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Interdisciplinary Research and Trans-disciplinary Validity Claims

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Foreword

Recent societal challenges are often in need of scientific decision support, which relies heavily on appropriate interdisciplinary efforts. The EA European Academy of Technology and Innovation Assessment responds to this demand. In doing so, methodological reflections are indispensable to improve the EA's overall mission for policy advice.

This study on *Interdisciplinary Research and Trans-disciplinary Validity Claims* involved internal staff and external experts from various disciplines. The mission of the working group was to reflect on the specific needs for scientific advice as well as at deliberating the potential of the science system to satisfy these demands, while critically reviewing corresponding assessment concepts and advisory frameworks.

The present book summarises the results of this effort. I thank the authors of the study Martin Carrier, Carl Friedrich Gethmann, Gerd Hanekamp, Matthias Kaiser, Georg Kamp, Stephan Lingner, Michael Quante and Felix Thiele for their enthusiasm towards this project and its final report.

The Shareholder Assembly of the EA, the Federal State of Rhineland-Palatinate and the German Aerospace Center, is gratefully acknowledged for supporting the idea of the project, which was institutionally funded from 2010 to 2013.

Bad Neuenahr-Ahrweiler, November 2014 Petra Ahrweiler

Preface

The modus of interdisciplinarity has become a familiar feature in modern research despite the ongoing specialisation of disciplines in science and humanities. Today, many questions addressing the science system are of complex nature and are thus directed towards a *diversity* of relevant disciplines. Hence, these disciplines have to organise themselves within interdisciplinary research frameworks in order to introduce their specific perspectives to these problems and their reflection appropriately. However, crossing disciplinary borders in this way is not trivial, especially with regard to the epistemic and procedural restrictions of such endeavours. Moreover, the topics of interdisciplinary research are often *societally relevant*, either simply in form of explicit research mandates from the addressees or implicitly by problems of uncertainty, incompleteness and ambiguity of modern scientific knowledge with respect to their impact on and utility for society. Hence, the extra-scientific dimensions of interdisciplinary deliberations might challenge the results and validity claims of these efforts.

From this background, the present study aims at critical reflections of the practise of interdisciplinary research and at its validity conditions within and beyond the scientific system. It is not a manual or recipe book for meaningful or best-practise interdisciplinary research but a basis for further discussion and improvement of interdisciplinary endeavours—no more, no less. The content of this volume is the result of more than 3 years' exercise by a working group, which has been established at the European Academy GmbH. The group met 15 times to discuss and frame the topic and to reflect relevant initial theses and papers, which were finally developed to the chapters and conclusions of this present study by iterative refinement in the course of the whole project.

The working group consists of experts renowned in the fields of epistemology, practical philosophy, technology assessment and scientific policy advice. The members and authors of this study are: Martin Carrier (Bielefeld), Carl Friedrich Gethmann (Siegen), Gerd Hanekamp (Bonn), Matthias Kaiser (Bergen/NO), Georg Kamp (Bad Neuenahr-Ahrweiler), Stephan Lingner (Bad Neuenahr-Ahrweiler), Michael Quante (Münster) and Felix Thiele (Bad Neuenahr-Ahrweiler). Most of the authors have contributed to other relevant publications within the academy's book series ‘Ethics of Science and Technology Assessment’ such as the volumes on “Rationale Technikfolgenbeurteilung” (1999), “Ethik in der Technikgestaltung” (1999), ‘Interdisciplinarity in Technology Assessment’ (2001), ‘Enabling Social Europe’ (2006) and ‘Business Ethics of Innovation’ (2007) among others.

Additionally, two events with the incorporation of external experts improved the formation of the findings of this study: In an early phase of the study, a public symposium on interdisciplinary research was held in October 2010 in Mainz. The conference aimed at exploring the tension between scientific validity claims of interdisciplinarity and societal expectances thereupon. Besides members of the working group, the following invited speakers contributed to the fruitful discussion with the audience: Prof. Dr. Claudius Geisler (Mainz), Prof. Dr. Bernward Gesang (Mannheim), Prof. Dr. Armin Grunwald (Karlsruhe), Prof. Dr. Eberhard Knobloch (Berlin), Prof. Dr. Klaus Mainzer (Munich) and Prof. Dr. Jan C. Schmidt (Darmstadt). Central papers of the conference were published in the Springer journal *Poiesis & Praxis. International Journal of Ethics of Science and Technology Assessment*, Vol. 7(4) in June 2011. In September 2013, a more focused workshop was held at the premises of the European Academy. The workshop aimed at the review and discussion of the working group’s interim results with those external researchers with specific competences in methodology of interdisciplinarity and scientific policy advice. We thank Prof. Dr. Hanne Andersen (Aarhus/DK), Prof. Dr. Armin Grunwald (Karlsruhe) and Prof. Dr. Harry van der Laan (Wyk/NL) for their evaluation efforts.

For careful editing of the ‘References’ section of this volume, we also express our gratitude to Bettina Schwab (Bayreuth) as well as to the academy’s receptionists for their helpful meeting support.

Siegen, November 2014 — Carl Friedrich Gethmann
Bad Neuenahr-Ahrweiler — Stephan Lingner

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About the Authors

The contents of the present volume were discussed, edited and integrated collectively by all authors of this study. The results reflect therefore the perspective and position of the entire group of authors. However, due to practical reasons individual authors took primary responsibility for single chapters as follows:

Chapter 1: all authors
Chapter 2: Martin Carrier (2.1; 2.2.1), Felix Thiele (2.2.2), Carl Friedrich Gethmann (2.3)
Chapter 3: Martin Carrier (3.1), Michael Quante (3.2), Stephan Lingner (3.3)
Chapter 4: Georg Kamp (4.1), Matthias Kaiser (4.2; 4.3.1), Gerd Hanekamp (4.3.2), Carl Friedrich Gethmann (4.3.3)
Chapter 5: all authors

Martin Carrier studied Physics, Philosophy and Education at the Westfälische Wilhelms-Universität Münster and completed his Ph.D. in Philosophy in 1984 from the University of Münster. His thesis is on Lakatos' Methodology and the History of Chemistry in the eighteenth century. From 1984 to 1989, he was Lecturer in Philosophy at the University of Konstanz and in 1989 he habilitated in Philosophy at the University of Konstanz dealing with the thesis on the Relation between Theory and Evidence in Space–Time Theories. From 1989 to 1994, Carrier was Akademischer Rat (tenured position) at the University of Konstanz, from 1994 to 1998 Full professor for Philosophy (philosophy of science) at the University of Heidelberg, and in 1998 Full professor for Philosophy (philosophy of science) at Bielefeld University. In 2000, he was appointed to the German Academy of Scientists, Leopoldina, from 2002 to 2009 he was member of the Board of Directors of the Bielefeld Center for Interdisciplinary Research (ZiF), in 2003 he was appointed to the Academy of Sciences and Literature, Mainz and in 2008 he received the Gottfried-Wilhelm-Leibniz Prize for 2008 bestowed by the DFG. Since 2008, he has been member of the University Council of Bielefeld University. Since 2010, he is member of the "Academia Europaea/The Academy of Europe". Since 2012 he is member of the Berlin-Brandenburg Academy of Science. His research fields include theory change, theory-ladenness and confirmation theory, conceptual relations among theoretical systems, space–time philosophy and methodological problems of applied research.

Carl Friedrich Gethmann studied Philosophy at Bonn, Innsbruck and Bochum; in 1968 lic. phil. (Institutum Philosophicum Oenipontanum); in 1971 Dr. Phil. at the Ruhr-Universität Bochum; in 1978 Habilitation for Philosophy at the University of Konstanz; in 2003 Honorary Degree of Doctor of Philosophy (Dr. phil. h.c.) of the Humboldt-Universität Berlin; in 2009 Honorary Professor at the Universität zu Köln; in 1968 scientific assistant; in 1972 Professor of Philosophy at the University of Essen; in 1978 private lecturer at the University of Konstanz; since 1979 Professor of Philosophy at the University of Essen; and gave lectures at the universities of Essen and Göttingen. He was called to the Board of Directors at the Akademie für Technikfolgenabschätzung Baden-Württemberg combined with a full professorship of Philosophy (1991, refused) and to full professorship at the universities of Oldenburg (1990, refused), Essen (1991, accepted), Konstanz (1993, refused) and Bonn (1995, refused). Since 1996, he has been Director of the Europäische Akademie zur Erforschung von Folgen wissenschaftlich-technischer Entwicklungen Bad Neuenahr-Ahrweiler GmbH (European academy for the study of the consequences of scientific and technological advance); member of the Academia Europaea (London); member of the Berlin-Brandenburgische Akademie der Wissenschaften; member of the Deutsche Akademie der Naturforscher Leopoldina (Halle); and member of the Bio-Ethikkommission des Landes Rheinland-Pfalz. 2006–2008 President of the "Deutsche Gesellschaft für Philosophie e.V." Since 2008 he is member of the Deutsche Akademie der Technikwissenschaften "acatech". Since February 2013, Gethmann is member of the German Ethics Council (Deutscher Ethikrat) on the suggestion of the German Federal Government. His main fields of research include linguistic philosophy/philosophy of logic, phenomenology and practical philosophy (ethics of medicine/ethics of environment/technology assessment).

Gerd Hanekamp worked for the Europäische Akademie Bad Neuenahr-Ahrweiler GmbH from 1996 to 2005 organizing and participating in interdisciplinary research, the last 3 years as the academy's Deputy Director. From 2005 to 2007, he was project coordinator for the Excellence Initiative of the German federal and state governments at the Office of the Science Council. He took his present position as "Head of Programs" at the "Deutsche Telekom Stiftung" in November 2007. Gerd studied Chemistry, Political Sciences, Business Administration and Philosophy of Science at the Universities of Heidelberg, Marburg and Hagen as well as at the Ecole Nationale de Chimie in Lille. He holds a Diploma in Chemistry and a Doctoral degree in Philosophy from the University of Marburg.

Matthias Kaiser completed Dr. Phil. at University of Frankfurt after studies at the universities of LMU Munich, Frankfurt am Main, Oslo and Stanford. He is Professor at and Director of the Centre for the Study of Science and the

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Georg Kamp merchandising apprenticeship in 1979–1982; retail salesman in 1982–1984; studies in Philosophy und German Literature and Linguistics in Bochum, Duisburg and Essen; scientific assistant at the Institut für Philosophie at the Universität Duisburg-Essen in 1993–1998; Ph.D. studies, graduation (Dr. phil.) with a thesis on "Praktische Sprachen. Zur Möglichkeit und Gestaltung des Argumentierens in regulativen Kontexten" at the Universität Essen in 1993–1998; member of scientific staff of the Europäische Akademie GmbH in 1999–2002; freelance consultant, lecturer and editor (during parental leave) in 2002–2005; Cooperative Education "Master of Mediation" at the FernUniversität Hagen in 2005–2006; coordinator of the project "continuo—Diskontinuierliche Erwerbsbiographien und Beschäftigungsfähigkeit in kleinen und mittleren Unternehmen" in 2005–2006; and since 2006 member of the scientific staff of the European Academy GmbH, coordinator of the interdisciplinary projects "Responsibility for Future Generations. Implementation of Sustainability in Schooling" (2006–2008), "Radioactive Waste. Technical and Normative Aspects of its Disposal" (2008–2010) and "Long-term Planning. The Relevance of Social and Cognitive Resources for Sustainable Economic Activities" (2010–2013).

Stephan Lingner has been deputy director of the European Academy since 2005. He studied Geosciences and Chemistry at Würzburg, Tübingen and Münster until 1990. He received his diploma in Geology at Tübingen University and gained a Dr. rer. nat. at Münster University for his contribution on the material and chemical evolution of the lunar crust. His early work focused on systems analysis of new space exploration options at the German Aerospace Center (DLR), Cologne. In 1996 he entered the Europäische Akademie GmbH as project coordinator of several assessment studies on climate, soil and noise protection as well as on this present study. He taught ecology at Koblenz University of Applied Sciences until 2007 and acted as managing editor of *Poiesis & Praxis. International Journal of Ethics of Science and Technology Assessment* until 2012. Stephan Lingner is member of the Philosophy of/as Interdisciplinary Network (PIN) and of the European Space Policy Research and Academic Network (ESPRAN). He is also coordinating member of the "Netzwerk Technikfolgenabschätzung" (NTA). His fields of work comprise methods and application of technology assessment, environmental studies, space exploration and planetary research.

Michael Quante (∗1962) is Full professor of Practical Philosophy in the department of philosophy at the Westfälische Wilhelms-University. He is Speaker of the Centrum für Bioethik and Co-editor of the *Hegel Studien*. His areas of specialization include German idealism, theory of action, personal identity, ethics and biomedical ethics. *Books*: *Hegel's Concept of Action* (Cambridge University Press 2004, pbk. 2007), *Enabling Social Europe* (Springer 2005; co-authored with Bernd v. Maydell et al.), *Discovering, Reflecting and Balancing Values: Ethical Management in Vocational Education Training* (Hampp 2014; co-authored with Martin Büscher), *Hegel's Phenomenology of Spirit* (Cambridge University Press 2008, co-edited with Dean Moyar), *Moral Realism* (Helsinki 2004 (= Acta Filosofica Fennica Vol. 76), co-edited with Jussi Kotkavirta) and *Pragmatic Idealism* (Rodopi 1998, co-edited with Axel Wüstehube). *Die Wirklichkeit des Geistes* (Suhrkamp 2011), *Menschenwürde und Personale Autonomie* (Meiner 2010; 2nd ed. 2014), *Karl Marx: Ökonomisch-Philosophische Manuskripte* (Suhrkamp 2009), *Person* (De Gruyter 2007, 2nd ed. 2012), *Einführung in die Allgemeine Ethik* (Wissenschaftliche Buchgesellschaft 2003, 5th ed. 2013), *Personales Leben und menschlicher Tod* (Suhrkamp 2002), *Ethik der Organtransplantation* (Harald Fischer Verlag 2000, co-authored with Johann S. Ach and Michael Anderheiden), *Hegels Begriff der Handlung* (frommann-holzboog 1993).

Felix Thiele has been Deputy Director of the European Academy since 1999. He studied Medicine in Hamburg and Heidelberg. In Heidelberg he gained a Dr. med. with research on high blood pressure. In addition, he holds a Master of Science in Philosophy and History of Science from the London School of Economics. At the academy he was member and leader of several projects dealing with moral problems of the life sciences and medicine. He is Privatdozent at the University Duisburg-Essen and teaches Practical Philosophy, especially Bioethics at the universities of Bonn and Mainz. He was co-leader of a research group on human dignity in medicine (2009/2010) at the Center for Interdisciplinary Research (ZIF) in Bielefeld and coordinator of a EU-funded research project on nanotechnology-based diagnostic systems for arthritis (2010–2014). His research covers a broad spectrum of bioethical topics such as animal and environmental ethics, health care and research ethics.

Chapter 1
Introduction

1.1 Background

Today, *interdisciplinarity* seems to become a matter of course and a promising answer for dealing with contemporary questions of science and/or society within a complex, uncertain and confusing world. Proponents of this idea would argue that this world appears to be much better accessible to interdisciplinary reflection than to ordinary disciplinary analysis. In this way, interdisciplinarity seems to become a "repair measure" against the specialisation paradigm of modern science (Mittelstraß 1992). Aligned to these conceptions, interdisciplinarity is often acclaimed by numerous public funding and research schemes in Europe and abroad. Consequently, modern research is frequently guided by the idea of interdisciplinarity. A simple "Google" search[1] gives a ratio of 796,000–185,000 hits for the term "interdisciplinarity" compared to "disciplinarity", which might reflect the above mentioned expectations and observations in some way.

On the other hand, interdisciplinarity might risk to become a fashionable *buzz word* for reasoning and upgrading of any, even poor analysis beyond well-established disciplinary practices. But interdisciplinarity is not a universal tool for every scientific question. Therefore, it seems to be neither necessary nor even prudent in every case or for each aim of modern research. For instance in Löffler's article on the shortcomings of a still promising research conception (German title: "Vom Schlechten des Guten"), the author examines cases of ignorant interdisciplinarity. One might be concerned that the notion of interdisciplinarity could lose its meaning by careless use (cf. Grunwald 2013). Similar experiences from other terminological hypes like those of "sustainability" and "innovation" might give an idea what could be at stake. The same holds true for the term trans-disciplinarity which is yet at the advent of its terminological career and which is sometimes even used as synonym or variant of interdisciplinarity.

[1] Search on 17 June 2013.

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Nevertheless, recent societal challenges often need rational scientific decision support, which frequently relies also on appropriate interdisciplinary knowhow, thus expanding its utility beyond the science system. Corresponding expectations and advisory offers from a *trans-disciplinary perspective* and resulting knowledge as well as related validity questions challenge the sensitive relation between society at whole and the mainly publicly funded science subsystem within that society.

At first, the study at hand will thus contribute to the clarification of the relation between interdisciplinarity and disciplinarity as well as to their specific objectives and practices. Corresponding efforts should reflect the state-of-the-art of the empirical and methodological knowledge from all relevant disciplines. Secondly, their outcome should be also acceptable and useful—in the trans-disciplinary arena and beyond the scientific perspective. The exchange of scientific and practical knowledge between these spheres will therefore also be an issue here. Within this, the demands from the society for sound advice as well as the prospects and conditions for its effective provision by the science system are considered. This encompasses also the comparative reflection of different operational trans-disciplinary approaches with regard to their different contexts, practical aims and theoretical foundations.

1.2 The Notions of Inter- and Trans-Disciplinarity as Applied in This Report

The apparent current plurality of scientific disciplines might be seen as a consequence of different and changing cognitive interests of researchers and practical challenges from their milieus over historical times, among others. This differentiation along emerging topics and appropriate theories, terminologies and methods to deal with, as well as specific societal needs for action might have led to the apparent distinction of disciplinary object domains. In modern democratic societies, choices are often difficult, ambivalent and uncertain with regard to their consequences and their acceptability—especially over long time periods of planning. Therefore, tailored scientific advice will not necessarily fit into the pre-established disciplinary landscape, which means to create cross-disciplinary frameworks, while securing the complementarity and coherence of the relevant multi-disciplinary expertise. *Societal relevance and complexity* of scientific questions will hence determine the adequate setting of related research. Depending upon this, four research types might be generally distinguished (see Table 1.1).

The matrix below visualizes, that research might be either conducted *disciplinarily* or *inter-disciplinarily*, depending upon the complexity of the respective scientific tasks. Within this framework, the perspective might be either *epistemic*—and therefore directed towards the enhancement of understanding—or it might be societal resp. *trans-disciplinary*, thus aiming at the solution of practical problems. Examples for the epistemic, cognition-driven types might—in the case of type 1—encompass fundamental research from mathematics, philosophical logics, theoretical physics

Table 1.1 The general modes of research

1. Disciplinary and Epistemic, e.g. field theory formulation and deduction	**4. Disciplinary and Trans-disciplinary**, e.g. design and construction of tunnels and bridges
2. Interdisciplinary and Epistemic, e.g. investigations in planetary evolution	**3. Interdisciplinary and Trans-disciplinary**, e.g. climate impact and policy assessment

etc. In case of the epistemic type 2, research might e.g. be represented by investigations of complex and past processes of planetary evolution, which need in contrast to type 1 joint effort of astrophysics, chemistry and geology (type 2) in order to understand the present planetary dynamics, material distribution and radiation patterns in the solar system.

Another trans-disciplinary type (type 3) however, might be represented by climate impact and policy analyses, which are conducted with respect to serious public concerns on the future habitability of the planet Earth. This type of research needs even more extensive and broad interdisciplinary approaches regarding quite different knowledge types and disciplines involving meteorology, ecology, epistemology, economy, jurisprudence, practical ethics and political science. Moreover, this research type is confronted with uncertain and conditioned scenarios of the future and with normative ambiguity with regard to intra- and intergenerational justice of proposed strategies to cope with the climate problem, thus making corresponding efforts so ambitious. Finally, trans-disciplinary research must not necessarily be rather complex or ambivalent with regard to its results (type 4). For instance, the construction of bridges is clearly an application oriented disciplinary task of engineering, only including the consideration of technical guidelines, which are still parts of contemporary engineering knowledge despite their normative aspirations.

In this way, research with trans-disciplinary perspective aims at the development of sound, *relevant* and—especially with regard to the complex type 3 research—*acceptable* advice for the solution of apparent societal problems, or of those, which are brought forward by the public or other stakeholders. Trans-disciplinary efforts address therefore the competent actors and institutions as well as the affected parties. In this respect they have to be compatible to the *orientation needs* of the addressees. Within the context of type 3 research, interdisciplinarity will not establish itself as an on-going universal scientific endeavour but rather as *targeted and topical* cross-disciplinary discourses with limited lifetimes. Corresponding research processes will therefore be organised as *temporary projects* rather than as infinite programmes. Figure 1.1 gives an impression of this idea in terms of the case of climate impact and policy evaluation

This example encompasses many and partially distant scholarly branches, thus establishing an extended interdisciplinary framework. Within this framework, meteorology is naturally a relevant discipline, which represents the knowledge on the object—the climate system—including its subsystems, their dynamics and their

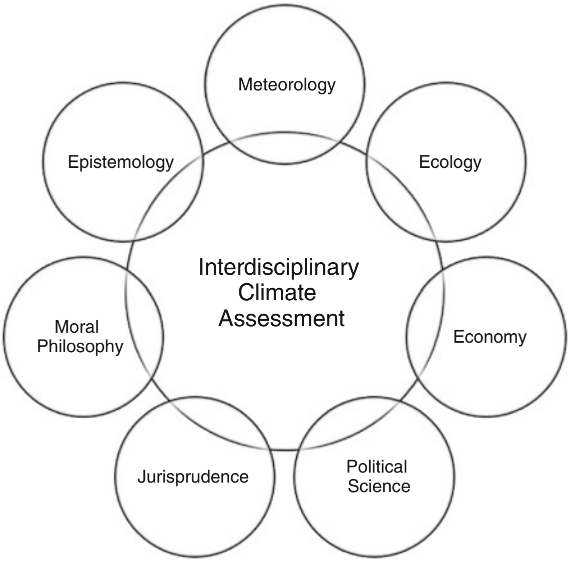

Fig. 1.1 Multi-disciplinary structure of a climate impact and policy assessment

simulation as well as their stochastic characterisation. Ecology and economy are the central impact dimensions of climate change with clear societal relevance. Philosophy of science adds here, especially with regard to the assessment of the knowledge status of modelled climate data and of choices under conditions of incomplete knowledge. Acting under unclear conditions is also an issue for moral philosophy, which aims at finding universal rules for dealing with long-term problems of the future and corresponding sustainability conceptions. Finally, binding normative claims from jurisprudence as well as the social reality of emerging climate regimes as domain of political science are central issues here. All these disciplines will have to be represented within such a climate project. The problem dimension of climate change is partially based on common knowledge, which feeds into the interdisciplinary assessment trans-disciplinarily from outside. Then again, the scientific appraisal might in the end work out practical recommendations, providing feedback towards society.

Figure 1.1 might serve also as point of departure for the description of the relevant scientific activities over time: The disciplinary circles still represent here permanent research programmes. The normal mode of disciplinary research makes them “high-end tools” with definite competences already at hand for specific purposes. The system of disciplines thus can be seen as a *toolbox*, which supports the solution of complex interdisciplinary questions. Related interdisciplinary research can thus be made to measure on demand according to specific cases without relying on any permanent de-disciplinised scholarly programmes with “fixed menues”. This option and the problem-oriented nature of this kind of research indicate conducting it in the form of dedicated projects with limited life-time. In the following chapters of this study, it is worked out *inter alia* on what theoretical basis and in which way interdisciplinarity can be organised à la carte as well as which socially relevant validity claims can be derived from the corresponding research.

1.3 Overview of the Major Aspects of This Study

The main body of the study is built up by three consecutive chapters starting with a description of the constitution of science as point of departure for the following discussions on knowledge and acting as well as on trans-disciplinary deliberation. The results of this exercise lead to reasoned conclusions, which aim at informing researchers and research politics as well as at scientific policy advice.

1.3.1 Science in Society

The validity of scientific research—whether disciplinary or interdisciplinary—relies upon its compliance to credibility rules established within the science system, which are not independent from broader societal contexts. Within this study, science is unfolded as a service under *pressure of practice*, where it operates for the benefit of the public and on the marketplace of commercial interests (Sect. 2.1). Corresponding research will thus also follow those epistemic patterns, which align with utility considerations. Not surprising, the same holds true for decisions with respect to the selection of research topics.

The processes between science and its societal background are here explained as co-evolutive mechanisms, where impacts of values on science and vice versa are intertwined (Sect. 2.2.1). For science, it is conceivable that offering its service within the political arena might influence its intrinsic confirmation procedures. This influence and the above mentioned politicized relevance decisions on research agendas denote corresponding scientific output as *not value-free*. The modern scientific system is based on society's hope for social utility of its research output and applications developed on this output. What 'social utility' amounts to and what the 'responsible' means of achieving this utility are is subject to an extensive normative debate. Applied ethics is a suitable tool for the argumentative interpretation of normative terms such as 'social utility' and 'responsible scientific research'. Since the aim of applied ethics is to master moral conflicts and as this mastering includes avoiding moral conflicts, ethical reasoning has an important role in the early phases of research, including research agenda setting (see Sect. 2.2.2). Ethical reasoning must be augmented by input from other disciplines. Research agenda setting taking into account normative challenges is an important example of interdisciplinary co-operation in the sciences. Worldviews, i.e. fundamental orientational values such as naturalness, have important impacts on individual moral landscapes, and play a decisive role in individual assessments of scientific developments. A better understanding of the functioning of worldviews might make it easier to transfer controversies on modern science into argumentative tracks, and to prevent them from becoming ideological quarrels only.

The following Sect. 2.3 explains the *disciplinary differentiation* and classification of the sciences and humanities so far and its underlying rationales, which stem partly but not completely from the above mentioned pressure of practice. On that basis, the authors unfold the meaning and interpretation of interdisciplinarity as well as frameworks of weak or strong interdisciplinary research. This section continues with the specification of trans-disciplinarity as competence for interaction between certain scientific and extra-scientific regimes. This competence seems not to be equally distributed among the disciplines so far; the section will then close with a note on the specific role of philosophy and its branches in trans-disciplinary research.

1.3.2 Knowing and Acting

Naturally, scientific knowledge cannot be simply transferred to the acting level. Therefore, it needs appropriate scientific expertise filling the gap between the respective different "worlds". The significance and broad acceptance of expertise depends highly on *trustworthiness*, which has epistemic but also social requirements to be realized (Sect. 3.1). In this way, the utility of expert judgment relies heavily on epistemic but also social *robustness*. The latter implies also the ambition for expert legitimacy and social participation. The epistemic robustness for instance can be attained by appropriate handling of modern model-dependent but uncertain knowledge.

Problem-oriented and thus often interdisciplinary research has to comply with certain *virtues and instrumental conditions*, depending upon different goals and frameworks of interdisciplinarity as chosen (Sect. 3.2). These conditions are specific to horizontal and vertical approaches of interdisciplinarity as well to those which aim either at mere analysis or even at the evaluation of problems at stake. Critical virtues of interdisciplinarity and trans-disciplinarity will be discussed as well as more general social aspects like trust and personal requirements in corresponding frameworks.

The Sect. 3.3 describes how interdisciplinary research is operationally conducted. A short overview reflects the landscape of interdisciplinary research worldwide. The focus moves then to a more thorough description of the method of "*rational technology assessment*" as an example for institutionalized interdisciplinary deliberation. Besides general features of this approach, the procedural steps of rational technology assessment will be shown.

1.3.3 Trans-Disciplinary Deliberation

Science-based policy advice is the ultimate objective of trans-disciplinary deliberation. Section 4.1 discusses the corresponding requirements for finding and formulating sound recommendations and suggestions for policy makers. It is debated whether scientific advice should be limited to aims-means considerations or if and how far the objectives of the clients themselves should be subjects of corresponding deliberations. Within this, the authors argue about the role of normative sciences[2] and of organizational matters including measures for quality management of policy advice.

It is also disputed in how far *public participation* might be an opportunity or a problem for trans-disciplinary deliberation. Section 4.2 first offers some clarification on the notion of participation as well as a historical overview of participation from the view of the social sciences and in the fields of public administration, development, technology transfer and technology assessment. Specific deliberative challenges from public participation stemming from limited competence of the citizens and possible stakeholder fatigue but also any levelling tendencies to "tribalise" science and its representatives, are here discussed further.

The validity of trans-disciplinary deliberations also depends on additional, more specific justification challenges, which arise around the notion of uncertainty and w.r.t governance and communication problems of scientific expertise: Scientific uncertainty is a typical consequence of modern research although it seemingly fades in the public debate. Uncertainty stems for instance from variable modelled knowledge or from diverse methodological designs of similar research questions. Section 4.3.1 illustrates a corresponding classification scheme for uncertainty and explains the *relation of uncertainty to precaution* as one possible option to react if societal issues are affected. For this aim, the notion of the precautionary principle is clarified and analysed w.r.t. choices of different strategies and options to act. Section 4.3.2 debates again the issue of scientific policy advice—this time w.r.t. its impact and its *governance*. Finally, the necessary communication between *science and society* is reflected (Sect. 4.3.3). For this aim, the science system is critically analysed w.r.t. its relation to society at large, w.r.t. the virtues of the researchers, w.r.t. scientific policy advice and w.r.t. the media. This section argues on problems of selective information, on appropriate selection of reliable experts and on the risk of medial trivialization of science ethics.

The report ends with a concluding appraisal on best-practice for interdisciplinary research and its trans-disciplinary validity (Chap. 5). Corresponding conclusions and recommendations address four main groups of recipients within research and politics: (1) scientists who are themselves involved in interdisciplinary research, (2) experts and their social and political clients in need for scientific advice, (3) decision makers in research politics, and (4) decision makers in higher education politics.

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[2] This continental European notion includes scholarly activities like jurisprudence or philosophical ethics.

Chapter 2
Science in Society

2.1 Science as a Transdisciplinary Endeavor

Martin Carrier

2.1.1 Science Operating in the Marketplace and the Social Arena

Science is not appreciated by the general public because it ventures to capture the processes in the first microsecond after the Big Bang or to identify the fundamental parts of all matter. Rather, public esteem—and public funding—is for the greater part grounded in the expectation that science-based technology development is a driving force of the economy and helps boost its competiveness. Consequently, it is not scientific understanding as such that is highly evaluated in the first place but the transdisciplinary character of science: research takes up problems posed and demands articulated from outside of science. The research agenda of science as a transdisciplinary endeavour is formed by extra-scientific influences.

It is true, science was never pure. Only a tiny portion of scientific research was conducted out of pure intellectual curiosity and nothing else. In fact, the promise of the Scientific Revolution of the seventeenth century included at the same time the improvement of human understanding of nature and the betterment of the human condition. Francis Bacon's slogan that knowledge is power meant that insights into nature's workings are suitable for creating an increased capacity for controlling nature. Knowledge was expected to further public utility and to serve economic interests right from the start. However, science failed to make good on this promise for the centuries to come. Take Christopher Wren who was well acquainted with the newly discovered Newtonian mechanics when he constructed St. Paul's Cathedral. Newton's theory was taken to capture the structure of the universe but failed to provide any help for solving practical problems of mechanics. Instead, Wren drew

C.F. Gethmann et al., *Interdisciplinary Research and Trans-disciplinary Validity Claims*,
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on medieval craft rules. The whole industrial revolution of the eighteenth century was propelled by artisans and tinkerers. Science was conspicuous by its absence. This has changed fundamentally. In the course of the nineteenth century, science acquired the complexity and sophistication to deal with the intricate conditions characteristic of real-world processes and industrial procedures. At the turn of the twenty-first century, the Baconian vision of science-based technological progress has come true—but is now regarded as a mixed blessing. Today, large amounts of research are financed by economic companies and conducted out of commercial interest. Public funding of research is mostly due to the same motives. Financial support of academic and industrial research alike is widely understood as a kind of investment in economic growth. Research in the natural sciences has become a major economic factor and is viewed as a catalyst of industrial dynamics (Carrier 2004a, 2010a, 2011b).

I distinguish between "epistemic research," on the one hand, and "application-oriented research" or "application-driven research," on the other, as the relevant kinds of research whose features need to be clarified in order to bring out the nature, benefits, constraints, and drawbacks of transdisciplinary research. Epistemic research is traditionally conceived as academic fundamental research and is characterized by the search for understanding; application-oriented research includes research endeavours that are driven by the search for utility. Yet this conceptual distinction is not supposed to entail that any given research project belongs exclusively into one of these categories. On the contrary, the same research endeavour may strive to accomplish some practical benefit and at the same time aim to deepen our understanding of natural processes. For example, Louis Pasteur famously sought to elucidate fundamental biological processes and by the same token to prevent beer, wine and milk from spoiling or protect animals and humans from rabies (Stokes 1997). Yet in spite of their possible numerical identity, epistemic and application-oriented research projects can be separated conceptually by appeal to the goals pursued or, correspondingly, by the success criteria invoked. The conceptual distinction does not rule out that a given research project serves both ends simultaneously (Carrier 2011a).

Worries about the prevalence of transdisciplinary research have been articulated that mostly grow out of concerns about the detrimental impact of commercialization on the quality of the knowledge produced. These worries are based on the impression that the dominance of economic interests might narrow the research agenda, encourage sloppy quality judgments and tendentious verdicts. Epistemic challenges that transcend immediate practical needs are feared to be ignored. Commercialization is assumed to undermine the demanding test procedures inherent in respectable research. In this vein, physicists Sylvan Schweber and John Ziman take the requirement of practical usefulness as a source of corruption of the research process. If science is guided by commercial goals and short-term material interests, it will lose its creativity and its spirit of critical scrutiny (Schweber 1993; Ziman 2002).

According to such voices, science is likely to suffer in methodological respect from the transdisciplinary orientation. The apprehension is that the prevalence of economic incentives and the limitation to short-term practical problem-solutions sap the epistemic standards that used to characterize research. Scientists are feared to lose their neutrality and objectivity and to adjust their research outcome to the expectations of the sponsor. Science is claimed in some quarters to be biased and to be for sale. Economic ambitions are said to drive out epistemic commitments (Carrier 2010a).

2.1.2 Epistemic Features of Application-Oriented Research

Such misgivings regarding the assumed diminution of research quality are motivated to a great extent by the supposedly purely pragmatic attitude prevalent in application-driven research. The proper functioning of some device is its chief criterion of success; intervention, not understanding, is at the focus (Polanyi 1962). Such a thoroughly pragmatic approach is claimed to induce three kinds of epistemic deficiency. The first flaw is the superficiality or the diminished epistemic penetration of application-oriented research. Theoretically integrated laws are replaced by observational regularities. Second, the emphasis on intervention brings lax standards of judgment in testing and confirming assumptions in its train. The third worry addresses a supposed lack of creativity. In sum, the claim of epistemic decline says that targeted, application-focused research tends to neglect understanding, to employ sloppy procedures of quality control and to be unimaginative and barren (Carrier 2011a).

As to the first item, I wish to explore an example in which application-oriented research seemed to ignore underlying mechanisms and to be satisfied with superficial causal relations. Consider the identification of starter genes which trigger gene expression and are thus suitable for controlling genetic processes. For instance, the so-called "eyeless" gene governs eye morphogenesis in *drosophila* and other species. If the expression of the gene is blocked, no eyes develop—which is why the gene is somewhat misleadingly called "eyeless." The activity of eyeless in suitable tissue is sufficient for eye formation. That is, eyes can be generated by appropriate stimulation in the legs or wings of flies. But eyeless merely prompts a cascade of intertwined genetic processes which only in its entirety generates eyes. The gene operates as a trigger and thus permits the control of eye morphogenesis without any deeper understanding of the underlying processes.

In the 1990s, biotechnologists argued on such grounds that the control of biological processes may well dispense with theoretical understanding. Genes are tools for producing certain effects, and this is what biotechnology is all about: identifying switches to press. There is no need to disentangle the concatenation leading from eyeless up to the working eye. Pressing the initial switch is everything biotechnology needs to care about (Bains 1997). Put more generally, the argument was that technical intervention can thrive on identifying the initial and the final steps of a causal sequence and may thus be satisfied with observational regularities and empirical adjustments. Epistemic penetration is an effort that application-driven research may well spare itself.

Yet the later development turned out to grossly violate this expectation. More specifically, the various factors that control gene expression have gained prime importance for biotechnological research endeavours. Gene activity is regulated by the action of proteins which are in turn produced by other genes within the cell or stimulated by other influences from outside. Thus, the activity of a given gene depends heavily on its context. For instance, in stark contrast to eyeless, the "distalless" gene acts in a more specific way and affects embryonic development differently. In caterpillar embryos, the expression of distalless stimulates the formation of legs, whereas in developed butterflies the same gene generates coloured eye-spot patterns on the wings (Nijhout 2003, p. 91). Obviously, in some instances the context is of critical significance for intervening reliably which speaks in favour of preserving the depth of epistemic penetration (Carrier 2011a, b).

The second worry concerns the emergence of a less careful practice of judging hypotheses in application-driven research. It is true; some such cases can be identified, in particular, a tendency to disregard welcome anomalies. If a device works better than anticipated before on theoretical grounds, most researchers in the context of application offer nothing but hand waving as to the underlying causes (Carrier 2004b; see Nordmann 2004). However, such shortcomings remain occasional and cannot be generalized. More often than not, the demanding standards of judgment that distinguish epistemic research are retained. The reason is not difficult to identify: superficially tested relations do not furnish a viable basis for operating devices reliably. Functional failures in products are often a threat to the manufacturer and this risk is augmented by gappy knowledge of the processes underlying the performance of a device. Conversely speaking, the theoretical integration or causal explanation of an empirical regularity improves the prospect of making reliable technical use of it. Disclosing causal mechanisms often opens up options for controlling a phenomenon, and giving a unified treatment may forge links to other relevant processes and thereby make accessible additional options for intervention (Carrier 2004b, 2011a).

However, there is a downside to application-oriented research. Most of the complaints about "science bought and sold," about superficiality and one-sidedness, focus on a single field: pharmaceutical research. More specifically, the overwhelming majority of reports about egregious methodological blunders, a striking loss of neutrality and disinterestedness, and the abandonment of more visionary goals stem from clinical trials of new medical drugs. As regards the reliability of clinical trials, surveys have demonstrated that comparisons of the efficacy of medical drugs agree to a large extent with the commercial interests of the sponsors of the pertinent study. In a survey of 107 comparative medical studies on competing drugs, not a single published paper was identified in which a drug produced by the sponsor of the study was found inferior to a product of a competitor. To all appearances, the prevalence of commercial interests does indeed create an epistemic predicament in medical research (Brown 2008).

However, clinical drug trials are uncommon in various respects and do not represent commercialized research in general. Properly speaking, the critical case of so-called phase III clinical trials (in which the efficacy of new drugs is tested on larger groups of patients) does not even constitute research in the first place since nothing new is intended to be discovered. This standardized legal procedure for admitting new drugs is taken as a cumbersome threshold that needs to be overcome. The research process, properly so-called, precedes these trials and is finished when the latter are set out. The aim pursued in clinical trials is not to generate new knowledge but to get supposed knowledge approved by the authorities in charge. Consequently, clinical trials are trivial in methodological respect; they involve nothing but routine procedures. No creativity, no novel perspectives are called for; the agenda involves no more than proceeding by the books. At the same time, the financial stakes involved are high. Finally, the relevant effects are often small and subtle; they arise only with a low frequency and can often be attributed prima facie to a lot of other factors. As a result, companies can hope to get away with a biased interpretation of the data.

These features are not typical of commercialized research. The exceptional factor is that those who pay for the study do not have any epistemic interests. The sponsors don't want to know; rather, they believe they know and want to pass an inconvenient and economically risky examination quickly. This is different in applied research proper, in which the sponsors of a study expect to gain new and useful knowledge. In fact, outside of the realm of phase III clinical trials, in none of the known scandals about data manipulation any outside interference on the part of the sponsors has been identified.[1] And this is plausible in the first place since tampering with the outcome would be against the interests of the sponsors. What they pay for is robust results which stand the test of practice, not the approval of wishful thinking that collapses under real-life conditions. Commercialized science does not enjoy the privilege of purely epistemic research to go wrong without thereby doing damage to the world outside of libraries and laboratories. As a result, the standards of reliability are frequently placed at a level comparable to academic research (Carrier 2010a).

The third epistemic worry mentioned is the supposed lack of imagination and creativity in application-oriented research. But this worry is without firm foundation. By contrast, what is striking is the seminal influence of application-oriented investigations on epistemic research. It sometimes happens that the basic knowledge requisite for generating some technological novelty is produced in the context of application. Practical challenges may raise fundamental questions which need to be tackled if the challenge is to be met. Applied research never merely taps the system of knowledge and combines known elements of knowledge in a novel way. Rather, applied research almost always requires constructing specific models which are apt to control the processes underlying a device. As a result, application-driven research is bound to involve some amount of creativity. But the amount of novel insights fed into the models varies among practical challenges. The invention of the

[1] The most spectacular cases of this sort in the past years were due to Friedhelm Herrmann and Marion Brach (Germany 1997), Jan Hendrik Schön (USA 2002), and Hwang Woo Suk (South Korea 2004).

dishwasher mostly relied on the ingenious connection of known parts and needed no new theoretical knowledge. Yet in *application-innovations*, new insights are gained in the course of an applied research endeavour. Such research is driven by technological aspirations, to be sure, but the necessary scientific basis is not yet available or not sufficiently developed. Application-innovation lays the scientific ground for a technological novelty. It is the epistemically fertile part of application-oriented research and improves our understanding of nature (Carrier 2011a).

Take the example of "giant magnetoresistance," which underlies today's hard disks. In the 1980s, the search for the effect was motivated by technological prospects. The relevant laboratories looked for physical means to efficiently alter electrical resistance by applying magnetic fields. One such effect was known for more than a century, namely, "anisotropic magnetoresistance," whose physical basis is the spin-orbit coupling of the conductor electrons. The pertinent research teams actively searched for stronger spin-related effects of this sort and eventually came across a new effect of spin-dependent electron scattering that relied on spin-spin coupling. This giant magnetoresistance represents a novel physical phenomenon which was discovered en route to the applied aim of packing data more densely. Moreover, the effect was correctly explained within this application-oriented research context (Wilholt 2006).

Application-innovative research is epistemically fruitful but unintentionally so. The motive lies with technological progress but among the results are epistemic gains. The attempt to improve the control of nature leads to better insights into nature's contrivances. Application innovation involves a mechanism for stimulating creativity. In such cases, technological difficulties raise theoretical problems that would have hardly been addressed otherwise. The lesson is that practical challenges may promote the development of novel and original epistemic approaches (Carrier 2011a). These considerations suggest that the transdisciplinary character of research, i.e., its focus on useful applications, need not diminish its depth, credibility and creativity.

2.1.3 Commercial Research Performed Secretly

A charge frequently levelled against corporate research concerns its insistence on intellectual property rights, which is criticized as engendering a privatization of knowledge. Robert Merton codified a system of "cultural values" that is supposed to be constitutive of the "ethos of science"; among these values is "communalism" (or "communism"—as Merton put it) according to which scientific knowledge is and remains in public possession. It is an essential and indispensable part of the ethos of science that scientific findings are public property. Scientific knowledge, Merton argues, is the product of social collaboration and is owned by the community for this reason. This is linked with the imperative of "full and open communication." Merton demanded, consequently, that scientific knowledge should be accessible to everyone (Merton 1942).

By contrast, industrial research and development projects are intended to produce knowledge that can be put to exclusive use. After all, companies are not eager to finance research whose outcome can be used for free by a competitor (Dasgupta and David 1994). As a result, important domains of scientific activity are constrained by industrial secrets or patents. The commercialization of research may thus go along with a privatization of science that compromises the public accessibility of knowledge (Rosenberg 1991; Concar 2002; Gibson et al. 2002). What is at stake here belongs to the essentials of scientific method: knowledge claims in science should be subject to everyone's scrutiny. The intersubjective nature of scientific method demands public tests and confirmation. Hypotheses developed behind closed doors are neither examined as severely as they would be if the hypotheses and their evidential basis were more widely accessible, nor can related research projects benefit from the new insights. From the epistemic point of view, such restrictions in the availability of knowledge are a cause of concern.

However, there are counteracting mechanisms that tend to push commercial research toward openness. Indeed, some features of present-day industrial research and development practice bear witness to the recognition that keeping research outcomes classified could hurt a company. Chief among them is the realization that openness brings important benefits in its train. This applies, first, to the cooperation among applied research groups that aim in similar directions. If two such groups each solved half of a given problem, sharing their knowledge may make them realize that they are done, whereas a lot of work would still be left to do if each one had proceeded in isolation. Sequestration can be a costly impediment to commercially successful research outcomes whereas cooperation may pay off economically (Carrier 2008).

Another incentive for waiving restrictions on communication is provided by the interaction of industrial research with the academic sector. One relevant effect is that taking advantage of the results produced by publicly funded fundamental research requires deeper understanding, which can be reached most conveniently by being locked into the pertinent research network. The reason is that part of the relevant know-how is tacit knowledge. It is hardly feasible to build a working device by relying on nothing but the knowledge published in research articles or laid down in the blueprints or patents that describe the operation of this device. Rather, research accomplishments achieved elsewhere are exploited most efficiently if one's own research laboratories have an advanced expertise in the relevant field—which they can best gain by conducting relevant research projects themselves. Yet being part of a research network demands making one's discoveries accessible to others (Rosenberg 1990; Dasgupta and David 1994; Nichols and Skooglund 1998).

Consequently, there are social mechanisms at work that discourage a policy of closed labs. In fact, a lot of industrial laboratories do publish their findings and seek recognition as scientifically reputable institutions. As a result, an exchange of ideas between academic and industrial research is found. It goes without saying that it would be better if all industrial research findings were publicly accessible from the outset. However, it would be worse if the knowledge had never been gained. It is not only the distribution of knowledge that counts but also its production. Given severe public budget restrictions, private funding contributes to securing academic research. Industrial sponsoring makes certain epistemic research projects feasible in the first place. Eventually, the conclusion is that secrecy in commercialized research raises problems, to be sure, but does not undermine the epistemic dignity of industrial research (Carrier 2010a).

2.1.4 Biases in the Research Agenda

The primacy of the context of application produces a change in how the research agenda is set (see Sect. 2.1.1). Whereas problems in fundamental research arise from the smooth unfolding of the proper conceptual dynamics of a discipline (Kuhn 1962, p. 164), the emphasis on utility directs attention to practical challenges. As a result, the agenda of transdisciplinary research in general and of industrial research in particular is shaped by commercial interests. Pursuing such interests is typically not of equal benefit for everyone. Consider the biased agenda of present-day medical research. Diseases prevalent in affluent countries enjoy high priority, as do methods of treatment that can be patented. Among the 1,360 new medical drugs that were admitted to the world market between 1975 and 2000, only 10 had been developed specifically for Third-World illnesses (Schirmer 2004).

The most popular remedy offered for such a biased research agenda is the democratization of science. Participatory procedures which are intended to include the stakeholders are widely advertised as a means for forging a consensus on science policy. A prominent proposal goes back to Philip Kitcher who elaborated the ideal of a "well-ordered science" in which representatives of the people engage in a process of deliberation. After having run through a process of mediation and tutoring by scientists, these groups of citizens decide about the research agenda (Kitcher 2011). It is doubtful, though, whether this procedure can be expected to produce the envisaged "fair" agenda. It is hard to believe that citizens of wealthy Western countries, even after having run through an extensive educational procedure, will approve democratically to cover the health care cost of developing countries. Democratizing topic choice in science cannot be expected to affect significantly the uneven distribution of drug research allocations on a global scale (Carrier 2010a).

An even deeper difficulty for the suggested public negotiation of the research agenda is that frequently no such agenda exists in advance. Many relevant research endeavours proceed in a knowledge-driven manner: the available knowledge including new findings is surveyed in order to identify ideas and options that could be transformed into novel devices. No systematic agenda is issued. Rather, the initial step is that new effects are disclosed and new capacities explored. Only subsequently is it asked to which use they might be put or which functions can be performed with them. That is, new effects drive new functions which in turn propel new technology. This is by no means an automatic process; it needs creativity and assistance. But basically the process of technological invention is driven from below. What can be accomplished technologically or which market niches exist are the salient questions, all of which are asked within a short-term perspective; no long-term ambitions are pursued. Many technological novelties are introduced into the market by knowledge-driven processes. Setting up a systematic, demand-driven research agenda for stepwise implementation is at odds with this anarchic, small-scale dynamics.

A more general conclusion is that commercialized research proceeds with the customer in view. In market economies the expected commercial success will decide about the industrial research agenda, and the latter has a lot to do with being in resonance with the desires and aspirations of the broader public. This means that commercialized research operates with an eye on the needs and demands of the people—at least of those with money to spend. It is true, the agenda of commercialized research is biased toward problem areas that bear economic prospects, but this emphasis is not completely at odds with the desires of the people (Carrier 2010a, 2011a).

2.1.5 Science in the Public Interest and Science as a Cultural Asset

However, it deserves emphasis that the biased research agenda set up in commercialized research is a questionable feature. It is impossible to do research on everything, and the selection of problems worth being studied depends on interests and values, which are often partisan and particular rather than universal and comprehensive. It can hardly be demanded of privately financed application-driven research that it be always conducted for the sake of the common good. The pursuit of public interests is in the first place a matter of the public. It is the lack of a public counterbalance which makes this obliquity so pernicious. The wrongful priority list of medical research is first of all the result of the decline of public medical research. In order to redress the balance, a different type of research is called for, namely, *science in the public interest* (Krimsky 2003). Research of this kind selects research questions according to the interests of all those concerned by the possible research results. Global warming is an example of a research endeavour of high practical importance, which neither grew out of epistemic research nor was it addressed by industrial research. Rather, the prevention or reduction of global warming is placed on top of the research agenda by political representatives and citizens. That is, biases in the research agenda can be corrected by setting the incentives appropriately and by publicly funded research. Science in the public interest is supposed to create a counterweight to the tacit influence of the rich and powerful on the questions to be pursued.

Market-oriented research and science in the public interest are equally transdisciplinary in kind. Problem selection is shaped by societal demands. However, epistemic research also merits support as a third branch of the scientific enterprise. One of the reasons that speak in favour of fundamental research is its cultural asset. This asset grows out of the pertinent mode of problem choice. Fundamental research picks research items according to epistemic aspirations. It chooses its problems independently of any extra-scientific interests. I take it that society will also benefit from an independent science that picks its topics autonomously and raises issues that are suggested by pursuing projects for the sake of understanding nature. Only under such conditions is science in a position to feed new issues and solutions into the public discourse. The problems of ozone layer depletion and climate change were brought up by scientists in the 1970s. It is indispensable that scientific research can turn to problems for whose solutions no one is willing to pay. Science needs the right to uncover inconvenient truths—even if the public prefers a reassuring lie. Science needs the right not to be customer-friendly. The conclusion is that transdisciplinary or demand-driven research is alright—provided that market-oriented research is complemented by research in the public interest, and provided that epistemic research receives its proper place as well.

2.2 Framing the Research Agenda

2.2.1 Values in Science

Martin Carrier

2.2.1.1 Science and Values Intertwined

Science is traditionally expected to tell us, what is the case—regardless of human intentions, wishes, or fears. As a result, values should have no place in the laboratory—or so it seems. In this vein, scientists sometimes object to the interrelations between science and values as a deplorable outgrowth of the politicization of science. On the one hand, political and social groups impose societal values on science; on the other hand, scientists appeal covertly to political values and pass their political views as scientific knowledge. This blurring between scientific information about what is the case and advocacy of a value-laden policy regarding what ought to be the case is said to corrupt science and politics at the same time. A plea uttered frequently from scientists is to keep these two areas of the descriptive and the normative strictly separate (Lackey 2007).

In the framework of this argument, values are assumed to cross the border between the descriptive and the normative when science becomes politicized. The interconnection between science and values is seen as an illegitimate offspring of the emergence of transdisciplinary science. In such a transdisciplinary setting, science responds to societal demand and enters the social arena, and this interrelatedness is criticized as overstepping conceptual bounds—to the detriment of the epistemic authority of science and of the room left to alternative policies. I argue in this chapter that the relationship between science and values is more complex. On the one hand, the distinction between the descriptive and the normative should be given heed to; on the other hand, some value judgments are inherently part of science. Values are involved in the process of knowledge acquisition in a large number of ways and respects. In particular, at least four types of values are relevant for science: *epistemic*, *economic*, *ethical*, and *social* values.

Epistemic or cognitive values express the commitment of science to the quest for understanding and truth. Moreover, such values give rise to evaluation criteria for hypotheses; they suggest, for instance, that hypotheses with a great unifying power or hypotheses elucidating causal mechanisms are to be preferred. Economic values commit science to contributing to utility. Such values are often criticized to dominate the contemporary research process. Ethical values concern demands of persons for health, liberty or integrity; social values express requirements of social groups to participate in social processes or to be protected against detrimental social effects. The interconnection between science and values manifests itself in a twofold way. On the one hand, science may influence the plausibility of value commitments; on the other hand, values may affect what is accepted as scientific knowledge. I go briefly into the first type of impact in Sect. 2.2 1.2 and will then dwell on the latter feature, the "value-ladenness" of science in Sects. 2.2.1.3 and 2.2.1.4.

2.2.1.2 The Impact of Science on Values

Scientific knowledge has an impact on values; in particular, knowledge may undermine or support epistemic, social and ethical value-commitments. Epistemic values express merits of knowledge beyond conformity to the facts. They serve to elaborate the commitment to science as a knowledge-seeking enterprise and express, in particular, requirements of *significance* and *confirmation*. Significance requirements are influential on the choice of problems and the pursuit of theories in epistemic research, confirmation requirements contribute to assessing the bearing of evidence on theory. As to the first role, epistemic values make the goals attributed to science as a knowledge-seeking enterprise more precise. For instance, scientists strive for knowledge that is valid in a wide domain; they appreciate universal principles. At the same time, they rate precision highly and correspondingly hold quantitative relations in esteem. Second, epistemic values are employed in assessing how well hypotheses are confirmed by the available evidence. Hypotheses need to exhibit certain virtues over and above fitting the phenomena in order to be included in the system of knowledge. Regarding confirmation, appeal to *non-empirical values* amounts to favouring certain forms of agreement with the observations over other forms. If a hypothesis coheres well with the background knowledge or anticipates new effects, it will be held superior to an empirically equivalent assumption that lacks these distinctions (Carrier 2011b).

The viability of epistemic values can be assessed differently according to the success or failure of certain hypotheses. In other words, sometimes we learn by interacting with nature which values can be upheld and which measures are called for to implement them. A well-known example is the discovery of the placebo-effect and the subsequent introduction of the methodological requirement of supplementing experimental subjects with a control group and doing tests in a double-blind manner (Laudan 1984, p. 38–39). Such methodological considerations derive from applying more basic epistemic values such as well-testedness to particular empirical arrays. Accordingly, observing and judging the success of particular scientific approaches is influential on the adoption of more general epistemic commitments.

Ethical values are taken by some people to rest on theological premises, and the latter can be sapped by science. Darwin's theory of evolution entails that humans did not emerge, at least not in their present shape, from the hands of the Creator. Thus, evolutionary theory undermines the credibility of a literal reading of the biblical *Genesis* and in this way reduces the binding force of certain moral principles, such as the commitment to the original sin, i.e., the claim that humans are by birth morally guilty.

Social values, too, may be influenced by scientific knowledge. For instance, the geneticist Luigi Cavalli-Sforza pointed out that humans form a comparatively young species and that, for this reason, they resemble each other genetically to a higher degree than the members of other biological species. Humans are related to one another to a degree uncommon in the animal world. This close genetic relation was invoked in support of ethnic equality. Claims to biological superiority or racism are discredited by the fact that the genetic variability within ethnic groups is larger than the difference between them.

In its traditional form, the thesis that science is value-free goes back to Max Weber. Weber claimed early in the twentieth century that empirical science can never enunciate binding norms or ideals. Values can be studied by science, to be sure: conceptual relations between values can be analyzed, the consequences and side-effects of adopting certain values be determined or the appropriateness of means for the realization of certain ends be examined. However, science cannot justify or disprove standards of value (Weber 1904). The adoption of this notion of value-free science leaves room for the *value-impact* of science, that is, the influence of science on normative attitudes—as just exemplified. This normative impact of science can be reconstructed by appeal to what Hans Albert has called "bridge principles." Such principles refer to factual presuppositions on which the persuasiveness of normative commitments rests (Albert 1968). For instance, the bridge principle of "congruence between cosmology and ethics" provides a basis for calling into question all those ethical commitments that draw on factors which are non-existent in light of accepted scientific knowledge. In the example given before, the moral implications of the biblical *Genesis* are undercut by abandoning the causal story upon which their credibility rests.

Although the adoption of bridge principles is compatible with the value-freeness of science in Weber's sense, science in this way gains a critical potential with respect to normative positions and may affect the persuasiveness of such positions. The body of accepted scientific knowledge may affect which value commitments appear convincing or implausible (Carrier 2006).

2.2.1.3 The Impact of Values on Science

The feature converse to this value-impact of science is the *value-ladenness* of science. Values play a role in science; they are brought to bear on judging knowledge claims. Values contribute to singling out what is admitted to the system of knowledge. Such values do not give way to science, as it was the case in the examples considered before, but rather shape what is taken as a piece of scientific knowledge.

First, it is largely uncontentious that epistemic values contribute to forging the body of knowledge in this way. The dependence of hypothesis assessment on non-empirical cognitive goals is brought to the fore, among other things, by the under-determination of theories by the available evidence. In order to drive the point home it is not requisite to appeal to in-principle or Duhem-Quine under-determination. Rather, it suffices to invoke sporadic and temporary under-determination. Transient empirical equivalence of theories repeatedly occurred in the history of science. For instance, Henri Poincaré's 1905 version of classical electrodynamics (based on Hendrik Lorentz' 1904 account) and Albert Einstein's 1905 special theory of relativity were observationally equivalent in their common domain of application until the Kennedy-Thorndike experiment was first performed in 1932. The salient point is that in spite of the agreement among the empirical consequences, a choice was made between the two theories by the scientific community. Once in a while, theories are taken to be superior although they lack the evidential edge on their rivals. Such cases serve to bring to light assessment procedures that operate more covertly in the more common cases of empirical divergence. They point to the fact that non-empirical cognitive values such as "fit with background knowledge" or "paucity of independent principles" in addition to "agreement with the facts" influence what is approved as scientific knowledge (Carrier 2011b).

Economic values or the commitment to utility shape large parts of scientific research. The underlying idea is that technological innovation is crucial for a thriving economy. The observation that science is generally viewed today as an essentially practical endeavour with a huge economic and social impact has spawned a large number of diagnoses which converge in the claim that science is presently undergoing a profound methodological and institutional transformation. Pertinent labels like "technoscience," "post-normal science" and "mode-2 research" suggest that the assessment procedures in science, the relationship between science and technology, and the relationship between science and society have been subject to fundamental change. The relevant positions hold that the context of application is now of primary importance for science and profoundly influences the formation of the research agenda, the assessment of hypotheses and the institutional features of research (Gibbons et al. 1994; Nowotny et al. 2001; see also Funtowicz and Ravetz 1993a, 1994, 2001).

Third, ethical values are commonly employed for judging the legitimacy of the means of knowledge gain. They serve to attach moral constraints on experimental setups used in research. For instance, such experiments must not violate human rights. The limiting function of ethical values on the procedures adopted in science is uncontentious. Still, in some cases, stem-cell experiments for instance, it is under debate what precisely our shared ethical commitments demand or prohibit.

2.2.1.4 The Politicization of Science

As a result, we witness a thorough influence of values on science. While epistemic and ethical values are widely granted legitimate rights within the research process, economic and socio-political values are often regarded as a potential threat to the objectivity of science. These worries are particularly pressing when such values become part of the test and confirmation procedures and are thus granted influence on which assumptions are adopted as part of scientific knowledge. One of the relevant arguments is that the dominance of socio-political values tends to induce superficiality and one-sidedness in the process of empirical scrutiny which undermine the demanding test and confirmation procedures in science. I argued in Sect. 2.1 that economic values have an ambivalent impact on the research agenda but leave the test and confirmation procedures largely intact. This is why the politicization of the research agenda and the context of justification are addressed more specifically in what follows.

Confirmation Procedures in Politicized Research

Political influences on the confirmation procedures in science are feared to undermine the credibility of science. If political factions and the general public are influential on the adoption or rejection of hypotheses, science seems to become part of political powerplay with the result that the scientific claims to objectivity and trustworthiness tend to be sapped. Examples of this sort can be found easily: Creationism or "Intelligent Design" is chiefly supported by certain social and political factions; the Bush-Administration was notorious for its attempts to silence critical voices from science warning against global climate change. The claim is that the adoption of certain assumptions is based on ideology rather than methodology and the consideration of the facts. Political influences of this sort can plausibly be assumed to impair the trustworthiness of science.

However, a closer look reveals that the situation is less transparent. First, not every form of politicization represents a danger to the epistemic integrity of science. For instance, Louis Pasteur's rejection of spontaneous generation was rooted in his anti-Darwinian attitude which was motivated in turn by his conservative political stance (Farley and Geison 1974). Still, Pasteur's arguments regarding this issue did not invoke such political considerations and appeared convincing from a scientific point of view alone. That is, his political views were merely "idle wheels" that did no epistemic harm.

In other cases political influence on the acceptance of hypotheses has an effect, but many of us tend to believe that this influence was for the epistemic good. Since the 1930s, IQ-tests are considered biased if they exhibit systematic differences in the overall test scores between male and female persons. In this case, a political influence on the test design can be surmised but it would not hurt the epistemic ambition of science. Analogously, early primate research focused on social conceptions like male domination of the females or male competition and fighting as a chief factor of reproductive success whereas today softer strategies like courting and making friends with the females are at the focus of attention. I take it that these two cases represent epistemically beneficial influences of political values on the confirmation practice of science. The challenge is to clarify which kinds of politicization are epistemically benign and which kinds do interfere with the objectivity of science.

Second, cases of external political pressure on science are rare and of limited impact. Galileo's fight for heliocentrism against the Catholic Church or Trofim Lysenko's assault on Darwinian evolution by variation and selection strongly backed by the authorities of Soviet Russia, remained short-lived episodes that quickly exited from the scientific scene. Characteristically, the Bush-Administration caved in after a few years and acknowledged man-made global warming.

Yet, political or social values typically act in a different fashion. They exert their influence not by outside pressure but without coercion and from within, as it were, when strong social attitudes encounter ambivalent evidence. Nineteenth century brain research is a case in point. The rise of physiology increasingly supported the conviction that brain structure should be able to reveal psychological features. Physiological parameters considered relevant at that time were brain weight, asymmetry between hemispheres, amount of convolutions, and prominence of the frontal lobe. Judged from today's perspective, these quantities are uninformative; psychological differences between humans do not depend on crude features of that kind. However, the history of nineteenth century brain research is replete with success stories claiming that correlations between such quantities and psychological or social properties had been established. The striking feature is that these alleged findings unfailingly reproduced the prejudices of the period. For instance, men, and mathematicians in particular, were purported to possess richer brain convolutions than women. Another ostensible finding was that brain physiological differences indicated descent (i.e., allowed a distinction between European and non-European origin), social rank, intelligence or personality (Hagner 1999, pp. 251–260).

The example reveals that attitudes regarding social groups may create expectations that are imposed, as it were, on unclear data and eventually dominate the interpretation of experience. On the other hand, even in the case under consideration, the malleability of the data was limited. The measured results were resistant to some interpretations and undermined the associated psycho-physiological hypotheses. For instance, the initial assumption of a correlation between intelligence and brain weight was abandoned in the later nineteenth century. After all, it appeared less than convincing that the brain weight of the then famous Göttingen mineralogist Friedrich Hausmann ranked in the lower third of the usual range (Hagner 1999, p. 259). To conclude, while the facts remain recalcitrant to some degree, it is undeniable that social values had a considerable impact on scientific thought (Carrier 2006).

This is by no means an exceptional case. Nineteenth century prejudices are easier to recognize because we no longer share them. In fact, however, a closer look at present-day scientific fields reveals an analogous entanglement of social notions concerning human gender relations with the content of scientific theories. Consider feminist archaeology. Traditional archaeology suffered from an androcentric perspective that produced a neglect of the role of women in the prehistoric world. For instance, conventional wisdom distinguished between man-the-hunter and woman-the-gatherer. Yet the work of female archaeologists brought to light that data supporting the prehistoric hunting and warfare of women had been consistently ignored. Graves of women with bows or swords as grave goods had been unearthed but not been recognized as indicating the existence of women bow hunters or women warriors. It became obvious that archaeologists had unwittingly invoked a family model prevalent in their own time for interpreting the excavation finds, namely, the breadwinning male and the housekeeping female. Such a recent example confirms the power of tacit value commitments within the context of justification.

Political Influences on the Research Agenda

A characteristic of epistemic research is its knowledge-driven mode of problem selection. The research agenda is set on the basis of previously solved problems and against the background of a theory or discipline. Problems are picked by theory-internal considerations and independently of practical concerns or aspirations. It is frequently assumed that any deviation from this mode of problem selection will degrade epistemic quality and impede scientific progress. For instance, Thomas Kuhn argued that addressing problems in the demand-driven way of engineers and medical scientists will slow down the growth of knowledge (Kuhn 1962, p. 164).

The claim that the mode of problem selection affects the epistemic quality of the outcome is not without support. A practical problem may be solved by drawing on a seemingly remote scientific principle or by combining knowledge elements in a novel way. This means that the theoretical resources apt for clearing up a practical difficulty are hard to identify beforehand. Rather, practical success may be made possible by findings that are *prima facie* unrelated to the problem at hand. If the solution to a problem requires forging new links or new insights, starting research from a practical perspective will be less than promising. Yet in many cases it is uncertain in advance whether the necessary knowledge is already available. Therefore, it is advisable to take the opposite direction and to proceed from the system of knowledge to the practical challenges that can be addressed on its basis. Accordingly, broad epistemic research, rather than narrowly focused investigations, is the royal road to bringing science successfully to bear on practical problems. In this view, demand-driven research is bound to come to grief; only knowledge-driven research can be expected to be successful in practical respect. As a matter of fact, this advice agrees precisely with the policy Vannevar Bush famously recommended for making science practically fruitful (Bush 1945, Chap. 2).

President Nixon's "war on cancer" represents an example of how demand-driven research can fail. This coordinated research program on fighting cancer in the 1970s set out to combat cancer by pursuing narrowly targeted, mission-oriented research projects on a large scale. Yet in spite of generous funding, no therapeutic progress was achieved. Instead, the program largely resulted in relabeling projects of fundamental research. This failure is attributed with hindsight to insufficient basic knowledge about the disease (Hohlfeld 1979). Yet incomplete knowledge of the fundamentals does not always thwart coordinated research endeavours. When the Human Genome Project was launched, the structure of the genome was not understood in depth and the relevant sequencing technologies were poorly developed. Technological revolutions were necessary for a successful completion of this ambitious endeavour, and these revolutions were anticipated and factored in when the project was conceived. This time the bold expectations were met. The puzzling result emerging from anecdotal evidence of this sort is that sometimes innovations can be stimulated and science can be pushed into a certain direction but that sometimes such attempts fail completely. Making science successful in transdisciplinary respect is a precarious endeavour (Carrier 2011a).

2.2.1.5 Values, Pluralism, and the Epistemic Attitude

The notion of a value-free science, as it is widely employed today, is stronger than Weber's and denotes the contention that the justification process in science ought to be kept free from non-epistemic values. Value-ladenness is only acceptable with respect to epistemic values.[2] Judged against the backdrop of the preceding considerations, the role of non-epistemic or socio-political values in science is ambivalent. Such values may exert a positive influence on the research agenda and even on the confirmation procedures, but they may also be detrimental to science. The trouble with values is that they are mostly partisan and non-universal and are thus feared to undermine the objectivity of science. Underlying is a distinction between facts and values, based on David Hume's insight that values do not follow from facts: "is" does not imply "ought" (Hume 1739/40, p. 469). Given this fact-value distinction the idea of restricting science to the realm of the factual expresses a commitment to *objectivity*. Objectivity in the sense relevant here means justified intersubjective agreement. All competent observers agree on ascribing certain properties to an object; the process of assessing claims is non-arbitrary and non-subjective (Longino 1990, 1993). Objective features are independent of our desires and concerns, and of what we appreciate or detest. By contrast, values are subjective and depend on our choice. They express subjective commitments and gain their binding force by agreement or convention. It is not implausible to regard the intrusion of values as a threat to scientific objectivity. Accordingly, it is a legitimate concern that non-epistemic values in the confirmation procedures undermine the credibility of science. On the one hand, including social values in the assessment of hypotheses is mandatory for a "responsible" science; on the other hand, a social bias of science tends to undercut the epistemic authority of science which derives

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[2] McMullin (1983), p. 23, Koertge (2000), p. 49, 53, Douglas (2010), p. 324. An even stronger notion of value-free science suggests that non-epistemic values neither are nor should be used in science, neither in selecting nor in judging hypotheses. This strong claim seems to be exclusively invoked today with a critical intent (Kitcher 2004, Kourany 2008).

from its factual basis. A science tied up too intimately with social values might lose the capacity of "speaking truth to power."

There are two possible ways of coping with the harmful impact of non-epistemic values without having to rule them out completely as part of the justification process—which seems hard to achieve and unrealistic. One way is admitting such values to the process of weighing the evidence; the other is bringing in such values as part of a pluralist setting.

As to the first item, Richard Rudner prominently argued that non-epistemic values rightly enter the confirmation procedures in science. His approach drew on the two premises that assessing hypotheses is essential to confirmation and that hypotheses are never entailed by any available evidence. Accepting or rejecting a hypothesis in light of data always incurs an "inductive risk": such decisions may produce false positives or false negatives. A high threshold level of acceptance reduces the risk of false positives but increases the hazard of false negatives, and vice versa. Rudner's suggestion is that weighing the non-epistemic consequences of these potential errors should decide about where to place the threshold of acceptance. This is said to imply that ethical values rightly affect theory-choice (Rudner 1953).

However, as Isaac Levi pointed out in the debate ensuing on Rudner's thesis, this argument fails to make research appear essentially pervaded by non-epistemic values (Levi 1960). First, accepting a hypothesis is not tantamount to acting on the basis of this hypothesis. The practical impact of research, to which Rudner's argument appeals, only emerges by the decision to take certain action by relying on the relevant beliefs. Yet in general, beliefs and actions are different kinds of things: the same set of beliefs can spawn different actions, and the same action can spring from different beliefs. The assumption that a vaccine is not safe can either lead to a stop of vaccination campaigns or to attempts to find an improved vaccine; conversely, the decision to continue with such a campaign may be based on the belief that the vaccine is safe or on the persuasion that the severity of the corresponding illness outweighs the risk of administering an unsafe vaccine. Second, many decisions about the acceptance or rejection of hypotheses do not have any specifiable practical import at all. Errors in identifying extrasolar planets or in classifying ancient architectural styles are unlikely to bring any non-epistemic consequences in their train. In such cases, hypotheses are assessed by appeal to their epistemic achievements only.

Still, the more general point is that the assessment of hypotheses requires balancing the risks of false positives and false negatives. Heather Douglas has emphasized that many factors in the design of a study affect its sensitivity for false positives or false negatives, respectively. It is not solely the choice of a threshold of acceptance that is influential on how suitable tests are for detecting mistakes of either kind; decisions about the procedures used for providing relevant materials or interpreting results affect the acceptance of hypotheses as well (Douglas 2000). As a result, Rudner's basic claim that finding the appropriate balance between false positives and false negatives demands the appeal to values has some force. However, only if the relevant research has a serious impact on the non-epistemic world or if the relevant hypotheses are taken as a basis for certain actions, the argument entails that ethical or non-epistemic values are rightly appealed to. Accordingly, in contrast to Rudner's own intentions, his argument fails to establish that non-epistemic values are in general a legitimate part of the confirmation process in epistemic science. Yet if research outcome is of practical relevance and is taken as a basis of actions, Rudner's argument about weighting the non-epistemic consequences of different types of errors supports the legitimacy of non-epistemic values in the context of justification (Carrier 2013).

Along these lines, Heather Douglas has introduced the distinction between a direct and an indirect role of values in science. In their direct role, values act as primary reason for adopting a hypothesis; in their indirect role, values are involved in judging whether the evidence is sufficient for accepting a hypothesis. The evidence is never sufficient for proving an assumption; an inductive gap always remains. Values used indirectly influence the assessment how serious such lingering uncertainties are; they help place the threshold of acceptance. Douglas argues that the indirect use of values is appropriate in science, regardless of whether these values are epistemic or non-epistemic. In this weaker role, values are confined to the realm left by the data; they are prohibited from competing with the data or from outweighing the evidence. In Douglas' view, restricting the influence of values to this indirect role recognizes the embeddedness of science in society, but at the same time protects the epistemic authority of science (Douglas 2009, Chap. 5; 2010).

An alternative to keeping non-epistemic values at bay is resorting to pluralism. Pluralism regarding epistemic values has been advanced frequently. Many competing sets of epistemic values have been suggested, and there is no way to either keeping such values out of science or to singling out a preferred set of values in a manner that is generally accepted as justified. Instead, the only way to curb the influence of specific values is counterbalancing them with diverging values. A notion of objectivity that is apt to take advantage of such a pluralist setting is centred on reciprocal criticism and mutual control. This social notion focuses on the social interaction between scientists who reciprocally censure their conflicting approaches. All scientists take some assumptions for granted. These beliefs look self-evident to them and are frequently not acknowledged as substantive principles in the first place. The trouble with such unnoticed or implicit assumptions is that they go unexamined. They are never subjected to critical scrutiny. This means that if one of these seemingly innocuous commitments should be mistaken, its falsity is hardly recognized.

Helen Longino has stressed that predicaments of this sort can be overcome by drawing on the critical force of scientific opponents. They will try hard to uncover unfounded principles and do their best to undermine one's favourite accounts. And if scientists proceed in a false direction, there is a good chance that their more fortunate adversaries will reveal the mistake. Such deficiencies are best uncovered by taking an alternative position. For this reason, pluralism is in the epistemic interest of science; it contributes to enhancing the reliability of scientific results. The pluralist approach to objectivity is essentially social. It thrives on correcting flaws by taking an opposing stance and thus demands the exchange of views and arguments among scientists (Longino 1990, 1993, 2002).

Within this pluralist framework, the objectivity of science needs to be separated from the objectivity of scientists. Individual scientists need not be neutral and disinterested. They may be eager to buttress or overthrow certain assumptions, and their motivation may be to promote their career, to strengthen some world-view or to devise some technological novelty. Such divergent values and goals need in no way undermine the objectivity of science. On the contrary, pursuing contrasting avenues is an important element of eventual epistemic success. In the pluralist understanding of objectivity, what matters is not to free scientists from all controversial ideas but rather to control judgments and interests by bringing in contrasting judgments and interests (Carrier 2008, 2013).

Pluralism in the adoption of theoretical principles and value-commitments is an important catalyst of scientific progress. I take the example of feminist archaeology (Sect. 2.2.1.4) to show that the elaboration of an alternative approach has improved science in epistemic respect. Women archaeologists have managed to uncover unsupported assumptions that had escaped notice; they have prompted new questions and suggested new lines of inquiry. The advancement of the feminist alternative has provided a deeper and more complete understanding of the archaeological evidence. Moreover, this epistemic benefit was not gained by dropping a one-sided approach and replacing it with a more neutral one. Rather, the alternative feminist approach involves a social model or political values as well. This time it is the role model of the working couple and of gender equality that guides theory development. We can make epistemic progress while continuing to bring value-commitments to bear. These considerations suggest that pluralism contributes to producing features of scientific research that can count as promoting objectivity.

Taking stock now, I distinguished between two accounts of how the appeal to non-epistemic and socio-political values might be made compatible with a commitment to the objectivity of science. The second approach, that I just discussed, centres on pluralism and a social notion of value-free science. According to this notion, competing individual value-commitments tend to cancel each other out through the process of reciprocal criticism. Science remains free from non-epistemic values at the level of the scientific community (in contrast to individuals). The first account is based on Rudner's argument. This account features the indirect role of values and emphasizes the connection between hypothesis acceptance and taking action. This latter account goes back to a proposal of Otto Neurath, according to which socio-political values legitimately come into play when action is important and the facts leave room for different accounts. Under such conditions, science is free to adopt a hypothesis that is best suited for acting (Cartwright et al. 1996; Howard 2009).

However, the concrete examples of such a strategy are not overly convincing. I mentioned Pasteur's anti-Darwinian attitude as a driving force behind his interpretation of his experiments on spontaneous generation (Sect. 2.2.1.4). Pasteur is construed as having interpreted these experiments in a biased way so as to support his conservative political stance. This was clearly action-oriented: he intended to rush to the aid of the Catholic Church that he saw endangered by Darwin. Yet if no new life could be created naturally, variation and selection could not get off the ground in the first place, and Christianity was beyond the reach of Darwin's impious assaults. So it seems it was perfectly "Neurathian" of Pasteur to take advantage of the room left by the data and to interpret them in such a way that they are suitable for social and political action (that is, keeping Darwin at a distance). But it looks dubious to many to regard Pasteur as a role model here. Rather, a move of this sort seems to undermine the intellectual authority of science and to do damage to its universal and objective character. It is true; Pasteur managed to eventually convince the scientific community by drawing on epistemic arguments alone. But if the epistemic situation had been more ambivalent, we would hardly have considered it legitimate to jump to the conclusion of ruling out spontaneous generation for political reasons.

Thus, it appears doubtful whether restricting values to an indirect role is apt to preserve the epistemic authority of science. Yet this sort of authority is imperative if science is expected to provide a common ground on which warring political factions can build. The challenge is to preserve the credibility of science and at the same time to strengthen its social relevance or responsibility. Science should be able to enter the socio-political arena without doing damage to its epistemic core. Pursuing a plurality of competing approaches that engage in epistemic arguments with each other seems to be a suitable way to proceed in this direction. As a result, science in general, not transdiciplinary science alone, is shot through with values. Value-ladenness does not, in general, undermine the epistemic authority of science, but it takes efforts to make the intrusion of value-commitment compatible with the objectivity of science. Chief among these efforts is to see to it that research proceeds in a pluralist fashion so that competing factions exchange arguments with each other and that the scientific community as a whole can be considered neutral or balanced in this respect.

2.2.2 Norms in Research Agenda Setting

Felix Thiele

2.2.2.1 Introduction

At all times (most) scientists have been conscious of the socially problematic consequences of their scientific work and frequently tried to take these into account. In addition, a great number of mostly only recently established advising committees refer to the request for expert-advice on social problems of modern science. Many think that the expanding possibilities for scientists to influence public debates on science and to shape political decision-making, especially research agenda setting, via policy-consulting are a welcome chance to strengthen the role of science in our societies.

But why should members of the scientific community engage in these debates in the first place? In Sect. 2.2.2.2 of this paper I will argue that the very existence of our highly differentiated science-system, mainly funded by public money, is due to needs that our societies try to fulfil through the sciences. Bluntly said: large parts of the scientific community work by order of society and have, therefore, certain responsibilities towards their sponsors. Note that with "by order of society" I here mean only that society expects science to fulfil certain needs society has. How detailed such orders can be, and whether they may set not only the goals of scientific endeavours, but also may have a direct or indirect impact on how science is actually done, will be discussed later in this paper. Using research with non-human animals as example, I will furthermore argue that it is not only the development of means for fulfilling social needs that the sciences should provide, but also the debate on what research aims should be pursued (Sect. 2.2.2.3). The latter involves normative questions bringing applied ethics to the fore. Applied ethics, so the argument goes, has a central role to play in the interdisciplinary process of determining the normative aspects of research agenda setting (Sect. 2.2.2.4). Finally, in Sects. 2.2.2.5 and 2.2.2.6, I discuss two objections claiming that the engagement of scientist's in public and political debates on research agenda setting bears the risk of watering down the high standards of scientific knowledge and argumentation.

2.2.2.2 The Social Utility of Science

In the early seventeenth century Francis Bacon suggested that the sciences should serve the goal of improving human welfare. The sciences should provide means for intervening into nature and for unchaining humans from natural and social constraints. In the pointed words of Bacon:

"But the greatest error of all the rest is the mistaking or misplacing of the last or furthest end of knowledge. For men have entered into a desire of learning and knowledge, sometimes upon a natural curiosity and inquisitive appetite; sometimes to entertain their minds with variety and delight; sometimes for ornament and reputation; and sometimes to enable them to victory of wit and contradiction; and most times for lucre and profession; and seldom sincerely to give a true account of their gift of reason, to the benefit and use of men." (Bacon 1857, I, p. 415).

That the sciences should serve the fulfilment of social aims is just one possible characterization. Other aims can be the satisfaction of human curiosity or the collection of pure, aimless knowledge. Who, for example, supports the exploration of outer space through manned space travel will have difficulties to plausibly explain how knowledge gathered on such missions will improve the human condition. Who, to give another example, changes the genome of sheep by using gene-technology in a way that these animals produce a bio-pharmaceutical product in their milk that could not be produced another way or at least not in the amount needed, will have less problems in demonstrating social utility. This article is not the right place to discuss this issue further.[3] It is obvious, however, that modern societies are largely shaped through the sciences, especially the natural- and engineering sciences and medicine. This is so, because these disciplines allow to successfully intervening into nature. Based on this observation one may well claim that the cost-intensive, in large parts publicly funded institution science owes it's very existence to the justified expectations in its social utility: why should society, for example, spend Billions for biomedical research and not for other parts of society, if she could not hope for practical benefits? These considerations refer to the natural- and engineering-sciences, to medicine, the life sciences, and the social sciences. The humanities likely will not fit into this framework.

There is no widely accepted canon of characteristics for the social utility of science. To the contrary we experience thoroughgoing controversies on what shall count as social utility and on how this utility shall be balanced with frequently existing risks. An example for such controversies is the debate on whether in face of obvious threats for humans and their environment it is justified to satisfy our still high energy demand by nuclear energy. Another example is the debate on green biotechnology and its application in agriculture in view of the still unclear risks of this technology. These and many similar conflicts are not only about scientific or technical issues, but frequently predominately about normative problems.

In a similar vein, and even more obvious the frequently heard claim that research must be conducted responsibly presupposes that the normative meaning of 'responsible' is clarified. Questions to be dealt with in this context are, for example: Opposite to whom should the research be conducted responsibly: only humans or other non-human animals too, or nature as a whole? Which normative principles should guide research conduct: justice, freedom, dignity, sustainability?

2.2.2.3 Example: Research Using Animals

The challenge for the scientific community's contribution to research agenda setting is most salient in cases where normative issues arise. The case of using animals for research for human purposes can serve as example: In biomedical research small rodents such as mice and rats, but increasingly also larger animals, e.g. pigs, sheep, and dogs, are for the time being indispensable. For a long time, this research that frequently comes with pain and death for the animals has been criticized (Sandøe and Christiansen 2008). Since roughly three decades it can be observed that public interventions in favour of animal protection are supported by ethical arguments (Singer 2006). Authors and activists like Peter Singer claim that humans have moral obligations opposite non-human animals, and that we, therefore, should radically change our behaviour towards them. Such arguments increasingly find their way

[3] For a detailed analysis of the application-orientation of much of modern science and the possible consequences for the quality of knowledge-production cf. Carrier, Sect. 2.1 in this volume.

into the legal and political realm. In Switzerland, for example, but also in Germany animal protection became part of the constitutional law.

The focus of most criticisms of animal research is on the moral status of animals and the level of protection that should be granted to them. Whether a rat or a dog is a moral subject, obviously is not a technical question but a normative one. Equally obviously, questions of this type have an impact on research agenda setting as can be illustrated by a judgment the Swiss Federal Court issued in 2009 on the use of rhesus macaques in basic research in neuro-informatics. The justification of the decision issued by the court contains amongst others the following moral assumptions:

> … Art. 74 BV [Swiss constitution] and the environment protection act … take into consideration the ranked order in the natural environment. … Even if it [the dignity of creatures] cannot and must not be equated with human dignity, it nevertheless demands that animate beings in nature are reflected and evaluated, at least in certain regards, on the same level as humans. … This proximity of the dignity of creatures and human dignity is especially clear in the case of non-human primates …[4]

This is not the place to evaluate the strength of the arguments given by the court. Though it would be worthwhile to discuss how the basically ancient and pre-evolutionary conception of a ranked order in nature (scala naturae) found its way into the deliberation of a twenty first century court procedure. Moreover, it would be interesting to understand what it means exactly that 1) the dignity of creatures cannot and must not ("kann und darf" nicht) be equalled ("gleichgesetzt") with human dignity, but that 2) this very dignity of creatures is the reason for (to a certain extent) equally reflecting and evaluating ("gleich reflektiert und gewertet") non-human beings and humans. The important point for my argumentation is that moral arguments (whatever their quality) played a key role in the court's decision against the use of rhesus macaques. Moral ideas mentioned by the court are: (i) the location of living beings in great chain of being as factor determining their moral status, (ii) the dignity of creatures that is, at least in the case of primates, is seen as similar to human dignity.

Now, research in primates may be due to the highly developed mental capacities of these animals deemed to be a special case in terms of the moral concerns it raises. However, even in the debate on the majority of research projects where animals with allegedly lower mental capacities, such as mice, are used, moral concepts—notably 'risk' and 'benefit'—are the controversially discussed hotspots: The higher the possible benefit of an animal-experiment is assumed, the more seems to speak in favour of this experiment. But about whose benefit are we talking: about the benefit for humans, or about the benefit for the test-animals too? In human research ethics

[4] Swiss Federal Court Judgement 2C_421/2008, 7 October 2009 (my translation) „… Art. 74 BV [Bundesverfassung] und das Umweltschutzgesetz… tragen der Rangordnung innerhalb der natürlichen Umwelt Rechnung…. Auch wenn sie [die Würde der Kreatur] nicht mit der Menschenwürde gleichgesetzt werden kann und darf, so verlangt jene doch, dass über Lebewesen der Natur, jedenfalls in gewisser Hinsicht, gleich reflektiert und gewertet wird wie über Menschen… Diese Nähe zwischen der Würde der Kreatur und der Menschenwürde zeigt sich besonders bei nicht-menschlichen Primaten….

there is a broad debate on the question under what conditions research with no direct benefit for the test-subject should be allowed. There, it is assumed for moral reasons that humans shall serve as test-subjects to a very limited extent only, if there is no direct benefit for them to be expected. While the need to protect humans is beyond question, there are widely differing positions when it comes to the justification and extent of protective measures for non-human animals. Moreover, it is also discussed how large the benefit should be, in order to justify animal experiments—is it enough, for example, that the research in question enlarges our general biological knowledge base?

What ‘utility’ actually means and whether non-human animals should be taken into consideration as possible beneficiaries or injured are but two of many open questions in this area. Equally controversial is the question of how to balance benefits and risks. Bluntly said: which benefit for humans and/or non-human animals justifies which damage (regularly only) for non-human animals? One easily could enlarge this list, but it already should have become clear that (1) the discussion of the pros and cons of animal-research basically is a moral discussion, and that (2) that in these matters we are confronted with a thoroughgoing moral pluralism generating a profound need for orientation.

My goal in this section was not to take sides in the debate on animal research. Instead I wanted to show that it’s justified to raise the above questions, and that, since these questions are moral questions, ethical problems are an integral part of research agenda setting.

2.2.2.4 Evaluating Normative Aspects of Research

In daily life we regularly claim the inter-individual validity of our moral statements. Nonetheless, critics claim that ethics cannot do more than help express the feelings and opinions of those concerned. In this perspective ethics is not more than a supplement, perhaps replacement for psychological care. Instead I hold the position that ethics is an argumentative method for convincing one’s opponent by giving reasons (Thiele 2004).

If a moral controversy involves more than just two interest groups, and if the relevant interests are manifold and complex—as is the case with debates of societal relevance—it may be advisable to reconstruct and clarify the debate and the argumentative standards governing it in a systematic approach. It has always been one of the main tasks of ethics to work out rules for moral discourse and to test if these rules are adequate for mastering moral conflicts. From this perspective ethics is clearly not the right instrument for providing final, irrevocable solutions to moral conflicts: the ethicist is not in the possession of “higher” insights and has no privileged access to absolute moral values or principles that would confer such competency to him/her. Rather, the specific role of the ethicist should be to counsel the concerned parties. He/she can advise the involved persons on suitable argumentation standards or give guidance for mastering conflicts. In the light of this, it becomes clear that the professional discourse between ethicists cannot replace societal procedures of decision-making. Nonetheless, it might be the case that society will benefit from the pool of suggestions and recommendations developed in professional debates and stored in the philosophical tradition (see also Kamp, Sect. 4.1. in this book).

Though the plurality of ethical approaches is striking, a closer look shows that there is a decisive exception to this plurality. The participants in the bioethical debate and other areas of applied ethics have one goal in common: i.e. the wish to master conflicts with the help of moral argumentation. This claim is an empirical observation that would need further support if it is to serve as a normative foundation for an ethical theory. However, for the purpose of this study it is sufficient to point out that all those participating in the debate of moral problems generated by the sciences subscribe to the view that ethics develops principles for the mastering of moral conflicts in an argumentative manner.

When it comes to the evaluation of normative aspects of research the methodology supplied by philosophical ethics is only one, albeit essential, component. In addition, substantial arguments about ethical problems need to be based on knowledge from the domain in which those problems originated. A discussion of the ethical problems of genetic counselling, for example, without the participation of physicians and others involved in this counselling would be futile. Similarly, it would be pointless to establish the impact that the results of a new technique, such as the genetic manipulation of food, will have on society without the participation of those social sciences that have the necessary empirical methods at their disposal. Finally, it would be vain to develop recommendations for societal regulations of animal research without the backing of research areas such as animal welfare sciences that should help linking the normative concepts of animal ethics with empirical findings (Engelhard et al. 2009). Applied ethics has to be embedded in an interdisciplinary co-operation if it shall serve as a tool for research-agenda setting (for a discussion of suitable institutional arrangements for this task see Lingner, Sect. 3.3 in this book). Instead of interpreting applied ethics as a philosophical sub-discipline embedded in an interdisciplinary co-operation with other sciences, it is sometimes said to be an interdisciplinary endeavour itself. Mostly a façon de parler this interpretation should not blur, however, that ethical reasoning is a distinguished part of evaluating challenges from the sciences.

Interdisciplinary co-operation aiming at the considered integration of normative challenges into research-agenda setting has been introduced as a argumentative, or rule based process. Frequently, however, it is argued that the (argumentative) quality of scientific, especially normative policy-consulting is seriously threatened if it takes into account public and political concerns and interests. Two objections of this sort are discussed in the following.

2.2.2.5 Does Scientific Policy Consulting Infringe Standards of Good Science?

In a recent work Peter Weingart (2001) describes what he calls the ‘politicalisation of science’ that in his view ultimately endangers the authority and credibility of science. Central to this process is policy consulting as the coupling element between the spheres of science and politics (see also Hahnekamp, Sect. 4.3.2 in this book). This process is powered by an ever-increasing interest in cooperation between science and politics in policy consulting. Reasons for this are amongst others:

i. to supply the scientific knowledge-basis necessary for decision-making on social problems caused by the sciences,
ii. to demonstrate the general public that political decisions are in harmony with scientific knowledge.

The first reason is uncontroversial, maybe trivial, once one adopts the view that policy-decisions concerning problems caused by scientific advance should be based on the knowledge of those scientific disciplines that either caused these problems or may supply the necessary means for mastering them.

So it clearly is desirable that both politicians and scientist have an interest in cooperating in these matters. Problematic, however, is the politician's alleged interest in demonstrating the general public that his decisions are in harmony with scientific knowledge. If 'demonstrate' does mean merely to 'give the impression of' being compatible with scientific results, the cooperation has mainly a marketing purpose that may be compatible with the scientist's aims but is surely not congruent with them. However that may be, it seems to be beneficial for both science and politics to engage in policy consulting, in order to establish a robust scientific basis for policy-making.

Now the argument to be examined assumes that the described coupling of science and politics results in an in principle infinitely increasing body of 'knowledge'. The problems for the credibility of science and, therefore, scientific policy consulting arise due to a difference in the quality of knowledge produced for purely scientific matters and knowledge produced for policy consulting. In this context Weingart differentiates between 'secure knowledge' and less well founded 'hypothetical knowledge'.

Clearly, in situations where politics demands advice on topics where only 'hypothetical knowledge' is available the co-operation becomes dangerous for science: either it delivers 'hypothetical knowledge' of dubious validity, or it admits that there is no conclusive advice to be given with regard to the current state of the art.

If science refuses to give advice at all it risks the affection and financial allowance of politics, if it gives dubious advice it risks its authority and credibility. (It is not exclusively politics though that constitutes a threat to the authority of science. Equally dangerous for the science's credibility is, if scientists turn into politicians and provide biased or hypothetical knowledge in order to support the political position they themselves champion.) If one assumes that most scientists will finally give way to the bittersweet temptations of power, then the authority and credibility of the scientific community at large will suffer heavily from this process and the science-system as it developed in the last several hundred years might disappear in the long run.

Plausible as this argument might seem on first sight some critical marks should be made. First, the differentiation between ‘secure’ knowledge and ‘hypothetical’ knowledge is misleading: all scientific knowledge is eventually hypothetical. What is meant probably is the difference between ‘knowledge’ and ‘opinion’. For the purpose of this essay knowledge can be interpreted to mean ‘justified belief’—in contrast to ‘true justified belief’. The line of divide between ‘knowledge’ and mere ‘opinion’ is, therefore, not that knowledge is somehow true. I confine myself to a much less presupposing view: Knowledge in contrast to opinion is connected to the pretension that there is some justification procedure that can be used in redeeming the knowledge claim.

In this terminology the crisis of policy consulting is caused by the increasing use of ‘opinion’ instead of ‘knowledge’. With this in mind it is worth looking again on the reasons why politicians draw on science in preparing their decisions: the main reason was, according to the discussed argument, to get hold of scientific knowledge for policy-making. If politicians actually are interested in knowledge there is no reason to assume an impending crisis of policy consulting. If, however, the main interest of politicians is rather to “somehow” demonstrate the general public that their decisions are in harmony with proper science, they likely will be content with mere opinion.

Let us assume for a moment what would have to be proven in the first place—that politicians are content with receiving advice that is based on opinion only. From an evaluative point of view it is totally clear that under these conditions scientists should stop participating in policy-consulting, since this participation does not satisfy the central aim of scientific work: acquiring and providing knowledge. The alleged win-win situation, where both scientists and politicians do benefit from cooperation, would not exist any longer.

By not differentiating between knowledge and opinion the description of the process of the ‘politicalisation of science’ seems to be more inevitable than it is. For sure one can assume that politicians and even some scientists do not understand or at least do not take into consideration the differentiation between ‘knowledge’ and ‘opinion’ in pursuing their aims. But the presumable correct observation that the credibility of scientific expertise is in some danger, does not by itself validate the claim that many or even the majority of scientists water down their (normative) concept of ‘knowledge’ and that, therefore, the concept of ‘science’ is changing its meaning.

The mistake made here is to neglect the difference between the description of the social practice ‘science’ and the normative criteria for valid scientific work adopted by those performing this practice. To claim, however, that scientists are not simply passive tiny cogwheels in the machinery of society necessitates to say something more on the double structure of the scientific community as a self-sufficient peer-group setting its own validity standards and as a social group dependent on certain stipulations imposed by the society on science (see also Carrier Sect. 2.2.1 in this book).

2.2.2.6 Are Public Concerns on Science are Frequently Ill Founded and Largely Irrelevant?

Over the past decade there has been a substantial debate on the shortcomings of expert discourse on societal implications of science and on the adequate participation of the general public in the formation of science policy including normative aspects of research agenda setting. In this section I will focus on a specific aspect of this debate: the claim, frequently made by scientists that public moral concerns about scientific advance are frequently ill founded and, therefore, largely irrelevant. (For a general discussion of public participation in interdisciplinary see Kaiser, Sect. 4.2 in this book.)

Beginning in the 1960s there has been an increasing interest of the general public in matters of science and technology. This likely was due amongst other things to an increasing awareness of environmental issues. Moreover, the growing ability of purposively intervening into up to then inaccessible areas of nature, e.g. the genome, contributed to that process. Initiated by these developments, political and scientific institutions started analysing the public opinion on science and technology.

The empirically working social sciences generated in the last decades a large body of knowledge on the ways members of the general public come to their evaluative attitudes towards science and technology.[5] In summary, one can say that for the great majority of the population, moral issues neither present themselves as abstract or isolated matters nor are they evaluated by reference to a single criterion or principle. At a social level, only single-issue groups or, at an individual level, people with strong ideological views take a position based on a unique evaluative angle and this only in cases of high salience issues involving their core values. For most other individuals, ethical questions arise in specific contexts composed or integrated by several overlapping domains, in which multiple and diverse values and ethical principles may apply, giving rise to a certain level of inconsistency or, at least, to a loosely coupled array of criteria for making up their mind and adopting a particular stance.

The complexity of explaining evaluative decision-making in the general public can be illustrated by the change in hypotheses underlying the explanation of public aversion against science and technology. In its early years the public understanding of science movement (PUoS) was mainly driven by the hypothesis that positive attitudes to science and technology are dependent proportionally on scientific literacy. By increasing the scientific literacy of the general public, so the argument went on, the level of support for science and technology in the general public should increase too. Unfortunately, this hypothesis proofed to be barely supported by empirical findings.

It turned out that in order to explain public perceptions in the area of science and technology one should include more general factors that are known to shape evaluative attitudes, and which tend to be more resistant to short-term change. These factors are commonly known as worldviews. The worldviews that people

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[5] This paragraph draws on Pardo (2012).

hold have an important role in how they assess the effects of scientific and technological developments on their lives. The term 'worldview' denotes fundamental value-dispositions that mirror an individual's general attitude towards science and technology:

> Worldviews are general social, cultural, and political attitudes that appear to have an influence over people's judgments about complex issues (Slovic 1999, p. 693).

Worldviews encompass beliefs on how the (natural and social) world is structured, how it is functioning, and also how individuals should interact with their world. Worldviews have been proven to be an important factor in forming individual's attitudes towards science and technology. Especially the fact that public attitudes on specific scientific techniques and applications are only partly dependent on the level of scientific literacy of the individuals polled was not fully understood until the concept of worldviews was introduced as a further parameter in explaining these attitudes.

Examples for 'worldviews' are such diverse attitudes as striving for 'justice' or 'naturalness'. Empirical studies have shown that especially 'naturalness' is an important idea that many individuals use in their assessment of scientific developments. This can have the effect that, for example, a certain biotechnological methods is rejected as 'unnatural' though the same method is deemed to be useful. The decisive point is that naturalness is taken as more fundamental value, which is not to be sacrificed in favour of other convictions. That worldviews represent fundamental moral convictions does not mean that they have to be accepted unchallenged. Especially conceptions of naturalness frequently lack a convincing argumentative basis. Worldviews, however, shouldn't be abandoned as simply irrational. If we understand what impact worldviews have in our moral landscape, and what role they have in individual assessments of scientific developments, it might become easier to transfer controversies on modern science into argumentative tracks, and to prevent them from becoming ideological quarrels.

2.2.2.7 Conclusion

The modern scientific system is based on society's hope for social utility of its research output and applications developed on this output. What 'social utility' amounts to and what the 'responsible' means of achieving this utility are is subject to an extensive normative debate.

The aim of this chapter was to argue that applied ethics is a suitable tool for the argumentative interpretation of normative terms such as 'social utility' and 'responsible scientific research'. Since the aim of applied ethics is to master moral conflicts and as this mastering includes avoiding moral conflicts, ethical reasoning has an important role in the early phases of research, including research agenda setting.

Ethical reasoning must be augmented, however, by input from other disciplines. Research agenda setting taking into account normative challenges is an important example of interdisciplinary co-operation in the sciences.

The presumable correct observation that the credibility of scientific expertise as part of political decision-making is in some danger, does not by itself validate the claim that many or even the majority of scientists water down their (normative) concept of 'knowledge' and that, therefore, the concept of 'science' is changing its meaning.

Worldviews, i.e. fundamental orientational values such as naturalness, have important impact on individual moral landscapes, and play a decisive role in individual assessments of scientific developments. A better understanding of the functioning of worldviews might make it easier to transfer controversies on modern science into argumentative tracks, and to prevent them from becoming ideological quarrels only.

2.3 Disciplinary—Interdisciplinary—Transdisciplinary: A Conceptual Analysis

Carl Friedrich Gethmann

Evidently, the terms 'interdisciplinary' and 'transdisciplinary' have a parasitic relationship to 'discipline'. Many publications start an attempt at the conceptual clarification of the terms 'interdisciplinary' and 'transdisciplinary' by trying to characterise the term 'discipline'. Most of the authors see coincidences in the disciplinary structure of the science cosmos, primarily the result of historically-contingent developments (cf. e.g. Stichweh 1984, 1994; Bora 2007, 2010), while others try to combine science-historical and science-philosophical methodological aspects (cf. e.g. Krüger 1987).

Apart from the questions of the scientific-historical recording and description, the attempt at a conceptual clarification also faces significant difficulties:

(i) the *de facto* use of the language of 'discipline', 'disciplinary', etc. is extremely diverse and the conceptual clarification therefore extremely difficult if one combines it with the task of describing the *de facto* use of language;
(ii) the systematic processing with the aim of a normative explanation of the term requires a strong epistemological and science-theoretical investment in highly controversial-specific areas. A normative explanation of the term should be understood as one that is oriented on the criteria of adequateness, coherence and consistency.

This already hints at the reasons that are critical so that one cannot discard the effort to achieve conceptual clarification fundamentally, independently of the sequence of the terminological introduction, if one wants to obtain a reliable orientation with respect to the different tasks that are connected with disciplinary, interdisciplinary and transdisciplinary recognition work.

2.3.1 Discipline

Many articles on the term 'discipline' are quick to assure the reader that the internal organisation of the sciences into disciplines is not due to a 'nature of the matter' and a system based upon that, but rather historical-social coincidences related to the self-organisation needs of scientists (cf. Stichweh 1984). The assumed disjunction requires clarification, however. On the one hand, the phrase the 'nature of the matter' suggests the image that disciplines reflect a 'structure of reality' that transcends recognition. On the other hand, the talk of 'coincidences' suggests the idea of extraneous arbitrariness. Both conceptions miss the point that scientific recognition work is aimed at a purpose and therefore guided by the attempt to organise purpose-related structures of knowledge formation, maintenance and dissemination. The central question in the philosophy of science with respect to the internal structure and the external boundaries of disciplines is therefore to concentrate on the question of purpose-related organisation of recognition (c.f. Gethmann 1990). Although this is primarily a systematic question, it is one that is focused on the social interaction of people engaged in science in a social-historical context. But a purely factual history of the development of disciplines would be insufficient if it did not address the question of purpose-related commitment.[6] For two reasons, however, a purely social-historical point of view with the assumption of systematic contingency seems trivial:

(i) The cognitive unity of the discipline has a strong normative power in terms of the qualification and award systems of science. It is only a seeming equivocation that the expression 'discipline' means a cognitively characterised subset of the cosmos of science and also the ability of an actor to self-control and self-determination. The ability to self-control in the sciences is seen particularly in the pursuit of a method. In addition to the subject, the interest-guided method[7] of recognising is that which characterises the cognitive unit of the discipline. It determines that physics is something different from chemistry, or historiography is something different from sociology.

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[6] In many cases, this unsystematic form of self-organisation is named as a reason for the fact that one must transcend these arbitrary borders of disciplines in an interdisciplinary and transdisciplinary way. This can be seen, for example, in Mittelstraß (1987).

[7] The expression 'interest-guided method' combines the *objectum formale quod* (regard, interest in the subject) with the *objectum formale quo* (procedure by which this is recognised) of the scholastic philosophy of science (based on Aristotle); *cf.* H. Schondorf, article: '*Gegenstand/ Objekt*'.—The transfer of the *objectum formale quo* to 'interest' is formulated by Kant in the term of 'reason interest' (e.g. *Kritik der reinen Vernunft*, A 804f B 832f), from which Husserl incorporated the connected ideas into his phenomenology (e.g. 1939 in *Erfahrung und Urteil,* Sect. 15–21). In Heidegger (1927), the idea is taken up in Sect. 18 of *Sein und Zeit* in the concept of 'involvement' (*Bewandtnis*). The relationship between 'recognition and interest' is a topos of the traditional philosophy of science that dates back to Aristotle. J. Habermas (*cf.* in the same work, *Erkenntnis und Interesse,* 1968) should have confronted it in the Bonn seminars of E. Rothacker on the basis of Husserl.

(ii) The different methods by which scientists control themselves and others in their recognition work and thereby (more presume than explicitly) specify whether or not someone belongs, for example, to their own 'discipline', give rise to the strong normative power from which the social processes of self-identification and identification by others follow. Based on just the historically-contingent drawing of borders, this alone would not be explainable. Therefore, social identification behaves like a parasite in relation to cognitive identification. Whether or not an individual scientist or a group of scientists is included in physics or chemistry, is not primarily the result of social or institutional characteristics, but rather of methods selected by them. Formulated differently: Scientists also do not leave the question off whether someone describes himself as a physicist, lawyer or philosopher to the arbitrariness of self-definition. A physicist, lawyer, philosopher is only someone who 'commands' certain procedures for obtaining and securing knowledge, i.e. has a certain 'discipline'. Whether or not he has this, is only recognised secondarily on diplomas and biographical information. In a borderline case, diplomas are even denied if it turns out that the expected expertise was only an appearance due to error, deceit or otherwise.

The sociologism and historicism that often prevail with respect to this subject are expressed, among others, in the well-known wordplay (which is no longer perceived as a joke by sociologists) that: physics is what physicists do. The 'joke' is precisely in the elliptical formulation. Physics is namely not what physicist do when they play golf, but physics is exactly what physicists do when they do physics. And that is now a circular definition *and* circular definitions are faulty, *because* they are semantically not informative. Furthermore, it should be critically noted that the question of whether the system of disciplines is due to socially-historically contingent or systematically necessary reasons contains a certain disjunctive use of the difference between 'cognitive' and 'social', which does not hold up in a more accurate reconstruction. Knowledge as a result of recognition is not, as Frege, Popper, and others assumed, an eternal treasure in a Third World that occasionally is perceived by the knowledge producers as residents of a Second World that addresses things in the First or lodges them in it. Rather, the formation of knowledge is a certain social process (of recognition according to criteria within the framework of the scientific communities), which must be described perhaps not with the means of empirical social research, but thoroughly pragmatically (as action context).

In this connection, especially the classification defended by many sociologists of the sciences, where there are 'internal' and 'external' control factors with respect to scientific disciplines, should be criticized (c.f. Gethmann 1981[8]). According to this interpretation, the internal standards based on the logic of science such as consistency, verifiability, fertility, etc. are observed, while the external standards based on the sociology of science such as innovativeness, utilisability, relevance, etc. are

[8] The following section is an editorial review of pages 26–28.

identified. This classification separates those norms that relate to propositional systems and those that relate to interaction connections, i.e. 'cognitive' versus 'social'. This disjunction is based on a platonic interpretation of the 'cognitive'. With respect to this, the interpretation of scientific argumentation as rule-based interactions is a 'pragmatising' of those cognitive dimensions which analytical philosophers of science address under the title of 'logic of science'. Arguments in this sense are presented not by formal logic in the sense of pure syntax and semantics, but rather by a pragmatic-normative theory of argumentation, i.e. a theory that prepares justified schemas for the pragmatic sequence of assertion, doubt, defence and finally consent. If scientists now act according to their argumentation rules, i.e. the specific rules for their scientific community, science is controlled 'internally'. However, the partially group-specific norms of scientists raise the general legitimation question that leads to the question of the universality of norms. If scientists act according to universal argumentation rules, science is then controlled 'externally'. One can see easily that the distinction between external and internal control ultimately does not represent an appropriate distinction for the classification of cognitive processes.

Institutional processes are ultimately to be assigned to certain norms. An abstract view of such institutional correlates allows for a type of quasi natural history of scientific institutions. These institutions are not, however, controlling factors, but rather products of controls, the legitimacy of which is tied to the underlying norms.

A consequence of this approach is that those general norms of scientific rigour, which sociologists of science view as specific for scientific communities, such as e.g. the principle of criticism or reasoning, are superordinate norms that extend beyond the scientific communities, while the norms, which the scientific institutions form, i.e. which the scientific research views as more of external origin, are to be viewed specifically for scientific communities.

In relation to the different rules for reasoning that are applied by scientists or scientific communities, it is necessary to ask the reasoning question. In this context, where the justification of the specific norms of the scientific communities is examined, i.e. their compatibility with universally applied norms, the question of the legitimation of the sciences arises. Dispensing of the legitimacy question leads—as can be seen in the example of Feyerabend's anarchism—to the dispensing of methodological thinking in general, to the dispensing of the difference between rationality and irrationality. Feyerabend has shown in impressive examples that there are no consistent norm systems of scientists in the history of science, that scientists have often not followed the set norms, and that the drawing of a border between science and non-science is arbitrary when viewed from a historical-descriptive point of view. It must be given to Feyerabend that in the history of the sciences things have unfolded *de facto* in a relatively anarchic way. But it should be countered that there are also reasons for avoiding such anarchism. The methodological anarchism is, however, only avoidable if rational discourse on the purposes of the scientific knowledge is not excluded by methodological restrictions.

2.3.2 Substantiating: Forms of Scientific Systematisation

The following is an attempt to lay the foundations for the semantic characterisation of 'scientific discipline' by reconstructing a formal-pragmatic understanding of scientific knowledge and then, building upon this, reconstructing the forms of scientific systematisation (term, assertion, generalisation, theory). This is done with a *reasoning-pragmatic approach* that implies a so-called 'epistemic' concept of the truth.

As a sketch of the definition for the explication of the concept of knowledge, the following is proposed here:

X knows that p: = For all Y: X can substantiate p with respect to Y

This approach deviates from the widely used definition, which characterises knowledge through a belief in p and the-case-being by p.

X knows that p: = BEL (p) and p.*[9]

This 'non-epistemic' approach is hurt by two hardly resolvable problems. For 'belief' is probably not semantically 'simpler' than 'knowledge' in any context; the definition is subject to the suspicion of *obscurum per obscurius*. It is also unclear how a performative or modally unembedded 'p' is to be understood. The commentary suggests that the modal operator is implicitly suspected of being a contingency.[10] Such an interpretation would, however, feed the problems of the semantics of modal logic such as the *de re/de dicto* problem into the explication of the concept of science, which would intensify the *obscurum per obscurius* problem in any case. In addition, a circle problem threatens because one will probably hardly be able to explain the modal operators without direct or indirect recourse to 'knowledge'. Finally, there is the problem that a modal or performatively unembedded 'p' may not formally be a sentence radical that could 'offer' itself to any embedding, but slightly suggests a pre-supposed epistemological realism in the commenting language. However, it would be a breach of the pragmatic principles of definition to decide a position in a large philosophical debate (realism vs. anti-realism), backhandedly, so to say, through word use rules.

On the basis of the reasoning-pragmatic approach, it is possible to define:

X recognises p: = Y is in the process of acquiring/producing knowledge
X thinks that p: = p is 'candidate' of knowledge that p
Y doubts that p: = Y requires X to substantiate p

The common use of the term knowledge makes the impression, in connection with a widespread vulgar Cartesianism, that knowledge is a private inner process that is occasionally 'expressed' by its 'owner' (Gethmann/Sander 2002). In respect to this, the sketch of the definition above uses the term resultatively, i.e. as a result of a social process, namely reasoning. Reasoning is a rule-based sequence of discourse

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[9] E.g. von Kutschera (1976) and Lenzen (1980); for criticism of the discourse action theory perspective, see Stelzner (1984).

[10] See the discussion in Lenzen (1980) in connection with Gettier's objections *loc. cit.*

actions that begins with a constative performative mode for which the 'assertion' is used here as an example.[11] A rule-guided sequence of discourse actions means 'discourse'.[12] Discourses can be described and explained empirically like all linguistic phenomena and thus fall within the scope of empirical linguistics. In order to reconstruct the rules of the correct discursive process interlingually, one must rely on the instruments of a formal pragmatics in discourse action and discourse action sequences. For the most important philosophical forms of discourse, the discourse of substantiation and the discourse of justification, the most important terminological specifications are listed in the following overview (Table 2.1).

On this basis, a five-place reconstruction of the predicator of substantiating is appropriate for the most interesting reconstruction contexts:

Subst (P, O, p, K, R)

P(roponent) substantiates with respect to O(pponent) the assertion of p with the support of K by transition rule R. The terms 'proponent' and 'opponent' describe social roles that can also be assumed by individuals, collectives, and—in borderline cases—also by one individual.

A functional schematisation results if one takes down the typical actions of proponents and opponents in columns (Table 2.2):

The discourse model sketched in this way is the basic model from which other 'deficient' *discourse models* can be formed, depending on the limitations in the competency of the person in the role (Table 2.3):

For further conceptual reconstruction, it is also important to differentiate between 'pre-discursive consent', which must be present ad hoc or in principle so that discourse with the prospect of success can be had on its basis, and 'discursive consensus', which is achieved by discourse.

In the first approach, a discipline should be understood as the ensemble of reasoning rules (usually acquired through socialisation) and the instruments necessary for reasoning discourse. Socialisation means that the actors know the rules, but often do not know how to make it explicit. Such rules have a similar status to the grammatical rules of the first acquisition language. The philosophy of science includes the explicit grammar of such disciplinary rules (see Sect. 4.3.3).

Specifically, it is necessary to ask which linguistic explication level is used to localise these rules *primarily*. A differentiation can be made between:

- ***Sentential level***: Reasoning: substantiation/justification rules, pre-discursive consent.

[11] Other examples would be proposals, predictions, conjectures, reports, findings, etc.

[12] Medieval Latin. *dis-currere means to pass through something step by step. Discourse here describes roughly what was called 'dialogue' in the Erlangen school on account of an erroneous etymology; δια λογον means by *locutionem* and by no means a discourse by two linguistic actors [δια ≠ δυο]). With regard to the verbal problems, see C. F. Gethmann/Th. Sander, 'Rechtfertigungsdiskurse' (1999). On the philosophy of language basics, see Th. Sander, *Redehandlungssequenzen.* Discursivity is, therefore, also no specific characterisation of 'discourse ethics'.

Table 2.1 Terminological specifications of discourse

class of discourse actions	constative	regulative
statement	descriptive	prescriptive
atomic statement	assertion	demand
molecular (e.g.)	doubt (regulative) consent denial	
sequence of discourse actions (rule guided)	constative discourse	regulative discourse
discourse in the case of continuing doubt (failure)	dissent	conflict
…, in the case of approval (success)	(constative) consensus	(regulative) consensus
status of initial statement in the case of factual success	(relatively) substantiated	(relatively) justified
… in the case of the situation-invariant success…	(absolutely) substantiated: = true	(absolutely) justified: = right
argument: = aircourse action sequence scheme, which should constantly lead from… premises to… conclusions (allegedly, supposedly)	true	right
	sound	
(actually), i.e. in meeting the validity criteria	valid	
incompatibilities	propositional: contradiction pre-suppositional: incoherence	
objector to argumentation	sceptics	fanatics

Table 2.2 Functional scheme of proponents' and opponents' actions

O	P
	$\vdash H$
? H	$K \Rightarrow_R \vdash H$
? K ? R	$\cap$
$\cap$ ($\Vdash H$)	

Legend of Performators: $\vdash$ assertioning; $\Vdash$ agreeing (in the strongest sense of the accepting of an assertion); ? doubting (in the cognitive sense of demanding an substantiation or justification); $\cap$ other rule-guided actions

Table 2.3 Basic and "deficient" discourse models

Basic model:			
	Proponent	–	Opponent
'Deficient' models:			
	⇓		⇓
	Speaker	–	Listener
	⇓		⇓
	Informant	–	Recipient
	⇓		⇓
	Transmitter	–	Receiver

- ***Sub-sentential level***: Concepts, terminologies.
- ***Super-sentential level***: Theories (e.g. theory of gravitation); macro theories (e.g. theory of evolution); subsumtions (of the type 'physics is a natural science').

On the basis of the preceding explanations, the following sketch of a definition can be provided:

Δ is a discipline:=
Δ is 5-tuple consisting of

- *{reasoning rules}*
- *{pre-discursive consent}*
- *{system of concepts = terminology}*
- *{theories}*
- *{super-theoretical subsumtions}*

It is important to note that on account of the pragmatic introduction of the reasoning concept, this characterisation is both a cognitive (resultatively it involves knowledge) and a social characterisation (it involves the interaction of actors), or better that the distinction is not separated and therefore included.

The specified definition framework also allows us to rationally reconstruct the change in disciplines. Within the five parameters of the definition, more or less extensive changes are possible while keeping the other parameters constant. In this way, it is possible to historically reconstruct what changes in one discipline, while on the other hand it is also possible to reconstruct the fact that there is still the discipline that changes. The continuity of the discipline in change corresponds to Wittgenstein's metaphor of the identity of a rope and the variety of the fibres (Wittgenstein 1958, p. 87, 1963, p. 66 ff.; 1984, p. 74). This also means that it is somewhat arbitrary to see a new discipline emerge with the sufficiently extensive changes in the parameters that determine a discipline. In this respect, the provided definition also contains the concerns of those who in the genesis of disciplines see solely historical-contingent factors at work. On the other hand, the continuity through historical change is explainable without platonistic assumptions.

2.3.3 Interdisciplinarity

2.3.3.1 Meaning and Its Interpretation in the Context of Philosophy of Science

The term 'interdisciplinarity'—building on the semantic characterisation of 'discipline' proposed above—is a collective average between disciplines with respect to at least one parameter related to the sets of:

- {Reasoning rules} or
- {Pre-discursive consent} or
- {Terms = Terminology} or
- {Theories} or
- {Super-theoretical subsumtions}

Scientific disciplines have arisen from lifeworld problems without exception through history and reacted to this more or less adequately. By ex-post systematisation, an epistemic unit was constructed more than found in the course of the development of the disciplines. This fact provides the approach for a social-historical analysis of the history of disciplines, which, as it turns out, however, have a parasitic relationship to epistemic identification by oneself and by third parties. In that regard, a far-reaching a priori 'fit' between lifeworld types of problems and disciplines is a little surprising at first. It is precisely for this reason, however, that the defects of the fit are conceivable. It can happen that:

- a lifeworld problem does not find any scientific answer in the context of the disciplinary lists of questions;
- in extreme cases, it is provable that no answer can be given within the framework of the traditional canon;

- it cannot be ruled out that a new problem has not yet found any clear disciplinary classification and that multiple sciences react to one problem.

In cases of the last type, it is seen that an interdisciplinary jurisdiction can be the adequate scientific reaction. In principle, such a constellation of problems is not new and the interdisciplinary handling of a problem is not a phenomenon of the most recent history of science (cf. Stichweh 1984). For example, biochemistry has developed since the beginning of the nineteenth century from biology, chemistry and medical physiology, and from the beginning was closely linked to genetics and cell biology. The reason for the interdisciplinary connection is clear: the phenotypic language of macro-biology (zoology and botany) was no longer sufficient for 'understanding', i.e. the development of reasoning processes, the choice of terminology, formation of theories, etc. with regard to the cell as a 'chemical factory'. The discipline that addressed substrates of life (living organisms and their parts) began to use the language of organic chemistry. Such interdisciplinary averaging constantly takes place, for example, through the inclusion of mathematical methods in the empirical sciences,[13] chemical methods in historiography (e.g. in the case of the study of seals), or chemical processes in clinical disciplines.

2.3.3.2 Weak Versus Strong Interdisciplinarity

Some forms of interdisciplinarity in relation to the cognitive task are, so to say, 'not worth talking about' due to the relationships between disciplines. These are ones where on account of the crude identity of the object or subject of research and the proximity to the interests of recognition, which are reflected in pre-discursive consent, terminology, etc. intellectual cooperation is obvious. One thinks of the cooperation between archaeologists and material scientists with respect to a find, or between historians and literary scientists with respect to a source, the cooperation between mechanical engineers and electrical engineers with respect to a large device or the cooperation between clinical disciplines and laboratory chemistry with respect to an ill patient. In such cases, one should speak of 'weak' interdisciplinarity. It is by no means uninteresting from a philosophy of science perspective, but it is not necessary to reflect further on the pragmatic and science-policy relationship to 'transdisciplinary' issues.[14]

The discussion of weak and strong interdisciplinarity requires a philosophy of science basis of its measure. In the case of weak interdisciplinarity, the pre-discursive identity of the object or subject is beyond dispute, as is the cognitive interest in the subject (e.g. understanding, explaining, predicting, etc.). In the case of strong interdisciplinarity, it is doubtful, however, whether there is pre-discursive consent

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[13] Which are thus due to purely pragmatic needs and not mysterious pythagoreanism of nature.

[14] See Sect. 2.3.4.

with regard to the subject. There are no a priori criteria for the fact that two cognitive acts have the 'same' subject as their intentional object. This applies not only between 'distant' cognitive modes such as seeing, remembering, telling, reporting, fantasizing, etc. In this respect, unreflected discourse on 'the' object of knowledge is to be commented on with mistrust as long as no clear identity criteria have been named. In addition, the cognitive interest and primarily the processes of examination derived from this are polar opposites and may even be contradictory or have a disparate relationship to each other. One may think of a toxicologist's investigation of a correlation with respect to a threshold and a lawyer's establishment of a limiting value, a therapist or health economist's determination of treatment therapy or similar things. For transdisciplinary issues, the strong interdisciplinarity may not be required logically, but is probably typical and in any case problem-intensifying.

2.3.3.3 Classifications of Disciplines

In the policy rhetoric in connection with interdisciplinarity, strong interdisciplinarity is frequently mentioned as the interdisciplinarity between the natural sciences and the humanities. This shows that discourse—however it is organized—on the relationship between disciplines makes use of a science classification pre-suppositionally (but usually in an unreflected way). A classification of disciplines is a super-sentential arrangement of the disciplines, such as the above-mentioned division of all disciplines or of a subclass[15] of disciplines into natural sciences and the humanities. But it is precisely that which raises considerable philosophical problems of adequateness.

Anyone who differentiates between the natural sciences and the humanities seems to implicitly assume the validity of the Cartesian dualism, namely the division of all created substances into those of >*res cogitans*< or >*res extensa*<. When there are fundamentally two sorts of objects, there are also fundamentally two sorts of sciences. This dualism is not defensible for several reasons. The first objection is that it does not take into account the Hegelian discovery of the >objective mind<. Hegel discovered the existence of phenomena that are just as predetermined for the individual actor as natural phenomena which are, however, made by man (cf. Hegel 1991, Section 385). Language can serve as an example that is predetermined for the individual in the sense that he will be socialised in an existing language. Language is predetermined for the individual like a natural phenomenon, but it is a product of human activity. Another phenomenon that is predetermined for the individual can be found in law, yet laws do not exist in nature, but rather result from human actions. The mind (*Geist*) encounters us in

[15] While the unreflected *on-dit* science policy rhetoric assumes a classification of the entire cosmos of disciplines as a *totum dividendum*, their inventor, W. Dilthey, only refers to the disciplines of the Department of Philosophy at that time. Dilthey basically does not address the higher departments (with the exception of the history of law).

such cases in an objective form, i.e. virtually naturally, but is man-made. Accordingly, it can be seen that the disjunction between >*res cogitans*< and >*res extensa*< fails because there is a third factor. Dilthey, unlike Hegel, did not want to understand the objective mind (*Geist*) as the subject of philosophy, but rather conceived of an independent type of empirical science that addresses language, history and literature, namely the so-called >humanities<. Therefore the mind (*Geist*) of the humanities (*Geisteswissenschaften*) is the objective mind (*Geist*) in Hegel. Consequently, according to Dilthey, it is necessary to differentiate between three types of empirical sciences: Natural sciences, psychology as a science of the subjective mind, and humanities as sciences of the objective mind. This is often overlooked in the distinction between the natural sciences and the humanities. Hardly anyone seriously defends Cartesian dualism for this reason. In addition, the distinction cannot be complete in relation to the cosmos of disciplines. Economics falls neither within the humanities (the formulation as ‘humanities and social sciences’ is at best an expression of a classification quandary[16]), nor is mathematics a natural science because it does not deal with natural objects, but rather with linguistic constructs. Where should sports pedagogics, architecture, forensic medicine, clinical psychology, church law be categorised? So, one must obviously differentiate more strongly.

A classification proposal (Gethmann 2010)[17] suited for many purposes is to differentiate between10 kinds of science. The first disjunction is between a priori and a posteriori sciences. A priori sciences[18] are (**1**) philosophy and (**2**) mathematics. The subjects of nature, society and mind (*Geist*) can be differentiated a posteriori. In relation to ‘nature’ (in different meanings), there are (**3**) natural sciences (physics, chemistry) (**4**) life sciences (bio-sciences and medical disciplines) and (**5**) engineering sciences. In relation to society, there are (**6**) behavioural sciences (psychology, sociology, political science), (**7**) jurisprudence and (**8**) economics, each of which cannot be reduced to each other. Within the objective mind (*Geist*), history and language can be differentiated involuntarily, as a result of which there are (**9**) the historical sciences and (**10**) the philologies.[19]

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[16] It is a consequence of the fact that one frequently means all the non-natural sciences when one speaks of the ‘humanities’ or the ‘humanities and social sciences’. This assumption is also usually made by those who use ‘science’ in its English sense (in order to concede the importance of opera, ballet, Dokumenta 13 and the humanities according to the two cultures dictum in a culturally generous way).

[17] The medical disciplines are missing.

[18] The concept of a priori knowledge does not assume, as it does in Kant’s use of the term, the universality and necessity of this knowledge, but rather solely the pre-suppositional function of certain knowledge content relative to material knowledge contexts.

[19] The German Association of University Professors and Lecturers (Deutscher Hochschulverband) differentiates between 74 subject areas for approximately 6,000 subjects, so that the differentiation between 10 kinds of subjects is a pragmatic moderate reduction of complexity.

The continuation of the previously proposed characterisations should be viewed as 'strong' interdisciplinarity if the interdisciplinarity between at least two of these 10 kinds of subjects is pursued.[20]

2.3.4 Transdisciplinarity

The progressive differentiation of scientific disciplines in the sciences through the modern era is primarily due to the 'inner' (cognitive) needs of knowledge and less to the 'outer' requirements for the application of knowledge. It is a more recent phenomenon that science is 'applicable' in a technical or political sense, whereby the society-related disciplines such as education, economics and jurisprudence and the engineering sciences (contrary to a widespread prejudice[21]) have moved far ahead of the natural sciences. The first natural science that successfully reacted to a purpose dictated from outside (increase in agricultural production to fight hunger) was agricultural chemistry (Krohn/Schäfer 1978). Since the middle of the nineteenth century, society has increasingly expected scientific solutions in the area of technical issues (such as transport and the supply of energy), the health of humans and animals, the education of children, the prudent use of the environment, development of the population and, in particular, the waging of war. In addition to engineering disciplines, the sub-disciplines of chemistry, biology and medicine are also under pressure to demonstrate a societal purpose. Besides the cognitive internal purposes in the development of science, there are transdisciplinary purposes.

Transdisciplinary purposes of this kind logically do not force interdisciplinary cooperation in the weak or even strong sense. Monodisciplinary transdisciplinarity is also easily possible. However, large engineering projects, especially in the military field, have virtually required interdisciplinary cooperation since around the 1930s ('big science') (De Solla Price 1963). This resulted in the development of system-technical approaches for the integration of various disciplines, especially at large non-university research institutes. The development of the atomic bomb in Los Alamos as well as the moon landing by NASA were the successful model of transdisciplinarity (for which, it should be noted, not the large number of integrated individuals, but rather the structural combination of disciplines for a non-scientific purpose is characteristic). In Germany, nuclear energy initially played a leading role, which was supposed to solve not only energy problems, but also issues related to ship drives and other technical questions, including nuclear medicine. Both use

[20] 'Scientists' in the English notion are usually conceived as representatives of the above mentioned disciplinary classes 2, 3, 4, 5 and sometimes also of class 6. However, the meaning of "scientist", "scientific" etc. in this article is broader, encompassing all ten types of the above mentioned disciplines. The application of the term "discipline" instead of "science" might thus avoid any misconceptions here.

[21] A variant of the 'scientism', according to which solely the 'sciences' in the English sense of the word are to be taken seriously with respect to application.

for nuclear energy (nuclear physics, mechanical engineering) as well as nuclear medicine (radiation physics, radiation biology, tumour medicine) forced strong interdisciplinarity under the pressure of transdisciplinary expectations. Society-related issues as addressed by jurisprudence, economics, and ultimately also ethics were recognised as relevant later. Transdisciplinarity today consists primarily of the interdisciplinary bundling of different natural science and engineering disciplines as well as medical disciplines, which now include economics (for efficiency considerations), jurisprudence (for the examination of the necessary regulations *de lege lata et ferenda*) and ethics (for the clarification of the conflict-resolving compatibility of instruments for purposeful realisation with other normative orientations such as particular religious beliefs or universal human rights). Parallel to the formation of the word ‘strong interdisciplinarity’, it is possible to speak of *‘big transdisciplinarity’* in the case of such far-repeating inclusion of disciplines (without assuming a fixed catalogue). Cases of the ‘agricultural chemistry’ type can be mentioned as ‘minor transdisciplinarity’ (without a clear distinction).

Occasionally, there are calls to dissolve the disciplines and transfer all research to an interdisciplinary under transdisciplinary setting of the purpose (e.g. v. Weizsäcker 1969). This call fails to recognise that interdisciplinary cooperation is based on the cognitive performances of the disciplines and that transdisciplinary expectations can be met reliably if the disciplines render their cognitive performance. However, it is important to take into account the fact that the transdisciplinary issues are those that the public is particularly interested in. Cases of fraud in transplantation medicine attract more attention than those in elementary particle physics. However, it would be an optical illusion if this phenomenon of public perception clouded the fact that the majority of problems that scientists are concerned with are disciplinary problems for good reasons. The disciplines are the cognitive pillars on which the interdisciplinary cooperation of scientific disciplines is based for purpose-related projects on time.

While the approaches of radical interdisciplinarity try to overcome the disciplinary status of the sciences, the discussion on the so-called mode II assumes a coexistence between traditional scientific disciplines (mode I) and those forms of knowledge that depart from the concept of a disciplinary matrix under the pressure of societal demands, phenomena of mass communication and other challenges (mode II) (Gibbons et al. 1994, Nowotny et al. 2003). The relationship between these two forms of science remains unclear, however.

2.3.5 Transdisciplinarity as Interaction Competency

In many cases, the word ‘interdisciplinarity’ is associated with an individual scientist’s diversified competency. We think of great scientists in the modern age of science such as G. W. Leibniz, H. von Helmholtz, B. Russell or C. F. von Weizsäcker. In the case of the weak interdisciplinarity, it is thoroughly conceivable that there is diversified competency in this sense. In the case of strong interdisciplinarity, however, doubts can be raised as to whether there can still[22] be intellectual individual competency called 'interdisciplinarity' in view of the intellectual complexity of scientific disciplines and the associated training requirements. If it is assumed that the individual competency of a scientist in his discipline can only be acquired through socialisation in this discipline so that this socialisation is not primarily the acquisition of *knowledge*, but rather largely the acquisition of *ability*, and ultimately that the competency rules mostly remain practical rather than implicit and usually function in a solely pre-suppositional way, then one must categorise the case of real diversified competency as unlikely and consequently fairly rare. If one includes the virtue requirements required in transdisciplinary connections (see Sect. 3.2), then it becomes clear that interdisciplinary research in the transdisciplinary direction is initially and usually *interaction* competency. It requires from the individual scientist the willingness and ability to use and withdraw its disciplinary perspective in light of other valid perspectives and to focus on transdisciplinary purposes in collective work. By including obvious pragmatic assumptions (such as the shortage of available time and mental energy), this results in the fact that transdisciplinarity is manifested in interdisciplinary (in the sense of strong interdisciplinarity) work groups focused on time.

This discovery raises the following question of the plan of which the working group expertise consists and how it will be recognised, if need be. Here, some of the criteria are at hand. Anyone who wants to work successfully in an interdisciplinary work group with a transdisciplinary focus should:

(i) be recognised in his field; since the representatives of other disciplines are naturally uncertain whether they can rely on the professional testimony of colleagues *(principle of trust)*;
(ii) demonstrate relevant research in the area of the subject (which can be roughly determined by anyone, notwithstanding the above-mentioned problems of identity criteria) (*principle of relevance)*;
(iii) be closely affiliated with the 'prevailing doctrine' (heterodox positions are to be tolerated in the disciplines to a certain extent, but are not suitable for interdisciplinary interaction) (*'no extremists'—principle of moderation)*;
(iv) be prepared to see the bigger picture and hold the view that other disciplines also have something to say *(the principle of modesty)*;
(v) recognise the implicit pre-suppositions of one's own discipline and also question them *(principle of self-critique*).

With the explication of the concept of strong interdisciplinarity and the listing of types of disciplines, the scope of the subjects taken into account may be outlined, but the question remains which subjects are primarily involved in the

[22] 'Still', because one cannot argue with Leibniz that he was as good a lawyer as he was a mathematician.

interdisciplinary work with a transdisciplinary orientation. Initially, it is possible to differentiate between poietical and practical disciplines[23]:

(a) Poietical Disciplines
Poietical disciplines are ones whose researches consist of scientific subjects that prompt a need for societal discussion. They refer to this need from the point of view of discovering technical operations or intervening in them. The focus is on the possibilities for action opened up by the sciences in the areas of energy, transport, environment, health, etc. For this reason, it requires no laborious discussion to see that the natural and engineering sciences as well as the medical disciplines are being addressed. The selection of scientists capable of working in groups is in principle, according to the above criteria, not so difficult. It is more difficult to convince proven experts that it is useful to interrupt or put aside normal production in order to dedicate oneself to interdisciplinary research in a transdisciplinary direction. Moreover, in most disciplines in view of the internal qualification and reward structures in a field, it should be obvious that collaboration in interdisciplinary project groups with a transdisciplinary focus is not suited for scientists in the qualification process. Ultimately, such tasks also cannot be delegated to younger scientists. In interdisciplinary working groups with a transdisciplinary orientation, 'the heads work personally' (J. Mittelstraß).

(b) Practical Disciplines
These are disciplines where the results of research can be expected to make a contribution to the solution of the suggested problems. The solution to the problems is usually a change or the invention of regulations on different rule-setting levels so that *jurisprudence* is indispensable here. Furthermore, as a rule, there is serious intervention into economic processes (development of new products, modification of market structures, necessity of state intervention, etc.) so that the *economy* is addressed. Ultimately, in almost all cases, non-trivial questions of normative orientations are up for debate, independently of the legal territories, requiring the reflection competency of *ethics* as a sub-discipline of philosophy (and no other discipline) for the overcoming of societal conflicts. In contrast, the classical *humanities* (historiographies and philologies), as mentioned in Sect. 2.3.3.3 have played effectively no role hitherto; an intellectual role in this context is difficult to conceive of against the background of the paradigmatic self-understanding of these disciplines.

[23] The distinction poietical/practical follows the Aristotelian distinction between the modes of action in producing (ποιησις) and interpersonal action (πραξις). Its philosophy of science meaning, based on the Bacon principle, is that the sciences are generally to serve humanitarian purposes such as the avoidance of natural (through poietic knowledge) and social (through practical knowledge) constraints.

2.3.6 Unsuitability for Interdisciplinarity

The determination of the conditions for strong interdisciplinarity and thus interdisciplinary research with a transdisciplinary focus is, by way of contraposition, a result of the specification of the conditions for interdisciplinary research. However, there are general attitudes among scientists with respect to scientific recognition work which make their collaboration in interdisciplinary contexts with a transdisciplinary orientation a priori impossible. In this connection, mention should be made in particular of fundamental dissolutions of scientific validity claims and a basically normative scepticism.

(a) Destruction of Validity Claims
In principle, this form of unsuitability consists of the fact that scientists deny the scientific validity claims of other, in borderline cases, of all disciplines on the basis of the competency in their discipline. Very often, this view of scientific work is connected with the immunisation of the scientist's own validity claims, because the corresponding attacks on the validity claims of other disciplines—otherwise with a penalty of performative self-contradiction—cannot be maintained. Such questioning of scientific validity claims is fundamentally conceivable from the perspective of all disciplines. That is why it is sufficient to illustrate these variations of unsuitability for interdisciplinary work in some examples:

- A neuroscientist, who is considering the presentation of scientific discoveries in other disciplines as a determined result of electrochemical processes in the brain, interprets validity claims and accordingly the discourse related to them and their application as determined natural processes. It is obvious that this makes a cognitive exchange between disciplines impossible.
- A theologian, who interprets counterarguments to his interpretation from the perspective of other disciplines as an expression of reprehensible disbelief, explains the validity claims of other disciplines and discourses related to them and their application as not to be taken seriously by him from the beginning.
- A sociologist, who interprets the claims and application of other disciplines as a contingent social interaction phenomenon, relativizes scientific validity claims to essentially social relationships of exchange, without the specificity of the interaction relationships in scientific communities, namely the effort to check, dispose, confirm or take seriously the validity claims.
- A psychiatrist, who interprets validity claims as an expression of typically masculine virility, denies the intellectual authenticity of discourses related to scientific claims and their application. Anyone who wants to be involved in an interdisciplinary working group with a transdisciplinary orientation in order to expose the motives of the members of other disciplines violates the aforementioned entry requirements and is thus not suited for interdisciplinary work.
- An economist, who interprets the discourse related to the scientific claims and their application solely as a monetary waste of time and pleads for their avoidance, does not get the game of scientific claims and their application.

An important variation on this form of unsuitability for interdisciplinarity is in the approach taken by some social and cultural sciences of observing the scientific validity claims as a particular expression of the 'tribe of scientists'. This *tribalisation of the sciences* is seen in the fact that scientists are considered as a social group alongside others without taking into account that the internal defining characteristic of the sciences is the self-commitment to rationality standards that allow one to say under certain (of course: not trivial) conditions that a claim is 'true' and a demand 'right' (Gethmann 2001).

In principle, it can be seen in the provided examples that the recognition relationships to be encouraged between the disciplines in the interest of interdisciplinary research must fundamentally include that the cognitive negotiations of scientists are attempts at purposeful realisation and not pure natural processes (naturalism). The naturalistic understanding of action consists in general of the fact that actions are to be observed as the effects of (natural) causes. Depending on the type of the causes, there are variations of naturalism.[24] The error of the naturalistic understanding of action consists in the basic confusion of

- actions as attempts at purpose realisation;
- behaviours as effects of causes (including the elaborate form: as a function of a system).

Both the indication of the purpose as well as the causes (conditions) of an action can be, depending on the context, a sensible explanation of the action. However, it is necessary to criticise the interpretation that actions as attempts at purpose realisation (finalism) are somehow inadmissible, indecent, unscientific, etc. and require a reduction to the causes.

(b) Normative Scepticism

Normative scepticism means the quite widely held view that nothing can be said about the questions of the normative orientations of human action by means of scientific rationality, and the sciences should abstain from statements in this regard. Behind this is often the so-called value-freedom thesis, connected with the mostly not explicit conviction (a type of professional axiom) that orientation questions are to be conceptualised in the value terminology. If normative statements are not generalisable in principle or also under consideration of certain rationality rules, transdisciplinarity would either lose its point or it would be exposed as cached 'worldview'. In any event, the questions on the assessment of options for action, which make the sciences possible, and their handling would escape a priori the domains of the sciences. Scientific policy advice would be impossible in principle. By contrast, normative issues are handled by the familiar normative sciences such as jurisprudence, economics, ethics and pedagogy, which handle the norm questions from their specific disciplinary perspectives. However, this handling consists

[24] It therefore makes no critical difference whether one interprets actions as effects of electrochemical processes in the brain, as effects of genes or as an effect of the order of siblings in the family.

not only of the description and explanation of normative convictions of individual and collective actors (so to say, *de lege lata*), but rather also under the aspect of a review of the instrumental adequateness, coherence and consistence of normative convictions. In this way, there is prescriptive interference in normative conviction systems (*de lege ferenda*).

2.3.7 The Role of Philosophy in Transdisciplinary Research

Philosophy has understood itself to be a negotiator between life and science since its Greek founding fathers, both theoretically (in regards to the cognitive foundations) as well as practically (in regards to the recommendations for action with respect to society and the state). Since Socrates, Plato and Aristotle, philosophers have again and again (in different roles) taken up the task of 'society advising' (Mittelstraß 2010). For this reason, philosophy has always relied on new developments for interdisciplinary efforts to solve social problems that go beyond the limits of its disciplinary fields, and made impressive contributions to the epistemological and ethical design of interdisciplinary work. At present, philosophy is also making contributions to central issues of scientific-technical culture, both related fundamental problems (such as the reliability and clarity of different forms of knowledge, the meaning of terms or the acceptability of regulations) as well as substantive individual issues (such as the moral status of the embryo, the social acceptability of energy systems or long-term responsibility).

In principle, philosophy is involved in the concert of interdisciplinarity through its sub-disciplines overall. A special emphasis may however be naturally placed on questions that are traditionally assigned to the philosophy of science and ethics.

2.3.7.1 Philosophy of Science

The philosophy of science as a sub-discipline of philosophy has the task of reconstructing the methods, the formation of terms, the development of theory and the theory of scientific disciplines in general and specifically. One of the fundamental tasks of the philosophy of science consists in formulating the criteria for differentiation between scientific recognition claims with respect to pseudo-sciences on the one hand and everyday recognition on the other.

This criteria task is primarily significant for the use of the philosophy of science in the interdisciplinary work context. It is precisely in issues with a transdisciplinary orientation that recognition claims outside of the sciences from everyday intuition to situation- and context-related recognition capacities, pseudoscientific claims and charlatanism have to be carefully differentiated from scientific recognition claims due to their different capabilities to create universally approved knowledge, which in some cases must also be included in a transdisciplinary issue.

Especially when the disciplinary work flows into scientific policy advice, it is necessary to review the respective validity claims. This expectation of the philosophy of science is reinforced when referenced questions of science promotion need to be answered. The question of what cognitive efforts should be promoted by science policy depends on, among other factors, the extent and type of cognitive capacity in different cognitive validity claims. In advance, no one knows what recognition efforts will ultimately be successful. It is also not superfluous to rule out excessive or even obscure recognition projects on account of considerations related to the promotion of science. However, for this, science promotion needs philosophical criteria.

In the interdisciplinary context, the philosophy of science also has the task of keeping attention on both the systematic achievements of scientific disciplines in the narrower sense and the fundamental enlightenment function of science. Science should not simply accumulate knowledge, but rather make a contribution to the liberation of people from physical burdens, social constraints and cognitive mistakes (Bacon principle) (cf. Schäfer 1993).

Ultimately, the cognitive achievements also include the worldview function of the sciences, i.e. the cognitive orientation for the human's understanding of the world and himself as a whole. The philosophy of science is to pay attention that the worldview function of the sciences is validated with respect to non-scientific ideologies and anti-scientific religious conceptions.[25] When the philosophy of science observes these tasks, it makes a contribution to the self-assertion of scientific-technical culture. In addition, it provides criteria for the self-evaluation of this culture.

2.3.7.2 Ethics

A philosophically adequate understanding for ethics as an academic discipline and normative science in the interdisciplinary business alongside jurisprudence and economics is hampered by a number of factors:

- A vulgar Max Weber interpretation that the sciences have to be 'value-free' is considered by many to be axiomatic. It is overlooked that Max Weber did not declare this for all normative issues by any means, but rather only with respect to 'life and death' (cf. Weiß 1985).
- The terminological reconstruction of orientation problems in the value jargon burdens ethics with unnecessary and maybe also irresolvable problems that would not support a reconstruction in the framework of virtue ethics, deontological ethics or utilitarianism (primarily overly ontological commitments, capacity for the truth of moral imperatives, rigorism).

[25] An example is the epistemological differentiation between evolutionary biology on the one hand and religiously based creationism on the other. On account of the philosophy of science criteria, it can be made clear that there are not two opposing scientific paradigms here.

An important element in the assessment of ethics (moral philosophy, “Sittenlehre”) is that ethics as a discipline (*arsethica*) is confounded with its subject, the ethos (the moral, “die Sitte”). An ethos does not primarily consist of sentences, but rather of behaviour and habits.[26] In the interests of understanding the ethos systems (morals), the methodological construction has, however, proven its ability to interpret actions as (usually implicit) obedience to rules. Moral rules can in turn be interpreted as conditional demands and thus as ones that serve the direct guidelines for action. For example, a sentence of a family moral could be as follows: ‘We should have one meal together every day!’; an economic moral could include the sentence: ‘You should not throw good money after bad money!’; the sentence: ‘You shall not covet your neighbour’s wife!’ can be the habit of a larger group moral.

In contrast to the ethos, *ethics (ars ethica)* consist mainly of sentences, namely those that direct demands at anyone. In contrast to the sentences of morals, these do not serve as a guide for action, but rather for the *judgement* of action. A well-known ethical sentence is the *golden rule*: ‘Do unto others as you would have them do unto you!’ This demand does not say what to do, but rather *how* actions are to be judged: One should only take those actions with consequences for others that one would allow others to take with respect to oneself. Other ethical demands include, for example, the *utilitarian rule*: ‘Act so that you will cause the greatest happiness for the greatest number of people through your action!’ Or the *categorical imperative*: ‘Act so that the maxim of your action could become a (general) norm at any time!’ It is the task of the ethics to reconstruct morals for the rules implied in them and to check these moral rules on the basis of ethical judgement instances, and ultimately to judge these instances of judgement according to general points of view such as functionality and consistency. In ethics, rules for the judgement of action are developed and checked from the point of view of universalizability.

The philosophical discipline of *ethics* is basically concerned with discovering the orientations of action that are universalizable, i.e. fundamentally reasonable for everyone. Against the background of the current level of development in the technical culture, there is the quite new task (from a historical perspective) of formulating universalizable rules for action under the conditions of uncertainty and inequality.

If the philosophical layman hears of such a task, it will not be rare for him to shift to a kind of defensive position of the kind: “what justifies ethicists to call someone to take on or refrain from a certain action?” In reality, one could leave it to anyone to act according to their own maxims if this would not lead to conflicts with other actors in a sufficiently large number of cases. The experience of conflict in action is therefore the lifeworld starting point for the necessity of ethical reflection. Through this, it must also be seen how the experience of conflict can lead to a should claim. A basic requirement for this is the possibility of understanding human action such that—*on the one hand*—there can be real conflicts in general, and—*on the other*—that there are strategies for solving conflicts without violence.

[26] Ethos as ‘ensemble of conventionalities’; cf. Marquard (1981, 1986).

Experience has shown that people can strive for a variety of purposes. In some cases, actors try to achieve purposes that cannot be combined with each other, i.e. cannot be achieved simultaneously; this is the situation of the *conflict*. Conflicts can be managed in many ways (i.e. avoided, eliminated or balanced). In principle, the non-discursive strategies can be differentiated from discursive ones. *Non-discursive* strategies range from simple persuasion to desisting from purposes to the liquidation of the opposing actor; fundamentally, they more or less represent a subtle use of violence. *Discursive* strategies are aimed at the non-violent persuasion of actors to desist from their purposes or to shift them to conflict-avoiding aims. The distinction between purposes and goals allows for entry into an argument about whether the desired goals cannot be achieved by another or different setting of purpose. If the actors have an interest in discursive conflict management (which they may in turn not be discursively 'forced' to, of course), then it will be important to reconstruct the rules of such argumentative discourse for purposes and goals. The reconstruction of actions as the following of demands also serves the purpose of making actions accessible in terms of discourse, since the demands can be reconstructed as the conclusions of arguments. In more detail, the task of ethics is to reconstruct these rules of discursive conflict management. It specifies the rules of procedure in moral discourse.

In discourses related to the goals and purposes (*justification discourses*), the discourse parties strive for a discursive agreement on the purposes. If such an agreement is reached, then it is valid for the parties, i.e. the actors draw on the discourse results for their *permission* as well as their *obligation* to complete certain actions. Permission and obligation are therefore also bound to the basic possibility of discursive conflict management. If there are no conflicts or the actors are not persuaded that non-discursive strategies (e.g. due to higher effectiveness) are preferable, one can clearly not speak of permission and obligation.

Almost all known morals have a particularistic orientation because they limit the discourse participation to people who are characterised by certain points of view (belonging to a tribe, caste, confession, race, class, gender, etc.). Particularistic morals can satisfactorily control the group-internal management of conflict, but they always reach their limits when there are conflicts between groups. If, as a precaution, one attaches importance to the maximum exploitation of conflict solution possibilities, one must allow everyone as a discourse participant (*universalism*). Above all in a view of the emerging global society, the ethical universalism is the position that is preferred by ethics. This is the functional reason why the ethical rules always aspire to universalizability.

If the morals are subject to ethical criticism, it is necessary to check whether the maxims that make up these morals can be universalized. If ethics judges morals as not universalizable, it is to be explained how the inherent maxims must be changed so that they can be universalised and are thus conflict-free. Everyone who can enforce a claim through the statement of a demand should participate in the moral discourse—and thus potentially produce conflicts. The universality of the ethical imperative covers *everyone who is able to perform or understand the speech act of demanding*.

Chapter 3
Knowing and Acting

3.1 Scientific Expertise as a Branch of Transdisciplinary Science

Martin Carrier

3.1.1 Introduction

Transdisciplinary science addresses problems of specifically practical concern that are raised by the general public or by politics. In this chapter we deal with "scientific expertise", i.e., recommendations issued by scientists on problems that are relevant from an extra-scientific point of view. These recommendations are based on scientific knowledge, but directed at concrete problems. Expert recommendations address specific challenges and are expected to provide tailor-made proposals as to how to deal with these challenges. Scientific experts often give policy advice and draw on science for elaborating their recommendations. For instance, scientific experts decide about the efficacy of medical drugs or about the safety of tanning devices or cell phones (as the German radiation protection commission, the *Strahlenschutzkommission*, does). The usual self-understanding of scientific experts is that they bring scientific knowledge to bear on the particular case at hand. Expertise is often passed off as the mere tapping of the repository of knowledge or of simply applying scientific knowledge to experience.

I argue in this chapter, first, that scientific expertise is based on scientific knowledge, but that, second, scientific expertise draws on additional knowledge elements and is governed by specific quality standards. Scientific expertise needs to bridge the gap between general knowledge and narrow, local challenges. The purpose of this chapter is to sketch preconditions, significance requirements, and social settings that are specifically relevant for expert recommendations. The first group of criteria is *epistemic* in kind. Experts face the challenge to elaborate

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scientific knowledge such that it is relevant for elucidating the particular problems placed on their agenda. It is not a matter of course that the appeal to scientific generalizations is the best option for handling small-scale or even singular cases that are characterized by an intermingling of influences and causal factors that perhaps does not obtain a second time in quite the same way. The second group of criteria concerns the *social processes* involved in producing science-based recommendations. Critical factors are which kinds of considerations are included in the deliberation process and whether stakeholders and experience-based experts are integrated. Social quality criteria of scientific expertise are *social robustness*, *expert legitimacy*, and *social participation* (Carrier 2010b).

3.1.2 Epistemic Demands of Significant Expert Judgments

Scientific expertise is faced with problems that originate from outside of science. As a result, helpful answers are typically not part of the system of knowledge ready for the picking. Rather, scientific knowledge needs to be developed such that the general truths of science are able to deliver on these concrete practical demands. Universal accounts need to be adjusted for the sake of illuminating singular circumstances. Scientific generalizations often refer to controlled laboratory conditions or the undistorted core of the phenomena, so that their pertinence for the complex and messy questions expertise needs to tackle is not a matter of course. In addition, such practical demands usually cross disciplinary boundaries so that approaches from different theories and branches of science need to be combined. It is often not clear at the outset what kind of expertise is relevant for a particular case at hand.

Expert judgments are expected to provide specific or tailor-made recommendations that incorporate the pertinent scientific information. Yet given the rift between the general and particular, it may appear doubtful whether scientific knowledge is the most appropriate basis for dealing with such practical challenges. For instance, the celebrated case-study on the relationship between scientific knowledge and lay experience conducted by Brian Wynne (1996) makes a case for the claim that it is sometimes the laypeople, being familiar with the local conditions, who are much better in giving good advice than scientists. The case-study refers to the problems British sheep farmers encountered after the Chernobyl accident in 1986. These sheep farmers had long-term experiences with low-level radioactive fallout from the nearby Windscale reprocessing plant and understood a lot about the economic and ecological conditions of sheep farming. By contrast, the scientists sent by the government in order to solve the problems of the sheep farmers were rushing in with grossly incompetent advice. They failed to adjust their general, science-based models to the local circumstances (Wynne 1996; see Whyte and Crease 2010, pp. 415–417 for a summary of the case-study).

The suggested conclusion is that generalizations might not be relevant for meeting the disordered and unique practical challenges that scientific experts are demanded to deal with. In this section I analyse the role of scientific knowledge in expert judgment and defend the claim that scientific expertise implements a set of epistemic and non-epistemic values that is different from the one employed in epistemic or fundamental research. If these particular commitments are given heed to, science-based expertise is vindicated. I address the relevance problem of science for scientific expertise by sketching epistemic criteria that are supposed to guide the elaboration of scientific knowledge.

3.1.2.1 Model-Building as a Basis of Expert Judgment

My chief argument for the relevance of science for expertise is built on the so-called model debate of the 1990s which contributed much to clarifying the conceptual process of bringing scientific theories to bear on particular phenomena. Its result was that general principles can never be simply applied to experience; they rather need to be enriched with low-level generalizations and empirical adjustments of various sorts. Theoretical principles apply to idealized conditions that may deviate significantly from what is observed in practice. However, rather than abandoning theory-centred approaches, scientists draw on them in order to structure the problem-situation in conceptual respect. Theory is used for highlighting significant features and for distinguishing them from irrelevant aspects. The overarching lesson is that when it comes to mastering complexity, the models used are more heterogeneous than the theoretical models designed to cope with undistorted arrays. Models rely on observational regularities and correction factors for filling the interstices left by general theory. However, the models are still conceptually shaped by theory (Carrier 2004b, 2010b). My chief argument for the relevance of scientific knowledge to expert judgment is that this same pattern of model building is followed in both fields. The pertinent accounts in either field make use of theoretical principles but correct for their shortcomings by appending empirical adaptations. These accounts include empirical generalizations without theoretical support and draw on incongruous models. Because of this similarity among the modes of reasoning, scientific theories can inform expert advice.

Let me buttress this claim by sketching two cases of epistemic research that exhibit the characteristics mentioned. The first one stems from theoretical chemistry and concerns the evaluation of the so-called activation energy of molecules by Henry Eyring and Michael Polanyi in Berlin in 1929 and 1930. The activation energy determines the behaviour of the pertinent molecules in chemical reactions. It can be derived from the values and changes of the total binding energy of the molecules involved in the relevant experiments. The binding energy depends on two contributions, namely, the Coulomb attraction between electrons and protons, and the resonance energy due to the quantum mechanical delocalization of the electrons. The binding energy and its contributions should be computable ab initio from Schrödinger's equation, but the value Eyring and Polanyi produced was in poor agreement with measured values for the hydrogen atom. More complex cases were beyond the reach of the computational methods available at the time. Eyring and Polanyi proceeded by determining the total binding energy from spectroscopic data. The so-called Morse-equation expresses a purely empirical, measured relation between the binding energy and the distance between the atoms in a diatomic molecule. Then they used the theoretical value for the Coulomb interaction that appeared more trustworthy to them than the value for the more specifically quantum mechanical resonance energy. They obtained the latter value by subtracting the theoretical value of the Coulomb interaction from the empirical value of the binding energy. Finally, they introduced some experience-based corrections to their results, extrapolated them to other atoms and reactions without theoretical justification, and presented the outcome as their evaluation of activation energies (Ramsey 1997).

This example is intended to make plausible that model-building in epistemic research may be characterized by combining theory-based and data-based reasoning such that the models are conceptually structured by theory, but need to be completed by appending empirical generalizations without theoretical underpinning.

In a similar vein, epistemic science sometimes combines incongruous partial models. For instance, no single consistent model of the atomic nucleus could be developed; rather, two contrasting models need to be invoked for accounting for different aspects of the phenomena. The so-called liquid drop model treats nucleons analogously to particles in a liquid drop: they move rapidly and undergo frequent collisions. This model is able to account for the absolute values of nuclear binding energy and its approximate dependence on nuclear mass. The complementary shell model takes into consideration that nucleons possess quantum properties and, in particular, obey Pauli's exclusion principle. This model takes care of nuclear spin, and adds small-scale corrections to the nuclear binding energy as estimated on the basis of the liquid drop model. The pivotal aspect is that these quantum features cannot simply be added to the liquid drop approach. Quantum considerations are inconsistent with the basically classical approach pursued in the liquid-drop account. As a result, no single, coherent picture of the atomic nucleus emerges; we are left with incompatible models for different purposes. Each one renders only part of the relevant features and cannot be enriched such that it yields a comprehensive picture (Morrison 1998).

In this case, both models are shaped conceptually by overarching theories. The trouble is that two inconsistent theories need to be invoked. Accordingly, the two partial models are stitched together without theoretical justification and applied to the data according to the explanatory purposes at hand.

The two examples are intended to suggest that reasoning and explaining in epistemic research occasionally proceeds in a more makeshift fashion than one might assume. Models used in epistemic research are sometimes heterogeneous in that they include observational regularities and correction factors and in that they are composed of incoherent parts. Yet they remain conceptually shaped by general theory. In the second step I want to make plausible that these same characteristics apply to expert advice as well. Scientific expertise likewise operates by drawing on general principles that do not always go together well and by correcting for their shortcomings by appeal to empirical adaptations. As a result, scientific generalizations can be helpful for clarifying challenges that are less regular and transparent than the situations these generalizations were originally intended to capture.

The first example of expert judgment presents a case in which the advice essentially relies on experience-based estimates devoid of theoretical foundation. The mentioned "radiation protection commission" takes care in Germany of protecting the population from unwholesome effects of radioactivity. One of the arguments used for establishing suitable threshold values for the public is that no harm should be caused by doses that lie within the range of natural variations of the background radiation. No physiological analysis is underlying this limit value but only the consideration that a move within the country could bring an additional radioactive exposure of the same magnitude in its train. If the pertinent threshold value were placed within this interval of natural fluctuation, one would have to evacuate certain regions of the country. No epidemiological data suggested such a dramatic step. This deliberation is not based on advanced scientific theory but relies on pragmatic reasoning.

In a similar vein, combining various approaches of a diverse conceptual nature to an incoherent overall account is a feature typical of expert reasoning as well. For instance, the radiation protection commission finds it necessary at times to invoke conflicting scientific accounts. Assessments of the detrimental effect of ionizing radiation are chiefly based on three sources. One is epidemiological and relies on the observed impact that the atomic bomb explosions of Hiroshima and Nagasaki and the accident of Chernobyl had on humans. The other one is cell physiological and relies on experiments with cells subjected to radiation. These latter investigations suggested the existence of cellular repair mechanisms that are able to cope with low-dose radiation. The third one is likewise experimental and concerns the study of the damage done to animals by exposing them to known doses of radiation. The salient point is that the first approach intimated a linear dose-effect relationship, while the other two methods indicated a quadratic relationship. If the quadratic relationship holds, low doses of radiation would be less harmful as compared to a linear relationship. The commission was faced with conflicting results that suggest different kinds of low-dose regulation. The respective credentials are unclear: the evidence that speaks in favour of the linear relationship refers to humans, but comes from uncontrolled conditions, whereas the data supporting a quadratic relationship concern animals and cells but are gathered under controlled circumstances. What the commission did in the end was "interpolating" the two incoherent outcomes.[1] As a result, we find the two strategies highlighted for model-building in epistemic science mutatis mutandis in expert reasoning as well. In both frameworks, experience-based estimates are appealed to and conflicting accounts are stitched together. The modes of model-building used in expert reasoning are analogous to those employed in epistemic research. This is why the latter is relevant to the former (Carrier 2010b).

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[1] Interview conducted with Günther Dietze, a former chairman of the *Strahlenschutzkommission*, on June 16, 2010.

3.1.2.2 Universality and Specificity

I tried to make plausible in the preceding paragraph that scientific principles are of some use in meeting specific challenges but that more restricted regularities or local knowledge are also important for bridging the gap between the general claims made in science and the particular requirements of practical problems. Local knowledge may acquire a crucial role in conceiving tailor-made solutions to a specific or narrow problem. I mentioned Wynne's sheep farmers who possessed relevant experience that was neglected by the scientists (see Sect. 3.1.2). Yet the farmers understood a lot about the soil and the ecological conditions at the relevant location that was completely unknown to the scientific experts. Harry Collins and Robert Evans speak of "experience-based experts" in this connection. Such people have advanced knowledge of the relevant details in virtue of their familiarity with the field at hand, but their competence is not recognized by any academic degree or official certificate (Collins and Evans 2002, p. 238). Such non-certified experts frequently have important factual contributions to make; they should not be confused with stakeholders who bring to bear local interests (see Sect. 3.1.3.3). Taking up this local knowledge can be a means for improving expert analyses and recommendations. This epistemic contribution of the public is a far cry from advocating stakeholders' interests.

Experience-based knowledge of this sort is highly relevant for lots of experts' tasks such as protecting the coastline or preserving the extant variety of species. Its advantage is that it derives from the enduring and unmediated encounter with the specific problem areas addressed. As a result, experience-based knowledge is local right from the start; unlike scientific knowledge it need not be adjusted to the local particulars. However, this limitation to a local, restricted perspective exhibits a major weakness of experience-based knowledge. If the ecological impact of garbage dumps or sea-based wind-turbines is at stake, the local peasants and fishermen are able to assess directly some of the emerging consequences on site but are not competent for appreciating the benefit and drawback elsewhere, nor their long-term effects. There are assets and liabilities of both scientific and local knowledge (Carrier 2010c).

3.1.2.3 Robustness

Scientific expertise is subject to quality standards that depart in a characteristic way from the criteria of judgment current in epistemic science (Weingart et al. 2007, pp. 299–304). The pivot of this reorientation is the notion of epistemic robustness. "Robustness" is intended to express that the thrust of an expert recommendation remains intact even if the relevant influences vary to some degree. "Epistemic robustness" designates the invariance of the outcome if the pertinent causal factors and factual conditions fluctuate or are unknown; "social robustness" expresses the same invariance regarding a range of interests and value commitments (see Sect. 3.1.3). Robustness marks the scope of acceptability in the face of ignorance of the precise circumstances and being confronted with a diversity of non-epistemic commitments. For instance, an expert recommendation for devising a national energy supply system should be suitable to secure allocation even under unpredictable circumstances. That is, the amount of electricity provided should be guaranteed in spite of fluctuations in renewable resources like wind power or solar radiation, and regardless of political uncertainties concerning the access to fossil resources like oil or gas. In addition, the technologies employed should operate in conformity with the interests and the values professed within the society concerned. Such considerations could favour the use of renewable resources and make nuclear power plants less than attractive. Epistemic robustness outlines the kind of reliability that is relevant for expertise and designates the leeway of feasibility, including its limitations. Social robustness refers to the room left for societal compatibility of a recommendation and respects or at least lays open the constraints involved in its social or political implementation.

Problems addressed by scientific expertise arise within the realm of practice. For this reason they are usually subject to a multitude of causal influences and characterized by high complexity. In contrast to epistemic science, such practical problems can only rarely be simplified in a way that they can be treated adequately by appeal to idealizations and approximations. This might make one suspect that scientific experts are compelled to appeal to superficial models of the relevant processes and are only able to provide rough tendencies and insecure estimates. Yet this potential deficiency need not hurt the usefulness of an expert analysis or recommendation. More often than not, dealing successfully with practical challenges does not require accounting for the details; rather, expounding the striking features of the issue will suffice. Epistemic robustness is an important objective for scientific expertise since addressing the minute particulars is often immaterial for deciding about how to respond to a practical challenge. The situation needs to be clarified only to an extent that allows experts to provide an unambiguous analysis or recommendation (Funtowicz and Ravetz 1993a, 1994).

Speaking more generally, in questions of practical import, it is essential to stay within a corridor of admissible values while it is much less important what the precise numerical quantities of the relevant parameters are. Regarding ionizing radiation, for instance, the crucial requirement is to remain below the corresponding limit value. Given that you stay within this range, it is much less urgent to ascertain accurately what the radiation level is. As a result, in general it is neither desirable nor feasible to anticipate the precise magnitudes; the only predictions that count are those that make a difference for human action. The commitment to epistemic robustness tends to reduce the importance of accuracy.

3.1.2.4 Dealing with Hazard and Uncertainty

Expertise is part of the social arena and thus needs to take into account the practical impact of recommending certain actions. Philosophers of science have suggested that research accompanied by practically relevant outcome requires giving heed to the practical consequences of an erroneous adoption of a hypothesis. These considerations can be brought to bear on scientific expertise as well. The critical factor is the distinction between falsely positive and falsely negative judgments. In the former case, a hypothesis is erroneously adopted; in the latter it is incorrectly rejected. Epistemic researchers are prone to prefer false negatives to false positives. The reason is that a false negative is typically regarded as being due to proper epistemic care: a hypothesis is only accepted if it is clearly supported by the evidence. Setting a demanding threshold for including assumptions in the system of knowledge is supposed to vouch for the dependability of the knowledge gained. By contrast, false positives are liable to be taken as indicating rash and premature acceptance and lack of severe standards in adopting assumptions.

However, in matters of practical relevance, misjudgements may lead to unacceptable risks and produce lots of damage beyond the walls of libraries and laboratories. From this angle, a shift in the requirements for adopting or discarding recommendations can appear vindicated. Assume that much greater non-epistemic risks are incurred by erroneously dismissing a hypothesis (i.e., by a false negative) than by its mistaken approval (i.e., by a false positive). Under such circumstances, it is plausible to adjust the standards for embracing and discarding hypotheses. In contradistinction to epistemic research, false positives could become preferable to false negatives (see Sect. 2.2.5) (Rudner 1953; Douglas 2000, 2004).

These considerations can be directly transferred to scientific expertise: if epistemic uncertainties prevail, the non-epistemic risks involved should become part of the analysis and recommendation. This might lead to accepting a hypothesis more quickly than in epistemic research. Climate change is a case in point. If we wrongly suspect that climate change is anthropogenic, the damage done is confined to superfluous investments in environmental protection. If, by contrast, we mistakenly assume that climate change unfolds unaffected by human action, then we run the risk of making this planet a fairly inhospitable place. This distribution of hazards speaks in favour of adopting the hypothesis of man-made climate change as a basis for action even if this hypothesis is not buttressed by hard evidence to an amount demanded in epistemic science. Accordingly, scientific experts are well advised to give heed to the practical consequences of error.

It is noteworthy that considerations and recommendations of the German radiation protection commission betray an increasing awareness of the problem of false negatives. Until approximately the turn of the century the commission was inclined to dismiss hazards and risks associated with new technologies unless detrimental effects were scientifically proven. The intention was to reassure the public and to dispel worries. However, this ploy backfires if it turns out later that the technology was not as harmless as the experts had declared. The credibility of the commission would be seriously impaired by false negatives of this sort. Beginning in the late 1990s, the commission has dealt much more cautiously with this problem. For instance, a more recent recommendation treats the hazards possibly associated with non-ionizing radiation emitted by cell phones. The traditional mode or reasoning would have been that scientific knowledge does not indicate any effects of this sort and that therefore no such risks exist. In their more recent study, the reasoning of the commission is that the available state of knowledge does not support the assumption that such risks exist and that for this reason more research on the issue is requisite. This changed strategy shows the awareness of the threat that false negatives pose to the credibility of the commission (Krohn 2012). Further, the stronger emphasis on the limits of knowledge and on ignorance is in much better agreement with the actual practice of producing advice than the earlier attitude of omniscience. As I tried to make plausible, the actual reasoning of expert commissions in general includes tentative modelling, ad-hoc interpolations between conflicting scientific statements, and pragmatically motivated assumptions. A practice of this sort does not square well with a self-understanding that takes recommendations as being produced directly and unambiguously by scientific considerations of the relevant matters of fact.

To conclude this section on the credentials of expertise, my claim is that expertise is governed by epistemic and non-epistemic values that diverge from the values brought to bear in epistemic research. The values relevant for expertise deviate from the aforementioned values characteristic of epistemic research by three related features. First, scientific expertise places heavy emphasis on special incidents in contradistinction to general kinds. Expertise focuses on the individual case in its own right. As a result, scientific expertise is characterized by a set of epistemic values that overlaps with their counterparts within epistemic science but is distinctive in important respect. Robustness and specificity are placed in the limelight whereas universality and precision are shifted to the backdrop. Second, this commitment to the primacy of narrowly framed problems suggests a more important role of experience-based experts who are often the ones in command of the local knowledge required to account for the specifics of the case at hand. Third, the social impact of a recommended problem solution is of crucial importance in scientific expertise. It is contentious whether non-epistemic values are legitimately appealed to in fundamental research, but there is no doubt that such non-epistemic values rightly play a salient role for the appropriateness of scientific expertise.

3.1.3 Social Conditions of Appropriate Expert Judgment

Scientific experts operate at the interface of science and society. As a result, the ability to take up social aspirations and concerns is an essential element of good expert advice. Three such features are particularly relevant: social robustness, legitimacy of experts, and the uptake of value considerations (Carrier 2010b).

3.1.3.1 Social Robustness

A socially robust analysis or recommendation is acceptable within a wide spectrum of diverse interests and value commitments (see Sect. 3.1.2). Social robustness articulates the leeway and the limits of societal compatibility. Giving heed to local points of view is best achieved by listening to the stakeholders, that is, by social participation. In other words, social robustness aims at social inclusion. Expert analyses and recommendations are advised to take stakeholders' views into account. This political requirement is a far cry from the epistemic requirement addressed earlier (see Sect. 3.1.2.2). Including local voices as an epistemic resource is a means for advancing the process of deliberation, while social participation as a political measure seeks to promote the odds of implementation of the relevant advice. Both kinds of participation need to be kept distinct conceptually. Local interests are of quite a different nature than the epistemic contributions made by local knowledge. Yet participatory approaches are apt to serve both ends; they tend to promote the political and the epistemic objective.

Accordingly, social robustness may be considered as an attempt to get analyses and recommendations of high epistemic quality politically realized (rather than enhancing their quality). Social robustness is a policy for implementing expert recommendations. From an epistemic angle, there is often room left for resolving an issue of social import. This room is filled by taking social interests and value commitments into consideration.

3.1.3.2 Expert Legitimacy

The quality of expert advice is not only dependent on its substantive elements. Rather, the assumed legitimacy of the experts involved also contributes to the persuasiveness of expertise and thus fosters or detracts from social robustness. The requirement that scientific experts be objective is influential on this legitimacy. Notions of scientific objectivity are governed by two ideal types, namely, objectivity as adequacy to the facts and objectivity as reciprocal control. The former notion goes back to Francis Bacon and conceives objectivity as a detached stance of scientists and the conformity of their views with the situation. No distortions intrude, no bias is admitted, and all unfounded prejudices are abandoned (Bacon 1620). If this approach is transferred to scientific expertise, legitimate experts are characterized by their independence and neutrality. Their judgment is guided by nothing else than the painstaking reflection of the relevant matters of fact.

The pluralist understanding of objectivity considers it an epistemic virtue to take conflicting views into account (see Sect. 2.2.5). Objectivity is tied up with mutual control and reciprocal criticism that serve to keep different biases in check. Error and one-sidedness is regulated or managed by confronting them with different kinds of error and one-sidedness. Transferring this approach to expertise means to distinguish the objectivity of expertise from the objectivity of experts. Objectivity may not be a virtue of a single expert advice but rather accrue from the competition between an expert report and a contradictory report. The persuasiveness of expertise requires that in case of doubt affiliations and interests are counterbalanced by opposing affiliations and interests.

Ascertaining the persuasiveness of expertise by ensuring the legitimacy of experts operates by selecting experts either according to their neutrality or plurality. The notion of neutrality underlies the requirement that experts be impartial. A close affiliation between clients from politics or the economy and experts in matters like examining the health hazards or the side-effects possibly associated with medical drugs prompts the suspicion of partisan, one-sided and less than trustworthy judgment. For instance, the results of comparative clinical trials of new medical drugs have turned out to be in complete harmony with the economic interests of the sponsors of these studies (see Sect. 2.1.2). The notion of plurality is implemented in selecting the members of expert committees according to the maxim that different approaches should be given a voice in the committee. Experts of a diversity of persuasions should be part of the committee. This principle of selection is sometimes applied to expert committees that give political advice (such as the German Ethics Council (*Deutscher Ethikrat*)).

The prevailing notion of objectivity is the Baconian understanding as detachment and neutrality. The trouble is that it is difficult in many areas to find experts who are at the same time competent and disinterested. This is particularly salient in medical research where almost every specialist has financial ties with one or several relevant manufacturers. Advanced knowledge and non-epistemic interests are heavily intertwined in such fields. The pluralist notion of objectivity provides an approach to approaching objectivity even under such conditions. The key is to contrast experts with counter-experts and to see to it that a balance of opposing interests emerges. Consider this example of an expert controversy. In the early 2000s, scientists articulated worries to the effect that the anti-clotting efficacy of aspirin would decrease over the years. The claim was that a habituation effect occurred. Some years later it was revealed by a journalist that the whole debate had been launched by a company producing alternative anti-clotting agents. Conversely, some of the leading scientists opposing this alleged drop in effectiveness had been funded by the aspirin manufacturer Bayer (Wise 2011, p. 288). Norton Wise takes this event as an indication of the skewing effects of commercialization. I take it as an indication of the corrective influence of competing economic interests. Competition can create a pluralist setting that serves to promote objectivity. In sum, transparency of institutional affiliation and the confrontation of a multiplicity of non-epistemic commitments is the only choice for bringing expert knowledge to bear on commercialized fields.

3.1.3.3 Taking Values into Consideration

Giving heed to social robustness means that experts take existing value attitudes and interests into account in order to improve the odds of getting their recommendation implemented. However, experts also legitimately deal with value issues directly. This is not to say that scientific experts ought to set values on their own. The epistemic authority of science is restricted to the realm of the factual. Citizens represent the only source of normative orientation and value commitment. Nevertheless, on some occasions scientific experts legitimately address value issues. I briefly go into two such cases, namely, determining exposure limits and examining the consistency of value commitments.

First, scientific expert commissions are likely to pretend that they speak on behalf of nature—or at least their scientific discipline. However, some of the issues experts deal with make one realize quickly that this is a self-deception. Take the example of establishing threshold values. In Germany, expert commissions recommend limits of exposure to certain substances at the workplace. To be sure, such recommendations have a scientific basis. The toxicity of certain substances certainly is of relevance for placing such limits. But when it comes to actually establishing occupational exposure limits for hazardous substances such as asbestos or PCB, additional considerations creep in. Given the variation of sensitivity to such substances among humans, it is not possible to avoid all risks short of prohibiting all handling of such materials. Because of this individual variability, the question what is innocuous to humans has no unambiguous answer. Accordingly, the issue is frequently transformed into the question which risks are acceptable. Quite obviously, science does not respond to this question. In fact, the relevant reasoning involves balancing values such as prosperity or the appreciation of a technology-based way of life with values such as health and naturalness. Another consideration advanced in this context is that some risks are legitimately incurred if people are aware of them and do voluntarily what they do. This reasoning illuminates that setting exposure limits requires interlacing scientific knowledge with evaluative judgments.

Second, and quite differently, the consistency or incoherence of values can be subject to expert judgment. Consider a consulting assignment in which politicians request comprehensive advice as to the impact of various forms of agriculture on biodiversity. Consider further that in the process of making this request more precise, the question is contracted to comparing conventional agriculture with cultivating genetically modified crops. Scientific experts may legitimately point out the discrepancy between the more general initial commitment and the much narrower version in the actual assignment which excluded ecological agriculture as a further option.[2] Dealing with values and interests in this analytical mode stays well within the limits of legitimate policy advice. This mode involves the examination of relations between different values and makes the incoherence between explicitly stated goals and the more concrete operational assignments salient. Addressing values in this mode serves the aim to make values, interests, and their impact on an expert recommendation transparent and consistent.

3.1.4 Conclusion

Science in the context of application is intended to address practical problems. Research of this sort is "transdisciplinary": it proceeds as a demand-driven rather than a knowledge-driven endeavour. Its results are supposed to be relevant to public affairs, but it takes efforts on the substantive and the organizational side, first, to

[2] The example is due to Matthias Kaiser, oral communication.

justify the credibility and relevance of these results and, second, to gain the public recognition of these results. This applies in particular to scientific expertise, i.e., the attempt to illuminate concrete practical issues by appeal to scientific knowledge.

The reputation of scientific experts has suffered in the past years or decades. And rightly so: Experts were notoriously wrong in the 1980s and 1990s in coping with the Chernobyl accident, the hazards posed by the early rise of BSE, and the requirements facing energy supply systems. Likewise, remember the gross underestimation of the risks involved in spraying DDT, a substance that was advertised after the war by scientific experts as the ultimate insecticide. Conversely, they exaggerated the millennium bug (if we recall this piece of false alarm from oblivion), and the risks associated with avian flu and swine flu. Blatant misjudgements and egregious errors of economics regarding economic predictions and recommendations of political measures are notorious and nourish doubts concerning the relevance of economics for understanding the economy. Further, the studies mentioned regarding the close connection between who pays for a clinical trial and who benefits from it have made critics suspect that science is venal. Research findings are considered to be for sale in some quarters (Krimsky 2003). Science is accused of serving its customers in a particularly obliging manner. Whatever there is to such feelings, they have contributed to creating an attitude of suspicion and distrust toward science.

Consequently, science in the social arena and scientific expertise, in particular, need to regain public trust. An important first step in this direction is to acknowledge uncertainties and not to convey the specious impression of infallibility. It is difficult to extend scientific principles to the particulars of the phenomena. As a result, sometimes no unambiguous answer of science to a practical question exists. A major stride in regaining public trust is that scientists and experts recognize the limitations of the grip of science. Openness of this sort is a factor in making scientific opinions and suggestions epistemically robust. Second, taking up substantive contributions from the public is crucial for establishing the trustworthiness of scientific views. The exclusion of experience-based, non-certified experts is a sure means for destroying trust. Participatory mechanisms are not to be seen as a vehicle for suspending representative democracy but rather as a channel for enriching the process of deliberation.

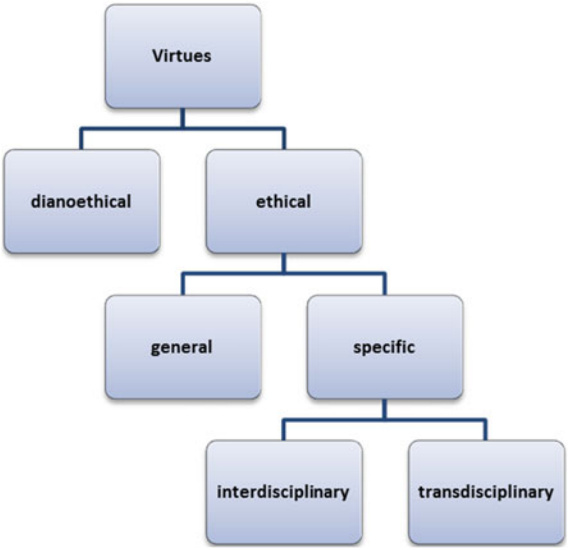
3.2 Virtues and Rational Aspects of Interdisciplinary Research

Michael Quante

I play where my trainer tells me to.
(Football adage)

Interdisciplinary work constitutes a specific scholarly context which places special demands, also in regard to normativity, on the persons involved. For this

Fig. 3.1 Interdisciplinary and transdisciplinary as subsets of specific ethical virtues



reason, the following will discuss whether—beyond the epistemic and ethical virtues relevant for scholarly conduct—there are ethical virtues and normative requirements which are crucial for interdisciplinary research.[3] In this undertaking, we base our assumptions on the following simplified system (Fig. 3.1).

If the special case of the intrapsychic interdisciplinary work of one individual researcher[4] is disregarded, interdisciplinary research generally presents a case of cooperative action. In order for cooperation to be successful, adherence to certain norms is a prerequisite. Moreover, certain norms are presupposed to be held valid by the persons involved, so that violations of the expectations on which these norms are based can lead to significant disturbances in the cooperation and thus to the failure of the working relationship. In that regard, differentiation is to be made between two kinds of conflicts, even if they actually often overlap. On the one hand, conflicts caused by the respective constellation of the disciplines involved can arise (as explicated in Sect. 3.2.1.2). On the other hand, conflicts can also result from the respective individual procedures of the persons involved (as discussed in

[3] For epistemic virtues, cf. Sects. 2.2.1 and 3.1.

[4] For the purposes of this study, intrapsychic interdisciplinarity should be understood as internalised interdisciplinary cooperation in the sense of assumption of internalized roles. This does not deny that there can be successful intrapsychic interdisciplinarity. Using the analogy with multilingualism and the problem of translation connected with it as an illustration, Quine's thesis cannot be considered appropriate that the problem of translation could be circumvented through "settling down" for "heimisch werden" (becoming a resident): "We now see a way, though costly, in which he can still accomplish radical translation of such sentences. He can settle down and learn the native language directly as an infant might" (Quine 1960, p. 47). The costs in this case would not only be too high, but the witticism of the division of labour would also be missing.

Sect. 3.2.2). In other words, both the roles predetermined by the fields within interdisciplinary cooperation and also the respective individual interpretations of these roles by the scholars involved can lead to tensions and conflicts which endanger the cooperation. It is thus helpful to come to an understanding in regard to both of these sources of conflict and their particular respective nature.

In the first part of this chapter, the challenges of interdisciplinarity resulting from the specific form of cooperation are identified. The attempt to determine types and conditions for their eventuality should contribute to clarification of the demands on those involved and enable them to approach this task with greater reflexive clarity. The structures identified in this process concern cooperation between disciplines near to one another, as well as between disciplines which are widely divergent from one another, for example, in regard to their methods and research interests.[5]

The second part is then devoted to the question of which ethical virtues should especially have to be assumed and required in this context. The minimum prerequisite for this is for these virtues to be systematized cautiously (whereby it cannot be assumed that each of these or even their entirety is exclusive for interdisciplinarity). In the best case, such a basis can even be used for the formulation of rules for exercising this virtue in the context of interdisciplinarity and transdisciplinarity. These can then be followed by determining the plausibility—in the case of conflicting demands—of rules of priority among the virtues specified. Since it is a matter of a complex and extremely context-sensitive type of problem, however, such systematization has to be formulated cautiously. This system certainly cannot claim to be a manual with operationalisable rules for identification of scholars who are suitable for interdisciplinarity or transdisciplinarity. Nevertheless, it would have value as a heuristic checklist and as a diagnostic instrument for determining the causes for unsuccessful cooperation in this context of activity.[6]

An emphasis on the ethical virtues of the persons involved in the cooperation should not contest the existence, beyond these, of registered norms for cooperation and an ethically significant institutional dimension in regard to interdisciplinarity and transdisciplinarity (just think of the indispensability of society's trust in the subsystem of "academics", which is, however, currently increasingly at stake). Moreover, the misconception should be dispelled that the emphasis chosen here constitutes a meta-ethical position for the ethics of virtue. On the one hand, the fact that virtues also play an important role in deontological ethics shows that such a conclusion is not mandatory. On the other hand, there is the possibility of integrating the aspects mentioned here (virtues, norms, and the institutional ethical

[5] For determination of "great/small interdisciplinarity", cf. Sect. 2.3.

[6] In the end, neither the judgment of the interdisciplinary or transdisciplinary agent nor the judgment of the persons putting together an interdisciplinary or transdisciplinary team can be eliminated. And, of course, it can also depend on the "chemistry" of the entire team which virtues have precedence for a person to be appointed to the team. This is where an irreducible holistic character appears, as is probably familiar to trainers and sport managers in the area of team sports.

dimension) into a comprehensive conception of acknowledgement.[7] Such acknowledgement-theoretical integration is also attractive because it allows the epistemic and normative disparities to be understood as justifiable from the structure of the respective cooperation and thus acceptable as being reasonable (however, this only holds true for conflicts resulting from the structure of the disciplines involved, and not for those arising from the individual configuration of the respective roles).

Looking ahead, there is then a final discussion on how the requirement profile for interdisciplinarity relates to the set of virtues appropriate for transdisciplinarity. In this connection, it becomes clear that the requirement profile for these two fields of activity confronted by scholars are not identical, but instead only partially overlap with one another, which is not entirely surprising in theory, but extremely important in practice.

3.2.1 Types of Possibilities for Interdisciplinarity and Conditions for Them

In order to achieve successful interdisciplinary teamwork, it is essential to determine clearly the goal and the type of the cooperation involved (Sect. 3.2.1.1). Clarification of these is what sets the basis for conditions for possible development of successful cooperation, and these conditions are not independent of the type of interdisciplinarity concerned (Sect. 3.2.1.2) (cf. deliberations by Mittelstraß (1991)). It is only on this basis that the conditions for successful interdisciplinary teamwork can be set, and this is the only way to achieve a common goal for the respective concrete cooperation.

[7] The present discussion about virtues is thus to be understood in just the same way as the discussion of values in a pre-theoretical sense, i.e. remaining understandable in real life and, at the same time, neutral in regard to meta-ethical differences. At this point, it is important to point out a dual distinction: on the one hand, a differentiation has to be made between a virtue (as a disposition) and the concrete practice of it (as completion of an action). On the other hand, and in a crosswise relation to the differentiation between disposition and completion of an action, a differentiation has to be made between the virtues and the abilities of a participant. Qua dispositions, virtues are counted, on the one hand, among abilities (if this term is equated with dispositionality); on the other hand, the ability for mathematical competence and great retentiveness, for example, are not virtues (in contrast to patience or the ability to engage oneself with the thought process of a different discipline, for instance). The latter differentiation requires not only abandonment of the equation of abilities and dispositions and the use of a narrower meaning for "ability", but also differentiating between abilities (in the narrower sense) and virtues. These issues are not able to be pursued further in our investigation, so we limit it to the following two points: first of all, it is conceded that there are also abilities relevant for interdisciplinarity and transdisciplinarity (perhaps the ability to acquire large amounts of data in a different discipline is one of these). And secondly, it should hold that virtues differ from abilities in that the practice of them is accompanied by positive evaluation of performance providing a valuable contribution to the specific cooperation.

This approach suggests a method of description which starts from the assumption of malfunctions in the cooperation since a division of the constellation of actions gives rise to an awareness of the conditions for success which otherwise usually remain implicit. Thus, a consequence of our approach is also the emphasis on reflection upon the cooperative action. This approach should, however, neither contest that interdisciplinary and transdisciplinary action function in most cases, nor does it claim that successful interdisciplinary and transdisciplinary action may not also be performed intuitively by the participants involved. It is exactly because it is a matter of practical knowledge and experience that interdisciplinary competence cannot be and usually is not explicit.

Before addressing the two tasks referred to above, let me illustrate the relevance of these considerations using a current example: the debate about interprofessionality.

Excursus: Interprofessionality

The complexity of our modern society and of many of current problems, for example, in our social institutions demands willingness for interdisciplinary cooperation, not only on the part of academics. Systematic (not just occasional) cooperation is also increasingly required from various professional groups, for example, those in our social security system. In light of this, there is a debate about the structures of this kind of cooperation and the requirements involved with it. "Interprofessionality" in this sense designates the development of closely knit, functioning, and continuous cooperation between representatives from different professions and disciplines (for example, in the area of early education; social work with families, etc.). In relevant literature, interprofessionalisation is understood as the process during which cross-discipline cooperation for fulfilment of the complex needs of users are developed in practice and whose conditions for success are considered in regard to teamwork between different professions and disciplines. In this case, it is assumed that there is a necessity for lasting integrative interaction and the development of "knowledge sharing" (cf. D'Amour and Oandasan (2005); Malin and Morrow (2007)).[8] It is thus a matter of repetitive, stable patterns demonstrating cooperation.

Analytically, various forms of interprofessional teamwork can be differentiated:

(a) Multiprofessionality: at least two persons from different professions work on the same problem in parallel, but separately (and in isolation from one another).
(b) Interprofessionality: representatives of different professions share information and knowledge, but the contexts of action are divided into separate working areas or processes which are oriented on a common goal, but function in isolation from one another.

[8] Since the term "discipline" is used in a specified sense for determination of interdisciplinarity in our study, we differentiate strictly between discipline and profession—in contrast to the usual terminology in literature on interprofessionality. For this reason, mention is only made of professions in the typology, and not of representatives of different disciplines.

(c) Transprofessionality: representatives of different professions share information and also have command of the knowledge and abilities of the other professions (or even of the academic disciplines underlying these). As a result, they are in a position to assume the role of the other partners in the cooperation (partially) and understand the self-concept (i.e. inner perspective) of this other profession. This enables a more intensive exchange of information than in multiprofessionality or interprofessionality and results in an enhanced transfer of the respective abilities between the professions.

Analysis of these different forms of interprofessional cooperation indicates that, in addition to the participants' commitment to and identification with the entire team, the success of interprofessional work depends not only on the communication within the teams, but also on the personal qualities of the participants, as well as on the possibility for all participants within this cooperation to further develop the methods and forms of the teamwork. It is evident that these indicators for successful interprofessional teamwork include structural features of cooperation, as well as individual characteristics (abilities and virtues) of the participants. This also applies to the dimensions identified in literature for determining the working atmosphere in interprofessional teams (cf. Brodbeck and Maier 2001):

- ***Visions*** in the sense of superordinated goals indicating a general direction and which are motivating: characteristics of team vision are clarity in regard to measures and goals, high regard for the professions represented (and their representatives), and consensus between the members of the groups concerning goal contents and also the extent of goal attainability;
- ***Task orientation*** understood as endeavouring to achieve high performance and quality throughout the collective achievement of goals: characteristics for successful interprofessional cooperation include a high level of team reflection and the generation of synergy effects, for example, through mutual assistance;
- ***Participative assurance*** in the sense of enabling exertion of influence on decisions: characteristics for successful interprofessional cooperation include sharing information within the team, fostering relations, and interaction which is not perceived as a threat (or, if so, at least as a small one);
- ***Support for innovation*** characteristics for successful interprofessional cooperation include willingness for innovation, as well as implementation of innovations on the part of team members and the surrounding framework.

As shown in the next sections of this chapter, these empirically determined findings go very well with the structural characteristics of interdisciplinary cooperation and can be further substantiated philosophically by identification of virtues appropriate for interdisciplinarity.

3.2.1.1 Types of Interdisciplinarity

For determination of different types of interdisciplinary teamwork, the adoption of two differentiations is helpful. The first one concerns the purpose of the cooperation envisaged (a), and the second (b) its structure.

(a) Problem Analysis Versus Problem Solution
The first type of cooperation is characterized by an aspiration for teamwork between different disciplines due to the necessity for understanding or analysing a problem or phenomenon adequately. This form of interdisciplinary teamwork for *elucidation of a problem* is found in the area of exploration awareness, for example, where there is a broad consensus that different disciplines have to cooperate with one another in order to be able to describe and analyse a phenomenon of conscious mental conditions. This is how brain researchers, neuroscientists, cognitive scientists, biologists, and philosophers cooperate in an attempt to develop a suitable in this case description and explanation of a phenomenon.

The second type of cooperation sets the goal of solving this problem and producing a result which extends beyond analysis of the phenomenon or problem. *Problem-solving* cooperation can, for example, have the goal of developing a promising therapy in teamwork between medicine and psychology or the goal of trying to find a suitable form for social security systems for today's societal circumstances in teamwork between medicine, psychology, social sciences, law, economics, and philosophy. Furthermore, there is a wide range of cooperation between scientists and engineers when it is a matter of coping with technical problems. The attempt to formulate legislation in teamwork between legal experts, theologians, and philosophers can definitely also be understood as interdisciplinary cooperation which extends beyond problem analysis to the goal of developing solutions to problems in the form of practical norms or legal regulations. Thereby, a further distinction then has to be made between cooperation with the goal of implementing a given normative guideline in an appropriately ethical and effective manner and that in which the priority should be set on determining rationally justifiable and socially acceptable norms. This differentiation is important in order to avoid (or to clear up) the misunderstanding that social implementation of scientifically and technically developed suggestions for the solution of problems could be limited (usually) to either economists or legal experts, depending on the issue in question. Such a limitation would not only skip over the level of determination of plausible norms, but would also mask out the fact that this level of implementation features genuine ethical questions (appropriateness, reasonable limits, etc.).

(b) Horizontal Versus Vertical Interdisciplinary Cooperation
It is useful to distinguish between vertical and horizontal forms of interdisciplinary cooperation. *Horizontal* cooperation takes place whenever there is collective understanding of a problem *and also* a collective definition of the goal of the cooperation. Without the conjunction of these two conditions, it is a matter of *vertical* cooperation.

In vertical cooperation, there can, for example, be a lack of collective understanding of a problem. In this case, one discipline determines the root of the problem, and the other disciplines then formulate the questions whose answers are relevant for the problem understood as such. Secondly, assuming the plausible precondition that cooperation in solving the problem is not possible without successful analysis of the problem or phenomenon, then vertical cooperation can also exist, with one discipline defining the goal of the cooperation extending beyond shared analysis of the problem and formulating questions for the other disciplines, whose answers to these questions are then relevant for achieving this goal. Thirdly, vertical cooperation can even take the form of one discipline stipulating both the understanding of the problem, as well as the goal of the teamwork, whereby this discipline then also formulates the questions for the other participating disciplines.[9] It is clear that the normative expectations and claims of the participants have to correspond to these three forms of cooperation and to the respective roles played in them.

Before discussing conditions for the possibility of interdisciplinary cooperation, two explanatory notes in regard to the differentiation between vertical and horizontal teamwork are necessary.

First of all, this differentiation also allows hybrid forms with greater or smaller common ground with respect to understanding of the phenomenon, problem or goal. For this reason, it is important to identify "pockets" of consensus in practical teamwork and to differentiate between them and areas in which no consensus can be reached. In an interdisciplinary project, these two types of cooperation are thus able to co-exist, depending on whether or not it is the consensual part that is being worked on.[10] The teamwork can, however, suffer if it is not clear which mode of cooperation exists at a given moment, which is the reason why differentiation is important.

Secondly, the vertical type can be referred to as hierarchical since it has one discipline which simultaneously has sovereignty of definition and also appears as the "taskmaster" for the other disciplines, which perform the groundwork for it. In contrast, the structure of horizontal cooperation can be characterized as democratic and consensual because of the collective formulation for problems, goals, and

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9 A special constellation results when interdisciplinary research cooperation is accompanied by ethical research or when an Ethical Board is established. Then the other disciplines are on equal terms with ethics (with exception of the special case when it is philosophers who are integrated in the cooperation). Perception of the contribution of ethics can take on different forms: as a counsellor, as a watchdog (for example, as stipulated in funding formats), as a fig-leaf, or as an orientation for discussion partners.

10 Alternatively, speaking of a consensual part of the problem can be given up and the situation also described as several problems originating from the original problem assumed, so that a differentiation can then be made between consensual and non-consensual problems. The strategy of pre-defined transdisciplinary specifications can, however, result in academia no longer addressing the social inquiry (the problem) since it is broken up into several different problems. If this process is not understandable for non-scientific addressees, the impression arises that academia is evading the transdisciplinary question or simply rejecting it; cf. also Quante (2012).

questions involved. The structural description just mentioned is thus also accompanied by subliminal moral concepts. Many people will certainly be apt to want to speak of "true" teamwork only in the case of horizontal cooperation.[11] In this chapter, however, no valuation is made in connection with the differentiation between vertical and horizontal cooperation. The type of cooperation deemed appropriate and promising is dependent on the nature of the problem and the respective goal specified. Significant disturbances can, however, threaten interdisciplinary teamwork if there is no clarification in advance as to which type of cooperation is involved. Misunderstandings can arise with respect to the function of the individual disciplines in the cooperation and with respect to the nature of their respective contribution. For this reason, all the participants have to be aware that the question of what actually constitutes *the* issue is not a trivial one and that it is not self-evident for their own discipline to be the one to define *the* or the *actual* problem.[12]

3.2.1.2 Conditions for the Possibility of Cooperation

In accordance with the above definition of the difference between horizontal and vertical cooperation, the latter only comes about when there is collective understanding of the phenomenon, i.e. of the problem and the goal of the cooperation. In gradualist formulation: horizontal cooperation is only possible with respect to the aspects of a phenomenon or problem which are jointly regarded as relevant by participants. In all other cases, it is only vertical cooperation that can take place.

In the first step, it is a matter of the conditions which must exist in order for horizontal cooperation to be at all possible (a). As to be expected, it will become apparent that such teamwork becomes more difficult the more strongly the disciplines involved differ with respect to their methodological and ontological prerequisites. Conditions for successful vertical cooperation are, however, neither trivial nor even existent in all cases (b).

(a) Conditions for the Possibility of Horizontal Cooperation

In order to achieve collective understanding of a phenomenon or problem, a whole series of pre-conditions must exist, extending into the self-conception of the substance and methodology of the respective scholarly disciplines. This means, on the one hand, that each of the participants has to not only have explicit awareness of the self-conception of their own discipline, but also be willing for it to be scrutinized through the perspective of the other disciplines. On the basis of the plausible

[11] Such normative criticism of empirical social research with divided responsibilities has been formulated by Adorno, for example (cf. Demirovic 1999, pp. 757 ff.). The appealing task of differentiating general effects of divided responsibilities from the ones specific to interdisciplinary division of work cannot be pursued here.

[12] Willingness to take up and accept this approach can certainly be considered as one of the cardinal virtues of interdisciplinarity.

assumption that, in the individual disciplines, there is also a certain spectrum of views about these aspects, it must also be expected that the manner in which a discipline functions in interdisciplinary cooperation is also dependent upon the nature of the self-conception of the respective individual representing the discipline. This consideration is thus also relevant for the question of which individual representative of a discipline (and also which of the directions represented in a discipline) is selected for interdisciplinary cooperation.[13]

The subject of the self-conception of the respective disciplines can be illustrated through the following series of questions—which is neither complete, nor arranged in any specific order:

- What relevant aspects of the phenomenon have to be clarified?
- What aspects of the phenomenon have to be considered as problematic?
- What is the level of analysis?
- What type of explanation are we searching for (conceptual, causal, historic-genetic)?
- What is even deemed to be an explanation and through what methods can it be achieved?
- What is the goal to be achieved?
- What conditions for adequacy are sufficient for the description or analysis of a problem?

Even if puritanically exact conformity is not required in all points, it becomes clear at first glance that successful cooperation will not be possible if discordance exceeds a certain threshold. This begins with the question of what *the* problem to be solved even is. Is it, to take an example from the area of exploration of the mind, a question of finding causal origins for certain phenomena? Or is it about clarifying the grammar of our mental vocabulary—or maybe about describing bodily processes on the level of brain procedures—or about subjective experience? What aspects of the spiritual are even relevant or are problematic? Is the evidence for the evolutionary utilization of consciousness sufficient as an explanation? Or does such a strategy miss the essence of the problem? And what methods may be used to approach the problem? Is introspection allowed, or must solely measurable observations be resorted to here? Does the goal consist solely in finding a better (more adequate, more comprehensive) explanation, or should it also be the base for guidelines for action, for example, for social institutions?

If there are serious differences with respect to the answers to these questions, it is an error or an inadmissible simplification to assume that even the same problem is

[13] In this context, the question also comes up of whether a representative of the so-called prevailing opinion in a discipline or rather someone holding more heterodox (or at least peripheral) position is selected, for example. This is relevant for recognition of the respective representative within the group (professional reputation), as well as for recognition of the results of the interdisciplinary cooperation by the academic community and society; cf. Sect. 2.3.

being referred to by all. At this point, a differentiation must be made between at least[14] the following constellations:

- cooperation within the natural sciences
- cooperation within the social sciences
- cooperation within the humanities[15]
- cooperation between natural and social sciences
- cooperation between natural sciences and the humanities
- cooperation between social sciences and the humanities
- cooperation between natural sciences, social sciences, and the humanities

Generally speaking, there is a common understanding within the natural sciences about what the relevant aspects of a phenomenon or its problematic characteristics in need of explanation are; as a rule, this understanding comes more easily than within the humanities or between natural sciences and the humanities. Although the natural sciences do indeed also have academic disputes manifested in different understanding of methods and even of problems, but, on the whole, more common ground prevails within each scientific discipline and even between the different scientific disciplines than between and within the humanities (with the exception of the case of disciplines which vie for responsibility for a subject area, which, of course, can also occur within the natural sciences). Moreover, it has also been found that—upon the interdependent prerequisite of the respective relevance and respectability of the other discipline—cooperation progresses more smoothly when the participating disciplines are "farther" apart because the problem of conflicts regarding competence and responsibility arises less often.

It is possible that the primary difference between the humanities and the social sciences, on the one hand, and natural sciences, on the other, is that the subject area of the humanities and the social sciences consists in the self-conception of these areas of academics and in the awareness of the problem and the methods to a greater extent than in the natural sciences. The concept of being able to refer to an objective subject area largely independent of a theoretical approach has its place much more in the natural sciences than in the humanities.[16] The extent to which an objectively stipulated problem is not required, however, is also the extent to which the view of the phenomenon and problem simultaneously becomes the constitution of the phenomenon and the problem. It seems obvious that, under these "constructivist" pre-assumptions, the conflicts in identification of the problem can be greater and the demarcations between the areas of responsibility more and more in conflict with

[14] Engineering sciences can enter into any of the above listed constellations, whereby the aspects of technical feasibility and the goal of practical-technical implementation come into account (and thus an accompanying expansion or even shift in the approach to the problem and the goal).

[15] Philology and the historical disciplines, as well as philosophy are usually counted among the "humanities".

[16] In this paper, it can remain undiscussed whether this conception is appropriate in the area of the natural sciences. Here, the only significant factor is that, in any case, rejection of this "realistic" intuition leads to a greater manifest difference between different disciplines or schools.

one another. With regard to the social sciences, the adverse effect can also appear of occasional unclarity in their disciplinary self-conception about whether they also want to raise normative claims or limit themselves to a purely explanatory perspective.

Nevertheless, the natural sciences, the social sciences, and the humanities each have their own certain collective prerequisites and convictions, and participants in cooperation with one of the other groups are always aware of this when they attempt to cooperate with members of a different “block”. If equal interdisciplinary cooperation, for example, between the humanities and the natural sciences, is aspired to, basal differences come into play and have to be overcome if there is to be any chance at all for horizontal cooperation.[17]

To this end, first and foremost, the differences in question between the disciplines must be recognized. Up to a certain degree, all the participants in this process have to become “multilingual” and learn how to adopt the different respective point of view in regard to the phenomenon and the problem. This is the only way that there is any chance at all of developing a collective platform, which means acquiring the ability to take up the perspective of the other discipline, but not having to absorb complete knowledge of these respective disciplines, of course. In the first place, that would be structurally overwhelming and secondly, it would make interdisciplinary cooperation superfluous. But it does mean understanding how to access the respective different discipline, comprehend its own character and—to express it like Wittgenstein—the esprit of this particular academic language game. Obviously, translation is usually only possible if a real-life formulation (in daily language) is found for the question and is accessible for all the participants and sufficiently theory-neutral (with overlapping interpretation as a collective starting point).

Excursus: The Special Case of Philosophy

In this difficult interdisciplinary concern, there are three reasons why philosophy is a special case. If the dimension of the history of ideas in philosophical argumentation is masked out, philosophy is characterized *first of all* by systematic access; which runs counter to the views of different disciplines in regard to phenomena and problems. The reason for this is that philosophy directs its attention towards concepts and forms of argumentation in order to describe phenomena or problems and attribute an explanation to them. It is clearly anything but self-explanatory or easy for non-philosophers to understand this philosophical perspective. As a result of this difficulty, philosophical considerations of this type are often dismissed as irrelevant. When, for example, philosophers consider certain terms to be problematic or certain patterns of argumentation to be inadmissible, this opinion is, for the most part, considered to be philosophically “interesting”, but seldom deters anyone from continuing to use exactly these terms or patterns of argumentation. In such cases, it seems as if others consider philosophical analysis and its presentation

[17] The same also holds true for the constellation of social sciences and natural sciences, however, or for the humanities and natural sciences.

of a problem to be irrelevant and without consequences for the actual questions or their own respective academic practice. *Secondly,* a special role is also attributed to philosophy in the context of interdisciplinary teamwork because its concentration on the tools of our way of thinking, i.e. discussion of the issue of concepts and forms of argumentation, enables it to recognize and discuss common ground and differences between the various disciplines to a very special extent.[18] With its methodology and analysis, philosophy seems to be the appropriate discipline for determination and awareness of the conceptual conditions for the possibility of interdisciplinary cooperation. Based on this, justification of the demand for philosophy to play a moderating function in the interdisciplinary context is understandable. However, this structural affinity must not lead to limiting philosophy to (i) the function of an academic-theoretical mediator, nor to (ii) narrow its role in the interdisciplinary concerns to this. Especially, however, this affinity must not lead to (iii) a representative of philosophy taking on the role of the director or producer in interdisciplinary cooperation.[19]

Admittedly, it is not easy for representatives of other disciplines to cope with the special case of philosophy because little in this discipline can be considered to be an indisputable consensus. For this reason, interdisciplinary teamwork with philosophy is usually characterized to a large extent by the concrete philosophical assumptions of the participating philosopher(s). Obviously, this openness is not always seen as an advantage from the point of view of the other cooperating disciplines. With regard to the differentiation between disciplines which are more application-oriented or—to use the terminology above—problem-solving and those which are primarily problem-analysing and phenomenon-analysing, it should be noted that this differentiation takes on a special significance in regard to the systematic interests of philosophy since its principled considerations are often considered irrelevant or even disruptive and adverse to practice. It is only when there is acceptance that appropriate analysis of a phenomenon and problem is an essential condition for ultimate adequate problem-solving that there is any willingness at all to recognize fundamental philosophical considerations as relevant.[20]

Thirdly and finally, philosophy is a special case due to the fundamental ethical questions posed; while their direct relevance is usually understandable, the

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[18] Naturally, this does not hold in every instance, nor is it ruled out that representatives of other disciplines also have this flexibility and competence for adoption of perspectives.

[19] For this would mean, namely, that representatives of the philosophical field would not be a part of the cooperating group; a similar, just as inadmissible result would occur if a representative of psychology questioned the contributions of the partners in the cooperation in regard to their psychological backgrounds or if a representative of sociology explained participants' actions in relation to their social backgrounds.

[20] There is a special problem between philosophy and the disciplines which did not emancipate themselves from it until the last century (for example, political science and sociology). Many different points of friction have resulted because, on the one hand, a demarcation setting them apart from philosophy still partially contributes strongly to the "identity" of these disciplines and, on the other hand, the issue of whether these disciplines are to take a purely explanatory or rather a normative direction has not been conclusively (nor uniformly) settled.

meta-ethical considerations used to answer them are, in contrast, often suspected to be irrelevant, as mentioned above. In respect to normative questions in general, this problem also arises for law and jurisprudence as soon as normative statements have been formulated. The same is true for economics, which also raises explicit normative claims depending on the respective self-conception of its representatives. Furthermore, the (also interdisciplinary) question is raised of how these types of normative claims relate to one another.

(b) Conditions for the Possibility of Vertical Cooperation

The conditions for the possibility of vertical cooperation make this form of cooperation considerably easier to implement because one source supplies the all guidelines for it; at least, this holds true as long as the constitutive rules and role assignments are clear and not revoked. Nevertheless, there are also pre-conditions for setting up a framework in the case of vertical cooperation. The three most important of these are:

- the questions and tasks have to make “sense” for the discipline surveyed
- the expectation for validity of the answers has to be reasonable
- the expectation of the demand for accuracy of the answers has to be realistic

Although these conditions may seem self-evident, some elucidations—in reverse order—should be made.[21]

There are often disappointments in interdisciplinary talks because the answers are too vague or the precision desired by the inquirer is absent. If, for example, simple answers are expected or desired for complex ethical problems, carefully weighed philosophical positions with many eventualities have a dissatisfying, evasive, or even arbitrary effect. Vice versa, scientists, and also economists, for example, occasionally face the problem that exact demarcations, threshold values, or developmental procedures which they cannot realistically deliver are expected from them—especially with regard to ethical questions.

Furthermore, the claim for validity accompanying answers given also has to be in compliance with the inquirer’s expectations. If, for example, the natural sciences can only make probability prognoses, the need for certainty is just as inappropriate as, for example, the need of a lot of non-philosophers for ethics to assert and substantiate the universal and categorical validity of their normative statements. Here, too, especially with regard to normative questions, it is true that philosophy renders an inconsistent picture. For, there are certainly representatives who claim such a universal and categorical validity for their philosophical ethics, whereas other philosophers are considerably more reserved with respect to the validity of normative statements.[22]

[21] These elucidations are also enlightening for transdisciplinary requirements.

[22] Divergent standards for dealing with probabilities and with the expectation of validity for claims and justifiability of ethical statements are a constantly bubbling source of misunderstanding, not only in the interdisciplinary context, but also in the transdisciplinary one; cf. Sect. 4.3.1.

For two reasons, the third condition necessary for the success of hierarchical cooperation is the most difficult to grasp. Its essence is that the question or task set for a different discipline does not imply incompatible or area-transcending categories or strategies of explanation. Anyone who asks a human geneticist about the gene for human dignity, for example, makes the same kind of error in terms of category as someone who demands a causal explanation of mental phenomena from a philosopher. Formulated in the opposite way, it means that the person posing a question for a different discipline in hope of an answer for helping solve the problem she herself has formulated has to have a certain understanding for the respective discipline asked, for its awareness of the problem, its methods, and its scope. Vice versa, it is, of course, very helpful when the cooperating disciplines also develop an understanding of the "actual" problem, i.e. of the problem as stipulated by the other discipline. Thus, analogous to horizontal cooperation, there is also the requirement for becoming multilingual within (narrow) limits in the area of vertical cooperation. Willingness for this and also mental flexibility for engaging oneself in the—usually long and arduous—process of taking a different perspective is indispensable for successful cooperation in interdisciplinary contexts.[23]

In summary: A pre-condition regarded as necessary for successful cooperation is clear understanding that the imputation of equality and claims to equality which are incorporated in horizontal cooperation cannot be fulfilled to the same extent or in the same manner in vertical cooperation. For, it is only under this prerequisite that disappointment and indignation resulting from a violation of implicitly set norms of horizontal cooperation can be avoided. Broadly speaking, there are three different cases of perceived norm violation: (i) violation of the norms of horizontal cooperation in a horizontal cooperation context; (ii) violation of the norms of vertical cooperation in a vertical cooperation context; and (iii) apparent violation of the norms of horizontal cooperation in a vertical cooperation context. The third case is explained by the differentiations suggested in this section, whereas the first two cases can at least be clearly articulated by these reflections.

Moreover, it is only under the prerequisite of more exact clarification of the respective structure of cooperation tailored for the individual factual issues that conflicts resulting from competing demands on responsibility or sovereignty of definition can be avoided.

The basic condition for interdisciplinary cooperation is the assumption of (and, if necessary, the claim for) mutual respect for the discipline and the expertise of its respective representative. This has to be required as a general imputation of trust and cannot just be established in the context of the collective work. It is more a matter of this imputation of trust proving itself and, through clear determination of the function and contribution of the individual disciplines to the respective problem

[23] It is exactly the process of acquiring an understanding of the problem of a different discipline that can be strenuous or even painful because (and so far as) it demands revision of one's own understanding of the problem.

definitions, not being destroyed by false expectations or even the inadmissible overstepping of boundaries.[24]

Furthermore, the willingness and the ability to take on a different perspective are indispensable for enabling discussion beyond the boundaries of a discipline and making answers from the individual disciplines compatible with other disciplines and thus able to be processed. Within horizontal cooperation, it has to be assured structurally and from the individual attitude that all the participants have a chance to take part in the collective definition of the problem. This comprises willingness on all sides to let one's own viewpoint be put into question by the, possibly at first irritating, perspective of the other disciplines and willingness to modify that perspective. At the same time, it has to be assured within the cooperation that this challenge belongs to the normal functioning of interdisciplinary teamwork and is not interpreted as an attack on responsibility and relevance. In this respect, it should be assured that the willingness necessary for self-examination and, if necessary, for modification is not assessed as an expression of uncertainly or even professional incompetence. Otherwise, it is to be expected that the impending danger of "losing face" would undermine the willingness for professional self-reflection and flexibility required for interdisciplinarity and lead to "rigid fronts", which would hamper productive cooperation or make it impossible.[25]

If action in interdisciplinary research is explained as a complex event of acknowledgement, the idea suggests itself of representing this phenomenon of interaction as a "struggle for recognition". This is consistent with the fact that exactly such incidents make implicit structures visible and thematic in most of the (functioning) cases and also that many parts of academics are still a culture of respect and honour—despite all the neo-liberal economization in this area. It would, however, be completely wrong to misunderstand this representation to the effect that the participants' openness and obligingness would not be just as important. It is rather a question of appropriately expressing the dialectics between this openness and flexibility, on the one hand, and a strong identity in one's own respective discipline (with strong confidence in one's own abilities), on the other hand. The latter is a pre-condition for involvement with the other disciplines, their points of view, and their formulations of a question without losing the sole justification for participation in interdisciplinary cooperation: the ability to contribute one's own discipline in a cooperative and problem-solving manner.

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[24] A differentiation must be made between this institutional trust and the personal trust of the participants in their cooperation partners. It is possible for these two dimensions of trust not to go hand in hand with one another, i.e. that a discipline is trusted, but not its concrete representatives; or, vice versa, that a person is trusted as a scholar although his discipline is met with structural distrust (the latter is only possible, however, up to a certain degree which is also determined by the internal plurality of the discipline in question). The relevance of personal trust as a factor not reducible to trust in the disciplines and the academic institutions also explains why interdisciplinary cooperation usually favours longer term and recurrent cooperation with the same participants.

[25] In my opinion, research results from the area of interprofessionality can be used fruitfully; cf. D'Amour & Oandasan (2005), Malin & Morrow (2007) and Molyneux (2001).

This attitude can perhaps be expressed as an interdisciplinary imperative in the following way:

> Act in such a way that everything you do is understood as a contribution towards solving the collective problem!

This means renouncing questions going beyond the scope or even demands for exactness (for example, for clarification of a philosophical term) if it does not result in an advantage for the collective problem and the objective agreed upon. However, it does allow further queries if one participant's action is not understood to be in the sense of this interdisciplinary imperative and also allows the request for this relevance to be demonstrated.

3.2.2 Virtues of Interdisciplinarity

Not all scholars are interested in interdisciplinary and/or transdisciplinary research work, and, obviously, not all of them are suitable for it. The following considerations refer only to scholars who, on their own initiative, are interested in this form of work and are not obliged to it by external constraints. In this connection, we can leave open whether it is a matter of intrinsic interest or rather a strategic one aroused, for example, by research-political framework conditions. The imputation is that both types of motivation are compatible with sufficiently virtuous pursuit of this activity. In other words: neither is the attitude that interdisciplinary work is an intrinsic value a condition necessary for qualification for it, nor would it be conceptually appropriate to impute that "intrinsic" and "strategic" cannot extensionally overlap one another.

It is obvious that de facto qualifications are not a necessary condition for having or developing an intrinsic or even a strategic interest in interdisciplinary and transdisciplinary research. Establishment of a balance between qualifications and interests may indeed generally be a central part of a successful (professional) life, but certainly not an essential condition for development or non-development of certain interests. Even prolonged or repeated experiences of failure are not a sufficient condition for determining such a fit if the significance of the goal unsuccessfully aspired for is esteemed very highly by the participant in question. This is the reason for the necessity of reflection on the abilities and attitude which determine the suitability of an individual for successful cooperation in interdisciplinary contexts. In the selection of the composition for an interdisciplinary research team, for example, an expression of interest thus should not and cannot be abandoned.

In the first section, it has been shown that central norms are implicitly included in the structure of horizontal cooperation and that these have to be explicated in order to prevent possible disturbances and friction. Beyond these norms, however, this section has additionally touched on claims in regard to the deportment and attitudes of the cooperating researchers which are laid out in the structural requirements for interdisciplinarity. Explanation of these implications for the individual virtues is thus a helpful project for optimization of interdisciplinary research.[26]

When, for example, the suitability of a person (and not of a discipline) for teamwork in an interdisciplinary project is considered, it quickly becomes apparent that some operative metaphors—for example, "looking beyond your own nose" or "taking in a broad horizon"—and a virtue-ethical vocabulary express and substantiate a candidate's suitability or unsuitability. These aspects can, of course, be overlaid by other considerations which, for example, can pertain to the composition of the group or to the contextual position of a certain researcher in her discipline or with regard to the problem to be treated. Despite all sympathy for successful cooperation, however, this certainly justified strategic point of view misses the point of the following question and also the—philosophical—problem lurking behind this question: What virtues enable a person to contribute her own discipline constructively in an interdisciplinary and transdisciplinary context? No compelling answer to this question will or need be found because very plausible answers can be given.

3.2.2.1 The Virtues of Interdisciplinary Work

In order to be able to be an active force in interdisciplinary contexts, a scholar has to be able to represent her own discipline confidently and with a clear, methodological self-conception. Contrary to a commonly accepted bias, applied or interdisciplinary research demand more, and not less, competence in one's own respective discipline.

As a general rule, the representatives of each discipline participating have to be involved in the interdisciplinary dialogue with awareness of the methodological and motivation-theoretical autonomy and characteristics specific to their own disciplines. Each participant also has to possess confidence in her own discipline because the required adoption of the perspective of a different discipline is not cognitively productive if knowledge regarding the central elements of one's own perspective (or even the perspective specific to one's own discipline on the whole) gets lost. Thus, a discipline can only make a contribution to successful interdisciplinary cooperation when its representatives bring the specific aspects of their own discipline into the collective work. This makes it quite clear that only representatives of a

[26] This thematic focus is compatible with a number of meta-ethical structures and thus does not specify one particular virtue-ethical conception. Furthermore, our approach implies neither that each of the virtues discussed in the following is exclusively relevant for interdisciplinarity, nor that there is one specific list that is only applicable for interdisciplinarity. The careful philosophical systematization undertaken in this section serves solely for explication of the pattern according to which, for example, we implicitly make selections for appointment to an interdisciplinary study group and thus for better orientation (and justification of such decisions). General academic virtues (such as truthfulness and probity) or even general epistemic virtues connected with "knowledge sharing", are thus required as part of the general framework, but not discussed separately here.

discipline who are reflexively conscious of the specific constitution of their own disciplinary perspective are worth considering for interdisciplinary cooperation.[27]

For this, evaluative self-confidence is also essential since cooperation requires not only mutual recognition as competent, relevant, and academically sound representatives of a discipline, but also respect for the respective disciplines as such. This also requires each representative of a discipline to bring along trust in her own discipline, in the sense of disciplinary ego-strength, and to claim and express fundamental argumentative equality outwardly, as well as for herself, and in a credible and sustainable manner. It is only on this basis that adoption of the perspective of a different discipline and willingness for taking on understanding of the problem and approach to a different disciplinary area suggested by different disciplines is able to take place without the loss of the specific identity of one's own perspective. The conditions for vertical cooperation require, furthermore, that the individual representatives of a discipline put themselves in the position of the leading or assisting discipline—according to the constellation of the problem—without undermining the evaluative self-image and the claim for fundamental equality. Flexibility in these roles and willingness to withdraw the perspective specific to one's own discipline in the process of adoption of an interdisciplinary perspective are thus not to be understood as an expression of disciplinary ego-weakness or even a crisis of disciplinary identity, but rather as competent flexibility grounded on a disciplinary identity which is self-confident (and which expresses this to the cooperating partners).[28] On the cognitive and evaluative levels, interdisciplinary cooperation thus permanently requires establishment of a harmonious balance between specialized identity and interdisciplinary flexibility. Although, analogous to one's own biographical life, such a successful balance can come about intuitively (for example, without an explicit and reflexive plan for one's life) and be managed with virtuosity, it stands to reason that the complexity of the interdisciplinary connections, along with the many possible (and factual) disturbances, will entail reflection upon this process on the part of the respective participants.[29]

Some disciplines obviously have to develop this capacity and able to discuss the issues internally to a greater extent than others; these are then especially the ones in a position to take on a mediating role in interdisciplinary teamwork. For philosophy,

[27] This means that interdisciplinary work usually cannot be a concern for novices in the discipline, i.e. for so-called junior researchers.

[28] Occasionally, a representative of one discipline identifies herself with a different discipline regarded as especially successful or influential. This can be paraphrased as a type of "identification with the aggressor" and demonstrates the representative's lack of disciplinary or evaluative self-confidence. The judgment handed down by the members of that representative's own discipline in such a case is that no colleague of theirs had been noticed, which brings home the point succinctly.

[29] This happens at the latest when the phase of accusations of blame has been surmounted and an analysis of the successful and especially unsuccessful cooperation takes its place.

this basically describes the permanent exercise of its constitutive ability for critical self-reflection of its own constitution in a specific context of application.[30]

This balance is not representable productively in an abstract way as a general structure, but rather has to be embellished in each specific context of the problem. For this, in addition to the structural conditions resulting from the questions prepared by the participants and the combination of disciplines participating, the personal factor also comes into play because there are obviously different individual kinds of temperaments and thus sorts of persons establishing this balance intra-psychically and in the group. Passion for one's own discipline and a sense of proportion in implementation, as Max Weber put it, are, however, indispensable. This is the only way to overcome bad dialectics between stubborn insistence on one's own perspective, on the one hand, and loss of autonomy possibly due to fascination by the different perspective or admiration of the possibilities of the other discipline, on the other hand, in favour of a hermeneutically productive dialogue.[31]

If interdisciplinary research is understood as work-sharing cooperation, as demonstrated here, it can be explained as a complex structure of recognition in which the norms of cooperation and the specific virtues of the participants who are able to assert themselves successfully in this cooperation and engage productively in interdisciplinary teamwork constitute the overall evaluative relations.[32] The structures presented in detail in the first part of this chapter are joined by the aspects of the individual participants which are represented here as virtues. The detailed remarks stated so far emphasize the significance of the virtues for interdisciplinary cooperation which are carefully systematized in the following list. It remains to be seen whether these virtues are specific to interdisciplinary research or whether they are virtues for academic activity which are practiced or required in a special way or to a special extent only in interdisciplinary cooperation. The reason for this is that our detailed remarks do not make the claim of being a factually justified systematic ethical theory in the form of "virtue-ethics of interdisciplinarity".[33]

[30] This structural affinity is another factor contributing to the special role of philosophy in interdisciplinary cooperation which has already been discussed.

[31] Such fascination can result from different sources (and always depends on the specifics of one's own discipline). Exemplary are such different points as the presentation of causal clarification, the possibility of technical intervention, the visualization of processes, the ordering of phenomena into a social or historical context, the pursuit of conceptual questions, or even the mathematisation of complex connections.

[32] "Evaluative" is used here as a generic term for ethical norms, virtues, and values.

[33] In order to avoid any misunderstanding, let us mention again that this does not result in any strict distinction between general abilities and virtues; the rough rule of thumb should be that participants value their abilities based on their virtues positively. There could be a narrower and more specific demand for appreciation of these abilities with regard to their function of enabling or improving interdisciplinary cooperation.

Our set of requirements for participants with the capacity to act successfully in interdisciplinary cooperation is divided into three groups:

A. General Virtues Not Only Specific to Interdisciplinary Cooperation

(A.1) The *intellectual* dimension includes openness to new points of view presented in a question from a different discipline and curiosity about this expansion of perspective. The latter also demands a certain capacity and willingness to suffer, for example, due to the necessity to adopt knowledge foreign to one's own discipline. Furthermore, interdisciplinary work demands creativity, for example, in the generation of new understanding of a problem, and a great, and usually also quick, capacity for perception, which is often used for situatively necessary adoption of knowledge alien to one's own discipline. Finally, let us include action-orientation, which is especially indispensable in cooperation directed at practical application or implementation and could be paraphrased colloquially as "drive towards the goal" or "product-oriented".

(A.2) The *social* dimension is a consequence of our context of interdisciplinary cooperation being a matter of considerable complexity. The capacity for empathy and adopting a new perspective (in regard to different disciplines, as well as to their individual representatives) is just as indispensable as flexibility, which, for example, is necessary for understanding or adopting the customary terminology of the other discipline.[34] Especially the beginning of cooperation requires patient listening and the capacity for compromise, which can, for example, consist in initial lowering the standards for clarity, evidence, and justification, as well as methodical standards (in regard to one's own discipline and to those of the other participants).

(A.3) Last of all, a *reflexive* component can be identified which includes cognitive self-confidence (in the sense of disciplinary self-insights), as well as evaluative self-confidence, i.e. knowledge of evaluative aspects of one's own disciplinary identity. This self-confidence should not function as a bulwark against the unreasonable demands of the "self-confidence" of the others (in other words: the disciplinary self-conceptions of the others), but rather as a point of departure for openness and exchange in the interdisciplinary work. This remains an indispensable basis, however, because without such a component, an independent contribution from the respective discipline would not be able to be performed, and it is a matter of exactly such independent contribution.[35]

[34] One of the greatest obstacles for interdisciplinary projects which should result in a collective publication is generally known to be reaching an agreement in regard to the manner of quoting.

[35] For a more extensive presentation of these dialectics from a philosophical perspective, cf. Quante (2011).

B. Trust
Division of work in any form presupposes trust; otherwise, the costs of controlling the cooperation partners would be too high and teamwork pointless (or at least inefficient).

(B.1) The following four aspects in the form of *trust in* belong to the basic configuration of any approach suitable for interdisciplinary cooperation: (i) trust in performance ability of one's own discipline and (ii) in one's own professional competence. Such trust is not expressed through a naïve omnipotence complex or self-suggestion of universal competence in the discipline, but rather through a certain disciplinary modesty and conscious self-limitation, i.e. manifestation through articulated knowledge of the limits of one's own performance ability.
Also belonging to trust with regard to the other participants is trust in (iii) the respect and recognition for one's own discipline, as well as (iv) respect and recognition for one's own professional competence by the representatives of other disciplines participating in the cooperation.[36]
Characterizations of a participant as "resting within herself" or "radiating a certain serenity" target at this basal trust in fulfilment of the conditions necessary for interdisciplinary cooperation.

(B.2) In the form of *trust toward*, two aspects can be differentiated: (i) trust in the performance ability of the participating disciplines and (ii) in the professional competence of their individual representatives.[37] These two requirements rely on the third and fourth aspects of *trust in*; in the end, this expresses the necessity for de facto existence of these aspects for longer-term stable cooperation in order for the structure of recognition to function.

C. Personal Respect
Personal respect means the capacity of a participant for expressing recognition of and—if necessary—claiming it from representatives of different disciplines and one's self (as a representative of one's own discipline) through one's own manner of action. This is *personal* respect in the double sense of it not being directed towards institutions (academia can be respected without respect for each of its representatives) and of it also not being expressed towards individuals solely because of their institutional roles (as an accused person possibly respects the judge due to his role and position).

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[36] One of the conditions for the possibility of interdisciplinary cooperation in the first place is that the disciplines and their professional representatives extending the invitation recognize one another respectively as competent and relevant participants for the problem. Any discourse in regard to this condition (or questioning of it) should thus also not be regarded as a move in the chess game of interdisciplinary cooperation, but rather belongs in the forecourt.

[37] The question of the extent to which rankings and other standards of the academic system can contribute to stabilization of this aspect of trust towards individual partners in cooperation (and are thus able to or should serve as criteria for the selection of participants) cannot be pursued here; cf. Sect. 2.3.

(C.1) *Self*-respect is an additional element in the reflexive-evaluative identity of the participants and is shown in the limits they set on flexibility and openness, as well as in the obligingness they claim in these points from the representatives of different disciplines.[38]

(C.2) Respect *towards others* is shown primarily by examining their contributions openly and constructively in respect to their relevance and their power problem-solving ability. It is also manifested in patience in interdisciplinary communication and creative willingness to become involved in a perspective alien to one's own discipline. Last of all, it probably manifests itself most clearly through taking the limits set by the self-respect of the other participants seriously and not simply brushing them aside as a *déformation professionnelle* or individual quirks.

3.2.2.2 And What About the Ethical Virtues of Transdisciplinarity?

Do these considerations on ethical virtues which are necessary or at least conducive for interdisciplinary cooperation also assist with regard to the question of what virtues those scholars who are working at the interfaces to society should bring along? As already stated, this external transdisciplinary contact does not necessarily require interdisciplinarity (or compulsorily provoke interdisciplinary endeavours). It does, however, often suggest that interdisciplinary cooperation targeted at practical implementation will also have a transdisciplinary component.[39]

It stands to reason that the situation "academics—society" is understood as an extension of the situation "specialized research—interdisciplinary context". Viewed in this light, it becomes obvious that the general virtues (area A) will also have relevance in the transdisciplinary context. The "breaking" of one's own respective disciplinary perspective and the flexible attitude in regard to divergent perspectives and needs connected with a certain problem required by interdisciplinarity is certainly helpful for the multifaceted breaks necessary in transdisciplinary communication. There is, however, a decisive difference: in interdisciplinary concerns, one can assume that all the participants feel fundamentally obliged to the ethos of scholarly action, whereas this framework is eliminated in the transdisciplinary context.

[38] The act of earning respect by insisting on the relevance of one's own discipline and also by claiming recognition (in the case of disregard) is, once again, a toy in the forecourt of interdisciplinary cooperation. The analogy to sports would, however, be misleading here because "respect" is also often "earned" there through overstepping rules, hardness, and foul play.

[39] The following is limited to the interface between academics-in-society, with no differentiation here of the different participants in "society". For the question of whether the so-called public, in whatever form, should participate actively in interdisciplinary cooperation (in whatever form), cf. Sect. 4.3.2.

In academics, shared fundamental understanding of the epistemic importance of probabilities, of the obligation for justification of one's own statements, and of the justification for demanding such substantiation may be assumed. Although the specific characteristics of coping with operational or epistemic risks, for example, may vary according to discipline and although the standards for substantiation and procedures may also vary according to discipline, there are still limits to this variability which scholars may not transgress. Moreover, the obligation to allow the arguments for one's own convictions to be scrutinized intersubjectively upon request is also essential (falsification maxim in academics). Even if this cannot rule out that scholars also occasionally do not comply de facto with this standard, this investigation is relevant since there is at least the justified possibility for demanding adherence to this standard (for example, by subjugation to peer reviews).

This standard inscribed to academic rationality is also accompanied by the obligation to become conscious of one's own assessments and partialities, to block them out as far as possible, or to justify them as evaluative (or normative) statements for their part. Academic integrity demands, even from its methods and its esprit, a certain measure of neutrality and reflective distance to one's own attitudes regarding norms and values.[40]

These pre-suppositions may not, however, simply be transferred from the area of academic concerns to the common social space, which is structured by issues, economic interests, and political power relations. As long as transdisciplinarity is practiced with discursive means and not just applied in a declamatory or demagogical way, assumption of a minimum level of rationality is self-evidently unavoidable. It would, however, be wrong for scholars in transdisciplinary deployment to introduce all the things which they justifiably claim in scholarly discourse as a standard of measurement under the heading of "rational discourse" (cf. also Quante 2012). It is exactly dealing with issues sensitively—possibly beginning with the emotive edge of some terminology—that requires sensitivity and patience extending far beyond the capacity for empathy required for interdisciplinarity.

Last, but not least, a very brief reference to two temptations closely connected with transdisciplinary concerns: power and money. Even if scholarly disciplines are able to claim the power of reason for themselves, the risk still exists of succumbing to the enticements of derivative power by (too) close alliance with political or economic forces which are in the position in society to assert their interests. Although it would be wrong to use ideological criticism to show distrust of power as such as a matter of principle in this paper or to consider the presence of interests in general to be reprehensible, it should be ensured that the voice of the scholar does not gamble away the only capital it really has: its independence.

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[40] This demand for neutrality, however, does not require denying a connection to purpose or goal in academic action in favour of strict detachment a "view-from-nowhere" although it will probably create gentle pressure in the direction of universality.

A scholar has to be able to resist these enticements and—at least sometimes—irrational obstacles without losing the capacity for communication with the diverse participants. It can and must remain open here what virtues are necessary in order to remain simultaneously upright and flexible and also what socialization processes and what institutional framework conditions of academics are able to promote this character profile. At the least, it has to be demanded that the economic situation of scholars and the spirit of their institutions not be of such a nature that they undermine this requirement profile.

3.3 The Organisation of Interdisciplinary Studies and Research

Stephan Lingner

Interdisciplinary cooperation is conducted at different professional levels, at distinct thematic sectors and with different intensity of integration depending upon the specific needs for interdisciplinary work and the individual cognitive interests of the participating scholars and—where necessary—of their funders. This section will thus start with a rough overview of different styles of interdisciplinary endeavours represented by selected institutions while attributing those to a "socio-cognitive" typology of interdisciplinary research frameworks established by Rossini and Porter (1979). This view will also include those post-graduate studies, which are combined with early interdisciplinary research.[41]

3.3.1 Landscaping

On the level of university colleges, there are many *graduate schools* worldwide which are already engaged in the interdisciplinary education and cross-disciplinary research of young graduates. The "GradSchools" directory[42] for instance indicates more than hundred interdisciplinary post-graduate study programmes globally—most of them in the Anglo-American world. Among them, three types might be distinguished:

1. The first type of schools addresses cognitive questions which need only marginal transgression of disciplinary borders. The "Gender, Society and

[41] For a detailed stocktaking of interdisciplinary research, its management and institutionalising, see Frodeman at al. (2012).

[42] http://www.gradschools.com/search-programs/campus-programs/interdisciplinary-studies/doctorate (on 5 Feb 2013).

Representation" programme of the University College London represents this type, extending from a still disciplinary core (in this case: social sciences).

2. The second type aims at problems which need a second (auxiliary) profession, like the curricula and research within the "Corporate Communication" programme of the University of Lugano (Italy). This type can be described as an extension of a key discipline (namely communication studies) towards another (business administration).
3. A third one claims even higher cross-disciplinarity between several disciplines with quite similar importance. This type is for example represented by the "Environmental Studies" programme of the York University (Canada), which encompasses the relevant natural and social sciences as well as humanities and arts. Similar approaches towards broad multidisciplinary integration are realised by the newly established Interdisciplinary Graduate School (IGS) at Nanyang Technological University (Singapore)[43] or by the Carl von Linde-Academy at the Technical University of Munich (for details see Mainzer 2011).

However, the tension between intended scientific excellence *and* multidisciplinary competence in one trained person is quite challenging and might disappoint high-flying expectations. Recall that nowadays the professional standing of any polymath would be constrained by the sheer mass of present scientific knowledge on one hand and by the limited resources for higher education on the other hand. Therefore, interdisciplinary post-graduate research is usually organised by joint efforts of several fellows from the relevant disciplines. The teams are normally coordinated by professors or experienced post-doc researchers and in thus, they often resemble Rossini and Porter's "integration by leader" concept. However, the unilateral epistemic dependence within this structure often adds only multi-disciplinary knowledge to the leader while the post-graduates stay and operate mainly within their disciplinary borders. This approach is thus applicable for syntheses from the comprehension of neighbouring disciplinary issues. Therefore, the claimed multidisciplinary competence-building will be limited to the leading researchers and will merely result in either less extend or poor depth of interdisciplinary reflection by them (Andersen 2012)—thus questioning the appropriateness of the described framework for the solution of *ambitious* and broad-ranged interdisciplinary questions, which might arise in specific contexts of scientific policy advice (see also Sect. 3.2 on virtues of interdisciplinary research in this volume).

Extended and professional interdisciplinary research may be therefore better based upon shared but individually located competences of outstanding scientists within those working group settings, which are adequate for the cognitive problem at stake. Room for corresponding professional research units is—for instance—given by the *Deutsche Forschungsgemeinschaft* (DFG) which is funding respective consortia within temporal research groups. Their related sub-projects represent the elements of a multilateral epistemic structure and are conducted by representatives

[43] http://igs.ntu.edu.sg/Pages/Home.aspx (on 5 Feb 2013).

[44] http://www.dfg.de/en/research_funding/programmes/list/index.jsp?id=FOR (on 28 Feb. 2013).

of the relevant (sub-) disciplines, which interact with each other towards a more integrated analysis quite similar to the "negotiation among experts" framework as described by Rossini and Porter (1979). However, DFG's actual record does only barely reveal extended interdisciplinary research in its research units so far[44] although there is no principle barrier against it. Other interdisciplinary strands beyond the DFG's perspective are represented by the so-called *ELSI-studies*, which reflect the ethical, legal and social implications of certain technology developments. Corresponding restricted deliberations are occasionally funded by different national or European programmes as adjoining efforts to larger technology projects. However, this approach allows only for poor interdisciplinary integration, which again may group them to the above mentioned "integration-by-leadership principle".

Another more focused cross-disciplinary exercise is realised by the *Intergovernmental Panel on Climate Change* (*IPCC*), which compiles (a) the global state of the art in climate science as well as (b) in related risk assessments for the society and (c) in options to act for (climate) policy makers, thus covering the whole field between climate research and climate protection. The three thematic clusters are arranged within respective huge IPCC "Working Groups", which aim at the regular compilation of corresponding updated reports over time. Their respective coherences are organized along internally approved outlines and by corresponding contributions to the single chapters by selected author collectives, which are coordinated by outstanding lead authors on different chapter levels and whose work are reviewed in progress, regularly. However, the comprehension of hundreds of contributions hardly allow for a real shared analysis neither for the reports of each "Working Group" nor for their superior synthesis. Especially the weak coherence between the three working groups is challenging because reflections on measures (Working Group 3) could better profit from the *current* scientific basis of climate change (Working Group 1)—which is however compiled *separately* at the same time and which is therefore not punctually available. Despite of the high reputation of the IPCC and of its regular findings, there have been also some more critics,[45] mainly about the somewhat arbitrary nomination procedure of the contributing authors and of the reviewing experts, which might even end up in conflicts of interests of responsible parties (Schellnhuber 2012a; EcoWorld 2007). Especially the structurally given and apparently intended influences of governments' into the review process of the draft reports and of the summary for policy makers are criticized from a scientific point of view (Schellnhuber 2012a), which might have already given reason for several shortcomings and in-coherences within some findings of the IPCC (Meyer and Petersen 2010). It seems that a more appropriate distribution of work between researchers and political stakeholders might thus improve IPCC's results even more (von Storch 2011).

Other more institutionalized deliberations are conducted by various *university-based or extra-facultative centres* as forums for interdisciplinary reflections on a broad spectrum of questions arising from the ambivalent relation between science,

[45] C.f. Beck et al. (2004).

technology and society. This holds true especially for those scientific academies, which were established in the tradition of Leibniz (Stock 2012). Institutional examples are the German National Academy of Science and Engineering (acatech) but also the Munich Center for Technology in Society or the Bielefeld Center for Interdisciplinary Research (ZiF), which aim at scientific-based policy advice. The institutions' common approach is the establishment of intimate and manageable project groups, which are capable to deal with quite different complex and demanding problems like the adequate handling of options for internet privacy, for e-mobility or for adaptation on climate variability. Corresponding cooperative working groups consist of renowned scientists and—in several cases—also of representatives from industrial practice or from the relevant stakeholders. Similar settings with specific focus on advisory on bioethical issues were furthermore realised by the Nuffield Council on Bioethics (UK), the German Ethics Council and the former US President's Council on Bioethics. The question, if and how to integrate non-scientists in interdisciplinary deliberation efforts, is handled quite differently within the respective institutions. Among them, the *European Academy* in Bad Neuenahr-Ahrweiler near Bonn pursues a straight experts-oriented approach of interdisciplinary research, which will be explained in the following sections as a showcase for interdisciplinary research.

3.3.2 Rational Technology Assessment as Institutionalized Interdisciplinary Deliberation

3.3.2.1 Introduction and General Features

The interdisciplinary concept of "rational technology assessment" with this notion (Grunwald 1998a) was established from 1996 on at the Europäische Akademie,[46] which is a non-profit organisation, carried by its partners, the German Federal State Rhineland-Palatinate and the German Aerospace Center (DLR). The academy "*is concerned with the scientific study of the consequences of scientific and technological advances for individuals and society, as well as for the natural environment. … As an independent scientific institution, the Europäische Akademie pursues a dialogue with the world of politics and society at large.*"[47] Within this scope and with regard to the social ambivalence of technological progress, the academy's studies are especially directed towards the chances and risks of scientific and technological advance in the modern society. They aim at developing reasonable

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[46] Full name: "Europäische Akademie zur Erforschung von Folgen wissenschaftlich-technischer Entwicklungen Bad Neuenahr-Ahrweiler GmbH". Since May 2014, the academy has been renamed as "EA European Academy of Technology and Innovation Assessment GmbH" due to its reframing.

[47] Wording as of the academy's former research programme "(http://www.ea-aw.org/fileadmin/downloads/Forschungsprogramme/FP_e_201110.pdf on 4 March 2013)".

strategies towards adequate incentives or regulations regarding either desirable goals or adverse effects of innovation. Typical objects of (rational) technology assessment belong to new technological options related to energy, mobility and health. The further design of the academy's working method is guided by several characteristics, described below.

- *Scientific reflection* (including normative sciences) on technology and society and the furnishing of related orientational knowledge aims at finding favourable and just ways for shaping technology.[48] Corresponding validity claims are delivered by organised scientific discourses, thus establishing procedures for *discursive rationality* (Gethmann 1995). This pragmatic rational approach might serve non-arbitrarity, universality and acceptability of resulting recommendations and subsequent decisions (Brown 1988; Rescher 1988). Namely, the realisation of procedural rationality and the transparency of processes as well as the orientation along weak validity claims (coherence of argumentation; accepted world-views; common weal considerations) might lead to sound and legitimate choices (Gethmann 2011). In this way, rational assessments and scientific policy advice can be seen as prerequisites for corresponding socially robust developments in democratically constituted societies (Gethmann 1998).
- The subject of rational TA clearly extends disciplinary borders and needs therefore for a reflective interdisciplinary approach. It has also a trans-disciplinary perspective, taking into account the specific socio–technological problems at stake and their desirable solution. The task can be accomplished by problem-specific and appropriately composed *working groups*, which have to be established at the academy on demand (working group principle).
- The horizon of corresponding science and technology assessments (TA) offers advice especially with respect to acting problems with medium-term over intergenerational to long-term perspectives. This horizon aims therefore at more general statements on shaping our scientific and technological culture with longer validity than on incremental or possibly short-lived recommendations thereupon. The "tragedy" of actual (but temporally variable) acceptance or preference claims of stakeholders (Grunwald 1998b) and the principle impossibility of (public) representation of the future will thus give hardly added value or legitimacy to extended public participation in assessments of long-term problems. This could be seen as an indirect argument for the *recruitment of experts* (experts' principle) within the academy's working contexts. The experts' principle is correlated with the a.m. rationality claim whose legitimacy is, however, in detail debated controversially (Gethmann 2001; Reuzel 2001).
- Finally, the above mentioned time horizon will allow the academy's working groups ample time for thorough analysis of the specific problems to be reflected instead of being forced to the formulation of ad-hoc statements with limited significance. Otherwise, the academy's perspective and its objective of giving

[48] In contrast to that, purely technical approaches towards rationality in normative questions (Bush 1945) might give way for unwanted scientific paternalism.

timely advice on emergent socio-technologic developments needs for punctual finalisation—and therefore for organisation of its work in terms of *temporary projects* (project principle). The project orientation of the academy denotes it as an "Arbeitsakademie" instead of those traditional scientific academies, which pursue more permanent research programmes with less practical relevance.

The academy's concept of interdisciplinary project groups continues the scientific practice of the former "Academy of Sciences and Technology in Berlin".[49] The interdisciplinary experts-oriented approach of the European Academy offers a specific feature within the differentiated landscape of European TA-methods, where it can be seen as a complementary element with regard to different problem contexts, aggregation levels of analysis and addressees of societal advice in questions of science and technology (Lingner 2010). This specifity includes also other typical —but not exclusive—characteristics of the rational technology assessment approach, which rely mostly on the following *philosophy-based competences*:

Rational technology assessment is aware of the bi-directional interrelatedness of *science and technology*. The latter means that scientific progress depends on technological innovation, too—in contrast to more common beliefs that fundamental research generally precedes technology development (see also Sect. 4.3.3.1 in this volume). Examples might be climate or genetic research, which are based upon sophisticated technological practises (numerical modelling or automated sequencing, respectively). Therefore, rational technology assessment reflects both, on the consequences of technology advance as such *and* on scientific but technology-driven research—and thus on derived but "constructed" and debatable world-views, scientists teach the society (Janich 1996).

Moreover, rational TA extends also to modern health issues, as technology use is also expanding into the *medical sector*. Corresponding concerns reach from risk assessments for patients under high-tech therapy to delicate moral considerations, e.g. with respect to end-of-life questions or those of just distribution of medical services. The moral dimension of technology use advocates *professional ethics* to be included as competence into most rational TA projects for better reflection and guidance in ambiguous problem cases. The participation of philosophers within the project groups will also enable to deal with the relevant *epistemological questions* of interdisciplinary research and with pertaining acting problems from a trans-disciplinary perspective. They will clarify *terminological problems* which might inhibit rational interdisciplinary discourses and will contribute to rational advice in "wicked" normative questions with respect to the means and aims of (potential) actors and stakeholders (for details see Sect. 4.1: Scientific policy advice).

[49] See preface of its research report "Arbeitsgruppe Umweltstandards" (1992). The academy's successor is the "Berlin-Brandenburg Academy of Sciences and Humanities" which ties on the tradition of its precursor.

Finally, the scope of rational technology assessment and similar methods aims at the anticipation of consequences of scientific and technological advance. This *prospective view* is directed towards the prevention of possible adverse side-effects respectively at incentives for desirable developments in order to contribute to coherent developments of technology and society and to efficient policies thereupon. However, the fulfilment of this objective is complicated by the problem of sound prognoses on the development of complex socio-technical systems, which is known as the "Collinridge dilemma" of governance (Collinridge 1980). The work with plausible future scenarios and more general recommendations for technology governance is therefore seen as appropriate here.

3.3.2.2 The Practise of Institutionalised Rational Technology Assessment

As described above, the endeavour of technology assessment is necessarily an interdisciplinary effort with a trans-disciplinary perspective w.r.t. relevant societal problems and demands. This has to be organized in a proper way in order to manage prevalent obstacles of interdisciplinary work, which for instance might suffer from the different disciplinary accustomed practises and epistemic-driven reluctances to co-operate, effectively (Löffler 2010; Miller et al. 2008). These limits are also accompanied by social factors (Löffler 2010; Andersen and Wagenknecht 2012). In the worst case these factors might lead to effectless discussions within (poorly arranged) interdisciplinary working groups (Blättel-Mink 2003), which has to be avoided for levering added value. Thus, core conditions for successful interdisciplinary enterprises have to be realized, beforehand. They are specified by Mittelstraß (2005) especially w.r.t. the necessity to hedge—but not to neutralize—the disciplinary perspective(s) and to "de-disciplinize" the concluding appraisals within interdisciplinary settings. The respective practice of the European Academy aims at these principles throughout the consecutive phases of its TA-projects, which are specified below (for more details see (Decker and Grunwald 2001; Decker 2007):

Preliminary phase: Within this phase, trans-disciplinary knowledge will be transferred to a scientific task, e.g., by picking-up *recent questions of the technology debate* and new technological prospects either with foreseeable or assumed incidental consequences for the society or even those desirable innovations, which suffer from inadequate framework conditions. For instance, current developments towards "synthetic biology" arouse hopes for advanced and better production of goods but also fears w.r.t. the creation of alien life-forms, which might become harmful for the society and its environment. Corresponding concerns gave reason for related technology assessments at the academy as well as at other similar institutions. This and other topics are explored by the scientific staff by the systematic study of the relevant media, by exchange of ideas and their discussion on conferences and with stakeholders, and—last but not least—by insight and experience from the staff's own life practise. Moreover, suitable TA-themes are also suggested or even prescribed by third parties (the public, relevant institutions, project funders, etc.). Once having identified a challenging TA-problem, a subject-specific staff scientist will be nominated who will be responsible to guide the upcoming project within its lifetime of 2–3 years. This time frame proved to be appropriate for the elaboration of reasonable but still up-to-date results. For initiating the project, the project coordinator will first have to specify the topic and to adapt its planned assessment w.r.t. the methodological, institutional and financial conditions of the academy. Further internal discussions and advice from the academy's independent board of experts will result either in a *preliminary programme of work*, in a grant application, or even in a piloting study—depending upon the specific framework conditions of the planned exercise or the expectations of the customers. On the basis of corresponding programme papers, the necessary core competences for the future interdisciplinary project can be identified, which precede the nomination of corresponding key experts from the relevant disciplines.

Nomination of experts: Initially, a multi-disciplinary *core group of experts* is established, who represent the main relevant disciplines or problem-specific fields within the project. Their task is—first of all—to specify the programme of work in more detail. This is a crucial step as it will set the course of the whole project. Thoughtful formulation of the "right" questions in this early phase benefits the project, especially w.r.t. its trans-disciplinary aspiration. Having once consolidated, the fine-tuned programme of work will allow recognizing further critical competences, which have to be added to the project, either by new members joining the working group or by written expertise of third parties to specific questions of the project—depending on corresponding demand for interdisciplinary reflection. The *consolidated project group* typically consists of five to ten members from the relevant disciplines; this number can be effectively managed by the project coordinator. This design is in-line with the a.m. experts' and rationality principles, which aim at the science system as one of the addressees of the TA-results.

However, the recruitment of experts is a sensitive step with respect to the success and validity conditions of a joint TA effort: Simonis (2001) blamed experts' approaches as elitist and far from daily life. This might be seen as a general problem for taking the trans-disciplinary perspective—although experts might not necessarily be scientists, if they bring-in the relevant problem-specific expertise or local knowledge. Nevertheless, recruited scientific experts are no alien species—they share similar circumstances and problems in daily life like "ordinary" people. Moreover, they are also lay-persons within interdisciplinary discourses beyond their own disciplinary limits and depend therefore on rules for and trust of rational argumentation, which have to be accepted by all members before. The former means also, that interdisciplinary reflection has to be conducted in everyday language in order to meet the goals of mutual comprehension. This claim complies also to the trans-disciplinary transfer of the final results, later-on. A more severe problem seems to be the proper selection of individual experts, who should not only be competent in their respective fields of work but whose obligations should not compromise overall results, e.g. by particular interests. Ideally they should (prove to) be independent with regard to the assessment context in order to enable *open-ended deliberations* on "wicked" questions on shaping technology and society, adequately. Alternatively, one might consider establishing a plurality of "lobbyists" within the working group with the aim of neutralizing internal or individual biases. Similarly, the constitution of some scientific heterodoxy might enrich the interdisciplinary reflection beyond the mainstream, provided blockades between the parties will not occur. This would allow for introducing new perspectives and for contesting of any extreme positions in these discourses.

Within this experts' model a *regular* participation of stakeholders is normally not foreseen—apart from public hearings on demand and depending upon the particular context, e.g., with respect to the better comprehension of the trans-disciplinary problem at stake. The main reason for this caution is the above mentioned aim for open-minded discussions free from compromising personal interests. Additionally, the long-term perspective of the academy's projects principally do not allow for a balanced representation of stakeholders as possibly affected future persons naturally cannot participate present reflections on emerging technologies.

Main project phase: This project phase starts with the specification of the programme of work and the agreement on the *extended outline* of the joint effort as well as on the distribution of topical main responsibilities within the group of experts. Then, each member compiles early drafts on the respective topics for their subsequent plenary discussion. The results of this *interdisciplinary reflection* will be implemented in the course of the further development of increasingly mature papers on the specific issues. For serving these aims, the entire working group will reconvene for further progress meetings on a regular basis about bi-monthly. These relatively *tight meeting cycles* correspond to the high efforts to be taken considering the complex and ambiguous questions at stake with regard to the aim of rational technology assessment. After about 2 years of continuous progress the preliminary and incoherent drafts will be collaboratively elaborated towards a comprehensive and integrated appraisal of the analysed problem in a *joint authorship* of the whole group.

In detail and within the course of the project course, three steps of the interdisciplinary discourse and assessment might be distinguished, which could also overlap somehow. (1) The analysis starts typically with a *stocktaking* and discussion of the problem issue from the respective disciplinary perspectives. The resulting multidisciplinary topology is naturally yet incoherent and barely critical with regard to the respective disciplinary point of views. (2) Thus, inconsistencies, redundancies and gaps have to be identified as well as specific notions of core terms for improvements and clarification beyond the disciplinary perspectives. The effort aims at the combination and *integration* of still disciplinary statements and views towards a comprehensive and interdisciplinary reflected draft. (3) The critical stocktaking and its interdisciplinary appraisal should finally open into *sound conclusions* and justified recommendations aiming at science-based guidance of the actors' levels. This step corresponds to the transfer of scientific weighed knowledge on chances and risks of new technologies to the societal addresses.

The joint interdisciplinary effort will be accompanied by several *quality management* measures, which will uncover any gaps and shortcomings in progress; they might also seed new ideas into the assessment process. Corresponding feedbacks of the state-of-the-art can be organized at different project stages by a cascade of evaluation workshops with external experts and within dedicated sessions of public conferences.

Final phase: The preliminary project results will be finally evaluated by the academy's independent and multidisciplinary *board of experts*. Its vote will also give advice, which specific publication strategy should be considered for the proper *dissemination of results*. Depending thereupon, the conclusions will be presented by public hearings, broadcast contributions and—after final editing—by publication in written form (books, journal articles, memoranda, press releases etc.), both, in print and electronic media. The diversity of publications aims at meeting the relevant but different addressees of the studies, named below. This goal is also fulfilled by further publications of the involved group members even in post-project phases as well as by monitoring of the project's impacts by the respective co-ordinators.

The addressed audiences of rational technology assessments belong to three major groups:

(1) Important recipients of the assessments are the members and boards of the *science system* itself as responsible actors in the earliest parts of the value chain. In this way, TA contributes to the self-governance of the science system. This holds true—for instance—for the case of research on synthetic biology as an emerging technology, which might pose incalculable risks to the society and its environment. Rational technology assessment aims thus at reflection, incentives and rules on and for *early* techno-scientific developments *before* society becomes locked-in into possibly undesirable and irreversible innovations. The academy's science-based approach corresponds to the specific communication demands of the recipients and the necessary transfer of appropriate knowledge beyond disciplinary borders. In this respect, the institutionalised concept of rational technology assessment offers a unique selling point within the European TA-landscape (Lingner 2010).

(2) Moreover, rational technology assessment aims also at (scientific) *policy advice* with specific focus on the relevant ministries and on administrative and parliamentary bodies, which are also targets of other competing TA methodologies. However and different from these, the long-term perspective of rational technology assessment aims rather at principle evaluation of foreseeable socio-technological developments and at related recommendations for acceptable strategies or frameworks than for specific, local or ad-hoc suggestions for daily politics. In this way, rational TA is related to practise but not necessarily to direct implementation (Gethmann 1998). Another specific of this TA-approach and its trans-disciplinary target is its *European perspective* regarding the consequences of scientific and technological advance and their assessment needs. The trans-boundary dimension of the societal effects of most developments as well as the growing dispense of national competences to the European authorities give reason for unfolding this perspective on the continent's level. Therefore, science and technology-related choices are more and more made on European levels thus depending increasingly

on advice with a supra-national perspective. Correspondingly, rational TA has to offer its service on related levels. (3) Finally, rational technology assessment aims at information and transfer of critical knowledge to (and from) the interested public and to the *stakeholders* in recent or upcoming conflict situations on new technologies in society.

The crucial point of any assessment is to avoid biases, e.g. due to interest-based experts' dilemmas and resulting subjectivity of evaluation. However, objective advice instead—although generally desirable—actually cannot really be offered, because evaluators are basically bound to their individual and inevitably subjective perspective. After all, the concept of organised experts' discourses within rational technology assessments will result in *inter-subjective statements* on technology questions. This inter-subjectivity is particularly achieved by the necessity to align individual beliefs to be contested within the reflection processes of the interdisciplinary working groups. In the end, the joint efforts aim at consensual results. However, even intensive discussion cannot easily resolve every upcoming dissent. In those cases, where these major dissents sustain, dissenting or conflicting votes have to be made explicit within the final conclusions—thus making the degree of subjectivity of results at least transparent to the addressees—which would still give valuable insights.

Chapter 4
Trans-Disciplinary Deliberation

4.1 Policy Advice

Georg Kamp

4.1.1 Preliminary Remarks

Interdisciplinary scientific and scholarly work, in particular when it is occasioned by transdisciplinary goals, is largely aimed at advisory services: When technical or social developments are not just passively tolerated, not simply accepted as 'the way things go', rather when they should be actively shaped so that the desired consequences are most likely to occur, and the unwelcome consequences most likely avoided, then the need for professional and interdisciplinary advice increasingly arises in the question of the means that are to be taken. Thereby, it is a basic topos that advice-receiving clients emphasize as well as advice providers when they deal with their advisory services that good advising is a service that as smoothly as possible supports the client in the achievement of the goals *he* has set—according to his own preferences.[1] Advising that arouses the suspicion of being guided by interests or ideology is criticised. Especially if advisory services involve services for public institutions and decisions in the interest of society overall, then it is clearly important to prevent advising from becoming an instrument for arbitrary or spontaneous implementation of certain interests or ideologies (although of course people who bow to the idol consider their ideologies 'inherent constraints'). Even those who feel 'obligated to the public

[1] Cf. as one and possibly the most well-known example Pielke's characterisation of the scientific advisor as an "honest broker" (2007, p. 2): "The defining characteristic of the honest broker of policy alternatives is an effort to expand (or at least clarify) the scope of choice for decision-making in a way that allows for the decision-maker to reduce choice *based on his or her own preferences and values*." (italics G. K.).

C.F. Gethmann et al., *Interdisciplinary Research and Trans-disciplinary Validity Claims*,
Ethics of Science and Technology Assessment 43,
DOI 10.1007/978-3-319-11400-2_4

interest' do not necessarily know what *the* public interest is—and it is not even certain whether there is *the* public interest (in the sense of the singular *volonté general*) at all when it comes to decision-making situations that require advising. Rather, it is expected that with complex and disparate social interests, the preferences must be determined first of all—and it is important to avoid one-sided and biased input in the guise of advising during the process. From the legitimate efforts to prevent undesirable influence, the conclusion is easily drawn, however, that the adviser is to limit himself to the developing or discovering and factual/objective presentation of the *means* and *options*, while it is solely the client's responsibility to set the *purposes*.[2] The latter is indeed indisputably the client's privilege—and that will not be disputed by any means in the following: Wherever societal advising by scientists or scholars may be necessary, it is 'the society' or 'the sovereign' who autonomously decides its purposes, and 'the science' as less as 'the humanities' should not have any influence on this. On the contrary, the starting point is explicitly based on the premise that some 'higher wisdom' on the part of science and humanities with respect to the 'right' purposes is not to be expected (this premise also explicitly includes philosophy and ethics). But it is a fallacy if the sole decision-making authority of the recipient of the scientific advising is used to derive an understanding of the adviser's role that assigns him a purely receptive relationship to the purposes and specifies the announced purposes as a framework for action within which he is to conduct research on the best means, but whose boundaries he is not to exceed. And it is the result of a misunderstanding when one perhaps expects support from the social sciences in the collection of the purposes of the sovereign acting as the social collective, but otherwise dismisses every critical review and commenting of the collected data with reference to the 'value-free' approach of science and humanities (Gethmann 1998, p. 3f; Grunwald 1998, p. 29ff). Precisely because advising feeds into *recommendations* for action, which the addressee can accept or reject, the adviser *can* also critically address the purposes of the client being advised without questioning the decision sovereignty of the client.[3] And the normative thesis defended in the following is: Scientific advising *should* also do this—just not on the basis of a higher purpose-competency, but rather because in optimised social decision-making with a division of work to a certain extend it can—not alone and not uncontrolled by others—assume the task of checking the promulgated purposes against the standards of reason and placing beside the 'I want' a reflected and—concerning the prevailing circumstances—enlightened 'one should'.

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[2] That is e.g. how it is determined in the German *Act on the formation of economic expert councils for the assessment of the economic situation*: "The group of experts is to illustrate mistakes and ways to avoid or eliminate them, but not to make any recommendations for certain economic or social policy measures." (*Gesetz über die Bildung eines Sachverständigenrates zur Begutachtung der gesamtwirtschaftlichen Lage*, Sect. 2; own translation).

[3] This also corresponds to the position of Max Weber that is often misunderstood and presented in an oversimplified manner: "[Scientific] criticism is not to be suspended in the presence of value-judgments" (Weber 1949, p. 52), but its sources do not allow for the determination of "binding norms and ideals from which directives for immediate practical activity can be derived" (ibid.). Stated more pointedly, that means: '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.' (ibid.: 54).

Because a realistic determination of the objectives is also always dependent on opening paths and because there is usually more than one path that leads to specific objectives, a form of interdisciplinary preparation of advisory services is now the rule for the complex, scientific advising on critical questions of societal relevance.

In order firstly to define the action possibilities and limits of scientific advising; in order secondly to check what interdisciplinary advising can do beyond pure professional advising; and in order thirdly to develop quality criteria for scientific advising services, it will be helpful to regard advising practice not only in structural, systematic or institutional terms, but also as what it first and foremost is: (linguistic) action. And because everything in advising feeds into recommendations or proposals, it is worthwhile to begin with a more detailed examination of these forms of speech acts.

4.1.2 Recommendations and Proposals: Means to an End

He who acts has an influence on the course of things—by interfering or changing them, or by abstaining from such interference. Since this kind of intervention usually has a number of consequences and an actor usually pursues a number of purposes (each with varying degrees of intensity), one assumes a gradual understanding of the success: Success mostly only occurs more or less. All the consequences of his action are never completely known to the actor, and the risks of unintended consequences can never be completely avoided. This applies first of all for reasons of principle: Consequences are those matters that exist because an actor has acted this way and not otherwise. And because this is the case without temporal limits, the space for consequences is also unlimited in principle and cannot be regarded in full by the actor. Secondly, it is also sensible to use certain relevance criteria to limit the sufficient certainty of action for pragmatic planning reasons: The ongoing optimisation of the level of information already requires quick resources whose use must appear inappropriate to the rational actor when considering all the purposes. And if there are references to risks of action, then the avoidance of these risks usually requires measures (the execution of or refraining from actions), which in turn trigger subsequent processes that the actor cannot regard in full. He who wants to act will do so as a rule with more or less good, but not with full information about the consequences. In some cases, the information allows for an estimate of probabilities, but fundamentally areas of uncertainty will remain. Action is—along with the known consequence—fundamentally subject to risks.

The assessment of technology consequences has endeavoured to illuminate the consequences of technical action with societal relevance in a more systematic and deeper way, and to make recommendations on how technology can be designed so that it makes as achievable as possible the purposes for which it was made, without preventing the future achievement of other purposes—whether they are the purposes of the actor himself or those of others. With all the complexity of the matter and all the nuances in the formulations, such recommendations always at the end provide answers to questions of the type: 'What should I do?'—regardless of whether the question is so or different and also regardless of whether it has been explicitly asked at all. Because, of course, he who wants to give a recommendation doesn't always have to wait for the 'utterance' of the question, but can also offer support to the person to whom he attributes the problem for good reason.

The most general form of the statement that can be used for a recommendation may be roughly formulated as 'If you want this and that to be the case, then you should do so and so' or—in the more precise and also generalizing and philosophical tradition of discourse theory—as follows:

> If X wants to achieve the purpose P_1 … P_n, then X should undertake action A* with instrument I* in situation S*.

'A*', 'I*' and 'S*' thereby mean actions, instruments and situations of a certain type, that is determined by a specific, more or less complex set of properties to be explained in more detail in the actual recommendation (for example: If X wants the bookshelf to be fixed (P_1) and the board is rather hard (S*), she should drill screw holes (A*) with an electric power drill (I*)). Other parameters may be added, such as those for required materials, including cooperation partners, specific modes of action and other aspects, with the achievement of the set purposes being made dependent on them.

This is not the place for an extensive theoretical reconstruction of all the conditions for success with recommendations in discourse action. A few basic building blocks, however, are readily at hand: A recommendation may be considered *correct* if the purposes addressed in the 'if'-part of the recommendation proposal suit the addressee, i.e. were attributed justifiably and if the means identified in the 'then'-part prove to be suitable in the given situation for the achievement of these purposes. However, the *addressee* of a recommendation will not be able to or not want to check the correctness of this as e.g. a scientist checks his hypotheses: by producing appropriate situational conditions and taking the appropriate action with the specified instruments and observing whether the conditions occur that he formulated as the purpose or not. Not only is the recipient of the recommendation, if these conditions do not occur, literally against his own purposes. As long as he himself deals with the consequences of his actions, takes responsibility for them and does not dump this on the author of the recommendation (or e.g. his insurance), he will hardly risk such 'proof is in the pudding'.

The same applies *a fortiori* for the review of the *quality* of a recommendation: Not always and more as an exception is it to be expected that precisely just one means is suitable for realizing a particular purpose. Often the agent has a whole range of possible options for achieving the conditions that the agent named as his purposes, frequently with the choice of the means and the level of certainty of achieving the objectives varying. The choice of different means often comes with different future consequences, some of which are thoroughly desirable in that they make it possible for him to achieve different purposes, while others of which are less desirable in that they oppose his purposes—and depending on this, different actions may appear more or less advantageous to him. Sometimes it makes a difference whether an action is taken sooner or later, in this or that situation, whether one executes it with this or that instrument, etc. The degree to which the choice of a means M makes the purposes possible for actor X is commonly described as benefit or utility (from M for X), the degree to which the choice of a means is opposed to the achievement of its purposes is described as the costs (of M for X). Put crudely and in general, one can say: The lower the cost of a recommended means, the higher the benefits, the better (the higher the quality of) the recommendation. He who asks for recommendations then has the expertise for the purposes in question and can thus check the *relevance* of a recommendation—that only requires that the recommendation is given in a manner that explicitly specifies the purposes in the 'if'-part the means in the 'then'-part are recommended for. But the *quality* of a recommendation, in how far and at what costs the named means will be useful for his purposes—that he cannot (or for reasons of efficiency does not want to) assess. Accordingly, his reaction to recommendations given by others cannot depend on judgement competencies that focus on the means-end relationships provided in the explanation of the recommendation alone. Rather, the addressee of a recommendation in deciding as to whether or not he will follow the recommendation must rely on some other indicators that serve as a proxy. Clues such as the reputation or authority of the author, maybe the majority owners among those who affirm or dissuade him, may help him with specific procedures or institutional 'filters'—but in the end he will make his decision as to whether or not he wants to follow a recommendation based on trust.

This trust is all the more important as the actor looking for a recommendation usually is only too conscious (at least he should be) that giving a recommendation is an action that should also satisfy *criteria of success* for the recommending author: Of course, the author of a recommendation also pursues *his* purposes and hopes that his action primarily brings about what *he* wants to be the case—so terminologically we speak of the *success* of a recommendation if the author's purposes are reached. So for a professional adviser who earns his living from making recommendations for action, the achievement of the income necessary for this may be the purpose of his recommendation action and the receipt of payment in his account the decisive criterion for its success. Accordingly, the correctness and the success of recommendations are initially completely independent of each other in life; correct recommendations do not ensure success for the author, and also with incorrect recommendations he can be successful.

A variant of the recommendation with its own criteria for success is the proposing, which differs from other forms of recommending in that the one who provides the recommendation is simultaneously part of a collective that jointly considers how it should act, and on which he directs his recommendation. The yardstick for the applicability of proposals therefore consists of the purpose of the involved actors, with 'you should' being replaced by 'we should':

> If we want to achieve purposes $P_1 \ldots P_n$, then we should undertake action A* with instrument I* in situation S*.

It is not mandatory for the purposes of the involved actors to be harmoniously coordinated into a coherent and consistent purpose map and to guide the actors toward uniform purposes in a conflict-free way, where they only consider the right means. Rather, the proposal of an actor may aim at the ideal achievement of his own purposes and recommend means that are entirely opposed to the purposes of the other. Of course, here too it is necessary to check the recommendations, and if the joint action is organized with a division of labour, the decision will usually have to depend on others and on the knowledge of the means-end relationships addressed in the formulation. He who wants *his* success as the proposal author here, however, will have to design his proposals so that they have the prospect of being accepted by other actors by aiming at the collective choice of such means that have the prospect of *also* being accepted by others because they make it possible to achieve not only his goals, but also the goals of others to an extent sufficient for approval.

But precisely then, when it is necessary to coordinate a choice of the means with each other in order to pursue the purposes jointly in action, it is important to accept not one's own purposes, but rather those of the other(s) as given. It is more promising to address the purposes and coordinate jointly in collective advising so that it is possible to achieve the greatest possible (i.e. usually relatively high) benefits for all participants from a cooperative use of the means for coordinated purposes. The gathering of purposes in conversation and their coordination with a view to considering possible joint actions and the expectable consequences is called *conferring* (with each other). But as the purposes can become the subject in this reflexive form of advising, this may also be the case where non-reflexive advising takes place, i.e. where the 'you should' and not 'we should' is asked, where the author and the addressee of a recommendation do not coordinate joint action, but rather assume completely separate roles: The recommendations that such advising aims at are, however, solely determined to be correct in the purposes set by the addressee. This does not mean, however, that his benefits could not increase through advising, and thus the quality of a recommendation could also depend on the fact that previously in advising his purposes could be made the object.

4.1.3 Advising also Addresses Purposes

If the correctness of a recommendation depends on whether the purposes addressed in its 'if'-part could be justifiably ascribed to the addressee and whether the means named in its 'then'-part are demonstrably suited for the achievement of the purpose, then the correctness of a recommendation does not yet guarantee its quality. On the one hand, the designated means as compared to the alternative available means may prove to be less than optimal, for example. On the other, the designated purposes may be correctly assigned and the recommendation relevant, but the designated means may prove to be unbeneficial with regard to other purposes. The completeness of the gathering of purposes is fundamentally to be assumed as little as the consistency of all the purposes correctly ascribed to an addressee—this applies to individual actors and to corporate or even collective ones. It is virtually a part of the *condition humaine* that the focus on decisions that are directly pending shifts certain purposes into the foreground and that others end up in the shadows of the attention cone—the attention to one's own purposes is virtually domain-specific,[4] some purposes can be ascribed justifiably to an actor, even if this person is not currently aware of his purposes or permanently not aware of them. Such *latent* setting of purposes[5] has not only been the subject of the psychological and psychiatric practice since Freud, it is the object of every qualified advising practice, from simple sales pitch to complex policy advising. Since politics, according to Max Weber's famous dictum, is to be compared to 'politics means strong, slow drilling through hard boards' (1919, p. 66/2008, p. 207) it might be allowed to illustrate the complex case of policy advising by an everyday example that could happen in any hardware store—or even better, as here as so often the abnormal will throw light on the normal (Austin 1957, p. 6), that could not or at least should not happen:

> *Client:* I must make a few holes in a pair of hard boards and urgently need a power drill for this.
>
> *Seller:* Are these all of your purposes?
>
> *Client:* Yes.
>
> *Seller:* I would recommend that you purchase this model—it will let you drill perfect holes easily. Furthermore, we earn the most on it. It is our most expensive product. It has a shelf-life of approximately 100 drill holes and must then be disposed of as hazardous waste. Furthermore, by the way, the maintenance costs are particularly high because the carbon rods wear out so quickly. However, we can always deliver a replacement within a few weeks. Unfortunately, a repair service has not been set up yet. If the machine does not allow for tiring work due to its unergonomic body cladding, then at least the danger of getting a deadly shock is relatively low because the machine only runs at 110 volts.—So you'd have to go to Japan, for example, to drill your holes. If you go right now, you may get lucky and be in time to catch the cherry blossoms.

Although the customer will be likely to view this purchase recommendation as unfavourable and will presumably not follow it, one cannot ascribe that to the lack of correctness in the recommendation for the announced purpose: The recommended means of drilling the holes with the suggested machine is suited for this purpose according to the seller's information. And certainly the seller could have legitimately recommended any of his other models for the client's explicitly stated 'only' purpose. The recommendation was therefore quite correct—but it will certainly not be successful and motivate the customers to make a purchase (supposed this was the purpose of the seller). Rather, the seller's 'excessive' explanations

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[4] The current behavioural-economics research knows and confirms numerous examples and differentiates types of such domain-specific purpose absolutising. Cf. Kahneman (2003), Ariely (2008) and other texts that address the phenomena of 'bounded rationality' (e.g. tunnel vision, framing, binding of cognitive resources to an issue, methodism, group-think, etc.).

[5] Cf., here and in the following, the considerations about the "opacity of our need structure" in Tenbruck 1972, p. 24ff.

show the customer what purposes he also has in addition to the explicitly named ones, i.e. what ‘concealed’, ‘latent’ purposes are opposed to the purchase and the use of precisely this machine.

Latent purposes are purposes that a person P (here the addressee of the recommendation) has in the same way as manifest ones—but not in the sense that he himself is aware of them or that one would, given that sufficiently sophisticated technology was available, be able to read their presence from the physical data of his brain. That X has a certain purpose rather means that one can justifiably ascribe it to him according to the established standards of a practice,[6] partially by observing his behaviour and drawing conclusions about his inclination to prefer the one state of affair against the other, and partly by extrapolating the existence of other purposes from the manifestation of specific purposes, since purposes are not independent of one another, can be mutually dependent or exclude each other. And there are always some purposes we can ascribe to a certain X that he isn’t aware of now, that he sets aside in his actual mood, in a certain hormonal state or after having a few drinks, but that will be manifest—action-leading and behaviour-determining—tomorrow again. We may even ascribe purposes that are just latent as long as they become manifest in case of emergency or when one is miserable. From its very beginning, the efforts of philosophical reflection on agency (action) have been systematically to make the plurality of our purposes compatible and consistent with each other, to reconstruct the orders and hierarchies of purposes that determine our decisions, and to complete these orders (according to developing criteria) so that the agent does not pursuit his manifest purposes at the expense of purely latent ones.[7] Actions that are taken in consideration of such whole critically reconstructed and circumspectively completed orders of purposes everyday language as well as philosophical terminology calls ‘reasonable’. The reason that determines such actions, that allows us to act that way and that often is taken for the *differentia specifica* of humankind, Kant defined as the “Vermögen der Prinzipien” (‘the faculty of principles’; AA IV, p. 192), that allows for generalised judgements. It is not a special organ or a state of our brain, but rather a potential that is provided by our brains and that we detect as an ability to behave in a certain manner, the ability to grasp things conceptually, to make them comparable, and to relate them and organise them in relation to each other. Such abilities, however, are handled in language and require language; the order abilities are to be checked intersubjectively, even if it involves the ordering of subjective objects such as the purposes set

[6] H.L.A. Hart's concept of ‘Ascription’ (Hart 1948/1949) shows that positive criteria are not always required for the attribution of non-manifest properties, but rather a practice of mutual attribution is functionable and maybe even more functionable when the attribution is handled temporarily according to certain evidence and virtually until the opposite has been proved. The practice described by him in the example of the attribution of ‘guilt’ and ‘responsibility’ is more similar to the objection proceedings in court than the scientific justification model. *Cf.* Kamp (2005).

[7] Especially Aristotle’s *Nicomachean Ethics*, the philosophy of Epicurus and the Stoa are famous for this approach.

by an actor or actor collective.[8] The setting of purposes is therefore accessible to external and also scientific advising—not by placing all the purposes of an actor up for discussion, but rather by also being able, when he declares a purpose, to assign other purposes than the declared ones and having good reasons for this. A decision that the decision-maker also assesses as worthwhile when the consequences of the action have occurred requires not only an overview of the anticipated consequences, but rather also the fullest possible collection of the relevant (manifest and latent) purposes as well as their consistent classification in a viable preference system that also appears reasonable after the consideration (Schwemmer 1980). Should the decision-maker not continue to rely on himself in these tasks (and nothing says that he must continue to rely solely on himself) and should recommendations also be offered in view of a trained and declared 'preference ordering competence' with respect to the relevant factual connections such as how the actor should organise his purpose map, then the actor is offered advising, which also makes the collection and ordering of his purposes its object. This need is all the more urgent if the actor is not an individual, but rather a collective and if an occurring incompatibility of purposes is not merely a incoherence in individual preference systems, but rather a conflict between authors that set different purposes with the same authority.

Therefore, according to the theory, advising should not only elevate the factual purposes declared by the person seeking a recommendation and research the most suitable means for them. Rather, advising should also discuss the purposes of the addressee themselves and make them the subject of a critical-analytical discussion. The goal is not to give him purposes that he should have, that everyone must have or that everyone has who wants to be considered reasonable, intelligent, moral, Christian etc. The goal should rather be explicitly to address those purposes that the addressee should make a subject in his planning 'insofar as reason has decisive influence on his actions', as Kant (2002, p. 34) puts it: "Whoever wills the end, also wills […] the means that are indispensably necessary to it that are in his control ("Wer den Zweck will, will (sofern die Vernunft auf seine Handlungen entscheidenden Einfluß hat) auch das dazu unentbehrlich nothwendige Mittel, das in seiner Gewalt ist"; AA IV 417). And if action is required for the production or maintenance of suitable conditions, then he will—if the consideration makes the connected investments appear worthwhile—want this action as the 'indispensable necessary means' for the achievement of his purpose. In other words: the purposes that an individual sets are not independent of each other—and where the surrounding conditions only permit a limited number of means for the achievement of an 'end purpose' P_E, then it is possible to identify the derived purposes $P_1, \ldots, P_n$ which may be legitimately attributed to the ones that pursue P_E—even if the purpose-setting actor himself, whether due to a lack of conviction or because he not see factual connections, does not recognize $P_1, \ldots, P_n$ explicitly as his purposes. In part, such derived purposes are specifically for an end purpose kept in sight; in part,

[8] Cf. the so-called "private language argument" in L. Wittgenstein (1953/2001, pp. 244–271).

it is possible to derive the purposes that may always be attributed if an actor pursues purposes at all. It is also possible to justifiably attribute purposes that an actor should pursue *today if* he determines an (end) purpose in the near or distant future, and part of correct current advisory service suggests that the conditions for achieving a *future* purpose may be endangered by using certain means in the pursuit of a *current* purpose. Here reference should be made to the sustainability debate as an example:

Critically reconstructed and circumspectively completed orders of purposes (or preference systems, as decision theory calls them) show the willingness of the agent to categorise purposes into higher and lower ones in relation to each other and to sacrifice one purpose for the benefit of another in the event of incompatibility. As a result, preference systems—at least from the contemporary perspective of the agent —also represent an ordering of the anticipated permanence of the set purposes. The collection of a contemporary preference system allows for conclusions to be drawn on the likely future preference system and the future purposes.—In such a theoretical framework, it is possible to reconstruct the central moment of the often nebulous sustainability discourse in an informal way: An action is sustainable when its intended and unintended consequences at least do not threaten the conditions for the future realisation of higher-order preferences (cf. de Haan et al. 2008).

Insofar as any future planning depends on the reliable availability of the resources necessary for the success of the future action, sustainable action includes the longer-term securing of the resource availability required for the future realisation of the purposes, according to this reconstruction. In addition to the resources required for future action in the strict sense (e.g. materials, instruments), it is also necessary to retain the availability of and develop the cognitive resources of proven intervention knowledge (e.g. technical know-how)—which cannot be ensured without learning, research and development activities. Sustainable action is also primarily aimed at the retention of the ability to act in general (e.g. the avoidance of health risks) and the securing of the planability of future action itself (e.g. by retaining essential, natural cycles and social or political stability). A future that can at least in part reliably planned is an essential condition for the readiness to invest in current action and the creation of institutions and companies that are designed for the long term and support the success of current and future action. Scientific advisory services—as the assessment of technology consequences makes an effort to do—should not only limit themselves to recommendations that specify the suitability or unsuitability of newly developed technical means for explicitly stated purposes. Instead, they should identify and outline those means that appear to be the most promising with regard to an explained preference system. Good scientific advising will therefore also address the purposes of the addressee. Whether or not he follows the recommendation at the end, remains open and is solely a decision for the addressee.

4.1.4 The Role of Normative Sciences in Interdisciplinary Advising

It is by no means a new phenomenon that scientific-technological developments have an unwelcome effect for certain actors in addition to the effects for which they were pursued. For example, war-related technical progress did not always just lead to a shift in the balance of power between countries, but also had an impact on the internal structure of societies. Windmills, irrigation technology or the mechanisation of production altered the economic circumstances for the benefit of some and to the detriment of others. Industrialisation accelerated the depletion of European forests and brought about health risks such as smog-related rickets due to an increase in the emission of carbon dioxide and particulate matter. Nonetheless, it would be too simplistic to always reconstruct such cause-and-effect chains from the outset as causality chains, just as it would be too simplistic to assume that mechanical achievements always have only mechanical effects or the harnessing of chemical processes can always lead only to chemical effects: The changes are often due to multiple decisions by numerous individuals to use or not to use a technology, to no longer use it or to really use it because others use it, and to react to unfolding unintended consequences or not to react to them. And when one correctly ascribes the development of the telescope and the view of the 'blue planet' earth from a manned spaceship as having an influence on the self-conception of contemporaries, then one will not want to reconstruct the emerging changes in the picture of the world and humans—whether desired or (as for the philosophical opponents of Galilei) undesired—as a *causal* effect of occasion-related technical developments, just as the changes in the reproductive behaviour of women is a causal effect of the invention of oral contraceptives or the changes in communication behaviour of young people are causal effects through social media or SMS.[9]

If the course of events that are connected causally with the installation and use of technical new developments can hardly be kept track of for the reasons well-known from the debates in philosophy of science,[10] this is especially true for those effects that first and foremost result from the conscious or unconscious (non-)reaction of human actors. Predictive knowledge that allowed for a forecasting of the reactive behaviour of individual actors or actor collectives as a result of knowledge of the action environment is not to be expected. In this sense of principal unpredictability, people are free in their decisions and in their behaviour. Nonetheless, all of our worldly practices are based on relatively stable behavioural expectations and—even if often only intuitively available—intervention knowledge about how the

[9] By no means should ontological dualism in the sense of an immaterial beside the material world be assumed with regard to the non-causal consequences of technological developments. It is only assumed that one can reconstruct the cause-effect relationships in relation to the changes in the balance of power, economic risks, communication behaviour or worldview, but such designated objects do not belong to the object areas of the empirical sciences.

[10] See detailed description in Grunwald (1994).

behaviour of others can be influenced. The behavioural expectations and our knowledge of how we can influence the anticipated conduct are a virtual condition with regard to the possibility of endless daily forms for the coordination of action—from raising children, to the practice of law, all the way to economics and politics. Since people are beings of nature who respond to some stimuli with natural reactions, but also because they are socialised in communities with specific, already established expectations; since ultimately neither purposes can be set an unlimited number of times nor can an unlimited number of means be selected, it can be expected that there will be regularity in the relationship between the given situational conditions and the behaviour showed under these conditions.[11]

Based on the perception that our practices of the lived-in world are susceptible to problems and our knowledge of intervention prone to shortcomings and failure, numerous scientific disciplines have developed to secure and optimise these practices. These disciplines work on the basis of the explicit reconstruction and typification of specific conditions, ways and inclinations for action in order to attempt the systematisation and methodological control of this intervention knowledge. In accordance with its subject, the epistemic basis of these sciences is not the testing of intervention effects on the course of events under systematically constructed and varied laboratory conditions, but rather the empirically based reconstruction of regularities in human behaviour.

On the basis of this, these disciplines—the circle is by no means limited to ethics, law, political science, sociology and economics, even if they are often in focus—can provide two kinds of expertise for society advising with respect to scientific-technical developments: With recourse to their methodologically organised knowledge of the conditions for action under which the affected actors react to the implementation of a new development (the de facto morality, the existing laws, the internal organisation of societal decision-making processes, the consumption needs and the consumer behaviour, the reaction to the perceived risks), they are capable of estimating at least across a broad spectrum how the anticipated consequences of scientific-technical developments fit to the actual and latent purposes among the given actors. Only with a fit or a non-fit do we find the emergence of the need to react to a development in a *normative* way; and estimating this need is the expertise of—in the broadest sense—*normative* sciences, i.e. sciences or humanities that do have means and ends and not just causes and effects as their topic. This expertise is just as indispensable as the natural-scientific survey of the relevant consequences of scientific-technological developments. From the above, it is clear and only mentioned here to avoid misunderstanding that the normative sciences' contributions to society advising do not lead to demands for norms or anything of the like, but rather to recommendations which can be accepted or rejected and which the advising normative scientist plausibly justifies, however, in order to make his efforts

[11] Within certain limits, this regularity can be tested in systematic, laboratory conditions. Since a rule applier stands in a fundamentally different relationship to the situational conditions than an object subject to the laws of nature, these limits are restrictive (Dörner 1994).

successful and rewarding, and which will be proven to be correct in view of the explicitly and (to the greatest extent possible) fully reconstructed purpose map.

But secondly and in particular, the recommendations that are provided as a result of complex advisory services by scientists or scholars depend in terms of content on how the spectrum of possible reactions to a determined need for action is perceived. Engineers tend to see more engineering strategies, lawyers are experts for legal strategies, economists for economic ones, and presumably the requirements that are made of a 'solution' will prove to vary from discipline to discipline, too. The range of options is not a fixed size, but rather can be expanded creatively, within its limits, through combination and the intelligent coordination of various strategy proposals.

Precisely with regard to the complementary function of the disciplines in the development of interdisciplinary recommendations, it is possible to justify the demand often made that interdisciplinary work must be more than 'the sum of the disciplinary contributions'. This assumes forms of interaction that require the participants to have an understanding and an assessment of the strategy candidates that are not entirely anchored in their respective discipline. In part, an ability to judge beyond the purely factual (also as a result of the previous joint work) will be required; in part, confidence in the ability of others to judge.

To the extent that it is necessary to prepare recommendations that are justified in interdisciplinary terms and bear the imprimatur of all involved disciplines, there are both particular requirements made of the participants (see Sect. 3.2) and of the forms of interaction: Because it partially involves creative processes, a creativity-encouraging organisation is required; and because it involves trust-based negotiations, the organisation should be formed in a trust-fostering way.

4.1.5 Not Only Good Advice Is Expensive

Advising is first and foremost an action—and thus, like any action—a means that the agent chooses in order to pursue *his* purposes (be they egoistic, altruistic or whatever). By nothing it is ensured, but also it is by no means excluded that the adviser also takes the action success of the advised client and addressee—i.e. the achievement of their purposes—in consideration (even and precisely then when one interprets the adviser according to the model of a rational egoist). Since the addressee requested advice on account of the difference in expertise between himself and the adviser, but cannot check the correctness and quality of the advisory service, he must protect himself differently and use other criteria to check whether the resulting recommendations are in *his* sense (however much he paid for) and whether or not he should follow them.

Moral appeals—such as "act like an honest broker!"—that call for the foregoing of one's own benefit prove to be fully inadequate, not only because the adviser's action does not provide any visible indicator that lets one ascertain whether or not he has made this appeal his own in the present case. And in fact, by no means can it be objected to morally that a professional adviser is directed by 'self-interest'. The baker already discussed in Smith's example opens his shop early in the morning not on account of his desire to satisfy the needs of his customers better than his competitors because the well-being of his compatriots lies so very close to his heart. Rather, he makes an effort to satisfy his customers for the sake of his own future well-being. In a world defined by competition and in which certain options are excluded (such as the forceful elimination of his competitor) by statutory competition rules, it is inevitable to serve the customers at least better than others to have them return and to do further business with them (1776, Chap. 2). Completely analogous quality assurance, in which a market competitor, for future advisory work and in the interest of his own ongoing welfare, insists that the professional adviser bind the success of his current advisory services to the success of the advice-seeking client, is already a thoroughly successful practice in many areas. But because in many cases such as economic or medical contexts, the sanctioning power also often disappears with the failure of the poorly-advised client, since good medical or social work advice should ensure that the customer is helped and does *not* come again; because often at least the cycles in which professional advice is requested are too long so that the hope of future welfare may be 'discounted' to a certain extent and threatens to lose influence on the current advisory service, market solutions alone would not solve the problem. Therefore outside of scientific advisory practice reputation systems have already been established that offer the provider of advice strong incentives to attach high importance to the quality of his advisory services, and which offer the requester of advice a basis for determining whether or not he can trust an advisory service. The spectrum ranges from on-line assessment forums to criteria-based membership in professional associations all the way to revocable state permits.

This can only be transferred to the area of scientific society advising, despite some analogous problems, to a limited extent. Accordingly, the incentives for a scientist or scholar are often different from those of a professional adviser. Usually, advising is not his (primary) business and it is not part of his job title. Whether the criteria for his success are in the long-term securing of his well-being (however this is determined), may be doubted and should at least not be interpreted in an under-complex way: The providing of transdisciplinary advisory services usually does not give the scientist or scholar any significant economic advantages. In any case, it cannot be anticipated that he will enjoy an interdisciplinary improvement in reputation or promising positions in a competitive pool for attractive positions (even if this may vary from discipline to discipline) as the confidence of a client requesting advice would do with a professional adviser. A higher dependency on the scientific careers of non-scientific advisory services can ultimately not be in the interests of a client requesting advice: In view of the fact that the consequences of scientific-technological developments often occur only after longer periods of time, it would be fatal if incentives originated to provide 'accommodating' advice (and thereby possibly squandering required purpose-'criticism') for the sake of one's own professional future.

Nonetheless—if one does not want to decouple scientific advisory services from the research and thus risk their up-to-dateness—the organised incentive system within science will have to bear the burden of the quality assurance of scientific advisory services. In the background are, above all, the reputation systems within science. Reputation systems in science are mainly linked to the ideal of the open discourse on reasons, according to which it can only be maintained, and only that which has been proven through argument and test should be included in the arsenal of theses. In the excessively complex thesis and theory landscapes, primarily when the disciplinary borders have been touched or even passed, researchers also usually orient themselves on indicators such as e.g. the recognition of an author within a field by professional colleagues or other actors in the scientific system, the location of the theses and the 'scientometrically' measured rank or fit of a thesis to already existing convictions. Accordingly, in addition to the pure discourse on reasons, a reputation system has also been established in the sciences and the humanities and is tied to easily understandable and easily interpretable (also for laymen) indicators that should offer information about the ability to provide professional explanatory services. The request for advice should also be tied to these forms of scientific self-organisation. Scientists and scholars should therefore be heavily involved in the development of the advising infrastructure on the basis of its self-organisation, and 'feed' the appropriate advising boards on the provider side rather than have them 'appointed' on the client side.

4.2 Public Participation as Opportunity and Challenge

Matthias Kaiser

The political realities surrounding the issue of public participation leave us in no doubt: public participation is wanted—at least in principle—and in many areas it is already inscribed in relevant laws, regulations, international declarations, treaties and conventions. Here is what is written into the *Convention on Access to Information, Public Participation in Decision-Making and Access to Justice in Environmental Matters* (Aarhus, Denmark 25 June 1998):

> ***Article 3, point 3***: Each party shall promote environmental education and environmental awareness among the public, especially on how to obtain access to information, to participate in decision-making and to obtain access to justice in environmental matters.
>
> ***Article 7***: Each Party shall make appropriate practical and/or other provisions for the public to participate during the preparation of plans and programmes relating to the environment, within a transparent and fair framework, having provided the necessary information to the public. … To the extent appropriate, each Party shall endeavour to provide opportunities for public participation in the preparation of policies relating to the environment.

As of 2009, the Convention was ratified by 41 countries, including the European Union, and has since entered many national regulations. In this respect it is an impressive document as it legally binds the signatories to implement the principles of the Convention in their laws and regulations, and citizens may draw their authorities to court if they do not follow the Convention.

But aside from legally binding national and international instruments, it is obvious that public participation holds a significant appeal to many parties concerned with designing a sustainable future for humanity. For instance, we read among the Resolutions adopted by the Johannesburg Summit, (26 August—4 September 2002):

> ***Article 26***: "We recognize that sustainable development requires a long-term perspective and broad-based participation in policy formation, decision making and implementation at all levels. As social partners, we will continue to work for stable partnerships with all major groups, respecting the independent, important roles of each of them."

An echo is again found in paragraph 13 of the Rio + 20 outcome document, *The Future We Want* (2012):

> We recognize that opportunities for people to influence their lives and future, participate in decision-making and voice their concerns are fundamental for sustainable development.

The intuitive appeal of public participation may, however, fade significantly if one contrasts it to the kind of instrument or mechanism which it may (but need not) replace. Typically the design of sustainable (alternatively: environmentally friendly, socially robust, serving the public good, etc.) policies was originally made dependent on the input of the best available scientific expertise, of scientific impact assessments, of risk-cost-benefit analyses or the like. Given this background, the question arises how public participation is slated in relation to scientific or technological expertise.

Perhaps we can detect a tendency towards a political counter-move in the current calls for evidence-based policies. This was given a heightened public attention during the Blair administration in the UK, "which was elected on a platform of 'what matters is what works'. Blair spoke of ending ideologically-based decision making and 'questioning inherited ways of doing things'" (Gary Banks 2009, p. 3; here citing Blair and Cunningham 1999: Modernising Government, Prime Minister and Minister for the Cabinet Office, London, UK).

The term 'evidence-based policy' is taken over from what in medicine is known as "evidence-based medicine" , and has led to the Cochrane Collaboration which basically seeks to base policy advice on meta-analyses of randomized controlled trials (RCTs).

In some sense the call for evidence-based policies can be perceived as running counter to the call for more public participation, especially when the choice is portrayed as a choice between "objective knowledge" versus "subjective preferences" or "evaluations" . Below we shall argue for the importance of breaking through this apparent dilemma and getting behind this opposition.

However, the situation is also complicated by another factor, and that has to do with apparently competing views of how democracy works and what legitimizes democratic decision making. There are scholars and others who believe that the days of representative democracy are definitely counted, and that the only way to achieve legitimate decisions in our societies is through strengthened public participation. We cite W. Baron as an example of this view:

> Within a normatively oriented, theoretically democratic framework, participation is designed to afford possibilities for actively taking part in and influencing political policy-making over and above the act of voting in the elections of a representative democracy, which is increasingly being experienced as insufficient. (…) participation constitutes the central principle of action of modern societies, i.e. *without opportunities to participate, society would no longer be capable of consensus and thus be incapable of survival*. (Baron 1997, p. 147; here cited from Streffer et al. 2011; our italics).

Thus the question arises what kind of legitimacy, or perhaps more appropriately, what kind of value-adding input is gained through public participation. As one may expect, any answer to this question will certainly be influenced and guided by the values and views of the authors—a reflexive point which the authors have no problem to adhere to.

4.2.1 Defining Public Participation

One of the main drawbacks of many legal or political texts mentioning public participation is that they do not clearly delineate what exactly is meant by that term. They provide leeway for many and sometimes competing views on what is implied by the term. As a quick overview over presumably public participation activities reveals, there are indeed very many processes and instruments that go under this name. They involve everything from public hearings, community gatherings, focus groups, citizen juries and consensus conferences. But obviously one can also claim that the public participates in societal/political decisions in countless ways, many more than those depicted above. Casting your vote for a candidate in the parliament is in a sense public participation, so is certainly participating in a demonstration or riot, and so is writing a letter to the editor of a newspaper, supporting a non-governmental organisation in regard a particular issue, or going to court with a complaint over a certain policy. Yet, the 'intended' meaning of the term 'public participation' is assumedly a bit more specific, though usually not delineated with very clear references.

We choose to adopt the following working definition for our purposes:

> Public participation refers to any of several possible instruments, tools, or processes which are intentionally instituted in the preparation, design or execution of administrative, governmental, technological, industrial, environmental or economical decision or policy, and which address and include stakeholders, societal or professional groups or associations, or the lay public.

In accordance with this definition a public hearing counts as public participation, as well as e.g. town meetings, or round tables. On the other hand, going to court or engaging in a strike or riot would not count as public participation in the above sense. It may be noteworthy that we do not a priori differentiate between public participation addressing the lay public and public participation addressing stakeholders. Though there are important differences between these two modes of public participation, we believe it is important to retain both in the same category. Here we follow e.g. Beierle and Cayford (2002).

4.2.2 A Brief History of Public Participation

As all histories, the history of public participation can be told from different standpoints, different perspectives, and will yield a somewhat different picture of participation. We think that the history of public participation can interestingly be told from the viewpoint of: (a) public administration, (b) social science, (c) development work and technology transfer, and (d) technology assessment.

4.2.2.1 Public Administration

Following Max Weber, public administration is to assimilate the model of rational decision making. Government administrators are thus "entrusted to identify and pursue the common good" (Beierele and Cayford 2002, p. 2) and the way they used to do this was to draw on scientific evidence and assessment, and in the end to weigh costs against benefits within the existing legal framework of the State. With increasing scope and complexity of administrative tasks, a network of bureaucracies and scientific institutes emerged which served each other's designated tasks. This can be labelled the "managerial" model of governance, which is essentially a top-down structure where decisions are prepared through the interactions of scientific and administrative experts and then implemented with the expectation of effective follow-up by those concerned. In most European countries we find for instance some scientific institutes whose most salient task is advice to governmental authorities on certain subjects. This happens e.g. every year when fishing quota for the Atlantic Ocean are negotiated between countries: on the basis of scientific data and models a quota of total allowable catch (TACs) are recommended by national institutes together with the *International Council for the Exploration of the Seas* (ICES; cf. also: Dankel et al. 2012), and then a political process ensues which fixes the quotas. Often they do not agree with the scientific recommendations due to intervening interests, though. Still, all participating countries stress that scientific expert advice is the backbone of the decision making.

The model faces the principal problem that the level of expertise is not matched by the level of accountability. In other words, if the policy fails to achieve its intended consequences, or does in the end not serve the public good, it rarely implicates the experts who were decisive to bring about the policy in the first place. Furthermore, the model does not do justice to the fact that even the arguably best policies will have to face up to opposition from various societal corners, and that opposition may lead to new political constellations but not necessarily or rather seldom to changes in expert administration. Because of the close entanglement between experts and politics, the experts are often criticised as political actors themselves without having to stand up for election.

Especially in the later post-war period of the 1960s and 1970s the managerial model lost some of its attractions in some areas, and was replaced by the "pluralist" model of governance, according to which the public administrator essentially was an arbiter between competing social interests. The pluralist model worked on the assumption that a "contingent public good was to be debated and arrived at by negotiation among interests" (Beierle and Cayford 2002, p. 4). This new model then inspired a wealth of new acts and regulations which embraced participatory mechanisms as crucial element in arriving at an outcome. Examples of this are the planning- and building acts in some countries (e.g. in Norway). The common element in these is the greater public access to governmental information and decision making. In Germany the early work of Peter Dienel on Planning Cells was an important move in this direction (Dienel and Renn 1995). Large projects with significant environmental impacts can rarely proceed without at least a public hearing where (all) available evidence is presented to the public. Ideally, the decision making is not fixed until after the public consultation process, though critical voices suspect that the public hearings sometimes only serve the purpose of window dressing.

A perhaps unintended side-effect of this new thinking was the new role of interest-groups or NGOs. They could now operate with the claim to represent certain interests or values and demand both information on and integration into the decision making process. They could also file law suits if they believed the case could be made that the authorities did not have the greater public good in mind when decisions were made.

However, in recent years also this model has come under pressure, and has been criticized for its inherent adversarial nature. The implicit claim is that one underestimates the capacities of citizens to elaborate and eventually embrace positions beyond their own narrow interests and in the interest of a well-understood public good. This "civil-society" model of public decision making relies on the self-regulating mechanisms of public deliberation and dialogue, and the role of public administration is largely confined to setting the appropriate processes in motion, providing the information input, and then being attentive to the outcome of the process. As Laird (1993) has argued, the provision of large amounts of information is not sufficient in these processes, what is required is setting the stage for an 'improved understanding'. The civil-society model is based on the assumption that citizens will transcend their original interest-based viewpoints and seek out a nuanced communal optimum, a consensus. The possibility to critically discuss and debate alternatives in a "herrschaftsfreien" context in the Habermasian sense is seen to be decisive.

There are, of course, numerous problems with the civil-society model of public decision making, if not in principle, then at least in practice. One immediate problem is that it potentially dismantles public administration to the point where it is reduced to being a facilitator rather than a provider of services. Another important issue is that the model seems to discard or ignore the importance of the media in modern societies in influencing and to some extent form public opinion. Finally, though the civil-society model initially appeals because it seems to empower a larger section of the public to steer its own affairs, it also raises the problem of accountability. Those that are instrumental or even decisive for reaching a certain decision, e.g. the siting of an incinerator, are in effect invisible and not accountable when problems occur at a later stage. Thus the cost of taking a position is often too low in relation to its potential harm.

4.2.2.2 Social Science

One would not adequately understand the assumed attractions of public participation unless one includes how the move towards it also emerges from within the (social) sciences themselves. The social sciences have experienced a tremendous growth and increased attention after WWII. In the USA the social sciences have been greatly influenced by what some characterise as the "positivist" tradition,[12] in particular as it was expressed in behaviourism. This was somewhat different in Europe, where both "Phenomenology" in a wider sense and the "Frankfurt-School" exerted significant influences. Yet, the pressure to join ranks with those scholarly activities which assumedly produced "objective" knowledge was noticeable nonetheless. An interpretation of Max Weber's plea for a value-free science (in the sense of "Wissenschaft", thus in particular including social science) paved the ground for a social science which understood itself as a by standing cartographer of social and psychological forces, theorising about basic dynamics in the social web and testing them in various observations.

Given such an understanding, it is but a short step to a conception of applied social science. One would just take the best theory about social behaviour, add some initial conditions and constraints, and derive the framework which would yield a desired outcome. It was, of course, very early clear that this ideal does not work in practice, at least not usually, with perhaps the exception of some more recent psychological insights used in marketing and industrial design.

The American Kurt Lewin is credited to have initiated the first model of what was coined "action research" (cf. Clausen et al. 1992). Lewin's basic idea was simply to establish a closer contact with the problem and how it is perceived by those involved, and then plan a series of "actions" which are subjected to the feedback of those involved, and then the actions are successively improved according to their advice. The goal was to improve the effectiveness of scientific or other interventions. In a project about changing the food habits of inhabitants in an American city during the war, Lewin noted that group discussions among housewives were significantly more effective than lectures addressing them with lots of information.

Action research came to European social science during the 1960s and 1970s, years in which many were looking at the social sciences as potential provider of new solutions to pressing social and organisational issues and innovations. It was particularly influential in the Scandinavian countries. It was initially received as a broad outlook which both addressed the concrete needs of social change in diverse settings, and aimed at deeper insights into social interaction based upon these concrete actions. Yet, the ambiguity of the goals of action research soon led to a

[12] This is not the place to quarrel about words, though some of us might be tempted to add that what goes under "positivism" here is a far cry from what it's supposed inventors, the "neo-positivists" had in mind.

split in the social sciences in their utilisation of action research. Clausen et al. (1992)[13] identify two main streams in the emerging social sciences in Scandinavia.

First, there is the technocratic-functionalist tradition of action research which basically aims at developing social techniques to manage given social problems in a certain situation. This is closely related to what Lewin was doing in the 1940s. The issue here is that certain policies only provide frameworks of action, whereas the concrete implementation of them leaves room for interpretation and variation. Social science could then be employed to improve the effectiveness of these policies in designing concrete follow-ups and implementations in close collaboration with the involved actors. The iterative format of action research, as well as the participatory mechanisms of involvement, was retained in this model, but it made no pretentions of empowering actors against top-down policies. Traces of this thinking are still dominant in for instance evaluation studies applied to governmental or industrial policies.

The second line of action research can be coined the culture-radical tradition where the aim is to arrive at radically new ways of problem formation, seeking out new social understandings and in collaboration with the under-privileged build a basis for the change of culture, changing the "Überbau" in the sense of Marx. This is a variant of what was later coined a standpoint-epistemology, supposedly having a liberating effect for the involved parties—both scholars and under-privileged sectors, united in the opposition against the ruling powers. What is retained from Lewin's action research is that solutions are not pre-given, but made up as you go along, and participation is the mechanism that ensures the quality of the outcome. It was immediately clear that this kind of research eradicates the lines between acting as a scholar or researcher, and acting as a political activist.

In spite of the ideological divide between these different schools, the methodological apparatus employed by them was often very similar or even identical. With the difficulties of central planning, especially in the Welfare State, becoming more and more apparent, the participatory methods gained a wider recognition and a larger application than the ideological nature of the above mentioned debate could indicate.

Since the late 1980s action research and its methods has apparently found a somewhat new and unifying perspective in the notion of "dialogue". Based on the works of Anthony Giddens and Jürgen Habermas, many felt entitled to utilise communicative action in a "herrschaftsfreien" setting as a new forum for social science. While neither Giddens nor Habermas are very specific on the notion of "dialogue", others, as e.g. Galtung, have developed more specific approaches which bridge dialogue with action research. Thus dialogue could pave the way to embedding participatory actions. Qualitative research in social science as a supplement to quantitative research like surveys etc., now comprises an arsenal of participatory instruments.

[13] Here based on Langsted (1973).

4.2.2.3 Development and Technology Transfer

The world is divided between the rich and the poor, and speaking in global terms, often the very poor. It is perhaps not only since the fall of Colonialism, but certainly also as a consequence of it, that the rich part of the world felt it in its best interest to provide support and help to the poor in order for them to "develop" into a richer society. Global programs were started to accelerate development. A well-known example is the Green Revolution. Countries provided development aid and ministries were set up to administer this in the 1960s and 1970s. One important argument—alongside the moral (equity) and political (allies) ones—was that aid to poor countries to develop socially, economically and technologically will in the end result in new markets for products of the rich countries.

Yet, it did not work out this way. It turned out that development was apparently far more than moving expertise and technology from one place to another. Central, top-down planning activities proved futile and rarely achieved their goals. Since large sums of money are involved, there was an obvious interest to improve the effectiveness of development activities. The question was, how?

Since the 1980s at least the answer that development agencies rely on is participation. Problems were identified as being rooted in the local culture of the intended beneficiaries. Adopting a new technology, say in agriculture, did not only involve capacity building to handle the technology, but also its integration into local culture, social standings, gender roles, beliefs and values. Thus the task of development was eventually redefined as empowering poor and often uneducated groups in developing countries to utilise the technological means and knowledge of developed countries within their own particular cultural settings, allowing them implementations that would fit their given constraints. The means to do this was through participatory actions.

The practice of the World Bank is a good example for this. Centralised planning activities are criticised as lacking the 'human' or 'social' dimension, while integration of actors' perspectives are seen as the way to secure the success of development investments. According to Paul Francis (2001) various models of participation dominate in the projects of the Wold Bank, such as "beneficiary assessment" , "social assessment" and "participatory rural appraisal" (PRA). Typically for training developers in e.g. PRA is the downplay of the role of established knowledge and the role of manuals. Instead, one seeks to enhance self-critical awareness of the developer and stressing personal responsibility. Interpersonal communication skills are encouraged and adapting a learning rather than a teaching mode. Epistemologically speaking, the most important source of knowledge is indigenous knowledge which is valued above (Western) scientific taxonomies. This also extends to valuing the visual over the verbal, the perception over logos.

Francis (ibid.) provides a thorough and at times polemic critique of PRA and the other participatory methodologies embraced by the World Bank, based on the Participation Source Book (World Bank 1996) and scientific literature on the involved methodologies. Among others he claims that the focus on community consensus in the participatory designs overlooks the empirical lessons that often units above and below the community level are the most decisive when it comes to decision making on adapting certain practices. Furthermore he claims: "The importance of charismatic specialists with esoteric training, combined with the centrality of the moral dimension, the inner-directedness expressed in the precept to 'follow your own judgement', and the symbolism of 'reversal' (cross-culturally a marker of ritual events), taken together, recall the role of the shaman" (Francis, ibid. p. 80). David Brown, one of the editors of the influential book Participation—The new Tyranny?, responds with "Rules of thumb for participatory change agents" of which rule 1 states: "Don't work for the World Bank". Behind this somewhat rhetorical appeal lies the conviction that the interests of the "participatory development establishment" as represented in development agencies as the World Bank, run principally counter to the original goal of participation to empower poor, small and under-privileged communities to design their own strategies for a better life. Participation becomes instrumentalized and obscures the factual influences of central control. It results in the co-optation of seductive language in the service of maintaining the status quo. David Brown, in the same book, suspects that the roots of the problem lie equally within the participatory movement itself. "They relate to the vanity of a movement which has sought to appropriate to itself rights over policy not just by criticizing the status quo … but also by moralizing its own boundaries" (ibid, p. 249).

Though participation is still the cornerstone of most Poverty-Reduction-Strategies for international concessional aid, its methodological function and epistemological standing is raising legitimate concerns based on precisely those visions of empowerment which originally served to legitimize it.

4.2.2.4 Technology Assessment

The case can be made that the term "technology assessment" arose with a report from the US Congress Sub-Committee for Science, Research and Development. The committee was established in 1963 with Emilio Daddario as chair. Daddario was later considered to be the 'political father' of technology assessment (TA). He saw the tasks of TA as mainly identifying and assessing the implications and effects of applied research and technology. On the basis of this the US Congress established the Office of Technology Assessment (OTA) in 1972. Fundamental for this new institution was the growing awareness that technology had a Janus face: it did not only solve problems, but it also created new ones. TA was originally exclusively expert-based and resulted in practice usually in comprehensive reports. The wave of TA swept over to Europe during the first half of the 1970s and the OECD published several reports on TA. Several TA organizations were formed and in 1989 the European Parliamentary Technology Assessment (EPTA) network was formed.

An important watershed was the 1987 European Congress on Technology Assessment, held in The Hague and organized by the Netherlands Organisation of Technology Assessment (NOTA). The congress resulted in a number of national reports on the status of TA around Europe and the world. The compilation of this information about TA and its various organisations and methods gave rise to considerable academic interest in this new field. During the 1990s a move towards more participatory TA can be clearly discerned among several countries and organisations.

Jon Fixdal argues convincingly in his doctoral thesis (1998) that, broadly speaking, four different categories of arguments were brought to the field to support participatory TA. The first three categories relate to various criticisms of the role of experts in TA, while the fourth relates to the conception of technological development.

The first group of arguments could be labelled the 'democracy argument'. It claims that "expert based problem solving disenfranchises the public of its democratic right to control policy, and that it therefore is incompatible with democratic ideals … The public should be involved in the definition of issues, in the determination of impacts and possible alternatives to be examined, and in defining and assessing the corresponding values and resolution strategies" (Fixdal, ibid, p. 18). The basic contention here is that when an issue has large consequences for many, those affected by it should have a say in the formation phase of the policy.

The second group of arguments could be labelled the 'social robustness argument'. Here it is claimed that policies often fail and public controversies emerge because the public is unwilling to accept the judgement of scientific experts if it is backed up by science alone. The public asks for a variety of perspectives to be considered. Therefore, so the argument goes, inclusion of the public through participatory mechanisms, deliberations between different assessments, may lead to more robust policies in a given social environment.

The third group of arguments could be labelled the 'knowledge argument'. This argument takes its starting point in the undeniable fact that even the best scientific or technological knowledge is always beset with inherent uncertainties. In regard to highly complex scientific or technological issues, such as e.g. the issue of gm food or the issue of climate change, the system uncertainties are very significant while the values at stake are very high. This is the situation that Funtowicz and Ravetz characterized as 'post-normal science' (Funtowicz and Ravetz 1993). Facts and values become intertwined, and assessing the quality of the knowledge extends the capacities of normal scientific peer-review. Furthermore, experts often disagree with each other on these issues, and this raises the practical problem how to adjudicate these scientific debates. On the other hand, ordinary people or the affected communities may have local, personal knowledge that could usefully supplement scientific expertise. The work of Bryan Wynne on local knowledge of sheep farmers in contrast to expert assessments in relation to a Sellafield blowout is often cited as support for this assumption.

The forth type of arguments could be labelled the 'social-shaping-of-technology argument'. Wiebe Bijker has provided ground-breaking work on the social and cultural elements that prove decisive in the emerging designs of new technologies (cf. e.g. Bijker 1995). Technologies evolve according to this view as the result of negotiation processes between different actors who have different interests in them, while the mere technical aspects are seen to be flexible and adaptable to various concerns. In the light of this view, TA activities move now from a merely reactive activity to a proactive activity, aiming to influence the shaping of the technology in question while design issues are still open. It is the move from 'downstream' to 'upstream' assessments. It would thus reduce the human costs of trial and error in regard to large technologies which essentially make all of society their laboratory (cf. Ulrich Beck 1992).

These four types of argument make the case for more participatory TA, as Fixdal and others argue. As of today, most organizations dealing with TA, in particular those dealing with parliamentary TA (PTA) include an arsenal of participatory activities and instruments. The European Commission has voiced a clear interest in utilizing participatory TA to a larger extent and is currently funding the PACITA project to spread methodologies across all of Europe.

4.2.3 Challenges and Problems

As the above brief history has indicated, participation has spurred interest from several corners of activities, and it has been applauded by political bodies as a future-oriented method to tackle important issues of societal and technological development. There can be no doubt that it has grown out of a legitimate criticism of shortcomings of traditional approaches to dealing with these issues. We want to make it very clear that any simple dichotomy of an a priori choice between expert-based approaches versus participatory approaches is doomed to failure. There have emerged insights that we need to face and deal with. These are in particular:

Mere expert-based assessments and top-down planning activities are prone to neglect important uncertainties and complexities that typically arise when processes and technologies are embedded in a socio-cultural reality.

While scientific expertise has a principal bonus on factual, descriptive information, and while some parts of scholarly activities provide important insights into normative matters (such as jurisprudence, ethics, philosophy), evaluative judgements and decision making on the basis of them belongs principally to the rightful arena of democratic deliberation.

The shaping of evolving technologies and the design of practices and institutions is subject to a variety of influential factors and constraints, and only a sub-set of them is of a scientific or technological kind. Ethical considerations of justice and fairness call for giving affected people and groups a say in matters that may have significant consequences for them. The competence of ordinary people to form well-informed opinions about complex issues, even in regard to some areas of science and technology, is often underrated.

On the basis of these insights we would argue and defend the thesis that there is a prima facie case to be made in favour of some form of participation in matters as described above. In many practical settings, participatory mechanisms offer prospects of insights and contributions that are both important and easily lost otherwise. Therefore we principally endorse efforts to include participatory mechanisms of various kinds as a tool to prepare democratic decision making, and we argue that further efforts to improve participatory methodologies are welcome.

But as the above brief history also has revealed, there are problematic issues and misunderstandings connected to participation that need to be addressed openly and in part rejected. We shall now look at some of the most important problems in turn.

4.2.3.1 The Tribalisation of Science

There has been a tendency in some quarters of social science, in particular those associated to the thesis of the social construction of knowledge, to state that knowledge claims appear in different sectors of society, and that different sectors of society adhere to different criteria of validating such claims as accepted knowledge. In other words, just as a scientist would accept carbon dating (or similar techniques) as a valid method to arrive at estimates of the time of emergence of life on earth, so would a creationist use the word of the Bible as evidence for an alternative view. Both believe to be in the possession of knowledge. The extreme tribalisation thesis would claim that one is in no position to adjudicate between these claims and that therefore the one should be equal to the other. Scientific expertise would thus lose all ground to provide privileged input in assessments of a scientific or technological kind, it would just appear as one among several possible views. One reason to support this view is that none seems to be in the position to actually produce definite and convincing proofs; all are beset with some uncertainties.

Philosophers of science (and indeed some sociologists, as e.g. Merton) have spent many years to show that scientific knowledge by and large evolves as the result of rigorous testing which in most cases bestows a relatively high epistemic credibility to it. There are indeed many alternative views how this happens in scientific practice, and it would be wrong to claim that philosophers have reached a consensus about this. Yet the differentiation between knowledge and mere belief, and the differentiation between knowledge based upon critical testing, and knowledge based upon personal trust and tradition is upheld by most of them. One need not claim that science produces pure 'objectivity' in order to rank scientific knowledge prima facie higher than mere subjectively held beliefs. Furthermore, one need not reject the value of 'folk knowledge' or 'indigenous knowledge' in order to accept the qualities (and also the limits) of scientific knowledge.

However, apart from the philosophical critique of the tribalisation thesis of scientific knowledge, there can be a socio-political observation which also throws some critical light on the thesis. We believe that many social scientists originally embraced the tribalisation thesis because they thought it liberating from the social power of scientific expertise. Yet, what we have witnessed in recent years is that the same strategy has operated as a powerful tool of largely ultra-conservative groups to counter-act the political influence of scientific knowledge. The debate about the teaching of evolution versus creationism in public schools is, of course, one telling example. Oreskes and Conway in their book Merchants of Doubt (2010) provide further examples—from tobacco smoke to climate change—how the existence of scientific uncertainty is strategically used by powerful conservative groups and industry to cast doubt on proposed policies to counteract expected harm. The underlying philosophy of these groups is that of the tribalisation thesis: one uncertain knowledge claim is just as good as any other uncertain knowledge claim.

In regard to our topic of participation we would thus like to stress that participation needs to accommodate differentials in epistemic standing, and that scientific or expert input needs to be presented and regarded according to its knowledge quality for the given purpose. "When participation is conceptualized without adequate allocation of the different functional roles citizens and scientists have in participatory processes, and when the different statements of experts and lay person are merely treated as a "variance of opinions", one has fallen victim to the tribalistic fallacy" as Gethmann et al. (ibid.) state.

4.2.3.2 Overtaxing of the Evaluative Competence of Citizens

It seems as if many participatory activities work on the assumption that the involved citizens know both what is best for themselves and, by extension, what optimally promotes the public good. And there are indeed many cases in which the expert simply has to step back and leave this judgement to the citizen or the affected party. What kind of life I want to lead and how and perhaps even when I want to die is basically a very personal choice. Yet, complications arise as soon as the individual transcends the personal sphere and enters social interaction and community welfare. One issue that has been noted in several settings is the occurrence of so called NIMBY arguments (not-in-my-backyard). Sometimes a society needs to distribute differential risks and benefits among its members in order to increase public welfare. An incinerator typically serves a city or large community, while for those living in its neighbourhood it may incur disadvantages (loss of property value) or even harm (uncertain health effects from pollution).

A further issue is the differentiation in risk attitudes that occur when people make judgements in reference to their own private life versus public or professional risks. One has found that people not only differ among each other in terms of risk averseness and risk willingness, but also one and the same individual may take the one or the other attitude depending on what sectors or roles of his life are affected. Just like the NIMBY arguments, this issue raises the question to what extent citizens may rise above their particular interests, preferences or attitudes and participate to design socially acceptable risk levels.

A further complication arises when even larger issues are at stake. Garrett Hardin drew attention to the 'tragedy of the commons' (1968), which basically depicts the problem when a shared resource is used by individuals (communities, states) on the basis of rational self-interest, but when their usage results in the long-term depletion of that resource. The oceans of the world are a prime example of this. Now, if we assume a participatory process in some coastal community along the Norwegian coast and the question of the optimal dimensions of fishery are put to them, it is reasonable to expect that even the best willing and best informed individuals might end up with recommendations that not only may contradict those that one would get in a Spanish coastal village, but that also would make any global recovery of ocean fish stocks impossible. The scaling of the issue provides important constraints on how much evaluative competency an individual citizen might muster.

As a consequence it seems that participatory activities need to be designed so that the involvement of citizens' judgement seems appropriate to the issue, while in some cases the distanced view of expert assessments based on robust social values or acceptable norms might have priority. It should also be very clear that participation in no way replaces or pre-empts the democratic decision making that has to be done by the competent authorities or elected bodies. Participation may hopefully provide relevant input into the decision making process, it may be an important element in new modes of governance, but it does not relieve decision makers from final judgements. In the end it is the competent authorities and elected bodies that are accountable, not the citizen who lends his voice to an issue, nor the scientist who provides the opportunity.

4.2.3.3 Stakeholder Fatigue

The social scientist staging a participatory exercise will often experience that his interest in doing this exercise is greater than the interest of the people to participate in it. One may even sometimes detect a competition among several groups of social scientists to mobilise relevant stakeholders or citizens. Then the question should be asked: whose interests are we really serving in our participatory exercise? Every new scientific development nowadays immediately sets social scientists in motion to stage participatory processes about them. Currently, synthetic biology seems the focus of many social scientists following the techno-scientific development. Yet, according to a special issue of Public Understanding of Science (2012; cf. also Kaiser 2012) very little controversy trickles down to the public, and indeed very little science is actually done in the field, at least in Europe. Perhaps we already have more participatory exercises about the possible problems of synthetic biology than we have science to produce any of these?

This raises the question of the timing of participatory exercises. In our view the right timing of participatory exercises should emerge as the optimal balance between two essential factors: the amount of factual information and on-going activity on the one hand, and the predictability that some kind of regulatory, economic, scientific or technological decision needs to be made in the near future on the other. Is there really enough conflict material to warrant a participatory exercise? And is there any decision process looming in the future, or any relevant agency waiting for the result of the exercise? These are crucial questions that need to be answered before participation takes place. Any sensible member of the public would prefer a good conversation with friends in a pub, rather than the tightly structured dialogue on an issue he does not see how it affects him and what his contribution to the topic will result in. People "for the most part are seeking to fend off the burdens of individual responsibility" (Gethmann et al.) and rely on the fact that for most of the issues agencies and authorities exist that will competently deal with the issue.

The misuse of the public voice is an issue that directly addresses the community of scholars dealing with TA and PTA. If participation is taken as default action, rather than as carefully evaluated and chosen as adequate for a particular purpose, it will soon run out of steam—and rightly so.

4.2.3.4 The Quality of the Participatory Process

There is a wealth of different participatory mechanisms. The Wold Bank has its canon, while TA has another. To mention just a few: *Canadian Round Tables, Citizen Juries, Consensus Conferences, Future Workshops, Negotiated Rule Making, The Three Step Procedure, Scenario Workshops, Town Meetings, and Ethical Matrix Workshops.*

On top of that one may differentiate these along different categories, as e.g.:

Value-based approaches/assessments

- lay people participation
- e.g. consensus conferences, future workshops etc.
- larger policy issues; future perspectives

Interest-based approaches/assessments

- participation of stakeholders and NGOs
- e.g. scenario-/ethics- workshops
- societal conflicts between groups/divergent outlooks on what needs to be done.

In addition one can differentiate along another dimension too:

- ***Expert-centred***: The public provides information for the expert to evaluate and systematize, and/or the expert provides the options that are evaluated by the public.
- ***Lay-centred***: Experts provide information service to the public; the lay panel is in control of issue and outcome.

As we have been arguing above, participation should not become the automatic default for a TA exercise, nor should its form be the result of mere expediency of organisation. Conscious choices need to be made which take due regard of the nature of the issue at hand and the purpose of the activity. To this end, information on methodological and practical issues in participation needs to be available for the practitioner. In principle one would suspect that this kind of information is precisely what social science is good at. Social science takes pride in being largely self-reflexive, and engaging in critical discussions of methods. Questions about the extent to which for instance charismatic facilitation, or mobilisation through emotionally beset contexts, or self-selection versus randomized sample, or long-and-intensive versus quick-and-dirty-processes influence outcomes, should be of interest to all practitioners of participatory exercises.

Yet, when it comes to participation we note that very little systematic evaluation and study is available on the success, failure, or shortcomings of participatory exercises that have been performed. In particular, one would like to see more comparative studies on the issue. This lack of scientific interest in evaluations and methodological reflection fosters the suspicion that participation is understood as a self-justifying end, rather than a means to a given end. While we share the concern that participation sometimes is instrumentalised for purposes that do not go well with its appeal to empowerment and transparent democracy (like in the critical discussions of the World Bank), we would still uphold that participation is not an end in itself, and thus its instrumental character needs to be made transparent. The social sciences and the TA community have still some way to go before the issue of participation is in good shape.

4.3 Specific Justification Problems

4.3.1 On Scientific Uncertainty and the Precautionary Principle

Matthias Kaiser

The pursuit of scientific research serves many different purposes. To a certain extent it satisfies our all too human curiosity: we come to understand a certain phenomenon better and we thus experience greater cognitive satisfaction or harmony. For instance, a grasp of plate tectonics will make us realize the natural processes which lead to earthquakes or the eruption of volcanoes—we need not worry about the possible wrath or punishment of Gods or other supernatural powers. Insights from the humanities might e.g. help us understand and communicate with other cultures. Science also helps us to correct our paths in life and eradicate false beliefs. While people for a long time assumed that cigarette smoking had beneficial effects on health, we have now learned that the opposite is the case. Some sciences, e.g. the social sciences, have a critical function. They point to outcomes which perhaps contradict our expectations or promises. For instance we might find (as Norwegian researchers actually did), contrary to our expectations, that the insertion of special training classes for driving at night and on snow or ice did not decrease the number of accidents for young drivers, but actually increased them. And science may lead to technological development and innovation. The electromagnetic transmission of signals through a wire was—in the end—the scientific basis for the invention of the telephone. The aspiration of science does not only make us understand the world, but also to lead to something useful and beneficial which traces all the way back to the Scientific Revolution. Yet, this aspiration was, and to a certain extant still is, coupled to another vision: the dream to control our environment and being able to effectively plan the future. It is this aspiration which assumedly is the backdrop of the widespread function of science to provide political advice. A whole sector of policy-related research has emerged, serving the interests of rational public administration and policy. In most countries a cluster of laws and legal regulations ensures that scientific advice is sought before major developments take place, e.g. developments which may have a significant environmental impact are to go through an environmental impact assessment (EIA) before being realized. Risk-cost-benefit analyses are also often prescribed. The reasons for seeking out scientific advice before major technological or other interventions in nature or society are carried out, are certainly that the costs of being wrong or causing unintended side-effects may turn out too big to be politically acceptable.

There is, however, a catch to it: as long as the produced knowledge is reliable and certain, all is well, but as soon as there are major uncertainties involved in the knowledge which reaches the decision maker, the advice may at worst have detrimental effects on the outcome of the policy. Herein lies a potential conflict and perhaps even clash of institutional cultures between the production of scientific knowledge and the utilisation of knowledge for action. Epistemic rationality does not fully mirror practical rationality. In other words, scientists typically go to great length to avoid making a type 1 statistical error, a false positive, while committing a type 2 error, a false negative, is not similarly discredited. In plain language: in science one needs to have strong evidence before making any positive claims about a possible causal connection between A and B, while overlooking the causal connection between A and B in the absence of strong convincing evidence is not discrediting the scientist. For a person (institution etc.) placed in a context of decision making the situation is quite different: in order to minimize the risks of wrong decisions (given intended outcomes), information about what we can ascertain with some certainty is as important as information about what we actually might be overlooking, what we simply do not know with reasonable certainty but which still might be the case.

The potential conflict between these two types of rationality in our landscape of political and administrative realities is often sharpened by a further psychological feature of the decision makers. To the extent that equivocal and definite information about expected outcomes is handed to them from the scientific experts, they have sufficient justification for pursuing this recommended action, and they can, should unexpected and detrimental outcomes arise, blame the provider of the information as the responsible party. They "naturally" prefer certainty, preferably in the form of numbers, above mention of uncertainty. This is the basis for the well-known joke about Richard Nixon who is reported to have said that he would wish for a one-armed scientific adviser. Why? "Well, the scientists I have always tell me something along the lines of: on the one hand …, but on the other hand …!"

4.3.1.1 The Disappearance of Uncertainty

Simple as it sounds, the neglect of uncertainty can cause grave mistakes and conflicts. As illustration we shall report a case based on the experience of the leading author of this chapter[14]:

When the new airport of Oslo (Gardermoen) was planned around 1994, the politicians decided to place it right on top of the largest aquifer in Norway, with groundwater which could serve as a reservoir for drinking water for up to 150 000 people. As a result of public concerns raised by some NGOs, the Norwegian parliament (Stortinget) demanded that the new airport is to be 100 % environmentally safe.[15] This was set as a condition for granting the necessary permissions, and was then to be handled through the Norwegian Pollution Control Authority ("Statens forurensningstilsyn": SFT). There are always environmental values at stake when big building projects like an airport are planned. One particular challenge was related to the protection of the ground water underneath the runway. During wintertime airplanes typically need to de-ice wings and body before take-off in order to avoid accidents (Fig. 4.1).

In Norway winters can be particularly harsh so that de-icing is part of the usual routines of an airport during this period. Two types of de-icing fluids were in use at the time, containing among others mono-propylene glycol and potassium acetate. These chemicals are easily degradable under aerobic conditions, but degradation consumes oxygen. When large spills or high concentration of the chemicals occur in the run-off water, all available oxygen may be used up and anaerobic conditions can be created. This could lead to organic sulphur compounds contaminating the ground water. The extant glycol was considered a potentially significant environmental threat and was thus given the greatest attention by the developer. Since not all de-icing fluids sprayed on the plane will take-off with the plane and eventually be dispersed by wind, some amount will enter the ground on the airport. The plan was roughly to collect run-off water via ditches in delay-basins where they could be collected and glycol could be recycled.

In order to apply for a permit for the discharge of de-icing fluids into the ground, a comprehensive study (an environmental impact assessment) was ordered to determine safe loads. Altogether ten different studies were commissioned by the airport developer OHAS. The scientists were given a strict time frame of only a few months for the study covering one winter and spring season, since the building activities were simultaneously following a tight schedule. All data collection and

[14] The case presented in the following is extracted from a Master Thesis by Rakkestad (1996), supervised by Matthias Kaiser.

[15] The selection of the site among three possible sites was widely discussed at the time and afterwards. All agree that the decision was a politically motivated decision, not one based on the technical and scientific studies available at the time which were not unanimous or unequivovally providing a clear recommendation. But by extension it involved a technological decision in the sense of Ronald Giere: "… a decision to develop or employ a specified technology in a given context for a stated purpose" (Giere in Caplan & Engelhardt (1987), p. 142).

Fig. 4.1 De-icing airplanes in winter conditions (open source, Wikipedia/Steve torquay)

analysis had to be performed within this time. Furthermore, the scientists were not entrusted the precise chemical composition of the two substances in use, due to industrial secrecy. The system for cleaning the spills was to be developed while these building activities were already under way. The scientists had to develop a new research design since they lacked experience with this problem. No other comparable studies were available. This was in particular true of the issue about the soil's remediation capacity, which was considered crucial in relation to the question if any glycol or other polluting substances would enter the ground water. A specific set of studies was set aside to elucidate this issue, both studies with site samples and laboratory studies.

One unfortunate circumstance of the studies relates to the precipitation during the time of the research. As a matter of fact, it turned out that the precipitation during this time was just 2 % of the average for this month. This affected the depth the fluids would reach. The researchers assumed, without any more specific justification, that normal precipitation would not affect the results by more than 40 cm, a number which appeared out of nothing. This introduced a major uncertainty, but there were others.

For instance, there is the uncertainty related to the measurements that were performed on site. Samples of the soil were taken from several lysimeter trenches and analysed. The standard deviation for the results of these measurements was close to 60 % of the mean. Given the constraints of the projects, the scientists had to live with this. These results did apparently not affect the scientists' estimate of the statistical uncertainty of the data which for the remediation studies was presented to lie somewhere between "±5–10 %". Furthermore, it was known that the ground structure under Gardermoen was varied, and it was thus recommended that mineralogical studies needed to be performed to assure the transferability of the measurements from the lysimeter trenches, but apparently time was too short and resources too limited to perform these studies, so that the transferability of the measurements remained a major source of uncertainty. Other uncertainties were not explicitly addressed in the study, such as e.g. uncertainties due to framing. All real systems are necessarily open systems, and their openness is not only a result of their outer limits to other systems, but also the result of what parameters are used to describe the system. Living organisms within the system can be affected from micro- and macro-level, or causal factors like accumulated low-dose radiation, which again might influence the system in the long-term. Thus any description of the (real) system under study assumes decisions in regard to local vs global, short-term vs long-term, and micro vs macro descriptors which may have causal influences. But even in the laboratory studies there were major uncertainties—aside from those resulting from the question of the representativity in regard to the real system. Since the content of the chemical additives of the industrial de-icing fluids was not given, the studies could not provide a full analysis of degradation. Potassium acetate and glycol were also tested on separate soil profiles, so that the joint behaviour of these substances as it would occur in real life could not be observed. The soil temperature in the laboratory experiments was 8 °C, but there were no data available that would indicate the soil temperature at the airport site, though it was known that the decomposition rate was highly sensitive to temperature.

More uncertainties could be added here, but our main point so far is simply that the sources of uncertainty are typically varied, and that the time frame (as well as available resources) of the decision processes introduced some serious constraints for research to deal with them. Moreover, not all inherent uncertainties could have been dealt with, even if the time frame was longer. As Zbiginiew and Kundewicz (1995) observed: “Uncertainty in hydrology results from natural complexity and variability of hydrological systems and processes, and from deficiency in our knowledge”.

What happened then? The different studies did not typically address all the uncertainties involved in the results, but occasionally some sources of uncertainty or ignorance were mentioned in footnotes. A summary report for all studies addressed the remediation capacity of the soil and here virtually all mentioned uncertainties were absent. This then resulted in the shortened application to authorities (SFT) in which no uncertainties occurred, but instead it gave some threshold numbers for which no evidence was provided (Table 4.1).

Table 4.1 Reported soil thresholds

Component to be decomposed	Load (kg/m^2/year)	Capacity (kg/m^2/year)
Acetate	2	13–33
Glycol	0.3–1.2	3–40

This was then seen as sufficient evidence for fulfilling the condition set by parliament to protect the ground water and to make the airport 100 % environmentally safe.

In other words, when scientific results reached the decision makers, all inherent uncertainties had vanished and were replaced by numbers which suggested precision and reliability. No objection was raised when the airport was portrayed as environmentally safe. Critics in the media were answered by claims that scientific expertise has shown that there is no reason for concern.

There was an aftermath to this case. A master thesis was written during the construction of the airport charting the inherent uncertainties of the studies. When the airport opened some years later, the ditches to collect the run-off water from the de-icing were in use. However, roughly half a year later an environmental organization conducted tests of the ground water under the new airport and found much higher concentrations of glycol and other substances than were assumed by the authorities in their permission to discharge the fluids. This caused the media to voice concern and criticize the developer OHAS. OHAS on the other hand, pointed to the scientists as the responsible source of the error, and pointed to the significant resources they had spent to ensuring environmental safety. Now the scientists entered the media, since they felt unjustly criticized. They pointed to the politicians who had demands which, according to the scientists, were simply unrealistic: 100 % environmental safety will never be achieved. The water quality has never been satisfactory anyway, and that it was basically wrong of the politicians to try to protect the ground water at any price. They were all along aware of the danger that the fluids might indeed pollute the ground water. The science was right, they said, but the politics was wrong. At this stage of pointing fingers to assumedly guilty parties the National Committee for Research Ethics in Science and Technology (NENT, represented through M. Kaiser) entered the media and pointed to the master thesis conducted some years earlier. In essence the committee argued that the neglect of inherent uncertainties in the preparatory studies was a major flaw in scientific conduct. These uncertainties should have reached the decision makers. In a charged debate with the scientists it emerged that they had initially agreed to abstain from any active participation in the public debate about the airport, as a condition for receiving the commissioned study. They had agreed to stick to "facts" and their research, and not to enter the political or normative debate around the development.

The point we want to make with this case is not to claim any lack of scientific rigour on the part of the scientists. Nor do we want to present this case as fully typical for all EIAs conducted nowadays. But we do want to point to the tendency in such studies to make uncertainties disappear before they reach the decision maker, or not to devote much attention to them in the first place. If the quality of knowledge can roughly be defined as being fit for a given purpose, then the quality of the produced knowledge in the above case was poor, in spite of being conducted with great scientific rigour and competence.

4.3.1.2 Uncertainties Due to Methodological and Model Variations

If we stick to water management as a decision making task, entering politics on the basis of scientific assessments, we can also provide another illustrating case. Refsgaard, van der Sluijs, Brown and van der Kaur (2006) provided an interesting case study illustrating not the disappearance of uncertainty, but the all too real manifestation of uncertainty through expert disagreement. The county of Copenhagen conducted a study in the year 2000 based on a real water management decision. A new Water Supply Act forced them to provide an action plan to protect the ground water from any pollution. The county asked five of the country's most distinguished consulting firms to conduct studies of the aquifer's vulnerability in an area west of Copenhagen. The key question was: which parts of this area are most vulnerable to possible pollutions and need to be protected?

What was special about this consultation was that the authorities asked fived consulting firms instead of usually only one. Furthermore, these well reputed firms were known to apply different methods, or at least slightly modified methods, in their studies. They simply had different views and preferences on how to conduct such studies.

The resulting recommendations are shown in this picture (Fig. 4.2):

It should perhaps be pointed out that all consultants used the same raw data, thus the differences in their estimates cannot be traced back to different data. The authors explain:

"It is apparent that the five estimates differ substantially from each other. In the present case, no data exist to validate the model predictions, because the five models were used to make extrapolations. Thus, it is not possible, from existing field data, to tell which of the five model predictions are more reliable. The differences in prediction originate from two main sources: (i) data and parameter uncertainty, and (ii) conceptual uncertainty. Although the data and parameter uncertainties were not explicitly assessed by any of the consultants (as is common in such studies), the substantial differences in model structures and the fact that the consultants all used the same raw data point to structural uncertainty as the main cause of difference between the five model results and as a major source in model predictions." (ibid., p. 1587).

This case illustrates not so much the vanishing of known uncertainties, but rather the occurrence of inherent model uncertainties due to different scientific approaches. This raises the question how a decision maker should handle this, how uncertainties can enter the more explicit framework of decision making. It also raises the question how science-for-policy, i.e. scientific advice to decision makers could become more reflexive in terms of dealing with inherent uncertainties.

Mapping Uncertainties

It was Funtowicz and Ravetz (1990) who drew attention to the various pitfalls of neglecting uncertainty, and proposed a scheme to address these uncertainties, the

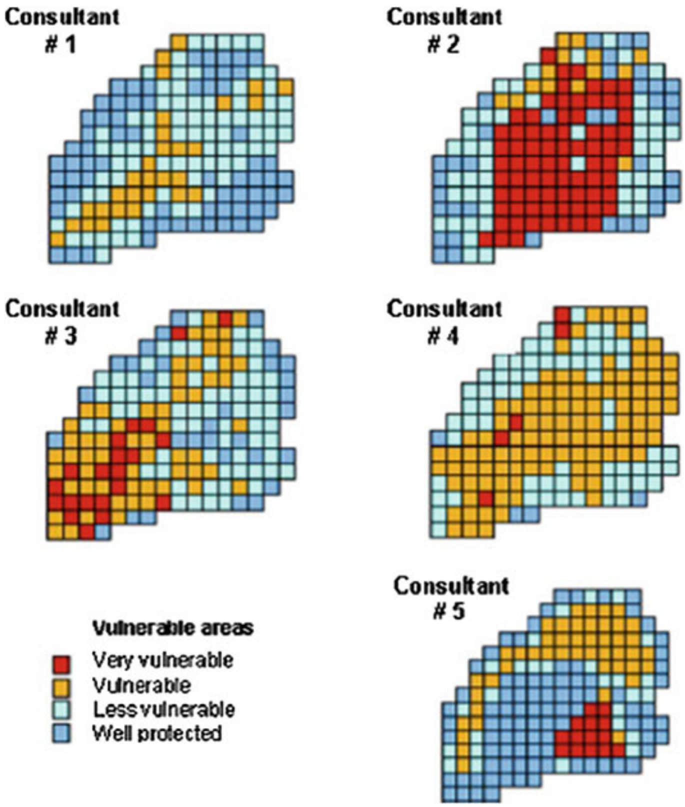

Fig. 4.2 Different predictions on aquifer vulnerability in an area west of Copenhagen (reproduced after Refsgaard et al. 2006)

so-called NUSAP scheme. This acronym was devised to represent the following categories: *Numeral, Unit, Spread, Assessment*, and *Pedigree*, a scheme which starts from more quantitative features to include more qualitative features. The first to work out this scheme more detailed in relation to a specific case was Jeroen van der Sluijs in his PhD thesis about uncertainties in risk assessments of anthropogenic climate change (1996). Since then a significant number of scientific publications have addressed the question of how to map uncertainties in concrete studies (e.g. Fjelland 2002; Knol et al. 2009; Funtowicz and Strand 2011; Garnåsjordet et al. 2012). We point in particular to the more recent work of Jeroen van der Sluijs.[16] An important observation in these contributions is that uncertainty comes

[16] For an update see his publications: www.jvds.nl or www.nusap.net.

in different flavours or dimensions. It has different sources, either from the epistemic basis (what we know or do not know yet) or from the system under study (stochastic processes, complexity, chaos etc.). It also has different locations, e.g. in the data, the models, or the problem framings. Furthermore, it involves several levels of uncertainty, e.g. statistical uncertainty or scenario uncertainty. Finally it may relate to the issue of more or less implicit value choices. In other words, uncertainty in assessments will not be exhausted by e.g. the introduction of error bars in the final results.

An uncertainty matrix as proposed of van der Sluijs has the following form (Table 4.2):

What is important here is to stress that explicit inclusion of various levels and sources of uncertainty are an essential part of what has come to be called *knowledge quality assessment*. We recall that the quality of knowledge refers to being fit for a given purpose. The issue in scientific policy advice is then to capture the various dimensions which may affect the overall quality of the output. This starts already at the level of problem framing which in effect is decisive for the knowledge that is brought to the fore, but in many cases is contested by other parties who defend another framing of the problems. In many assessments the analysis involves the inclusion of stakeholders which raises questions of who the affected parties really are, whether other groups should have been consulted, and how they are made part of the overall assessment process (see chapter on participation). A summary of key issues in knowledge quality assessment is provided by van der Sluijs (Table 4.3):

Given that one agrees that mapping and communication of major uncertainties in scientific assessments is indeed a crucial element in policy advice, the next question is then what strategies a decision maker can employ to handle uncertain information. We shall in the remainder highlight the Precautionary Principle as one such strategy.[17]

4.3.1.3 The Precautionary Principle

The early stages of national and international environmental policies can be characterized by a curative model of our natural environment: With increased environmental impacts of growing populations and industrialization, the environment could no longer cure itself; it should thus be helped to repair the damage inflicted upon it by human activities. For reasons of equity and feasibility governments sought to apportion the economic costs of such intervention by requiring polluters to pay the cost of pollution. The *Polluter Pays Principle* was aiming also at functioning as a deterrent to engage in risky operations which might lead to large liability claims. It soon became apparent, however, that this *Polluter Pays Principle*

[17] The following is in part extracted from Kaiser (2006), and some parts are extracted from Kaiser (2013).

Table 4.2 Uncertainty matrix (adopted from van der Sluijs et al. 2003)

	Level of uncertainty			Nature of uncertainty		Qualification of knowledge base			Value-ladenness of choices		
Location ⇓	Statistical uncertainty	Scenario uncertainty	Recognised ignorance	Knowledge-related uncertainty	Variability-related uncertainty	–	0	+	–	0	+
Context											
Expert judgement											
Model structure											
Technical model											
Model parameters											
Model inputs											
Data											
Outputs											

Table 4.3 Key issues in the assessment of knowledge quality (reproduced from van der Sluijs et al. 2008, p. 3.)

Foci	Key issues
Problem framing	Other problem views; interwoven-ness with other problems; system boundaries; role of results in policy process; relation to previous assessments
Involvement of stakeholders	Identifying stakeholders; their views and roles; controversies; mode of involvement
Selection of indicators	Adequate backing for selection; alternative indicators; support for selection in science, society, and politics
Appraisal of knowl-edge base	Quality required; bottlenecks in available knowledge and methods; impact of bottlenecks on quality of results
Mapping and assessing relevant uncertainties	Identification and prioritisation of key uncertainties; choice of methods to assess these; assessing robustness of conclusions
Reporting uncertainty information	Context of reporting; robustness and clarity of main messages; policy implications of uncertainty; balance and consistent repre-sentation in progressive disclosure of uncertainty information; traceability and adequate backing

was practicable only if accompanied by a preventive policy, intended to limit reparation to what could be compensated. This 'prevention is better than cure' model marks the second stage of governmental action for environmental protection. This stage was characterized by the idea that risks are known and quantifiable, and the *Prevention Principle* guided policy making. This was the heyday of quantitative risk assessment and risk-cost-benefits analyses. Note that this principle does not prescribe fixed thresholds of acceptable risks, but leaves this decision on a case-by-case basis to the decision maker. For instance, if a certain, very small risk, say 0,1 %, of a food ingredient leads to some allergic reactions, the decision maker might regulate that this is acceptable for an adult population, but perhaps not to infants if the decision maker wants to provide a higher level of protection to them. The threshold is, then, a value based decision.

The emergence of increasingly unpredictable, uncertain, and unquantifiable but possibly catastrophic risks such as those associated with GMOs, climate change etc., has confronted societies with the need to develop an additional third, anticipatory regime to protect humans and the environment against unanticipated risks of (new) technologies: the Precautionary Principle (PP) or 'better safe than sorry' model. The emergence of the PP has marked a paradigmatic shift from a posteriori control (civil liability as a curative tool) to the level of a priori control (anticipatory measures) of risks (de Sadeleer 2002). While the Prevention-Principle rests on some reasonably well established and quantified risk assessment, the PP goes beyond this and aims at dealing with uncertain risks, i.e. risks where no reliable quantification of magnitude is at hand, but which still are backed up by some plausible evidence or plausible scientific model. Thus, the PP is about managing uncertainty and not about risks in general. This is an important distinction which even otherwise well informed commentators sometimes overlook (e.g. Sunstein 2005).

Over the past decades, the Precautionary Principle has become an underlying rationale of a large and increasing number of international treaties and declarations in the fields of *inter alia* sustainable development, environmental protection, health, trade, and food safety. The PP is on its way to become a widely accepted part of international law. In its basic form, the precautionary principle states that action to protect human health and the environment to avoid possible danger of severe and irreversible damage need not wait for rigorous scientific proof (Weiss 2003). In practice, different and somewhat diverging formulations, definitions and interpretations of the Precautionary Principle can be found. Further, multitudes of contradicting perspectives of what makes up a precautionary approach coexist amongst major players in the international arena.

A common reference point when discussing the PP is often §15 of the Rio Declaration (United Nations 1992) where it reads:

> In order to protect the environment, the precautionary approach shall be widely applied by States according to their capabilities. Where there are threats of serious or irreversible damage, lack of full scientific certainty shall not be used as a reason for postponing cost-effective measures to prevent environmental degradation.

Several points need to be noted here: First, this quote does not mention a principle, but only a precautionary approach. Second, the text is far from functioning as a satisfactory definition. It contains a triple negation (i.e.: ***lack*** of full certainty … shall ***not*** be used … as reason ***not*** to act) which makes it difficult to see what is actually implied. Third, it contains two important provisos which allow for problematic interpretations, i.e. both the reference to national capabilities, and reference to cost-effective measures. Fourth, from a philosophical point of view one may rightfully question whether science ever achieves full certainty. In spite of this, the mentioning of precaution in this context marked something close to a watershed in international law. The PP has already earlier played a role in some national legislation (e.g. in Germany and Sweden), and, indeed, to some extent in international law, as e.g. in the North Sea Treaties. But most significantly, the PP in several varieties of definition has entered many other international agreements in later years, such as e.g. the Cartagena Protocol on Biosafety (2000), the Stockholm Convention on Persistent Organic Pollutants (2001), or the EU Communication on the PP/the EU Nice Treaties (2000).

At the same time, a lively debate about the PP ensued, both politically and academically. This is certainly not surprising at all, since the PP can be seen as touching different contexts at the same time, as e.g. a scientific, a legal, a political, and a cultural and ethical context. Bridging all these areas involves grasping rather complex connotations, traditions and institutions. Thus, some see the PP as essential anti-scientific, anti-rational, anti-innovation, anti-sustainable use, or Northern in outlook. Others defend it as an ethically founded principle for responsible co-existence in a globalized context, as a safeguard to care for future generations, as integral to sustainable development, as truly responsible science. Much of the debate has focused on the use or abuse of the PP in international trade where some fear it may be used as a new instrument for trade barriers, while others stress that the PP provides the assurance to Nation States that their chosen levels of safety will not be compromised by international trade.

Furthermore, the fact that the PP in its official documents does not have a unique, clear and precise definition also invites critical comments. Per Sandin (1999) identified not less than 19 definitions and one might safely assume that others have emerged since. Though the conceptual vagueness of the PP might be a crucial reason for its political success (at least in some states and contexts), it is also raising issues about the implications for its actual application. Thus, several authors make do with delineating between weak and strong versions of the PP. Roughly speaking, weak precaution would simply state that some kind of preventive or monitoring action is not precluded when being in a situation where there is no conclusive evidence that some serious harm actually will occur. This somehow captures the folkloristic wisdom: “better safe than sorry” . A strong version, on the other hand, would go beyond weak precaution and require some active measure to counteract or delimit some uncertain but scientifically plausible and serious future harm. This typically appeals to institutions to consider regulatory or other kind of controlling measures to accompany a scientific or technological development as long as basic uncertainties prevail. One should note that even a strong version of the PP is not identical to an extreme form of the PP which would require that an activity should not proceed until proven to be safe. This is obviously extreme and unrealistic, since from a scientific point of view zero-risk is an impossibility and proofs of total safety are beyond the possibilities of science.

Another issue of debate has been whether the notion of approach versus principle actually makes much of a difference. Again commentators are divided. Some view the notion of approach as the more flexible term, allowing for contextual adjustments and various operational strategies. This they contrast to the notion of principle which is then viewed as a legally binding concept to apply strict measures to prevent uncertain but possible harm of an activity to occur. The crucial question seems to be whether precaution has become part of customary international law. Yet, this debate may in the end only reflect the difficulty of different legal systems to accommodate broad principles that allow for some discretion and judgement in their concrete application (Cooney 2005). Thus, in an international context it makes perfect sense not to differentiate sharply between these two notions.

There is an important political context behind these issues as well. This can perhaps best be illustrated by pointing to the fact that acceptance or rejection of the PP is seldom coherent even within the domestic policies of a country, but seems to follow considerations of national interest. For instance, the USA has policies that are strongly precautionary in wildlife protection, but opposes the PP in a global trade context. Australia has domestic obligations to apply the PP in their national environmental policy decisions, but joins the USA in their resistance to accepting PP as an international legal principle. In other areas, e.g. within the Convention on International Trade in Endangered Species of Wild Fauna and Flora (CITES) or within the International Whaling Commission both countries are supportive of the PP. Within the EU one has noted that Southern European countries allow the sales of unpasteurized cheeses in spite of the risk that it may harbour Listeria monocytogenes and other dangerous bacteria. In this they seem to contradict the precautionary policies for food safety the EU propagates in other areas. They do so because of the long traditions of this kind of cheese making and their role in the food-culture of the countries. Such variation in the preferred approach to the PP within different areas of application gives easily rise to the suspicion that States support the PP when it can meet their environmental and other safety standards at little or no cost, but that they reject other states to use it when this implies high costs for their own economy. In the context of globalisation of trade and technology it emerges that the interests of states to protect certain rights (IPRs) over a technology or the interest to export technologies to countries with less stringent safety regulations may further intensify the inequalities between the developing countries and the industrialized countries.

Finally, there is an ethical and cultural context. Our dealings with nature, our considerations of human health and our dealings with risks imposed on us by others are typically deeply embedded in a cultural framework of understanding and valuation. How risk-aversive or risk-taking people are in various areas is influenced by value-laden concepts and their role in the respective culture. The degree to which people consider a risk acceptable depends not only on magnitude and probability of harm. Risks tends to be seen as less acceptable if the (perceived) controllability of consequences is lower; if the nature of the consequences is unfamiliar and dreadful; if one is exposed to the risk involuntarily; if the benefits of the activity are less clear and smaller; if the effects are more acute and more nearby in space and time; if risk and benefits are unfairly distributed; and if the likely harm is intentional. Attitudes towards risks vary from person to person and across cultures (e.g. risk-seeking versus risk-averse). That is one of the reasons why participatory approaches are needed in implementing the PP.

Other values, e.g. values stressing individual autonomy versus values conducive to social coherence, vary culturally. The same holds for religious versus secular values. The European/World Values Surveys provide evidence based on empirical data from almost 80 societies worldwide that post-industrial change brings remarkable changes in people's worldviews (Inglehart 1997; Inglehart and Baker 2000; Inglehart and Welzel forthcoming). As the knowledge economy replaces the prominence of the industrial sector, values that emphasize conformity to group discipline and institutional authority tend to give way to values that emphasize human self-expression and individual choice (Welzel 2003). These attitudes have a profound impact on our views on moral responsibility. This applies e.g. to conceptions of both inter-generational and intra-generational justice. These cultural factors also have a large impact on how we view the moral standing of nature and wildlife.

Precautionary "thinking" has been with humanity probably for a very long time and one may trace examples of it in the history of technology. Precautionary approaches also go back in history for quite some time. An important study on "Late lessons from early warnings" (Harremoës et al. 2001) mentions the example of Dr John Snow, who in 1854 recommended removing the handle of a London water pump in order to stop a cholera epidemic. The evidence for the causal link between the spread of cholera and contact with the water pump was weak and not a "proof beyond reasonable doubt". The simple and relatively inexpensive measure was very effective.

The Precautionary Principle, however, seems of a more recent historical date, and it implies a comprehensive and legally binding obligation to use precaution in special cases.

History: The "Vorsorgeprinzip" in German Environmental Policy

The precautionary principle is one among altogether five central principles in German environmental policy (see Boehmer-Christiansen's contribution in O'Riordan and Cameron 1994). The other principles are "the polluter pays", "cooperation" (Kooperation"), "proportionality between costs and profit (Wirtschaftliche Vertretbarkeit") and "joint responsibility" ("Gemeinlastprinzip"). While the principle of proportionality indicates that no enterprise or trade should be imposed higher costs than it is able to bear without going bankrupt, common responsibility means that any enterprise or trade can be subsidised in order to introduce measures to stimulate the environment. The precautionary principle may be traced back to the first draft of a bill in 1970 aiming at securing clean air. This document expressed that the bill aimed at preventing damaging environmental effects: The greater the danger, the greater the need for measures taken by the authorities to protect the people. This also set the legal framework for active measures that were not aiming at repairing damages that had already taken place, but preventing their occurence. The law was passed in 1974 (as "Bundes-Immissionsschutzgesetz, BImSchG") and covered all potential sources of "air pollution, noise, vibrations and similar processes".

The most unambiguous explanation and definition of the precautionary principle in German environmental policy came in a report from the Ministry of the Interior of the federal parliament (Bundestag) in 1984. Here it was stated that: "Responsibility towards future generations commands that the natural foundations of life are preserved and that irreversible types of damage, such as the decline of forests, must be avoided ". Thus:

> The principle of precaution commands that the damages done to the natural world (which surrounds us all) should be avoided in advance and in accordance with opportunity and possibility. "Vorsorge" further means the early detection of dangers to health and environment by comprehensive, synchronised (harmonised) research, in particular about cause and effect relationships…, it also means acting when conclusively ascertained understanding by science is not yet available. Precaution means to develop, in all sectors of the economy, technological processes that significantly reduce environmental burdens, especially those brought about by the introduction of harmful substances. (cf. Bundesministerium des Innern 1984, p. 53; here quoted after the translation by Sonja Boehmer-Christiansen in O'Riordan and Cameron 1994).

The combination of the precautionary principle with the development of cleaner technologies is typical of the German ideas of environmental protection. By way of structural measures one has given support to the development of technical solutions to environmental problems. In Germany the environment is first of all protected via the use of technology (BAT, "best available technology", "bester Stand der Technik" respectively). This has created jobs and environmental technology has become a growth area.

Defining the Precautionary Principle

The German interpretation of the PP is one of many definitions. There seems to have been little convergence yet towards a common definition of the PP in the various international treaties. The North Sea Treaties (Bremen 1984, London 1987, Den Haag 1990, Esbjerg 1995; all reprinted in Esbjerg 1995) are early examples of international treaties where the PP has had a very strong position. What is interesting is the shift of reference to the PP in the various North Sea Treaties:

From: '… timely preventive measures …' given 'insufficient state of knowledge' (1984) to: '… a precautionary approach is necessary which may require action … even before a causal link has been established by absolutely clear scientific evidence…' (1987) and: '…apply the precautionary principle … even when there is no scientific evidence to prove a causal link…' (1990) to finally: '…the guiding principle …is the precautionary principle …—…the goal of reducing discharges and emissions … with the aim of their elimination'. (1995)

Scientists often criticize the notion of precaution as being too imprecise; that there is no definition available that allows an immediate operationalization of the principle (cf. Sandin 1999; Graham 2001; Goklany 2001; Morris 2000). This is, of course, true for all the diverse definitions and formulations that this principle has undergone over the years. None of these formulations allow for a mechanical application of the principle. All need interpretation. The scepticism seems to persist in quarters of science, in spite of the many academic efforts to clarify precaution further (cf. e.g. O'Riordan and Cameron 1994; FoS 1997; JoRR 2001; JAGE 2002; Cottam et al. 2000; Freestone and Hey 1996; Fjelland 2002; Raffensperger and Tickner 1999; Tickner 2003; Lemons and Brown 1995; Lemons 1996). A UNESCO report under the auspices of its World Commission on the Ethics of Scientific Knowledge and Technology (COMEST) compares some of the better known versions of the principle (COMEST 2005). In the following Table 4.4 we add some additional ones:

It was already in 1994 pointed out (O'Riordan and Cameron 1994) that the vagueness of the principle is by no means surprising, nor is it a drawback. In 1999 Jordan and O'Riordan state that "the application of precaution will remain politically potent so long as it continues to be tantalizingly ill-defined and imperfectly translatable into codes of conduct, while capturing the emotions of misgivings and guilt" (Jordan and O'Riordan 1999). The Precautionary Principle has a similar semantic status as moral norms or ethical principles (like human dignity, equity, and justice) or the principles of Human Rights. It needs to be interpreted and specified on a case-by-case basis, and it will sometimes change its specific content

Table 4.4 Definitions of the precautionary principle

Source	Definition	Optional/mandatory action
United Nations World Charter for Nature (1982)	"[When] potential adverse effects [of activities] are not fully understood, the activities should not proceed"	Strong: Requires a moratorium in the case of uncertainty
London Declaration: Second International Conference on the Protection of the North Sea (1987)	"Accepting that, in order to protect the North Sea from possibly damaging effects of the most dangerous substances, a precautionary approach is necessary which may require action to control inputs of such substances even before a causal link has been established by absolutely clear scientific evidence"	Weak: Includes qualifying language such as "may require action" and "before … absolutely clear … evidence"
Rio Declaration: United Nations (1992b)	"In order to protect the environment, the precautionary approach shall be widely applied by States according to their capabilities. Where there are threats of serious or irreversible damage, lack of full scientific certainty shall not be used as a reason for postponing cost-effective measures to prevent environmental degradation"	Weak: Includes qualifying language such as "according to their capabilities" and "…postponing cost-effective measures." Contains triple negation
International Joint Commission (1994)	"All persistent toxic substances are dangerous to the environment, deleterious to the human condition, and can no longer be tolerated in the ecosystem, whether or not unassailable scientific proof of acute or chronic damage is universally accepted"	Strong: Bans use despite uncertainty of effects
EU communication on the PP (2000)	"The precautionary principle applies where scientific evidence is insufficient, inconclusive or uncertain and preliminary scientific evaluation indicates that there are reasonable grounds for concern that the potentially dangerous effects on the environment, human, animal or plant health may be inconsistent with the high level of protection chosen by the EU"	Strong: Requires intervention to maintain the high level of protection chosen by the EU
Wingspread Statement on the Precautionary Principle	"When an activity raises threats of harm to human health or the environment, precautionary measures should be taken even if cause and effect relationships are not fully established scientifically . . .[The] proponent of the activity, rather than the public, should bear the burden of proof"	Strong: Clearly places the burden of proof on the proponent of an action to show that it does not pose a danger of environmental harm

according to the available information and current practices. With ethical principles it is well recognized that for instance the protection of human dignity sometimes calls for a certain measure of paternalism (e.g. when institutionalising certain patients) while paternalism in other cases might be the direct opposite of respect for human dignity. This is quite similar to precaution. In order to protect for instance the biodiversity of a given region it may be a wise measure simply to leave a disturbed or polluted river leading into this region to its further natural course, and stop all kinds of human interaction with the river. But in some cases it may rather be indicated to take active steps to bring this river back into a quasi-natural state again, e.g. by restocking fish species, reducing its salinity etc. We need to look at the case at hand in order to find out what precaution means in that specific case. Partly this is due to the complexity of the scientific facts that we need to relate to. But partly this is also due to the varying interests and values that enter such a case. Typically there will be competing interests (aside from e.g. biodiversity) at stake, and sometimes these interests deserve special attention (e.g. to preserve some cultural diversity by providing the economic basis for some human settlements). While the precautionary principle can remind us of our moral duty to prevent harm in general, it cannot prescribe what kind of sacrifice we should be prepared to make in each and every case. Thus the precautionary principle has the semantic status of a general norm rather than that of a detailed step-by-step rule of operation. It follows from this that it may make its occurrence in the guise of a multitude of different formulations and goal expressions.

Despite the differences in the wording, there are several key elements that most definitions or mentions of the PP in treaties have in common. These are, according to (COMEST 2005):

- "The PP applies when there exist considerable scientific uncertainties about causality, magnitude, probability, and nature of harm;
- Some form of scientific analysis is mandatory; a mere fantasy or crude speculation is not enough to trigger the PP. Grounds for concern that can trigger the PP are limited to those concerns that are plausible or scientifically tenable (that is, not easily refuted);
- Because the PP deals with risks with poorly known outcomes and poorly known probability, the unquantified possibility is sufficient to trigger the consideration of the PP. This distinguishes the PP from the prevention principle: if one does have a credible ground for quantifying probabilities, then the prevention principle applies instead. In that case, risks can be managed by, for instance, agreeing on an acceptable risk level for the activity and putting enough measures in place to keep the risk below that level;
- Application of the PP is limited to those hazards that are unacceptable; although several definitions are more specific: Possible effects that threaten the lives of future generations or other groups of people (for example inhabitants of other countries) should be explicitly considered. Some formulations refer to 'damage

or harmful effects', some to 'serious' harm, others to 'serious and irreversible damage', and still others to 'global, irreversible and trans-generational damage'. What these different clauses have in common is that they contain value-laden language and thus express a moral judgment about acceptability of the harm;

- Interventions are required before possible harm occurs, or before certainty about such harm can be achieved (that is, a wait-and-see-strategy is excluded);
- Interventions should be proportional to the chosen level of protection and the magnitude of possible harm. Some definitions call for 'cost-effective measures' or make some other reference to costs, while others speak only of prevention of environmental damage. Costs are only one consideration in assessing proportionality. Risk can rarely be reduced to zero. A total ban may not be a proportional response to a potential risk in all cases. However, in certain cases, it is the sole possible response to a given risk;
- There is a repertoire of interventions available:
  (1) measures that constrain the possibility of the harm;
  (2) measures that contain the harm, that is limit the scope of the harm and increase the controllability of the harm, should it occur;
- There is a need for ongoing systematic empirical search for more evidence and better understanding (long-term monitoring and learning) in order to realize any potential for moving a situation beyond the PP towards more traditional risk management." (COMEST 2005)

It was on the basis of these common elements that the COMEST-UNESCO working group suggested a new working definition of the PP. The suggested definition is this:

Precautionary Principle, a working definition
When human activities may lead to *morally unacceptable harm* that is scientifically plausible but uncertain, actions shall be taken to avoid or diminish that harm.

Morally unacceptable harm refers to harm to humans or the environment that is

- threatening to human life or health, or
- serious and effectively irreversible, or
- inequitable to present or future generations, or
- imposed without adequate consideration of the human rights of those affected.

> The judgment of *plausibility* should be grounded in scientific analysis. Analysis should be ongoing so that chosen actions are subject to review.
>
> *Uncertainty* may apply to, but need not be limited to, causality or the bounds of the possible harm.
>
> *Actions* are interventions that are undertaken before harm occurs that seek to avoid or diminish the harm. Actions should be chosen that are proportional to the seriousness of the potential harm, with consideration of their positive and negative consequences, and with an assessment of the moral implications of both action and inaction. The choice of action should be the result of a participatory process.

This definition has the advantage of formulating the PP on the basis of positive criteria, including the demand that the possible harm referred to, in spite of being uncertain, needs to have some scientific backing. Furthermore, it allows for a wide range of precautionary actions, provided they appear effective in order to either avoid or diminish the possible harm. This answers the criticism that the PP is too narrow a tool for innovation policy as long as it only provides the "go" or "no-go" options.

Anticipating surprise requires measures that are robust against uncertainties. A key concept here is *resilience*: the capacity of a system to tolerate disturbance without collapsing into a qualitatively different—usually undesired—state. One definition of the concept states that resilience is "the ability to prepare and plan for, absorb, recover from, and more successfully adapt to adverse events" (The National Academy of Science, here cited after Linkov 2014). For instance, a resilient ecosystem can withstand shocks and rebuild itself when necessary. Resilience in social systems includes the capacity of humans to anticipate and plan for the future, and to adapt to inevitable unanticipated conditions. Humans depend on ecological systems for survival and their actions are continuously impacting ecosystems from the local to the global scale. Resilience is a property of these linked social-ecological systems.

The PP implies a need for trans-disciplinary approaches to science and policy. Science for policy in the face of uncertainty requires new trans-disciplinary contacts and integration (internal extension of the peer community) on the one hand, and new contacts with policy makers, non-governmental organizations (NGOs), industry, media and the public (external extension of the peer community) on the other hand to meet the challenges of quality control in the assessment of complex risks.

Choice of Precautionary Strategies

Once one has established that the Precautionary Principle has to be applied, one faces the question of what to do about it. How precisely shall we act (including refraining from acting at all)? What measures should be counted as precautionary in some sense? This is the important question one has to address once the above conditions for the application of the precautionary principle are met. It is normally at this point that differences of opinion loom large.

Any action that can be assumed to effectively reduce the risk of the potential harm occurring, or that may contain the scope of the harm should it occur and that prepares us for handling the potential harm could be counted as a precautionary strategy. Given such a characterization of a precautionary strategy, it seems clear that in most cases we have to select among a whole range of precautionary options. Choosing a strategy invariably involves taking a stand on basic value issues.

The EU Communication on the PP (2000) specifies a number of constraints on possible PP measures:

- non-discrimination (between identical problems in different areas),
- consistency (of policies),
- cost-benefit analysis (needs to be considered for action and non-action),
- proportionality (of measures in relation to possible harm),
- examination of scientific development (even after implementation),
- burden of proof (on those who propose a practice).

Kaiser argued in a paper (1997) that once it has been established that the Precautionary Principle should be applied, one is still facing a multitude of possible precautionary strategies. There is no one best strategy in any objective sense. One may for instance opt for a go-slow (step-by-step) strategy if one believes that the decomposition of the innovation into analytically small steps and then testing these, is improving the safety of the operation. Or one may try to restrict access to the innovation only for selected experts under heavily controlled laboratory conditions, like for instance in some virus research. Or one may demand the development of environmental technologies that effectively control effluence to the environment. One has to make trade-offs, for example between effects on nature and effects on society. This is certainly legitimate, but it is not a question of straightforward science. It is a value decision.

4.3.2 Governance of Scientific Policy Advice

Gerd Hanekamp

4.3.2.1 Governance

Interdisciplinary research in a transdisciplinary context most frequently aims at giving advice for political decision-making. This, however does not mean that it is confined to giving advice exclusively to politicians nor does it mean that advice is given solely to everybody officially involved in the ‚political system', to the exclusion of those outside the system. The scope of the addressees rather has to include all those involved in political decision-making in the sense of *governance*, i.e. the context of deliberation and decision-making which includes formal *and* informal processes and institutions. The term governance stresses the wider scope of decision-making processes relative to 'traditional' policy-making.

In order to specify what governance is intended to mean, let us assume a simple picture of democratic representation: A representative party is elected by the people and a government is formed which executes the policies based on the legislation of the representative body. The citizens, not being representatives, do not take part in the political process in between elections. Seen in this way, participation is exclusively representative and formal. The political process is unbiased and professional. Scientific policy advice (SPA) is exclusively concerned with policy *issues*; the scientists are external to the policy-making-process.

The picture just sketched is a high-school textbook version of representative western democracies. But, of course, there are also associations, NGOs, civil action groups, the media, public opinion, to name the most important extraneous factors, which are missing in this scenario. There is lobbying, there are demonstrations, there is civil disobedience, public opinion sets its own agenda, certain events are covered by the media, others are not, et cetera. The picture is, in fact, far more complicated. Not only are the elected representatives involved in policy-making but also anybody who is interested and willing to invest time and money. This engagement can be temporary or long-lasting, it can be expensive in terms of time and money or inexpensive, it can be conventional or unconventional, et cetera. All forms of participation described by the terms: representative/direct and formal/informal are present (for an analysis of current representative democracies see Ezrahi 2012).

4.3.2.2 Legitimacy

It is more complicated not only because of this multiplicity but also because questions of legitimacy arise that were taken for granted in the simple picture initially described. In the latter case, questions of legitimacy exclusively concern the representative system, generally installed by a constitutional act. In fact, the legitimacy of all actions aiming at influencing the political process has to be discussed. For this there are two common paradigmatic approaches that can be characterized as elitist or participatory, respectively. The elitist sees the extraneous influences as disturbing the political process, for the participationist they are its basic elements. The elitist sees the system as formally established and refers to the qualified specialists who do their work within the government. The participationist is driven by the idea of direct democracy, where everybody should have a say about every policy issue.

Both positions, in their extremes, neglect certain traits of modern mass democracies, such as the structural transformation of the public sphere, the size of their population, the heterogeneity of customs and beliefs, the complexity of certain policy decisions, the motivation of citizens to actually participate, et cetera. Under these conditions it is clear that we need specialists in policy-making, on the one hand, but that we cannot expect citizens to sit still and do their personal book-keeping until the next election, on the other hand (Rödel et al. 1989).

As elaborated above (Sect. 4.2), public participation is wanted and already inscribed in many regulations. Also the elitist understands that today the acceptance of policy-making decisions is tied to public participation.

4.3.2.3 Participation

In order to be able to discuss questions of participation, the starting point should be the formal principles of public decision-making at work (cf. Hanekamp 2001). To take part in these processes can be called *democratic participation* and is the foundation of the western democratic polity. Policy making is positioned relative to this framework. Policy makers decide relative to a certain body of knowledge deemed relevant. They lead fictional discourses in order to arrive at a decision since it is impossible in a modern liberal democracy to actually involve everybody concerned in each and every decision making process. Nevertheless participatory elements might come into play here—e.g. because the 'risk of fictional discourses' becomes greater since decisions are made under varying degrees of uncertainty. This type of participation can be called *administrative participation*. Provided that the institutionalization of decision making in large democracies of the western type is adequate—for the general subject cf. Di Fabio (1994), this type of participation has to be judged relative to the arguments which support the former. If e.g. the relevant group, i.e. the group affected by a decision, is relatively small, and persons involved are affected in the same way, this type of procedure might be sensible. On a larger scale, though, the questions of the relevant public come into play (cf. the classic Habermas 1962; for the discussion within the context of scientific advice cf. Renn et al. 1997; Felt/Fochler 2009; Felt et. al. 2012). It is important to specify who exactly is becoming engaged in a participatory method.

For the sciences the awareness of the researcher's actions' influence on the results substantially changes the conceptual framework and participation becomes an important issue within the social sciences—*research participation*. As a side effect the border between the social and the social sciences blurs due to the social-constructivist view facilitating a 'take-over' of the social and thereby the political by the social sciences. As a consequence an immediate status is attributed to their results and it is sometimes difficult to decipher the relevant arguments in terms of the particular types of participation and their legitimatory status involved. The boundary between the researcher and the political activist that was mentioned above (Sect. 4.2) has to be specified although it is questionable whether this boundary is ever a sharp line once the researcher has entered a political discussion as advisor except when he simply delivers answers or recommendations without further participating in the following discussions.

Robert Goodin has developed two concepts of deliberative democracy exploring the conceptual space between elitist and participatory approaches that are helpful here: “Democratic deliberation within” (Goodin 2003) and “Micro-deliberation” (Goodin 2008). Both are meant to spell out what democratic deliberation can mean in mass democracies where the direct consensual resolution of problems is not feasible. He argues that instead of simply counting votes it is better to develop approaches that approximate a deliberative resolution as closely as possible. One way is to personally approach a problem or challenge from as many perspectives as possible: Democratic deliberation within—a virtue for political decision makers and their advisors. The other is to see the different kinds of procedures of administrative participation as discovery procedures where public opinions are spelled out. These contexts can only be discovery not decision procedures because they are necessarily limited in scope and participating parties. A multiplicity of discovery contexts—some administratively induced others organized by public actors themselves—together with other voicing of public opinions Goodin suggests to integrate in a concept of network accountability that representatives relate to in their daily political business. Micro-deliberations are thus not instruments of policy-making or participational scientific advice; they are constitutive elements of current representative democracies.

The model of the formally responsible decision maker who relies on scientific advice directly given to her is thus substantially extended. Likewise, as described in Sect. 4.2, the scope of scientific advice is widened.

4.3.2.4 Scientific Policy Advice

But even in the governance view, however, where informal aspects are explicitly taken into account and public participation is mandatory, the formally responsible decision-makers are dependent on commissioned scientific advice. It therefore makes sense to differentiate two types of SPA: public and non-public whereas the latter follows the track of the instrumental approach (the left side in Fig. 4.3). If e.g. environmental standards are supposed to be set the decision maker will want to know what happens to somebody who is exposed to a certain dose of a particular chemical. She will want to know which effects a certain environmental standard will have on enterprises that rely on the use of the chemical et cetera. The sociologist is supposed to tell her what people presumably think about the issue at stake and the philosopher will tell her which implications certain argumentational pathways do have. All this follows the line of a non-public relationship between politics and science and is necessary for the decision maker to get clear about the problem the decision is supposed to take care of. It is clear, however, that the scientific effort must not be understood as a reliable anticipation of what will happen in the public arena once the issue is on the agenda. Social robustness cannot be constructed and the public discourse cannot be domesticated.

This is valid not only for non-public SPA but also for the public type although the public approaches appear to have integrated the public discourse. One reason for the misunderstanding that public discourses can be “organized” and the results integrated in SPA, is a blurring of the border between the social and the social sciences alluded to above. The corresponding endeavour appears to be overloaded by incorporating all three kinds of participation or at least by not clearly differentiating the three corresponding aspects. The arena to reach decisions is the one described as governance above with the legitimized institutions as core elements. The relationship between science and politics in public SPA is ideally organized as a steady dialogue with varying partners in order to achieve answers which are pertinent and in the particular context.

Scientific advice for policy decisions has to provide the relevant scientific results—relevant in terms of the policies under discussion including their implementation and relevant in terms of the political/strategical context (cf. Sect. 4.1). Scientists involved in SPA have to be aware of the context as a whole. They have to transcend their focus on the formal decision-making process. Since scientists or groups of scientists are at work here, Heather Douglas’ distinction between the direct and indirect role of values in science and her restriction to the indirect role as a regulatory element in SPA are pertinent after all (cf. Sect. 2.2.1.5).

SPA in this perspective is not only concerned with *issues* but also with *procedures* and thus with procedural design and procedural assistance. The more traditional tasks of providing specific knowledge and the ‘public opinion’ about particular issues are widened and so, therefore, is the horizon for the assessment of SPA itself. Scientists enter or are drawn into contexts of governance whose questions are the starting point of a transdisciplinary scientific endeavour. This becomes clear reviewing the example of the planning of a new airport in Norway given above (Sect. 4.2). This example shows that it is important to follow the discussion one has provided scientific input for. Robert Hoppe (2011) convincingly elaborates a governance approach to scientific advice that stresses the importance of asking the right question in an adequate framing from the start in order to avoid what he calls the “wrong-problem problem” (cf. Sect. 4.1).

Thus a key element for the assessment of SPA is the relationship between science and politics. According to Yaron Ezrahi (1990) this relationship has within the last decades changed substantially. His analysis is in line with the picture presented so far: Politics can no longer rely on scientific advice based on publically certified knowledge to buttress policies as adequate instrumental actions; it cannot rely on an ‘attestive visual culture’ (274), a culture exemplified by the public lectures of the traditional academies of science. Instead, science and politics are today entangled in a “reflexive orientational culture” (286). Science cannot provide an obvious truth and politics cannot rely on an obvious good. A reflexive orientational culture has to deal with epistemological and social “uncertainties” and the roles of actor and observer are not statically distributed. Hence the public cannot be seen as a mere observer any more ensuring adequate action in the political arena. This is the social in western liberal democracies with a public which is involved in political actions not merely observing them. An analysis that is coherent with the governance view on policy-making.

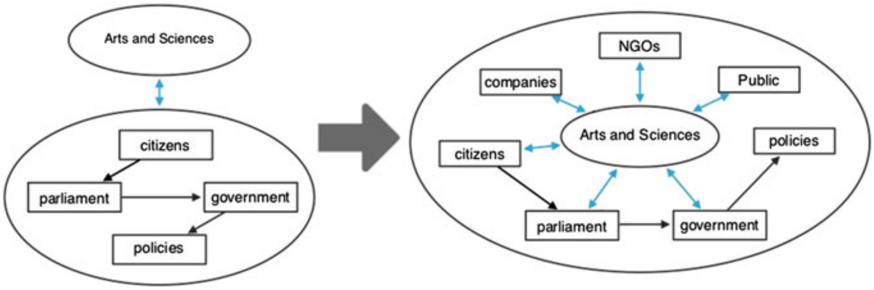

Fig. 4.3 Communication modes of arts and sciences in decision-making processes

Scientific knowledge is not simply *provided* by independently working experts. The experts are part of the game. They are invited to take part and decide to take part or not to take part in the decision-making process. The diagram above shows what this step means in terms of communicational complexity (Fig. 4.3):

Ezrahi (2012) suggests that current democracies are oftentimes not able to handle this complexity: “Contemporary democracy […] is a system of government in which public policy consists of an eclectic patchwork of half-baked programs, where politicians tend to posture rather than act, where the public sphere is more a site of shifting amorphous moods than a clash of ideas.” (ix).

In this indeed frequently confusing picture it might help to structure situations according to their legitimatory and participatory status and to view the scene starting with the formal institutions of modern democracies. What does this mean for scientists and scholars who attempt to invest their expertise in political advice? What does it mean for the public system of research and higher education?

In Sects. 2.1 and 2.3 above the primary perspective was scientific and the concern was to preserve the epistemic integrity of science. From the perspective of the decision maker or the citizen participating in the governance process this concern might appear presumptuous. Epistemic hegemony is a claim that is not granted in the deliberative procedures described here. It is not sufficient to have the better argument (acceptability), the argument and its presuppositions have to be accepted in these discussions (acceptance) (cf. Hanekamp 1998, 2004, 2006).

A scientist or scholar can personally decide whether she wants to get engaged in endeavours of scientific policy advice. The arts and sciences do not have this choice. They take responsibility to provide advice in varying contexts and form institutions that are organizing scientific policy advice. Oftentimes the national academies of science are involved and struggle to manage their thus widened mission (cf. Grunwald 2008). What they would have to manage is the transition depicted in the above display.

What especially needs further attention—and this appears to be the missing link to a reflexive orientational culture—is the role of the public for the relationship between politics and science starting from the role it has for the governance of political decision-making at large. Here the analogy to the status of NGOs is helpful. The influence of NGOs on policy-making, e.g. in the environmental and developmental sector, relies on the support they enjoy in the public sphere (Habermas (1992), S. 399ff.). The public sphere temporarily lends “legitimacy”, it mandates the NGO to influence the political process according to their particular position. NGOs are lobbyists for their supporters.

The public sphere is the arena for articulating an opinion or for forming an opinion where a multiplicity of public opinions can be observed. Coherence most likely is a local phenomenon. *The* public opinion is very unlikely to happen in contrast to its frequent use in political and public discussions.

It is astonishing that there is no reliable relationship between the sciences and society or the public. The relations existing are best described as heteronomous: public understanding of science, participatory methods in technology assessment, administrative participation, research participation—the opportunities to participate are conceded to the participants or even: the complicated issues of the sciences are didactically simplified to fit the limited horizon of the public. In the governance perspective developed here any organization that is engaged in SPA should address the public. It is not sufficient to present recommendations, it is not sufficient to find a common language and a common standpoint in an interdisciplinary group of scientists, and, consequently, it does not suffice for those engaged in SPA to be good or even the best scientists in their discipline.

Apart from the personal traits discussed in Sect. 3.2 there are consequences for an institution that attempts to organize SPA projects: It should be independent, work transparently and consider all available evidence, publish this perspective, including the complete material produced by the expert group as well as the peer and stakeholder review, and recommendations for the policy decisions at stake. It has to present the results to policy makers and the general public, be prepared for a public discourse on these results and conduct a continuous follow-up.

An organization of this kind has to work independently in analogy to NGOs in the public sphere in order to find acceptance as one important element of the dialogue between politics, the sciences and society. Its “projects” are long-lasting, accompanying the political and public discussions. These projects do not have to integrate the stakeholders of the political discussion trying to simulate it by whatever instrument although they include their perspectives. An organization of this kind finally would have the right, capacity, and aspiration to put issues on the political and public agenda.

4.3.3 Communication Between Science and Society

Carl Friedrich Gethmann

Dyadic patterns of perception, such as those forming the basis of the phrase communication and interaction between science and society, often lead to seeming paradoxes due to incorrect conceptual explication. In the given case, the error becomes immediately clear if one explains that the sciences are part of the society and not opposed to it as an *aliud* like, for example, nature. Therefore, it involves a communication and interaction relationship of society with a part of itself. The talk of system and subsystems therefore seems reasonable.[18] Formulated in the language of set theory; it is not about the relationship between two sets, but rather between a subset and the totality. Consequently, the task of explication is to make it clear where the characteristic of science is in respect to the characteristics of other societal subsystems (political system, education, law, economics, culture, art, religion, etc.). The description of such characteristics depends on the historical and cultural circumstances of the relevant subsystems that relate to science, and also on the self-understanding on the basis of which science is pursued. The interactions between the social subsystems can be consolidated in turn through various forms. In the following, this should be explained in two directions after a clarification of the fundamental relationship of (modern) science to the surrounding society: in regard to the relationship between science and other subsystems in the framework of the scientific policy advising and in regard to the role of the media in this interaction relationship.

4.3.3.1 Science and Society

The famous dictum of knowledge as power probably represents the greatest possible consolidation of the statements that characterise the difference between the ancient-medieval and the modern understanding of science—that modern understanding of science in which we are all still involved in. The dictum comes from Francis Bacon's *Novum Organon* (Bacon 1620; Schäfer 1993), and this *new organon* is antagonistic to an old one, namely that of Aristotle, which was definitive for the science of Antiquity and the Middle Ages (Mittelstraß 1970). The point of this dictum becomes clear when one realises the distinctions that are implicit in it. This dictum involves two differences that should reflect a two-fold transformation. *On the one hand*, (a) there is a transformation of the recognition style that one can summarise with the phrase 'contemplation versus intervention': The contemplation of nature is replaced by intervention in nature. *On the other*, (b) it involves a transformation of the science forms of poietical and practical knowledge, which stand in relation to each other. Otherwise, both distinctions are not new; however, the relationship between them, as Bacon defined it, is new.

(a) According to the antique and medieval interpretation, knowledge can only be produced by a contemplative mind-set, i.e. a mind-set dedicated to inner reflection with respect to nature. Plato provided a concise reason. There can only be knowledge from the general and unchanging; the world around us, however, is particular and changeable, therefore we can only obtain knowledge through

[18] An analogy can be found in the question of the relationship between the brain and the body: the brain is a part of the body, after all.

contemplation of the general. In contrast, modern science, whose roots, of course, lie in the late Middle Ages, is persuaded that causal or conditional relationships are recognisable only through intervention in natural processes. The paradigm of this transformation in the style of recognition is the experiment that of course—contrary to a widespread *on dit* with respect to the *natural* sciences—knowledge is recognised not in nature, but rather in a culturally produced place of human labour—the *laboratory*. Between a contemplatively acquired knowledge and the power—the *potestas*—better translated as 'capability to dispose' than 'power' over nature, there is no direct connection. And thus into the late Middle Ages we also see virtually no systematic connection between knowledge and poietical—i.e. producing—control of nature, for which we often use the term 'technology' today. Only by means of knowledge due to intervention in natural processes is it possible to achieve the alliance of knowledge and engineering, which is a characteristic of the modern era.

Capability to dispose appears in two places with respect to knowledge, *once* as a consequence of knowledge. He who finds out the reasons in nature through the skilful isolation of causal factors can also prevent the processes in principle: he gains *potestas* over nature. This form of power comes in a certain sense from an adjustment to the nature. Bacon speaks of a kind of obedience with respect to nature; but it must not be forgotten that this obedience is only possible because there has been interference in nature beforehand. *On the other hand*, however, we gain capability to dispose over nature through intervention in it only by possessing certain know-how, and this aspect is frequently overlooked. The power of control is not only a consequence, but rather also a condition of knowledge. That this circumstance is often overlooked can be seen, for example, in the usual relationship between engineering and science. It is known that scientifically-based engineering is a consequence of knowledge, and it may be accepted that engineering is an applied science in this respect. But engineering also precedes knowledge. Without a real-life available elementary processing of the body and skill in handling elementary device (all the way to measuring equipment), there are no laboratories, no experiments, i.e. no interventions. In this sense, science is also applied engineering, and that is why scientists value inventors so much. Therefore, Bacon is probably the first who particularly appreciated the role of the inventor for science. If the know-how is a consequence of certain knowledge, but also possibly a condition of the next knowledge, then one can imagine that a type of connection, a chain of forms of knowledge emerges, all connected and tied to each other through certain skills in the handling of equipment and in intervention into nature—a connection that chains itself to 'progress'. And in this sense, progress is a specific characteristic of modern science. It is not that there would not have been something new in Antiquity or in the Middle Ages from time to time, but that the backdrop of the interventionist style of recognition allows for a knowledge continuum of action that makes the acquisition of knowledge planable. 'Knowledge is power' also means: progress can be planned.

(b) A second significant difference enters into Bacon's dictum, and it has also been common, in principle, since Aristotle. It is the difference between the *poietical* (the producing)—and the *practical* (concerning interpersonal relations). The determination of their relationship is also new here. Aristotle provides the definition that poietical knowledge has its purpose outside of the recognising actor, e.g. in connection with the production of an artefact, while practical knowledge has its purpose in the actor himself. Practical knowledge is the knowledge that one needs in order to advise a friend, in order to organise a society, e.g. through the creation of jurisprudence, or what one needs in order to practice healing. Bacon's dictum now connects these two aspects in one peculiar way. The poietical, the technical-scientific knowledge that is obtained through intervention in natural processes, serves a practical purpose that lies in the discovering actor. Somewhat generalised: it is used for the liberation of people from natural and social constraints. And Bacon presented us with such a world—freed from natural and social constraints, in his utopian travel novel 'Atlantis', one of the early modern utopias. According to Bacon, modern science is not without any purpose, but rather tied to a general human purpose. In a successful and favourable case, it allows for the achievement of controlling knowledge, which is necessary in order to cause the release from natural and social constraints.

The contemplative understanding of knowledge did not by any means die at the end of the Middle Ages with the rise of the modern age, but rather it also continues to shape the verbal self-portrayal of natural scientists today, however not—fortunately—their actions. Especially in their often cited speeches, natural scientists like to take up the platonic *topoi* of knowledge as reflection on the truth, of a theoretical form of life shaped by noble motivation such as curiosity in order to weave a self-understanding for the expression 'basic research'. This requires that the scientist leads a life of purely cognitive motivation and that a person who is committed to knowledge and truth embraces a particular form of existence, a *vita contemplativa*, a *bios theoretikos*. The natural scientist is supposed to generate his work energy solely out of his individual curiosity—not out of greed, honour or any other motive. And therefore, the natural scientist also has nothing to do with the consequences of the knowledge, i.e. with that which other, so-called users, do with his findings. This self-interpretation of science misjudges what one can call the *practical sense* of modern science.

However, one must also see that the virtually residual insistence on a contemplative understanding of science has easily understandable motives. The understandable motives for the defence of the idea of a science without a purpose can be seen, for example, when one keeps in mind the post-war discussion in Germany, which continues to shape the self-definition of the universities. It is obvious that the political use of science, for example, in connection with the concept of German physics and mathematics, but also, on the other hand, in the case of the Lyssenko biology or the conception of partisan science, explains the defence which had built up against the idea of binding science to a purpose. There is, in fact, a confounding problem between the practical sense of science, as Bacon views it and a political 'finalisation' of the research for particular interests. In this regard, the phrase purpose-free science may be inappropriate, with the defensive against politically particular use not only understandable, but also legitimate. The talk of the purpose orientation of science also produces a feeling of discomfort for many scientists because it reminds them of catchwords such as science as a force of production and thus an attempt to place the sciences in the service of economic interests (whatever they are). However, in the present, we no longer have anything to do with a theory of society or history that uses the sciences practically for particular purposes (through inclusion in the so-called class standpoint). Rather, in reverse, this has much more to do with an economisation of the sciences within the framework of the arena of the market. Research becomes 'innovation', no university curriculum without 'practical relevance'; knowledge is a 'resource', research results are measured as the success of consumer goods, namely by 'demand'. It is, by the way, a curiosity that the neo-marxist interpretation of science as a 'force of production' seems to have been completely accepted analytically—not in an ideology-critical way, but rather affirmatively. With respect to this view of things, it should be noted that science is not subject to utilitarian and particular benefit criteria, but nonetheless serves a—as one could say—trans-utilitarian purpose; it is not to be broken down into group interests, and 'purpose'—in terms of a universal and human purpose—is to be clearly differentiated from (economic) 'benefits'. Science legitimates itself with respect to general human purposes, which also include the purpose of intellectual orientation, but not with respect to monetary benefit.

Above all, there are three distinctions that are not always regarded with sufficient care. *One* is between the *recognising* and the *knowing*, between the recognising as a spatial-temporal and social process and the results of the process, the knowing; *on the other hand*—as already explained—between the *purpose* of the entire enterprise and the many particular *benefits*, the universal purpose, which every scientist mostly sees implicitly, sometimes explicitly, connected, and the many benefits that one can draw from this; finally the difference between the purpose of *science* and the individual motives of scien*tists*; curiosity is a legitimate motive of an individual scien*tist*, but one can very well ask why it should be the only legitimate motive—no individual motive, however, determines the legitimation of science as such.

The interventionist style of recognition in modern science and the practical understanding of knowledge immediately produce the problems that make the relationship between science and society appear so precarious at times. He who says 'power' immediately calls the idea of control to the plan. For that reason, one can characterise the problems that result from the phrase knowledge as power in short as the problems of controlling power. When the knowledge produces power, it is necessary to ask how this power is controlled and may be limited, how science adequately exercises its power and who has the power over knowledge. The founders of modern science, by the way, also reflected upon these questions. However, their considerations appear to convince less and less under the conditions of current scientific and also political developments. On the first question as to how science as a source of power is controlled, the professional modern enlightened science provides the answer: it is controlled and limited by the ethos of the scientists (Sect. 4.3.3.2). On the second question, there is the answer that science adequately exercises its power not e.g. through direct rule like Plato's philosopher kings, but rather through scientific policy advising (Sect. 4.3.3.3). And on the third question: Science is controlled by the media, which exercises control virtually as a representative for society (Sect. 4.3.3.4).

4.3.3.2 Science and the Ethos of Scientists

Firstly, one must talk about control within, since ultimately internal control is unavoidable due to the expert dilemma. Control of science always ultimately means self-control. This entails directly: It doesn't work without the ethos of the scientists. The strength of this ethos has now been questioned, however, by various events in recent years. Indisputably, there is something like a crisis of credibility among scientists. Moreover, by no means is it explained sufficiently in some spectacular fabricated research in the recent past. Rather, these events could only have such a dramatic effect because a more or less latent mood of unease was there, which was reinforced by these events.

The credibility crisis in the sciences has many different aspects that in a sense boil down to one point, however, and generate due to this simultaneous movement the enormous scepticism with which scientists are frequently confronted today, and whoever leaves their laboratory or library in order to hold lectures for the public will encounter this scepticism. In the process, different developments overlap as scepticism-producing factors. *Firstly*, there is the already familiar older scepticism, which is produced by the technical consequences that result from the application of scientific knowledge. Since Los Alamos and Hiroshima, it is clear that scientific knowledge and scientifically based technology are no guarantee of a blessing. Contrary to a popular tradition passed down from Plato, the view has prevailed that knowledge is not good for the sake of itself, but rather only when it serves good purposes. The purposes that knowledge serves are not known in advance by either scientists or, of course, laymen, and it is also not rare for scientists to disguise this.

In addition, however, there has been a *second* phenomenon for some time, namely that we discuss moral problems not after application of the knowledge, as in the case of the atomic bomb, but rather increasingly during the production of knowledge. Since modern science—as shown—is not merely contemplative, but rather intervening science and one obtains causal knowledge only through intervention in natural processes, it can happen that the activity in the laboratory and not after (post) the laboratory produces morally relevant problems. Admittedly, this is for the mechanical laboratory and the balls rolling in it—even if they are very small balls—still not a problem. But the modern bio-sciences are characterised, for example, by the fact that what they do for the production of knowledge is not morally irrelevant. One must only think of the questions of genetically modified plants, of the questions that are connected with biodiversity or the problem of human experiments in bio-medical research in order to explain that the morality of the research is already significant in the production of knowledge and not in the application of knowledge. While the physicists might have been able to refer to the morality of the users, i.e. the politicians and military, which Dürrenmatt (1962), of course did not want to accept with them, the matter in the biomedical sciences does not come down to the technical skills and cognitive competency of the researcher as a producer of knowledge, but rather his morality. The restriction that one may not experiment with humans is one that relates to the production of knowledge and not to the application of knowledge.

Finally, one can speak of a credibility crisis due to the condensed experience in the recent past where scientists from the outset, i.e. on account of selection processes that lead to the scientists existence, may be more intelligent and hopefully more diligent than other people, but are not necessarily more courageous, modest, reliable or, above all, credible. That, in any case, is the suspicion that the spectacular cases of fraud in recent years have also corroborated so unpleasantly. The events are regretted, of course, by almost all scientists; the science organisations have passed codes of conduct and procedures very quickly in order to prevent such incidents in the future.

But is it so dramatic for society that one of their professional groups is affected by a credibility crisis? If installers or taxi drivers have a crisis of credibility, would anyone get so upset about it? The special and dramatic aspect of the fraud cases in science is that the credit of science and thus the self-understanding of a scientifically-technically defined culture is questioned overall by the scientists' crisis of credibility. That is why science must counter a certain fashionable enthusiasm for the rejection of all validity claims and institutions laying claim to validity, which have gripped the sciences since Paul Karl Feyerabend's 'Anything goes' (1976, 1979). The anarchistic philosophy of science set in motion a trend that was reinforced by the philosophy of postmodernism (Lyotard 1979). If, as is claimed here in connection with Nietzsche, validity claims, also scientific validity claims, were solely power claims and thus actually instruments of oppression, then science would have to renounce all validity and truth claims in the name of human freedom. But then one could also no longer condemn fake of scientific results—on the contrary: one would have to welcome it as an expression of the colourful diversity of opinions. The crisis of credibility in science is also seen in those crypto-philosophy-of-science processes that try to make a virtue out of necessity and to describe the crisis of credibility as a form of expression in the slogan 'let a thousand flowers bloom'. Such attacks on the fundamental recognition of scientific validity claims are by no means pure attacks on the future self-understanding of a social group, namely that of scientists, but rather an attack on the fundamentals of a scientifically-technically defined culture that must have a vital interest in the fact that there is acknowledgement for processes that ensure the universalizability of assertions and thus the reliability of scientific methods. However: in the field of scientific research, ultimately only the scientist, even if it is a scientist who advises political institutions, can ensure the control of science, and precisely for this reason, the morality of the scientist is of great significance. But this also means that the scepticism of the public with respect to a number of scientific developments is not easy to eliminate by an improvement in information. The citizen is, of course, not sceptical about the cognitive skills of scientists, on the contrary, he often fears precisely that the scientist will achieve what he announces; rather, the citizen is sceptical about the morality of the scientists. The battle cry of a 'public understanding of science' is therefore also not (at least not alone) the right answer to the crisis of the ethos of research. It involves not only and not primarily the truth of the recognitions, but rather the reliability and truthfulness of the researchers.[19]

As shown above, the purpose orientation of science results in the right of the society to control the creditability of the scientists. However, it is necessary to consider here the by no means trivial objection of whether one can still speak at all of the ethos of scientists in the age of mass communication and what a control of this ethos can mean in an operationalisable way. Usually we orient our ideas of the ethos on situations involving small group interaction. And in this area the control still works reasonably well. Whether a statement is reliable, whether someone keeps his promise or someone tells the truth, can in principle on a case-by-case basis, at least under the conditions of small group interaction, be controlled. Science in the age of 'big science' has long ago become a communication phenomenon that is subject to the conditions of large group interaction. One of these conditions is the anonymity of the actors. Under complex conditions one can often not rely on the habitualisation of certain ways of behaviour. The verifiability of this assumption is, however, largely inoperable. The mass communication society tends to be a society of communication-destroying mistrust, and science must deal with this circumstance. A scientific and technical culture needs strategies that, under certain conditions of mass communication, make it possible to cash in on the expectations with respect to the credibility of the scientists, at least, in principle. This cannot exhaust itself in appeals to scientists that amount to winning back the framework conditions of small group interaction. The virtues of the individual scientist must be supplemented by an institutionalisation of possibilities for review.

This raises the question of how institutional structures should look under the conditions of a mass communication society where functionally these structures may replace direct personal control. Such considerations on the adequate institutionalisation of scientific control appear to fail overall on account of the expert dilemma: ultimately, the scientists, irrespective of the physical aggregate state, judge scientists. However, this type of dilemma is by no means a *proprium* of science; for example, it is, of course, always judges who judge dishonest judges. Therefore, it all depends on ensuring the right form for the control process. Considerations in several directions are necessary:

The *first* consideration is heading toward the non-scientific public. With regard to this, the starting point for all considerations is that science can only perform its practical role when the public is capable of understanding its discursive processes at least in principle and in terms of structure. A scientific-technical culture is only successful if a certain degree of scientific enlightenment is also reality. Scientific enlightenment here means the ability to understand to a certain extent methodologically what is happening in the sciences, but not the accumulation of knowledge, which one may acquire through crash courses in certain disciplines. It does not so much involve material, substantive knowledge, but rather understanding the procedures of scientific forms of recognition.

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[19] However, these remarks should not be misunderstood as questioning any scientific pluralism.

This condition is clearly only the outer surface of science. A *second* consideration is how the ethos of science can be realised in the internal structure of scientific work. And here it is necessary to look back on the scientific professional ethos. Apart from that, it is not only a phenomenon of science, but rather a phenomenon of the society in general, that professional morals lose credit and the state tries to adopt the control of all societal relationships by using a particular instrument, namely legislation. In this respect, one should also promote a kind of division of labour; in society there is a *moral division of labour* alongside the productive division of labour (the economy) and the cognitive division of labour (science). With regard to the moral division of labour, a kind of subsidiarity principle is also to be embraced, according to which the state should not do what the professional organisation can do. Besides this, the legislator remains dependent, both in his enforcement of the law and due to the jurisprudence stabilising the consideration of the law, on the at least partially intact professional morals (e.g. of civil servants and judges). A society that wants to replace ethos completely with law would not be operational. Under conditions of mass communication, it is inevitable that the scientific professional ethos condenses into institutional procedures in cases of particular importance and urgency in accordance with permanence. In so far as these procedures solely function, the state should not get involved in them.

However, a professional ethos can function more or less; its validity can be recognised more or less. And a professional ethos also includes many other conditions and a kind of canon of symbols that stabilise it. We must conclude that in all sectors of society, also in science, the community-creating symbols were dismantled in recent decades without recognisably-new ones replacing them. In total, the de-symbolisation of German science (this is above all a German phenomenon, as everyone experiences who has participated in an academic event at foreign scientific institutions) has not allowed any recognisable emancipatory progress on the other hand. Shedding the robe did not have any enlightening effect; the 'trick' was clearly somewhere else.

A *third* aspect, which is particularly closely connected with the current cases of fraud, has to do with the increasingly intense and complex competitive situation. The failure of individual scientists is also certainly explained (not: excused) by the increasing competitive pressure. It should, however, be acknowledged that the competition is a vital element in scientific research and one of the most important driving forces for the involvement of individual scientists. However, 'competition' in various social contexts means something else. Competition in sports means something other than competition in the economy, and scientific competition is yet another thing. Criticism, therefore, should not focus on the competitive structure of scientific work as such, but rather on the strong tendency of the current science policy to impose the competitive model of commercial competition on science. Research institutes can only be equated with companies to a very limited extent. Research results can only be regarded like commercial products to a limited extent (a successful experiment can be a 'disadvantage'; a failed one a 'benefit'). Accordingly, scientists can only be equated with market actors like producers and consumers to a very limited extent. However, it is to be admitted that science itself has not yet contributed very imaginative ideas to this need for differentiation. It remains a task of the science organisations to defend against an uncritical transfer of economic competitive models by developing science-specific structures that, for example, allow for an assessment of performance that is different than economic measurement processes.

4.3.3.3 Scientific Policy Advising

The question of how science should exercise its power in society on the outside assumes the validity of the Bacon principle. If science were a result of purposeless contemplation, then it would not have any consequences for society and thus would cause no problems, with the exception of those problems that result from the fact that idlers pursuing science need some material subsistence. This is why, of course, the pre-modern scientists and philosophers were usually rich or not in need or were both as members of monastic communities. The power that modern science exercises in this respect should be exercised by it, in accordance with the concept of the founders of modern science, within the framework of a type of policy advising. One should, however, speak of ‘society’ advising according to a proposal by Jürgen Mittelstraß, since the recipients of the advice are, only on the surface, political actors in the narrow sense of the word (cf. Mittelstraß 2010). If science is an element of self-organisation for scientific-technical societies, then we are actually speaking about a form of societal *self-advising* mediated by science. As a result, the relationship between science as power and those who have power and who must legitimate themselves for this in a democratic state becomes especially problematic. The experts for the possible consequences of knowledge, the scientists, are not directly legitimised to exercise power.

This is an outline of the problems that the catchword of scientific policy advising raises. The ethos of the scientists here refers not only to what the scientist himself does as scientist (e.g. careful experimentation), but to what he does in society and where he will also influence and shape societal decisions more or less extensively. In the case of policy advising, it is by no means just a leisure hobby of some scientists or the activity of non-scientific ‘users’, but rather a genuine aspect of scientific work for the fulfilment of the ‘practical sense’ of science. However, this can only be considered a sensible task of science if one follows Bacon, Leibniz, Kant and many other theorists who held the modern understanding of science, according to which it is the task of the sciences to provide not only controlling knowledge (poietical knowledge), but rather also orientation knowledge (practical knowledge).

If one accepts this, however, the problems are only just beginning. Our society is currently not suffering from too few scientific expert boards in which scientists—often alongside other so-called representatives of societal life—provide their expertise for policy. On the contrary, we currently have to deal with the phenomenon that there is an incalculable and otherwise also disorganised variety of boards and responsibilities without a clear mutual demarcation of tasks and jurisdictions. In addition, the border required by the principles of a separation of powers is increasingly being exceeded by such councils of experts. Here, in particular, the trend is moving in the direction of a de-substantiation of parliaments. With regard to such tendencies, it is necessary to consider some postulates that are probably no longer justified as such. They are founded in the idea that science can only exercise its power *modulo* of scientific policy advising successfully if this takes place with consideration given to the requirements of transparency in the procedures, legitimation through expertise and impartiality in the formation of the judgement. With respect to all three postulates, we currently have to combat significant problems (Gethmann 2009).

The *transparency of the procedures* is questioned by the fact that the recruitment of scientific advising boards is completely opaque. Here it should be demanded that the appointment to such boards does not take place through the addressees of the advising, but rather through the institutions of the scientific community, whereby one must ask whether the scientific community has these institutions. The choice of scientists through the addressees of the advising now makes the undeniable impression of courtesy advising.

As regards the *legitimation through expertise*, it is unfortunately necessary to point out that many scientists evidently act very generously with respect to the definition of their own relevance and expertise. Therefore, it is to be demanded that the scientific community has a controlled certification of relevance and responsibility. And for this, too, there are still—at least in Germany—no institutional arrangements.

With regard to the requirement of *impartiality of judgement*, it is from the point of view of the ethos of the sciences scandalous that Enquête Commissions in the German Parliament—as far as the scientist bank is concerned—are filled by fractions in the German Parliament. Therefore, there are CDU scientists, SPD scientists, etc. In this respect, it is urgent that scientist banks in advisory boards are filled by the organisations of science.

If one looks at the current situation of policy advising in Germany on the basis of these three postulates, then one must note in summary that the German science landscape is lacking an institution that monitors the transparency of the procedures, the review of expertise and the impartiality of the judgements, and in the borderline case also punishes misconduct through a *correctio fraterna*. This is, in any case, the part of the problem that scientific organisations can resolve.

4.3.3.4 Science and Media

The importance of science for society is obvious, starting with the fact that it pays for the research done at a considerable expense, but primarily due to the fact that it profits from the allegedly positive consequences of scientific-technical developments (often accepting these as a matter of course), though it also must address the allegedly negative consequences. The public in modern societies is—as one says in an abbreviated way—imparted ‘medially’ (in reality there is not only newspaper, radio, television and the internet, but also many other 'media' through which the societal formation of opinion and will is imparted—such as economics, sports, art, morality, law, religion—but 'media' has naturalised itself as the *pars pro toto*). Many (but not all) journalists who deal with reporting on science are sceptical about scien*tists*. That is their right, and perhaps even their duty, and shall not be questioned here. They express their doubts in their media—an instrument that is available to them not without limitations and as they choose, but nonetheless very easily. This often sceptical treatment of modern scientific-technical developments has, however, given rise to significant unease among scientists for some time. This discomfort may be based on a feeling of helplessness with respect to the 'power of the media'. How can a scientist react if he considers a report misleading or even putting his person in a bad light? In professional journals—and these are the forums in which a scientist usually moves—there is generally a fair opportunity to reply, with the hope of a knowledgeable audience. Readers' letters in daily newspapers on the other hand are known to be a questionable instrument, since editorial departments reserve the right to select and change them in the form of abbreviation. Furthermore, one must also anticipate comments by the criticised journalist. Anyone who engages in a 'media war' meets with the same antipathies as the athlete who criticises the referee. 'Media' is thus wrapped in an aura of sanctity, and anyone who tries to rebel is suspected of not being able to handle criticism or not having understood the value of freedom of the press for a democratic society. Accordingly, scientists' annoyance usually expresses itself with a shrug of the shoulders in regard to 'the communication fringes': at public lectures (usually in the introduction) or talks with colleagues—or in interdisciplinary working groups. From reactions of this kind, three aspects of science reporting in the media shall be addressed in the following without any claim to completeness, namely (a) the selective form of information, (b) the opaque selection of overseers and (c) the essayisation of certain topics.

(a) Selective Information

Scientists repeatedly find confirmation of what everyone should know, namely that the world of the media operates according to a man-bites-dog scheme. The resulting presentation of a subject is often not what the scientist learned in his intellectual socialisation with respect to the methodological sequence, the actual weighting or the epistemological qualification of a subject. Ignorance of the methodology and distortions of the weighting are frequently prevalent phenomena. The misleading epistemological qualifications are particularly annoying. Here, linguistic clichés of political rhetoric are regularly confused with the epistemological *topoi* of science. That one suspects something, often means in politics, but not in science that one is subjectively certain of the thing. That one considers a subject important often means in politics, but not in science that one supports a corresponding position. That one assumes something often means in politics, but not in science that one is convinced of something. That something is likely often means in politics, but not in science that one desires the condition. That two entities are comparable, often means in politics, but not in science that they are (roughly) similar (the result of a comparison can be namely equality, inequality or similarity, therefore two unequal entities are comparable, otherwise one could not determine the inequality). When, for example, a scientist says that addressing the subject of the disposal of nuclear waste is important to him, he must know that his journalist interlocutor understands that the scientist promotes nuclear energy, etc. He should therefore explicitly say that the subject is just as important for nuclear energy promoters as it is for nuclear energy opponents, etc. The difficult issue is the question of who should change his linguistic sensitivity—the scientist or the journalist. Perhaps it is enough if both try to understand the other's language in order to know the sources of possible misunderstandings.

(b) Questionable Overseers

The drama over media presentation seems to include that one can find an opponent to every presented position. This seems to entail the requirement of fair reporting. Since it is known that one can find a person with the title of professor to defend every crazy opinion, one will always be able to achieve this proponent-opponent drama with more or less effort. The expertise of the proponents and opponents is not subject to any meaningful review. For the scientist, however, it makes a considerable difference whether or not someone worked in a field of research in recent years. Whether or not a toxicologist, who investigates certain contaminations of food (and thus is certainly doing something good for mankind), can also make a statement on the question of Leukaemia in certain regions is, however, evidently not checked by the media. This, however, is also a fault of science. While citizens can inform themselves about vehicle experts at any chamber of industry and commerce, there is no scientific institution in Germany where scientists are registered with their research expertise. Scientists tend to characterise their scientific field of expertise quite extensively. Another problem stems from the fact that the academic practices on the part of the media are unknown (or is there a type aversion to dealing with it?). Anyone, for example, who has acquired the title of 'Dr. phil.' is not necessarily a 'philosopher' and should also not be presented in a talk show with a corresponding band; rather, he acquired his certificate in the Department of Philosophy and can discuss the later quartets of Max Reger or the subjunctive in old Icelandic on account of his research. Nothing gives him the ability to professionally handle questions of identity formation between the first- and third-person perspective or of problems of love in the post-industrial society. He who bears a professor's title may be the director of an institute at a university or non-university research institution, but also be a respected industry representatives, trade unionist, artist, churchman or similar due to his professor title. But the fundamental competence question is not everything that a spectator at a talk show, for example, should see answered. The important thing for an assessment of a scientific overseer is also what role a 'professor' plays in his field, whether, for example, he represents the 'prevailing' opinion or an extremely deviant one.

(c) Essayisation of Science Ethics
Questions of the consequences of scientific-technical developments and the ethics of science are, above all, evidently suited not only for dry teaching, but rather they are easy to transform into entertainment (possibly with collateral educational value). That there is a scientific discipline called ‘ethics’ and—as a sub-discipline—’science ethics’, is either ignored or even ridiculed with smug formulations such as ‘professional ethicist’. Often there is also odd friction in the same medium, sometimes in one and the same newspaper: While the scientific research that prompts ethical problems is judged in scientific sections (occasionally enriched by the accusation that scientists have no real sense for the societal problems that resulted from their research), the ethical questions in science are handled in the essay section (*Feuilleton*) within the framework of worldviews, above all religious convictions. Quickly, it becomes impossible to differentiate between whether the scientific (e.g. professional-ethical) discussion is being reported or opinion-driven science policy pursued. The rhetorical hiatus between the natural sciences and humanities (some newspapers even divide up the corresponding sections), which, of course, is not due to something like a fact-based analysis of the kinds of science, but rather to a hasty Cartesian anthropology attributed to *common sense*, also represents a barrier that complicates the scientific-ethical debate according to rational standards.

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One of the frequent self-delusions of scientists may be that they consider it the task of science journalists to report on the scientific developments in an appropriate way, but on a popular level. They hope that science journalists will help generate a general understanding. But it is precisely this service role that most science journalists do not want to accept. Instead, they are explicitly of the opinion or drilled in it that they should defend humanity from the chaos-causing scientists in a way that resembles an agency without authority. They are persuaded that their role consists of being ‘lawyers of the whole’. By contrast, the popular representation of science is the task of the scientists themselves. The scientists must probably accept this self-mandate of many science journalists. To a certain extent, they can also do that since fortunately there are monthly appearing science journals, which can be acquired at any railway station kiosk and make an effort to include appropriate reporting and a popular representation, for example, through elaborate and helpful visualisation. Furthermore, many scientific institutions try to handle the representation through their own publications and, for example, through regularly appearing newsletters. Experience shows, however, that these journals as well as the newsletters from scientific institutions are primarily consulted by scientifically trained readers. How a scientist can reach a large audience and thus ‘the public’ continues to be an unsolved problem.

Chapter 5
Conclusions

Interdisciplinary research is not a fashionable niche within the concert of disciplinary research but a cross-cutting effort with a clear purpose. The appropriateness of the interdisciplinary mode of research depends on the specific scientific questions at stake and the necessary perspectives, descriptions, theories and methods. It should be noted that in the history of science, scientific research was not disciplinary until the early 19th century. From this perspective, disciplinary studies might be seen as special(-ised) cases of research rather than interdisciplinary ones. This involves the reversal of the "burden of proof" from the adequacy of interdisciplinarity to that of disciplinarity.

Today, interdisciplinary research often has an application-oriented societal perspective. It is *trans*-disciplinary in that problems are selected by socio-political forces according to practical significance. Therefore, this type of advisory research not only has to be scientifically sound but also *relevant* with regard to the problem at stake as well as *acceptable* concerning any normative conclusions and their consequences drawn from interdisciplinary deliberations. The validity of and societal trust in trans-disciplinary research rests therefore only partially on the processes and criteria designed to secure quality which are well-established in the sciences and humanities. Actually, broad, sustainable and effective acceptance of and compliance with trans-disciplinary advice depends also (but not entirely) on the ability of the scientists to satisfy the respective addressees in good faith and to take their (implicit) objectives and the relevant contextual knowledge into account. This is an ambitious and complex task, which also leaves room for uncertainty and ambiguity. Although they should aim at overall impartiality and trans-subjectivity in their messages, some individual perspectives and accents are unavoidable—thus leading to different but intrinsically reasonable appraisals of the same matter. The resulting plurality of competing assessments might well explore or exhaust the range of possibilities but could confuse the addressees unless the assumptions underlying the particular assessments have been made transparent.

Based on this view, more specific conclusions can be drawn from this study: The results of methodological reflections from the prior chapters on the constitution of science (Chap. 2), on knowledge and acting (Chap. 3) and on trans-disciplinary deliberation (Chap. 4) are condensed to central insights and recommendations for

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interdisciplinary research claiming trans-disciplinary validity. The following conclusions of the working group are directed towards interdisciplinary research itself (theses 1–5), towards its trans-disciplinary perspective if applicable (theses 6–10), as well as towards its addressees in research and education politics (theses 11–14).

5.1 Interdisciplinarity

(1) The social fundament of joint research is constituted by the mutual recognition of the cognitive competencies of the cooperating scientists.

In interdisciplinary frameworks, corresponding expectations are even more crucial than in disciplinary ones. The authority of researchers within interdisciplinary settings is bound to relatively narrow notions of their disciplinary competence, while some heterodoxy is still accepted in mono-disciplinary frameworks according to the established standards of the relevant discipline. Disciplinary competence and its recognition is therefore a prerequisite for successful interdisciplinary research.

(2) Scientific researchers could—to some extent—influence their respective native discipline.

Disciplines and their borders are not static and not predetermined by their objects alone. Their dynamics was and is influenced by cognitive interests of scientists and institutions as well as by changing demands of the broader society. That means: not only research findings, but also active researchers—although bound to their native disciplines—could (re-)shape the patterns of their professional discipline, at least in the long run (see Sect. 2.3). Apart from substantial disciplinary paradoxes, anomalies and methodological crises, interdisciplinary challenges are the most important stimuli which lead to reconsiderations on the basis of a discipline's methodology.

(3) Interdisciplinary efforts offer added values concerning the self-perception and the power of judgement of the participating researcher within multi-disciplinary settings.

Interdisciplinary discourses unravel the conditions and related limitations of single disciplinary perspectives. Experiencing disciplinary distance and engaging in critical reflection of one's own position (e.g., by the need to argue and to convince the discourse partners) might raise overall creativity and innovative potentials of the discourse participants. At the same time, their individual capabilities to transcend their own constrained disciplinary perspective might be improved.

Nevertheless, critical self-reflection of one's own disciplinary position should not be confused with giving-up a disciplinary perspective of the problem at stake (see Sects. 2.2.1 and 3.2).

(4) Interdisciplinary research requires social competence in addition to excellence on professional levels.

Taking up the cross-disciplinary perspective should not be confused with developing multiple disciplinary competencies in one person, which would be inefficient and squanders the opportunity of a critical cross-checking among different disciplinary views. The relevant disciplines rather need representation in a group of researchers with profound scientific competencies in different disciplines. This inevitably requires communicative skills to elucidate the contributions of one's discipline to members of other disciplines, to recognise approaches of other disciplines and to reach common conclusions (see Sect. 2.3). Within trans-disciplinary contexts, research has to fulfil specific roles and meet expectations that are articulated by the general public, even if they transcend the cognitive interests of the scientists (see Sect. 3.2).

(5) *Interdisciplinary research requires the ability and the willingness to transcend the disciplinary perspectives of each participating expert.*

Disciplinary answers to trans-disciplinary problems always presuppose an adequate reconstruction of the problem within the terminology, ontology, the systematics, models and scales of the respective disciplines. Scientists and scholars who want to serve policy and society by adequate and useful advice, informed and enlightened, but not biased by their discipline, have to ensure that the result of their reconstruction is appropriate to the problem, not just appropriate to the standards of their discipline. This is a necessary precondition for success, where trans-disciplinary problems require interdisciplinary treatment. In order to arrive at a complementary transcending of disciplinary boundaries, instead of competing disciplinary recommendations, an openness for other disciplinary views and a certain degree of understanding other disciplines' aims and strategies is imperative.

5.2 Trans-disciplinarity

(6) Effective interdisciplinary research with trans-disciplinary perspective presupposes sufficient organisational facilities.

Expectations on interdisciplinary research cannot be fulfilled unless critical conditions and provisions are met. Among them, the personal representation from the relevant disciplines and the adequate project duration has to be secured. Both have to be balanced with regard to the trans-disciplinary question at stake and its urgency as well as with regard to the connected funding requirements (see Sects. 2.3 and 3.2).

(7) Interdisciplinary projects with a clear trans-disciplinary perspective should recruit experts with normative competence, too.

The need of integrating normative competence from jurisprudence, economics and moral philosophy arises from the problem-oriented nature of the societal questions at stake and the requirement to give socially acceptable answers. Normative competence is needed when debates on uncertainties appeal to the practical consequences of error. Interdisciplinary discourse should therefore also comprise expertise from jurisprudence, moral philosophy and/or economics.

The alternative use of normative competence in the form of adjunct ethical boards, instead, often suffers from ad hoc or superficial interdisciplinary reflection.

The tight integration of normative competence in interdisciplinary project groups is advocated here (see Sect. 3.2).

(8) Scientific controversies should not compromise the advisory competence of interdisciplinary expert groups.

Scientific research aims more at the revision of knowledge than at its accumulation; research findings are tentative in general. This is why controversies among experts are a normal mode of exploring new ground scientifically and do not constitute a characteristic of science in the public arena. The plurality of different assessments of the same problem is often produced by diverging presuppositions with different associated research strategies. The underlying premises and assumptions should be made transparent to the addressees. In this way, the studies' results constitute a framework of different but similarly reasonable options for making policy decisions (see Sect. 3.2).

(9) Interdisciplinarity in policy advice should aim at explicitly addressing and mapping the scientific uncertainties involved in the assessment.

The process of mutual learning in interdisciplinary assessment tasks involves a special challenge with regard to scientific uncertainty. On the one hand, there might arise the temptation to disregard scientific uncertainty in order to make one's own disciplinary background knowledge appear firmer than justified; on the other hand, precisely because interdisciplinarity involves mutual learning, there is a positive opportunity in the group process to address scientific uncertainties explicitly and to assess their importance for the general recommendations. For the recipient of the policy advice, knowledge of scientific uncertainty is often as important as knowledge about established and agreed upon scientific facts.

(10) Scientific uncertainty on potentially harmful developments paves the way to a possible application of the precautionary principle, which is not to be confused with a principled avoidance of risky activities.

Modern policy advice is usually confronted with the management of significant scientific uncertainty. The precautionary principle is designed as anticipatory guidance for risk management, and as such it is essentially value-laden and ultimately in the responsibility of the decision-maker. This principle is interpreted quite differently worldwide. The authors of this study favour a notion which demands weighing different modes of action in light of both opportunities and risks of both action and inaction. Action and inaction are on a par regarding the need of this assessment, even though they may differ in terms of ethical reflection and normative assessments (see Sect. 4.3.1). The application of the precautionary principle might be accompanied with a shift in the burden of proof from the potentially affected people to the actors.

5.3 Research and Higher Education Politics

(11) Interdisciplinary endeavours need specific support apart from disciplinary funding programmes.

Interdisciplinary research is not simply additive but reflective. It requires adequate framework conditions and enough time, especially for clarification of key terms and their quite different notions across disciplines and sectors, for critical reflection of the disciplinary perspectives involved and for drawing sound joint conclusions. In particular, *appropriate institutional designs* and a framework for sustained research without narrow temporal constraints pave the way to sound conclusions (see Sect. 3.3).

Frameworks with strong interdisciplinary capabilities should be stimulated. This calls also for appropriate funding schemes or even for institutional support for the demanding definition phase of problem-oriented interdisciplinary projects. Interdisciplinary funding schemes should also consider the support of impact assessments of completed projects, which would promote further focussed policy-relevant research.

(12) Sound interdisciplinary research relies heavily on the learnt disciplinary competence of its participants.

The consequence for academic training is that primarily *profound disciplinary qualifications* (e.g., on BA levels) are still necessary—also as sound basis for further post-graduate education, which might either specialise further or even cross disciplinary borders.

The latter would be better conducted at full-scale universities than at technical universities with a limited range of relevant faculties at hand (see Sect. 3.3).

(13) The interdisciplinary aspect of higher education should be fostered in certain faculties.

Some professions like medicine or life sciences do have serious ethical implications and consequences of various kinds. Especially at interdisciplinary graduate schools these ethical dimensions should be made explicit both in the general educational programmes and in the layout of the individual research projects for doctoral students. Suitable training courses in applied philosophy (moral philosophy and philosophy of science) can be offered in the framework of the "*studium generale*" and realised by working out appropriate contextual regulations both in the programmes of the disciplines and in the graduate schools' internal modules.

Although such a 'reflective' element in terms of ethical and methodological consideration has to be studied in general, it should not be put onto the agenda of the study programmes too early, since it can only be useful if some basic knowledge and research practice in the home discipline has already been gained (see thesis 12 above). Furthermore, specialised and contextualised courses in applied philosophy should be offered which fit the more specific research topics of the graduate school as such or the individual research projects of doctoral students in corresponding graduate programmes.

(14) Specialised extensions should make students more sensitive to cross-disciplinary challenges of single professions.

Study extensions towards moral philosophy or philosophy of science cannot teach full expertise in these domains. Instead, they will raise the awareness of students for options and preconditions of interdisciplinary work and its division of labour.

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