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

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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

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[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 subdiscipline 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

[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

[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*.

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