowledge*, can be left aside here (Polanyi 1958 and 1985 [1967], Mannheim 1980, Loenhoff 2012b, Schützeichel 2012), especially since the expression and concept "tacit knowledge" may possibly be better represented by "traditionally established expressions and concepts" (Kogge 2012: 31; my translation), namely of *experience* (*empeiría*), as long as a capability is not referred to.

> A knowledge can be passed on; an experience must have been made 'on one's own body'[87] (Schneider 2012: 77; my translation).

In my opinion, this also provides a good dividing line between an epistemology without a cognizing subject (section 5.1), which deals with explicit knowledge, and an epistemology with a cognizing subject, which primarily examines the cognizing subject and its possibilities and limitations. The latter discusses the preconditions of cognition, the emergence of knowledge and the emergence of experience. Epistemology without a cognizing subject is mainly concerned with the context of justification of knowledge, whereas epistemology with a cognizing subject is concerned with the context of discovery (Reichenbach 1938: 6-7). Neither a possible distinction between *experience* and *capability* nor the relation between the two can be discussed in more detail here.

With these distinctions in mind, one can see why misunderstandings arise between scientistic scientists and perestroikans on this issue. While scientistic scientists work primarily on social technological knowledge in the form of propositional propositions (statements and rules as well as propositional systems and regulations), perestroikans are interested primarily in implicit, non-propositional knowledge. They want to help citizens achieve a level of expertise through applied phronesis that enables them to identify tension points and influence the political world in their favor. One does not exclude the other; these two concepts can also be thought of and treated as complementary.

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[87] Ein Wissen lässt sich weitergeben, eine Erfahrung muss man ‚am eigenen Leibe' gemacht haben (Schneider 2012: 77).

C. Forms of knowledge

Due to the structural differences between different *types of tools* explained above, the following forms of knowledge as well as distinctions between knowledge versus capabilities and theory versus practice result (10th chart, section 9.4.10):

a. *Analytical knowledge*: Conceptual and logical truths in the form of non-empirical, truth-apt statements.
b. *Procedural knowledge*: Methodologies in the form of truth-apt propositions.
c. *Empirical knowledge* in the form of scientific or social scientific statements or statement systems, including statements about norms and rules. Both analytical and empirical statements are *true* or *false.* There are three categories of empirical knowledge:
    I. *Descriptive-interpretative knowledge* in the form of truth-apt descriptions.
    II. *Explanative knowledge* in the form of truth-apt explanations.
    III. *Prognostic knowledge* in the form of truth-apt predictions.
d. *Practical knowledge* in the form of well-founded standards and regulations. There are three categories of practical knowledge:
    I. *Normative knowledge* in the form of *maxims for action*, moral and normative judgments and norms that are *right* or *wrong*. Political judgements and norms are *just* or *unjust.*
    II. *Pragmatic knowledge* in the form of *strategies for action* and pragmatic judgments consisting e.g. of different methodological approaches to cure the same illness. Pragmatic rules are *wise/unwise* or *desirable/undesirable.*
    III. *Technical knowledge* in the form of *instruments of action* and technical judgments, consisting, for example, of methods that contain concrete technical rules for curing an illness. Technical rules are *effective* or *ineffective.*

Practical knowledge is not just “knowing how to do something” (Baier in: Ryle 1969 [1949]: 26; my translation), but consists of three different components:

a. why or *normative component*, consisting of ethical-moral and political-normative values, maxims of action,
b. why or *pragmatic component*, objectives, goals and purposes, strategies for action,
c. how or *technical component*, means, here instruments of action, something is to be done.

A *capability* includes the practical competence to implement empirical and practical knowledge, “to be able to do something” (Kurt Baier in: Ryle 1969 [1949]: 26; my translation), the art of the doctor, citizen, craftsman, artist, engineer, teacher, manager, politician, scientist etc. to provide services in their field.

The personal anchoring of (authentic) knowledge can be verified using the first five conditions for authentic knowledge formulated by Rainer Enskat (2005: 124), or it can simply be determined whether someone is successful in practice.

Empirical (theoretical) sciences develop and justify empirical knowledge. Examples: natural sciences, empirical social sciences. Analytical and empirical knowledge is also propositional knowledge because both are formulated in the form of statements.

Actors are scientists, e.g. political scientists generate empirical and/or practical knowledge. Natural scientists generate empirical knowledge, technical sciences practical knowledge.

Practical (normative, pragmatic and technical) sciences elaborate and substantiate practical knowledge. Examples: medical sciences, technical sciences, practical social sciences.

The following political actors have a skill and thus practical competence: citizens, politicians, civil servants, administrators, entrepreneurs. They are all practitioners, act in practice and can also bring about political decisions.

D. Knowledge (theory) versus practice (action)

a. Knowledge: sphere of cognition

A scientist is always a theoretician, regardless of whether he makes empirical statements about political reality using an empirical methodology or whether he also justifies norms or regulations with a practical methodology. In the first case, the scientist generates empirical knowledge or theories, in the second practical knowledge or theories.

If one accepts the distinctions made above, there are no *applied* sciences, only *practical* sciences and *scientifically* trained *practitioners* who apply the forms of knowledge described above, and *scientists* who legitimize scientific knowledge.

b. Practice: sphere of action

A *practitioner* (citizen, politician, civil servant, administrator, entrepreneur) changes the (political) reality, whether he refers to scientifically based, empirical and practical knowledge and makes rational decisions or makes subjective gut decisions (11th chart, section 9.4.11).

Knowledge (theory) and practice (action) are thought of as complementary and not hierarchical. An *equivalence* between the two, as is usual in the Bacon program, cannot be justified either.

5.2.5 Scientific operations using the example of political science

> Theories of politics involve three operations:
> - Statements about political facts, about what is.
> - Statements about causal relationships combined with predictions about what is likely to

> be in the future.
> - Conclusions about desirable developments and reflections on what should be[88]
> (von Beyme 2007 [1991]: 11; my translation).

Because of the structural differences between tool types, forms of knowledge, and types of science, as elaborated in this chapter, theories of politics, in my view, involve *seven* operations. One is a logical-analytical operation. Three empirical (descriptive-interpretative, explanative, and prognostic) operations relate to what is, and three practical (normative, pragmatic, and technical) operations to what ought to be. In each of these operations different goals are pursued and different scientific tools (concepts, theorems, theories, logics, ways of reasoning, methods and methodical approaches) are used (chart 9, section 9.4.9).

A. Analytical operation of political science

Analytical discourses comprise analytical operations and generate analytical knowledge. These include above all political concepts or categories, but also models for analyzing political reality. These are conceptual or logical truths in the form of non-empirical, truth-apt statements.

B. Empirical operations in political science

Empirical discourses involve empirical operations about what is or what constitutes political reality. Here, *truth-apt statements* (descriptions, explanations and forecasts), as well as truth-defined statements, are made about applicable norms and regulations of a political system. Within empirical discourses, three different discourses or operations can be distinguished: descriptive-interpretative, explanative and prognostic discourses or operations.

a. Descriptive-interpretative operation: descriptions of political reality

The first thing to do is to describe the political reality. What is, moves into the center of attention – with descriptive-interpretative methods, a picture could emerge of how everyday political life is shaped in a political system: power structures, dependencies and political decision-making processes are considered and examined in more detail. This also includes statements about action maxims (guidelines, norms, principles and values). These are recognized and described, e.g. in all state constitutions. But this also includes a detailed description of the action strategies and action instruments, for example the social security systems.

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[88] Theorien der Politik umfassen drei Operationen:
- Feststellungen über politische Tatsachen, über das, was ist.
- Feststellungen über kausale Beziehungen, verbunden mit Prognosen über das, was wahrscheinlich in Zukunft sein wird.
- Schlüsse über wünschenswerte Entwicklungen und Reflexionen über das, was sein soll (von Beyme 2007 [1991]: 11).

b. Explanative operation, explanations of political reality

However, political reality also requires causal explanations. There are explanations, for example, for demographic developments, but also for why social policy has developed in one way and not another.

c. Prognostic operation: predictions about future political developments

In addition, there is the need to make forecasts about future developments. A look into the future makes sense in order to provide decision-makers with important information in the present.

C. Practical operations of political science

Practical discourses consist of practical operations about what ought to be. This is where norms or regulations (maxims of action, strategies for action, instruments of action, instructions for action and practical judgments) are justified. Within practical discourses, three different discourses or operations can also be distinguished. It is important that *norms and rules are not truth-apt*, but have other properties (section 5.3. chart 10, section 9.4.10).

a. Normative operation

In the normative value discourse, the political maxims for action that are decisive for the standardization or regulation of the political system as a whole or of a political field should be justified. The normative level or dimension of politics is primarily intended to develop the maxims for action with the help of which one can judge whether a society is *just* or *unjust*. The most important guiding principle for social security is the welfare state postulate.

b. Pragmatic operation

Within the pragmatic objective discourse of the pragmatic or strategic dimension of politics, the political strategies of action that are decisive for the regulation of a political field are justified. Pragmatic rules are *wise* or *unwise*.

c. Technical operation

Political instruments for action and individual instructions for action should be justified at the technical or operational level or dimension of politics. Value, goal and means discourse are independent of each other. Technical rules are *effective* or *ineffective* (section 5.3).

5.2.6 The perestroikans' criticism of the scientistic knowledge conception

The lack of philosophical discussions within political science methodology criticized by Mark Bevir (2010 [2008]: 48-49) applies above all to the field of epistemology. In the handbook "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]), no separate contribution deals with the topic, and there are only sporadic comments on it. From this it can be concluded that some modern distinctions introduced in the 20th century, such as those by Ryle (1969 [1949]) and by Polanyi (1958,1985 [1967]), have been adopted *implicitly*.

In what follows, I will first present (section A) the Aristotelian distinctions between episteme, techne and phronesis as interpreted by Bent Flyvbjerg (2001, 2006), and point to Aristotle's far more comprehensive conception of knowledge. Furthermore, I will explain why Flyvbjerg's critique of the scientistic conception of knowledge completely misses the methodology of scientistic scientists at the beginning of the 21st century. Then I will show that assuming different assumptions simply leads to talking past each other (section B).

A. The three types of knowledge according to Flyvbjerg and their importance for research: episteme, techne and phronesis

Flyvbjerg attaches great importance to distinguishing between different forms of knowledge. He requires all political scientists to provide information about the form of knowledge they are developing. Therefore, these forms of knowledge, which go back to Aristotle, are presented here as Flyvbjerg interprets them.

a. Episteme

According to Flyvbjerg, scientistic scientists represent the following conception of knowledge:

> *Episteme*: Scientific knowledge. Universal, invariable, context independent. Based on general analytical rationality. The original concept is known today by the terms 'epistemology' and 'epistemic'. Political science practised as *episteme* is concerned with uncovering universal truths or laws about politics (Flyvbjerg 2006: 71).

The universal, invariable, and context-independent properties that Flyvbjerg attests to knowledge oriented towards natural science, and all epistemic sciences oriented towards this knowledge, are actually hardly proposed by anyone. The opposite is advocated by the scientistic scientists; the *if-then structure of knowledge* as well as the *hypothetical character* (section 5.4.7) of the same contradict the properties that Flyvbjerg cited as central.

No scientistic scientist demands freedom from context. The if-then structure of knowledge, its hypothetical character, is always implicitly considered. With any knowledge, the *ceteris paribus clause* must be considered. In other words, the formulated knowledge is only valid in a very specific context. This context should always be expressed openly in the *if* part or can also be reconstructed if the *if* part has not been formulated in detail.

The scientistic scientists look for causal regularities on the macro level or for causal processes on the micro level that can be generalized. The word “law” or the words “laws about politics” are no longer used in this context, but rather the words “regularities” or “legalities”, very rarely “probabilistic laws”. Therefore, there is nothing to prevent political science from only looking for *causal regularities* at the macro level, but also for *causal processes* or mechanisms at the micro level, just as is done in the natural sciences.

Neither natural scientists nor scientistic social scientists are looking for *universal truths*; only gurus and priests seek those. However, the ideal of truth has not been abandoned by the scientistic scientists. They represent the concept of an approximation to the truth. This means that science does not seek universal truths and that the truths found can still be changed in the process of finding the truth. Hypotheses are set up and are of course also rejected in the process of knowledge, just as interpretations are constantly changed by the interpretivists. I cannot see any fundamental differences in whether hypotheses or interpretations are set up and rejected in the scientific knowledge process.

b. Techne

In addition to the episteme, the scientistic scientists would also represent another form of knowledge, namely the techne:

> *Techne*: Craft/art. Pragmatic, variable, context dependent. Oriented toward production. Based on practical instrumental rationality governed by a conscious goal. The original concept appears today in terms such as ‘technique’, ‘technical’, and ‘technology’. Political science practised as *techne* is consulting aimed at better politics by means of instrumental rationality – a type of social engineering – where ‘better’ is defined in terms of the values and goals of those who employ the consultants, sometimes in negotiation with the latter (Flyvbjerg 2006: 71).

According to Flyvbjerg, such socio-technological investigations are carried out by scientistic scientists. As shown above, in order to pursue an applied political science that establishes social technologies, a political science that is oriented towards the natural sciences and searches for causalities is required. The limits and possibilities of this concept have been discussed in this chapter.

c. Phronesis

The two forms of scientific knowledge, episteme and techne, are sorted out by the phronetic perestroikans because it is said that phronesis is the only thing that matters. While social technology consists of rules or applied causalities, phronesis is viewed as a practical skill, a skill, or a set of skills:

> What is applied is not theory, but a philosophy of engagement that recognizes that phronesis is a skill and that having phronesis is iteratively dependent on practising phronesis (Flyvbjerg /Landman/Schram 2012c: 286).

Phronesis enables deliberation about values for practice. Phronesis is pragmatic and variable as well as contextual. It is very important for the perestroikans that phronesis be based on value rationality and that it guides action. Phronesis has no equivalent today. The perestroikans advocate an applied phronesis that enables a deliberation about interests, power and values and can be used in a problem-oriented manner:

> *Phronesis*: Ethics. Deliberation about values with reference to praxis. Pragmatic, variable, context dependent. Oriented toward action. Based on practical value rationality. The original concept has no analogous contemporary term. Political science practiced as *phronesis* is concerned with deliberation about (including questioning of) values and interests aimed at praxis (Flyvbjerg 2006: 71).

B. Critique of Flyvbjerg's conception of knowledge

However, it has to be said that this reference to Aristotle is a rather idiosyncratic interpretation by Flyvbjerg. In epistemology, the phronetic perestroikans represents a virtually pre-modern, or rather sophistical, perspective that does not even take into account a wealth of differentiations in Aristotelian work.

In the sixth book of the Nicomachean Ethics (Aristotle 1983 [4th century BC]: 152-176 [1138b18-1139a1] five types of knowledge are treated: knowledge of science (*episteme*), knowledge of skill (*techne*), knowledge of the agent, practical reasonableness or practical prudence (*phronesis*), knowledge of philosophical wisdom (*sophia*), and rational knowledge (*nous*).

Another form of knowledge can be found in metaphysics (Aristotle 1970 [4th century BC]: I, 1, 980a21–982a2), namely the knowledge of experience (*empeiria*) (Rese 2011: 126-127, Höffe 2006 [1996]): 42-46). It remains a mystery why Flyvbjerg does not go into all Aristotelian forms of knowledge or why he does not continue to use the possibilities created there, something of empirical knowledge (*empeiria*), for his methodology. There are also other *forms of rationality* that ground a broader understanding of science like that in the explanative-prognostic or the Platonic-Galilean tradition:

> Syllogistics, Aristotelian dialectics, rhetoric and poetics seem at first glance to be merely disparate subjects. Despite profound differences, they have one thing in common; they are ways in which human knowledge presents itself: forms of rationality[89] (Höffe 2006: 50; my translation).

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[89] Syllogistik, Aristotelische Dialektik, Rhetorik und Poetik scheinen auf den ersten Blick bloß disparate Themen zu sein. Trotz tiefgreifender Unterschiede haben sie aber eine Gemeinsamkeit; es sind Weisen, in denen sich menschliches Wissen darstellt: Formen von Rationalität. Höffe 2006: 50).

In the Aristotelian literature it is further pointed out that

> Aristotle distinguishes between practical philosophy as a philosophical reflection of practice and practical reasonableness (phronesis) as a reason guiding practice[90] (Rese 2011: 114; my translation).

Unfortunately, this very fruitful differentiation between practical knowledge and capability is not further developed by the perestroikans either.

The situation is similar with the principle of object-appropriate accuracy:

> [I]n ethics he [Aristotle] advocates a principle of objective accuracy that enhances the practical disciplines[91] (Höffe 2006 [1996]: 44; my translation).

Interpretivists could also use this distinction when it comes to different criteria of quantitative or qualitative research.

> In his [Aristotle's] treatises something comes to light that is not so common among philosophers, especially today, an *esprit de finesse* that knows about the plurality of epistemic possibilities and is able to apply them confidently[92] (Höffe 2006 [1996]: 41; my translation).

Flyvbjerg shows that there is also a lack of *esprit de finesse* among (social) scientists, since he thinks he can reduce the diverse forms of knowledge to one, phronesis. This reductionism is not only wrong because it does not adequately reflect the Aristotelian conception of knowledge; it also deprives the perestroikans of the possibility of developing a viable alternative to scientistic forms of knowledge. Indeed, the Aristotelian conception of knowledge offers a very powerful and comprehensive alternative to the narrow conception of knowledge within the explanative-prognostic or the Platonic-Galilean tradition.

Modern conceptions of knowledge are largely based on the explanative-prognostic or the Platonic-Galilean tradition. The aim is always to distinguish knowledge based on or legitimated by scientific (rational) authority from other forms of knowledge such as opinions or pseudo-knowledge. According to the scientistic scientists, only knowledge legitimated by science should also be accepted by society as acceptable knowledge. Everything else is discredited by them as pseudo-knowledge, just as any practice that does not share the axiological, epistemic, methodological, and ontological assumptions of the scientistic scientist is branded as pseudoscience. This idea was very successful

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[90] daß Aristoteles zwischen der praktischen Philosophie als einer philosophischen Reflexion der Praxis und der praktischen Vernünftigkeit (*phronesis*) als einer die Praxis leitende Vernunft unterscheidet (Rese 2011: 114).

[91] [I]n der Ethik vertritt er ein Prinzip der gegenstandsgerechten Genauigkeit, das die praktischen Disziplinen aufwertet (Höffe 2006 [1996]: 44).

[92] In seinen [Aristoteles'] Abhandlungen tritt etwas zutage, das unter Philosophen, zumal heute, nicht so häufig anzutreffen ist, ein *esprit de finesse*, der um die Pluralität der epistemischen Möglichkeiten weiß und sie souverän anzuwenden vermag (Höffe 2006 [1996]: 41).

and has prevailed, as evidenced above all by the scientification of all areas of life since the 20th century. There is hardly any political regulation that dispenses with scientific justification and thus refers to the current state of science.

While a narrow concept of knowledge comes into play here, the perestroikans and the other interpretivists have a much broader concept of knowledge:

> We define 'knowledge' as the certainty that phenomena are real and have identifiable properties[93] (Berger/Luckmann 2009 [1966]: 1; my translation).

This constructivist concept of knowledge has found widespread acceptance among interpretivists and goes far beyond the scientistic concept of knowledge:

> It [the concept of knowledge] includes not only the institutions and symbolic concepts, but also all forms of stored experience, for example also the body-bound knowledge about how to do this or that. Berger and Luckmann do not exclude values and norms (i.e. rules of conduct and legitimation), nor do they exclude feelings: knowledge about modern love relationships also includes recognizing and feeling feelings in these relationships[94] (Keller 2012: 229; my translation).

With his concept of knowledge, Aristotle offers an alternative to the explanative-prognostic or the Platonic-Galilean tradition insofar as he also considers other forms of knowledge (Höffe 2006 [1996]: 44). Flyvbjerg does not succeed in connecting with this differentiated conception of knowledge, be it with the interpretivists or Aristotle, because he only refers to phronesis and sets himself apart from episteme. In doing so, he further developed the Aristotelian conception in a rather simple form by integrating power analyses with reference to Michel Foucault (1971 [1966] and 1995 [1969]).

Furthermore, talking past each other is inevitable, above all because he attests to the scientistic scientists' epistemic striving for knowledge that is far removed from the scientistic scientists' ideas of knowledge at the beginning of the 21st century, or not held by any scientistic scientist (Kincaid 2023: 913).

5.3 Ideals and properties of scientific research

Misunderstandings within scientific-theoretical debates are also based on the differences that exist at the level of the ideals and properties of scientific research. At this level, the aim is to identify the predicates of scientific propositions (statements, norms or rules) or the properties with which scientific propositions can be evaluated.

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[93] ‚Wissen' definieren wir als die Gewißheit, daß Phänomene wirklich sind und bestimmbare Eigenschaften haben (Berger/Luckmann 2009 [1966]: 1).

[94] Er [der Wissensbegriff] bezieht nicht nur die Institutionen und Symbolbegriffe ein, sondern auch alle Formen einer gespeicherten Erfahrung, also bspw. auch das körpergebundene Wissen darüber, wie dieses oder jenes zu tun ist. Berger und Luckmann nehmen damit auch Werte und Normen nicht aus (also Verhaltensmaßregeln und Legitimationen), ebenso wenig wie Gefühle: Zum Wissen über moderne Liebesbeziehungen gehört auch das Erkennen und Empfinden von Gefühlen in diesen Beziehungen (Keller 2012: 229).

In the end, it comes down to the question of which ideals should at least be strived for within the scientific system.

The following *questions* are in the front here: Which ideals are striven for in science? What properties should scientific propositions have?

Since Plato, the line of demarcation between rational knowledge and non-rational knowledge has been justified by the fact that, firstly, knowledge requires justification, and secondly, that knowledge must satisfy criteria of truth, and that science ought to seek truth.

Despite all the difficulties, scientistic scientists strive for truth, or at least accept the *ideal of truth* as a regulative idea, i.e. they accept that all scientific propositions are *truth-apt* (either true or false). The perestroikans, as I will show, wrongly assume that scientistic scientists are looking for universal truth and, following postmodernism, take an antiveritative, skeptical position. But these are not the only possible positions.

In the following, I would like not only to present these two positions (5.3.1 and 5.3.2), but also to present arguments that support the scientistic position but reject the reductionism associated with it. I will show that there are also other ideals (correctness, justice, prudence and effectiveness) in practical discourses and that it is therefore not possible to reduce practical discourses to empirical discourses. Furthermore, I will list reasons why one needs *truth-analogous predicates* for norms and rules, or why norms and rules cannot be reduced to normative statements (section 5.3.3, and 6.7).

5.3.1 Ideal of truth in the sciences: veritative and anti-veritative positions

The *ideal of truth* has shaped science since antiquity, but there have always been *skeptical* and *antiveritative* tendencies that fundamentally question the search for truth or the possibility of finding it.

Scientistic scientists within political science are still committed to the ideal of truth. Accordingly, science justifies only truth-apt statements, meaning that statements are either true or false. As far as the properties of knowledge are concerned, some also take a reductionist approach that works with equivalences. *Norms* and *rules* are deemed equivalent to *empirical statements*, i.e. even norms and rules are reduced to so-called "*normative statements*", so that the true/false *bivalence* can be applied.

Logical and linguistic analyses and justifications in this regard are usually simply assumed to be justified. While the scientistic scientists within political science simply assume this equivalent, one finds a justification for this in Bacon's work:

> The two pronouncements, the active and the contemplative, are one and the same; and what is most useful in operating is truest in knowing[95] (Bacon 2000 [1620]: 104).

In this way, in the explanative-prognostic or the Platonic-Galilean tradition, an equivalence between efficiency (effectiveness) and truth is formulated. This equivalence is also formulated in pragmatism. Surprisingly, Schram falls back on Jacqueline Stevens' distinction between "*science as use*" and "*science as truth*" (quoted in Schram 2003: 850) and not on the pragmatic theory of truth, although he generally refers to American pragmatism. Another reference for the phronetics is Machiavelli:

> *Real* social science is when studying the world has the effect of changing it, by means of what Machiavelli calls *verita effectuale* (effective truth) (Flyvbjerg/Landman/Schram 2012a: 4).

A similar theory of truth is advocated in American pragmatism. William James (1907), in his "Lecture VI. — Pragmatism's Conception of Truth", defends the Schiller-Dewey conception of truth:

> Schiller says the true is that which "works" [...]
> Dewey says truth is what gives "satisfaction" (James 1907).

In other words, anything that is *functionally* useful is also *true*. Here, as in Bacon, there is an *equivalence* between truth and efficiency (efficacy), causality and action (Bacon 1990 [1620]). It should be emphasized that just like Bacon, James did not have an elaborate theory of truth.

In contrast, *anti-veritative* or *skeptical positions* have been widespread since antiquity and are still held today:

> Admittedly, there are also anti-veritative tendencies in the tradition, which not only deny the fact and the recognizability of truth, but also deny its relevance for the orientation of human life and put concepts such as practice, existence, will or interest in the place of truth and declare these to be the foundations of human life. If ancient skepticism and similarly early skepticism only doubted whether truth could be known, since Nietzsche and the postmodernism based on him the very meaning of truth and the possibility of orientation towards truth are disputed due to a radical relativism and perspectivism. Nietzsche made the claim: "Truth is the kind of error without which a certain kind of living being could not live"[96] (Gloy 2004: 3; my translation).

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[95] Ista autem duo pronuntiata, activum et contemplativum, res eadem sunt; et quod in Operando utilissimum, id in Sciendo verissimum (Bacon 1990 [1620]: 286, 4. aphorism, volume 2).

[96] Freilich gibt es in der Tradition auch *antiveritative Tendenzen*, die nicht nur das Faktum und die Erkennbarkeit von Wahrheit leugnen, sondern auch deren Relevanz für die Orientierung des menschlichen Lebens bestreiten und an die Stelle von Wahrheit Begriffe wie Praxis, Existenz, Wille oder Interesse setzen und diese zu Grundlagen des menschlichen Lebens erklären. Bezweifelte die antike Skepsis und ähnlich die frühzeitliche nur, ob Wahrheit erkannt werden könne, so wird seit Nietzsche und der auf ihm basierenden Postmoderne aufgrund eines radikalen Relativismus und Perspektivismus schon der Sinn von Wahrheit und die Möglichkeit der Ausrichtung auf Wahrheit bestritten. Nietzsche hat den Anspruch

With their critique of the ideal of truth, the phronetic perestroikans partly follow these anti-veritative and skeptical traditions, particularly those of postmodernism. On the other hand, they are committed to American pragmatism.

The search for the one truth is certainly a premodern way of thinking.

> "What is truth?" Pilate asked mockingly, not wanting to stay to hear the answer. Pilate was ahead of his time. For "truth" itself is an abstract noun, a camel of logical construction which cannot even pass through the eye of a grammarian[97] (Skirbekk 1977: cover text; my translation).

The scientistic scientists tend to advocate the concept of truth as a regulative idea, developed primarily by Popper.

5.3.2 Perestroikans criticism of the ideal of truth

Sanford F. Schram (2003 and 2005), based on Richard Rorty (1981), also takes an anti-veritative attitude. It is important to emphasize that he primarily criticizes the image or correspondence theory of truth. Moses and Knutsen also claim that naturalism represents a correspondence theory of truth (Moses/Knutsen 2019 [2007]: 8, 41). But today the correspondence theory is not represented by many philosophers or scientists. Even logical positivism witnessed a movement away from *correspondence* theory towards a *coherence* theory of truth:

> The truth theory of logical positivism developed step by step from a correspondence theory to a coherence theory[98] (Hempel 1977 [1934]: 96; my translation).

Carl Gustav Hempel recorded this in an article as early as 1934. Much later, in his Philosophical Investigations (Wittgenstein 1984c [1953]), Wittgenstein distanced himself from the correspondence theory of truth represented in the Tractate (Wittgenstein 1984b [1922]).

There can be no *independent observer* who compares the world on the one hand with its linguistic description on the other hand, since world cognition only works via language exploration or, in other words, every observation is theory-laden.

Today the focus, if at all, is not on the search for *"the" truth*, but only on the much more modest question of whether knowledge is *coherent* or *consistent*, which is why the majority of scientists advocate the *coherence theory of truth* (Rescher 1977 [1973]:

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gefällt: ‚*Wahrheit ist die Art von Irrthum,* ohne welche eine bestimmte Art von lebendigen Wesen nicht leben könnte' (Gloy 2004: 3).

[97] ‚Was ist Wahrheit?', fragte Pilatus spöttisch und wollte nicht bleiben, um die Antwort zu hören. Pilatus war seiner Zeit voraus. Denn ‚Wahrheit' selbst ist ein abstraktes Substantiv, also ein Kamel von einer logischen Konstruktion, das nicht einmal durch das Öhr eines Grammatikers hindurchgehen kann (Skirbekk 1977: Klappentext).

[98] Die Wahrheitstheorie des logischen Positivismus entwickelte sich Schritt für Schritt von einer Korrespondenztheorie zu einer Kohärenztheorie (Hempel1977 [1934]: 96).

337-390, Gloy 2004, Young 2008). This applies not only to quantitative but also to qualitative-interpretative researchers. Both groups strive to establish coherent models or theories in their work. But representatives of critical theory, to which Schram (2003 and 2005) also refers approvingly, now also see it the same way:

> The truth of a statement seems to be vouched for only by its coherence with other statements[99] (Habermas 2009a [1999]: 400; my translation).

This statement shelved not only the image or correspondence theory of truth, but also the consensus theory of truth, of which Habermas himself was the most important representative.

In my opinion, the perestroikans' criticism of the naturalists in this respect is mainly based on misunderstandings and is an expression of a lack of engagement with theories of truth (Skirbekk 1977, Gloy 2004 or Young 2008) as well as with the hypothetical deep structure of scientific knowledge (section 5.4.7).

Even if nothing is said about the properties of normative propositions in the 10th volume of "Political Methodology" (Box-Steffensmeier/Brady/Collier 2010a [2008]), it is nevertheless obvious that it also makes use of the ideal of truth.

According to the scientistic scientists, not only empirical statements, but also "normative statements" (Ladwig 2006) are truth-apt. Well-founded criticism that norms are also truth-apt has existed within logic for decades, and recently these views have also been criticized by philosophers of technology (section 6.7). These discuss why norms and rules are not truth-apt, but also why they have other properties: *right/wrong* (ethical-moral norms), *just/unjust* (political norms, normative action maxims), *wise/unwise* or desirable/undesirable (pragmatic rules, strategies for action) or *efficient/inefficient* (technical rules/instruments for action or instructions for action).

The following is just an overview of the properties of statements, standards and rules.

5.3.3 Properties of propositions (statements, norms or rules): veritative predicates as well as truth-analogous alternatives

To give up the ideal of truth just because one cannot determine "the" or a universal truth would therefore be throwing the baby out with the bathwater. The scientistic scientists are right to hold on to truth as a regulative idea, because otherwise a rational discussion is not possible. The principle of non-contradiction (PNC) must precede all judging and evaluating (section 5.4.1), but this requires that one can and must make bivalent distinctions.

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[99] Die Wahrheit einer Aussage scheint nur noch von ihrer Kohärenz mit anderen Aussagen verbürgt werden zu können (Habermas 2009a [1999]: 400).

Truth is *not* the only *regulative idea* of scientific discourses. Truth predicates can be assigned to empirical (descriptive-interpretative, explanatory and prognostic) statements, more precisely to propositions. Practical propositions (norms or rules) have other properties. The predicates that follow can be used for *scientific* propositions.

A. Ideal of truth: truth or false

Truth is the regulative idea within analytical and empirical (descriptive-interpretative, explanatory and prognostic) discourses. In these discourses, true or false statements can be made about the world or political reality, e.g., descriptions of political phenomena, political communication and meanings of political terms, and causal explanations or causal forecasts of political events. This also includes statements *about* norms, rules, standardizations, or regulations. Only statements are truth-apt, whereas norms or rules are not truth-apt (section 6.7).

B. Ideal of rightness (ethics): right or wrong; ideal of justice (politics): just or unjust

Ethical rightness and political justice are regulative ideas of normative value discourses. Ethical-moral norms and standardizations enable an evaluation of actions and social facts with the predicate *right* or *wrong* or *good* or *wrong*. Political action maxims, political actions and social facts can be evaluated with the predicate *just* or *unjust*. While ethical-moral norms are only indirectly referred to in the regulations of a political system, political action maxims form the core of a constitution.

C. Ideal of phronesis: wise or unwise

Phronesis and desirability are regulative ideas of pragmatic objective discourses; there are *wise* or *unwise* pragmatic rules (strategies of action) or rules that are good for someone (on the evaluative levels *good par excellence* (*gut schlechthin*), *good for someone* (*gut für jemanden*), and *good for something* (*gut für etwas*); Höffe 2009 [2007]: 22-28).

D. Ideal of effectiveness (efficacy): effective or ineffective

Technical effectiveness (efficacy) is the regulative idea of technical means discourses. Rules and regulations (action strategies, action instruments or action instructions) are *effective* or *ineffective* or are good for something (Kornwachs 2008 and 2012).

5.4 Limits of (political) scientific research and generation of knowledge

The boundaries between knowledge and other forms of cognition are defined by the philosophical foundations, in particular the scientific methodology; for the scientist, everything else is other knowledge (more in detail in section 5.2). Since ancient times, we have witnessed a tremendous expansion in this regard as a variety of methodologies have evolved and new innovations have emerged. The greatest advances came in the 20th century. In addition to the expansion, however, there is often a limitation of the scientific possibilities. Above all, the fundamental limits of science shall be dealt with here.

Scientific results can be refuted not only by means of empiricism (experience), but also by means of rationality (reason). Furthermore, *reason* determines the limits of scientific knowledge by excluding contradictions (5.3.1 *principle of non-contradiction*). Then the limits of axiomatic systems (5.3.2 *incompleteness theorem*) as well as *the limits of empirical confirmation or empirical refutation* are shown (5.3.3). In addition, the ambiguity of methodological criteria, weightings and specifications must be taken into account. There is the *prima facie property of norms* (5.3.4), the *aporia of practical reason* (5.3.5) and the limit between *is* (factuality) and *ought* (normativity; 5.3.6). Both empirical knowledge and practical knowledge have a *hypothetical character*, i.e. rational knowledge has an *if-then deep structure* (5.3.7). Furthermore, the impossibility of a *private language* and its consequences are discussed (5.3.8).

The perestroikans accuse the scientistic scientists of seeking universal truths as well as context-free knowledge:

> Scientific knowledge. Universal, invariable, context independent (Flyvbjerg 2006: 71).

Due to the basic limits of (pure and practical) reason briefly listed here, both the search for universal truths and the search for absolute, context-free knowledge *a priori* are doomed to failure because, for example, rational knowledge has a hypothetical character and an if-then structure.

The limits of scientistic methodology, and thus also of political science methodology, are among the seldom discussed questions of philosophy of science within political science. This also applies to the methodological manuals of the scientistic scientists analyzed in more detail here. Talking at cross purposes and misinterpretations are thus provoked. Nevertheless, there is no doubt that most of these boundaries are shared by the vast majority of scientistic scientists. These boundaries must therefore be thought of as *implicit assumptions* if one wants to assess scientistic methodology.

5.4.1 Principle of non-contradiction (PNC)

The *principle of non-contradiction* (PNC) has been at the forefront of all knowledge since antiquity, e.g. in Plato (Plato 1983c [4th century BC] Politeia: [434c-437a]), Aristotle (Aristotle 1970 [4th century BC] [Metaphysics: 1005b 11-34]) or Kant (Kant 1956 [1781 and 1787]: 207-209 [A 150/B 189-A 153/B 193]. Brandt reprinted and commented the crucial passages:

> Anyone who judges must either affirm or deny (which animals cannot), and he thereby submits to the PNC, because this sentence makes affirmation and negation distinguishable from one another. The PNC is not a beginning in such a way that everything that follows

> could be derived from it, it only has to precede all judgments and judging[100] (Brandt 2001: 24-25, PNC: Principle of non-contradiction; my translation).

Different predicates come into question for *bivalence*: true/false, just/unjust, wise/unwise and efficient/inefficient (section 5.3).

5.4.2 Incompleteness theorem

Within an axiomatic system there are inevitably propositions that can neither be proved nor disproved. Gödel's conclusion was that every axiomatic method has its limitations and is therefore essentially incomplete; even a complete axiomatization of complex models or theories is impossible (Gödel 1931 and Gödel 2003).

5.4.3 Methodological incommensurability (Kuhn underdetermination)

The *methodological* must be distinguished from the *general incommensurability* (Kuhn 1976 [1962], Feyerabend 1986 [1975]). In the case of *scientific revolutions*, the general incommensurability causes a *paradigm shift* and, due to *unbridgeable discontinuities*, leads to a collapse of scientific communication:

> To put it very simply, incommensurability [meaning general incommensurability] has been used since Kuhn and Feyerabend at the latest to characterize the relationship between two relativizing reference systems, when they compete in a conflicting manner and when none of them can be privileged. Incommensurability is the term that negates an absolute claim by simply treating conflicting claims as incomparable[101] (Hönig 2006: 15; my translation).

This general thesis is disputed by many scientists. The *methodological incommensurability* is usually recognized and says that there can sometimes be no clear judgments about different hypotheses and theories:

> What is meant by this [methodological incommensurability] is that the assessment of the performance of alternative hypotheses and theories requires weightings and clarifications because of the plurality and ambiguity of methodological criteria, about which a justified consensus can hardly be achieved. Therefore, no unambiguous judgment about hypotheses and theories is reached even when non-empirical, epistemic performance characteristics are added[102] (Carrier 2006: 105; my translation; see Kuhn 1977, especially the 13th chapter).

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[100] Wer urteilt, muß entweder bejahen oder verneinen (was Tiere nicht können), und er unterwirft sich damit dem SvW, denn durch diesen Satz werden Bejahung und Verneinung voneinander unterscheidbar. Der SvW ist kein Anfang in der Weise, daß alles folgende aus ihm abgeleitet werden könnte, er muß nur allem Urteilen und Beurteilen vorangehen (Brandt 2001: 24-25; SvW: Satz vom ausgeschlossenen Widerspruch).

[101] Grob vereinfacht gesprochen wird mit *Inkommensurabilität* [gemeint ist hier die allgemeine Inkommensurabilität] spätestens seit Kuhn und Feyerabend das Verhältnis zwischen zwei relativierenden Bezugssystemen charakterisiert, wenn sie konflikthaft konkurrieren und wenn trotzdem keines von beiden privilegiert werden kann. Inkommensurabilität ist der Begriff, der einen absoluten Geltungsanspruch negiert, indem er konfligierende Ansprüche schlicht als unvergleichbar behandelt (Hönig 2006: 15).

[102] Gemeint ist damit [mit der methodologischen Inkommensurabilität], dass die Beurteilung der Leistungsfähigkeit alternativer Hypothesen und Theorien wegen der Mehrzahl und

Philip Kitcher makes a different division, distinguishing three different incommensurabilities: conceptual, observational, and methodological incommensurability (Kitcher 2016: 636).

5.4.4 Prima facie property of norms

The prima facie property of norms (Ross 1967 [1930]) and political maxims for action means that instructions for action cannot be directly inferred from norms or maxims for action, so there is no simple possibility of derivation or subsumption. Using the aporias of practical reason, it can be shown why derivations and subsumptions are not possible.

5.4.5 Aporias of practical reason

The modern world expands the scope of action of the individual, but this comes with the cost of increased complexity that in turn generates new problems:

> [Because] at the same time as the scope for action is expanded, individual action is increasingly being integrated into network systems in which the individual is only found as a functional link within diverse cooperation structures that are hardly manageable for him or her. That is why it is much more difficult today than in the past for individuals to be clear in their actions about what they are actually doing[103] (Wieland 1999a: 101; my translation).

Anonymization, juridification (legalization), and *probabilization* (probabilistic character of scientific knowledge) are the three major challenges in a complex world that these new structures of action within diverse institutions first generate and that Wolfgang Wieland has excellently elaborated on using the example of medicine (Wieland 1986: 56-132).

> Practical reason aims not only to judge the world of action correctly, but also to shape it according to its ideas. Therefore, practical reason is about establishing universal norms, but also about applying these norms to individual actions and situations. It also seeks to motivate the actor to behave in accordance with the norms applied in this way. Ultimately, it strives to create the order that man needs in order to be able to live reasonably with his fellows. Practical reason can approach these goals, but in doing so it learns that it gets caught up in aporias if, instead of being satisfied with approximations, it tries to force solutions on principle. Their power is not sufficient to cope with the tasks that arise in the

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Mehrdeutigkeit methodologischer Kriterien Gewichtungen und Präzisierungen verlangt, über die ein begründeter Konsens kaum zu erzielen ist. Deshalb wird auch dann kein eindeutiges Urteil über Hypothesen und Theorien erreicht, wenn nicht-empirische, epistemische Leistungsmerkmale hinzutreten (Carrier 2006: 105, siehe Kuhn 1977, insbesondere das 13. Kapitel: Objektivität, Werturteil und Theoriewahl, 421-445).

[103] [W]eil zugleich mit der Erweiterung des Handlungsspielraums das individuelle Handeln in rasch zunehmendem Maße in Verbundsysteme eingefügt wird, in denen sich der Einzelne nur noch als Funktionsglied innerhalb vielfältiger, für ihn selbst kaum mehr überschaubarer Kooperationsstrukturen vorfindet. Deshalb ist es heute für den Einzelnen ungleich schwerer als in früheren Zeiten, sich in seinem Handeln darüber klar zu werden, was er eigentlich tut (Wieland 1999a: 101).

> context of applications and motivations with the claim to finality; it is too weak for the institutions in which it finds itself and whose existence it must demand to enforce conditions which any rule by men over men must satisfy if it is to be just rule. They could only escape their aporias under utopian conditions (Wieland 1989: 46; my translation). But such conditions are counterfactual[104] (Wieland 1989: 47; my translation).

Wolfgang Wieland reconstructs the following aporias of practical reason:

A. *Application aporia* as the epitome of the difficulties arising from the application of general norms to concrete situations:

   > The concept *application aporia* is here intended to denote the epitome of the difficulties arising from the necessity of applying general norms to individual, concrete situations[105] (Wieland 1989: 13; my translation).

B. *Aporia of motivation*, which raises the fundamental question of why one should conform to a norm at all:

   > Those who want to know why they should apply them [norms] at all fall into the *motivational aporia[106]* (Wieland 1989: 25; my translation)
   >
   > This marks the way to the *motivation aporia*: it arises because questions about the motivation to be reasonable and about the reasonableness of motivation challenge each other without ever coming to rest[107] (Wieland 1989: 31; my translation).

C. The *institutional aporia* expresses the dependency on institutions:

   > It [the *institutional aporia*] arises because practical reason cannot avoid extending its normative claim to the world of institutions, and at the same time must experience how

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[104] Die praktische Vernunft ist darauf aus, die Welt des Handelns nicht nur zutreffend zu beurteilen, sondern auch nach ihrer Idee zu gestalten. Deshalb geht es ihr darum, allgemeingültige Normen zu begründen, aber auch darum, diese Normen auf individuelle Handlungen und Situationen anzuwenden. Sie will überdies den Handelnden zu einem Verhalten motivieren, das den so angewendeten Normen entspricht. Schließlich strebt sie danach, die Ordnungen zu gestalten, deren der Mensch bedarf, um mit seinesgleichen auf vernünftige Weise zusammenleben zu können. Sie kann sich diesen Zielen annähern, doch sie erfährt dabei, daß sie sich in Aporien verfängt, wenn sie, statt sich mit Annäherungen zu begnügen, prinzipielle Lösungen erzwingen will. Ihre Kraft reicht nicht aus, die Aufgaben, die sich im Umkreis von Applikationen und Motivationen stellen, mit dem Anspruch auf Endgültigkeit zu bewältigen; sie ist zu schwach, für die Institutionen, in denen sie sich vorfindet und deren Existenz sie fordern muß, Bedingungen durchzusetzen, denen jede Herrschaft von Menschen über Menschen genügen muß, wenn sie gerechte Herrschaft sein soll. Nur unter utopischen Bedingungen könnten sie ihren Aporien entgehen“ (Wieland 1989: 46). „Doch solche Bedingungen sind kontrafaktisch (Wieland 1989: 47).

[105] Der Name der Applikationsaporie soll hier den Inbegriff der Schwierigkeiten bezeichnen, die sich aus der Notwendigkeit ergeben, generelle Normen auf individuelle, konkrete Situationen anzuwenden (Wieland 1989: 13).

[106] In die Motivationsaporie gerät, wer wissen will, warum er sie [Normen] überhaupt anwenden soll (Wieland 1989: 25).

[107] Damit ist der Weg in die Motivationsaporie vorgezeichnet: Sie ergibt sich, weil sich Fragen nach der Motivation zur Vernünftigkeit und nach der Vernünftigkeit der Motivation gegenseitig fordern, ohne jemals zur Ruhe zu kommen (Wieland 1989: 31).

> much it depends on this world and how much it remains dependent on it to secure its own existence[108] (Wieland 1989: 36).

The *right of resistance* (rebellion or revolution) is an example of an antagonism between individual reason and an institution of the legal order:

> Traditionally, the concept of the right of resistance designates the point at which the always possible antagonism between individual reason and the institutions of the legal order, and thus the institutional aporia, becomes visible in an intensified form. From the idea of a practical reason, the necessity of a legal order can be justified as well as the duty of the individual to orient his actions to the demands of this practical reason. An aporia arises, however, when the individual can no longer reconcile the immediate demands of this reason with the claims made in the name of the institutional order. No one can take a position from which he could hope to defuse a conflict between the two instances[109] (Wieland 1989: 41-42; my translation).

5.4.6 Is-ought (factuality-normativity) boundary

The is-ought boundary is not an ontological limit. In my opinion, solid arguments were made for separating these areas at the logic level as well as at the argumentation level. Following Weber, this limit is usually recognized by empirical political scientists, but rejected by perestroikans and interpretivists. This discussion has been dealt with in detail above (section 4.1 and 6.7).

5.4.7 If-then deep structure or hypothetical character of scientific knowledge

The if-then structure of scientific knowledge reminds us that within science no *absolute* knowledge is formulated and justified, but only *hypothetical* and *relative if-then relations* can be justified.

Nevertheless, this does not lead to an *antiveritative* position, as the perestroikans, among others, also claim following postmodern positions (Rorty 1981 [1980]). Thus, *no relativity of the truth claim* is postulated; the relation between presupposition and

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[108] Sie [die Institutionsaporie] ergibt sich deswegen, weil die praktische Vernunft nicht umhin kann, ihren normativen Anspruch auch auf die Welt der Institutionen zu erstrecken, und gerade dabei zugleich erfahren muß, wie sehr sie von dieser Welt abhängig und wie sehr sie zur Sicherung ihrer eigenen Existenz auf sie angewiesen bleibt (Wieland 1989: 36).

[109] Traditionellerweise bezeichnet der Begriff des Widerstandsrechts den Punkt, an dem der stets mögliche Antagonismus zwischen der individuellen Vernunft und den Institutionen der Rechtsordnung und damit die Institutionsaporie in verschärfter Gestalt sichtbar wird. Aus der Idee einer praktischen Vernunft läßt sich die Notwendigkeit einer Rechtsordnung ebenso begründen wie die Pflicht des Individuums, sein Handeln an den Forderungen dieser praktischen Vernunft auszurichten. Eine Aporie ergibt sich jedoch, wenn das Individuum die unmittelbaren Forderungen dieser Vernunft mit den im Namen der institutionellen Ordnung erhobenen Ansprüchen nicht mehr in Übereinstimmung bringen kann. Niemand kann eine Position beziehen, von der aus er einen Konflikt zwischen beiden Instanzen zu entschärfen hoffen könnte (Wieland 1989: 41-42).

consequence contains an *absolute truth* claim, it is about the *knowledge of facts under presuppositions* in scientific analyses.

> If one analyzes such a statement with respect to its deep structure, then almost always a structure of the type of the hypothetical statement, i.e., an if-then statement, is revealed. With its help it cannot be asserted that something is the case per se, but always only that it is the case if certain conditions are given. [...]
> The hypothetical depth structure of the theoretical-scientific statement by no means shows, contrary to a widespread misunderstanding, any relativity of its truth claim. It is true that the claim to validity of every elementary statement is relativized, as it were, if it is linked with a hypothesis and is of interest only as a link of such links. But if one asserts the existence of a corresponding relation between presupposition and consequence, then at least with this assertion the claim is connected to be valid par excellence and without restrictions. Therefore, modern science does not simply deal with the knowledge of facts, but with the knowledge of facts under presuppositions[110] (Wieland 1986: 31; my translation).

Not only is the relativity of the truth claim wrongly listed by the perestroikans, as shown, but also the criticism of the alleged context freedom of the knowledge generated by the scientistic scientists with a logical-mathematical research methodology. Because of principled limitations of all scientific tools that are generally recognized by scientists, especially the *if-then structure* of scientific knowledge, the charge of *context-free knowledge* is misplaced. This deep structure entails that something like context-free knowledge, if only for methodological reasons, is actually not advocated at all by the scientistic scientists either, if we look closely.

Now, the if-then structure of scientific findings is not seldom misappropriated by the scientistic scientists themselves, who should actually know better. Results are often communicated as if they are spatially and temporally universally valid findings. These misrepresentations then begin with the following words: “Scientists have found that y is true”. Universal, context-free findings can indeed be formulated in this way. If one takes into account the context as well as the hypothetical character of rationally based

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[110] Analysiert man nämlich eine solche Aussage auf ihre Tiefenstruktur hin, so zeigt sich dabei fast immer ein Gebilde vom Typus der hypothetischen Aussage, also einer Wenn-Dann-Aussage. Mit ihrer Hilfe läßt sich nicht behaupten, irgend etwas sei schlechthin der Fall, sondern immer nur, es sei der Fall, wenn bestimmte Voraussetzungen gegeben sind. […] Die hypothetische Tiefenstruktur der theoretisch-wissenschaftlichen Aussage zeigt, entgegen einem verbreiteten Mißverständnis, durchaus keine Relativität ihres Wahrheitsanspruches an. Zwar wird der Geltungsanspruch jeder Elementaraussage gleichsam relativiert, wenn sie mit einer Hypothese verknüpft wird und nur noch als Glied derartiger Verknüpfungen von Interesse ist. Wenn man jedoch die Existenz einer entsprechenden Beziehung zwischen Voraussetzung und Folge behauptet, so ist wenigstens mit dieser Behauptung der Anspruch verbunden, schlechthin und ohne Einschränkungen zu gelten. Die neuzeitliche Wissenschaft hat es daher nicht einfach mit der Erkenntnis von Sachverhalten, sondern mit der Erkenntnis von Sachverhalten unter Voraussetzungen zu tun (Wieland 1986: 31).

knowledge the formulation must be as follows: "Scientists have found out that if x^1, x^2 ... x^n are true, then y is true".

Every scientist can subscribe to this formulation. In this context it is important to point out that this does *not imply any relativization* of the truth claim. This if-then relation is *absolutely valid* in a *possible* world or in a mathematical-logical model. Whether it is valid in the *real* world is another question. In any case, it can be argued that, although it does not formulate a universal truth, it does formulate an approximation to the truth of the real world. The if-then structure of knowledge, therefore, makes it possible to formulate hypothetical knowledge without renouncing the ideal of truth. A relativism of the truth objective or even an antiveritative position is not necessary – indeed this would do a disservice to science.

5.4.8 Subjective private language versus intersubjective, objective or public language

Some interpretivists and perestroikans deny that all facts can be formulated objectively or intersubjectively. They do, however, claim to represent subjective sensations or subjective experiences in a *subjective* way by means of a (private) language. Is this possible at all with our language as it has developed so far?

Ludwig Josef Johann Wittgenstein expresses precisely this doubt with his famous arguments against a *subjective private language*:

> Wittgenstein claims that a private language is not a language. It makes no sense to speak of a private language, since in the form of life he describes such a conception is obviously based on language confusions; in this case, the language confusions are based on the bad use of the word 'pain'[111] (Lauer 1987: 37).

Even when talking about subjective sensations – Wittgenstein exemplifies this mainly by the example of one's own pain – one uses an intersubjective tool, namely an intersubjective, objective as well as public language. In other words, since there is *no private language*, even when formulating subjective experiences and subjective views, we are dependent on rendering them with the help of a language that functions only intersubjectively, publicly.

In the following, Wittgenstein's most important objection to a private language will be briefly presented. According to Saul Aaron Kripke (1982, Stegmüller 1986a), these questions are dealt with in paragraphs 138-242 of the Philosophical Investigations (Wittgenstein 1984c [1953]):

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[111] Wittgenstein behauptet, daß eine private Sprache keine Sprache ist. Es macht keinen Sinn, von einer privaten Sprache zu sprechen, da in der Lebensform, die er beschreibt, solch eine Auffassung offensichtlich auf Sprachkonfusionen beruht; in diesem Fall beruhen die Sprachkonfusionen auf dem schlechten Gebrauch des Wortes ‚Schmerz' (Lauer 1987: 37).

> In my view, the real ‘private language argument’ is to be found in the section preceding § 243 (Kripke 1982: 3; see p. 113).

In these paragraphs, above all, the most important argument is treated. According to Wittgenstein, there is not only one method, but many methods,

> as it were different therapies (Wittgenstein 1984c [1953]: § 133).

Therefore, one finds other arguments against a private language, which are not discussed here and are independent of this argument (Lauer 1987: 49).

The paragraphs Kripke examines are about *rule sequences:*

> There can be no private rule-following, but only a public rule-following within a community. Language and knowledge always function only within a public communication process. In the associated language games or functional units, words or sentences receive their specific meaning determined by the form or way of life (*Lebensform*, Lauer 1987: 49).

The three key terms in Kripke’s interpretation are: Conformity, way of life (*Lebensform*,), and Criteria (Kripke 1982: 96 ff.):

> Without a community in which there is agreement in the use of rules, there can be no language and understanding. [...]
> The set of responses in which the members of the community agree, and the way in which these responses interact with extra-linguistic actions, is the way of life (*Lebensform*) of this community[112] (Lauer 1987: 48).

The criteria are important for verifying consistency. Thus, the functioning of language depends on *public practice.*

Regardless of whether this interpretation is correct (critically, Baker/Hacker 1984), anyone who thinks they can do without an intersubjective, public language and criteria must demonstrate how a subjective private language can function. Meanwhile, even qualitative researchers or interpretivists no longer reject intersubjective criteria, even if one then speaks of “intersubjective comprehensibility” instead of “intersubjective verifiability” (Steinke 2015 [2000]: 323 and 324; see also Schwartz-Shea 2014 [2006] and section 6.4).

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[112] Es kann kein privates Regelfolgen geben, sondern nur ein öffentliches Regelfolgen innerhalb einer Gemeinschaft. Sprache und Wissen funktionieren immer nur innerhalb eines öffentlichen Kommunikationsvorganges. In den zugehörigen Sprachspielen oder Funktions–einheiten erhalten Wörter oder Sätze ihre spezifische, durch die Lebensform bestimmte Bedeutung (Lauer 1987: 49).

6 Methodology and political science

- Which political science methodologies can guarantee *scientificity*?
- What are the *limits and possibilities* of these methodologies?
- Which political science *methodologies* are used?
- How can the science war (*Methodenstreit*) be overcome?
- Can *structural differences* between empirical-interpretative (descriptive-interpretative), empirical-scientistic (explanatory and prognostic) and practical (normative, pragmatic and technical) methodologies be demonstrated at different levels?
- Is there *incompatibility* or *complementarity* between empirical-interpretive (descriptive-interpretative), empirical-scientistic (explanatory and prognostic), and practical (normative, pragmatic, and technical) methodologies? Or, to put it another way, are interpretive, scientistic, and practical methodologies complementary or antithetical to each other?
- What does a *differentiation* in philosophy of science look like on ten vertical and three horizontal levels?

The methodological foundations are by far the most complex of all philosophical foundations. That is why I have differentiated between seven levels here. It is important to note that this is only about the philosophical foundations of the methodology and not about concrete methods or methodological approaches that political scientists want to use to explain or change the world. In this chapter, five levels, the concept, sentence, theory, logic and argumentation level, are to be examined.

Mathematics and *language* are the most important tools for scientists. Since the beginning of the 20th century, *analytic philosophy* has led to a focus on logic and language, allowing for an enormous expansion and development of the same. Within analytic philosophy, precise clarification of concepts, clarity of expression, rigorous argumentation, logical stringency and objectivity are paramount.

The work of Friedrich Ludwig Gottlob *Frege*, George Edward *Moore* and Bertrand Arthur William Russell laid the foundations that enabled an innovative expansion of logic, which had hardly been developed since Aristotle. For the practical tradition in particular, the work of Walter Ernst Otto *Dubislav*, Jørgen *Jørgensen*, and especially Georg Henrik *von Wright* are of particular importance. I will go into these in more detail because they establish a fundamental distinction between the two *empirical* traditions on the one hand and the *practical* tradition on the other.

The so-called *linguistic turn* (Rorty 1967a) enabled not only an enormous further development of logic, but also of *language analysis*, which is why one also speaks of philosophy of language. I justify three fundamentally different traditions here, referring in particular to the works of Ludwig Josef Johann *Wittgenstein*, Stephen Edelston *Toulmin* and John Rogers *Searle*.

The most important criticism of analytical philosophy is that an overemphasis on *formal* elements of philosophizing overshadows the *content*, and thus the *relevance* of philosophy is lost. The accusations of the interpretivists and perestroikans against the scientistic scientists are in a similar vein: they argue that political science loses its *relevance* when theory-oriented instead of problem-oriented approaches (Green/Shapiro 1999 [1994], Flyvbjerg 2001, Schram 2005) and bloodless scholasticism (Mead 2010) determine the analysis.

Relevance and stringency are not mutually exclusive (chapter 7). The literary style, which is still widespread outside of analytical philosophy, has also led to the many misunderstandings in the science war, which is why I share Weber's attitude:

> Personally, I believe that there is no means in the world that is 'pedantic' in place to avoid confusion[113] (Weber 1973d [1917]: 510 [472]; my translation).

In this chapter, the philosophical (axiological, epistemic, methodological and ontological) differences will be discussed in an ideal-typical way on ten vertical and three horizontal levels (2nd chart, section 9.4.2). First, the central relevance of methodology will be discussed (section 6.1). The search for one or a handful of criteria to justify knowledge is not sufficient, as I have shown. In my view, scientific knowledge is legitimated by a variety of general and specific criteria at ten methodological levels (section 6.2). This diversity of constitutive, epistemic values for science will then be discussed in detail on seven methodological levels (sections 6.3 to 6.10).

6.1 Relevance and instrumental character of methodology for science

The central relevance of scientific methodology is given firstly because the methodology, in addition to the theory of knowledge, justifies and legitimizes the difference between science and other forms of knowledge. Science is the place where scientific knowledge is generated. This knowledge is guaranteed and constituted by means of methodology, and thus science confers scientific authority to this knowledge.

In antiquity, methodology justified the *transition from myth to logos*, and even today it enables a distinction to be made between scientific knowledge and other forms of

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[113] Persönlich bin ich der Ansicht, dass kein Mittel der Welt ‚pedantisch‘ ist, um nicht zur Vermeidung von Konfusionen am Platze zu sein (Weber 1973d [1917]: 510 [472]).

knowledge that are not generated scientifically, i.e. not methodologically and not systematically.

Plato and Aristotle can be considered the two founding fathers of political science. This applies both to the substantive issues of the discipline and to the axiological, epistemic, methodological and ontological issues that are important in this thesis, i.e. more formal, scientific-theoretical issues. In this context, Aristotle enjoys a higher reputation among empirically-oriented political scientists. From a methodological point of view, Aristotle's Organon can be considered the first book on methodology. In it, but also in many other works (on the distribution of methodological discussions within Aristotle's work, see Höffe 2006 [1996]: 37 ff.), Aristotle systematically discussed all rational or scientific tools with the help of which the leap from myth to logos was accomplished in antiquity – impartially, objectively, and factually, i.e., free of excitement, agitation, and passion; in short, without anger and zeal (*sine ira et studio*). In particular, he explored the possibilities and limitations of scientific methodology or tools in the broader sense (axiological, epistemic and ontological presuppositions) and in the narrower methodological sense (categories, logic (syllogism), dialectics, rhetoric).

Methodology is both a *tool* and an *object* of science. As a tool, it serves to generate content in the form of knowledge. The methodology itself is also the object of scientific research, and in the process, it is critically evaluated and further developed.

The methodology simply delineates the boundaries of the enterprise of "science": the concepts of "knowledge" and "science" belong together. In other languages, except in Latin, where the word "*scientia*" means both science and knowledge, this is not as obvious as in German where "*Wissen*" means knowledge and "*Wissenschaft*" means science. In English for example, there are two separate words, "science" and "knowledge"; in French, "*science*" and "*savoir*" or "*connaissances*". Scientists generate knowledge within scientific institutions with the help of various scientific tools.

The central relevance of scientific methodology is given because methodology, in addition to the theory of knowledge, constitutes the difference between science and other (social, religious, ideological, etc.) forms of knowledge acquisition; methodology guarantees that scientists justify *knowledge* and not *opinions*. Furthermore, the methodology as defined here also guarantees the unity of science:

> What made all this progress possible, I submit, is not any loosening of the discipline of political science. Rather, that progress is attributable to the strength of the discipline's discipline [...].
> The discipline is a pluralist one, but the plurality is contained within and disciplined by a discipline (Goodin 2011b [2009]: 32).

Karl Pearson put it similarly in his book The Grammar of Science (1892: 16):

> The unity of all science consists alone in its methods, not in its material (quoted in King /Keohane /Verba 1994: 9).

Scientific methodology primarily establishes a *line of demarcation* between knowledge and other forms of knowledge, or between *science* and *non-science* (Hansson 2016, section 5.2). This line of demarcation is constantly changing, moreover, precisely because of methodological developments, and the boundaries of knowledge are ceaselessly being expanded or narrowed.

Secondly, the methodology also plays a special role in areas other than knowledge generation in each individual subject, for example in the training and socialization of specialist representatives, in employment in scientific institutes and later in the promotion of research, such as the allocation of research funds or when facilitating publications:

> What becomes clear in this context is the extent to which methods currently serve as identity markers for various fields, including as gatekeepers for doctoral students embarking on comprehensive exams and dissertation research, graduating PhDs seeking jobs, junior faculty seeking promotion and tenure, and all seeking research funding, opportunities to present work in conferences, and publications outlets for research. And the issues arise as well in teaching, curricular design, and textbooks contents (Yanow /Schwartz-Shea 2014a [2006]: 421-422).

Internally, the methodology leads to the emergence of schools and *professional boundaries*. To dismiss these debates as fetishism would be to obscure the real or alleged incommensurability between different scholarly schools and traditions, as well as the constructed, individual and professional identities.

Basically, two different meanings can be distinguished: the *endogenous* meaning of the methodology lies in distinguishing scientifically legitimate knowledge from other forms of knowledge. The *exogenous* importance of the methodology controls the socialization and cooptation of new members in the scientific system and further contributes to the promotion of research and the development of society.

This does not say anything about the relevance, for example, of lifeworld, religious or ideological forms of knowledge generation; as a rule, this should be associated neither with appreciation nor with devaluation. The pejorative devaluation as “pseudoscience” of findings that are not made according to the methodological specifications of a school, for example in Critical Rationalism, is widespread. Findings of scientists from other schools are also subsumed under pseudoscience. Furthermore, within methodological studies, it is not possible to discuss the *value* of science (chapter 4 Axiology), nor is it possible to examine arguments for or against a *scientification* of politics.

Scientists generate knowledge with the help of various scientific tools. Methodological constructivism points out the central importance of scientific *theoretical foundations* (tasks, conditions, criteria, ideals and properties of scientific discourses) as well as *scientific tools* (concepts, propositions, theories, logics, modes of argumentation, methods and methodological approaches) in the scientific process. Scientists are subjects of a *self-generated worldview*.

The importance of methodology is rated even more highly by some scientistic scientists, and is even overemphasized by equating science and methodology. For example, Gary King, Robert Owen Keohane, and Sidney Verba, who authored one of the most widely acclaimed methodology books in political science (in the judgment of leading representatives of the subject; Collier/Brady/Seawright 2010 [2004]: 3), equate science with methodology:

> **The content is the method** [emphasis in the original] [...].
> The content of "science" is primarily the methods and rules, not the subject matter, since we can use these methods to study virtually anything (King /Keohane/Verba 1994: 9).

The word "method" is usually used in a very broad sense within the explanative-prognostic or the Platonic-Galilean tradition; as shown above, the word "methodology" is more appropriate. A variety of methods does not guarantee methodological pluralism and should not be confused with it. The methodological reductionism among scientistic scientists also comes into play because only a small number of methods are recognized:

> [S]cientific research adheres to a set of rules of inference on which its validity depends (King/Keohane /Verba 1994: 9).

Scientistic scientists recognize only those methods that enable the empirical identification of invisible causalities, and furthermore only the class of logical-mathematical methods.

The juxtaposition between methodology and ontology or subject area is unfortunate; of course, any subject area can be treated scientifically. Crucially, however, the content of science includes not only methodology but also knowledge gained through scientific methodology on any subject matter. Therefore, methodology is one of two pillars of science, alongside (scientific, rational) knowledge:

- The first pillar of science contains the methodology or the tools of science. Methodology, its development and innovation, is therefore an important field of activity of any science.
- The second pillar of science consists of the knowledge that can be generated and thus legitimized by means of the first pillar.

The perestroikans accuse the scientistic scientists of being method-driven and aim to counteract this with a problem orientation. In my opinion, they underestimate the importance of methodology.

Training within the field of political science, as is shown not least by the science war (*Methodenstreit*) discussed here, proceeds not only on the basis of axiological, epistemic and ontological preferences, but also on the basis of methodological preferences. A systematic *reconstruction* and *self-reflection* of scientific methodology is therefore necessary in every discipline. Due to the complexity of methodological issues, they are discussed here on ten vertical and three horizontal levels (1st and 2nd chart, section 9.4.1 and 9.4.2).

This chapter mainly discusses the *instrumental* character of methodology for the generation and legitimation of knowledge (endogenous meaning). Its relevance for the socialization of researchers and the promotion of research (exogenous meaning) is addressed in chapter four.

6.2 Methodological differences of scientific methodology on ten vertical and three horizontal levels

The opponents in the science war and their concepts and positions were presented above, but I still think it is urgently necessary to analyze the differences in more detail and more precisely, though this may be viewed as pedantry. A justification for pedantry can be found in Weber:

> Personally, I am of the opinion that no means in the world is too “pedantic” so as not to be in place for the avoidance of confusion[114] (Weber 1973d [1917]: 510 [472]; my translation).

Pedantry is not only appropriate in these matters, but has become necessary in order to avoid talking past each other or having a counterproductive dispute. Only in this way can the many misunderstandings that have characterized the science war since its inception be overcome.

The chapters on epistemology and methodology mainly discuss the endogenous, epistemic values by means of which scientific knowledge is generated and legitimized. Two different criteria, *general* and *specific*, were presented. This distinction goes back to Aristotle and is also used by Toulmin (2003 [1958]: VIII). Harold Kincaid (1996) distinguishes between *abstract* and *concrete* or *realized* scientific virtues. Gerald Doppelt (2009: 200-203) speaks of *universal* and *local* epistemic values. In the previous chapter, mainly the general criteria were discussed; the special, concrete or local criteria, in my opinion, can be found primarily within methodology.

The generation and legitimation of knowledge is a very complex process. One universal criteria, such as those presented by Popper with his concept of fallibilism, are by no means sufficient. In my opinion, a plethora of general and specific criteria must be taken into account. The second diagram offers an overview on ten vertical and three horizontal levels (2nd chart, 9.4.2).

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[114] Persönlich bin ich der Ansicht, dass kein Mittel der Welt ‚pedantisch‘ ist, um nicht zur Vermeidung von Konfusionen am Platze zu sein (Weber 1973d [1917]: 510 [472]).

On the vertical level, an ideal-typical structure of the different criteria is presented. The horizontal structure takes into account the fact that there are three fundamentally different types of criteria on the same vertical level. These justify three fundamentally different scientific traditions that I have identified: descriptive-interpretative tradition, explanative-prognostic tradition and practical tradition.

6.3 Levels one to three: axiology, epistemology and ontology

The first three levels were treated in detail in the three previous chapters (ontology chapter 3, axiology chapter 4, and epistemology chapter 5). The first concerns the subject area, i.e. ontological questions, the second axiological questions above all the tasks of science, and the third epistemic questions. In the following, the methodological questions are dealt with on seven levels.

6.4 Level four: concepts and categories

Concepts and categories are important scientific tools, as are methods and methodological approaches. While they constitute the simplest scientific tools, their use can have very far-reaching consequences. Depending on how they are handled, various misunderstandings can arise. For example, conceptualizations can be treated as justified, unproblematic, unquestioned assumptions, often even as hidden assumptions, and so decisively influence scientific results; this will be briefly illustrated first (section 6.4.1). Second, by subsuming concepts under conceptualizations, classifications are made that do not stand up to closer scrutiny (section 6.4.2). Thirdly, the same concept can mean quite different things; this will be discussed further below when distinguishing two different “qualitative” methods on the methodological level (see section 6.9). This chapter also introduces concepts that I use in this thesis (section 6.4.3).

6.4.1 Civic conceptualization or liberal terminology: “Lockean liberalism’s universalism”

The conceptualizations of the explanative-prognostic or the Platonic-Galilean tradition were founded in the 17th century. Many of them are sometimes applied very uncritically or lacking a reflexive examination (Bevir 2010 [2008], Hay 2011 [2009]). Furthermore, they are also applied to non-Western political systems, so that Susanne Hoeber Rudolph speaks of an “imperialism of categories” (Rudolph 2005a):

> America’s hegemonic Lockean liberalism would shape the very concepts and methods we used to acquire knowledge about an unfamiliar society and its politics (Rudolph 2005a: 5).

Liberalism provides both positive and normative concepts that, in contrast to the assumptions of causalism, are not hidden (Hardin (2011 [2009]), section 6.10). But while they are open, they are rarely questioned. John Locke’s (Locke 1989 [1690]) political writings, but also his Essay Concerning Human Understanding (Locke 1975 [1690]), are still very influential in this regard:

> That burden was a Lockean universalism that taught that the self and the other were the same because they shared a common human nature. The assumption that all persons share a common humanity is one of the normative glories of liberalism. It asserts the equal worth and common reason of all humans. But the presumption of sameness obliterates difference when it erases the markers that distinguish cultures and peoples and create identity and meaning. Survey research concepts and methods in 1957 took for granted that other cultures too were constituted by Lockean individuals (Rudolph 2005a: 6).

6.4.2 Social sciences versus cultural and human sciences (humanities)

The classification of the sciences according to subject areas – natural sciences, social sciences, cultural sciences and human sciences – is neutral in German and does not lead to any confusion. The situation is very different especially in the American context, where political scientists are classified first in the social sciences and second in the humanities. In the first case, especially those who are oriented towards the natural sciences speak of *disciplined scientists*, in the second case of *undisciplined theorists* (Dryzek/Honig/Philips 2009).

This is in my opinion due to the power- and money-oriented struggle for resources (*money- and power-driveness*) between the individual scientists and science schools. The Kuhnian metaphor fuels this struggle even more (section 2.6). Even the simple linguistic designations indicate that researchers within the humanities are stuck in the Middle Ages, while researchers who see and describe themselves as social scientists have long since arrived in the modern age.

Thus, political researchers, if they use a naturalistic methodology within the social sciences, claim to be (social) scientists. They consider their colleagues from the humanities to be theorists. Kerstin Monroe, who feels close to the perestroika movement, suggests that political science should be both:

> a humanistic as well as a scientific discipline (Monroe 2015: 423).

6.4.3 Practical-political (normative, pragmatic, and technical) concepts

In this work, I use concepts developed elsewhere. Only the concepts are presented below. I have explained the used procedure of explication above (section 5.2.5).

A. Politics

Politics is characterized by the fact that it has the competence-competence: in this area one firstly determines which problems must be solved publicly and which privately, and secondly, one decides on maxims of action, strategies of action, instruments of action and instructions for action, i.e. one makes political decisions. Furthermore, one determines within which subsystems and institutions and with which action strategies and means the publicly defined tasks to be performed by the community should be carried out.

B. Political maxims of action (*Handlungsmaximen*)

Political maxims of action (guidelines, maxims, norms, principles and values), in Kantian terminology maxims of action, include all norms that only contain propositions of ought (political, not ethical-moral norms). Maxims of action are *normative standardizations* that represent the *value system of a society.* They establish the *identity* of a political system and create the normative framework for social processes, whereby a political community gains stability. This applies both to maxims of action in the broader sense, such as "justice", "equality" or "fairness", and to subject-specific and concretizable maxims of action for social security.

The political maxims of action are determined in *political-normative value discourses*; they are criteria with which one can evaluate political-pragmatic strategies of action, political-technical instruments of action and individual political instructions for action with the predicates *just* or *unjust*; the justice of political measures is thus determined.

These maxims of action are not ultimate norms or values that are justified in religious or political-philosophical terms, as has been prominently advocated within political science in normative-ontological approaches, e.g. by Eric Voegelin (2004 [1952]) or Leo Strauss (1977 [1953]). They are not even norms or values from which one can derive political-pragmatic strategies of action, political-technical instruments of action, or instructions for action. It is important to note that all maxims of action, as David Ross has stated about ethical norms (Ross 1967 [1930]), are *prima facie* norms, i.e., one must be aware that one cannot directly derive concrete instructions for action from political norms just as from ethical-moral norms.

The codification of the political maxims of action is found in all states in their respective constitutions. Amendments are very rarely made, but the interpretation of these maxims of action is an everyday issue and is carried out by the judiciary as a whole, not only by the highest courts. Furthermore, the executive branch also contributes to further development through their daily application by using the room for interpretation that actually every regulation leaves open to the executors, and indeed must leave open for reasons of principle (cf. Aporias of Practical Reason, section 5.4.5).

With the help of political maxims for action, pragmatic strategies for action, technical instruments for action or technical instructions for action can be evaluated as to whether they are *just* or *unjust.* The focus within political-normative value discourses is the *justice* of political measures and actions.

C. Political strategies for action (*Handlungsstrategien)*

Political strategies for action refer to possibilities for action that have not yet taken concrete shape. These strategies indicate the path that can be taken in order to intervene in the social fabric of society with the help of concrete instruments of action.

These are always options that can be chosen depending on the situation. The subjects of action are associations, families, companies, but above all the state. The strategies for action are pragmatic regulations. Political strategies for action should, first, be based on a rational analysis; second, they should set clear, long-term goals; and third, they should explain which political instruments could be used to achieve the maxims of action applicable to a given policy area, taking into account the available resources and possibilities.

Political strategies for action are identified in *political-pragmatic target discourses*. With the help of the pragmatic strategies of action, one can evaluate technical instruments of action or technical instructions for action in terms of whether they are *wise* or *unwise*; the focus is on the *phronesis* of political measures.

D. Political instruments of action (*Handlungsinstrumente)*

The political instruments for action are located at the operational level. These form the concrete, practical implementation of action maxims and action strategies that specify their design normatively, but from which they cannot be derived because of the *prima facie nature* of norms (Ross 1967 [1930]). There is always a choice of options depending on the situation. Subjects of action in this case are also associations, families, companies, but above all the state. The instruments of action are technical regulations (sets of *is* and *ought*).

Political instruments of action are generated in *political-technical means discourses*. Political instruments of action or technical instructions for action can be evaluated in terms of whether they are *efficient* or *inefficient*; the *efficiency* (effectiveness) of political measures is the focus of this study.

E. Political instructions for action (*Handlungsanweisungen)*

Instructions for action or decisions are found at the operational level. Political-technical instruments of action usually consist of several instructions for action that specify a concrete action.

F. Practical judgments

Practical judgments are evaluations of political or social reality, i.e. of political actions and political regulations. Just as there are three different types of discourse (discourse of values, discourse of goals and discourse of means), there are also three different types of practical judgments:

a. **Political-normative judgments**: With the help of political action maxims, political and social reality is evaluated with the predicates *just* or *unjust* within a political-normative value discourse in which the normative approach is used.

b. **Political-pragmatic judgments**: Using the predicates *wise/unwise* or *desirable/undesirable*, political and social reality (strategies for action, instruments for action, and instructions for action) is evaluated within a pragmatic target discourse in which the pragmatic approach is used.

c. **Political-technical judgments**: The predicates *efficient/inefficient* are used to evaluate instruments of action within a technical means discourse in which the technical approach is used.

6.5 Level five: propositions or sentences

The analysis of single propositions, be they e.g. statements or norms, has received a great deal of attention within 20th century philosophy since the *linguistic turn* (Rorty 1967b); unfortunately, this is not the case in political science, surprisingly not even within the science war (*Methodenstreit*).

In the following, some important proposition-level distinctions will be briefly introduced. First, the distinction between assertion propositions (statements) on the one hand and demand propositions (imperatives, norms, prescriptions, or rules) on the other (section 6.5.1) will be discussed. Following this, the Jørgensen dilemma (Jørgensen 1937/1938) will be described. It can only be addressed on the basis of the distinction explained above (section 6.5.2). Finally, I discuss a distinction introduced by John R. Searle (2009 [1969]: 33 ff.), which differentiates between different norms and rules, namely between *regulative* (prescriptive, imperativistic) and *constitutive* (non-imperativistic) norms and rules (section 6.5.3).

6.5.1 Statements or assertion propositions (*Behauptungssätze*) versus demand propositions (*Forderungssätze*), imperatives or prescriptives

The scientistic scientists assume that scientists generate *statements* or *normative statements* and that both are *truth-apt*, even if this is not explicitly discussed. Due to *antiveritative* ideas, the perestroikans deny that statements and normative statements are truth-apt (section 6.3).

In the following, arguments are presented to oppose a reduction of *norms* and *rules* to *normative statements.* A normative statement is not a *contradictio in adjecto*, since two contradictory facts are not asserted. But norms and statements are nevertheless quite different things. Norms, like rules, are *demand propositions* (*Forderungssätze*) or prescriptions; statements, on the other hand, are *assertive propositions (Behauptungssätze).* The latter are *truth-apt*, while the former are not. Statements (assertion propositions) on the one hand and norms (demand propositions, prescriptions or rules) on the other hand cannot be treated in the same way, as Walter Dubislav rightly states:

> And so this is the crucial result: if one treats demand propositions (*Forderungssätze*) as assertive propositions (*Behauptungssätze*) and assumes that the demand propositions are also subject to the true-false alternative in the usual sense, although one can at first find

> no characterization of the true demand propositions differentiating them from the false demand propositions, then there is no justification and no refutation of demand propositions that can show anything more than that they are either free of contradiction or contradictory. It must therefore be the decisive flaw of any ethics that aims to be scientific that it has failed to recognize this fundamental difference between assertive propositions and demand propositions and that it also "naively" subjects the demand propositions to the true-false alternative known from the assertive propositions, even though the demand propositions are as little subject to this alternative as for instance numbers are subject to the alternative healthy-sick[115] (Dubislav 1937: 339; my translation).

Arguments against reducing norms and rules to normative statements, as scientistic scientists usually refer to norms and rules, exist not just at the proposition level but also at other levels (section 6.7). And there are good reasons to distinguish between different norms as well. This will now be shown in the following.

6.5.2 Jørgensen dilemma

In logic, we work with truth tables that contain truth values. Normative, prescriptive sentences (demand propositions, e.g. norms or rules), however, cannot be truthful, as Jørgen Jørgensen points out, and this brings us to the *Jørgensen dilemma*:

> By the word "imperative" I understand imperative sentences which I define as sentences in which the main verb is in the imperative mood. Imperatives in this sense may so comprise not only commands or orders but also requests, pleas, appeals and other linguistic expressions of willing or wishing something to be done or not to be done (Jørgensen 1937/1938: 288).
>
> "Be quiet" – is it true or false? A meaningless question. "Do your duty" – is it true or false? Unanswerable. The two commands may be obeyed, accepted and considered justified or not justified; but to ask whether they are true or false seems without any sense as well as it seems impossible to indicate a method by which to test their truth or falsehood (Jørgensen 1937/1938: 289).

Without the truthfulness of norms there can be no *logic of norms*. There are now two ways out of the Jørgensen dilemma. In the first, one renounces a logic of norms and rules and uses only deontic logic. Deontic logic works with *statements about norms* and not, as is very often falsely claimed (e.g., Ladwig 2006), with *normative statements*. The second possibility is to abandon the truth-ability of demand propositions and thus of norms and rules and look for *truth-analogous predicates* (section 6.7).

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[115] Damit ist das wichtigste Ergebnis gewonnen: wenn man Forderungssätze wie Behauptungssätze behandelt und annimmt, dass auch die Forderungssätze der Alternative wahr-falsch im üblichen Sinne unterstellt sind, obwohl man zunächst keine Charakterisierung der wahren im Unterschied zu den falschen Forderungssätzen finden kann, so gibt es keine Begründung und keine Widerlegung von Forderungssätzen, die mehr zeigt als Widerspruchsfreiheit bezw. Widerspruchserfülltheit derselben. Es muss als der entscheidende Mangel aller wissenschaftlich sein wollenden Ethik bezeichnet werden, daß sie diesen fundamentalen Unterschied zwischen Behauptungs- und Forderungssätzen nicht erkannt hat und auch die Forderungssätze der von den Behauptungssätzen her bekannten Alternative wahr-falsch „naiv" unterstellt, obwohl die Forderungssätze dieser Alternative so wenig unterstellt sind wie etwa die Zahlen der Alternative gesund-krank (Dubislav 1937: 339).

The first way out would mean that one would have to do without logical-rational analyses in practical (normative, pragmatic and technical) discourses, since a rational discussion depends on bivalence (see the principle of non-contradiction (PNC) above, section 5.4.1). Therefore, one would have to limit oneself to empirical (descriptive-interpretative, explanative and prognostic) discourses, as practiced by many scientists, especially with reference to Weber.

The second way out is much more attractive in my opinion. Von Wright (1977g [1974]) showed a way out of the Jørgensen dilemma on the purely normative-prescriptive level by distinguishing a deontic logic, a logic of *is-ought* (*Sein-Sollen*, statements about norms), from a logic of norms or an *act-ought* (*Tun-Sollen*) logic (section 6.7).

Klaus Kornwachs (2008) focuses primarily on the *technical level* and, following Mario Bunge (1967b), distinguishes between *statements* and *rules*. Rules are also not truth-apt, but *effective* or *ineffective*. On the technical level, an implementation logic (*Durchführungslogik*) is therefore required (comparison between statement logic and implementation logic in Kornwachs 2012: 186, Poser 2001).

Neither norms nor rules are *truth-apt*. In practical (normative, pragmatic and technical) discourses, *truth-analogous predicates* could be used as an alternative, so that bivalent distinctions would again be possible. In ethical-moral discourse, *right* and *wrong* (correctness, *Richtigkeit*) should be used for the discourse on the individual level and *just* or *unjust* (justice, *Gerechtigkeit*) for the political-normative discourse (normative judgments) on the collective level. In pragmatic discourses, the predicates *wise* or *unwise* (phronesis, *Klugheit*) as well as *effective* and *ineffective* (effectiveness, *Effektivität*) are used in technical discourses (section 5.3.3).

6.5.3 Searle: regulative (prescriptive, imperativistic) versus constitutive (non-imperativistic) norms and rules

Now the difference between *regulative* (prescriptive, imperativistic) norms and rules on the one hand and *constitutive* (non-imperativistic) norms and rules on the other will be discussed. The former are especially important in practical (normative, pragmatic, and technical) discourses, the latter when it comes to interpretations or descriptions. John Searle was the first to make this distinction:

> As a start, we might say that regulative rules regulate antecedently or independently existing forms of behavior; for example, any rules of etiquette regulate inter-personal relationships which exist independently of the rules. But constitutive rules do not merely regulate, they create or define new forms of behavior. The rules of football or chess, for examples, do not merely regulate playing football or chess, but as it were they create the very possibility of playing such games (Searle 2009 [1969]: 33).

Regulative rules have an imperativistic form, whereas constituent rules take a non-imperativistic form and are tautological or analytic (Searle 2009 [1969]: 34). Regulative rules can be used to make *evaluations*, while *constituent rules* lend themselves to *specifications* (Searle 2009 [1969]: 36).

Political norms and rules are usually all prescriptive or regulative and are also formulated in imperativistic terms, i.e., they require or prescribe something for all members of a political system or state. As a rule, people have an alternative: to behave in conformity with these commands or prohibitions, or to violate them. The *moral, social, and political norms or values* that influence science are dealt with in this book in the chapters on ontology (chapter 3) and especially in chapter 4 on axiology.

But Searle is interested in the constitutive rules because he can use them to specify his speech act theory, according to which:

> speaking a language is performing acts according to rules. The form this hypothesis will take is that the semantic structure of a language may be regarded as a conventional realization of a series of sets of underlying constitutive rules (Searle 2009 [1969]: 36-37).

The *constituent norms, rules or values* for the "enterprise" or "game" science are discussed in this chapter as well as in Chapter three Epistemology. It is certainly a worthwhile task to trace in the work of the interpretivists to demonstrate to what extent they think of constituent norms and rules when they reject a separation between *is* (factuality) and *ought* (normativity; section 5.4.6).

6.6 Level six: theories

At the theoretical level, it is important to separate two different functions or ways of uses: content (section 6.6.1) and methodological function (section 6.6.2).

6.6.1 The content function of theory

Theories are the most treated topics within political science, because they ultimately depict the *content* of a science. For example, in the eleven-volume Oxford Handbook, there is only one volume on methodology, while nine volumes contain different theories and one volume offers a general overview.

Empirical (descriptive-interpretative, explanative and prognostic) propositional systems generated by empirical political scientists can be taken for granted by practical political scientists. Practical regulations can be developed with reference to these political realities. Empirical theories thus become an important part of practical theories. The latter, insofar as they are pragmatic and technical regulations, must always have a connection to empirical reality if they want to be more than dystopias or utopias.

6.6.2 The methodological function of theory

Theories can also have a methodological function, precisely when they are used to generate other theories or are adduced or used as evidence for other theories. Causality theories, which were primarily developed in the philosophy of science, serve to develop methods and methodical approaches with the help of which one can concretely prove causalities in individual sciences (Brady 2011 [2009]). Rational choice theory, which I will deal with later in detail (section 6.10), can also be used to formulate methods and methodical approaches with the help of which additional content can be generated in the form of other methodological theories.

6.7 Level seven: logic

At the logic level, *formal inferences* and *rules of inference* are discussed in relation to scientific concepts and scientific propositions, in this case predicates and statements, norms and rules. A systematic discussion of this level is not found in any methodology books within political science, even if logic is occasionally referred to in aphorisms, as by Bent Flyvbjerg, quoting Pierre Bourdieu:

> [P]ractice has a logic which is not that of logic (Flyvbjerg 2001: 38).

There is no discussion of deontic logic or norm logic. In the following, the principal differences between these different types of logic are listed, as elaborated in particular by Georg Henrik von Wright (1977a).

This brief overview is intended to show that, for logical reasons alone, a fundamental distinction between empirical (descriptive-interpretative, explanative and prognostic) statements on the one hand and practical (normative, pragmatic and technical) norms and rules on the other is absolutely necessary. Within the field of political science, neither scientistic scientists nor perestroikans take these basic logical distinctions to heart. The perestroikans generally consider a separation between *is* and *ought* to be impossible. The scientistic scientists do distinguish between *is* (factuality) and *ought* (normativity), but they think that causal statements can simply be inversed and then transformed into *normative statements.* I have already pointed out in other parts of the thesis that one should not speak of normative statements but of technical rules (section 6.5.2). I now show on the logic level that different logics apply in the realm of *is* (factuality; section 6.7.1) and *ought* (normativity; section 6.7.2) and why one has to distinguish between statements, norms and rules for purely logical reasons.

6.7.1 Logic level of empirical (political) sciences

Logic has an analytical and a prescriptive character, so with the help of propositional and predicate logic as well as various modal logics, *truth-apt*, empirical propositional systems are analyzed – or, expressed prescriptively, the following logics must be observed by empirical propositional systems:

A. *Propositional logic* (propositional calculus, statement logic, sentential calculus, sentential logic) and *predicate logic* (first-order predicate calculus):
   It is true/false that...
   (Quine 1981 [1964], von Kutschera/Breitkopf 2007, Stuhlmann-Laeisz 2002), Detel 2007, Stegmüller/von Kibéd 1984).

B. *Alethic modal logic*: It is necessary/impossible/possible/contingent that... (Hughes/Cresswell 1978 [1968], Hintikka 1969a).

C. *Temporal/tense logic*: It will sometimes/will always/was sometime/has always been the case that... (Prior 1961, 1968, Goranko/Rumberg 2022).

D. *Epistemic logic*: It is known/is epistemically necessary/is inconsistent that... (Hintikka 1969b [1962], Lenzen 1980).

E. *Deontic logic* (is-ought, *Sein-Sollen*): It ought to be/it is forbidden/permitted/indifferent that... (von Wright 1977a [1951]).

Only in deontic logic is a *statement* made in the first proposition (existence statement) about a *demand proposition* (norm or rule). With all other modal logics, a statement is made about another statement. With the help of deontic logic one can examine the formal relationships of an *empirical* discourse, i.e. one can make statements about norms and rules and thus about norms and regulations. With the *logic of norms*, on the other hand, one can analyze a *practical* discourse. Within practical political science, one needs both, because pragmatic and technical theories contain statements about political reality as well as practical and technical regulations, i.e. norms and rules on how reality should be changed or shaped.

According to von Wright (1963: 105), the distinction between *norms* on the one hand and *statements about norms* on the other goes back to *Ingemar Hedenius*. Von Wright has shown in several articles (the most important ones were edited by Hans Poser, cf. von Wright 1977a) that there are fundamental difference, for example, between the statement or descriptive proposition “It is forbidden to kill” and the norm or normative proposition “You should not kill”. According to him, one must distinguish between an “is-ought” or a truth-apt deontic modal logic on the one hand and an “act-ought” or a non-truth-apt norm logic on the other. An “is-ought” relates the deontic operators to “action propositions” (more precisely action-statements), to facts or states of affairs, an “act-ought” to “action verbs”, to actions (von Wright 1977g [1974]: 120; see above section 6.5.2 Jørgensen dilemma. On the logic of norms, see Kalinowski 1973, von Kutschera 1973, von Wright 1977f [1974], von Wright 1977g [1974]).

Weber takes a similar view:

> When the normatively valid becomes the object of empirical investigation, it loses, as an object, its norm character: it is treated as “being” [*seiend*], not as “valid” [*gültig*][116] (Weber 1973d [1917]: 531 [493]; my translation).

This is exactly what happens when one makes empirical statements about, for example, political norms and rules. Which standardization and regulations within a

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[116] Wenn das normativ Gültige Objekt empirischer Untersuchung wird, so verliert es, als Objekt, den Norm-Charakter: es wird als ‚seiend‘, nicht als ‚gültig‘ behandelt (Weber 1973d [1917]: 531 [493]).

state are considered valid at a historical point in time is then empirically recorded. This in no way formulates normative, pragmatic, or technical claims or prescriptions. In my opinion, these logically correct conclusions and distinctions, which von Wright worked out very succinctly and which can be traced back to Weber, are unfortunately not made by the scientistic scientists either. On the contrary, the concept of normative statements only creates confusion.

The perestroikans as well as the interpretivists deny the separation between *is* and *ought*, a viable logical justification is nowhere in sight, and the mantra that this distinction is not possible is anything but convincing, especially since there are viable alternatives. These would have to be refuted first.

6.7.2 Logic level of practical (political) sciences

At the logic level of practical sciences, formal inferences are treated in relation to practical concepts and scientific propositions, in this case specifically to norms or (pragmatic or technical) rules. Since practical theories contain both empirical statements and practical norms or rules, empirical statements and statements about norms and rules must first be analyzed using the truth-apt logics described above. The following *non-truth-apt logics* are then used to analyze practical systems of demands; or, prescriptively formulated, the following logics must be observed by practical standards and regulations:

A. *Logic of norms* (act-ought, *Tun-Sollen*, von Wright 1977g [1974]. On the logic of norms see Kalinowski 1973, von Kutschera 1973. On the logic in general see von Kutschera/Breitkopf 2007, Stuhlmann-Laeisz 1983, 1986).

B. *Juridical logic* (Weinberger 1970).

C. *Implementation logic*: Technical rules and their formal relations cannot be reproduced with statement and modal logic but require an *implementation logic* due to the logical structure of technical knowledge. The predicates are either *effective* or *ineffective*. Thus, technical knowledge has a *sui-generis* character and technical sciences are not applied natural sciences (Bunge 1967b, Poser 2008b, Kornwachs 2008, Kornwachs 2012).

> All of this shows that we have to concentrate on methods, not on an ontology of artifacts, in order to mark the difference between sciences and engineering (Poser 2001: 195; see Poser 2012 [2001]: 315).
>
> Therefore, engineering as an applied science cannot consist in the application of pure science, even if the sciences might be and are helpful with respect to theoretical boundaries. Applied sciences have their own goals, and, consequently, their own methods (Poser 2001: 197).

These results of the philosophy of technology are also important for political science, because in this field too there is the belief that applied sciences or knowledge can be justified with the help of inversions of causal statements (section 4.2.2).

6.8 Level eight: modes or ways of argumentation

This level is concerned with *modes or ways of argumentation* of scientific theories or with the logical structure of scientific arguments; Popper speaks of "The Logic of Scientific Discovery", the title of his major work (Popper 2005 [1934]). This level is different from the level of logic discussed above. Here we are concerned with the *logical structure* of the arguments put forward, in short with *ways of argumentation* underlying a scientific theory, and not with the relation of individual propositions or predicates to each other as in logic.

In the following, Flyvbjerg's (2001) and Schram's (2003 and 2005) criticism of the deductive procedures within the explanative-prognostic or the Platonic-Galilean tradition will be explained in more detail. The two authors draw primarily on the book Return to Reason by Stephen Edelston Toulmin (2001) (section 6.8.1). Nomothetic versus ideographic argumentation, generalization versus case-by-case treatment are discussed in the next section (section 6.8.2). Then some examples of descriptive (interpretative), explanatory and practical ways of reasoning will be provided and the logical structure that descriptions, explanations, norms and regulations can assume will be shown (section 6.8.3).

6.8.1 Deductively based rationality versus inductively justified reason or reasonableness

A. Criticism of the perestroikans on deductive procedures

Toulmin, a noted Wittgenstein student, formulated his critique of the widespread focus on deductive methods in the mid-20th century in his first book. He does not argue after Aristotle, so he was very surprised that a reviewer presented his book as a reintroduction of the topic:

> Even the fact that the late Gilbert Ryle gave the book to Otto Bird to review, and Dr. Bird wrote of it as being a "revival of the *Topics*" made no impression on me. […]
>
> So, after all, Otto Bird had made an important point. If I were rewriting this book today, I would point to Aristotle's contrast between "general" and "special" topics as a way of throwing clearer light on the varied kinds of "backing" relied on in different fields of practice and argument (Toulmin 2003 [1958]: VIII).

From this one can also indirectly recognize that Aristotle, in contrast to many scientists today, did not favor any particular method of argumentation, but rather presented the possibilities and limitations of all the tools available at the time.

It is important to make some conceptual clarifications. Toulmin refers to the English concept's *rationality* versus *reason* or reasonableness as *twin concepts* (Toulmin 2001: 2). *Rationality* is nowadays always used in connection with *certainty*; it is a *descending, deductive way of reasoning* from ideas, general concepts, general laws, models or theories to individual facts or judgments that are formally valid or always considered valid:

> Rationality goes with focusing narrowly on matters of content, reasonableness with a feeling for the dozen ways in which a situation may modify both the content and the style of arguments (Toulmin 2001: 21-22).

One can only understand the twin concepts of *rationality* and *reason/reasonableness* by representing Toulmin's plea for a rehabilitation of emergent, epagogical, inductive modes of argumentation. In his early work, which Schram unfortunately does not address, Toulmin (1996 [1958]) argues in particular against an *argumentative reductionism* that posits *analytic and deductive modes of reasoning as standard* and representative of scientific reasoning. In doing so, he makes the following subdivisions:

a. Deductive, analytic, warrant-using, conclusive, formally valid modes of reasoning (the deductive-nomological model (HO scheme) and the evolutionary explanatory model can be seen as examples, but they were not listed by Toulmin).

b. Inductive, substantive, warrant establishing, tentative, not formally modes of argumentation. *Examples*: Hegelian dialectic, hermeneutic circle as spiral movement of understanding, argumentation model of Toulmin, argument maps (these examples are also mine).

> The divisions of arguments into analytic and substantial, into warrant-using and warrant establishing, into conclusive and tentative, and into formally and not formally valid: these are regimented for purposes of theory into a single distinction, and the pair of terms "deductive" and "inductive", which in practice – as we saw – is used to mark only the second of the four distinctions, is attached equally to all four (Toulmin 2003 [1958]: 134).

Above all, Toulmin points to the *area dependency*, which is also reminiscent of Aristotle, of the different *ways of argumentation*, and examines their different *structures*:

> This is best indicated, in general terms, a systematic divergence between two sets of categories: those we find employed in the practical business of argumentation, and the corresponding analyses of them set out in books on formal logic. Where the standards for judging the soundness, validity, cogency or strength of arguments are in practice field-dependent, logical theorists restrict these notions and attempt to define them in field-invariant terms (Toulmin 2003 [1958]: 136-137).

According to Toulmin, an *imbalance* between *rationality* and *reasonableness* began in the 17th century. This only came to full fruition at the beginning of the 20th century within logical empiricism or the philosophy of ideal language and critical rationalism:

> So the three chief Dreams of Rationalism turn out to be aspects of a single larger Dream. The Dream of a Rational Method, that of an Exact Language, and that of a Unified Science form a single project designed to purify the operations of the Human Reason by *desituating* them: that is, divorcing them from the compromising association of their cultural contexts (Toulmin 2001: 78).

Toulmin's goal in his book "Return to Reason" is not, as the phronetic perestroikans believe, a replacement of the deductive (*top down*) by inductive or epagogic (*bottom up*) procedures, but a restoration of the equilibrium and the balance between descending and ascending conclusion procedures:

> The chief task of this book is to show what is needed if we are to treat that injury, and reestablish the proper balance between Theory and Practice, Logic and Rhetoric, Rationality and Reasonableness (Toulmin 2001: 13; see also chapter 10: Redressing the Balance, p. 155 ff.).
>
> Rhetoric is not a rival to Logic; rather, it puts the logical analysis of arguments into the larger framework of argumentation [...].
>
> Rhetorical tricks are on occasion used to evade or conceal a substantive point, but that once again is a matter of what *may* happen, not what *must* happen: overall, the act of arguing still has the dual role of seizing the hearers' attention and using this to convince them of a well-founded claim (Toulmin 2001: 165).
>
> But it was no part of my agenda to tip the scale entirely, or to elevate Practice, in turn, at the expense of Theory. What I intended to do was, indeed, to restore a proper balance between them: to recognize the legitimate claims of "theories" without exaggerating the formal attractions of Euclidean reasoning, and to defend the lessons of actual "practice" without denigrating the powers of theoretical argument (Toulmin 2001: 171).

Toulmin is right, it is not important to play deductive versus inductive arguments against each other. Rather, it is important to use the variety of arguments correctly and, in my opinion, to continue to supplement and develop them further.

B. Principal limit of deductive methods on the example of the DN model of explanation

It is surprising at first glance that Brady (but also all other authors in the volume "Political Methodology"; Box-Steffensmeier/Brady/Collier 2010a [2008]), even in a chapter on causality, does not refer to the deductive-nomological model (DN model) at all. In the 20th century, this model was the most widespread and the mode of reasoning accepted by the overwhelming majority of all scientists (*communis opinio doctorum*), also called the HO (Hempel-Oppenheim) scheme after its "inventors" (Hempel/Oppenheim 1948, Hempel 1972 [1966], but also Popper 2005 [1934]). However, this model was already developed in the 19th century:

> In point of fact the "Popper-Hempel" theory of explanation had been something of a philosophic commonplace ever since the days of Mill and Jevons (von Wright 1971: 175).

The DN model of explanation claims *universal validity* for every type of explanation, not only for causal explanations. The concepts of *cause* and *effect* are therefore not even mentioned in the explanation scheme. The explanation consists in the logical deduction from other facts and superior laws; therefore, this is also called a *subsumption* theory of explanation or simply *covering law model.*

Georg Henrik von Wright (1971: 10 ff.) presented two basic models of the DN model: deductive-nomological explanations, subsumption theory of explanation (a) and inductive-probabilistic models and explanations (b).

a. Deductive-nomological explanations, subsumption theory of explanation

I. *Antecedent, singular conditions* (*conditio*) C1, C2 [...] Ck
II. *Explanans* (that which explains), *general laws* (*lex*) L1, L2 [...] Lk
____
III. *Explanandum,* the event to be explained E

The deductive-nomological explanation:

> answers the question, "Why did the explanandum event occur?" by showing that the event resulted from the particular circumstances specified in C1, C2 [...] Ck in accordance with the laws L1, L2 [...] Lk[117] (Hempel 1972 [1966]: 239; my translation. See Hempel /Oppenheim 1948 and Popper 2005 [1934]).

However, the DN model only applies in a *deterministic* world:

> The causal view, on the other hand, finds its adequate expression in the Laplacian demon. From this point of view – and this is exactly what the discussion of the DN explanation of the HO schema showed – the schema of the prognosis is the same as that of a retrodiction. In this way, the past and the future are fundamentally similar to each other, because both are equally enlightened by the present, and the future will bring nothing fundamentally new, because the laws are already established[118] (Poser 2012 [2001]: 283; my translation).

This causal determinism was formulated using the example of physics by Pierre-Simon de Laplace (1749-1827), which is why Hans Poser uses the phrase "Laplace's demon":

> We may regard the present state of the universe as the effect of its past and the cause of its future. An intellect which at a certain moment would know all forces that set nature in motion, and all positions of all items of which nature is composed, if this intellect were also vast enough to submit these data to analysis, it would embrace in a single formula the movements of the greatest bodies of the universe and those of the tiniest atom; for such an intellect nothing would be uncertain and the future just like the past would be present

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[117] beantwortet die Frage, ‚Warum trat das Explanandum-Ereignis ein?', indem sie zeigt, dass sich das Ereignis aus den besonderen in C1, C2 [...] Ck spezifizierten Umständen in Übereinstimmung mit den Gesetzen L1, L2 [...] Lk ergab (Hempel 1972 [1966]: 239, vgl. Hempel /Oppenheim 1948 und Popper 2005 [1934]).

[118] Die kausale Sicht dagegen findet ihren adäquaten Ausdruck im Laplaceschen Dämon. In dieser Sicht – und gerade das zeigte die Diskussion der DN-Erklärung des HO-Schemas – ist das Schema der Prognose dasselbe wie das einer Retrodiktion. Damit sind Vergangenheit und Zukunft einander im Grundsatz ähnlich, denn beide werden von der Gegenwart her gleichermaßen erhellt, und die Zukunft bringt nichts grundsätzlich Neues, weil die Gesetzmäßigkeiten jetzt schon festliege (Poser 2012 [2001]: 283).

> before its eyes (*Pierre Simon Laplace, A Philosophical Essay on Probabilities,* quoted from Wikipedia: https://en.wikipedia.org/wiki/Laplace%27s_demon).

b. Inductive-probabilistic models and explanations

Probabilistic explanations would have the same logical structure. They differ from deductive-nomological explanations, among other things, in that, first, some or all of the laws are of *probabilistic-statistical form.* Second, the truth of the *explanans* does not make the truth of the *explanandum* certain, only more or less *probable.*

The first model, the DN explanatory model, is *symmetrical*: one can explain the past with it as well as predict the future. The second model, the *evolutionary explanatory model*, which has come more and more to the fore since the rise of the biological sciences in the second half of the 20th century, is *asymmetrical* because *mutations*, i.e. simple chance, cannot be predicted by laws but can only be explained in retrospect.

> It is understandable that the logos of genesis is no longer seen in terms of causality – because every causal view (and every HO explanation) fails as soon as the historical emergence of biological, social and cultural diversity becomes the subject of the question[119] (Poser 2012 [2001]: 281-282; my translation).

But this already provides one reason why one should not use the DN model in political science. Another reason, probably the decisive one or at least no less important for Brady, lies in the status that laws have within the model.

The *general laws* connect the *explanandum* event with the conditions listed in the *explanans.* General laws have the status of *explanatory factors* with regard to the event to be explained, from which the concept *covering law model* comes in English. From the *explanans* to the *explanandum* there is a *deductive certainty* or *inductive probabilities.* In *explanation*, the *explanandum* is known first, in *prediction*, the *explanans.*

> Logical deduction guarantees, so to speak, the transfer of the positive truth value, the truth, to the derived statements. Furthermore, it can be shown in a relatively simple way that the falsity of a statement results in the falsity of at least one of the premises. Thus, one could speak here of a retransfer of the negative truth value[120] (Albert1967c [1965]: 409; my translation).

Now that one realizes the great importance of laws, one can also see why Brady does not use this model at all. Probabilistic laws, unlike deterministic laws, *cannot* provide

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[119] Daß der Logos der Genese nicht mehr in der Kausalität gesehen wird, ist verständlich – denn jede kausale Sicht (und jede HO-Erklärung) versagt, sobald die geschichtliche Entstehung biologischer, sozialer und kultureller Vielfalt zum Gegenstand der Frage wird (Poser 2012 [2001]: 281-282).

[120] Die logische Deduktion garantiert gewissermaßen den Transfer des positiven Wahrheitswertes, der Wahrheit, auf die abgeleiteten Aussagen. Außerdem lässt sich auf eine verhältnismäßig einfache Weise zeigen, daß sich aus der Falschheit einer abgeleiteten Aussage die Falschheit mindestens einer der Prämissen ergibt. Man könnte also hier von einem Rücktransfer des negativen Wahrheitswertes sprechen (Albert 1967c [1965]: 409).

deductive certainty. This now also has fatal consequences for fallibilism: one cannot refute a hypothesis with a counterexample, since this could be an exception. By *definition*, probabilistic laws or regularities do not apply to all cases, but at most to the overwhelming majority of cases.

Also, Popper's (2005 [1934]) *exaltation* of *deduction* and *demonization* of *induction* becomes obsolete. If one has identified a regularity on the macro level by means of the methodological approaches described above, one cannot deduce it on the micro level; in other words, there remains the pairing problem, which can only be solved on the micro level. *Deduction comes up against principle limits, as does induction.* Thus, the *hiatus* between micro and macro level cannot be overcome logically correctly in any direction.

An example: the probabilistic law or regularity that smoking causes lung cancer does not provide the explanatory factor in each individual case, because of its probabilistic nature. One cannot explain the following individual cases: why, someone who smokes does not have lung cancer ; why someone who does not smoke gets lung cancer; and why someone who smokes gets lung cancer from, say, other factors such as environmental pollution (section 4.2.2: equifinality, asymmetric causality, multicollinearity and conjunctural causality).

In addition, regularities also cannot explain the course of the cause-effect mechanism, which in this case is biological, leading from smoking to cancer. In other words, only the *why* is answered, not the *how*.

Here we come up against fundamental limits of deduction: there is *no deductive certainty*, just as there can be *no inductive certainty.* If one has recognized the biological *causal mechanism* at the micro level, one cannot conclude that this connection applies to all cases at the macro level. Smoking does not explain lung cancer in all cases, or smoking usually causes lung cancer but not in every case.

Deduction, like induction, has structural limits. It is also an argument against fallibilism. However, it makes no sense to demonize deduction as Popper (2005 [1934]) did with induction. Brady does the right thing by simply ignoring the DN model; it simply is not usable in a non-deterministic world – and the real world, especially the political world, simply is not deterministic. Furthermore, we should again recall that he insists that all four methodological approaches mentioned above are necessary for adequate causal discussions.

With the help of the first three approaches (regularity, counterfactual and manipulative approach) one can only recognize regularities on the macro level and thus the nomological property of causality. The *pairing problem* and the precise explanation of the *cause-effect mechanism* can only be solved with the fourth approach, the mechanism and capacity approach, at the micro level, where the micro level is simply each individual case. Thus, the *ontic* property of causality, i.e. the precise explanation of the causal mechanism, cannot be determined at the macro level, i.e. by examining all cases of smoking and cancer within the regulative, counterfactual or manipulative approach (section 4.2.2).

6.8.2 Nomothetic versus ideographic ways of reasoning – generalization versus case-by-case treatment

A. Nomothetic versus ideographic ways of reasoning

On the level of argumentation methods, another approach besides deductive and inductive argumentation methods is presented here in order to highlight a fundamental difference between natural sciences and, in this case, above all historical sciences. Wilhelm Windelband was the first to make a distinction between *nomothetic* or *law sciences* (*Gesetzeswissenschaften*), which want to recognize the general, and *ideographic* or *event sciences* (*Ereigniswissenschaften*), which want to recognize the particular or the individual.

> The sciences of experience seek in the knowledge of reality either the general in the form of natural law or the particular in the historically determined form; they consider in one part the always unchanging form, in the other part the unique, intrinsically determined content of real events. The one are law sciences, the other event sciences; the former teach what always is, the latter what once was. Scientific thinking is – if one may form new terms of art – in the one case nomothetic, in the other ideographic[121] (Windelband 1900 [1894]: 12; my translation).

The distinction between nomothetic and ideographic knowledge is adopted by the perestroikans, who claim that the scientistic scientists favor a nomothetic approach, while the social sciences also require an ideographic way of argumentation.

The perestroikans construct a contrast to the scientistic scientists that no longer exists. Furthermore, they overestimate the ideographic approach; this too must be supplemented with nomothetic ones. Here again, a more careful study of Aristotle's work would have been of benefit:

> The history of philosophy knows, simplistically speaking, the epistemological antagonism of "empiristic" disregard and "idealistic" overestimation of the general. Aristotle's theory of epistemic increase strikes a promising middle course between the two positions. Although the higher knowledge is directed to something general and shows a superiority

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[121] [D]ie Erfahrungswissenschaften suchen in der Erkenntnis des Wirklichen entweder das Allgemeine in der Form des Naturgesetzes oder das Einzelne in der geschichtlich bestimmten Gestalt; sie betrachten zu einem Teil die immer sich gleichbleibende Form, zum anderen Teil den einmaligen, in sich bestimmten Inhalt des wirklichen Geschehens. Die einen sind Gesetzeswissenschaften, die anderen Ereigniswissenschaften; jene lehren, was immer ist, diese, was einmal war. Das wissenschaftliche Denken ist – wenn man neue Kunstausdrücke bilden darf – in dem einen Falle *nomothetisch*, in dem andern *ideographisch*" (Windelband 1900 [1894]: 12).

> in it, the lower levels are left with an intrinsic value; and this exhorts the higher levels to more modesty[122] (Höffe 2006 [1996]: 45; my translation).

B. Generality, general relationships versus individual cases

> In the one, thought drifts from the determination of the particular to the conception of general relations; in the other, it is held fast in the affectionate expression of the particular. For the natural scientist, the individual given object of his observation never has scientific value as such; it serves him only in so far as he may consider himself justified to regard it as a type, as a special case of a generic concept and to develop this from it; he reflects in it only on those features which are suitable for insight into a lawful generality[123] (Windelband 1900 [1894]: 16; my translation).

As I have shown above (section 2), both subdivisions have a long history. Both the nomothetic and the ideographic approach are also followed by *scientistic scientists*. Also, individual cases are not analyzed just so that generalizations can be made. The individual case is not only seen as a special case on the micro level, but on the individual case the causal mechanism, for all similar cases must be proved, the pairing problem and the complex causal structures must be disentangled. Strictly speaking, only correlations can be proven on the macro level. One can only speak of causality if the causal mechanism, the *how*, has also been proven.

The situation is similar with the *interpretivists*. They too have methodological approaches and methods whose focus is one-sided on the macro or micro level. The (post-) structuralists and discourse theorists analyze in particular the macro level, structures, discourses, framing (frames) that shape communication and the creation of meaning. Others use above all interpretative-hermeneutic methods and methodical approaches in order to work out meanings and family similarities within concrete language games through the analysis of individual cases (Münch 2016: 35 ff.).

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[122] Die Geschichte der Philosophie kennt, vereinfachend gesprochen, den wissens–theoretischen Antagonismus von ‚empiristischer' Geringschätzung und ‚idealistischer' Überschätzung des Allgemeinen. Aristoteles' Theorie der epistemischen Steigerung schlägt zwischen beiden Positionen einen erfolgversprechenden Mittelweg ein. Obwohl sich das höhere Wissen auf etwas Allgemeines richtet und darin eine Überlegenheit zeigt, bleibt den unteren Stufen ein Eigenwert; und dieser mahnt die höheren Stufen zu mehr Bescheidenheit (Höffe 2006 [1996]: 45).

[123] In der einen treibt das Denken von der Feststellung des Besonderen zur Auffassung allgemeiner Beziehungen, in der andern wird es bei der liebevollen Ausprägung des Besonderen festgehalten. Für den Naturforscher hat das einzelne gegebene Objekt seiner Beobachtung niemals als solchen wissenschaftlichen Wert; es dient ihm nur soweit, als er sich für berechtigt halten darf, es als Typus, als Spezialfall eines Gattungsbegriffs zu betrachten und diesen daraus zu entwickeln; er reflectiert darin nur auf diejenigen Merkmale, welche zur Einsicht in eine gesetzmäßige Allgemeinheit geeignet sind (Windelband 1900 [1894]: 16).

In the Philosophical Investigations, Ludwig Josef Johann Wittgenstein (1953) also treats both levels at once, even though the interpretivists, who almost always refer to Wittgenstein, emphasize only his contributions to the micro level.

The first part of the Philosophical Investigations is not about *thinking*, but about *looking* (Wittgenstein 1953: § 66). It is therefore important to record the use or application of the words within *language games* and to look for *family resemblances.* The practice of language use is in the focus here; the use of language is presented as part of an activity, more precisely a form or *way of life* (*Lebensform*):

> [T]he fact that the speaking of language is part of an activity, or of a form of life (Wittgenstein 1953: § 23).

In this first part, the well-known *usage theory of meaning* (*Gebrauchstheorie der Bedeutung)* is formulated:

> The meaning of a word is its use in the language (Wittgenstein 1984c [1953]: § 43).

It is not the conditions under which statements become true that are examined, but how they are used. This:

> is a meta-theory of assigning meanings (rule sequences, *Regelfolgen*) that displaces the competing meta-theory of truth conditions[124] (Stegmüller 1986a: 113, Kripke 1982: 74 ff. and 87; my translation).

However, Wittgenstein also deals with the macro level, in this case with our way of life and how language can function in it at all (section 5.4.8). There is also a hiatus between the micro and the macro level within language. The investigations on the different levels have other goals. On the macro level, the goals are, first, to determine under which conditions language is possible at all and which rules are necessary for it; And second, how language functions in concrete terms at the micro level, by examining language games and the use of words and by identifying family resemblances. In doing so, different complexes of questions are discussed; it is not possible to draw conclusions from one level to the other, because according to the slogan:

> Don’t say [...], but *look and see* (Wittgenstein 1984c [1953]: § 66).

transparency and an overview are first established within language games. Then Wittgenstein analyzes the preconditions that make language possible. One can neither conclude from the individual to the general nor, vice versa, from the general to the individual.

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[124] ist eine Metatheorie der Zuordnung von Bedeutungserfassungen (Regelfolgen), welche die mit ihr konkurrierende Metatheorie der Wahrheitsbedingungen verdrängt (Stegmüller 1986a: 113, Kripke 1982: 74 ff. und 87).

Similarly, one can say that a causal mechanism does not rest on causal regularity. There is a fundamental hiatus between the macro and micro level that cannot be overcome in any direction, neither deductively (*top down*) nor inductively (*bottom up*).

Windelband is right: there is a *fundamental difference* between *nomothetic* and *ideographic* approaches, and the former is at the macro level and the latter at the micro level. However, this does not justify an argument for the difference between scientific, nomothetically oriented sciences on the one hand and humanistic, ideographically oriented sciences on the other. Both levels are studied in all sciences and, just as crucially, there is a fundamental difference between these two levels in all sciences. The former speak of causal regularities or regularities on the macro level, formerly of causal laws, the latter speak of linguistic rules, structures, patterns, discourses, interpretation schemes, frames. At the micro level, the focus is on causal processes or mechanisms as well as individual cases. I have described in detail above how this is implemented in the explanative-prognostic or the Platonic-Galilean tradition (section 4.2.2). A reference to the frame analysis should suffice here, in which a distinction is also made between *framing* and *naming* (Rein/Schön 1993).

In his investigations, Wittgenstein distinguishes between conceptual, *linguistic problems* on the one hand and *natural scientific problems* on the other, as I have elaborated on elsewhere (Lauer 1987).

According to Wittgenstein, philosophy can only contribute to the solution of conceptual or linguistic problems, i.e. in his case it can solve “pseudo-problems”. This is the therapeutic function of which philosophy is capable. However, it cannot contribute to the solution of natural scientific explanations or problems.

Analogously, one can now claim that interpretivists work on conceptual or linguistic problems and questions and scientistic scientists on causal problems. Therefore, science wars between these traditions are actually pointless because they are not competing ventures. What’s more, the two could achieve more together and would be more fruitful as partners or allies rather than opponents. They can fight separately and win together. Victory would mean that both contribute to a scientific-rational world explanation and world change. The science war is therefore a waste of resources and only stands in the way of the common goal of world recognition and world change.

For political scientists, and this is especially true of the perestroikans, interpretive analyses, understanding of meaning, or meaning making are usually only means to an end. The focus is on *power analyses*. Conceptual analyses are meant to make power structures or power relations visible. Therefore, the work of Michel Foucault (1971 [1966] and 1995 [1969]) is much more central. There is nothing wrong with this. I do not deal with them in more detail because the only purpose here is to show the relationship between the macro and micro levels, which serve similar functions for both scientistic scientists and perestroikans or interpretivists.

Thus, by referring to the nomothetic and the ideographic approach, no principled difference between the different traditions can be justified.

6.8.3 Descriptive-interpretative, explanatory, and practical ways of argumentation: descriptive-interpretative, explanatory, normative, and regulatory.

A. Explanatory ways of argumentation: explanations

The best-known explanatory reasoning is causal explanation, discussed above in detail (section 4.2.2).

B. Descriptive-interpretative ways of argumentation: descriptions

The two most significant descriptive-interpretative ways of argumentation are the *hermeneutic circle* as a spiral movement of understanding (a) and the *dialectic* (b).

a. Hermeneutic circle as a spiral movement of understanding

Hermeneutics describes how a common understanding through dialogue is possible. Only when this is available can one proceed to explain something (von Wright 1971). Actually, the hermeneutic circle is based on a spiral movement. Two dialogue partners enter into interaction; they both exhibit a certain prior understanding of the topic. Their positions differ at the beginning of the conversation. Each dialogue partner reconstructs the perspective of the other and adds new thoughts, which leads to a convergent process at each stage of exchange, resulting in a convergent process. This convergent process continues in a spiral movement until a merging of horizons occurs – two divergent positions have evolved into a common understanding along this communicative path. Thus, something more than the mere understanding of another opinion, which is subjectively colored, emerges. Since the interlocutors stand in the same intellectual-historical tradition, there is a mutual control process, which contributes to a balancing of prior understanding and foreign opinion. The result is comprehensible for all participants, thus fulfilling the scientific demand for intersubjectivity (Gadamer 2010 [1960], Habermas 1970, Poser 2012 [2001]: 217-242).

b. Dialectic

The dialectical three-step consists of *thesis*, *antithesis* and *synthesis* and shows the conceptual movement that takes place in a conversation.

> Conversation is according to Gadamer the basic figure of hermeneutics, through which the problem of understanding becomes clear. For the treatment of the dialectic it is advisable to start from there as well; however, it is not the understanding of the conversation that is

> to be examined, but the conceptual movement that takes place[125] (Poser 2012 [2001]: 243; my translation).

C. Practical ways of argumentation: standardizations and regulations

Practical reasoning is a form of inference:

> which, in contrast to deontic logic, directly involves the intentional problem and the ends-means relationship[126] (Poser 1977: XI; my translation).

The best-known practical modes of reasoning are (chart 12, section 9.4.12):

a. practical syllogism

b. intentional syllogism

c. pragmatic syllogism.

a. Practical syllogism

Practical syllogism was described by von Wright as follows:

> A intends to bring about p.
> A considers that he cannot bring about p unless he does a.
> Therefore A sets himself to do a (von Wright 1971: 96, see von Wright 1977c [1963], and 1977d [1972]).

According to von Wright, practical syllogism closes an existing methodological gap in the human sciences. He provides an explanatory scheme that is a clear alternative to the covering law model of explanation. This scheme of a practical syllogism is an *inverted teleological explanation.*

b. Intentional syllogism

> From now on *A* intends to bring about *p* at time *t*.
> From now on *A* considers that, unless he does *a* no later than at time *t´*, he cannot bring about *p* at time *t*.
> Therefore, no later than when he thinks time *t´* has arrived, *A* sets himself to do *a*, unless he forgets about the time or is prevented (von Wright 1971: 102; see Anscombe 1963).

c. Pragmatic syllogism

Pragmatic syllogism has already been discussed when dealing with the inversion of causal clauses or the *inversion* of the fundamental explanatory scheme (section 4.1.4, chart 12, section 9.4.12).

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[125] Die Grundfigur der Hermeneutik, an der die Problematik des Verstehens deutlich wurde, bildet bei Gadamer das Gespräch. Für die Behandlung der Dialektik empfiehlt es sich, ebenfalls von dort auszugehen; jedoch nicht das Verstehen des Gesprächs ist zu untersuchen, sondern die begriffliche Bewegung, die sich dabei vollzieht (Poser 2012 [2001]: 243).

[126] die im Gegensatz zur deontischen Logik die intentionale Problematik und die Zweck-Mittel-Beziehung unmittelbar einbezieht (Poser 1977: XI)

6.9 Level nine: methods

The method level is at the center of methodological work. In spite of this, or precisely because of it, there are some conceptual confusions here, which will be uncovered and explained in the following. Ways of overcoming them will also be formulated. At this level, the structural differences between empirically descriptive-interpretative, empirically explanatory and practical methods are identified (section 6.9.1). After that, the importance of experiments or experimental methods and simulations is discussed (section 6.9.2), and finally the differences between experiments and tests are presented (section 6.9.3).

6.9.1 Quantitative methods as well as qualitative-mathematical versus qualitative-interpretative methods

In the following, the focus is on this tripartite distinction between quantitative, positivistic-qualitative and traditional qualitative methods, as Yanow and Schartz-Shea call them:

> What we are increasingly looking at these days methodologically is, instead, a tripartite division among quantitative, positivist-qualitative, and traditional qualitative methods. The latter have increasingly been termed "interpretative" methods because of their intentional, conscious grounding in or their less explicit but nonetheless recognizable family resemblance to the ontological and epistemological presuppositions of the Continental interpretive philosophies of phenomenology and hermeneutics (and some critical theory) and their American counterparts of symbolic interactionism, ethnomethodology, and pragmatism, among others (Yanow/Schwartz-Shea 2014a[2006]: XX).

I am talking about *quantitative-mathematical* (section A), *qualitative-interpretative* (section B) and *qualitative-mathematical* (section C) methods. I end this subsection by explaining the distinction between data-set observations (DSOs) versus causal-process observations (CPOs, section D).

A. Quantitative-mathematical methods for determining causal regularities or probabilistic laws

There is no confusion when speaking of quantitative methods. Since the orientation towards the exactness of mathematics in the explanative-prognostic or the Platonic-Galilean tradition is visible through the use of logical-mathematical methods, I also use another attribute besides *quantitative* and *qualitative*, namely *mathematical.* This is broader than *metric* or *statistical* and is likely to apply to both quantitative and qualitative methods within the explanative-prognostic or the Platonic-Galilean tradition. This is necessary so that one does not confuse the *qualitative* methods of *determining meaning* with the qualitative methods of *determining causality.*

Quantitative methods are primarily used to identify causalities between different events at the macro level. The aim is to provide *explanations* for causal patterns, universal influencing factors or causal structures at the *macro level.* Specifically, the *aim* is to use correlation or regression analyses to determine *regularities*; the concept of "law" is hardly used anymore.

To make matters worse, due to the complexity of the relationships, often not all the conditions that are placed on causal conclusions can be met, so that at most descriptions or descriptive inferences can be drawn up using quantitative methods:

> Researchers now know that most regression equations simply provide a multivariate summary of the data – at best a *descriptive inference* [emphasis not in the original] – not a sure-fire causal inference about them (King, Keohane, and Verba 1994) because the conditions for justifying a causal interpretation of regression coefficients are not met. Although establishing the Humean conditions of constant conjunction and temporal precedence with regression-like methods often takes pride of place when people use these methods, we know that they seldom deliver a reliable causal inference. Rather regressions are often more usefully thought of as ways to describe complex data-sets by estimating parameters that summarize important things about the data (Brady/Collier/Box-Steffensmeier 2011 [2009]: 1022).

As described above (section 4.2.2), there is still a long way to go from correlation to causality. In addition to macro-quantitative methods, experiments or simulations and qualitative-mathematical methods are necessary.

B. Qualitative-interpretative methods and descriptions

However, misunderstandings arise when speaking of qualitative methods. Therefore, to avoid confusion, I always use the adjective *qualitative* in conjunction with another adjective, either *interpretative* or *mathematical.* To describe methods that have emerged within the human and cultural sciences (humanities) for *interpreting* and *understanding* the social world, I speak of *qualitative-interpretive* or *qualitative-classificatory* methods.

Qualitative-interpretative research aims to describe contexts of meaning or visible phenomena by means of text analyses and linguistic-interpretative tools (concepts, methods and methodological approaches). Linguistic-interpretative tools are used for text analyses; among others, the following qualitative-interpretative methods are discussed in relevant method books: qualitative content analysis, document analysis, and discourse and conversation analysis (Flick/von Kardorff/Steinke 2015 [2000], Schmitz/Schubert 2006a, Denzin/Lincoln 1994, Blatter/Janning/Wagemann 2007, Creswell 2013 [1998], Yanow/Schwartz-Shea 2014 [2006], Bevir/Rhodes 2016a).

C. Qualitative mathematical methods for solving the pairing problem and identifying causal mechanisms

In the following, the qualitative-mathematical methods are presented first, with a focus on the different goals of these methods compared to the quantitative methods (section a). Then the most important goals are explained: first the solution of the pairing problem and the clarification of concrete causalities (section b), and second the determination of causal mechanisms (section c).

a. Qualitative mathematical methods

The *qualitative mathematical* methods that have emerged in the Anglo-Saxon world since the 1970s are used to identify causalities and specifically to identify cause-effect mechanisms, causal mechanisms or causal processes within case studies, small-N studies or medium-N studies (20-50 cases) at the micro level. Gary Goertz and James Mahoney, in their overview “A Tale of Two Cultures. Qualitative and Quantitative Research in the Social Sciences” (Goertz/Mahoney 2012), also use the concept *qualitative* in the sense mentioned above. They at least point out in a footnote that there is also another usage. However, in their work they deal exclusively with qualitative-mathematical methods, but speak of qualitative methods:

> Thus, while interpretative analysts will not find their tradition of research represented in the qualitative culture that we describe, they nonetheless will find many of the tools of their tradition put to use in our analysis (Goertz/Mahoney 2012: 5, footnote 2).

This statement, although I have not found it *expressis verbis*, also applies to the above-mentioned volume “Political Methodology” (Box-Steffensmeier/Brady/Collier 2010a [2008]).

The concept of *two cultures* was introduced by Charles Percy Snow (1965 [1959]) at a time when the concept of qualitative still had a *qualitative-interpretive* meaning. Snow wanted to point out the difference between literary and natural scientific intelligence (Snow 1987 [1965]). Thus, the division into natural sciences and human or cultural sciences, which was developed primarily in German-speaking countries, was also introduced as a cultural boundary in English-speaking countries. Goertz and Mahoney now see a cultural boundary between quantitative methods for determining causal regularities and qualitative methods for determining causal processes. This cultural boundary separates two sections within APSA:

> In political science, there are two methodology sections, the section on Political Methodology, which represents quantitative methodology, and the newer section on Qualitative and Multi-Method Research. In sociology, the section on Methodology stands for mainly quantitative methods, whereas the kinds of qualitative methods that we discuss are associated with the section on Comparative and Historical Sociology (Goertz/Mahoney 2012: 5).

In particular, German political scientists (Wolf 2015, Héritier 2016), who are in the explanative-prognostic or the Platonic-Galilean tradition, think primarily of QCA (Wagemann 2015) or process tracing (Starke 2015) when they speak of qualitative methods, rather than qualitative content analysis or document analysis.

Qualitative-interpretative ways of reasoning, e.g. interpretation, hermeneutics and dialectics, play no role here. The fact that an understanding must precede an explanation (von Wright 1971) is also not a problem at all. First, one has to agree on a description of the events between which causal regularities are to be demonstrated at the macro level. This is also true at the micro level: first a phenomenon must be described, then an invisible causal process that produces the phenomenon can be explained.

The quantitative methods are designed to demonstrate causal regularities at the macro level using large-N studies. The qualitative-mathematical methods are needed at the micro level, first, to solve the pairing problem (Bennett 2010 [2004], Brady 2010 [2004]) – QCA is at the center of this – and second, to identify causal mechanisms or causal processes (Fearon/Laitin 2011 [2009]; see also "Mechanism and Mechanism-based explanations", Hedström 2010 [2008]: 321):

> Despite some claims to the contrary in the qualitative methods literature, case studies are not designed to discover or confirm empirical regularities. However, they can be quite useful – indeed, essential – for ascertaining and assessing the causal mechanisms that give rise to empirical regularities in politics. We have argued that random selection of cases for narrative development is a principle and productive criterion in studies that mix statistical and case-study methods, using the former for identifying regularities, and the latter to assess (or to develop new) explanations of these (Fearon/Laitin 2011 [2009]: 773).

So, while large-N studies are used to determine causal regularities at the macro level with the help of quantitative methods, i.e. they clarify the *why* question, the issue now is determining the causal cause-effect mechanism at the micro level, i.e. to clarify the *how* question.

In the U.S.A. in particular, new methods have emerged that have received the attribute *qualitative*, although they differ significantly from the qualitative-interpretative methods for capturing meaning and contexts of meaning outlined above, i.e. for text analysis.

b. Qualitative mathematical methods for solving the pairing problem

These *qualitative-mathematical* methods, as I call them, found their way into the explanative-prognostic or the Platonic-Galilean tradition because they also contribute to the uncovering of concrete causalities and causal mechanisms or causal processes:

> Since QCA [Qualitative Comparative Analysis] originated in the American context, it is not surprising that the underlying understanding of qualitative methods here belongs to

> the causal-inference-oriented direction (as formulated by Goertz and Mahoney 2012, p. 9)[127] (Wagemann 2015: 430; my translation).

It involves both causal inference or deduction and a causal explanation (not a description, as the English concept suggests) of the causal mechanism, although the English concept is "descriptive and causal inference". These four words are usually found together, and not infrequently in prominent places, as chapter titles or subheadings in various methodology books (King/Keohane/Verba 1994, Brady/Collier 2010 [2004], Box-Steffensmeier/Brady/Collier 2010a [2008]).

The American case study tradition and the quantitative approach were important to the emergence of this understanding of qualitative methods:

> This includes the use of formulas, graphs, algorithms, and also special mathematics (Schneider and Grofman 2006), which is why there are then recipe-like instructions for performing a QCA[128] (Wagemann 2015: 436; my translation).

Thus, QCA was propagated as:

> a third way [...] and thus as an alternative to prevailing qualitative and quantitative approaches[129] (Wagemann 2015: 429; my translation).

Here Wagemann might still use the word *qualitative* in the sense of *qualitative-interpretive*.

Due to the great importance of mathematical procedures, the attribute *qualitative-mathematical* for these methods is justified. An alternative to be considered, which would be much more cumbersome, is:

> qualitative methods of the causal-inference-oriented direction[130] (Wagemann 2015: 430; my translation).

Furthermore, in order to point out these nevertheless significant differences to the other qualitative-interpretative methods, one could use the concept *quasi-qualitative*, especially since in France a second Q is also used, which stands for:

> *Analyse Quali-Quantitative Comparée* (Wagemann 2015: 429).

However, this is likely to add to the confusion.

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[127] Da QCA im amerikanischen Kontext entstanden ist, ist es nicht verwunderlich, dass das hier zugrunde liegende Verständnis qualitativer Methoden der kausal-inferenz-orientierten Richtung (so die Formulierung bei Goertz und Mahoney 2012, S. 9) zuzuordnen ist (Wagemann 2015: 430).

[128] Dazu gehören die Verwendung von Formeln, Graphen, Algorithmen und auch einer speziellen Mathematik (Schneider und Grofman 2006), weswegen es dann auch rezeptartige Anleitungen zur Durchführung einer QCA gibt (Wagemann 2015: 436).

[129] dritter Weg [...] und damit als Alternative zu vorherrschenden qualitativen und quantitativen Ansätzen propagiert (Wagemann 2015: 429).

[130] qualitative Methoden der kausal-inferenz-orientierten Richtung (Wagemann 2015: 430).

c. Qualitative-mathematical methods for determining causal mechanisms or causal processes

Process tracing, in addition to QCA, is another qualitative-mathematical method:

> Process tracing is a research method for causal explanation in which multiple empirical observations within one or more cases are understood as potential implications of theoretical causal mechanisms. The most complete possible empirical reconstruction of causal processes through case studies allows conclusions to be drawn about (alternative) theoretical explanations[131] (Starke 2015: 454; my translation).

The focus is not on correlations but on causal processes:

> In contrast to many primarily case-comparative and/or quantitative methods of explanation, however, process analysis focuses not on the correlations between independent variables and the dependent variable, but on the causal mechanisms that link independent and dependent variable and that can be demonstrated within one or more cases[132] (Starke 2015: 453; my translation).

Andrew Bennett also presents process analysis as a powerful tool for distinguishing between correct and incorrect causal explanations. Since these, like all other qualitative-mathematical methods, are mainly used in single case studies and in studies with a small number of cases (small-N studies), i.e. on the micro level, it is the first time that the major goal is to solve the pairing problem. It is precisely this achievement that Bennett highlights in his conclusion, although he does not use the concept *pairing problem*.

> Yet with appropriate evidence, process tracing is a powerful means of discriminating among rival explanations of historical cases even when these explanations involve numerous variables (Bennett 2010 [2004]: 219).

Brady demonstrates the performance of *causal-process observations* (CPOs) versus *data-set observations* (DSOs) on a case. Also in this paper, an explanation made using data-set observations is refuted (Brady 2010 [2004]).

Thus, it is evident that the researchers who designed the qualitative-mathematical methods were guided by (alethic) *logic* as well as by *mathematics*, especially *set theory*, and by the methodology of existing quantitative methods and their scientific-theoretical assumptions (King/Keohane/Verba 1994, Brady/Collier 2010 [2004], Box-Steffensmeier/Brady/Collier 2010a [2008]). Therefore, I think that the term “logical-

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[131] Prozessanalyse (engl. *process tracing*) ist eine Untersuchungsmethode zur kausalen Erklärung, bei der vielfältige empirische Beobachtungen innerhalb eines oder mehrerer Fälle als potentielle Implikationen theoretischer Kausalmechanismen verstanden werden. Die möglichst vollständige empirische Rekonstruktion kausaler Prozesse durch Fallstudien erlaubt Schlussfolgerungen über (alternative) theoretische Erklärungen (Starke 2015: 454).

[132] Im Unterschied zu vielen in erster Linie fallvergleichenden und/oder quantitativen Methoden der Erklärung stehen bei der Prozessanalyse jedoch nicht die Korrelationen zwischen unabhängigen Variablen und abhängiger Variable, sondern die Kausalmechanismen im Zentrum, die unabhängige und abhängige Variable verbinden und die sich innerhalb eines oder mehrerer Fälle nachweisen lassen (Starke 2015: 453).

mathematical research methodology" is justified for both quantitative and qualitative-mathematical methods.

D. Data-set observations (DSOs) versus causal-process observations (CPOs)

However, there are also differences between quantitative and qualitative mathematical methods. Goertz and Mahoney have identified 25 differences (Goertz/Mahoney 2012).

One of the reasons given for the difference between quantitative and qualitative-mathematical methods is the distinction between different CPOs (causal-process observations) and DSOs (data-set observations):

> We define causal-process observation as an insight or piece of data that provides information about context, process, or mechanism, and that contributes distinctly to causal inference. A data-set observation (DSO), by contrast, is the standard quantitative data found in a rectangular data set (Brady/Collier/Seawright 2010 [2004]: 2).

Data-set observations continue to be defined in more detail like this:

> All the scores in a given row, in the framework of a **rectangular data set**. It is thus the collection of scores for a given case on the dependent variable *and* all the independent variables. This includes **intervening** and **antecedent** variables (emphasis in original; Brady/Collier 2010 [2004]: 324).

The DSOs form the basis for quantitative correlation and regression analyses, while CPOs form the basis for qualitative-mathematical analyses using e.g., QCA or process tracing:

> DSOs are the basis for the standard rectangular data set of the quantitative researcher, with rows corresponding to cases and columns corresponding to variables. This data set is the foundation for correlation and regression analysis. In relation to this rectangular data set, the term "observation" has a very specific meaning. It is not the ordinary language meaning, in the sense that one "observes" phenomena in the real world. Rather, an observation is specifically an entire row in the rectangular data set. It is all the score for a given case. A CPO, by contrast, is an insight or piece of data that provides information about context, process, or mechanism and that contributes distinctive leverage to causal inference. It is not part of a rectangular data set; it provides a separate type of inferential leverage. Our goal in selecting this label is to incorporate the term "observation", which as just noted has a special status in relation to causal inference in quantitative research, and to juxtapose it with the idea of causal process (Brady/Collier/Seawright 2006: 355).

Furthermore, there is a fundamental difference between statistical and causal inferences, which therefore require different approaches as well as different forms of observation:

> In statistical inference, one typically uses information obtained from a limited number of observations – usually based on a random sample – to draw conclusion about the likely value of some *parameter* in the population at large such as regression coefficient or a standard deviation. In causal inference, as the term is used here, the information being used is not necessarily confined to a specific sample, but a range of different sources of information provide various pieces of the causal puzzle (see Brady and Collier 2004 and their notion of "causal process observation"). Furthermore, the entity one seeks to generalize about is not the parameter of a statistical model but the process by which something has been brought about and the *mechanism* governing this process. [...]

> The type of “mechanism approach” discussed in this chapter also differs in another important respect from more traditional quantitative approaches. The focus is not on relationships between variables, but on actors, their relationships, and the intended and unintended outcomes of their actions. Properties of actors and/or their social environments often influence the outcomes of individuals’ actions. These properties as well as the action outcomes can be measured and represented in the form of variables, but the causality does not operate at the variable level (Hedström 2010 [2008]: 320).

Large-N studies, which are generated with the help of *quantitative*-mathematical tools, generate primarily causal, probabilistic regularities. Generalizations are necessary here as well, because it is usually not even possible to capture all past cases, let alone future ones. Case studies and small-N studies use *qualitative*-mathematical methods to explore concrete, causal cause-and-effect mechanisms. When both approaches are combined, we speak of *multi-method research* or *mixed-methods research* (Wolf 2015: 491):

> In general, in recent years a mixed-method approach has been more and more frequently used in order to benefit both from the advantages of quantitative and qualitative approaches [here, too, qualitative-mathematical and not qualitative-interpretative approaches are meant], i.e. an overall view of the phenomena on the one side and in-depth insights on the other (Biesenbender and Héritier 2014; Caporaso 2009). Quantitative analysis is apt to ensure the generability of the results, qualitative case studies of the processes might help to identify the causal mechanism at work for a subset of the units of analysis (Biesenbender and Héritier 2014; Caporaso 2009) (Héritier 2016: 24).

The preference for laws or regularities and alleged context-free knowledge is at the heart of the perestroikans’ critique, as explained above. Here is another quote that gets to the heart of this criticism:

> They [area scholars] resisted the practice of subsuming the particular *sub specie aeternitatis*, or treating local thought and practice as instances of some abstract universal (Rudolph 2005a: 11).

The introduction of qualitative-mathematical research methodology to political science has meant that researchers in the explanative-prognostic or the Platonic-Galilean tradition can no longer be accused of this. The fact that it was not possible to solve the pairing problem within the regulative approach, as described above, or to find the answer to the question of how the causal mechanism works exactly, led to the development of qualitative mathematical methods, in which the precise context is analyzed using case studies.

In short, that is precisely the procedure and objective that the perestroikans (Flyvbjerg/Landman/Schram 2012a) claim is missing in the scientistic “mainstream”.

E. Complementarity and triangulation between quantitative, qualitative-mathematical and qualitative-interpretative methods

Mark Bevir also makes critical reference to the importance of supplementing the causal research perspective in his contribution “Meta-methodology: Clearing the Underbrush” within the Oxford series:

> Why should political scientists worry about the shift toward contextual and historical forms of explanation? In stark terms, the answer is that it implies that their correlations, classifications, and models are not properly speaking explanations at all. They are, rather, a type of data that we then might go on to explain using contextualizing historical narratives. Correlations and classifications become explanations only if we unpack them as shorthands for narratives about how certain beliefs fit with other beliefs in a way that makes possible particular actions and practices. Similarly, although models appeal to beliefs and desires, they are mere fables that become explanations only when we treat them as accurate depictions of the beliefs and desires that people really held in a particular case (cf. Rubinstein 2006; 1995) (Bevir 2010 [2008]: 67).

Just as explanations and descriptions can complement each other, different methods can also be used in a complementary manner. The magic word here is triangulation or *method-linking research* (*methodenverbindende Forschung*, Wolf 2015). Triangulation is about an approach that takes place primarily at the method level in a narrower sense. It shows how desirable complements of epistemological, methodological and ontological perspectives can be implemented in concrete terms.

Triangulation is used both for cumulative validation of findings and to complement perspectives:

> Triangulation as a cumulative validation of research findings and triangulation as a complement of perspectives that allow for a more comprehensive coverage, description, and explanation of a subject area, with recent literature emphasizing the aspect of complementarity, that is, the complement of perspectives over the aspect of validation[133] (Kelle/Erzberger 2015 [2000]: 303-304; my translation).

It is important to note that Kelle and Erzberger have primarily qualitative research in mind here, of the type carried out in particular by qualitative-interpretative researchers.

The methods are complementary. But this is not true for the results generated by these methods, because results can converge, be complementary or contradict each other:

> A uniform concept of method integration that assigns a certain research-logical or theoretical status a priori to qualitative and quantitative research results – for instance, in the sense that qualitative and quantitative results must in principle complement each other – can thus not be derived from these different functions and uses of method integration.

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[133] Triangulation als kumulative Validierung von Forschungsergebnissen und Triangulation als Ergänzung von Perspektiven, die eine umfassendere Erfassung, Beschreibung und Erklärung eines Gegenstandsbereichs ermöglichen, wobei in der neueren Literatur der Aspekt der Komplementarität, das heißt der Ergänzung von Perspektiven gegenüber dem Aspekt der Validierung hervorgehoben wird. (Kelle/Erzberger 2015 [2000]: 303-304).

> Results of qualitative and quantitative studies can converge, be complementary, or contradict each other, and each of these possibilities can be fruitful for the research process. [...]
>
> The right “mix of methods”, however, always depends on the nature of the subject area under investigation and the theoretical concepts used[134] (Kelle/Erzberger 2015 [2000]: 308; my translation).

This is the state of affairs in the Aristotelian or descriptive-interpretative tradition. What is the state of affairs in the explanative-prognostic or the Platonic-Galilean tradition?

If a pluralistic methodology is envisioned within the explanatory-prognostic tradition (see “Toward a Pluralistic Vision of Methodology”, Brady/Collier/Seawright 2006 and “Rethinking Social Inquiry. Diverse Tools, Shared Standards”, Brady/Collier 2010 [2004]), this is a plea for a *diversity of methods for determining causalities*, specifically for supplementing quantitative methods with qualitative-mathematical methods, and by no means a plea for a comprehensive, pluralistic methodology, since, for example, causal reductionism is not called into question. The idea is a triangulation between quantitative methods to determine correlations on the macro level and qualitative-mathematical methods to determine causal mechanisms on the micro level on the basis of case studies.

In addition to the concept of triangulation, the concepts *mixed-method* and *multimethod* are used, especially in English-language contributions. It is important to note that confusion can arise due to different research traditions. Triangulation can indicate four different combinations:

a. Triangulation of different qualitative-interpretative methods or “triangulation in qualitative research” (Flick 2015 [2000]), Kelle/Erzberger 2015 [2000]).
b. Triangulation of quantitative and qualitative-interpretative methods (Schubert/Bandelow 2009, Monroe 2015).
c. Triangulation of quantitative and qualitative-mathematical methods or “Integrating Qualitative and Quantitative Methods” (Fearon/Laitin 2011 [2009]; Wolf 2015 and Hérretier 2016 also argue along these lines).

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[134] Ein einheitliches Konzept der Methodenintegration, das qualitativen und quantitativen Forschungsergebnissen einen bestimmten forschungslogischen oder theoretischen Status a priori zuweist – etwa in dem Sinn, dass sich qualitative und quantitative Ergebnisse grundsätzlich ergänzen müssten –, lässt sich aus diesen verschiedenen Funktionen und Verwendungsweisen von Methodenintegration also nicht ableiten. Ergebnisse von qualitativen und quantitativen Studien können konvergieren, komplementär sein oder sich gegenseitig widersprechen, wobei jede dieser Möglichkeiten für den Forschungsprozess fruchtbar sein kann [...]
Der richtige ‚Methodenmix‘ ist aber stets abhängig von der Art des untersuchten Gegenstandsbereichs und den verwendeten theoretischen Konzepten (Kelle/Erzberger 2015 [2000]: 308).

d. Triangulation of different data sources (King/Keohane/Verba 1995), triangu–lation of DSOs and CPOs (Brady/Collier 2010 [2004]).

a. Triangulation of different qualitative-interpretative methods

Within qualitative research, triangulation means the use of different qualitative-interpretative methods; it is recommended, for example, as a validation strategy (Flick 2015 [2000]).

b. Triangulation of quantitative and qualitative-interpretative methods

In many German methodology books, quantitative and qualitative methods (qualitative-interpretative methods are always meant here) are presented in different chapters on an equal footing and complementarily, and all methods are recommended to students depending on the problem they search (Schubert/Bandelow 2009). Triangulation is made between quantitative and qualitative interpretative methods.

Kristen Renwick Monroe praises the perestroikans for having contributed to a similar methodological opening in the U.S.A. as in Europe, so that young scientists can increasingly use quantitative and qualitative (meaning qualitative-interpretive) methods:

> Portman’s thesis utilized multiple methodologies – interviews, surveys, content analysis of speeches and public documents – to reveal the psychology of activists involved in politics in the United States, including their belief systems, personality traits and senses of individual and collective identity (Monroe 2015: 423).

c. Triangulation of quantitative and qualitative-mathematical methods

Although the volume “Political Methodology” (Box-Steffensmeier/Brady/Collier 2010a [2008]) does not contain the word “triangulation”, the topic of method-linking research is discussed in the article “Integrating Qualitative and Quantitative Methods” by James D. Fearon and David D. Laitin (2011 [2009]). They are also of the opinion

> that qualitative work might be integrated into a research program as a complement to rather than as a rival or substitute for quantitative analysis (Fearon/Laitin 2011 [2009]: 775).

What is important, however, is that by “qualitative” Fearon and Laitin do not mean the qualitative-interpretative, but the qualitative-mathematical methods.

d. Triangulation of different data sources for causal studies

King, Keohane, and Verba understand triangulation to be the combination not of different logics or methods, but of different data or data sources with the goal of generating as much data as possible on causal theories or hypotheses:

> Triangulation involves data collected at different places, sources, times, levels of analysis, or perspectives, data that might be quantitative, or might involve intensive interviews or thick historical description (King/Keohane/Verba 1995: 479). [...]

> Triangulation, then, is another word for referring to the practice of increasing the amount of information to bear on a theory or hypothesis, and that is what our books is about (King/Keohane /Verba 1995: 480).

Also, in order to improve causal analyses, Brady and Collier in particular advocate the addition of *causal-process observations* (CPOs) to *data-set observations* (DSOs) (Brady/Collier 2010 [2004]).

F. Schism between quantitative and qualitative-interpretative methods and associated misunderstandings

The schism between quantitative and qualitative methods is considered by both sides to have been overcome (for the establishment Goodin 2011b [2009]), for the perestroikans Monroe 2015, for the interpretivists Bevir and Rhodes):

> In short, the quantitative/qualitative distinction is at best unhelpful and at worst meaningless (Bevir/Rhodes 2016b: 19; see also Moses/Knutsen 2019 [2007]).

Kristen Renwick Monroe argues that the perestroikans helped to overcome the schism and that at least young researchers are using both without prejudice: she claims in her review “What did Perestroika Accomplish?” (Monroe 2015) that the perestroika movement has achieved a great deal. Above all, the gap between disciplined political scientists and undisciplined political theorists has been closed, not least thanks to the overcoming of the schism between quantitative and qualitative-interpretative researchers:

> Its most critical triumphs were its successful challenge to the view that there was only one way to do political science and its legitimation of the view that political science was a humanistic as well as a scientific discipline (Monroe 2015: 423).

This is noticeable on the methodological level in the overcoming of the schism between quantitative and qualitative-interpretative methods and through a

> shift in the discipline’s attitude toward methodological pluralism (Monroe 2015: 423).

How does Monroe justify this? First, she refers to one of her doctoral students, Bridgette Portman, who in her work drew on all the methodologies available in the methodological toolkit to solve her problem:

> But perestroika shifted the battle, with more scholars, especially younger ones, now realizing there are many ways to examine a political issue and that good work will address a political problem using all the appropriate instruments available in the methodological kit (Monroe 2015: 423).

Scholars belonging to the explanative-prognostic or the Platonic-Galilean tradition also believe that they have overcome this schism and consider this criticism misplaced (Goodin 2011a [2009]), mainly because on the one hand they maintain a pluralistic habitus, and on the other hand with reference to the “qualitative-mathematical” methods established since the 1970s. This is not true, for in the volume “Political Methodology” (Box-Steffensmeier/Brady/Collier 2010a [2008]) just as, for example, in the “Handbook of Policy Research” (Wenzelburger/Zohlnhöfer 2015), no qualitative-interpretative methods (e.g., qualitative content analysis) are discussed, but only qualitative-mathematical methods such as QCA (Wagemann 2015) and process tracing (Starke 2015). A focus in process tracing is causal processes (Hedström 2010 [2008]); in QCA, it is solving the pairing problem at the micro level.

The scientistic scientists have contributed to an enormous confusion of concepts, because they have given new meanings to firmly established concepts like “qualitative” and “descriptive”. However, *only the conceptualization is worthy of criticism, not the methodological innovations achieved with it.* The qualitative-mathematical research methodology developed since the 1970s is a necessary complement to quantitative and experimental methods, because the qualitative-mathematical research methodology makes it possible to determine the *ontic* property of causality as well as to solve the pairing problem.

However, as shown above the schism, in my opinion, still remains. The confusing terminology alone simulates progress.

6.9.2 Experiments or experimental methods as well as simulations

Identifying causality is a very difficult and complex task (Brady 2011 [2009], section 4.2.2). The regulative approach alone is not enough. For the counterfactual and manipulative approach, experiments and simulations are required in addition to models; this is the only way to satisfy the criteria of objectivity, reliability, and intersubjectivity. Therefore, it is not surprising that with the rise of causal thinking within political science, experimentation also entered the discipline. Simulations, on the other hand, have, at least so far, played a role mainly in sociology (Braun/Saam 2015):

> Computer technology has also led to a greater ability to engage in survey experiments, and to deal with the statistical and other methodological issues that are sometimes involved in field and natural experiments. Technology has transformed political science into an experimental discipline (Morton/Williams 2010 [2008]: 340; see Gerber/Green 2011 [2009] and Kanitsar/Kittel 2015; the latter speak of experimental methods).

Even if this judgment does not apply to political science as a whole, experiments have become decisively more important, at least within the explanative-prognostic or the Platonic-Galilean tradition, so that although one cannot speak of a revolution, because the other research programs have not been displaced and are still being applied, one can at least justifiably speak of the introduction of an *experimental research methodology or program* within the explanative-prognostic or the Platonic-Galilean tradition, at the center of which are experiments or experimental methods and simulations.

Talk of a transformation of political science as a whole into an “experimental discipline” (Morton/Williams 2010 [2008]: 340) is an exaggeration; these authors also seem to have revolutionary metaphors in mind; however, a significant further development of causal thinking occurred by means of establishing experiments within political science. From 1950 to 2005, although there was a dramatic increase in experimental articles in APSR, AJPS, and JOP, experiments only played a role in causal analyses.

The establishment of experiments within political science is relatively new, but not so in other sciences. Already in the 17th century, Francis Bacon pointed out the importance of experiments for theory building, shortly after Galileo Galilei had conducted the first experiments. Even if there are limitations to experiments in principle, both experiments and simulations are indispensable for causal analyses, as they are especially irreplaceable for the determination of causalities in the counterfactual and manipulative approach.

What I call the *experimental research program* or *experimental research methodology* will continue to have a rightful say in the development of the subject in the future. Rebecca B. Morton and Kenneth C. Williams see experimentation on the rise for two reasons, the *internet* and *cognitive science*:

> First, the expansion of interactive Web-based experimental methods will allow researchers to conduct large-scale experiments testing game-theoretic models that have previously only been contemplated. […]
> Second, advances in brain-imaging technology will allow political scientists to explore much more deeply the connections between cognitive processes and political decisions. These two types of experiments will join with traditional laboratory and field experiments to transform political science into a discipline where experimental work will one day be as prevalent as traditional observational analyses (Morton/Williams 2010 [2008]: 354).

The perestroikans address neither experiments nor simulations in detail; the criticism of the establishment or of the "*political scientists*" remains on a very general level (keywords *method-driven*, *scholasticism* etc.). A specialized science is almost dependent on the introduction of new scientific tools. Of course, it is important to take into account both the possibilities and the limitations of these tools. Here, too, Kuhn's terminology is misleading; there is neither a new paradigm nor has political science immediately become an "experimental discipline" due to revolutionary processes (Morton/Williams 2010 [2008]: 340). There is no *general*, but only a *methodological incommensurability* between experiments and other scientific tools.

Firstly, experimental methods can be used both for a cumulative validation of results and to supplement perspectives. Second, the introduction of an *experimental research program* within the Plato-Galilean tradition has expanded the scientific toolkit – no more, no less. These developments are by no means insignificant, especially given the new technologies in the information age.

6.9.3 Experiments versus tests

As a rule, empirical scientists do not distinguish between *experiments* and tests. Within the philosophy of technology, however, the fundamental difference between these has been worked out. Experiments matter only in empirical sciences, but not in practical sciences where tests are important. In the experiment, the *validation of a theory* is checked, but not the *fulfillment of a function*. The latter is the aim of tests. In experiments, theories and thus systems of statements are checked, in tests rules are tested for their validity in practice:

> In an experiment, a theory or a regularity is examined to determine whether it proves to be correct to a certain degree or what the probability is that it applies. Here the focus is always primarily on the possibilities of generalization [...].
>
> When testing a rule, on the other hand, a component, an assembly or an entire system is checked for the fulfillment of functions that were previously assumed as a function of the assumed boundary and initial conditions[135] (Kornwachs 2013: 92; my translation).

These fundamental differences between experiments and tests contain important arguments against methodological *reductionism*. Both within an applied methodology (social technology of the scientistic scientists) and a problem-oriented methodology (applied phronesis of the perestroikans), an inversion of causalities is assumed or considered unproblematic.

6.10 Tenth level: methodological approaches

There is a wealth of methodological approaches within political science (von Beyme 2000 [1972]: 87-178); the following is my classification:

- Quantitative methodical approaches:
  - behaviorist approach
  - rational choice approach
  - quantitative-comparative approach
- Qualitative-linguistic or qualitative-interpretative methodical approach:
  - institutional approach
- Both quantitative and qualitative-linguistic methodical approaches:
  - functionalist approach
  - historical approach
  - comparative approach

At the heart of the “Political Methodology” handbook (Box-Steffensmeier/Brady/Collier 2010a [2008]), as described above, is causal and empirical reductionism explored through a logical-mathematical research methodology. Model thinking, specifically rational choice theory or the rational choice approach, plays a role in this

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[135] Im Experiment wird eine Theorie oder eine Regelmäßigkeit daraufhin untersucht, ob sie sich zu einem bestimmten Grad bewährt bzw. mit welcher Wahrscheinlichkeit sie zutrifft. Hier wird der Blick immer vorrangig auf die Möglichkeiten einer Verallgemeinerung gerichtet [...].
Beim Test einer Regel werden hingegen ein Bauteil, ein Zusammenbau oder eine ganze Anlage auf die Erfüllung von Funktionen überprüft, die vorher in Abhängigkeit von angenommenen Rand- und Anfangsbedingungen vermutet worden sind (Kornwachs 2013: 92).

methodology volume primarily in the contribution “Normative Methodology” by Russel Hardin (2011 [2009]). This is the only paper that discusses normative methodologies, so I have to deal with it in more detail because of the focus on practical methodology.

Since the so-called rational choice revolution (Goodin 2011b [2009]: 13) within political science at the beginning of the 1970s, the rational choice approach has dominated or at least assumed a prominent role in American political science. While model thinking has also spread within sociology, in contrast to economics and political science, it is not limited to the rational choice approach (Braun/Saam 2015). A tendency towards reductionism is thus also evident at this level of methodical approaches within political science and in economics, on which political science is strongly oriented.

The alleged triumph of the rational choice approach between 1983 and 1993 in the U.S.A. was described by von Beyme based on the statements of the presidents of the APSA as follows:

> The rise of rational choice is nowhere more evident than in the “state of the art of the discipline” compendiums produced by the American Political Science Association in 1983 and 1993. In 1983 political theory was treated in two chapters as “empirical” and “normative political theory” (Gunnell 1983). In addition, the pioneer of the Rational Choice movement William Riker (1983) was allowed to ride his hobbyhorse “coalition games” under a special title. Ten years later, “formal rational choice” was presented as the winning movement (Laman et al. 1993: 77). […]
>
> Nothing had come between “rational choice” and normativism. Empirical political theory outside of the rational choice approach seemed to have disappeared[136] (von Beyme 2000 [1972]: 142; my translation).

Russel Hardin (2011 [2009]) goes one step further and also excludes normativism, because according to him the rational choice approach is not only sufficient for recognition of the world, but a *normative rational choice theory* also enables the normative assessment and practical change of political reality.

That was wishful thinking then (1993), and still is, by the supporters of rational choice, and it is probably due to the ubiquitous revolution metaphor. Every new paradigm will erase everything else and represents the pinnacle of progress. In revolution mode,

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[136] Der Aufstieg von Rational Choice wird nirgends deutlicher als in den Kompendien zum ‚state of the art of the discipline‘, welche die American Political Science Association 1983 und 1993 vorlegte. 1983 wurde die politische Theorie in zwei Kapiteln als ‚empirische‘ und ‚normative politische Theorie‘ abgehandelt (Gunnell 1983). Daneben durfte der Pionier der Rational Choice-Bewegung William Riker (1983) unter einem Spezialtitel sein Steckenpferd ‚Koalitionsspiele‘ reiten. Zehn Jahre später wurde ‚formal rational choice‘ als siegreiche Bewegung dargestellt (Laman u.a. 1993: 77). […]
Zwischen ‚Rational Choice‘ und Normativismus war das Nichts getreten. Empirische politische Theorie außerhalb des Rational Choice-Ansatzes schien es nicht mehr zu geben (von Beyme 2000 [1972]: 142).

ASPA presidents also obviously disregard endogenous values and commit technical errors that they would seldom make as “normal” scientists. But the reality was different. In 1993, Green and Shapiro counted 41 articles in the APSR (American Political Science Review), of which 15 were rational choice articles, which is just 36.6%. In 1983 the proportion was around 21% (Green/Shapiro 1994: 3). An increase in the spread of this methodology can be undeniably proven, but just like the other two, this “revolution” had not even reached the majority of political scientists, yet claimed that the mainstream implements exactly this methodology.

In the volume “Political Methodology” (Box-Steffensmeier/Brady/Collier 2010a [2008]), the rational choice approach plays an important role in addition to the four approaches to determining causality that were presented in detail above (section 4.2.2). In my opinion, model thinking, discussed here paradigmatically using the example of the rational choice approach, forms an innovative, methodological research program within the explanative-prognostic or the explanative-prognostic or the Platonic-Galilean tradition alongside causal thinking, quantitative, qualitative-mathematical and experimental research programs. In no way is this associated with a revolution that displaces other research programs within political science; it is rather an independent and efficient research program introduced from economics.

6.10.1 Rational choice approach and game theory

The rational choice approach was developed in economics starting in the 1950s and subsequently found its way into political science. In the following, the goals pursued by the rational choice approach and game theory will be explained. First, a necessary distinction between theory and methodology will be justified.

A. Rational choice: methodology or theory

The concepts “rational choice *approach*” and “rational choice *theory*” are often used synonymously. This leads to some misunderstandings and problems, as can also be seen in Hardin’s (2011 [2009]) contribution. I find it necessary to always distinguish between *methodology*, formal knowledge or tools (in a very broad sense) on the one hand, and *content* knowledge or theories on the other, even though Hardin says:

> In this program, method and theory tend to merge (Hardin 2011 [2009]: 99).

The rational choice approach would then be a methodological tool that can be used to generate empirical or practical knowledge. Rational choice theories would be analogous to the causality theories of science that tell us how we can rationally determine knowledge or what elements the rational choice approach should have.

Rational choice approaches could then be used to generate *causal statements* in counterfactual models (Levy 2010 [2008]: 630 and 637). The rational choice approach could also be used in the form of game theory to generate technical knowledge and thus *technical regulations*. That there are principal differences between causal statements and technical regulations has been addressed above at several methodological levels (sections 6.4 Conceptual level, 6.5 Sentence level, and 6.7 Logic level).

If, as Hardin believes, method and theory coincide, one has the problem that it is difficult or impossible to distinguish between empirical or normative assumptions on the one hand and the results or the empirical or normative knowledge that this theory offers on the other. The problems involved are presented in the next subsection.

B. Rational choice approach

The rational choice approach was developed with the *aim* of designing a comprehensive explanation for social and political behavior. The claim is to formulate “lawlike claims about measurable phenomena” as well as

> developing theories that can explain and predict observed patterns or behavior and practice (Ferejohn 1991, 280, quoted in Green/Shapiro 1994: 10).

While systems theory and (neo-) Marxism offer explanations with a methodological *holism* from the perspective of the whole, models of rational choice behavior (rational choice models) try to generate explanations by means of *methodological individualism*, specifically the so-called *Coleman’s bathtub* or boat (Coleman 1990, chart 5, section 9.4.5), to generate explanations from the actor perspective, where actors can be not only individuals acting individually, but also collectives (organizations, classes, or the state) (Braun 1999: 17-53, Schluchter 2006 and 2007, Schwinn 2013b [1993a] and 2013c [1993b]).

The rational choice approach has undergone various changes in the course of its development, so that Green and Shapiro distinguish between undisputed and disputed assumptions. They identify the following *undisputed* assumptions:

a. The *utility maximization* of any order of preferences is a central aspect of rational action, with no particular goal in the front.

b. *Rationality* must satisfy certain *consistency requirements*:

> First, it must be possible for all of an agent’s available options to be rank-ordered. This is sometimes called the assumptions of connectedness (Green/Shapiro 1994: 14). Rational choice theorists also assume that preference orderings are transitive. If A is preferred to B, and B is preferred to C, then this consistency rule requires that A be preferred to C (Green/Shapiro 1994: 14-15).

c. *Expected value*:

> Third, rational choice theorists routinely assume that each individual maximizes the expected value of his own payoff, measured by some utility (Luce und Raiffa 1957, 50). The focus on expected rather than actual utility is required because decision making often takes place under conditions of uncertainty (Green/Shapiro 1994: 15).

d. *Methodological individualism*:

> Rational maximizing agents are individuals (Green/Shapiro 1994: 15).

e. *Stable expectations*:
   Finally, rational choice theorists generally assume that their models apply equally to all persons under study – that decisions, rules, and tastes are "stable over time and similar among people" (Stigler und Becker 1977, 76, Green/Shapiro 1994: 17).

According to Green and Shapiro, there are two *contentious assumptions* about human goals, however:

a. *Thin-rational account*: It is assumed that actors are rational only in the sense that they use the means available to them efficiently in pursuit of their goals.
b. *Thick-rational account*: The researcher applies not only rationality but also some additional description of the actor's preferences and beliefs. The second contentious assumption concerns whether the actor has perfect or imperfect information.

C. Explanations in the social sciences and the micro-macro problem

The properties of causal explanations have been discussed above independently of an object. What's more, due to the orientation towards the natural sciences, examples from these, especially physics and microbiology, were in the front. Now causal explanations are in the focus, which above all explain *social action on the basis of individual action*. Within the explanative-prognostic or the Platonic-Galilean tradition, the rational-choice approach deserves the credit for bringing *actors* and thus *people* back into the focus of research:

> "Bringing men back" was a merit of the rational choice approach[137] (von Beyme 2000 [1972]: 145; my translation).

Many epistemological considerations suggest a *hiatus* or a *fundamental gap* between the *macro* and *micro* level, since formally there can be neither a *valid induction* from the micro to the macro level nor, conversely, a formally *valid deduction* from the macro to the micro level, since causal regularities are probabilistic in nature and even with regard to language rules, exceptions tend to confirm the rule rather than refute it. This applies not only to empirical knowledge (empirical theories), but also to practical knowledge or practical theories. Norms and regulations have a prima facie property (Ross 1967 [1930]), i.e. that derivation schemes do not help even within a practical methodology (6th and 7th chart, section 9.4.6 and 9.4.7).

Rational choice theorists in particular try to overcome the micro-macro hiatus with the help of Coleman's bathtub (Coleman 1990: 1-23, 5th chart, 9.4.5). Coleman diagnoses a gap between social theory on the one hand and empirical research on the other:

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[137] ‚Bringing men back' war ein Verdienst des Rational Choice-Ansatzes (von Beyme 2000 [1972]: 145).

> Social theory continues to be about the functioning of social systems of behavior, but empirical research is often concerned with explaining individual behavior (Coleman 1990: 1).

The goal of the explanatory mode sought by Coleman is to explain social behavior systems using individual behavior. This is necessary because:

> The principal task of the social sciences lies in the explanation of social phenomena, not the behavior of single individuals (Coleman 1990: 2).

Furthermore, he argues for a methodological individualism that would offer a more fundamental explanation and would have to be located below the system level:

> Later I will suggest that a natural shopping point for the social sciences (although not psychology) is the level of the individual – and that, although an explanation which explains the behavior of a social system by the actions and orientations of some entities between the system level and the individual level may be adequate for the purpose at hand, a more fundamental explanation based on the actions and orientations of individuals is more satisfactory (Coleman 1990: 4).

Coleman explicitly draws on Weber's teleological theory of action:

> The individual-level theory of action I will use in this book is the same purposive theory of action used in Weber's study of Protestantism and capitalism (Coleman 1990: 13).

This actor-centered approach is undertaken not only for empirical reasons but also, which is central to my investigation, for practical reasons. The aim is to include the level from which *changes* in the social system are possible:

> Just as observations are often most naturally made at levels below that of the system as a whole, interventions must be implemented at these lower levels. Thus a successful explanation of system behavior in terms of the actions or orientations of lower-level units is ordinarily more useful for intervention than is an equally successful explanation of which remains at the system itself (Coleman 1990: 3).

Furthermore, he says:

> an explanation is sufficiently fundamental for the purpose at hand if it provides a basis for knowledgeable intervention which can change system behavior (Coleman 1990: 4).

If one stays only on the macro level, one can generate naturalistic explanations, but one can neither explain the causal process behind them nor say how humans could change the world due to these mechanisms. Empirical researchers had come to these conclusions because of the development of causal theories, as I have explained in detail above using Brady's contribution as an example. Coleman comes to the same conclusion for quite different reasons. In naturalistic causal analyses, not only do humans as actors disappear, but explanations also have a deterministic character. Interventions of actors are not foreseen; thus developments can only be understood as *fate*:

> In theories of this sort the proposed causes of action are not a person's goals or purposes or intents, but some forces outside them or unconscious impulses within them. As a consequence, these theories can do nothing other than describe an inexorable fate; they are useful only to describe the waves of change that wash over us. At the mercy of these

> uncontrolled external or internal forces, persons are unable to purposefully shape their destiny (Coleman 1990: 17).

The rational choice approach and specifically the schema developed by Coleman (Coleman's bathtub) are intended to overcome precisely this shortcoming in the social sciences and thus include an individual theory of action in causal explanations.

The double objective known since Bacon is also visible in the rational choice approach: world recognition and world change. Coleman not only wants to explain social behavior, but hopes that these explanations are also suitable for interventions in the social system. He is silent about which epistemological mechanism paves the way from explanation to intervention. The insights and suggestions of Francis Bacon, Maximilian Carl Emil Weber, Karl Raimund Popper, and Hans Albert discussed above indicate the broad direction, but they were not even acknowledged, let alone elaborated, by Coleman in his work. It is also regrettable that Russel Hardin (2011 [2009]) does not explain this process. However, a normative theory of rational choice must necessarily do this.

What do the actor-centered explanations look like in detail? Coleman wants to determine explanations at the system level via the detour of the individual level. This is demonstrated, among other things, using the example of Weber's theses on the importance of Protestant ethics. In the following presentation (chart 5, section 9.4.5) I will also use the methodological approaches to determine causalities that Brady developed (Brady 2011 [2009]). This seems to me to be necessary because rational choice theorists, like interpretivists and in contrast to empirical researchers (Brady 2011 [2009]), always speak *en passant* of explanations, causes and effects, but these complicated concepts remain with them in mental semi-obscurity.

With the help of the regulative methodological approach, namely correlation and regression analysis, one can first of all establish a correlation between the doctrine of the Protestant religion (in the 5th chart, section 9.4.5, collective characteristic (a)) and capitalism (aggregate characteristic (c)).

Within the counterfactual approach, model analyses and experiments can be used to establish that the correlation is not just *random*, but *valid*.

Experiments in the manipulative approach make it possible to demonstrate that the collective feature (a) precedes the aggregate feature (c) in time. At the end of the investigations using these methodological approaches, it is clear that there is a *causal regularity* at the macro level between the *doctrine of Protestant religion* (cause, in the 5th chart, section 9.4.5, collective feature (a)) and *capitalism* (effect, aggregate feature (c)). This would justify the *collective hypothesis* on the macro level. From the collective *hypothesis* one gets to the *causal regularity* that the Protestant religion promotes capitalism.

Now, like all theorists who prefer an individual or *actor-centered theory of action*, Coleman believes that the only way to understand the *causal mechanism* or process, in this case how the Protestant religion promotes capitalism, is to take a detour through the micro level or the individual level.

Coleman describes the path from the macro to the micro level, via the teleological individual explanation and from the micro back to the macro level, as follows:

The *context hypothesis* (A) is as follows:

> 1. Protestant religious doctrine generates certain values in its adherents (Coleman 1990: 8).

According to Coleman, the *individual hypothesis* is as follows:

> 2. Individuals with certain values (referred to in proposition 1) adopt certain kinds of orientation to economic behavior. (The central orientations to economic behavior are characterized by Weber as antitraditionalism and duty to one's calling) (Coleman 1990: 8).

The *aggregate characteristic* is summarized as follows:

> 3. Certain orientations to economic behavior (referred in proposition 2) on the part of individuals help bring about capitalist economic organization in a society (Coleman 1990: 8).

The *context hypothesis* (A) is generated from the *logic of the situation*: the assumptions made here model the relationship between the situation and the actor. Coleman refers to these as *rules of the game* that ensure the transition from the macro to the micro level.

The *logic of selection* enables the individual hypothesis – in which the rules and preferences are listed – on the basis of which individuals make their choice of action. Behind this lies a *teleological theory of action* on the individual level.

The *logic of aggregation* consists of transformation rules based on which the collective *explanandum* or aggregate characteristic is derived. Coleman describes these as *rules of the game* that secure the transition from the micro to the macro level (Coleman (1990: 19, Braun 1999).

Thus, one could successfully explain the causal process at the micro level. In this case, one could show how Protestant religion changes economic behavior via its values, which then ultimately contributes to the promotion of capitalism. With this, I would have explained the argumentation of the rational choice approach.

Coleman believes that Weber failed to provide empirical evidence, but what is important in this context is only the methodological approach and not whether it was properly applied in a specific study.

Another science-theoretical distinction is very important. Only within the individual hypothesis are final causes worked with – when within an individual theory of action, the teleological orientation of actors is concerned. The aggregate feature in this case is an effect or an *immediate effective cause*. Coleman speaks in the original of *final cause* at the micro level and of the aggregate feature at the macro level being an *efficient cause*:

> When the actions treated as purposive are actions of individuals, however, and the action to be explained is the behavior of a social system, behavior which derives only very indirectly from the actions of the individuals, then the explanation of system behavior is not in terms of final causes but in terms of efficient causes (Coleman 1990: 16).

This is especially relevant because teleological explanations would not have to be considered causal explanations that could operate at the macro level.

D. Game theory and normative rational choice approach

The *collective dilemma* consists in the central question of

> reconciling individual egoism and collective welfare[138] (Braun 1999: 19; my translation).

Game theory can be used to devise various strategies to solve existing social or political dilemmas.

> Game theory as a branch of this approach [*rational choice approach*] focuses primarily on constellations in which players and opponents must make strategic choices in order to maximize their utility[139] (von Beyme 2000 [1972]: 141; my translation).

How this is done is not relevant for this study (for a detailed account, see Braun 1999). The only point to be made here is that game theory relies on normative assumptions that cannot be justified by game-theoretic means. Thus, it shall be demonstrated that game theory is suitable for the creation of means or technical solutions, but that the goals and purposes associated with it have to be justified and thus legitimized elsewhere. This is mainly due to the fact that game theory relies on empirical and normative presuppositions that are not further analyzed. It is precisely this problem that will now be critically examined.

6.10.2 Criticism of the rational choice approach and the normative rational choice theory

In the following, I first address the empirical and the normative (liberal and utilitarian) assumptions of the normative theory of rational choice (section A). Second, I discuss the critique of rational choice as a methodological approach. The focus here is on the rationalistic critique of model thinking (section B). Furthermore, the lack of empirical foundation is explained (section C). Then I explain the basic limits of rational choice or objective aggregation, as they were held in the impossibility theorem or Arrow paradox (section D). Finally, the limits and possibilities of rational choice are summarized (section E).

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[138] Frage nach der Vereinbarkeit von individuellem Egoismus und kollektiver Wohlfahrt (Braun 1999: 19).

[139] Die Spieltheorie als Zweig dieses Ansatzes [Rationalwahlansatz] konzentriert sich vor allem auf Konstellationen, in denen Spieler und Gegenspieler strategische Wahlhandlungen vornehmen müssen, um ihren Nutzen zu maximieren (von Beyme 2000 [1972]: 141).

A. Empirical and normative (liberal and utilitarian) assumptions of the normative rational choice theory

What assumptions underlie Russell Hardin's (2011 [2009]) *rational choice normative theory*? First of all, there is the premise elaborated above that *causality* holds the world together in its innermost core, so to speak; only by this means can one explain the political world with rational choice models. And secondly, by *inversion* of causal propositions, one can also change them. By inverting causal propositions, however, one can only formulate socio-technological regulations; these assumptions are not even addressed and thus *cannot* be *legitimized.*

The two sides of causality, namely world explanation and world change, are not traced back to Bacon, Weber and Popper by Hardin, as I have done above, but to Thomas Hobbes, John Locke and David Hume, who according to Hardin were striving for the same thing as Bacon with Causality:

> Hobbes's (1642; 1651) great works of political theory, *De Cive* and *Leviathan*, were published in the first and last years, respectively, of the English Civil Wars, one of the most devastating periods of English history. Against this background, his view of the role of political theory is the *explanation and therefore the enablement of social order* [emphasis not in the original], a focus that continued through Locke and Hume, although they are increasingly concerned with the working of government and the nature of politics (Hardin 2011 [2009]: 89).

Hardin mentions other *normative* assumptions that are equally accepted for positive and normative theories:

> Note that these three sets of assumptions – individualism, self-interest, and the collective benefits of self-seeking behavior – are the assumptions of *both positive and normative theories* [emphasis not in the original]. This should not be a surprise because the world we wish to judge normatively is the same world we wish to explain positively (Hardin 2011 [2009]: 93).
>
> A fully adequate normative theory must therefore fit both positive and normative assumptions and must depend on both positive and normative methodologies. Often this means that the methodological demands of normative claims are more stringent than the methodological demands of any parallel positive claim. Normative claims must pass muster on both positive and normative methodological standards (Hardin 2011 [2009]: 94).

According to Hardin, the foundations of rational choice theory were also laid by Hobbes, Locke and Hume:

> If any of these three theorists [Hobbes, Locke und Hume] were concerned with "the good society", they would have meant a society that is good for individuals. In an important sense, they are normatively behaviorist. That is to say, they attempt to explain rather than to justify political institutions and behavior […].
> They are normative theorists only in the very limited sense of *explaining* what would get us to better state of affairs, in the sense of those states` being de facto in our interest or better for us by our own lights. From this vision, the main contemporary approaches to explanation derive (Hardin 2011 [2009]: 89).

Hardin thus indirectly confirms what philosophers of technology (Kornwachs 2012) claim: discourses on technical means also need *prescriptive* elements. And value discourses or goal discourses even more so. The fact that no value discourses or goal discourses can be conducted in an empirical science or with an empirical (descriptive-interpretative, explanative and prognostic) methodology has actually been common sense within the explanative-prognostic or the Platonic-Galilean tradition since Weber at the latest (Dasgupta 2009).

Now the question arises how one can scientifically justify the empirical and normative assumptions of the normative theory of rational choice. Failure to do so would lead to the absurd notion that the normative (liberal and utilitarian) assumptions (individualism, self-interest, and the collective benefits of self-seeking behavior) were not made by humans, but are either naturalistically given or downright God-given. In other words: views with an descriptive claim to validity, which one finds and wants to explain as political reality, become normative categories, or liberalism and utilitarianism are presented as naturalistic givens and are not normative theories set by humans that first have to be legitimized.

A naturalistic normativity is also the consequence when the dichotomy between *is* (factuality) and *ought* (normativity) is undermined; there is no doubt that Hardin does this, although empirical scientists always strive for the opposite:

> That would be a profoundly sad separation of normative from positive theory, the worst such separation in the history of social theory, worse than the separation of economic from utilitarian value theory wrought by G.E. Moore (1903, 84) a century ago, when he literally took utility into the vacuousness of outer space (Hardin 2011 [2009]: 99).

It is very surprising that someone would refer to Hume and question the is-ought distinction as well as the naturalistic fallacy. Hume is considered the one who first formulated the is-ought dichotomy that an *ought* (normativity) cannot be inferred from an *is* (factuality):

> A set of true statements about empirical or metaphysical facts, no matter how large, does not permit us to derive from them a norm of command or prohibition[140] (Birnbacher 2007: 363; see Hume 2007 [1739/1740]: 302. Book 3, Part 1, Section 1. For a comprehensive, especially logical, analysis of this problem, see Schurz 1997).

George Edward Moore (1965 [1903]) puts Hume's

> insight on a broader basis. Moore's argument states that not only no normative statement, but also no other kind of evaluative statement can be derived from purely descriptive premises by logical means. In order for an evaluative statement to be derivable, at least one of the premises must also be evaluative (Birnbacher 2007: 363; see Moore 1965 [1903]. A comprehensive, purely logical analysis can be found in Stuhlmann-Laeisz 1983).

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[140] Eine noch so große Menge zutreffender Aussagen über empirische oder metaphysische Sachverhalte erlaubt es nicht, daraus eine Gebots- oder Verbotsnorm abzuleiten (Birnbacher 2007: 363

If one uses the rational choice approach with a practical intention, i.e. if one wants to use it to justify technical regulations, one has to rely on liberal and utilitarian norms, which actually belong to this approach by definition. This means that the normative assumptions of the rational choice approach cannot be justified with the help of this approach within a technical discourse. Thus, any legitimation of technical regulations is also dependent on the pragmatic target discourse and the normative value discourse (section 4.1.3).

The lack of a critical examination of the normative (liberal and utilitarian) and empirical assumptions or their uncritical assumption was also rightly criticized by Susanne Hoeber Rudolph:

> Rational choice inquiry and explanation replaces alternative formulations of motive and identity with a uniform, singular concept of utility maximization. [...]
>
> Theoretically most relevant to my earlier discussion of Lockean liberalism's universalism is the propensity for formal theory to attribute motives rather than investigate them (Rudolph 2005a: 9).

The perestroikans also rightly claim that these assumptions are not objective facts, but rather contextual, theory- and value-laden assumptions:

> [I]ts supposed objective facts were more context dependent, value laden and theory laden than it was prepared to admit (Schram 2003: 847).

This brings us to a misunderstanding that has been cited for decades as a criticism of scientistic scientists and stems from a lack of understanding of differentiation and specialization within modern science.

The if-then deep structure of scientific knowledge (section 5.4.7) means that one starts from assumptions or prerequisites that one does not have to question, at least in an upcoming work. This in no way means that these assumptions cannot be empirically tested in other works, insofar as they are empirical assumptions. As far as practical-normative assumptions are concerned, these can be justified or legitimized in practical (technical, pragmatic or normative) work.

Thus, one can justify the normative presuppositions made, for example, in an investigation using the rational choice approach, within a technical means discourse, within a pragmatic goals discourse or normative values discourse. *Value ladenness* can thus be *resolved discursively*. The same is true for *theory ladenness*; here the critics focus mainly on philosophical (axiological, epistemic, methodological and ontological) presuppositions. Thus, for example, utility maximization can be justified pragmatically as a practical assumption (as a goal or purpose) or normatively (as a value or maxim of action).

If it is an empirical assumption, descriptions, explanations and forecasts can be used to prove or disprove the *utility orientation* (as an empirically ascertainable attitude towards action) of the actors under investigation. Thus, recourse to obscure loadings is not necessary in any case. Even more: the *context dependence* of scientific knowledge is revealed.

The formulation "scientists have found out that", which is unfortunately also used by serious scientists, is simply wrong. The following formulation would be correct: "Scientists have found out that if these conditions apply, this effect occurs". With such phrasing nobody could overlook the context dependence of the scientifically determined findings.

The fact that rational choice theorists make normative assumptions that they cannot justify using the rational choice approach proves that a normative methodology consisting only of a rational choice theory, as Hardin demands, is simply deficient.

Kenneth Joseph Arrow shows (section 6.10.2, D) that an aggregation of individual preferences into social welfare cannot satisfy either democratic or rational grounds for reasons of principle. This, however, eliminates rational choice as a normative methodology that can be used to legitimize values, goals or purposes. They can only be inserted as presuppositions, for instance in game theory, but not justified. But it is precisely normative rational choice theory that is recommended to us as the latest normative revolutionary invention in Hardin's (2011 [2009]) Oxford series.

B. Rationalist critique of model thinking and thus also of rational choice approach

Hans Albert formulates both an empiricist and a rationalist critique of model thinking and has highlighted several points that have become topical again in the current Anglo-Saxon *science war*. Albert raises the charge of *model Platonism*, which favors thinking in models:

> It is a characteristic of a particularly sophisticated economic method: thinking in models – which, however, for those theorists who cultivate the neoclassical style of thinking, essentially amounts to a new form of Platonism[141] (Albert1967c [1965]: 417; my translation).
>
> The neoclassical style of thought, with its emphasis on thought experimentation, reasoning by means of illustrative examples and logically possible extreme cases, model construction on the basis of plausible assumptions, so-called diminishing abstraction, and similar procedures, seems to have had such a formative effect on economic methodology that even

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[141] [E]s handele sich um einen Wesenszug einer besonders hochentwickelten ökonomischen Verfahrensweise: *des Denkens in Modellen* – das allerdings bei denjenigen Theoretikern, die den neoklassischen Denkstil pflegen, im Wesentlichen auf eine neuartige Form des Platonismus hinausläuft (Albert 1967c [1965]: 417).

> theorists who value experience very highly find it difficult to break away from this methodological style[142] (Albert 1967c [1965]: 410; my translation).

Another characteristic of this thinking is an *immunization against experience*:

> Model-Platonism of pure economics, expressed in attempts to immunize economic statements and sets of statements (models) against experience by applying conventionalist strategies[143] (Albert 1967c [1965]: 410; my translation).

Thus, according to Albert, it is a matter of

> immunization of statements and models against the facts[144] (Albert 1967c [1965]: 417; my translation).

Green and Shapiro, who scrutinize political science contributions by rational choice theorists, also point to precisely this shortcoming.

C. Lack of empirical foundation as well as method orientation

Green and Shapiro (Green/Shapiro 1994, Shapiro 2005) criticize the rational choice approach in particular, arguing that parts of political science are *method-driven* and thus unworldly and lack empirical foundations. Schram (2003 and 2005) refers to the criticism of Green and Shapiro.

At the heart of Green's and Shapiro's critique are the unrealistic empirical assumptions of rational choice theory and the thesis that rational choice theorists have made no contribution to empirical research:

> Despite its enormous and growing prestige in the discipline, rational choice theory has yet to deliver on its promise to advance the empirical study of politics (Green/Shapiro 1994: 7).
>
> Impressive as many of the analytic results of rational choice theory might be, it remains to be established that they tell us anything new and reliable about politics (Green/Shapiro 1994: 11).
>
> Contrary to the assertion of Riker and others that rational choice theory fares well in political science because the field is *theory poor,* in fact rational choice theory fares best in environments that are *evidence poor* (Green/Shapiro 1994: 195; my emphasis).

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[142] Der neoklassische Denkstil mit seiner Betonung des Gedankenexperiments, des Räsonnements an Hand illustrativer Beispiele und logisch möglicher Extremfälle, der Modellkonstruktion auf der Basis plausibler Annahmen, der sogenannten abnehmenden Abstraktion und ähnlicher Verfahren scheint in so starkem Maße prägend auf die ökonomische Methodologie gewirkt zu haben, daß selbst Theoretiker, die den Wert der Erfahrung sehr hoch einschätzen, sich von diesem methodischen Stil nur schwer lösen können (Albert 1967c [1965]: 410).

[143] *Modell-Platonismus* der reinen Ökonomie, der in Versuchen zum Ausdruck kommt, ökonomische Aussagen und Aussagenmengen (Modelle) durch Anwendung konventionalistischer Strategien gegen die Erfahrung zu immunisieren (Albert 1967c [1965]: 410).

[144] Immunisierung von Aussagen und Modellen gegen die Tatsachen zu erreichen (Albert 1967c [1965]: 417).

Therefore, Green and Shapiro suspect

> as the accumulation of empirical knowledge about politics proceeds, the influence of rational choice theory, in its present form at least, will diminish (Green/Shapiro 1994: 195).

The style of research must change in favor of giving empirical investigations greater weight, because up to now the rational choice approach has only produced little empirically proven knowledge. A division of labor in which some do theoretical work and others do empirical work cannot succeed:

> Arguably, mastery of the subject matter under empirical observation is required to guide and inspire innovative theorizing (Green/Shapiro 1994: 197).

The rational choice approach follows

> the commitment to pure universalism and the concomitant tendency to ignore, absorb, or discredit competing theoretical accounts (Green/Shapiro 1994: 203).

Also, quite rightly, doubts are raised about a *holistic*, *universal* theory of politics, which supporters of the rational choice approach strive for with an individual approach:

> We remain skeptical that a universal theory of politics could survive systematic empirical scrutiny (Green/Shapiro 1994: 1202; see the Duhem-Quine thesis above, section 3.3.3).

The practical aspects (normative, pragmatic and technical) important to this work are not discussed by Green and Shapiro:

> We shall have little say about the ideological or prescriptive dimensions of rational choice (Green/Shapiro 1994: 11).

D. Fundamental limits of rational choice or objective aggregation: impossibility theorem or Arrow paradox

The central question or dilemma of *social welfare* is how to aggregate individual preferences into social welfare, or: can there be an objective aggregation of individual preferences into social welfare? If this could be done, then there would be a scientific, not democratic, legitimation of actions that could be justified by this approach.

A positive answer is not possible for reasons of principle; this is the central message of the *impossibility theorem* or the Arrow paradox. The aggregation of individual preferences of all citizens into a consistent and complete social welfare function is impossible, according to Arrow, because it is either *arbitrary* or *dictatorial.* Thus, it satisfies neither *rational* nor *democratic criteria.* No *decision procedure* is capable of satisfying all of the following requirements simultaneously:

a. *Universality, condition 1:* all logically justified preference orders of individuals are to be admitted (Arrow 1963 [1951]: 24).
b. *Transitivity or positive association of social and individual values, condition 2:* if someone prefers alternative x to alternative y and y to z, then x should be preferred to z and society as a whole must also come to this decision and specifically prefer x to z, provided that the majority of its members do so (Arrow 1963 [1951]: 25-26).

c. *The independence of irrelevant alternatives, condition 3*: irrelevant alternatives must not influence the decision process (Arrow 1963 [1951]: 26-28, see Gettier problem in section 5.2.1).
d. *Citizens' sovereignty, condition 4*: the social welfare function must not be imposed from outside (Arrow 1963 [1951]: 28-30).
e. *Nondictatorship, condition 5*: an individual must not dictate the order of precedence. In society, a dictator who manipulates the hierarchy must not appear (Arrow 1963 [1951]: 30-31).

E. Limits and possibilities of rational choice

Why is the impossibility theorem or Arrow paradox so important, especially for practical discourses? The normative rational choice approach is supposed to justify social action. This means, on the one hand, that it shows technical ways or means to realize goals and ends. In other words, this model cannot provide a way out of arbitrary decisionism. The rational choice approach alone is not sufficient for either empirical/positive or practical methodologies.

Despite these fundamental shortcomings, the perestroikans are wrong when they claim that *game theory* makes political science less relevant. In the Internet age, in which private corporations such as Google, Amazon, Apple, Microsoft and Yahoo as well as state institutions such as the NSA (National Security Agency) have such great influence and even election campaigns are conducted via the Internet, the importance of logical-mathematical research methodology including rational choice theory is obvious to everyone and will continue to increase in the future.

Therefore, it is more likely that the methodological reductionism that comes into play here is a problem; the collateral damage that this narrowed worldview causes should be the focus of the debate, as has long been discussed in public. An overview of this discussion, unfortunately with some apocalyptic exaggerations, is offered e.g. by Frank Schirrmacher in his book "Ego. The Game of Life" (Schirrmacher 2013). The only problem, then, is the narrowing of the perspective, and specifically the reductionism to one approach. On the other hand, it is not appropriate to condemn game theory wholesale.

For example, *model thinking* and hence rational choice models are important in counterfactual approaches, where the construction of possible (counterfactual) worlds is important; rational choice models are therefore referred to in the paper "Counterfactuals and Case Studies" (Levy 2010 [2008]: 630 and 637). Furthermore, rational choice models are important in the construction of actor models, e.g., in game theory, which could be used to solve practical problems.

The broadening of the research perspective, which is explicitly demanded by the perestroikans, is of course also indispensable here. The criticism of causal reductionism as well as of game theory is therefore necessary for other reasons; revolutionary pathos is also counterproductive and, above all, factually inappropriate, both on the part of the rational choice theorists and the perestroikans, who for their part want to revolutionize and thus, if possible, replace the rational choice revolution with applied phronesis.

7 Plea for an institutionalization of the subdiscipline philosophy of political science

- What role can an *institutionalization* of the subdiscipline “philosophy of political science” play in the future?
- How can the *further development* of the *philosophical foundations* (axiological, epistemic, methodological and ontological) of political science best be guaranteed?
- What are the *tasks* and *relevance* of the subdiscipline?
- What are the scientific *limits* and *possibilities* of political science?
- How can a *politicization* and *moralization* of political science be prevented?
- What *value* should scientific results have for the state and society?

The *seventh chapter* is a plea for an *institutionalization* of the subdiscipline "philosophy of political science" with a corresponding institutional anchoring in the system of science, e.g. in curricula, chairs and journals. First, the most important arguments are presented that speak for the relevance of the subdiscipline (section 7.1). In my view, the subdiscipline should have at least two broad missions: First, it should identify the scientific *limits* and *possibilities* of the discipline as well as *further develop* the philosophical foundations of political science. Second, this subdiscipline could prevent the *moralization* and *politicization* of political science (section 7.2).

7.1 Relevance of political science research

First, the political and public relevance of political science is discussed (section 7.1.1), then the importance of the philosophical discourse for the relevance of political science within the scientific system is discussed (section 7.1.2).

7.1.1 Political public relevance of political science

The relationship between political science and practical politics is a major complex of questions; more precisely, it is about the relevance of political science research for daily political business.

While the (neo)positivists emphasize a stringent methodology, which alone can guarantee scientificity, this strategy is denounced by the perestroikans as scholasticism; scholasticism, methodologism, method-driven, theory-driven are the most important terms of this criticism. What is demanded is a problem-oriented or problem-based science, which above all pays attention to substantive research, and also involves the active involvement of the researchers. In this way, it is hoped to gain a greater public hearing and, ultimately, more influence, i.e. more relevance.

As with the interpretivists, the perestroikans are also critical of the scientistic scientists' methodology, which is primarily oriented toward relevance. In this context, the decidedly methodical researchers of the explanative-prognostic tradition are accused of having lost touch with reality; above all, a lack of substance and technocratic practical relevance are put in the front:

> Substance must remain more important than research technology. It is better to be vague than precise but not relevant […].
>
> The technocratic practical relevance must be replaced by the responsibility of the intellectual and by direct political commitment and creative speculation. The professionalization of the subject that has occurred, which was essential in the consolidation phase, is to give way to an intellectual-humanistic view of the role of the political scientist in society, which necessarily promotes the politicization of the subject[145] (von Beyme 2000 [1972]: 118, my translation).

In addition, there is the demand for diversity or pluralism in questions of methodology (section 2.2).

Methodology and stringency are indispensable; otherwise, political science will lose even more importance compared to economics. Today in the information age of the 21st century, in times of Google, Facebook, NSA, artificial intelligence etc., it is downright unworldly to accuse the logical-mathematical research methodology of being irrelevant. However, the dominance of this methodology also leads to unforeseeable collateral damage (Schirrmacher 2013). Therefore, the limitations of this methodology should be pointed out and the importance of a complementary linguistic-interpretative as well as a practical research methodology should be explained.

The explanative-prognostic or the Platonic-Galilean tradition is extremely important, but it is not enough because of the various reductionisms (causal, empirical and methodological reductionism). Complementarily, two other methodologies (descriptive-interpretative and practical) are necessary, as explained in detail in this book. Ignoring understanding and description is fatal, without a qualitative-interpretative methodology one cannot describe and evaluate political phenomena such as demonstrations, coup d'etat or the diverse political regulations (constitutions, laws, decrees, ordinances, etc.).

7.1.2 Methodology and scientificity: The importance of the discourse on philosophical foundations for the position of political science within the scientific system

According to John Gunnell, the simultaneous pursuit of scientificity and practical relevance has led to a fundamental paradox between scientificity and relevance:

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[145] Substanz muß wichtiger bleiben als Forschungstechnik. Es ist besser, vage als präzis, aber nicht relevant zu sein […].
Der *technokratische Praxisbezug* muß durch die Verantwortlichkeit des Intellektuellen und durch direktes politisches Engagement und kreative Spekulation abgelöst werden. Die eingetretene Professionalisierung des Faches, die in der Konsolidierungsphase unerlässlich war, soll einer intellektuell-humanistischen Auffassung der Rolle des Politikwissenschaftlers in der Gesellschaft weichen, was die Politisierung des Faches notwendigerweise fördert (von Beyme 2000 [1972]: 118).

> This search fort the authority of science, however, paradoxically distanced the social sciences, physically and conceptually, from the very world that they wished to influence, and, at the same time, they became bound to the vagaries of transformations in the images of science to which they were beholden (Gunnell 2015a: 414).

The following summarizes why there is no paradox between science and relevance. On the contrary: the relevance of science depends on the stringency of scientific work. The relevance of a discipline in the interaction of all sciences is given by the fact that methodologies that were developed in this discipline were adopted by other discipline. In other words: the philosophical innovation of a discipline mainly determines the reputation and relevance of the discipline within the academic system.

The difference between ideologies, utopias and slogans of the round table on the one hand and scientifically based theories on the other hand is not in the content (which can be the same), but in the methodological approach. Only hypotheses based on a scientific methodology can claim the status of scientific theories and thus scientific knowledge.

The tools (*organon*) of scientific research include: concepts, sentences, theories, logics, arguments, methodological approaches and methods as well as the axiological, epistemic, methodological and ontological foundations.

The primary goal of the methodology is to claim the authority of science for one's own investigations. According to the scientistic scientists, political science should be part of the social sciences; the protagonists themselves absolutely see themselves as disciplined scientists.

They are perhaps a little envious of the scholars or theorists within the humanities, who are able to reproduce their results in several classical and modern languages, and always have a suitable quotation from Homer, Cicero, Shakespeare, Goethe etc. ready. Unfortunately, the whole thing is only on feature (*Feuilleton*) level, they cannot claim the authority of science, because a corresponding, scientific methodology is missing, so it is only about subjective insights, which lack any objectivity. This movement of the disciplined political scientists to distance themselves from the undisciplined political theorists, which has decisively dominated the methodological development since the emergence of political science, still characterizes the attitude of many scientists today, one hundred years after the emergence of political science in the USA.

Science is a work in progress. This applies not only to the content or the substantive knowledge, but also to the methodology or the formal knowledge. Constant improvement in methodology necessarily entails ever greater rigor. Therefore, methodological stringency cannot be played off against scientific relevance. On the contrary: in public perception, political science is in direct competition with other sciences.

If political science wants to make up ground against economics, which is much more present and influential in the public sphere when it comes to political, social or economic issues, it must deliver better empirical-explanatory and empirical-prognostic analyses, as well as empirical and technical rational choice analyses.

In describing or interpreting political reality, political science is in direct competition with sociology in particular. In the long run, the better, more stringent methodology plays a decisive role alongside other resources such as human resources (size of the institutes) and publication opportunities.

The Perestroikans are right about one thing, and that is to criticize the American scientistic establishment for its narrow understanding of science ("narrow conceptualization of what [...] good 'scientific' work" [is], Monroe 2015: 423). If the linguistic-interpretive research methodology is omitted, the discipline is extremely weakened. Only with a linguistic-interpretive research tradition can political texts (constitutions, party programs, citizens' demands, etc.) be understood and critiqued, and regulation changes justified.

Political science is also in direct competition with jurisprudence. Practical politics primarily falls back on suggestions from lawyers when it comes to designing and justifying complex norms and regulations for political control. The practical tradition worked out above all in this book offers the methodological basis for a practical political science in order to generate scientifically founded norms and regulations.

7.2 Tasks of the subdiscipline "philosophy of political science"

In this section, the two main fields of activity of the subdiscipline will be presented: First, it should determine the scientific limits and possibilities of the discipline and further develop the philosophical foundations of political science (7.2.1). Second, this subdiscipline could prevent a moralization and politicization of political science (7.2.2). The scientific religious war is an expression of an identity crisis. If these disputes are dealt with in a subdiscipline, these philosophical disputes could become signs of vitality (7.2.3).

7.2.1 Determination of scientific limits and possibilities as well as further development of the philosophical foundations of political science.

An examination of the philosophical presuppositions is urgently necessary, because otherwise political science will stop at a core area, the philosophy of political science, and thus remain behind (out of touch):

> But my main point is simply that political methodologists are dangerously out of touch. Philosophical thinking has altered dramatically in ways that render highly problematic the meta-methodological assumptions of many political scientists. Discussion of methods and their utility are profoundly impoverished by a lack of thought about their epistemological, ontological, and explanatory assumptions (Bevir 2010 [2008]: 48-49).

For the mainstream of empirical scientists,

> critical rationalism, at least implicitly, has become a cultural asset that guides research[146] (von Beyme 2016: 47).

Unfortunately, in political science, the axiological, epistemic, methodological, or ontological assumptions of critical rationalism and the rational choice approach are rarely subjected to critical analysis.

Division of labor and specialization rightly contain reductionist methodologies. The if-then structure of knowledge makes it possible, and specialization requires a detailed, reductionist approach. It is important, however, not to lose sight of the implicit and unspoken prerequisites or the hidden and tacit assumptions that form the context of the respective findings, but also of the used methodologies; at least *ceteris paribus* conditions should be considered, if not formulated. The contextuality of knowledge is known not only to perestroikans but also to scientistic scientists.

Due to the if-then structure of scientific knowledge and other fundamental limits of scientific research and their complexity, it is certainly necessary to work with epistemological assumptions that one cannot or does not have to prove oneself just asserting, not demonstrating them. However, one must be aware of the associated philosophical perspective of the results. A lack of rigor in axiological, epistemic, methodological and ontological questions only leads to confusion, and therefore to inadequate results.

The philosophy of political science as a special field not only enables a therapeutic dimension to eliminate misunderstandings, but also innovative insights can be developed that can advance the discipline in many ways. For these reasons, I argue for an institutionalization of the subdiscipline "philosophy of political science" with a corresponding institutional anchoring in the system of science, e.g. in curricula, chairs and journals.

7.2.2 Preventing the politicization and moralization of political science

The relevance of norms and values within science is extremely high. In the science war, this has often been extremely truncated down to the question of whether values influence science or whether science should be value-free. In this book, I have tried to do justice to the complexity of these questions by addressing non-epistemic norms and values in the fourth chapter on axiology and epistemic norms and values in the fifth and sixth chapters on epistemology and methodology (summaries in sections 8.3 and 8.4).

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[146] der Kritische Rationalismus wenigstens implizit als gesunkenes Kulturgut forschungs–anleitend geworden (von Beyme 2016: 47).

Pluralism is a value that hardly anyone questions today, even if quite a few practice exactly the opposite, as I have shown with the example of the causal and phronetic reductionists. In short, the opponents, scientistic scientists as well as perestroikans, display a *pluralistic habitus*, even though they pursue a *reductionist agenda* on a daily basis. Therefore, the science war has led to the *politicization* and *moralization* of the discipline. The negative effects are particularly evident in the fact that the quality of the discipline suffers because researchers are tempted to demonize other perspectives and approaches in a revolution mode.

A systematic reconstruction and self-reflection of scientific foundations is therefore necessary in every specialized science. An adequate reduction of complexity can make exactly this possible. Furthermore, this is the only way to prevent the misuse of science. The Manichean religious war is, mainly because of the way it is conducted, an expression of an identity crisis and also affects other subdisciplines of political science. A revolutionary discourse is counterproductive because it leads to a politicization and moralization of science. In the end, this achieves exactly the opposite, because it produces black-and-white thinking and thus causes an infantilization of the discourse (section 2.6).

The scientification of politics and of life as a whole has progressed extremely far. In some cases, secularization has elevated "science" to the status of a religion as collateral damage. Therefore, it is particularly important to work out why scientific findings are not *definitive* but only *hypothetical* answers (section 5.4). The if-then structure of all scientific knowledge and thus both the limits and the possibilities of science can be worked out in particular in the subdiscipline "philosophy of political science", i.e. that the limits and possibilities of political science research can be ideally discussed on the basis of the axiological, epistemic, methodological and ontological foundations.

However, politicization and moralization can also have positive effects, provided that this contributes to the improvement of one's own methodology and to better craftsmanship. Competition, provided it is meritocratically driven, always leads to progress. Within this subdiscipline, it would be important to allow an adequate reduction of complexity as well as to design an evolutionary advancement. A revolutionary discourse is counterproductive because it leads to a politicization and moralization of science. In the end, this achieves exactly the opposite, because it produces black-and-white thinking and thus promotes an infantilization of the discourse.

7.2.3 Philosophical disputes: Expression of an Identity Crisis or Sign of Vitality?

John Gunnell, who in his article "Pluralism and the Fate of Perestroika: A Historical Reflection" (Gunnell 2015a) subjects the perestroika movement to historical reflection, sees the science war as an expression of a *perpetual identity crisis* of the discipline:

> the perennial identity crises that have marked the evolution of political science. [...] Perestroika was basically a reverberation of longstanding problems about the relationship between political science and politics and about the tensions between the search, on the one hand, for intellectual unity and, on the other hand, the commitments to both disciplinary and political pluralism (Gunnell 2015a: 408).

In my opinion, philosophical arguments are more a sign of the vitality of a scientific discipline because of the central importance that is attached to methodology, for example. The further development of each discipline depends primarily on the extent to which philosophical renewal and innovation occurs. Science wars are counterproductive because religious warriors call for the abandonment of existing philosophical traditions. The methodological reductionism of the explanative-prognostic or the Platonic-Galilean tradition has contributed to enormous methodological advances in determining causality due to the differentiation and specialization that goes with it (section 4.2.2). On the other hand, ignorance of other traditions by those who even exaggerate the importance of the methodology ("The content is the method", King/Keohane/Verba 1994: 9) is counterproductive for the discipline as a whole.

The establishment has responded to the caucus revolt with a 'hug strategy', namely

> encouraged the establishment of separate working groups at APSA meetings, established a separate body, "PS", devoted almost exclusively to professional problems and disputes, and initiated a series of constitutional amendments[147] (Falter 1982: 60, my translation).

There was also a similar reaction to the challenges posed by the perestroikans (Hochschild 2005), including the founding of a new journal within the APSA, namely Perspective on Politics in 2003. This approach (Easton method) gave the religious warriors their own playground or offering their own magazine is not enough. On the contrary, this only promotes segregation and means that one no longer notices what methodological advances are being made elsewhere. It could be shown here that the perestroikans simply did not notice the methodological advances at the end of the 20th century within the explanative-prognostic or the Platonic-Galilean tradition and in some cases criticized a state of research that was obsolete. The perestroikans at least grappled with the methodology, albeit flawed. On the other hand, ignorance

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[147] die Einrichtung separater Arbeitsgruppen auf den APSA-Tagungen förderte, ein eigenes Organ, „PS", gründete, das fast ausschließlich berufssständischen Problemen und Auseinandersetzungen gewidmet ist, und eine Reihe von Satzungsänderungen einleitete (Falter 1982: 60).

dominates, which becomes apparent above all from the fact that linguistic-interpretative research methodology is not dealt with at all in a volume that claims to give an overview of the entire political science methodology. As shown above, there is a book for the Platonic-Galilean tradition, "The Oxford Handbook of Political Methodology" (Box-Steffensmeier/Brady/Collier, 2010a [2008]), which excellently discusses the possibilities and limitations of a logical-mathematical research methodology is informed. However, it does not offer an overview of the entire political science methodology, as the title suggests.

Two further volumes are missing, one on linguistic-interpretative methodology, as there is already one for sociology (Flick/von Kardorff/Steinke 2015 [2000] and Denzin/Lincoln 1994). Furthermore, there is no volume on the practical (normative, pragmatic and technical) methodology (Lauer 2017). The applied phronesis offered by the Perestroikans (Flyvbjerg/Landman/Schram 2012) does not even reach the level of research presented in the Aristotelian Organon, let alone the current state of practical discourse. What is needed, on the other hand, is a joint work by all methodological traditions within the discipline, oriented towards the Aristotelian Organon and elaborating in the Aristotelian spirit an overview of the current possibilities and limits of political science methodology.

A *systematic reconstruction* and *self-reflection* of scientific foundations is therefore necessary in every discipline, not least because this is the only way to prevent the misuse of science. An adequate reduction of complexity can make precisely this possible. Furthermore, only in this way can the misuse of science be prevented.

The Manichean science war is, mainly because of the way it is conducted, an expression of an *identity crisis* and also affects other subdisciplines of political science. A revolutionary discourse is counterproductive because it leads to a politicization and moralization of science. In the end, this achieves exactly the opposite, because it produces black-and-white thinking and thus encourages an infantilization of the discourse (section 2.6).

The *establishment* of a subdiscipline "philosophy of political science" can contribute to these disputes being perceived as a sign of vitality in the future. If it is no longer a question of who wins and who loses, but of making the foundations of political science visible and developing them further. In short: *evolution not revolution* is called for (section 2.6).

8 Conclusions

The book is aimed at *practitioners* and *scientists* who want to discover the *limits* and *possibilities* of science and how to draw a *demarcation line* between *science* and *non-science*. The slogan "Follow the science" revealed the *total scientification* of all areas of life, including politics. Precisely for this reason, a critical and prominent examination of the *philosophical foundations* of science and how to guarantee *scientificity* should be the main topic of scientific, political and social debates.

In this chapter, I would like to summarize the results of the book. First, I will address its main aims:

- *Logical geography* of the philosophy of political science: to provide an overview of the philosophical (axiological, epistemic, methodological and ontological) foundations on *ten vertical* and *three horizontal levels* as well as to identify the limits and possibilities of political science research (section 8.1).

Then I will mainly deal with the subject complexes of the subdiscipline, for the further development of which I have developed *my own* proposals:

- Overcoming the science war (*Methodenstreit*): complementarity between three philosophical traditions (descriptive-interpretative, explanatory-predictive and practical) (section 8.2),
- Further development of political philosophy/political theory into a practical political science (section 8.3),
- Epistemic norms or values constitute scientificity: enabling science and demarcation between science and non-science (section 8.4),
- External influences on science: Non-epistemic norms or values (section 8.5),
- Evolution instead of revolution: the need for a new framework for the philosophical debate and methodological approaches to critical reception of, reflection on, and further development of philosophical tradition with a systematic intention (section 8.6),
- Plea for an institutionalization of the subdiscipline philosophy of political science (section 8.7).

8.1 Logical geography of the philosophy of political science and the limits and possibilities of political science research

An *overview* of the philosophical foundations of the discipline of political science has been established in this book, among other things, with the help a chart (2nd chart) that consists of *ten vertical* and *three horizontal levels*. A *logical geography* of the philosophy of political science thus becomes visible (section 8.1.1). Then the curriculum for the subdiscipline "Philosophy of Political Science", which I developed for the first time, is discussed. It consists of bachelor and master seminars as well as extensive charts. These seminars provide a didactically elaborated introduction to the subdiscipline. The book's charts offer a reduction of complexity for the most important topics. By answering the questions listed there, both the possibilities and the limits of the subject of political science can be determined (Section 8.1.2).

8.1.1 Logical geography of the philosophy of political science: overview of the philosophical foundations on ten vertical and three horizontal levels

The *main goal* of this monograph was to present an overview of the philosophical (axiological, epistemic, methodological, and ontological) foundations of political science research. The *ten vertical* and *three horizontal levels* provide a mental overview or *logical geography* of the scientific foundations of the discipline. Thus, an orientation in the thinking or topography of scientific reason becomes possible.

This procedure was found to be an adequate analysis grid. One can only deal adequately with both the limits and the possibilities of all philosophical traditions if, as has been done here, they are discussed ideally on several levels. Although complexity could still be reduced further, at least an overview of the most important questions could be outlined and some of the answers given by the opponents could be explained without losing sight of the arrow of time and the associated dynamics of development. This enabled me to demonstrate the structural differences between descriptive-interpretative, empirical-explanatory and practical (normative, pragmatic and technical) traditions on ten levels.

The *ten vertical levels* (1st chart, section 9.4.1 and 2nd chart, section 9.4.2) contain first the axiological, epistemic and ontological levels, followed by seven methodological levels (concepts, propositions, theories, logics, modes of argumentation, methods and methodological approaches level).

Furthermore, the *complementarity* between descriptive-interpretative, empirical-explanatory and practical or between interpretative, scientistic and practical traditions could be shown. The *three horizontal levels* (2nd chart, section 9.4.2) reveal structural differences between the three philosophical traditions: the *descriptive tradition*, the *explanative-prognostic tradition*, and the *practical tradition*. In the respective columns of the table, one can see the most important features of a tradition on ten levels.

Firstly, the diverse axiological, epistemic, methodological and ontological foundations were identified. Secondly, these classifications reveal the limits and possibilities of the philosophical foundations of scientific research. Furthermore, the dynamics of historical developments can also be captured, because the focus is primarily on the issues under discussion and not so much on the individual answers, which change over time by being further developed or replaced by completely new answers.

8.1.2 Curriculum for the subdiscipline "philosophy of political science": bachelor and master seminars as well as extensive charts

The third major objective of this book was to establish the first *curriculum* for the subdiscipline "*philosophy of political science*", and to show what should be considered. A *basic* (undergraduate) and an *advanced* (graduate) *seminar* were developed for *beginners* and *advanced prospects*. The *charts* in the book provide a quick *overview* of the most important topics and a *reduction in complexity* of special discussions and subject areas.

The goal of a *curriculum* is to regulate learning goals and learning content of a study program. In this book one finds what is, to my knowledge, the first attempt to establish a curriculum for the subdiscipline philosophy of political science. First, it develops a bachelor's seminar for undergraduate study with ten courses preferably of four hours each. Second, I present a master's seminar for graduate study with seven courses of four hours each. Third, I have created twelve charts suitable for use in both seminars.

8.2 Overcoming the methodological science war (*Methodenstreit*): complementarity between three philosophical (descriptive-interpretative, explanatory-predictive and practical) traditions as a way out

The science war is not limited to methodology. On the contrary, without a proper discussion of axiological, epistemic and ontological issues, one cannot overcome the science war, because such issues have a decisive influence on the methodological approach. This Manichean war, which has been raging since the 19th century, is characterized by the opponents talking past each other, mainly because they largely ignore the positions of the opposite side (scientistic scientists) or because, like the interpretivists, they are insufficiently aware of the constant methodological developments and therefore introduce straw men and slay these instead.

In the following, the opponents in the science war will be briefly presented (section 8.2.1). Second, it will be shown that there is a complementarity between the philosophical traditions. This complementarity between three philosophical (descriptive-interpretative, explanative-prognostic and practical) traditions makes it possible to overcome this dispute (section 8.2.2).

8.2.1 Opponents in the science war: scientistic scientists of the explanative-prognostic or the Platonic-Galilean tradition versus phronetic perestroikans within the Aristotelian tradition

Since the establishment of political science as a discipline in the early 20th century in the United States, there has been a division between disciplined political scientists and undisciplined political theorists. This dispute continues to this day, flaring up at intervals of a few decades. Because of the many rational shortcomings of this discourse, it is more of a Manichean war than a scientific controversy. Since it is not only a dispute about different methods (quantitative or qualitative), but, as I have shown, differences can be identified on ten levels, the term methodology dispute (*Methodologiestreit*) instead of method dispute (*Methodenstreit*) would be more appropriate.

At the beginning of the 21st century, two opponents face each other: on the one hand the mainstream, as we have seen, or rather the *scientific establishment*, with a logical-mathematical research methodology (section A), and on the other hand the *perestroikans*, with a linguistic-interpretative research methodology (section B). When it comes to practical research, the establishment prefers an applied, technical methodology, more precisely a *normative rational choice theory*, whereas the perestroikans want to revolutionize the social sciences in this respect with an *applied phronesis* and contribute to making them more publicly relevant again. I have summarized the positions of the opponents in several charts (position of the scientistic scientist in the 3rd chart, section 9.4.3 and position of the perestroikans in the 4th chart, section 9.4.4). Furthermore, these differences were discussed in detail within four chapters (chapters 3-6).

A. The scientists or the explanative-prognostic or the Platonic-Galilean tradition within political science

The scientistic scientists (positivists, naturalists) within political science want to set themselves apart from the humanistic traditions by orienting themselves towards the natural sciences and introducing a logical-mathematical research methodology (sections 2.2 and 4.2). Their own axiological, epistemic, methodological and ontological foundations are assumed (section a). Research is carried out using a logical-mathematical research methodology (section b).

Contrary to widespread assumptions, quantitative studies show that only a minority of all political scientists feel committed to the explanative-prognostic or the Platonic-Galilean tradition and write causal studies. But since it is a very influential minority, I speak of the *scientistic establishment* (section c).

a. Axiological, epistemic, methodological and ontological assumptions: causal, empirical and methodological reductionism

The search for *invisible causalities* is the common thread that holds together the explanative-prognostic or the Platonic-Galilean tradition – *causality* that is generally believed to be *invisible*, i.e. not directly observed. Only very special observations (*data-set observations* and *causal-process observations*) that enable *causal inferences* are visible. All methodological innovations (quantitative methods, model thinking, qualitative-mathematical methods, experiments) serve the sole purpose of identifying invisible causalities: firstly, *causal regularities* between different events, and secondly, *causal processes* or concrete cause-effect mechanisms to *explain.* Within the explanative-prognostic or the Platonic-Galilean tradition, one only searches for causalities; causality is the only relation that counts, other relations or even contexts of meaning are of no interest. Hence my term *causal reductionism.*

Causality is the ontological presupposition par excellence. It is seen as what holds the world together at its core, or the cement of the universe. Those who identify causalities can recognize and change the world. Both are only possible because there is an *equivalence* between *causality* and *action.* Only under this condition can one convert knowledge (theory) into action (practice), i.e., into social technology, by inverting causal propositions. With this, standardizations or regulations can be formulated in passing as part of an *applied* (*non-practical*) political science. The *ethical-normative* and *pragmatic* dimensions are not addressed at all, as has been done in practical philosophy or political philosophy since ancient times. Only a halved, *instrumental reason* is at work here.

In the focus of the explanative-prognostic or the Platonic-Galilean tradition within political science are *empirical causal analyses*, which have been created since the 1950s with the help of *quantitative tools* (concepts, methods and methodical approaches) as well as *deductive* and *inductive ways of argumentation.* Since the 1970s, logical-mathematical *model analyses* have been added – in political science mainly *rational choice models* and *qualitative-mathematical methods* (QCA, process tracing) –, and since the 1990s *experiments* (in political science, in contrast to sociology, hardly any *simulations*).

Methodological individualism, another fundamental characteristic of this tradition, goes back to Hobbes, but above all to Weber and von Hayek, and is preferred to holism in the explanative-prognostic or the Platonic-Galilean tradition. Because liberal categories have been added since Locke and utilitarian ones since Mill, this *Great Revolution* can be called *liberal.* Therefore, Rudolph quite rightly speaks of a *Lockean liberal universalism.*

Liberalism and universalism on the one hand as normative prerequisites and causal and empirical reductionism, methodological individualism and model analyses as methodological assumptions on the other hand are the *hidden assumptions* that usually flow into empirical research without reflection. If they do not expressly mention and consider these assumptions, researchers try to use a logical-mathematical research methodology to feign an objectivity that on closer inspection does not exist. The contextuality or the *if-then structure* of the knowledge should always be taken into account; it would be best if the *ceteris paribus conditions* are made explicit.

b. Research programs within the explanative-prognostic or the Platonic-Galilean tradition instead of "revolutions" within a Great Revolution

According to Goodin, there were *three revolutions* within American political science: first, its establishment as a science and its *orientation towards the natural sciences*, above all through the introduction of causal and *empirical thinking* and of *deductive* and *inductive* ways of *argumentation*, the introduction of *professional* and *systematic* approaches and the establishment of the discipline at American universities at the beginning of the 20th century. Then came the second revolution, the *behavioral revolution*, in the 1950s; the third revolution, the *rational choice revolution*, finally took place from the 1970s onwards.

In my opinion, the scientistic methodology consists of several *methodological research programs* (3rd and 6th chart, section 9.4.3 and 9.4.6), which *complement* each other and are *constantly evolving*:

1. scientific research program oriented towards the natural sciences
2. causal reductionism
3. model thinking
4. quantitative research program
5. qualitative-mathematical research program
6. experimental research program

c. Spread of causal thinking within political science

As empirical evidence has shown, causal thinking and quantitative methodology do not dominate the mainstream of political science, as many believe, but only the very influential liberal establishment, particularly in the US. Quantitatively, only 5 percent of professorships in the U.S. are occupied by scientistic scientists. Causal analyses make up far less than 50 percent of the publications in the influential American journals. However, it is still far more than a few East Coast Brahmins who prefer causal thinking using quantitative-mathematical research methodology and, according to the perestroikans, also dominate the American Political Science Association (APSA) (section 2.2.3, A, c).

B. The phronetic perestroikans within the Aristotelian tradition

The Aristotelian tradition is significantly more heterogeneous than the explanative-prognostic or the Platonic-Galilean tradition. I will first summarize the most important features of the interpretivists (section a). Then I will address the perestroikans, who are the important opponents of the scientistic scientists at the beginning of the 21st century (section b).

a. Sense making, meaning making or understanding using linguistic-interpretative research methodology: language and qualitative-interpretative tools (terms, methods and methodical approaches)

There has been criticism of the explanative-prognostic or the Platonic-Galilean tradition since the emergence of the social sciences in the 19th century, with recourse to the Aristotelian tradition and with reference to the special nature of the human or cultural sciences, which, according to these representatives, are fundamentally different to the natural sciences. Within the Aristotelian tradition, both the empirical and the practical approaches, as well as the axiological positions, are heterogeneous. In the second half of the 20th century, this debate shifted primarily to the level of methods, so that a distinction was made between *quantitative* and *qualitative-interpretative* research. Since philosophical positions have increasingly come to the fore within the debate, the discussion has been centered on the pairs of opposites: *naturalism* versus *constructivism* and, in particular, *naturalism* versus *interpretivism.*

Focusing on creating invisible causal explanations and causal predictions is not enough. Before one can determine a causal relationship between events, one has to describe these events – but this requires linguistic-interpretative methods. These methods are also necessary to understand the meaning of the diverse political symbols (texts, audios, images and videos). Secondly, a qualitative-interpretative methodology is required to describe *visible events* (occurrences or phenomena); only then can invisible causalities be explained. The data-set observations (DSOs) and causal-process observations (CPOs) are in my opinion examples of the description of phenomena, although they were developed within the explanative-prognostic or the Platonic-Galilean tradition.

A *logical-mathematical* as well as a *linguistic-interpretative research methodology* is therefore necessary to recognize political reality. Only qualitative-interpretative and qualitative-mathematical methods enable adequate empirical (descriptive-interpretative, explanative and prognostic) knowledge; this is the only way to successfully justify descriptions, explanations and forecasts.

b. The phronetic perestroikans: tension points and applied phronesis

I have chosen the perestroikans within the interpretivists mainly because they also offer a practical methodology with recourse to Aristotle. The practical aim of the phronetic perestroikans is to create a *practical methodology* by means of an *applied phronesis.*

Furthermore, they try to bundle all the criticism of the scientistic scientists expressed so far. Drawing on Foucault's work, the phronetic perestroikans also seeks to analyze power and tension points. These *tension points* should then be resolved in the interests of the disadvantaged. Smart scientists (problem-based, problem-driven) could then use case studies, applied phronesis, and inductive reasoning to describe and explain political reality and develop alternatives to political regulation.

8.2.2 Complementarity between three methodological traditions

In ancient times, the path from myth to logos was successfully contested, and the scientific tools used were exemplarily recorded in the Aristotelian Organon. The greatest methodological innovation since antiquity was the emergence of the Platonic-Galilean tradition. Several methodological research programs (causal thinking, model thinking, quantitative as well as qualitative-mathematical methods, experiments and simulations) could be developed and contributed to the enormous success of the sciences, so that the scientification of the world has been a fact for decades.

The explanative-prognostic or the Platonic-Galilean tradition could not displace the Aristotelian tradition in the social sciences and thus also in political science. On the contrary, various methodological research programs (phenomenology, understanding of meaning, hermeneutics, qualitative-interpretative methods) emerged within the Aristotelian tradition.

The phronetic perestroikans are right about one thing, namely their criticism of the scientistic establishment for its narrow understanding of science. If one omits the hermeneutic and interpretative research perspective that enables text analysis and understanding of meaning as well as phenomenology, the subject is extremely weakened. Political texts (constitutions, party programs, citizens' demands, etc.) can only be understood and criticized with a linguistic-interpretative research methodology. Therefore, both methodologies, an explanative-prognostic as well as a linguistic-interpretative research methodology, are indispensable for political science.

A Manichaean science war is factually unjustified simply because there is no unbridgeable discontinuity between scientistic scientists and perestroikans and interpretivists. There is no general incommensurability between the explanative-prognostic or the Platonic-Galilean and the Aristotelian traditions.

The agonistic escalation to a competitive confrontation that can only have one winner and is thus aimed at destroying the other side goes too far. It destroys a scientific culture that is enlivened by competition, but does not necessarily exclude the other side. An antagonistic view would squander the possibility of a complementary approach. Only the complementarity of the methodologies would enable coexistence according to the motto: fight separately and win together.

An *empirical* (descriptive-interpretative, explanative and prognostic), *value-free political science* is not only possible, but also offers the best conditions for recognizing political reality. In addition to determining causalities, it is also necessary to describe meanings and contexts, which is why both the logical-mathematical and the linguistic-interpretative research methodology are necessary. Weber advocated both methodologies, in contrast to the majority of contemporary quantitative researchers who, while rightly defending freedom from value judgments, underestimate the importance of linguistic-interpretative research methodology for knowledge or consider it dispensable. I only share the perestroikans' critique of the scientific establishment when it comes to recognizing the linguistic-interpretative research methodology as a complementary, not an alternative, methodology.

How can the Manichaean science war be overcome? Neither *revolutions*, as the scientistic scientists believe, nor *turns*, as the interpretivists demand, will help; what is required is *evolution and innovation* instead of revolution, innovative further development instead of revolutions or turns. The existing methodologies must first be explained, made explicit, specified and reconstructed: the heritage must first be worked out before it can be reformed and further developed in an innovative way. There is *no Archimedean point*, just a variety of proven methodologies and perspectives. This can best be realized if the complexity of the tasks is adequately discussed at several levels. Maintaining philosophical traditions does not conflict with innovations and further developments, i.e. it does not prevent scientific dynamism. Both are necessary, hence my motto "Combining tradition and progress".

The axiological, epistemic, methodological and ontological debates within political science are not least characterized by the complexity of the topics they contain. Due to the complexity of the topic, it is necessary and sensible to ideally explain the relevant questions on ten vertical and three horizontal levels. Only through an increase in complexity can the counterproductive dispute turn into a constructive debate about the limits and possibilities of political science research. Firstly, the structural differences between descriptive-interpretative, empirical-scientistic (explanative and prognostic) and practical (normative, pragmatic and technical) methodologies have been demonstrated. Due to this complementarity, mutual recognition instead of a science war and thus a constructive debate is possible (2nd chart, section 9.4.2).

8.3 Further development of political philosophy/political theory into a practical political science that complements an empirical political science

I have identified a structural difference between *empirical* and *practical sciences* (2nd chart, section 9.4.2, 5th chart, section 9.4.5, and 6th chart, section 9.4.6). Importantly, a reductionist approach cannot deal simultaneously with empirical and practical issues, as both scientistic scientists and the perestroikans claim. The critique of the concepts of an *applied science*, on the one hand the *normative theory of rational choice* of the scientistic scientists and on the other hand the *applied phronesis* of the perestroikans, should therefore first be summarized (section 8.3.1).

Furthermore, there is a need for a *genuinely practical* (normative, pragmatic and technical) methodology, since this differs in fundamentally from an empirical (descriptive, explanative and prognostic) methodology on ten methodological levels. Such a practical methodology can lay the foundations for a *practical political science* that complements an empirical methodology (section 8.3.2). Third, the arguments are presented that speak in favor of further developing the subdiscipline *political philosophy/political theory* into a *practical political science* in which practical (normative, pragmatic and technical) knowledge can be rationally justified (8.3.3).

8.3.1 Criticism of the concepts of an applied science: the normative theory of rational choice of the scientistic scientists and the applied phronesis of the perestroikans

The following discusses the possibilities and limitations of the practical methodologies advocated by scientistic scientists and phronetic perestroikans. First, the *is-ought* differentiation is discussed (section A); second, the limitations of normative rational choice theory are summarized (section B), and third the limitations those of applied phronesis (section C).

A. *Is-ought* differentiation

In contrast to the scientistic scientists, the phronetic perestroikans, referring especially to American pragmatism and the Frankfurt School, deny a principled distinction between *is* (factuality) and *ought* (normativity). Nevertheless, both follow a reductionist methodology.

The distinction between *is* and *ought* exists because in principle one needs different methodologies for the justification of empirical (descriptive, explanatory, and prognostic) knowledge on the one hand and practical (normative, pragmatic, and technical) knowledge on the other hand (2nd chart, section 9.4.2).

A technical or socio-technological *reductionism* is now dominant not only among the establishment but also among the interpretivists. This applies to Fischer's (Fischer/Forester 1993a) argumentative turn and to Schubert's (2009) pragmatic political field researchers, as well as to governance research, which disputes the separation between what is and what ought to be; they also believe that technical proposals can be justified with the help of causal analyses (8th chart, section 9.4.8). Following Aristotle with their applied phronesis for a real social science, the perestroikans also represents a methodological reductionism. Hennis (1963) proceeds in exactly the same way, methodologically exaggerating not the Aristotelian phronesis, but the Aristotelian topic.

After all, it is not only a question of cleverly implementing various social-technological regulations in practice, but also of *justifying* them with a practical methodology. The practical discourses used in empirical political science (normative rational choice approach) as well as the applied phronesis used by the perestroikans do not meet the requirements of current logical-analytical argumentation standards.

B. Applied methodology of an applied political science: normative rational choice theory

The scientistic scientists do not indulge in a *vita contemplativa* (Arendt 2006 [1958]) as the critique of the problem-oriented perestroikans suggests to us. On the contrary, the implicit presuppositions show that even within the explanative-prognostic or the Platonic-Galilean tradition the *vita activa* determines the true tasks of science, as not only Bacon demands.

The scientistic scientists emphasize that there is a principled difference between *is* and *ought*, and thus between empirical and applied political science. Because of the equivalence between causality (cognition, theory) and action (practice), a reductionist methodology suffices, insofar as it can contribute to the identification of causalities. Thus, world cognition and world change virtually coincide. Once one has empirically identified causalities, one can use this knowledge within e.g. an applied political science by inverting causal propositions into instructions for action.

One of the most important methodological insights that has been specified and reconstructed in this book is that there is no equivalence between causality and action. Therefore, no practical or even technical proposals for regulation can be justified with causal analyses alone, as is assumed within the explanative-prognostic or the Platonic-Galilean as well as by the perestroikans of the Aristotelian tradition.

The explanative-prognostic or the Platonic-Galilean tradition has produced no methodology with the help of which one can conduct *legitimating discourses.* Not only are the fundamental limitations of empirical methodology worked out by Weber still valid, but the philosophy of technology has justified why not even a technical discourse is possible by means of empirical methodology alone, namely because these discourses also need prescriptive elements (norms and regulations).

While the normative rational choice approach allows empirical knowledge to be integrated into a technical discourse, the normative assumptions (usually liberal and utilitarian norms) must be justified elsewhere.

C. Problem-driven, problem-based methodology: applied phronesis as a phronetic political science methodology

Popper's and Weber's view that social-technological regulations can be justified by inverting causal propositions is not only shared by the explanative-prognostic or the Platonic-Galilean tradition, but also by the Aristotelian tradition. In American pragmatism, too, usefulness and truth coincide.

Despite the recourse to Aristotle, the phronetic perestroikans do not succeed in overcoming the gaps in the practical discourse (7th chart, section 9.4.7, and 8th chart, section 9.4.8). The reduction of practical questions to the ability to act, to the skill of subjects, is not enough or is a reductionist narrowing, just like the scientistic scientists' reduction of the practical to the technical.

Since antiquity, existing norms and regulations within practical philosophy and political philosophy have been questioned and alternatives formulated. Validity claims must be both legitimized and further developed. Simply adopting this, as the phronetics believe, does not do justice to the demands of practical science. Applied phronesis goes in the right direction, but it is not enough, since it does not even approach the complexity of Aristotelian practical philosophy. Even more, the phronetics do not even reach the methodological level established by Aristotle in practical and political philosophy.

8.3.2 Plea for an independent practical methodology

The search for scientific answers to political-practical questions is the core of my work. I take note of empirical (descriptive, explanatory and prognostic) answers, but my claim is to formulate practical (normative, pragmatic, technical) answers with scientific tools (concepts, propositions, theories, logics, modes of argumentation, methods and methodological approaches). In doing so, it is necessary to use existing scientific tools, to develop some further, and to discover and justify new ones.

The further development of a practical (normative, pragmatic and technical) methodology that complements the empirical (descriptive, explanatory and prognostic) methodology is imperative. In my view, practical (normative, pragmatic and technical) knowledge can also be justified rationally. This requires practical political science and a practical methodology, since neither an empirical nor an interpretative methodology can legitimize norms and rules.

A practical (normative, pragmatic and technical) methodology differs in principle from an empirical (descriptive, explanatory and prognostic) methodology. I try to make this clear as well (2nd chart). Therefore, it is also inappropriate to strive for less stringency like in empirical questions, because this cannot have a positive effect on practical questions, but only a negative one on empirical questions. In practical discourses, however, one is dependent on good empirical descriptions and explanations.

Neither the normative methodology of the scientistic scientists, the normative rational choice theory, nor the applied phronesis of the phronetic perestroikans can replace the variety of *genuinely* practical methodology presented in antiquity and innovatively developed since then. As I have shown, in both of the above methodological approaches, existing standardizations and regulations are not challenged. Only the constituent, not the normative, character of norms is taken into account. Norms as presuppositions are not questioned or legitimized. But this is exactly the main task of practical discourses of validity: to legitimize standardization and regulations.

One can use the rational choice approach to formulate more effective alternatives or to criticize ineffective action. Applied phronesis enables one to expose or resolve unjust power relations. However, the criteria and values by which this must be done cannot be justified. The advantage is that one can critique within a political system and undertake therapies according to existing regulations, since constituent norms are maintained through internalization and reproduction. The drawback is that one cannot legitimize principled alternatives or extensions. Regulative (prescriptive, imperatives) norms and rules need to be constantly legitimized; only then can existing norms be replaced by alternative or extended norms and rules. For this, one needs all the practical discourses mentioned above (discourse of values, discourse of goals and discourse of means; chart 8, section 9.4.8).

In contrast to empirical discourse, a *genuinely* practical discourse that satisfies current logical-analytical standards of argumentation is still a *desideratum* in political science. This applies both to purely technical (instrumental) questions (*means discourses*), which involve the choice of means (*instruments of action*) rather than ends (*maxims of action or strategies of action*), and to normative (*value discourses*) and pragmatic (*target discourses*) discourses, in which maxims of action and strategies of action are discussed and practical (normative, pragmatic or technical) judgments (*value judgments*) are made about actions and social facts.

The biggest *desideratum* is a handbook with a practical (normative, pragmatic and technical) methodology. To my knowledge, such a handbook does not yet exist. Within political philosophy/political theory, there is no sufficient distinction between practical methodology and the practical knowledge (theory) generated by this methodology, both being discussed in the same treatises. The topics that would need to be discussed in a *handbook of practical methodology* have been listed in various charts created here (chart 2, 7, and 8, section 9.4.2, 9.4.7, and 9.4.8).

8.3.3 Further development of political philosophy/political theory into a practical political science in which practical knowledge can be rationally justified

This book has argued for a *practical political science.* However, one cannot generate practical (normative, pragmatic and technical) knowledge using an empirical methodology. Therefore, a methodological extension in form of a practical (normative, pragmatic and technical) methodology is necessary; reconstructing, specifying and developing it is a main goal of my work. With the help of a practical methodology, one can justify maxims of action (constitutional norms, ethical-moral norms) within normative discourses of values, strategies of action (rules, goals and purposes) within pragmatic discourses of goals, and instruments of action (technical regulations) within technical discourses of means (9th chart, and 10th chart, section 9.4.9, and 9.4.10).

A practical political science would differ from *political philosophy/political theory* in that there would be a *separation* between *practical methodology* and *practical theory*; both together belong to and constitute practical *knowledge.*

Practical knowledge in the form of liberal, libertarian, conservative, (neo)Marxist or socialist regulations or theories can be reconstructed, criticized, justified or legitimized with the help of a practical (normative, pragmatic and technical) methodology within technical means, pragmatic goal and normative value discourses. Due to the limitations of scientific methodology, this is always *hypothetical knowledge.* Furthermore, this knowledge has an advocatory quality. One can identify scientific reasons for practical standardizations and regulations. *Definitive decisions* for all citizens can only be made by the corresponding political institutions. Only then is legitimacy secured, and the political system as a whole must also assume *liability* for the consequences, good and bad.

With an empirical (descriptive, explanatory and prognostic) methodology, adequate descriptions, explanations and prognoses about political reality can be generated, but no practical ones, not even technical-instrumental normative or regulatory proposals. Genuinely practical discourses (discourses of values, goals as well as means) require a practical methodology complementary to an empirical methodology. The scientific theoretical arguments for this have been explained in detail.

8.4 Epistemic norms or values constitute scientificity: enabling science, demarcation between science and non-science

Popper thought that to distinguish science from pseudo-science one criterion is sufficient. In my opinion, an extremely complex epistemology and methodology consisting of a plethora of general, abstract or universal as well as concrete, special or local *conditions or criteria* enables to *generate scientific knowledge* as well as to draw a *demarcation line* between *science* and *non-science.* These epistemic or constituent norms and values guarantee *scientificity*. Furthermore, they make it possible to show the limits and possibilities of political science research.

Epistemology, in my opinion, usually contains the general, abstract or universal conditions or criteria of knowledge (section 8.4.1). Within *methodology*, the concrete, local or special conditions of knowledge are discussed (section 8.4.2). Philosophers usually focus on the general conditions (chapter 5), while individual disciplines focus on the special conditions (chapter 6).

8.4.1 Epistemology: general, abstract or universal conditions or criteria of knowledge

Empiricism and *rationality* are the overriding principles that scientific research must comply with, since scientific theories consist of a logical-mathematical formalism and an empirical interpretation of (political) reality. Science requires a rational formalization and an equally rational approach as well as empirical anchoring. Rational principles or postulates of rationality are fundamental not only in the knowledge of an *is* (factuality), in this case the knowledge of political reality, but also in the legitimation of an *ought* (normativity), or practical regulations within the political system.

The purpose of criteria is to assess the extent to which rational formalization and empirical anchoring have been successful. With the help of *postulates of rationality*, *general* criteria of scientific research are formulated so that methodological (argumentative, logical, linguistic and methodical) precision can be guaranteed.

The search for general conditions or criteria of knowledge is necessary to distinguish between a *rational knowledge,* or knowledge founded on scientific methodologies, on the one hand, and *knowledge from other sources* on the other hand. Interpretivists are also beginning to recognize the importance of criteria of knowledge (chapter 5).

For practical research, a plethora of other general criteria play a role. Every university, every scientific institute, every journal, every scientific project works out a multitude of general conditions. You can always find them under the heading “Our Philosophy”. I have briefly listed two examples from political science: Guide to Professional Ethics in Political Science, and DA-RT, or Data Access - Research Transparency (section 5.2.2).

In the chapter on epistemology, I also discussed the *ideals* that scientists should strive for and the *properties* that scientific propositions should have (section 5.3). I showed that scientistic scientists and perestroikans come together in one crucial point: *truth* and *utility* are seen as two sides of the same coin. The corresponding passages in Bacon and Popper for the scientistic scientists and James for the followers of American pragmatism show this.

The *scientistic scientists* try to recognize the political world for the first time with true *causal statements* about it. The inversion of causalities then enables *social-technological regulations* to change the world. The *phronetic perestroikans*, with recourse to American pragmatism, try to uncover useful *instructions for action* for the first time and are convinced that they have *recognized the political world* at the same time. Both must presuppose an *equivalence* between *truth* and *utility.*

The search for *universal truths* is by no means the goal of the scientistic scientists, as all their opponents claim. The *ideal of truth* was not abandoned, because rational discourse simply cannot be conducted without *bivalence*; but it is clear that there can only be *hypothetical* knowledge. *Antiveritative* positions are not convincing. Scientistic scientists, too, especially rational choice theorists, work with a *coherence theory of truth* and not with a *correspondence* theory, as the interpretivists assume.

The *ideal of truth* applies only within the descriptive-interpretative and the explanatory-prognostic tradition, where propositions are made about the world. Within the practical tradition, where norms and rules for changing the world are established, other ideals apply and propositions have other properties.

Practical discourse should be conducted according to *different ideals* because of the principled differences that exist between the descriptive-interpretative and explanative-prognostic tradition on the one hand, and the practical tradition on the other. The norms and rules founded here have *other properties* than empirical statements.

The following questions are discussed within *normative value discourses*: Why should something be done? *Ethics* should be based on the *ideal of rightness*, the ethical norms are therefore either *right* or *wrong*. The *ideal of justice* should be decisive within *politics*, political norms, better *maxims of action* are therefore either *just* or *unjust.*

Pragmatic goal discourses enable pragmatic justifications of goals and purposes. *Strategies for action* are discussed here, the main question being: *What for* should something be done? Here the *ideal of phronesis* is appropriate, rules designed here are either *wise* or *unwise.*

Technical means discourses enable the justification of means, the search for instruments of action is the focus: How exactly should something be done? The *ideal of efficacy* is important here; these concrete regulations are either *effective* or *ineffective* (charts 6, 7, 8, and charts 9, section 9.4.6, 9.4.7, 9.4.8, and9.4.9, as well as sections 4.2 and 6.7).

8.4.2 Methodology: concrete, local or special conditions or criteria of knowledge

The concrete, local or special conditions of knowledge are found, in my opinion, within methodology (chapter 6). Methodology is the center of the science war, hence the term *Methodenstreit*. In order to do justice to the complexity of the questions, I have ideally distinguished a further seven methodological levels in addition to the axiological, epistemic and ontological levels. The following is a brief summary of what results should be considered at each of these methodological levels.

A. Concept level

Concepts are very powerful tools for science. The fact that there are structural differences between empirical (descriptive-interpretative, explanative and prognostic) and practical (normative, pragmatic and technical) concepts was emphasized (section 6.4).

B. Sentence level

At the sentence level, three key insights were highlighted:

a. There is a *fundamental difference* between *statements* (assertion propositions, *Behauptungssätze*) and *norms and rules* (requirement propositions, *Forderungssätze*).

b. It is also important to distinguish between *regulative* (prescriptive, imperative) and *constitutive* (non-imperative) norms and rules.

c. One consequence of the Jørgensen dilemma is that normative, prescriptive sentences (requirement sentences, e.g. norms or rules) *cannot be truth-apt* (section 6.5).

C. Theory level

There are *analytical*, *empirical* (descriptive-interpretative, explanative and prognostic) and *practical* (normative, pragmatic and technical) *theories*. Theories depict the *content* of a science. Therefore, there are as many theories as there are forms of knowledge and scientific operations (chart 9 and 10, section 9.4.9 and 9.4.10).

However, theories can also have a *methodological function*. This is the case when one uses them to generate, or cites them as evidence for, other theories (section 6.6).

D. Logic Level

At the logic level, the main differences between the different *types of logic*, as identified in particular by Georg Henrik von Wright, were discussed. In *norm logic*, the focus is on *act-ought* (*Tun-Sollen*), in *deontic logic* on *is-ought* (*Sein-Sollen*). All *practical* norms and rules are part of an *act-ought* about what should be done, thereby striving for *regulation* of the world. *Statements about* norms and rules are empirical statements and are part of an *is-ought*. Statements about norms and rules provide an empirical description of political regulation. Empirical propositions (*is-ought*) are *truth-apt*, practical propositions (*act-ought*) are *not truth-apt*. (Section 6.7).

E. Argumentation level

The most important result of the argumentation level is that the scientistic scientists simply no longer even mention the *deductive-nomological* or *covering law model* that was so important in the philosophy of science in the nineteenth and twentieth centuries. This is the case because meanwhile the fundamental limits of deduction, like those of induction, are known.

At the macro level, causal regularities are demonstrated using correlation and regression methods and experiments, while causal processes are demonstrated at the micro level using qualitative-mathematical methods.

There are different empirical and practical ways of argumentation (chart 12, section 9.4.12), each of which has different possibilities and limitations (section 6.8).

F. Method level: qualitative-mathematical versus qualitative-interpretative methods – experiments versus testing

The central misunderstanding in the science war at the method level is the different uses of the word "qualitative". Qualitative methods were first located within qualitative research. These methods were based on hermeneutic, dialectic, linguistic-interpretative, phenomenological and structural methods and were summarized in corresponding handbooks. Since the 1970s, other qualitative methods (QCA, process tracing) have been developed, especially in the USA, which are based on (alethic) *logic* as well as on *mathematics. Set theory* in particular is used to determine causal processes. When scientistic scientists today refer to qualitative methods, they always mean these methods. In order to avoid misunderstandings, in my opinion it is necessary to add an additional term: qualitative-mathematical or qualitative-interpretative.

Also at this level, philosophers of technology have introduced a distinction between *experiment* and *test*, which is also important in political science: in the experiment, the *validity of a theory* is checked; in the test, the *fulfillment of a function* (section 6.9).

G. Level of methodological approaches

At the level of the methodological approaches, the possibilities and limits of the rational choice approach were explained. This is against the background that scientistic scientists see *rational choice normative theory* as the best practical approach.

I have listed two main points of criticism: first, that no distinction is made between positive and normative theory; secondly, that questions of validity in an allegedly normative theory were put as assumptions and not legitimized as conclusion, although one claims to present a normative methodology. Liberal and utilitarian norms cannot be legitimized with this approach because they are *assumed* and *not justified* (section 6.10).

The principled objections to both the practical approach of the scientistic scientists (normative rational choice theory) and to applied phronesis, the practical approach of the perestroikans, shows that both opponents do not have an adequate practical approach that satisfies current scientific standards. In my opinion, this deficiency can only be remedied by a further development of the practical tradition. Hence my plea for a *practical political science* (section 8.6).

8.5 External influences on science: non-epistemic norms or values within science

Society, politics, citizens and scientists influence science enormously from the outside due to different interests. These interests are reflected in the so-called non-epistemic norms and values. At the center of the discussion is the value judgment dispute, or the question of whether norms and values should have an influence on scientific studies at all or to what extent. In my opinion, several completely different axiological questions must be examined if one wants to avoid misunderstandings.

For decades now, the so-called value judgment dispute has been characterized by people talking past each other in a way that is rarely found in such a serious form in science. The value judgment controversy is the most contentious part of the science war. This is mainly due to the fact that different questions are mixed together. Therefore, in the fourth chapter on axiology, I have distinguished six axiological complexes of questions and discussed them separately:

1. *Value relationship*: the scientist’s value beliefs, the scientist’s relationship with his research object (section 8.5.1)
2. The value or *political* and *public relevance* of science (section 8.5.2)
3. *Values for science*, or criteria that ensure the authority of science (section 8.5.3)
4. Values as an *object of science* (values in the object area) (section 8.5.4)
5. *Value base*: non-epistemic norms and values that influence scientific results (section 8.5.5)
6. The problem of *value judgments in the narrower sense*: value-free empirical science is possible, but empirical justification of norms is impossible, while practical justifications are possible (section 8.5.6).

8.5.1 Value relationship: the scientist' value beliefs, the scientist' relationship to his research object

This set of questions revolves around norms and values as well as the researcher’s judgmental statements about the object of his investigation, which determine the choice of problem. Enthusiasm, vocation or passion and thus also the personal norms and values of the researcher on certain questions and research objects usually do not constitute a fundamental problem for an objective and value-judgment-free science or can be neutralized.

In this book, I was able to show that methodologically or technically sound scientific work can be carried out without the scientist's own evaluations determining the results of the work from the outset.

8.5.2 The value or political and public relevance of science

The relevance or value of science refers to the function of science for certain non-scientific interest goals, whether these goals emerge from the state or from social actors.

There is no contradiction between a methodologically stringent approach and the relevance of science. On the contrary, the stringency of the scientific approach justifies the actual importance of science for society.

8.5.3 Values for science or criteria that ensure the authority of science

First, there are *endogenous* or *epistemic values* that provide the scientific criteria and methodologies approved by the research community. These epistemic and methodological criteria are constantly developed further. Due to their complexity, they are divided and discussed here on ten methodological levels (chapters five on epistemology and six on methodology). Following Searle, one can speak of *constitutive* norms of science.

Second, there are *exogenous* or *non-epistemic values*, those circumstances in which science is optimally unfolded. These are, for example, questions about how science can best thrive with the help of an optimal research policy. They also include all regulative (prescriptive, imperative) norms and rules imposed by society and policy on science, as well as the tasks and goals that scientists themselves consider desirable and feasible. These non-epistemic values are addressed in detail in the fourth chapter on axiology.

8.5.4 Values as an object of science (values in the object area)

Norms and values can be the subject of scientific investigations. It goes without saying that political norms and values can become the *object* of empirical research. When norms and regulations become the object of empirical investigation, they lose their norm character: they are then an *is* (factuality) and not an *ought* (normativity). An empirical political science thus makes statements about the existence of the practical (normative, pragmatic and technical) regulations of a political system (section 6.6, 6.7 and 6.8).

8.5.5 Value base: non-epistemic norms and values that influence scientific results

The greatest dissent is on questions regarding the basis of values between representatives of the explanative-prognostic or the Platonic-Galilean tradition and a few representatives of the Aristotelian tradition and researchers who feel committed to American pragmatism or the Frankfurt School.

Proponents of the Platonic-Galilean tradition hold that objective and value-free science is possible, and that values and norms do not necessarily affect the establishment of facts or the interpretation of data.

In my opinion, a precise elaboration of the philosophical foundations of political science in particular can prevent non-epistemic norms and values from unduly influencing scientific investigations.

8.5.6 The problem of value judgments in the narrower sense: value-free empirical science is possible, but also empirical justification of norms is impossible

This dispute concerns the self-understanding of the human and social sciences. The *problem of evaluative science* itself amounts to answering a normative question, namely the question of the task of science. Scholars within the Aristotelian tradition consider a rational justification of norms and rules to be possible. The scientists of the explanative-prognostic or the Platonic-Galilean tradition claim, in part by referring to Weber, that it is not possible to justify norms and values through social-scientific methods; the justification of social-technological rules for concrete purposes is affirmed.

Do we need a practical political science that formulates value judgments about its subject area, namely social reality? The scientific scientists answer this question in the negative, their opponents in the affirmative.

I answered this question in the affirmative. First, I show that there is a distinct practical methodology (2nd chart, section 9.4.2, and third column, 7th chart, section 9.4.7, and 8th charts, section 9.4.8). Second, I advocate a practical political science that differs fundamentally from both the scientistic scientists' and the perestroikans' ideas.

8.6 Evolution instead of revolution: the need for a new framework for the debate and methodological approaches to critical reception of, reflection on, and further development of philosophical tradition with a systematic intention

First, the polarized nature of philosophical debates about the foundations of political science will be described. This goes far beyond the usual level of polarization in the field (section 8.2.1). Second, the methodological approaches will be explained, with the help of which a constructive debate should again become possible. The revolutionary approach must, in my opinion, give way to an evolutionary approach. The following methodological approaches, which are applied to the reception and further development of philosophical traditions with systematic intent in this thesis, will be explained: identification of ideal-typical questions within philosophy of science, *ad fontes* reconstruction, and multilingual approach (section 2.3 and 2.4).

8.6.1 Failed scientific discussion: methodological struggles and polarizing science war from the emergence of the social sciences to the present day

The polarization on methodological issues within the social sciences differs very markedly and unfavorably from Aristotle's approach. The terms "method dispute" (*Methodenstreit*) and "positivism dispute" (*Positivismusstreit*), and of course "science war", testify to this; but many contributions are clear examples of talking past each other, something that is not found to anywhere near this extent in other areas of political science. Like *Kindergarten*, *Methodenstreit* is one of the few German foreign words that have found their way into the English language. Labeling methodological disputes as a quarrel (*Streit*) is a means of discrediting them in political science as well. Children quarrel, adults' debate seriously about the content. There are other negative phenomena as well: ignorance of methodological questions and a "just do it" pragmatism. Altogether, this has fueled a polarizing Manichean science war, from the emergence of political science until today (section 2.2).

In the following, I will go into the conceptual deficits of the science war (section A). Then I will show how the polarizing science war from the emergence of the social sciences until today has been favored by an unsuccessful reduction of complexity (section B). Finally, the counterproductive effects and unwanted collateral damage of the science war are briefly pointed out. The power and resource orientation, i.e. what I call the *power- and money-driveness* leads to polarization of discourse, politicization of science and moralization of politics (section C).

A. Conceptual deficits of the science war: talking past each other by ignoring and/or distorting

The science war is also caused by revolutionary concepts. The popularity of Kuhnian revolution metaphors is due to the fact that some scholars see themselves as revolutionaries who are determined to impose their paradigm, no matter what the cost. This leads to a great deal of collateral damage, which should actually be recognized to go against the self-imposed, scientific code.

Kuhn developed his terminology using the example of physics in order to describe the Copernican turn within physics in terms of the philosophy of science. However, this terminology is not suitable for describing the philosophical developments within political science; moreover, it leads to misunderstandings and acts like a fire accelerator. It has been suggested that revolutions or turning points have occurred in political science that have resulted in a complete readjustment and that, moreover, are *incommensurable.* Bacon was the first to claim a revolution with his *Novum Organon.* The phronetic perestroikans did so at the beginning of the 21st century. But there have been many innovations in the methodology since Aristotle's Organon, and it has never been a *tabula rasa*. The old methodologies, some of which were certainly renewed or developed, are still used, as are the new ones.

The many misunderstandings that led to the science war and, above all, make it so counterproductive, stem from the fact that philosophical (axiological, epistemological, methodological and ontological) questions are interrelated and that this is not sufficiently taken into account. Furthermore, when confronting opponents, static theses are primarily used, and Occam's razor is wielded energetically; but with it one can capture neither the complexity nor the dynamics of the methodological development.

An important characteristic of the science war since its emergence in the 19th century is that the opponents talk past each other or distort the position of their opponents – when they do not simply ignore it from the outset. At the start of the 21st century, the criticism came from the perestroikans, who, to put it bluntly, crafted a straw man that they could then easily take down. What is missing, as it is customary especially in the humanities tradition, is an *ad fontes* reconstruction based on original sources. Instead of a seamless historical reconstruction, only a superficial intellectual portrait is formulated. This is done by presenting the position of the scientistic scientist, if possible, with the help of a handful of theses. What remains is not an ideal type, but a cardboard cutout. This does not do justice to the power of the very differentiated and specialized logical-mathematical research methodology represented by the scientistic establishment. Even more regrettably, the many criticisms that this methodology quite rightly deserves are forgotten.

In short, the critique of the perestroikans is characterized by distortion. The scientistic establishment in turn almost completely ignores linguistic-interpretive research methodology; at best, its existence is referred to in footnotes (section 2.2).

B. Failed reduction of complexity: polarizing methodological science war from the emergence of the social sciences until today

The science war at the beginning of the 21st century has in common with the positivism controversy of the 20th century that the opponents talk past each other or distort the position of the other camp, when they do not completely ignore it.

Both sides in the methodological dispute make use of Kuhn's philosophy of science, which is why I refer to a *Kuhn narrative*. There are two versions of this: a *scientistic* and a *phronetic* Kuhn narrative. But the Kuhn narrative cannot be used to adequately explain the methodological development. In fact, this narrative is counterproductive, as it acts like a fire accelerator and contributes decisively to the many misunderstandings in the debate. The works of Georg Henrik von Wright and Imre Lakatos are better suited to present the development of philosophical traditions since antiquity.

In his pioneering work, Kuhn pointed out that not just rational, but also political, psychological and sociological reasons play a role in the adoption of new theories. In particular, he analyzed the development of physics, more specifically the (Copernican) *turn* from the Ptolemaic to the Copernican worldview. In doing so, he used new terminologies (paradigm, incommensurability, (Copernican) revolution, normal science) to describe and explain this revolution. These have been shaping not only the scientific disputes in all subjects but also the public discussion for decades, but unfortunately in a very undifferentiated way.

Thus, the term paradigm has become inflationary, used by everyone, but with very different meanings, frequently misleading. This is due in no small part to the vagueness of the term; Margaret Masterman identified 22 different meanings in Kuhn's book.

The misuse of Kuhn's concepts is very widespread; Kuhn's work might be the world's most quoted and least read book. Unfortunately, the most infantile meaning, that old things are bad and must be replaced by new things, has become very prevalent and favors every demand for renewal, new start, etc., no matter how platitudinous. Thus, in all areas, there are campaigns for the supposedly "new" against the "old" in place of objective, careful development: sentiment beats reflection.

Both revolutionaries and counter-revolutionaries like to use Kuhn's vocabulary. They position themselves as Manichean warriors who can distinguish precisely between light and darkness, good and evil: *tertium non datur*. As a result, discourse with dissenters is virtually impossible from the outset, since dissenters are presented only as uneducated, morally neglected straw men: Carl Schmitt's friend-foe scheme can be seen here in perfection. The common basis of left- and right-wing Hegelians becomes visible.

C. Counterproductive effects of the science war: power and resource orientation lead to polarization of discourse, politicization of science, and moralization of politics

Now the unwanted collateral damage of Kuhn's revolutionary imagery will be summarized. In my opinion, the struggle for resources such as power between scientists and scientific organizations, money for research, publication opportunities, positions at universities – what I call *power-* and *money-driveness* – has, in my opinion, fueled the science war since the emergence of political science in the U.S.A. In contrast to Aristotle and Plato, the overwhelming majority of political scientists have been working under precarious economic conditions since the discipline came into existence (section 2.1.3).

Since Kuhn published his work, there is little doubt that such non-academic disputes also have an influence on the organization of academic organizations and even on academic results. Power and resource orientation have influenced the methodological discussion in a very counterproductive way: talking past each other, ignoring, pluralistic habitus and reductionist practices are some of the preferred means.

As a rule, those who make apodictic judgments try to legitimize them with the authority of science and morality: politicization of science and moralization of politics is the result. The idea that one can determine "the truth" or "the good" beyond doubt is a pre-modern chimera, but one that people are unwilling to give up.

8.6.2 Methodological approaches to the critical reception of and further development of philosophical traditions with a systematic intent

In my opinion, the revolutionary approach must give way to an evolutionary approach. The approaches of von Wright and Lakatos are much better suited than that of Kuhn to trace the evolution within the sciences. Here is a summary of the arguments that justify why scientific progress proceeds better by evolution than by revolution (section A).

The following methodological approaches, which were recommended in this work for the reception of, reflection on and further development of philosophical traditions with a systematic intent, will be explained: determination of ideal-typical questions within the philosophy of science, reduction of complexity with the help of four ideal-typical thematic complexes into ten vertical and three horizontal planes (section B), *ad fontes* reconstruction (section C), and multilingual approach (section D).

A. Evolution instead of revolution: traditions, research programs and innovations: Kuhn versus von Wright and Lakatos

Kuhn's terminology is ubiquitous in political science, and not only in methodological matters. Goodin speaks of several revolutions within American political science, at the core of which is the introduction of new methodologies. Thus, I think it is better to speak of the introduction of methodological research programs. These have not completely displaced existing research programs, nor is there no exchange between researchers using different research methodologies. On the contrary, these methodologies are complementary, and some of them are applied by the same researcher. In short, *complementarity*, but above all coexistence of methodologies rather than incommensurability should characterize the methodological field. Therefore, von Wright's terminology, alongside that of Lakatos, is more appropriate to reconstruct the development of methodology from antiquity to the present day.

B. Reduction of complexity: four ideal-typical thematic complexes on ten vertical and three horizontal levels

The formation of traditions within the discipline of political science, as shown not least by the so-called *Methodenstreit*, proceeds on the basis of axiological, epistemic, methodological and ontological preferences.

An overview of the fundamentals of the discipline still requires an adequate reduction of complexity in addition to an adequate reconstruction. In order to cope with this, I also resort to an ideal-typical division of contents. In chapters three through six, axiology is treated independently, in addition to the "holy trinity" of ontology, epistemology, and methodology. Since axiological, epistemological, methodological, and ontological questions have such a distinctive importance in the methodological controversy, I think it is essential to keep these four ideal-typical question complexes apart (section 2.5). For these four sets of questions, the main issues have been formulated (section 1.2). Detailed answers to them can be found in the main part of the book, in chapters three to six.

C. *Ad fontes* reconstruction with systematic intent

The systematic intent was to trace currently important as well as controversial philosophical questions that constitute the foundations of the discipline. It was also important to me to pay attention to the genesis of these questions and to reconstruct the different positions of classics that put these questions on the agenda for the first time or that introduced important further innovations.

Talking past each other by ignoring and/or distorting can be avoided first and foremost by following an *ad fontes* approach, because this is the only way to allow constructive and productive discussion. All opponents are thus forced to deal with the sources (especially methodological manuals and classics of the corresponding philosophical tradition) of their opponents (overview in section 2.3, details in chapters three to six).

D. Multilingual approach

The philosophical foundations have been discussed in different languages for more than two millennia. In the second chapter, we discussed why different methodological approaches are therefore also necessary to analyze the most important questions adequately. It was assumed that language, along with mathematics, is the most important tool for scientists. Publication in English is absolutely necessary to enable global engagement. However, multilingualism increases the power of language as a tool of science.

A multilingual approach alongside an *ad fontes* reconstruction can additionally contribute to the avoidance of misunderstandings. Linguistic pseudo-problems can therefore be revealed or avoided through multilingualism. Furthermore, multilingualism improves the reliability of scientific results when the results are expressed simultaneously in English and in another language, and contributes to faster and, above all, more adequate problem solving. In this way, talking past each other as well as non-factual controversies can be overcome (section 2.4). The factual differences can be resolved with the help of complementarity.

However, a multilingual approach ensures that *new perspectives* bring fresh air into the discussion. The practical tradition is justified here with many arguments that come from German philosophers (Hans Albert, Walter Ernst Otto Dubislav, Rainer Enskat, Otfried Höffe, Klaus Kornwachs, Hans Poser, Wolfgang Wieland) but have not yet played a role in the discussion of philosophy of science.

A multilingual *ad fontes* reconstruction of the philosophical foundations of the discipline is especially necessary because an evolution and further development of these traditions is the better course of action. Arguments were made to show that the revolutionary alternative favored by many is not nearly as powerful. Therefore, the focus should lie on the following terms: evolution, research programs, ideal types, innovations, multilingual *ad fontes* reconstruction, further development, rather than on paradigm or revolution.

8.7 Plea for an institutionalization of philosophy of political science as a subdiscipline

I argue for the institutionalization of a subdiscipline of philosophy of political science, as there exist areas of study that can only be adequately explored within this framework. In my view, the philosophy of political science should have at least two major fields of inquiry: Firstly, it should aim to identify the scientific limitations and possibilities of the discipline, while also striving to further developing its philosophical foundations (section 8.7.1). Secondly, it should work towards preventing the politicization and moralization of political science (section 8.7.2).

8.7.1 Identifying scientific limitations and opportunities and further developing the philosophical foundations of political science

The relevance of political science in both society and practical politics will increase if all three methodological traditions are applied up to date. The descriptive-philological tradition enables us to advance in the description of political phenomena and the interpretation of political symbols (text, images, audio and video), especially compared to sociologists. The explanative-prognostic tradition is needed to establish its presence alongside economics. The practical tradition makes it possible to catch up with the jurists.

A subdiscipline such as the philosophy of political science could thus contribute to strengthening political science as an independent discipline and establishing a robust philosophical foundation for it. It would aid in ensuring that political science research is not only grounded in empirical evidence but also in solid philosophical principles. This could result in increased recognition and relevance of political science in society.

Moreover, the philosophy of political science could foster interdisciplinary cooperation by forging connections with other fields such as economics, law, philosophy, and sociology. This would facilitate fruitful exchanges and mutual enrichment between disciplines.

8.7.2 Tasks of the subdiscipline "philosophy of political science": Determination of scientific limits and possibilities as well as further development of the philosophical foundations of political science

The most important tasks of *political science* are to generate *political* (both *empirical* and *practical*) *knowledge* that can be used to adequately capture the complexity of political reality on the one hand, and to justify complex standardization and regulations for political control on the other.

Adequate reduction in complexity can do just that. A revolutionary discourse is counterproductive because it leads to the *politicization* and *moralization* of science. In the end, this achieves exactly the opposite, because it produces black and white thinking and thus promotes an infantilization of the discourse.

The establishment of a sub-discipline called 'Philosophy of Political Science' can help to ensure that these debates are perceived as a sign of *vitality* in the future. When it is no longer a question of who wins and who loses, but rather of making visible and developing the foundations of political science. In short, evolution, not revolution, is what is needed.

9 A curriculum for the subdiscipline "philosophy of political science": bachelor’s (undergraduate) and master’s (graduate) seminars, course templates and extensive charts

The goal of a *curriculum* is to regulate learning goals and learning content of a study program. This book represents the first attempt, to my knowledge, to create a curriculum for the subdiscipline “philosophy of political science”. It is important to emphasize that it is not a curriculum for the subject of political science or for other political science subdisciplines.

First, it develops a bachelor's seminar for undergraduate study with ten courses. Second, I present a master's seminar for graduate study with seven courses; both preferably of four hours each. Third, I have created thirteen charts suitable for use in both seminars.

In the following, I first discuss why a *curriculum* for the subdiscipline "philosophy of political science" is necessary and what should be considered (section 9.1). Secondly, it will be shown how basic course templates for a bachelor's seminar are structured and should be used. After that, one can find the outline of the ten courses (section 9.2). In the following section, the advanced course templates for a master's seminar are discussed before the seven courses are listed (section 9.3). The chapter is then concluded with 12 charts (section 9.4).

The charts of the book allow for a quick overview of the most important topics and for a reduction of the complexity. The first charts divide the philosophical premises of political science research ideally into ten levels. The second chart presents the three philosophical traditions (empirical-descriptive, explanatory-prognostic, and practical) whose complementarity has been demonstrated. The other charts provide an overview of complex as well as special disputes and topics. A reduction of complexity is to be achieved by listing the arguments and thus making their coherence visible.

9.1 Curriculum for the subdiscipline “philosophy of political science”

Scientia potentia est (aphorism attributed to Francis Bacon). The word “*scientia*” in Latin means both knowledge and science. Therefore, in my opinion, an accurate translation would be: *scientifically based knowledge is power.* In Bacon’s day, scientifically based knowledge competed with many other forms of knowledge. Science at that time largely stood in the shadow of religion. This has changed radically.

Since the 19th century, there has been a *scientification* of all of life, including politics. Science, and the knowledge it creates, is dominant both in politics and in society. Consequently, in today’s world not just scientists, but *every citizen*, regardless of their profession, should be aware of the limits and possibilities of science. The following didactic processing should enable an intensive discussion of all interested parties.

Science is organized skepticism (Merton 1979); the foundations of science consist in a *systematization* and *organization of doubt* and *mistrust*. Unlike religion, science does not offer *absolute truths*, but only *hypothetical truths* due to the fundamental limitations of scientific research (section 5.4). Philosophy of science is the subdiscipline within which the limits and possibilities of science in general are discussed. In this book, this is demonstrated using the example of political science. It is important to always keep in mind that some fundamentals are recognized in the scientific community (*communis opinio doctorum*), while others are questioned, since science is not a closed dogma but *organized skepticism*. Unlike Christianity for example, which can draw on a central foundational work, the Bible, science does not have a completed foundational work. The existing foundations are discussed and further developed in particular within philosophy of science.

The structure of this book, particularly chapters three through six, is intended, firstly, to reflect the *logical geography* or structure of the *subdiscipline* philosophy of political science. Secondly, the most important questions regarding the current *state of research* are discussed, and suggestions for the *further development* of the subdiscipline are provided.

I have developed a *basic* (undergraduate) *seminar* and an *advanced* (graduate) *seminar*. This should enable two things: firstly, these courses serve as a guiding thread, a *user's manual* for this book. Secondly, they contain *didactic course templates* for two different target groups: beginners and advanced students.

Successful completion of these seminars is achieved by working through the recommended reading sources from the book in all courses and the writing of a seminar paper on a special topic for each seminar; in the undergraduate course the length should be about ten pages (5.000 words), in the graduate course about twenty pages (10.000 words).

The further *bibliographical references* should firstly be used to check the interpretations given in these book based on the original passages. Second, the literature can be used to formulate other answers. Thirdly, these references and the list of references at the end of the book serve as a starting point not only for seminar papers, but also for more extensive work, e.g. bachelor's, master's or doctoral theses.

For reasons of space, I have only included the author, the year of publication and the title of the article or book in the course templates in the information on further reading. The complete bibliographic information can be found in the references at the end of the book.

9.2 Bachelor's or undergraduate seminar

The basic courses of the bachelor's seminar aim at elaborating the manifold philosophical (axiological, epistemic, methodological and ontological) foundations of political science. These fundamentals decisively determine scientific work as its prerequisites, but they are actually seldom explicitly stated. Above all these fundamentals, scientists generate scientifically based knowledge through methodologies. But, all philosophical four foundations determine both the limits and the possibilities of science. In short, the aim of the bachelor seminar is to provide an introduction to the subdiscipline *philosophy of political science*. Now a brief overview of the basic courses of the bachelor seminar:

- 1. Basic (undergraduate) course (bachelor seminar): introduction to the philosophy of (political) science.
- 2. Basic (undergraduate) course (Bachelor seminar): the science war (*Methodenstreit*).
- 3. Basic (undergraduate) course (bachelor seminar): methodological approaches to the critical reception of philosophical traditions.
- 4. Basic (undergraduate) course (bachelor seminar): ontological foundations.
- 5. Basic (undergraduate) course (bachelor seminar): non-epistemic values within the sciences, the value judgment dispute.
- 6. Basic (undergraduate) course (Bachelor seminar): tasks or objectives of (political) scientific research.
- 7. Basic (undergraduate) course (bachelor seminar): criteria of rationality, epistemic values, or general conditions or criteria of knowledge.
- 8. Basic (undergraduate) course (bachelor seminar): forms of knowledge.
- 9. Basic (undergraduate) course (bachelor seminar): concept, sentence, theory, logic and argumentation level.
- 10. Basic (undergraduate) course (bachelor seminar): method level and the level of methodological approaches.

In the following, all basic courses of the bachelor's seminar are briefly introduced.

9.2.1 Basic (undergraduate) course: introduction to philosophy of (political) science

The first basic course aims to provide a general introduction to the topic. The reading sources in the book are listed, as are the most recent articles on the subdiscipline philosophy of political science. In addition to the most important questions of the discipline, two other topics should be discussed: the importance of the philosophy of science for science, and the contradictions within political science.

The 19th century witnessed the emancipation of individual sciences from philosophy. But this process came to an end in the first half of the 20th century. At the same time, a new, special discipline was established within philosophy: *philosophy of science* (Humphreys 2016, Lohse/Reydon 2017, Okasha 2016 [2002], and Rosenberg/McIntyre 2020 [2000]).

The *general philosophy of science* analyzes the rational limits and possibilities of the sciences or of the science system. The fundamentals of science and scientific research are at the center of all its investigations. It discusses the importance of empiricism, methodology, practical relevance, rationality and values. Its central questions relate to the foundations as well as the limits and possibilities of scientific research. In short, it deals with guaranteeing *scientificity*.

At first, these issues were explained using the example of physics. Gradually, more disciplines joined in, including the social sciences (Cartwright 2014, Kincaid/Ross 2009, and Kincaid 2012, Humphreys 2016, Lohse/Reydon 2017). The 20th-century differentiation of philosophy of science has finally come to political science, one of the last disciplines to be reached by it (McIntyre/Rosenberg 2017, Kincaid/Van Bouwel 2023). On the one hand, there is a *philosophical awakening*; Moses (2020) speaks of a methodological awakening. On the other hand, this *philosophy of political science* still leads a *shadowy existence* (Lauer 2021b, Kincaid/Van Bouwel 2023a).

Goal

Present the subdiscipline philosophy of science using the example of political science.

Questions

- *Ontological* foundations: What is the *subject area* of political science as part of the science system? What kinds of *problems*, what *entities*, *properties*, *phenomena*, *relations* or *structures* are addressed within political science?
- *Axiological* foundations: What *tasks* and *goals* can and should political scientists pursue? To what extent can and should political *values* be researched? What *value* should scientific *results* have for *state* and *society*?
- *Epistemic* foundations: What basic *epistemic assumptions* do political scientists make? What *results*, political *knowledge*, or political *theories* can political science generate? How can political scientists *justify* knowledge? What *forms of knowledge* can they generate? What *values* must political science research satisfy? What are the *epistemic limits* of (political) science research and knowledge generation? What *ideals* does science strive for?
- *Methodological* foundations: Which political science *methodologies* can guarantee scientificity? What are the *limits* and *possibilities* of these methodologies? Which political science methodologies are used?

Reading sources

Chapter 1, section 2.1 and 8.1, chart 1, section 9.4.1.

Further references

Dowding, Keith, 2016: The Philosophy and Methods of Political Science.

Herfeld, Catherine, 2017: Philosophie der Politikwissenschaft.

Jensen, Magnus Rom/Moses, Jonathon Wayne, 2021: The state of political Science, 2020.

Kincaid, Harold/Van Bouwel, Jeroen, 2023a: Putting Philosophy of Political Science on the map.

Lauer, Johann, 2021b: Philosophy of political science. Prolegomena of a neglected subdiscipline.

McIntyre, Lee/Verbeek, Bruno, 2017: Why is there no Philosophy of Political Science?

Moses, Jonathon Wayne, 2020: Ontologies, Epistemologies and the Methodological Awakening.

Pozzoni, Gianluca, 2020: What, If Anything, is the Philosophy of Political Science?

9.2.2 Basic (undergraduate) course: science war (*Methodenstreit*)

The *philosophical foundations* of science have been discussed since ancient times, and several philosophical *traditions* have emerged. Since the 19th century there have been bitter disputes between the different traditions, giving rise to a *Methodenstreit* and, in the 21st century, even of a *science war* (Flyvbjerg 2001: 1).

Von Wright (1971) speaks of the *Aristotelian* and *Galilean traditions*, Moses and Knutsen (2019 [2007]) distinguish between *constructivists* and *naturalists*. These two books are recommended because, firstly, they deal with the most important issues in an exemplary manner, and secondly, because they also identify how the two traditions complement each other. Furthermore, they point to ways how the conflicts between the traditions could be overcome.

In this book, as well as in an earlier works (Lauer 2017, 2021a), I discuss the science war with particular reference to the situation at the beginning of the 21st century. Drawing on the work of Lakatos (1978), Moses and Knutsen (2019 [2007]) and von Wright (1971), I advocate a further development of the above distinctions and justify a separation between *three traditions*: a descriptive-interpretative, an explanatory-prognostic and a practical tradition (chapters 3-6, chart 2, section 9.4.2).

In the second basic course, the most important terminology, deficits and points of contention of the opponents in the science war are to be worked through.

Goal

Show the most important concepts, deficits and points of contention of the opponents in the science war (*Methodenstreit*).

Questions

- How did this science war *come about*?
- Who are the *opponents* in the science war?
- What are the most important *concepts*, *deficits* and points of *contention* in the science war?
- Which different axiological, epistemic, methodological and ontological assumptions or procedures do they prefer?

Reading sources

Section 2.2 and 8.5, charts 2, 3 and 4, section 9.4.2, 9.4.3 and 9.4.4.

Further references

Box-Steffensmeier, Janet M./Brady, Henry E./Collier, David, (ed.), 2010 [2008]: The Oxford Handbook of Political Methodology.

Bevir, Mark/Rhodes, Roderick Arthur William, (ed.), 2016: Routledge Handbook of Interpretative Political Science.

Flyvbjerg, Bent, 2001: Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again.

Flyvbjerg, Bent/Landman, Todd/Schram, Sanford F., (ed.), 2012: Real Social Science. Applied Phronesis. Cambridge: Cambridge University Press.

Gross, Paul/Levitt, Norman, 1994: Higher Superstition.

King, Gary/Keohane, Robert Owen/Verba, Sidney, 1994: Designing Social Inquiry. Scientific Inference in Qualitative Research.

Lakatos, Imre, 1978: The Methodology of Scientific Research Programmes: Philosophical Papers Volume 1.

Lauer, Johann, 2017: „Methodenstreit“ and Political Science. Methodological Science War at the Beginning of the 21st Century between the scientistic Establishment and phronetic Perestroikans.

Lauer, Johann, 2021a: Methodology and political science: the discipline needs three fundamentally different methodological traditions.

Moses, Jonathon Wayne/Knutsen, Torbjørn Lindstrøm, ^{3}2019 [2007]: Ways of Knowing. Competing Methodologies in Social and Political Research.

Schram, Sanford F., 2005: A Return to Politics. Perestroika, Phronesis and Postparadigmatic Political Science.

von Wright, Georg Henrik, 1971: Explanation and Understanding.

9.2.3 Basic (undergraduate) course: methodological approaches to the critical reception of the philosophical tradition

In the third basic course, the necessity of using hermeneutic-interpretive methods to study original sources will be elaborated. The importance of an *ad fontes* reconstruction as well as a multilingual approach for overcoming misunderstandings should be recognized.

Science is characterized by a *methodical* and *systematic* approach; therefore, a thorough discussion must include a listing of the methodologies with which one wants to proceed. Firstly, the methodology enables the researcher himself to systematically grasp a factual situation. Secondly, the demonstration of the methodology facilitates the scientific discussion; it enables other scientists to understand the reasoning.

A number of *hermeneutic-interpretative methods* are necessary if one wants to interpret sources correctly. Above all, the further reading serves to refer to textbooks in which these methods are presented (Bevir/Rhodes, 2016, Blatter/Haverland/van Hulst 2016, Creswell 2013 [1998], Flick 2008 [2002], Yanow/Schwartz-Shea 2014 [2006]).

I find the *explication* developed by Carnap (1963 [1950]), which is also used in this book to clear up misunderstandings, very effective and advanced (section 2.3.2).

The *science war* within philosophy of science is characterized by a “talking past each other” and by many *misunderstandings*. In order to avoid this, I recommend heeding the call, which first arose during the Reformation in the 16th-century Europe, to deal not only with secondary literature but with primary literature: “to the sources”, *ad fontes*, is as highly recommended and effective today as it was at that time.

I also recommend a *multilingual approach*. In order to achieve recognition in today’s globalized world, publication in English is a basic requirement. Despite this, or precisely because of this, a multilingual approach is necessary in order to avoid misunderstandings and introduce new perspectives (Lauer 2021e).

Goal

Emphasize the need to study original sources using *hermeneutic-interpretative* methods. Demonstrate the importance of *ad fontes* reconstruction and a *multilingual* approach to overcoming misunderstandings.

Questions

- With which methodological approaches and which *hermeneutic-interpretative methods* can one achieve an adequate overview of this complex of topics?
- What are the advantages and disadvantages of *conceptualization* with the help of *explication* in comparison to *definitions*?
- What is an *ad fontes* reconstruction?
- Can a *multilingual approach* contribute to a better understanding?
- Can *multilingualism* increase the performance of language as an instrument of science as well as the *reliability* of scientific results?

Reading sources

Section 2.3, 2.4 und 2.5.

Further references

Bevir, Mark/Rhodes, Roderick Arthur William, (ed.), 2016a: Routledge Handbook of Interpretative Political Science.

Blatter, Joachim/Haverland, Markus/van Hulst, Merlijn, (ed.), 2016d: Qualitative Research in Political Science. Interpretive and Constructivist Approaches.

Carnap, Paul Rudolf, ^{2}1963 [1950]: Logical Foundations of Probability.

Creswell, John, W. ^{3}2013 [1998]: Qualitative Inquiry & Research Design. Choosing Among Five Approaches.

Flick, Uwe, ^{5}2014 [2007]: An Introduction to Qualitative Research.

Lauer, Johann, 2021e: Multilingualism or publication exclusively in English? The central importance of multilingualism for science, the example of the philosophy of knowledge.

Yanow, Dvora/Schwartz-Shea, Peregrine, (ed.), 2014 [2006]: Interpretation and Method. Empirical Research Methods and the Interpretative Turn.

9.2.4 Basic (undergraduate) course: ontological foundations

The first basic course was a *general introduction* to the subdiscipline *philosophy of political science*. In the second course, the most important *controversial topics* were discussed, and in the third basic course, the *methodology* with which one can best familiarize oneself with the subdiscipline. In the seven remaining basic courses of the bachelor's seminar, the *philosophical* (axiological, epistemic, methodological and ontological) *foundations* will be presented, starting with the *ontological* foundations.

In the course of the 20th century, ontology, metaphysics and sociophysics have been sorted out by mainstream science as well as within philosophy. At the turn of the 20th to the 21st century, this thread is now being picked up again (Popper 1972, Popper/Eccles 1977). Surprisingly, it is also being picked up by scientists who were shaped by the very traditions that believed that one could dispense with these questions without a loss to science – and even that discussing them was detrimental to science (Popper 1968 [1934]). In short: *ontological questions*, i.e. questions about the subject area of political science, about what should be recognized or changed, cannot be ignored (Bevir 2010 [2008], Hay 2011 [2009]).

The limits and possibilities of a social ontology are also discussed in a current controversy. The point is not whether, but how a *social ontology* can generate important knowledge for science (Lauer, Richard 2019 and 2021; cf. Little 2009). Lohse (2020) and Kincaid (2021) submitted critical comments and suggestions for improvement.

Goal

Determine the importance of ontological questions for political science.

Questions

- What is the *subject* area of political science as a subfield of the scientific system?
- What kinds of *problems*, *entities*, *properties*, *phenomena*, *relations*, or *structures* are addressed within political science?
- What contribution can a *social ontology* make to social science?

Recommended reading

Chapter 3.

Further references

Bevir, Mark, 2010 [2008]: Meta-methodology: Clearing the Underbrush.

Bevir, Mark/Blakely, Jason, 2016: Naturalism and anti-naturalism.

Esfeld, Michael/Sachse, Christian, 2010: Kausale Strukturen. Einheit und Vielfalt in der Natur und den Naturwissenschaften.

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Hartmann, Nicolai, ^{3}1964 [1940]: Der Aufbau der realen Welt. Grundriß der allgemeinen Kategorienlehre.

Hartmann, Nicolai, ^{3}1949 [1942/1949]: Neue Wege der Ontologie.

Kincaid, Harold, 2021: Concrete Ontology: Comments on Lauer, Little, and Lohse.

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Lauer, Richard, 2022: Motivating a Pragmatic Approach to Naturalized Social Ontology.

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Moses, Jonathon Wayne, 2020: Ontologies, Epistemologies and the Methodological Awakening.

Popper, Karl Raimund, ^{5}1968 [1934]: The Logic of Scientific Discovery.

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Poser, Hans, 2016: Homo Creator. Technik als philosophische Herausforderung.

Wagner, Gerhard, 2012: Die Wissenschaftstheorie der Soziologie. Ein Grundriss.

9.2.5 Basic (undergraduate) course: non-epistemic values within the sciences, the value judgment dispute (*Werturteilsstreit*)

Epistemology, methodology, and ontology form the “holy trinity” of philosophy of science (Moses/Knutsen 2019 [2007]: 4). Axiological questions are so diverse and important, especially in the science war, that I agree with Creswell (2013 [1998]) that they should be treated independently alongside epistemological, methodological, and ontological questions. Because of the importance and complexity of axiological issues, I have devoted two basic courses and one advanced course to them. This basic course will focus on the so-called *value judgment controversy*, the sixth basic course will discuss the *tasks* or *goals* of (political) scientific research, and the third advanced course will analyze the limits and possibilities of a *practical political science*.

Weber, with probably his most read and most quoted article (Weber (2011 [1904]), triggered a debate between the opponents in the *Methodenstreit* that was just emerging at the time. This debate remains very controversial today. Besides Weber’s articles, I have included only 21st-century contributions in the further reading. In order to avoid the many misunderstandings, one should start with an *ad fontes* reconstruction of Weber’s position. Having done so, I think the following points should be noted.

Like all neo-Kantians of his time and most philosophers since antiquity, Weber presupposed a separation of *is* and *ought*, of *facts* on the one hand and *norms and values* on the other. At a time when empirically based social science was emerging, Weber rightly attached great importance to showing the *limits of empirical science*. He considered *objectivity* within the *empirical* sciences to be *threatened* should *non-epistemic* norms and values influence scientific analysis. This position is still shared by almost all scholars within the Platonic-Galilean or explanatory-prognostic tradition (Dasgupta 2009). I share this position, but not the associated view that a *practical social science*, i.e., the scientific justification of norms and values, is *not* possible. This topic will not be dealt with in this basic course but will be addressed thoroughly in the third advanced course.

For decades now, the *dispute* over *value judgments* (*Werturteilsstreit*) has been characterized by people talking past each other in a way that is rarely found in such a serious form in science (Lauer 2017). Not least because of this, the conflict has been referred to, not without reason, as a *science war* (Flyvbjerg 2001: 1). This is mainly due to the fact that different axiological questions are confused. Therefore, in the book I have discussed *six* non-epistemic, axiological sets of questions separately (section 4.1.3).

Goal

Identify the *non-epistemic* norms and values within the sciences that society and politics impose on science. Determine the *positions* in the value judgment dispute.

Questions

- What is the difference between *epistemic* and *non-epistemic* values?
- Is a *value-free*, *empirical* science possible?
- Can *values* be examined as *objects* of empirical science?
- To what extent do *norms* and *values influence* scientific results?
- What role do the *value beliefs* of the scientist play, i.e., the relationship of the scientist to his research object?
- What value or what political and public *relevance* does science have?

Recommended reading

Section 4.1, in particular 4.1.3, charts 3 und 4, section 9.4.3 and 9.4.4.

Further references

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Doppelt, Gerald, 2007: The Value Ladenness of Scientific Knowledge.

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Douglas, Heather, 2016: Values in Science.

Dupré, John, 2007: Fact and Value.

Elliott, Kevin C., 2017: A Tapestry of Values. An Introduction to Values on Science.

Flyvbjerg, Bent, 2001: Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again.

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Kincaid, Harold, 2023: Positivism and Value Free Ideals in Political Science.

Longino, Helen Elizabeth, 2002: The Fate of Knowledge.

Van Bouwel, Jeroen, 2023: How to deal with values in Political Science?

Weber, Maximilian Carl Emil , 2011 [1904]: “Objectivity” in Social Science and Social Policy.

9.2.6 Basic (undergraduate) course: tasks and objectives of (political) scientific research

When it comes to the tasks or objectives of (political) scientific research, both *non-epistemic* and *epistemic values* play a role. In my opinion, the opponents in the science war share broadly *similar ideas* about the *non-epistemic* tasks and objectives. But *large differences* between the opponents can be found in the methodology and thus the *epistemic* values with which these tasks and objectives are to be achieved. Both are discussed in this basic course.

Both the scientistic scientists and the perestroikans aspire to *world recognition* and *world change.* They both agree, I think, that knowing the world and changing the world are two sides of the same coin, i.e. that there *is* an *equivalence* between *truth* and *usefulness.* While the scientistic scientists first look for truth in the form of true causalities (Bacon 1990 [1620]), the perestroikans first want to discover the usefulness (James 1977 [1907]). Through simple inversion (Weber 1973c [1904] and Popper, 1984 [1972]) one can then see the other side of the coin. I have discussed these connections at length because they are seldom explicitly stated. If one wants to understand the limitations and possibilities of the different traditions, these connections must be discussed systematically.

This basic course also deals in detail with the *methodological approaches* with which the opponents wish to achieve their goals. Determining *correlations* and *causations* requires an extremely complex methodology. The path from correlation to causality is very challenging. The methodology of the perestroikans is, firstly, still in its infancy, and secondly offers no alternative to the methodology of the scientistic scientists; at most, I think, it provides a minor complement.

Goal

List *tasks* and *goals* and the different *methodologies* by which political scientists intend to fulfill their own tasks and goals.

Questions

- Which *tasks* and *objectives* should be fulfilled within (political) science?
- Which *methodology* should be used for the desired legitimation of knowledge?
- What approaches do scientistic scientists use to determine *causality*?
- How can *applied phronesis* generate knowledge and change reality?

Recommended reading

Section 4.2, charts 2, 3 and 4, section 9.4.2, 9.4.3 and 9.4.4.

Further references

Bacon, Francis, 2000 [1620]: The New Organon.

Brady, Henry E., 2011 [2009]: Causation and Explanation in Social Science.

Brady, Henry E./Collier, David, (ed.), ^{2}2010 [2004]: Rethinking Social Inquiry. Diverse Tools, Shared Standards.

Bunge, Mario, 1996: Finding Philosophy in Social Science.

Esfeld, Michael/Sachse, Christian, 2011: Conservative Reductionism.

Flyvbjerg, Bent, 2001: Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again.

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James, William, 1907: A New Name for Some Old Ways of Thinking. Lecture VI. Pragmatism's Conception of Truth.

King, Gary/Keohane, Robert Owen/Verba, Sidney, 1994: Designing Social Inquiry. Scientific Inference in Qualitative Research.

Popper, Karl Raimund, 1972: Objective Knowledge. An Evolutionary Approach.

Ruffing, Reiner, 2005: Einführung in die Philosophie der Gegenwart.

Salmon, Wesley C., 1989: Four Decades of Scientific Explanation.

Seawright, Jason/Collier, David, 2010 [2004]: Glossary.

von Wright, Georg Henrik, 1971: Explanation and Understanding.

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Weber, Maximilian Carl Emil, 1973e [1919]: Wissenschaft als Beruf.

Weber, Maximilian Carl Emil , 2011 [1904]: “Objectivity” in Social Science and Social Policy.

9.2.7 Basic (undergraduate) course: criteria of rationality, epistemic values, or general conditions or criteria of knowledge

The search for general *conditions* or *criteria* of knowledge forms a focal point within *epistemology* or gnoseology. In his dialogues Meno (2011 [4th century BCE]) and Theaetetus (1991 [4th century BCE]) Plato was primarily concerned with developing *conditions* or *criteria* with the help of which one could distinguish between *belief* and *knowledge*.

Edmund Gettier (1963) follows this discussion and has shown in an essay of only two pages that *truth* and *justification* alone are not sufficient. He formulates two objections, according to which a belief could also be *true*, first, *by coincidence* or, second, even on the basis of *false premises*. Thus, truth and justification are not sufficient to account for knowledge. This criticism is also known in the literature as the *Gettier problem*. Some claim that the Gettier problem is unsolvable (Zagzebski 1994, Bueno 2016).

Outside philosophy (Gettier 1963, Lehrer 1990, Zagzebski 1994, Enskat 2005), these debates are hardly noticed. Popper (2005 [1934]) has had the greatest influence beyond philosophy with his *fallibilism*. Popper wanted to separate *knowledge* from *pseudo-knowledge* and *science* from *pseudo-science*. This also implies that the knowledge generated by science is superior to all other forms of knowledge from the outset. Feyerabend (1986 [1975]) is one of the best-known critics of this preference for science. The distinction between *science* and *non-science* (Hansson 2016), or more precisely, in my opinion, between *scientifically based knowledge* and *other forms of knowledge*, is much better.

Intersubjectivity, *objectivity*, *reliability*, and *validity* are, in my opinion, the general *rationality postulates* that should apply in all *three* philosophical traditions. In addition, there is a plethora of general criteria. Under the heading "Our Philosophy", a wealth of general, abstract or universal conditions are formulated for almost every scientific journal, every institute of political science or even every scientific project, which should be taken into account by the participating *scientists*. I recommend reading one of the oldest and one more recent guide in political science: A Guide to Professional Ethics in Political Science (APSA 2012 [1968]), and Data Access Research Transparency (DA-RT, 2014).

As one can readily see, within *epistemology*, we are generally concerned with general, abstract, or universal conditions or criteria of knowledge. Within *methodology*, I believe the concrete, local, or special conditions of knowledge are discussed (chapter 6, basic course 9).

Goal

Elaborate *epistemic values*. Firstly, to get to know the *general conditions* of knowledge, the *rationality postulates*. Secondly, to deal with the *guidelines* that exist within political science, which ultimately are nothing more than epistemic values.

Questions

- Which basic *epistemic assumptions* do political scientists make?
- Which general, abstract, or universal *conditions* or *criteria of knowledge* do you know?
- What *general guides* for scientists do you know?

Recommended reading

Section 5.2.

Further references

APSA, 2012 [1968]: A Guide to Professional Ethics in Political Science.

Bueno, Otávio, 2016: Epistemology and Philosophy of Science.

Data Access Research Transparency (DA-RT), 2014.

Enskat, Rainer, 2005: Authentisches Wissen. Prolegomena zur Erkenntnistheorie in praktischer Hinsicht.

Feyerabend, Paul, 1975: Against method. Outline of an anarchistic Theory of Knowledge.

Gettier, Edmund, 1963: Is Justified True Belief Knowledge?

Hansson, Sven Ove, 2016: Science and Non-Science.

Lehrer, Keith, 1990: Theory of Knowledge.

Plato, 2011 [4. century BC]: Meno and Phaedo.

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Popper, Karl Raimund, ^{5}1968 [1934]: The Logic of Scientific Discovery.

Zagzebski, Linda, 1994: The Inescapability of Gettier Problems.

9.2.8 Basic (undergraduate) course: forms of knowledge

In this basic course, the focus is on the *output* or the results of scientific research. Therefore, the *forms of knowledge* and the *logical-analytical operations* that scientists carry out are in the foreground. There are many forms of knowledge in epistemology. I have listed some of them. Furthermore, I have made my own enumeration, which is based on some classifications, and then I developed them further. My classification is justified in chapters three to six, but above all in the sixth chapter on methodology.

Because of the *structural differences* between forms of knowledge, and types of science, as elaborated in this book, theories of politics, in my view, encompass *seven operations.* The first, the analytical operation, involves conceptual clarifications. Three empirical (descriptive, explanative, and prognostic) operations relate to what is, and three practical (normative, pragmatic, and technical) operations to what *ought* to be. In each of these operations, different goals are pursued and different scientific tools (concepts, theorems, theories, logics, ways of reasoning, methods and methodical approaches) are used (chart 9, section 9.4.9).

Goal

List the *forms of knowledge* that political scientists generate and the *ideals* they pursue in doing so.

Questions

- Which *forms of knowledge* can political scientists generate?
- Which results or which *political theories* can political science generate?
- Which *ideals* are strived for in science?
- What *properties* should scientific propositions or sentences have?

Recommended reading

Section 5.2.3, 5.2.4, 5.2.5 and 5.3, charts 9, 10 and 11, section 9.4.9, 9.4.10 and 9.4.11.

Further references

Aristotle, 2002 [4th century BCE]: Nicomachean Ethics.

Berger, Peter L./Luckmann, Thomas, 1966: The Social Construction of Reality. A Treatise in the Sociology of Knowledge.

Enskat, Rainer, 2005: Authentisches Wissen. Prolegomena zur Erkenntnistheorie in praktischer Hinsicht.

Flyvbjerg, Bent, 2006: A Perestroikan Straw Man Answers Back.

Haus, Michael, 2010: Transformation des Regierens und Herausforderungen der Institutionenpolitik.

Höffe, Otfried, 2009 [2007]: Lebenskunst und Moral oder macht Tugend glücklich?

Kooiman, Jan, 2003: Governing as Governance.

Kornwachs, Klaus, 2012: Strukturen technologischen Wissens. Analytische Studien zu einer Wissenschaftstheorie der Technik.

Mayntz, Renate/Scharpf, Fritz Wilhelm, 1995: Steuerung und Selbstorganisation in staatsnahen Sektoren.

Ostrom, Elinor/Cox, Michael/Schlager, Edella, 2014: An Assessment of the Institutional Analysis and Development Framework and Introduction of the Social-Ecological Systems.

Polanyi, Michael, 1958: Personal Knowledge. Towards a Post-Critical Philosophy.

Polanyi, Michael, 1967: The tacit Dimension.

Poser, Hans, 2001: On Structural differences between Sciences and Engineering.

Ryle, Gilbert, 2009 [1949]: The Concept of Mind.

Sabatier, Paul A./Jenkins-Smith, Hank C. 1999: The Advocacy Coalition Framework. An Assessment.

Wieland, Wolfgang, ^{2}1999b [1982]: Platon und die Formen des Wissens.

9.2.9 Basic (undergraduate) course: concept, sentence, theory, logic and argumentation level

The methodological foundations are by far the most complex of all philosophical foundations. That is why I have differentiated between seven methodological levels here. It is important to note that this section deals only with the philosophical foundations of the methodology and *not with concrete methods or methodological approaches* that political scientists use to explain or change the world. In this basic course, five levels are examined: the concept, sentence, theory, logic and argumentation levels.

Mathematics and *language* are the most important tools for scientists. Since the beginning of the 20th century, *analytic philosophy* has led to a focus on logic and language, allowing for an enormous expansion and development of the same. Within analytic philosophy, precise clarification of concepts, clarity of expression, rigorous argumentation, logical stringency and objectivity are paramount.

The work of Friedrich Ludwig Gottlob *Frege*, George Edward *Moore* and Bertrand Arthur William *Russell* laid the foundations that enabled an innovative expansion of logic, which had hardly been developed since Aristotle. For the practical tradition in particular, the work of Walter Ernst Otto *Dubislav*, Jørgen *Jørgensen*, and especially Georg Henrik *von Wright* are of particular importance. These should be addressed in more detail because they establish a fundamental distinction between the two *empirical* traditions on the one hand and the *practical* tradition on the other.

The so-called *linguistic turn* (Rorty 1967a) enabled not only an enormous further development of logic, but also of *language analysis*, which is why one also speaks of philosophy of language. I justify three fundamentally different traditions here, referring in particular to the works of Ludwig Josef Johann *Wittgenstein*, Stephen Edelston *Toulmin* and John Rogers *Searle*.

The most important criticism of analytical philosophy is that an overemphasis on *formal* elements of philosophizing overshadows the *content*, and thus the *relevance* of philosophy is lost. The accusations of the interpretivists and perestroikans against the scientistic scientists are in a similar vein: they argue that political science loses its *relevance* when theory-oriented instead of problem-oriented approaches (Green/Shapiro 1999 [1994], Flyvbjerg 2001, Schram 2005), and bloodless scholasticism (Mead 2010) determine the analysis.

Relevance and *stringency* are not mutually exclusive (chapter 7). The literary and rhetorical style, which is still widespread outside of analytical philosophy, has also led to the many misunderstandings in the science war, which is why I share Weber's attitude which can be seen as the motto of analytical philosophy:

> Personally, I believe that no means in the world should be viewed as \`pedantic\` if it serves to avoid confusion[148] (Weber 1973d [1917]: 510 [472]; my translation).

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[148] Persönlich bin ich der Ansicht, dass kein Mittel der Welt ‚pedantisch' ist, um nicht zur Vermeidung von Konfusionen am Platze zu sein (Weber 1973d [1917]: 510 [472]).

Goal

Explain the three fundamentally different traditions on five levels: conceptual, propositional, theoretical, logical and argumentation.

Questions

- Which arguments support a *principal differentiation* between three (descriptive-interpretative, explanative-prognostic and practical) traditions?

Recommended reading

Sections 6.4, 6.5, 6.6, 6.7 and 6.8, charts 2, 6, 7 and 12, section 9.4.2, 9.4.6, 9.4.7, 9.4.12.

Further references

Bevir, Mark/Kedar, Asaf, 2016 [2008]: Concept Formation in Political Science: An Anti-Naturalist Critique of Qualitative Methodology.

Dryzek, John S./Honig, Bonnie/Philips, Anne, 2011 [2009]: Overview of Political Theory.

Dubislav, Walter Ernst Otto, 1937: Zur Unbegründbarkeit von Forderungssätzen.

Green, Donald P./Shapiro, Ian, 1994: Pathologies of Rational Choice.

Jørgensen, Jørgen, 1937/1938: Imperatives and Logic.

Kornwachs , Klaus, 2012: Strukturen technologischen Wissens. Analytische Studien zu einer Wissenschaftstheorie der Technik.

Kripke, Saul Aaron, 1982: Wittgenstein on Rules and Private Language.

Monroe, Kristen Renwick, (ed.), 2005: Perestroika! The Raucous Rebellion in Political Science.

Poser, Hans, 2001: On Structural differences between Sciences and Engineering.

Rorty, Richard, (ed.), 1967a: The Linguistic Turn. Recent Essays in Philosophical Method.

Ross, David William, ^{8}1967 [1930]: The Right and the Good.

Rudolph, Susanne Hoeber, 2005a: The Imperialism of Categories: Situating Knowledge in a Globalizing World.

Schram, Sanford F., 2005: A Return to Politics. Perestroika, Phronesis and Postparadigmatic Political Science.

Searle, John Rogers, 2009 [1969]: Speech Acts. An Essay in the Philosophy of Language.

Stuhlmann-Laeisz, Rainer, 1983: Das Sein-Sollen-Problem: Eine modallogische Studie.

Toulmin, Stephen Edelston, ^{2}2003 [1958]: The Uses of Argument.

Toulmin, Stephen Edelston, 2001: Return to Reason.

von Wright, Georg Henrik, 1951: Deontic Logic.

von Wright, Georg Henrik, 1963: Norm and Action.

von Wright, Georg Henrik, 1972: The Logic of Action. A Sketch.

Wittgenstein, Ludwig Josef Johann, 1953: Philosophical Investigation.

9.2.10 Basic (undergraduate) course: method level and the level of methodological approaches

In the last basic course, the levels most familiar to political scientists are discussed. Concrete methods and methodological approaches are presented in detail in all method manuals. As a rule, their philosophical foundations are also taken into account, at least cursorily; only the philosophical foundations are discussed here.

The method level is at the center of methodological work. In spite of this, or precisely because of this, there is some confusion of concepts. This should be uncovered and explained in this basic course, and solutions formulated to overcome the confusion of concepts. The *structural differences* between empirically descriptive-interpretative, empirically explanativ-prognostic and practical methods are also worked out at this level. Then the importance of *experiments* or experimental methods and *simulations* should be discussed, and finally the differences between *experiments* and *tests* presented.

The distinction between *quantitative* and *qualitative* methods was unproblematic for a long time. Quantitative methods are based on mathematical, especially statistical, approaches, while qualitative methods are based on hermeneutic-interpretative approaches. Just as unproblematic were the words “explanation” and “description”. But since the 1970s, scientists within the Galilean, explanative-prognostic tradition have been using the concepts “description” and “qualitative methods” with a completely different meaning. One of the most important tasks in the method level section is to show these different uses and thus to overcome misunderstandings (section 6.9).

Depending on which questions and research objectives are being pursued in political science, several approaches are available: behavioristic, comparative, quantitative-comparative, institutional, functionalistic or historical. Since the 1970s, the rational choice approach has occupied a particularly prominent position, to the extent that some political scientists speak of a “rational choice revolution” (Goodin 2011b [2009]: 13). Therefore, only the limitations and possibilities of the rational choice approach are discussed in the section on methodological approaches (section 6.10). It is important for all interested parties to thoroughly review this approach.

Goal

Work out the *differences* between quantitative, qualitative-mathematical and qualitative-interpretative methods as well as between *experiments* and *tests*. Discuss the *limitations* and *possibilities* of the positive and normative *rational choice approach*.

Questions

- What are the *limitations* and *possibilities* of quantitative, qualitative-mathematical, and qualitative-interpretative methods, of experiments and tests, and of the positive and normative *rational choice approaches*?

Recommended reading

Sections 6.9 and 6.10, charts 2, 5, 6, and 7, section 9.4.2, 9.4.5, 9.4.6, and 9.4.7.

Further references

Arrow, Kenneth Joseph, ^{2}1963a [1951]: Social Choice and Individual Values.

Bennett, Andrew, 2010 [2004]: Process Tracing and Causal Inference.

Box-Steffensmeier, Janet M./Brady, Henry E./Collier, David, (ed.), 2010 [2008]: The Oxford Handbook of Political Methodology.

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Fearon, James D./Laitin, David D., 2011 [2009]: Integrating Qualitative and Quantitative Methods.

Goertz, Gary/Mahoney, James, 2012: A Tale of Two Cultures. Qualitative and Quantitative Research in the Social Sciences.

Green, Donald P./Shapiro, Ian, 1994: Pathologies of Rational Choice. A Critique of Applications in Political Science.

Hardin, Russel, 2011 [2009]: Normative Methodology.

Hedström, Peter, 2010 [2008]: Studying Mechanisms to strengthen causal Inferences in quantitative Research.

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Morton, Rebecca B./Williams , Kenneth C., 2010 [2008]: Experimentation in Political Science.

Snow, Charles Percy, 1965 [1959]: The two cultures, and a second look. An expanded version of the two cultures and the scientific revolution.

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Yanow, Dvora/Schwartz-Shea, Peregrine, (ed.), 2014 [2006]: Interpretation and Method. Empirical Research Methods and the Interpretative Turn.

9.3 Advanced or graduate courses

While getting acquainted with the philosophical heritage and especially their *possibilities* is the aim of the bachelor's seminar, the master's seminar should focus in particular on the *limits* of these foundations and thus also on the limits of the enterprise "science". The goal of the advanced courses of the master's seminar is to get to know the state of the art of research by discussing *controversially* the most important questions of the subdiscipline. I have presented my own theses, which should contribute to the *evolutionary development* of the subdiscipline. Here is a short overview of the advanced courses of the master seminar:

- 1. Advanced course (master seminar): overview or a logical geography of the philosophy of science using political science as an example.
- 2. Advanced course (master seminar): overcoming the science war. Complementarity between three philosophical traditions as a way out.

With these two courses, the first main goal of the book, to show a logical geography of the philosophy of science using the example of political science, should be worked out. They are a condensed repetition of the bachelor seminar. In addition, a further development of the subdiscipline is to be tackled in the second advanced course: overcoming the science war.

The following advanced courses are dedicated exclusively to the second main goal of the book, to undertake an *evolutionary further development* of the subdiscipline "philosophy of political science”:

- 3. Advanced course (master seminar): further development of political philosophy/political theory into a *practical political science* complementary to an empirical political science.
- 4. Advanced course (master seminar): *evolution or revolution.* Methodological approaches to the critical reception of philosophical traditions with systematic intent.
- 5. Advanced course (master seminar): *epistemology and methodology* as a demarcation line between science and non-science.
- 6. Advanced course (master seminar): *empirical validation* as the Achilles heel of correlations, causalities, models and narratives.
- 7. Advanced course (master seminar): *institutionalization* of the subdiscipline "philosophy of political science".

In the following, all advanced courses of the master seminar are explained in detail.

9.3.1 Advanced (graduate) course: logical geography of the philosophy of (political) science

The first advanced course is designed to provide a very *comprehensive introduction* to the subject. It builds on the first undergraduate course and aims to develop a logical geography of the philosophy of science using political science as an example.

One of the main aims of this monograph was to provide an *overview* of the philosophical (axiological, epistemic, methodological and ontological) foundations and premises of political science research. The *ten vertical* and *three horizontal* levels provide a conceptual overview or *logical geography* of the scientific foundations of the discipline. This allows for an orientation in thinking or the topography of scientific reason.

The *ten vertical levels* (first and second chart, section 9.4.1 and 9.4.2) contain first the axiological, epistemic and ontological levels, followed by *seven methodological levels* (concepts, propositions, theories, logics, modes of argumentation, methods and methodological approaches). Furthermore, the *complementarity* between descriptive-interpretative, explanative-prognostic (scientistic) and practical traditions could be shown.

The *three horizontal levels* (second chart, section 9.4.2) allow to show structural differences between the three philosophical traditions: the *descriptive-interpretative* tradition, the *explanatory-prognostic* tradition, and the *practical* tradition. In the respective *columns* of the table one can see the most *important features of a tradition* on ten levels.

Firstly, the manifold axiological, epistemic, methodological and ontological foundations could be shown. Secondly, these outlines can lead to the identification of the limits and possibilities of the philosophical foundations of scientific research. Thirdly, the dynamics of historical developments can be captured because the focus is primarily on the questions under discussion rather than on the individual answers, which change over time as they are developed or replaced by entirely new answers.

Goal

Develop a logical geography of the subdiscipline “philosophy of science” using the example of political science.

Questions

1. *Ontological* foundations: What is the *subject area* of political science as part of the science system? What kinds of *problems*, what *entities*, *properties*, *phenomena*, *relations*, or *structures* are addressed within political science?
2. *Axiological* foundations: What *tasks* and *goals* can, and should political scientists pursue? To what extent can and should political *values* be researched? What value should scientific *results* have for *state* and *society*?
3. *Epistemic* foundations: What basic *epistemic assumptions* do political scientists make? What *results*, political *knowledge*, or political *theories* can political science generate? How can political scientists *justify* knowledge? What *forms of knowledge* can they generate? What *values* must political science research satisfy? What are the *epistemic limits* of (political) science research and knowledge generation? What *ideals* does science strive for?
4. *Methodological* foundations: Which political science methodologies can guarantee scientificity? What are the limits and possibilities of these methodologies? Which political science methodologies are used?

Reading sources

Chapter 1, 3, 4, 5, 6, section 2.1 and 8.1, chart 1 and 2, section 9.4.1 and 9.4.2.

Further references

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von Wright, Georg Henrik, 1971: Explanation and Understanding.

9.3.2 Advanced (graduate) course: overcoming the science war, complementarity between three philosophical traditions as a way out

The second undergraduate course was about identifying the main concepts, shortcomings and points of contention of the opponents in the science war (*Methodenstreit*). The second advanced course requires this basic course. The focus is now on how to overcome this destructive Manichaean science war.

A Manichaean religious war is factually unjustified simply because there are no unbridgeable discontinuities between scientistic scientists and perestroicans or interpretivists. Neither can a breakdown of the scientific communication be identified, i.e. a general *incommensurability* between the explanative-prognostic or the Platonic-Galilean and the Aristotelian tradition cannot be justified.

Von Wright (1971) speaks of the *Aristotelian* and *Galilean traditions*, while Moses and Knutsen (2007) distinguish between *constructivists* and *naturalists*. These two books are recommended, firstly, because they deal with the main issues in an exemplary manner and, secondly, because they show how the two traditions complement each other. They also suggest ways in which the conflicts between the traditions might be overcome.

In this book, as in earlier works (Lauer 2017, 2021a), I discuss the science war with particular reference to the situation at the beginning of the 21st century. Drawing on the work of Lakatos (1978), Moses and Knutsen (2019 [2007]), and von Wright (1971), I argue for a further development of the above distinctions and justify a distinction between *three traditions*: a descriptive-interpretive, an explanatory-prognostic, and a practical tradition (chapters 3-6, chart 2, section 9.4.2).

If the basic course was only about taking note of my suggestion in this respect, the justification for the separation must now be thoroughly examined on ten methodological levels. Therefore, this course should critically discuss the way out of the methodological dispute through the complementarity of three philosophical traditions (descriptive-interpretive, explanatory-prognostic and practical).

Goal

Critically discuss the solution to the methodological dispute based on the complementarity of three philosophical traditions (descriptive-interpretive, explanatory-prognostic and practical).

Questions

- How did the *science war* come about, and how can it be *overcome*?
- What can *complementarity* contribute?
- Does the further development of three philosophical (descriptive-interpretative, explanative-prognostic and practical) *traditions* offer a way out of the science war?
- What other ways out are conceivable?

Reading sources

Section 2.2 and 8.5, chapters 3, 4, 5 and 6, charts 2, 3 and 4, section 9.4.3, 9.4.4, 9.4.5 and 9.4.6.

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von Wright, Georg Henrik, 1971: Explanation and Understanding.

9.3.3 Advanced (graduate) course: further development of political philosophy/political theory into a practical political science complementary to an empirical political science

This book demonstrates the *complementarity* between different practical methodologies. On the one hand, there is the empirical-interpretative (descriptive) methodology, and on the other hand, the empirical-scientistic (explanatory and prognostic) methodology. Furthermore, the book highlights a structural distinction across ten levels between empirical (descriptive-interpretative and explanatory-prognostic) and practical (normative, pragmatic, and technical) methodologies.

The limitations of *applied methodologies* are elaborated here in detail, both scientistic (normative rational choice theory) and phronetic (applied phronesis) approaches. Proponents of the explanatory-prognostic (Platonic-Galilean) tradition argue that *rational choice theory* is the only valuable practical methodology and dismiss all other normative theories as *esoteric* and *irrelevant* (Hardin (2011 [2009]), sections 4.1.5 and 6.10). The descriptive-interpretive (Aristotelian) tradition's adherents, the perestroikans, advocate for *applied phronesis* as a practical methodology (Flyvbjerg 2001, Flyvbjerg/Landman/Schram 2012).

Moreover, this book also argues for the indispensable need for a *genuinely practical methodology*, which encompassing normative, pragmatic, and technical aspects, to complement empirical approaches involving descriptive, explanatory, and prognostic methodologies. The book argues that only a genuinely practical methodology can provide the basis for *legitimizing* political decisions and critically evaluating political regulations.

Throughout this advanced course, we will critically examine the main arguments that stand against *applied science*, including the theories of *normative rational choice* and *applied phronesis*. It is essential to scrutinize these arguments to gain a full understanding of their limitations.

In parallel, you should also subject my arguments in favor of a *practical political science* to critical analysis. It is important to objectively evaluate the strengths and weaknesses of these arguments in order to refine and strengthen the basis of practical methods in political science.

By engaging with this practical methodology, we can develop a more robust and rounded understanding of *practical political science*. Practical political science will enable us to navigate the complexities of real-world political challenges, justify political legitimacy and make informed assessments of political regulations. In addition to the books mentioned in the Blackbox, I recommend the following literature: Grunwald 2008a and 2008b, Schatzki/Knorr-Cetina/Savigny 2001, Kielmannsegg 2006, Keuth 1989, Schelsky 1965 [1961], EU-Commission, 2001, Weingart 2006a and 2006b.

Goal

Show the pros and cons of further developing political philosophy/political theory into a *practical political science* complementary to an empirical political science.

Questions

- What are the concepts of *applied science*?
- What are the pros and cons of *normative rational choice theory*?
- What are the advantages and disadvantages of *applied phronesis*?
- What are the advantages and disadvantages of *practical political science*?

Recommended reading

Chapters 3, 4, 5, 6 and 7, section 8.6, charts 7, 8, 9, 10, 11 and 12, sections 9.4.7, 9.4.8, 9.4.9, 9.4.10 and 9.4.11.

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9.3.4 Advanced (graduate) course: Evolution or Revolution. Methodological approaches to the critical reception of philosophical traditions with systematic intent

We live in *exciting times:* digitization, globalization, climate change etc. undoubtedly brings new, incremental and disruptive innovations. Content and methodological innovations are the result. At the same time, we also live in *turbulent times.* Due to the laws of the attention economy, it is almost impossible to reach one's fellow human beings unless one makes a mountain out of every molehill: an idea becomes a paradigm, an innovation a revolution, a tool a methodology. In short, the *zeitgeist* thirsts for revolutions. Unfortunately, no utopia is in sight, but the consequences are *infantilization* and *polarization* of discourse. This revolutionary impetus also overshadows the philosophy of science. What's more, one of the most important philosophers of science, Thomas Samuel Kuhn, made a primarily indirect contribution to this.

To reconstruct *scientific progress*, various concepts are used to describe the different scientific methodologies, the innovations associated with them, and the progress they have achieved. These concepts are also used to distinguish between different schools or philosophical traditions. Therefore, this book has discussed the most widely used notions of *revolution* (I also refer to them as *Kuhn narrative*), and provided arguments why they are inappropriate for describing methodological developments in political science (section 2.6.1). The concepts of "research programs" and "traditions" are better suited to represent methodological developments since the Aristotelian Organon (section 2.6.2). The concepts of von Wright (1971) and Imre Lakatos (1982 [1978]), which I have developed further, are better suited than those of Kuhn (1976 [1962]), Schäfer (1993), Mittelstrass (1992), or Blumenberg (1975) to trace methodological developments.

The primary objective of this advanced course is to conduct a thorough comparison between two fundamental approaches to scientific progress: the *evolutionary approach* and the *revolutionary approach.* Throughout the course, participants will explore into the boundaries and possibilities inherent in each approach.

Through ext*ensive analysis* and *critical evaluation*, participants will acquire an understanding of how each approach shapes the development of political science methodologies. Ultimately, this advanced course aims to offer a thorough understanding of the complex nature of philosophical progress in political science.

Goal

Work out the limits and possibilities of an *evolutionary* and a *revolutionary* approach.

Questions

- What are the advantages and disadvantages of scientific *revolutions* and so-called *turns* or counter-revolutions?
- Which approach is more effective, scientific *revolutions* or *evolution* by means of innovative further development of existing philosophical traditions?
- What are the *advantages* and *disadvantages* of evolutions through innovative further development of existing philosophical traditions?

Reading sources

Chapter 2, especially section 2.6, charts 2, 3 and 4, section 9.4.2, 9.4.3 and 9.4.4.

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9.3.5 Advanced (graduate) course: epistemology and methodology as the *demarcation line* between science and non-science

The main aim of this advanced course is to explore the distinction between scientific knowledge and other sources of knowledge, and thus between science and non-science (Hansson 2016). The roots of this investigation can be traced back to Plato's dialogues Meno and Theaetetus (Plato 1983b and 1983d), where the question of the distinction between belief and knowledge was first raised. Gettier follows up on this discussion and, in a two-page essay, introduces a modern discussion that is hardly manageable, but which is not noticed outside philosophy (Gettier 1963, Lehrer 1990, Zagzebski 1994, Hansson 2016). Popper has the greatest influence outside philosophy with his fallibilism (Popper 2005 [1934]).

In this course, the focus will be on a *controversial* examination of my own thesis regarding the *demarcation* between science and other forms of knowledge. My thesis is: An extremely complex methodology consisting of a wealth of general, abstract or universal as well as concrete, special or local conditions or criteria is necessary to generate scientific knowledge and draw a demarcation line between science and non-science. These *epistemic* or *constitutive norms* and values guarantee *scientificity*. The justification of this thesis can be found in the fifth and sixth chapter. Charts 9, 10 and 11 (section 9.4.9, 9.4.10 and 9.4.11) show some results specific to this discussion.

By exploring a range of philosophical perspectives participants will develop a comprehensive understanding of the complex nature of scientific demarcation. The course encourages lively debates and discussions, enabling participants to refine their own perspectives on the demarcation problem. Additionally, it seeks to foster an appreciation for the intricacies of scientific methodology and the challenges associated with drawing clear boundaries between science and other forms of knowledge.

In conclusion, this advanced course provides a unique opportunity to explore into the philosophical underpinnings of scientific demarcation, allowing participants to engage in an intellectual journey and cultivate an understanding of the distinctions between scientific knowledge and other forms of knowledge.

Goal

Work out how to distinguish *science* from *non-science*. Controversially discuss my own thesis on the demarcation: An extremely complex methodology, consisting of a plethora of general, abstract, or universal, as well as concrete, special or local conditions or criteria, is necessary to generate scientific knowledge and to draw a line between science and non-science. These epistemic or constitutive norms and values guarantee *scientificity*.

Questions

- What basic *epistemic assumptions* do (political) scientists make, and how can they justify their knowledge?
- How can political scientists *justify* knowledge?
- According to which *epistemic values* should (political) scientists research?
- What are the epistemic *limits* of (political) scientific research and knowledge generation?

Reading sources

Chapter 5 and 6, charts 9, 10 and 11, sections 9.4.9, 9.4.10 and 9.4.11.

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9.3.6 Advanced (graduate) course: evidence-based science or empirical validation as the Achilles heel of causalities, correlations, models and narratives

If scientists simply want to create *science fiction narratives*, logically coherent and grammatically correct narratives that meet scientific requirements and are created using scientific methodologies will suffice (chapters 5 and 6, section 9.3.5). In political science, the vast majority of scientists want to make rational statements about political reality and rational regulations of political reality. Therefore, *scientific narratives* must also experience *empirical validation*. This is the only way to distinguish between the real world on the one hand and possible or virtual worlds on the other.

Narratives that are based in myth have shaped scientific discussion for centuries. This problem will become even more serious in the future. Thanks to generative artificial intelligence, so-called *hallucinations* are generated that are logically coherent and syntactically correct, but are nevertheless wrong (Blätte/Behnke/Schnapp/Wagemann 2018, Janidis/Kohle/Rehbein 2017, Kitchin 2014, Lauer forthcoming, Lyon 2016, Stulpe/Lemke 2016, Zwitter 2014). In other words, we are dealing here with an *epistemological sophism* or with the *Gettier problem* (section 5.2.1). These are false arguments or narratives because they are based on false assumptions or misunderstandings.

Mathematical models have the same problems as narratives. They are always based on assumptions, or more precisely on *observations*. If even one of these underlying observations is wrong, then the model is worthless, no matter how precisely it was calculated or how coherent it is: garbage in, garbage out.

The fifth advanced course covered the basics for creating logically coherent models and narratives in science and explained how they are differentiated from non-scientific methodologies. In this course we focus on generating *assumptions* and *observations* about the *real world*.

In order to ensure this within the framework of the descriptive-interpretive tradition, this book initially aims at an *ad fontes reconstruction* (section 2.3). Furthermore, own narratives are created using a *multilingual approach* (section 2.4). For the explanatory-prognostic tradition, it is particularly important to distinguish between *data set observation* and *causal process observations* (Brady 2010 [2004]). In the practical tradition, one should focus primarily on the distinction between *experiments* and *tests* (Kornwachs 2012).

Goal

To show the different strategies of how evidence-based science is done through empirical validation in the three traditions (descriptive-interpretive, explanatory-prognostic and practical).

Questions

- What methods and methodological approaches are used to *interpret* political symbols (texts, images, audio and videos)?
- How are empirical *observations* about political reality generated?
- How can political regulations be *tested*?

Recommended reading

Chapter 2, sections 9.4.6, 9.4.7 und 9.4.12.

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9.3.7 Advanced (graduate) course: institutionalization of the subdiscipline “philosophy of political science”

Initially, the study of science focused mainly on physics as a primary example. Later, this research expanded to include a wider variety of fields, including philosophical enquiry into the social sciences (Cartwright 2014, Kincaid/Ross 2009, Kincaid 2012, Lohse/Reydon 2017).

In the 20th century, the philosophy of science became more differentiated and also found its way into political science, one of the most recently established disciplines (McIntyre/Rosenberg 2017, Kincaid/Van Bouwel 2023). On the one hand, there is a *philosophical awakening*; Moses (2020) speaks of a methodological awakening. On the other hand, this *philosophy of political science* still leads a *shadowy existence* (Lauer 2017, 2021b, Kincaid/Van Bouwel 2023a).

Currently, there are no specialized journals or chairs dedicated to the subdiscipline of "philosophy of political science". Additionally, there are no working groups addressing this topic within political science institutions. Therefore, this publication puts forth a proposition to institutionalize the "philosophy of political science" and seamlessly integrate it into the science system. This could be achieved by incorporating it into curricula, establishing chairs, and establishing relevant journals.

Although there has been progress in exploring philosophical dimensions, the presence and influence of this field in political science is relatively extremely limited. This situation opens up opportunities for further exploration and engagement with the philosophical aspects of political science. Emphasizing the importance of philosophical research can lead to a deeper understanding of the fundamental issues and challenges in the field. By illuminating the philosophical underpinnings, political scientists can improve the rigor and precision of their research, thereby gaining more comprehensive and nuanced insights into the complexity of political phenomena.

In this advanced course, the most important arguments are to be worked out that speak for or against establishing the subdiscipline. In particular, the possible tasks of a philosophy of political science should be shown.

Goal

Work out the arguments for and against institutionalizing the subdiscipline "philosophy of political science".

Questions

- What role can an *institutionalization* of the subdiscipline philosophy of political science play in the future?
- How can the further development of the philosophical foundations of political science best be guaranteed?
- What *tasks* and *relevance* should the subdiscipline have?
- What questions should be dealt with within the subdiscipline?
- What are the scientific *limitations* and *possibilities* of political science?
- How can the *politicization* and *moralization* of political science be avoided?
- What are the pros and cons of creating a new subdiscipline?

Recommended reading

Chapters 2 and 7, section 8.7.

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9.4 Charts

The following charts were first published in Lauer (2017: 51-61). In particular, the first and second charts have been further developed.

9.4.1 The ten levels of the philosophy of political science

1.1 Philosophical foundations: Axiological, epistemic and ontological prerequisites of (political) scientific research. Limits and possibilities of (political) scientific research.	**1.1.1 Axiological level (1, first level)**	Tasks and goals of scientific research, non-epistemic values, relationship between politics and (political) science.
	1.1.2 Epistemic level (2)	General conditions or (core) criteria of knowledge, forms of knowledge, as well as ideals and properties of (political) scientific research.
	1.1.3 Ontological level (3)	Phenomena and symbols, causalities and political institutions. Kinds of *problems: entities*, *properties*, *phenomena*, *relations,* or *structures* within political science.
1.2 Scientific tools	**1.2.1 Concept level (4)**	Scientific concepts.
	1.2.2 Sentence level (5)	Scientific sentence (statements, norms, i.e. justice standards, pragmatic or technical rules).
	1.2.3 Theory level (6)	Scientific theories.
	1.2.4 Logic level (7)	Formal inference and inference rules related to scientific concepts and scientific sentences.
	1.2.5 Argumentation level (8)	Argumentation inside scientific theories or logical structure of scientific arguments (logic of research).
	1.2.6 Method level (9)	Scientific investigation of facts and judgements, generation and evaluation of facts.
	1.2.7 Methodical approaches level (10)	Scientific generation of theories.

9.4.2 Three philosophical (descriptive-interpretative, explanatory-prognostic, and practical) traditions at ten levels

Scientific methodologies and **types of science**	**I. Descriptive-interpretative tradition**: descriptive methodology (sciences)	**II. Explanative-prognostic Tradition**: empirical-explanative and empirical-prognostic methodology (sciences)	**III. Practical tradition**: practical (normative, pragmatic and technical) methodology (sciences)
1. Axiological level Tasks and goals of scientific research, non-epistemic values, relationship between politics and (political) science.	**Descriptions**: description of (**visible**) **phenomena**, interpretation of **symbols** (text, image, audio and video), mainly by means of language	**Explanations** and **predictions**: world recognition, world explanation of **invisible causalities** using especially logic and mathematics	**Evaluations and Regulations**: world change, practical (normative, pragmatic and technical) standards (norms) and regulations using logic, language and mathematics
2. Epistemical level Epistemic values, forms of knowledge, ideals and properties of scientific research.	Descriptive-interpretative knowledge	Empirical-explanatory and empirical-predictive knowledge	Practical (normative, pragmatic and technical) knowledge
	Ideal of *truth* Predicates: true or false		Ideal of *rightness* (ethics): right/wrong Ideal of *justice* (politics): just/unjust Ideal of *phronesis* (Klugheit): wise/unwise Ideal of *efficacy*: effective/ineffective
3. Ontological Level	Phenomena and Symbols	Causalities	Political Institutions
4. Concept level	Qualitative, interpretative or classificatory concepts	Quantitative, mathematical or metric concepts	Practical (normative, pragmatic and technical) concepts
5. Sentence level	Descriptive-interpretative propositions	Explanatory and predictive propositions	Norms, i.e. justice standards, pragmatic and technical rules
6. Theory level	Empirical theories consist of systems of propositions, including propositions about standards and rules.		Practical theories consist of regulations, i.e. systems of empirical statements and practical standards and regulations.
7. Logic level Formal inference and inference rules related to scientific concepts and scientific sentences	**Truth-apt logic**: *Propositional logic*: It is the case, that [...] *Predicate* logic: F "is a human". Modal logic, e.g. *alethic modal* logic: It is necessary/impossible/possible/contingent that [...] *Epistemic* (doxastic) logic: It is believed/considered impossible/conceivable that [...] *Tense* logic: It was/will be/always will be/always was the case that [...] *Deontic* logic (*is-ought*, Sein-Sollen): It ought to be/it is forbidden/permitted/indifferent that [...]		Unlike classical logic, these are **not truth-apt** (*Jørgensen's dilemma*). Logic of *Norms* (*act-ought*, Tun-Sollen, not *is-ought*, Sein-Sollen), logic of *imperatives, interrogative* logic, legal *logic*, logic of *implementation* (Durchführungslogik). Efficacy and rightness, Prima-facie property of ethical norms and political maxims of action. Conflicts of justice standards and mediation of justice standards.
8. Argumentation level Argumentation inside scientific theories or logical structure of scientific arguments (logic of research)	Analytical, dialectical, empirical, evolutionary or hermeneutic means of argumentation		Practical (normative, pragmatic and technical) argumentation
	Explaining-understanding-debate thought of as complementary		Practical, substantive argumentation, practical syllogism and pragmatic syllogism
	Understanding	Explanation	
	Abductive, inductive, substantial, warrant-using, tentative, formally invalid, epagogical argumentation: Hegelian dialectics, hermeneutic circle	Deductive, analytical, warrant-establishing, conclusive, formally valid argumentation: deductive-nomological model (or HO schema), evolutionary explanatory model	
	Aristotelian topic (dialectics)		
9. Methods level with reference to political science. Scientific investigation of facts and judgements, generation and evaluation of facts	Descriptive methods	Empirical-explanatory and empirical-predictive methods	practical (normative, pragmatic and technical) methods
	Qualitative methods: content analysis, document analysis, participatory observation	Quantitative methods: quantitative data collection, correlation and regression analyses	Arguing, discourse, deliberation, mediation, synopsis, categorical imperative, evaluation, implementation planning, technology assessment (TA)
	Triangulation: The application quantitative and qualitative methods on a phenomenon.		
10. Methodical approaches level with reference to political science Generation and evaluation of theories	Approaches with empirical and practical elements		
	Socio-technological, synoptic, practical-normative, critical-dialectical, empirical-normative, argumentative and pragmatic approach, rational choice approach, advocacy coalition approach, governance approaches, actor-centered approaches (decision arenas, networks, exchange and negotiation systems, regimes)		
	Empirical approaches		Practical approaches
	Historical, institutional and structuralist approach, narratives, frames, discourses	Behavioural, functional and quantitative approach	Participatory policy approach, decisionist, synoptic, normative, pragmatic and technical approach

9.4.3 The liberal-scientistic narrative: axiological, epistemic, methodological and ontological assumptions of the explanative-prognostic or the Platonic-Galilean tradition

Ontological assumptions	1. Naturalism: External reality both given and constructed 2. Causal reductionism or causality as an invisible relation that which holds the world together at its core. 3. Empirical reductionism 4. Equivalence between causality and action 5. Inversion of causalities: The transformation of empirical causal propositions produce social-technological rules or normative statements.
Epistemic objectives	6. Causal regularities and generalisations at the macro level 7. but also, causal processes or causal mechanisms at the micro level 8. Postulates of rationality: intersubjectivity, objectivity, reliability, validity 9. Ideal of truth, coherence theory or correspondence theory of truth 10. If-then deep structure and hypothetical character of knowledge, assumptions (conjectures), therefore neither search for universal truths nor freedom from context! The context is stated in the *ceteris paribus* conditions or implicitly assumed. 11. Methodological individualism
Methodological procedures	12. Logic and mathematics as the preferred means of world recognition and world change 13. logical-mathematical concepts, *data-set observations* (DSOs) and *causal-process observations* (CPOs) 14. Truth-apt empirical and normative statements 15. Falsifiable causal hypotheses 16. Deductive and inductive argumentation 17. Model thinking 18. Experiments (simulations) 19. Quantitative methods (e.g., correlation and regression analyses) 20. Qualitative-mathematical methods e.g., process analysis (*process tracing*), *Qualitative Comparative Analysis* (QCA)
Axiological, practical (normative, pragmatic and technical) assumptions	21. Liberalism 22. Utilitarianism 23. Universalism 24. Separation between Is and Ought 25. Normative rational choice theory as a practical approach

9.4.4 The phronetic narrative of the perestroikans: axiological, epistemic, methodological and ontological assumptions of the Aristotelian or descriptive tradition

Ontological assumptions	1. Constructed reality 2. Detecting tension points
Epistemic objectives	3. Contextuality of knowledge 4. Description of phenomena 5. Use of symbols (texts, pictures, audio and video), *naming* 6. Local knowledge 7. Language rules, interpretation schemes, forms of life (Lebensformen) 8. Framing, structures and patterns of interpretation
Methodological procedures	9. Qualitatively-interpretative tools (concepts, methods and methodical approaches, e.g. qualitative content analysis, discourse analysis, hermeneutics) 10. Quantitative tools: the quantitative-qualitative schism is rejected and a diversity of methods propagated. 11. Methodological holism
Axiological, practical (normative, pragmatic and technical) assumptions	12. Better practical relevance, problems with practical relevance (problem-driveness) 13. Pragmatism 14. Hermeneutics 15. Phenomenology 16. Philosophy of language 17. Critical theory 18. Structuralism 19. Applied phronesis
Assumptions that do not produce fundamental contradictions to the scientistic narrativ	20. No universal truths 21. Contextuality of knowledge 22. External reality constructed or given 23. Coherence theory of truth instead of correspondence theory of truth

9.4.5 Actor-centered explanations, the rational choice approach

<table>
<tr><td>Macro or
system level</td><td>1. (a) Collective characteristic</td><td colspan="2">(b) Collective hypothesis,
causal regularity, nomological explanations: "nomological conception of scientific explanation", "explanation2"
(Salmon 1989: 184).
→</td><td>4. (c) Aggregate characteristic</td></tr>
<tr><td>Transition from macro to micro level, explanation at the micro level and transition from the micro to the macro level</td><td>↘
A. Context hypothesis</td><td colspan="2">B. Individual hypothesis,
causal process, ontic explanations: "ontic conception of scientific explanation".
"causal/mechanistic explanation"
"explanation1"
(Salmon 1989: 182 and 184).
→</td><td>↗
C. Aggregation rules</td></tr>
<tr><td>Micro or
individual level</td><td></td><td>2. Individual characteristic (actor)</td><td>3. Individual characteristic (action)</td><td></td></tr>
<tr><td></td><td><i>Logic of situation</i>
The assumptions made here model the relationship between the situation and the actor.
Coleman describes these as rules that ensure the transition from macro to micro level.</td><td colspan="2"><i>Logic of selection</i>
Teleological action theory of the individual level, here concerning the rules and preferences on the basis of which the individuals choose what action to take.</td><td><i>Logic of aggregation</i>
Transformation rules based on which the collective explanandum is derived.
Coleman designates them as rules which ensure the transition from the micro to the macro level.</td></tr>
<tr><td colspan="5">Sources: My own presentation based on the sketches of Coleman (2010 [1990]: 10 and 13), and the 1st chapter (Coleman 1990), von Beyme (2000 [1972]: 136-150), Braun (1999: 17-52) and Salmon (1989).</td></tr>
</table>

9.4.6 Methodology of empirical political science

Type of science	Human and cultural sciences (*humanities*), interpretivists		*Real social/political science*, phronetic perestroikans	*Social sciences*, scientistic scientists
Forms of knowledge	Empirical (descriptive-interpretative) knowledge.		Empirical knowledge	Empirical (explanatory and predictive) knowledge
Tasks and objectives	World understanding, world interpretation, world description			World explanation
	Interpretation and interaction: describe or critique lifeworlds and text analyses.	Describe visible phenomena (*phainómenon*: visible appearance).	Discover tension points	Recognize invisible causalities between events, explain causal regularities and causal processes.
Knowledge objectives	Descriptive-interpretative, exploratory, critical			Explanatory, predictive
Tools in a large sense (*Organon*)	**Language** as a preferred means of world recognition, world interpretation and world description of (visible) phenomena, especially interpretative and qualitative-classificatory tools (concepts, methods and methodical approaches), e.g. qualitative content analysis, discourse analysis, hermeneutics.			**Logic and mathematics** as preferred means of world recognition and world explanation of invisible causalities, especially quantitative and qualitative mathematical tools, experiments and simulations.
Macro level	Language rules, interpretation schemes, forms of life, framing, structures and patterns of interpretation.	Description of phenomena at the macro level, e.g. demonstrations.	Show power structures.	Nomological explanations: probabilistic laws or regularities, complex interrelationships, causal or nomological regularities, (*explanation2*).
Micro level	Use of symbols (texts, pictures, audio and video), *naming*.	Description of phenomena at the micro level, e.g. demonstrator.	Discover tension points	Ontic explanations: Explain cause and effect mechanisms, causal processes or potentialities (*explanation1*).

9.4.7 Methodology of practical political science

Type of science	*Social science,* scientistic scientists	*Real social science,* phronetic perestroikans	Practical political science
Forms of knowledge	Applied knowledge.	Problem-oriented knowledge (*problem-based, problem-driven*)	Practical (normative, pragmatic and technical) knowledge
Tasks and objectives	World change, applied science	World change, problem-oriented science (*problem-based, problem-driven*).	World change, using genuinely practical methodology.
Knowledge objectives	Social-technological	Participatory, critical, deliberative	Practical (normative, pragmatic and technical)
Praxis	Establish social technology by inversion of causalities.	Change power relationships and tension points.	Legitimation of normative standardization and regulations.
Normative level, value discourse	Legitimisation of values is not possible: utilitarianism, like ethics of responsibility, is an "ethics of the second order" (Wieland 1999a).	No normative legitimation: "Our sociality and history is the only foundation we have, the only solid ground under our feet". (Flyvbjerg /Landman/Schram 2012c: 293).	Legitimation of maxims of action or political standards (*Handlungsmaximen oder politische Normen*).
Pragmatic level, pragmatic discourse	Selection and substantiation of objectives using a rational choice approach is not possible: Arrow paradox.	No pragmatic legitimation: "[W]here 'better' is defined by the values of phronetic researchers and their reference groups" (Flyvbjerg /Landman/Schram 2012c: 290).	Legitimation of strategies for action, individual-pragmatic rules and sociopragmatic regulations (*Handlungsstrategien, individualpragmatische Regeln sowie sozialpragmatische Regulierungen*).
Technical level, means discourse	Substantiate social-technological regulations with rational choice approach.	Applied phronesis enables empowerment	Substantiate instruments for action or practical instructions for action (*Handlungsinstrumente oder praktische Handlungsanweisungen*).

9.4.8 Practical methodology within political philosophy and political science

Levels of discourse	**Ethics and political philosophy**	**Political science: Policy analysis and governance research**		
		Actor-centered institutionalism and governance perspective	Advocacy coalition approach	Institutional analysis and development framework
Normative value discourses. Normative legitimations, normative (ethical and moral) **evaluations**, here maxims of action (*Handlungsmaximen*). **Why** should something be done?	Third evaluation stage: ethical-moral rationality, categorical imperative. 3.1 Virtue ethic (*Tugendmoral*), ethical and moral standards, 3.2 Legal morality (*Rechtsmoral*), (political) justice, legal norms (Höffe 2009[2007]: 26).	"Design perspective" (Haus 2010: 109), "third order governing", "metagovernor", "meta governing", "who or what - ultimately - governs the governors" (Kooiman 2003: 170 ff.).	Highest and most comprehensive level, the "*deep core belief system"* consisting of normative and ontological axioms, "basic ontological and normative beliefs, such as the relative valuation of individual freedom versus social equality" (Sabatier/ Jenkins-Smith 1999: 121, see 133).	Constitutional Rules-in-Use (Ostrom/Cox/Schlager 2014:285). "Constitutional-choice rules affect operational activities and their effects in determining who is eligible and the rules to be used in crafting the set of collective-choice rules that in turn affect the set of operational rules" (Ostrom/Cox /Schlager 2014: 284).
Pragmatic objective discourses. Pragmatic legitimations, **objectives** and **purposes**, here strategies for action (*Handlungsstrategien*). **What** should be done? State, market and civil society.	Second evaluation stage: Here objectives and purposes are evaluated. Pragmatic rationality, pragmatic imperative. 2.1 Individual pragmatic rules, 2.2 Social-pragmatic regulations (Höffe 2009 [2007]: 24-25).	"Level of structuring the practices of problem solving by 'institution building'" (Haus 2010: 109), "second order governing", "institution building" (Kooiman 2003: 153 ff.). "governance structure" (Mayntz/Sharpf 1995: 16).	Middle level of the "*policy core belief systems"* consisting of policy strategies with which central values are implemented, "normative commitments and causal perceptions across an entire policy domain or subsystem" (Sabatier/ Jenkins-Smith 1999: 121, 133).	"Collective Choice Rules-in-Use" (Ostrom/Cox/Schlager 2014: 285). "Collective-choice-rules affect operational activities and results through their effects in determining who is eligible and the specific rules to be used in changing operational rules" (Ostrom/Cox/Schlager 2014: 284).
Technical means discourses. Technical legitimations, **means**, here tools of action (*Handlungsinstrumente*). **How** should something be done?	At the *first stage*, means and ways are tested for their suitability for optional intentions or objectives. Technical rationality, technical imperatives. 1.1 Technical individual rules, 1.2 Social-technological regulations (Höffe 2009[2007]: 23).	Level of an operative practice of direct problem solving (Haus 2010: 109), "first order governing", "opportunity creation" (Kooiman 2003: 135 f.), "Service structure (industry structure" (Mayntz/Sharpf 1995).	Lowermost level with respect to "*instrumental decisions*" (Sabatier /Jenkins-Smith 1999: 133), "*secondary aspects of a coalition belief system*", e.g. design of specific institutions" (Sabatier /Jenkins-Smith 1999: 122).	"Operational Rules-in-Use" (Ostrom /Cox/Schlager 2014: 285). "Operational rules directly affect day-to-day decisions made by the participants in any setting" (Ostrom/Cox/Schlager 2014: 284).

Sources: Höffe 2009[2007], Ostrom /Cox /Schlager 2014, Mayntz/Sharpf 1995b, Kooiman 2003, Sabatier/Jenkins-Smith 1999, Jenkins-Smith/Nohrstedt/Weible/Sabatier 2014). Frank Fischer (2003: 193-198) identified **four** rather than three levels: *Technical-Analytical* Discourse: Programme Verification, *Contextual* Discourse: Situational Validation, *Systemic* Discourse: Societal Vindication and Ideological Discourse: Social Choice Comparison of actor-centered institutionalism and governance perspective in Haus (2010: 109).

9.4.9 Scientific operations and scientific discourses with reference to political science

1. Analytical operations of political science **Analytical discourses**		*Analytical discourse*: Analytical discourse includes analytical operations and generates *analytical knowledge.* This especially includes *political concepts* or *categories,* but also *models* for analyzing political reality. These are *conceptual* or *logical truths* in the form of non-empirical, truth-apt statements.
2. Empirical operations of political science or operations concerning *what is*, or what constitutes political reality, comprising of **sentences** (descriptions, explanations and predictions), and **sentences** about valid *standardization* and *regulation* of a *political system* **Empirical discourses**	**2.1 Descriptive-interpretative operation or descriptions of political reality**	*Descriptive-interpretative discourse***:** In this case, the aim is to *understand* political reality. What exists becomes the focus of attention – using *descriptive-interpretative methods* a picture is created of what everyday politics is like in a political system: Power structures, dependencies and political decision-making processes are considered and examined in more detail. This also includes truth-apt statements regarding maxims for action (guidelines, norms, principles and values). These are identified and described, e.g., the welfare state postulate, e.g., Article 20 of the German Constitution. However, it also includes a detailed description of action strategies and instruments such as social security systems.
	2.2 Explanatory operation or explanations of political reality	*Explanatory discourse*: Political reality also requires *causal explanations.* For example, there are explanations for demographic developments, but also for why social policy has developed in one way and not another.
	2.3 Predictive operation or predictions with respect to future political developments	*Predictive discourse*: The need to *predict* future developments is central: It makes sense to take a look into the future in order to provide decision-makers in the present with important key information.
3. Practical operations of political science or operations concerning *what ought to be*, containing discourses on *standardizations* or *regulations* (maxims of action, strategies of action, instruments for action, instructions for action and practical judgements) **Practical discourses**	**3.1 Normative operation or normative dimension of policy**	*Normative discourse* or *value discourse*: In this case, the political maxims of action (*Handlungsmaximen*) which are decisive for the standardization or regulation of the political system as a whole or of a policy area should be discussed.
	3.2 Pragmatic operation, strategic level or dimension of policy	*Pragmatic discourse* or *objective discourse*: In this case, the political strategies of action (*Handlungsstrategien*) that will be decisive for the regulation of a policy area should be discussed.
	3.3 Technical operation, the operational level or dimension of policy	*Technical discourse* or *means discourse*: In this case, the political instruments for action and individual instructions for action (*Handlungsinstrumente*) that are decisive for the regulation of a policy area should be discussed.

9.4.10 Knowledge (*Wissen*) versus capability (*Können*)

1. Knowledge, theory **Actors**: **Scientists**, such as political scientists, generate empirical and/or practical knowledge – natural scientists' empirical knowledge, technical scientists practical knowledge.	Form of knowledge: *Analytical knowledge* in the form of propositions.	*Conceptual and logical truths* in the form of non-empirical, *truth-apt* statements.
	Form of knowledge: *Procedural knowledge*: in the form of propositions.	*Procedural knowledge* in the form of *truth-apt* methodologies.
	Form of knowledge: *Empirical knowledge* in the form of natural or social science *propositions* and *propositional systems,* including statements about standards and rules. Type of science: *empirical* (theoretical) *sciences.* Examples: Natural sciences, empirical social sciences. Analytical and empirical knowledge is also *sentences knowledge*, because both are formulated as *sentences.*	*Descriptive-interpretative knowledge* in the form of *truth-apt* descriptions.
		Explanatory knowledge in the form of *truth-apt* explanations.
		Predictive knowledge in the form of *truth-apt* predictions.
	Form of knowledge: *Practical knowledge* in the form of practical standardizations and regulations Type of science: *Practical* (normative, pragmatic and technical) *sciences.* Examples: medical sciences, technical sciences, practical social (political) sciences. *Practical knowledge* consists of three different components: ➢ *Why*, or the normative component, consisting of ethical-moral *evaluations*, in this case maxims of action ➢ *What for*, or the pragmatic component, *objectives* and *purposes*, in this case action strategies ➢ *how*, (the technical component, *means*, here action instruments).	*Normative knowledge* in the form of maxims of action (*Handlungsmaximen*) and normative-political judgments that are *just* or *unjust.*
		Pragmatic knowledge in the form of action strategies (*Handlungsstrategien*) and pragmatic judgments consisting of e.g. different methodological approaches to cure a disease. Pragmatic rules are *wise* or *unwise.*
		Technical knowledge in the form of tools for action (*Handlungsinstrumente*) and technical judgments, e.g. methods that contain practical technical rules for curing a disease. Technical rules are *effective* or *ineffective.*
2. Capability (*Können*) **Actors: Practitioners** – citizens, politicians, civil servants, administrators, entrepreneurs can make political decisions.	Practical *competence* in implementing empirical and practical knowledge, to be able to do something, e.g. the ability of the physician, craftsman, engineer, teacher, manager, politician, scientist to produce outstanding achievements in his or her field. *Capability* consists of dispositions, competencies, skills in doing something. This is the area covered under the label of *implicit, non-propositional knowledge.* This is only one part of expertise (know-how), that of practical capability. Ryle´s conception of *know how* include what I understand under practical capability and practical knowledge, *know that* includes analytical and empirical knowledge.	

9.4.11 Knowledge (theory) versus praxis (action)

1. Knowledge (theory): **Sphere of cognition and knowledge**	A *scientist* is *always* a *theorist*, no matter whether he asserts empirical propositions regarding political reality with an empirical methodology or whether he also legitimizes standardizations or regulations using a practical methodology. In the first case, the scientist generates *empirical knowledge*, in the second *practical knowledge.* There are *no* applied sciences, but only practical sciences and scientifically trained *practitioners* who apply knowledge, and *scientists* who generate knowledge.
2. Praxis (action): **Sphere of action**	A *practitioner* (citizen, politician, official, administrator, entrepreneur) changes (political) reality, whether he refers to scientifically based *empirical* and *practical knowledge* and *makes rational decisions*, or makes subjective *gut decisions.* Theory and praxis are considered as *complementary* and not hierarchical. Equivalence between the two, as is usual in the Bacon program, is also rejected.

9.4.12 Empirical and practical ways of argumentation

1. Practical syllogism	"A intends to bring about p. A considers that he cannot bring about p unless he does a. Therefore A sets himself to do a" (von Wright 1971: 96, see von Wright 1977c [1963], and 1977d [1972]).
2. Intentional syllogism	"*A* intends to bring about *p*. *A* considers that he cannot bring about *p*, unless he (first) learns (how) to do *a*. Therefore *A* sets himself to do *a*" (from Wright 1971: 101, cf. from Wright 1977c [1963], from Wright 1977d [1972]). "From now on A intends to bring about *p* at time *t*. From now on *A* considers that, unless he does *a* no later at time *t´*, he cannot bring about *p* at time *t*. Therefore, no later than when he thinks time *t´* has arrived, *A* sets himself to do *a*, unless he forgets about the time or is prevented" (from Wright 1971: 107, see Anscombe 1963).
3. Pragmatic syllogism	A *causal theorem* (C) is *equivalent* to a *technical rule* (TR): **C** (**A** => **B**, if **A**, then **B**) <=> **TR** (**B** per **A** if **B** is desired, do **A**). *Critique*: "Pragmatic syllogism is a result of the pragmatic interpretation of a deductive-nomological explanation and its connection with a normative principle, e.g. that B is desired. Bunge sometimes calls this expression a ´technological rule´" (Kornwachs 2012: 67, my translation). There is only one *pragmatic*, but no *logical* relation between lawful or regulative *propositions* , e.g. if A, then B, and associated (technical) *rules* or instructions, e.g. B per A, if you want to achieve B , then try A (Kornwachs 2008: 139 and Kornwachs 2012: 64 ff.). There is a difference "between the propositions **A** and **B** and the associated action **A** or a real state **B**, which is put into action by action **A** " (Kornwachs 2012: 65, my translation). Kornwachs takes this *notation* from Mario Bunge (1967b: 132-139).
4. Deductive-nomological model or HO schema or subsumption theory of the explanation (*covering law model*)	I. **Antecedens**, singular conditions (**conditio**) **C1**, **C2** [...] **Ck** II. **Explanans** (the explanatory), general laws (**lex**) **L1**, **L2** [...] **Lk** ____________________________________ III. **Explanandum**, the event to be explained **E** Sources: Hempel 1972 [1966]: 239, see Hempel/Oppenheim 1948 and Popper 2005 [1934].
5. Inversion of causal statements to technical regulations	I. Causal truth-apt statement, **A** => **B** (if **A**, then **B**) II. Equivalence between causality **A** and action **B**, **A** <=> **B** (**A** if and only if **B**) or (**A** => **B** and **B** => **A**) III. Pragmatic syllogism, **C** (**A** => **B**, if **A**, then **B**) <=> **TR** (**B** per **A**, if **B** is desired, do **A)** ____________________________________ IV. Technical rule, **B** per **A** (if **B** is desired, do **A**). Sources: My own presentation, Equivalence between *causality* and *action* (Bacon 1990 [1620]: 80, 3. Aphorism, Volume 1), *Pragmatic Syllogism* (Bunge 1967b: 132-139 and Kornwachs 2008: 139 and 2012: 64 ff.).

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