Theory and History in the Human and Social Sciences

Jaan Valsiner *Editor*

Social Philosophy of Science for the Social Sciences

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Theory and History in the Human and Social Sciences

Series Editor

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Chapter 1
General Introduction: Social Sciences Between Knowledge and Ideologies – Need for Philosophy

Jaan Valsiner

Social sciences are crucial in our understanding of the increasingly globalizing ways of living in the twenty-first century. Rapid technological advancements in our societies—"East" and "West", "North" and "South"—are paralleled with resistances by traditional social orders to them. Local social norms and political control systems (Chaudhary, Hviid, Marsico, & Villadsen, 2017) that sometimes erupt as revolts or revolutions (Wagoner, Moghaddam, & Valsiner, 2018) constitute the braking systems in development. Development and resistance to it go hand in hand—leading to tensions in the building of new knowledge.

Societies worldwide are characterized by disquietude in which various kinds of tensions are constantly growing. The "volcano" of our "global society" can easily erupt into a new global war—economic, discursive,[1] or military. The results of such wars are likely to be devastating—but like our predecessors taking the *Titanic* to cross the Atlantic, we might not be aware how our ordinary social life gives rise to such apocalypse.[2] Are we granting quality in science through watching out for the correct uses of established methods? Or are we part of an institutional effort of making the given discipline homogeneous in its methodological practices in ways similar to mass volume production of various consumer products? Is the publication of a research paper in a "peer-reviewed" journal such a

[1] I use the notion of "discursive war" to indicate the clashes of opinions of different ideological backgrounds that do not lead to new ideas but insist of social power dominance of existing perspectives (e.g., fights against "dualisms" in the social sciences which do not lead to solving the problem). In contrast, philosophical look at how to solve such problems would be important.

[2] Georg Simmel (1904) pointed out that wars are being prepared during peace times.

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product or an act of communication with fellow scientists? Why is the "open access" movement in scientific communication—an appealing idea for any science (see Wenaas, 2019 in this volume)—becoming a battleground of fights between various institutions? What can be the stake of a political establishment—a US president declaring a decade as that of the study of the brain or European Union requiring specific "breakthroughs" in its Research Council's science funding programs—in the actual processes of scientists' intellectual endeavors in trying to create new knowledge? What is the value of university administrators who expect scientists to bring in research grants with "overheads" in the actual making of new knowledge? These are difficult questions on the border areas of real *Wissenschaft* and the socio-political administration of science. This volume will provide some answers to these questions and raise some new puzzles.

Society's Suicide: Reliance on Opinions

Gaston Bachelard back in 1938 pointed to the paradoxical role opinions (in his terms these are" dead thoughts") play in human knowledge:

> Opinion *thinks* badly; it does not *think* but instead *translates* needs into knowledge By referring to objects in terms of their use. **It prevents itself from knowing them**. (Bachelard, 2002, p. 25, added boldface)

How can an opinion "prevent itself" from knowing the objects about which they are expressed? Very simply, my particular opinion "S is P" ("politicians in country X are corrupt") gives a taken-for-granted characteristic (P = "corrupt") and attaches it indiscriminately to all cases (S = "politicians" in X), thus not allowing me to inquire into the possibility of some of the S to be non-P or strive toward becoming non-P. Instead of a nuanced view of the field (S) which would allow recognition of variety, I create a totalitarian prejudice against S overwhelmed by P.[3] My opinion—which in other terms is my prejudice—stops my further inquiry into the varieties of S. My political prejudice ("politicians are corrupt", "refugees are terrorists," etc.) pre-sets my understanding in ways that lead to serious absence knowledge and failure to understand rapid changes in the world.

From the perspective of the scientists, reliance on opinions—of themselves, their "peer reviewers," politicians, and "opinion polls"—sets limits for new knowledge. It has the potential to stop further inquiries by creating consensually fortified "black box" explanations that remain in fashion for long times and may become encoded even in textbooks of a given discipline. For example, the notion accepted in psychology since the 1930s that "the scientific" approach to phenomena necessarily involves quantification ("assigning numbers") has led the field to a conceptual impasse (Toomela & Valsiner, 2010) that has neither historical (Porter, 1995) nor

[3] Notice that the opinion here is produced by substitution of "all" for the doubt-allowing "some" ("some S are P"). Bachelard's rejection of opinions equals the reduction of heterogeneous classes of phenomena (fuzzy sets) to their representations as if these were homogeneous classes (crisp sets).

mathematical (Lamiell, 2019; Michell, 1999; Rudolph, 2013) foundations. Its history of entrance into social sciences is well described as an avalanche of the "empire of chance" over the twentieth century (Gigerenzer et al., 1989). It is here where ideological guidance of axiomatics of science can be located. The *social* convention of quantification as necessary for psychology to be "scientific" has overridden the more important question of *what do the quantified data represent*?

That latter question of course makes sense only if the notion of data as signs—which represent something else—is axiomatically accepted. If not, the data and the phenomena become fused into one, and an assigned number becomes a valid data point. We can see exactly at this junction how the social sciences need philosophy—to sort out their axiomatic bases to understand what kind of knowledge is possible in their particular field given the underlying assumptions that are made. Can knowledge in some field of social science (anthropology here is an example) be considered ideally "local" by axiomatic decision—leading to stopping of any efforts to generalize beyond the particular context? The result is the hyperactive production of empirical observations without theoretical innovation and even proud assertion that theories are not needed; we just need to figure out "what is really there" in the social practices of another tribe—be it in New Guinea or backwaters of Norway.

So, philosophy is needed, but what kind of philosophy? Can philosophy of science be that of science only? Or is it embedded in a wider ideological field that governs the given society at the given time? How can it be that some research questions—which are not popular in science at the given time—are not only ignored but *actively disliked*. Darryl Bem's (2011) technically perfect experimental proof of some aspects of human thinking that have parallels with parapsychological research themes of the past have been actively disliked in contemporary psychology (Zickfeld & Schubert, 2019—in this volume). The few scholars who dare to say that there is something valuable in the inquiry into these topics are not just silenced but *vigorously dismissed* in their suggestions. Such affective outbursts of social stigmatization point to the extra-philosophical origins of the philosophy of science presences in the social sciences.

To summarize, philosophy of science at times stops being philosophy and becomes an affective display. What is needed is careful scrutiny of the triad **ideology<>philosophy of science<>science** itself, and that kind of scrutiny can be presented as *social philosophy of social science*—the general theme of this volume.

Social Philosophy of Science: Beyond Paradigms to Sociodigms and Metadigms

Social philosophy of social sciences occupies the arena of investigation that includes *paradigms* (Thomas Kuhn's invention) in their relation with *sociodigms* and *metadigms* (Yurevich, 2009). Sociodigms complement paradigms with practical societal demands for application of sciences:

> The main dividing line between academic and practical psychologies is probably the divergence of corresponding *communities*, which warrants describing them as different

> *sociodigms*—not reducible to Kuhn's paradigms—and making it necessary to go beyond that concept. It has rightly been noted that the academic and practical psychology are like two sub-personalities of a split personality; academic and practical psychologists have different circles of communication and different "authorities", practical psychologists do not know the names of the directors of academic institutions and academic psychologists do not know the names of "star" psychological practitioners. (Yurevich, 2009, p. 97)

Similar functioning of sociodigms in other social sciences can be found in Norwegian sociology (Aakvaag, 2019 in this volume), economics (Lind, 2019 in this volume), and neuroscience (Watzl, 2019 in this volume). The "fashions" that episodically capture a particular field in the social sciences in a particular country are guided by wider societal projects. Such projects may prescribe arenas of applicability to a science based on the general reference to "needs of the society."

The *metadigms* are organizational means of higher order that unify both practices and sociodigms into various kinds of rationalities—"Eastern" versus "Western" thought, or religious versus secular understandings of the world. Within the Occidental tradition, various practices of healing are presented with their scientific bases in focus—while the practices themselves may coincide with those used by grandmothers without any evidence base or are similar to Oriental practices largely embedded in the philosophical and religious frameworks of Buddhist societies.

Functioning metadigms include buffering mechanisms against situations where scientific activities might lead to findings that are damaging to the metadigm. This selectivity may be at the root of difficult transitions of Kuhnian paradigms from "normal science" to its "revolutionary" counterpart. The implicit opinions "direction X is not for science" maintain social barriers that keep a specific domain of knowledge from being investigated.[4]

Secondly, a metadigm can set up dominance hierarchies in the realm of different parallel perspectives in the given science. In the social sciences of the past century, we observe unqualified and doubts-free prioritization of the quantitative research tools over their qualitative counterparts. The latter have been stigmatized as "soft science," while the former have been superimposed upon the study of phenomena which by their nature defy quantification. The result is the same for use of both—the prioritized quantitative approaches fail to produce breakthroughs in our knowledge because they miss the fit with phenomena, and the underprivileged "soft science" perspectives have no chance to provide alternatives other than producing "anecdotal evidence." Even if the prioritization were to be reversed—qualitative approaches set as priority over quantitative ones—the dominance reversal would continue to produce many new data

[4] This boundary defense also applies to the exposure of doctoral aspirants to ideas from philosophy of science. Strand (2019; Chap. 3) mentions a former dean at the University of Bergen, in a research education strategy meeting, remarking "It is OK that Ph.D. students with individual research projects go through these courses, *but we do not really want that Ph.D. students hired on prestigious, international research projects come to doubt their own science*, do we?" (added emphasis). In this admission the role of institutional goals—producing knowledge proletariat of Ph.D. level who is freed from doubts about their science—is clear. What this agenda, if developed further, would mean for innovation in the social sciences is more than uncertain—would it eliminate innovation?

without challenging the basic assumptions on which the research enterprise is based (Branco & Valsiner, 1997). The values of the metadigm are protected—while the scientific enterprise proceeds in its ever-active empirical productivity. The ever-increasing flow of empirical data in the social sciences masks the lack of paradigmatic breakthroughs—a feature that keeps social sciences at distance from having a stake in the politically set work on crucial social problems in a society.

The third buffering mechanism of metadigm involves creating a complex pattern of the paradigm-sociodigm relation on the border of the private<>public disclosure area. The sociodigm side of the relationship involves demonstrations of practical efficiency of the paradigm and its relevance for users in society. At the same time, many aspects of the research process (paradigm side) involve actions that are not directly useful in any aspect of societal living. Their role in the pattern becomes presented as minimized, while other sides of the pattern are presented in their full societal usefulness. Still, as Yurevich points out:

> Imagine what may happen if a psychoanalyst's permanent client who has paid him a hefty sum of money, suddenly discovers that all his actions had been based on myth and metaphor and hunches, and not on solid scientific knowledge; as the client had sincerely trusted. And what if the client, as psychological clients often do, turns out to be an influential and griping individual who is hurt by the very thought that he is being fooled and that his time and money had been wasted, and makes common cause with other such clients? Would it not result in high-profile trials of psychologists as quacks and trigger another witch-hunt. (Yurevich, 2009, p. 102)

The potential for social scientists to become objects of witch-hunts in the twenty-first century is remote—protected by the pre-emptive direction of the topics of scientific investigation in directions that do not entail challenges to the prevailing ideologies that are the core of a metadigm.

The unity of metadigms, sociodigms, and paradigms in science creates the need for a new kind of philosophy of science. In contrast to the traditional philosophy of science built on the notions of classical logic and epistemology and overlooking social sides of the whole thinking about creating basic knowledge, the new version situates the philosophical discourses of science in their societal frames. I call it social philosophy of science—and in this volume it is outlined in our collective effort toward establishing it for the social sciences.

What Is *Social* Philosophy of Science?

Roger Strand (2019) (Chap. 3 in this book) gives a concise answer to the question what is philosophy of science? It is a:

> Subfield of philosophy, in which the presuppositions, methods, structures, goals and impacts of science are examined. (p. 34)

It could be seen as "simply" epistemology—but the focus here is on the examining of the whole array of knowledge creation tools. Any act of examination is a social act—performed from the perspective of some metadigmatic goal-orientation. In other terms, philosophical examinations of the basic assumptions and theoretical constructions in any social science have direction from some metadigmatic point of view.

Back in 1991, we reported the societal conditions in Soviet Union in the early 1930s in our analysis of the work of Lev Vygotsky (Van der Veer & Valsiner, 1991). The expedition that Alexander Luria organized to Central Asia to study the positive impact (metadigmatic positive objective) that introducing literacy would have for illiterate people became—in the course of a 3–4-year period—viewed in terms of negative metadigmatic perspective (as demonstration of "cognitive backwardness" of the "New Soviet Man" building "new society"). The change from positive to negative metadigmatic perspective coincided with rapid political changes in the Soviet society. The data from the scientific side of the Central Asia expeditions remained the same—but their societal value interpretation changed diametrically with the general ideological change of the prevailing rhetorics in the Soviet society.

What can we learn from the histories of metadigm-sociodigm-paradigm relations for the social sciences of today? First of all, caution about the wider waves of fashions and extra-scientific expectations directed toward streamlining the social sciences. The appealing labels of "usefulness" or "evidence base" are complex social dialogues—what is "useful" for one social agent (or agency) may look different from the perspective of another. What is called "evidence" may be a presentation that selectively highlights one kind of ideologically fixed opinion and hides its opposites. The social philosophy of the social sciences is needed to keep us all aware of these background negotiations of the opposite tendencies of social expectations in our opinion-based discussions of what is the valuable next step in our disciplines, how to provide ratings on our peers' grant proposals, how to resist the narrowing of the scope of our investigations by extra-scientific social powers, and—most importantly—how to guarantee our researchers' basic rights to create new knowledge.

The Origins and Overview of the Present Volume

This volume emerges from the regular once a semester series of seminars on Philosophy of Science at the Faculty of Social Sciences at the University of Oslo that took place in 2016–2018. In covering the topic of the traditions of philosophy of science and the social practices of different social sciences in these seminars, it became obvious that a wider international and interdisciplinary volume is needed to support inquiries by social scientists. The seminars were unique as they were supposed to bring together "junior-level"[5] researchers who aspire toward their

[5] The "junior level" in practice involved the whole range of the life course—from the 20s to the oldest participant being 71 during the participation in the seminars.

Ph.Ds at the University of Oslo in the whole of social sciences. Furthermore, the seminar sequence was designated as a "*mandatory* doctoral course[6]" for all doctoral students in the social sciences. The positive result from these encounters over the 3 years of the (total 6) weekly seminar sequences was a productive "snapshot" of the intellectual worlds of Norwegian scholars in sociology, anthropology, economics, psychology, and history.

Even as it started in a narrow—didactically set—context, this book transcends the narrower set of tasks that were covered in the Oslo seminar settings and brings in contributions from all over the world. David Carre (2019) (Chap. 2)—based on the context of Chilean economists relating to their society—discusses what various social sciences give back to the societies that, in its turn, make their further research possible. We presume that commodity is knowledge—but it needs to be elaborated what kind of knowledge becomes appreciated by "the society." It is far from clear what is subsumed under that generic label—especially if it is designated as *the*, rather than *some part of*, society. In Chap. 3, Roger Strand introduces the basics of *Wissenschaftstheorie* in its fullness. The benefit of the Norwegian perspective on the philosophy of science is the preservation of this wider European notion of knowledge construction, rather than accepting the Anglo-Saxon *science* "versus" *humanities* opposition. Sebastian Watzl (Chap. 4) gives us a glimpse into how our occidental metadigmatic worldviews deal with making sense of the brain functions through the lens of cultural dichotomy of gender. The assumptions of strict oppositions—"male" versus "female"—as applied to the brain are blatantly inadequate (Watzl, 2017), yet these are replicated in almost all new investigations. In Chap. 5, Rolf Reber and Nicholas Bullot (2019) suggest a solution to philosophy of the social sciences in terms of *conditional objectivism*, a strategy that aims to guide decision making in the face of value-laden subject matters of scientific inquiry. To explain the heuristic procedure by means of which the strategy provides practical recommendations, they use a decision tree. Anna Zadrozna (Chap. 6) brings the readers to the anthropological world view and reminds us about the dangers of moving our philosophies too far from societal realities.

Causality is a perennial question in the traditional philosophy of science. Raino Malnes (2019) (Chap. 7) provides an in-depth analytic view of the history and ontology of the issue, while Jaan Valsiner (Chap. 8) makes the proposal to follow chemistry in its move from thinking in terms of causes to that of catalysts. Janis Zickfeld and Thomas Schubert (Chap. 9) address the complicated issue of replicability in the social sciences, providing accounts of new practices to guard their field

[6] It is symptomatic of European institutions of higher education in the 2020s to establish institutional organizational forms of "doctoral schools" which bring together under one label (and with mediocre additional funding) various doctoral projects usually funded by different grants (or not funded at all). Such streamlining of the highest level of aspirations toward knowledge I can only see as an example of administrative control over knowledge construction processes. The creation of *mandatory* "courses" at the doctoral level is yet another symptom of such control that sets the whole system of higher education into a crisis about advancement of *Wissenschaft* on the one side and the production of army knowledge workers on the other (Valsiner, Lutsenko, & Antoniouk, 2018).

(psychology) against possible flaws of illusory replication. Henrik Skaug Saetra (Chap. 10) gives a commentary on the issues raised in the previous three chapters with a focus on emergence of new knowledge. The paradox of replicability as a value for science is its contradictory status with creating new knowledge—if something new is found, it is necessarily an example of non-replicability of what had already been discovered.

The social philosophy of the social sciences needs to consider normativity in the realm of the different social sciences. The issue of normativity is worked out in psychology (Svend Brinkmann (2019) in Chap. 11) and generalizes to all social sciences—as we see in the chapters on economics (Jo Thori Lind—Chap. 15) and sociology (Gunnar Aakvaag in Chap. 14). The socio-political issues of "open access" (Lars Wenaas—Chap. 13) give the wider frame for normativity in bringing the results of the social sciences to the audience. Psychology also is an arena for demonstrating how a research enterprise over a century goes wrong under metadigmatic guidance—loss of the person-centered approach in psychology over the twentieth century has accentuated the crisis in the field (Lars-Gunnar Lundh (2019) in Chap. 12). Finally, in Chap. 16 I will sieve through the key new moments that the different perspectives in the volume could contribute to the worldwide discussions of how social sciences could proceed in the age of the rapidly changing context where politicians resort to tweets (Valsiner, 2018) and where short declarative messages expressing metadigmatic opinions act as social anesthetics for our scientifically valuable capacities to doubt the present beliefs and explore new alleys of knowing.

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Part I
Social and Cognitive Roots for Reflexivity upon the Research Process

Chapter 2
Social Sciences, What for? On the Manifold Directions of Social Research

David Carré

In fact, nothing is more conservative than science. Science lays down railway tracks.
And for scientists it is important that their work should move along those tracks.

—Ludwig Wittgenstein (ca. 1946 in Monk, 1991, p. 486)

The following chapter revolves around one simple yet intriguing question: what do social sciences give back to the societies supporting their work?[1] The most common—and certainly not wrong—answer is knowledge: social sciences, through different institutions, artifacts, and practices, provide the social world with knowledge about human and cultural affairs. This knowledge, moreover, ought to be novel, as it is meant to change what was previously assumed—either by revealing new findings or by transforming what is currently taken for granted. Typically, this knowledge should address human affairs that are puzzling or unknown, and therefore demand forms of inquiry other than common sense or first impressions—which, it is assumed, are easily available to non-social scientists. Thus, if we understand social sciences as a counterpart of natural sciences (cf. Dilthey, 1883/1989),[2] the

[1] It is likely that the acute reader have already noticed that this question naïvely assumes that giving back something is actually desirable for science in general and social sciences in particular—thus disregarding the possibility that scientific endeavors should be pursued with as little worldly constrains as possible while the former is in fact assumed, at this point, the reason behind this is properly elaborated along the chapter.

[2] While Dilthey's (1883/1989) distinction between human and physical sciences is usually invoked to affirm that both are systematic endeavors for pursuing knowledge (*Wissenschaften*), it must be noted that the crux of this distinction originally was to make clear that human ("soul" = *Geist*) sciences (*Geisteswissenschaften*) and natural sciences (*Naturwissenschaften*) cannot be possibly compared, due to how differently they seek knowledge—*Verstehen* and *Erklären*, respectively.

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former should aim to make the mysteries of the human world understandable, just as the latter have thoroughly explained the natural, physical world over the last three centuries. An apt example of the former line of reasoning is that, as social research has consistently shown, giving cash money to people living in impoverished conditions is not the golden solution to their struggles—as conventional wisdom suggests. The efficacy of such money transfers actually depends on whether they are conditional or unconditional (Manley, Gitter, & Slavchevska, 2013), applied in rural or urban populations (Fernald & Hidrobo, 2011), or whether they are given to male or female beneficiaries (Stampini, Martinez-Cordova, Insfran, & Harris, 2017). In doing so, this set of social science studies makes a contribution to address pressing social issues, like poverty, in a much more efficient way than our lay ideas would allow us to. Or at least so we are expected to think.

Acknowledging that the vision presented above is problematic in many aspects[3] is not really difficult. Yet it is hard to argue against its main implication, namely, that social sciences are meant to create valuable knowledge for the societies supporting their work. But even if we agree on this point, the question about what social sciences give back is certainly far from being settled. For the notion of (scientific) knowledge hides behind its noble guise many troubling questions (see Collins & Evans, 2002), which are especially thorny for social sciences: who decides which topic should be investigated—scientific communities, funding agencies, or citizenry? Following which methodologies—quantitative, qualitative, or mixed ones? Moreover, what kind of knowledge is desirable? Does it have to have a concrete impact in the world? Or should it instead raise theoretical problems? Regarding their subject, should social sciences only aim to solve pressing issues? Or should they, on the contrary, aim to raise problems where we assume that there is none? And, last but not least, should the knowledge-making efforts have priority funding over physical sciences, or vice versa?

The existence of these many questions would not be a problem should contemporary social sciences—as corpus of knowledge and collective of researchers—have clearly defined answers to them. Yet the case seems to be the exact opposite, as even different research groups within a single social sciences' faculty or department are likely to have different answers to these questions. If we acknowledge the existence of this confusion, it could not be left just as an open question. Therefore, and as the extensive use of "should" in the previous paragraph makes clear, the discussion presented in this chapter takes the subject of this volume—social philosophy of social sciences—necessarily into moral grounds. More specifically, it draws the discussion into explicitly addressing what kind(s) of scientific knowledge should social sciences give back to the societies promoting their research. In other words, discussing what is the purpose of contemporary societies by supporting the existence and development of social scientific research. I am fully aware that even raising this close connection between scientific activity and social interests is contentious for

[3] Particularly the idea that social sciences are ultimately supposed to be nothing more than the counterpart of natural ones, as noted in the previous footnote

many readers—as it apparently reenacts the "anti-science" arguments developed during the so-called science wars (Segestråle, 2000), which presented scientific activity as a façade of objectivity that merely echoed dominant social discourses (e.g., Fuchs, 2000). "Pro-science" defenders, on the other hand, claimed that the problems of science precisely start when this activity is tied or restricted by worldly constrains like political or ideological agendas (e.g., Bauer, 2000); and thus science should develop free from any societal limitations. Instead of swinging to any of the extreme positions proposed back in that intellectual quarrel, in the present chapter I approach the science-society relation through the notion of *guidance* developed by Valsiner (2012).

The concept of guidance is, first and foremost, a reminder that crafting scientific knowledge is—and has always been—an activity embedded in very particular social institutions and historical contexts, as the history of psychology in the United States makes extensively clear (Valsiner, 2012; see also Danziger, 1979). Acknowledging the former is an essential step to understand that scientific criteria are not the only determinants for the paths through which scientific communities develop their research—as it is also a response to contingent social demands and events. Thus societies—through several institutions and policies—*guide* the creation of scientific knowledge towards certain areas and topics, *without forbidding,* nor encouraging, alternative developments (Valsiner, 2012). A contemporary example of how science is socially guided is the funding ban that, since 1996, the Centers for Disease Control and Prevention (CDC)—United States' public agency devoted to address menaces to public health—has to exert over any research projects that aim to address gun-related violence as a public health issue, following the so-called Dickey Amendment (Omnibus Consolidated Appropriations Act of 1997, 1996). In practice, this Congress amendment establishes that no public funding can go to study the causes, prevalence, or consequences of violence exerted by means of a firearm within the United States, as it "may be used to advocate or promote gun control" (p. 244). While this restriction could be assumed as overtly banning that topic of research, it rather states—through an official act of Congress—that the creation of such knowledge represents no value for the country. Therefore, through the funding policy of its public agencies, the United States has guided scientific communities away from studying gun-related violence, thus discouraging yet not censoring research on it. Hence, the pressing need for getting better knowledge on this subject in the United States has had to be almost entirely[4] pursued through private or international initiatives—as the acknowledgments of existing research on the subject show (e.g., Swanson, McGinty, Fazel, & Mays, 2015). Thus the interest of scientific communities and the public for gaining scientific knowledge on firearm violence has not been suppressed—but discretely silenced (Kellerman & Rivara, 2013).

[4]It is important to note that, following initiatives from the Obama administration, in 2013 the National Institutes of Health (NIH)—also a public agency—opened calls for research projects on the causes of firearm violence, effectively funding two projects addressing the consequences of possessing firearms (for a review, see Rubin, 2016). These projects, however, remain as the exception rather than the rule.

As shown above, the notion of guidance (Valsiner, 2012) is useful to understand that scientific activity is tightly connected to the needs and priorities of societies, but not forcefully subsumed to follow them. Thus scientific development is, so to say, socially canalized towards certain paths while nudged away from others. Such orientation, however, is not necessarily exerted from the outside towards scientific communities, as groups of scientists can also make—and usually do—efforts to push their disciplines towards particular directions in order to favor their social positioning rather than scientific merit (Danziger, 1979). More importantly for this chapter, and as the example presented above makes clear, such guidance is neither static nor univocal. On the one hand, the directions in which different disciplines are guided are in constant change along the time, just as societies' interests change too (e.g., the sudden interest in nuclear physics during the first half of the twentieth century). On the other hand, scientific activity can be guided towards many paths at the same time. In the example presented, United States' funding agencies have driven scientists away from studying the mortal consequences associated to owning firearms, while international agencies (e.g., London-based Wellcome Trust) have specifically promoted the creation of exactly that area of research. Therefore, it is indeed possible to identify a dominant guiding trend, yet it is not possible to conclude that it is the only one at play. Nowadays, social sciences seem to be guided (simultaneously) towards quite different paths by different actors: from being the critical reserve that observes societies from afar (e.g., Crandall, 2017) to the social engineers devoted to address the pressing issues of the twenty-first century (e.g., Western, 2016), to the scholars responsible of bringing forth issues that societies have failed to acknowledge (e.g., Foucault, 1961/2006), and to be devoted to discover the basic laws governing human and cultural affairs (e.g., Werner & Kaplan, 1963; Luhmann, 1984/1995). Thus, if social sciences seem to be lost in their purpose, it is probably because they are trying to be guided, at the same time, towards producing quite different *kinds* of knowledge on different, ever-changing *topics* of social interest. Given this scenario, it becomes essential to hold an open discussion about how social sciences should navigate through these multidirectional, dynamic efforts of guidance, as they could lead social research towards quite different purposes and subject matters. The present chapter aims to provide a proper framework for such dialogue by outlining six dominant directions to which social science has oriented its knowledge.

Holding such a discussion is, in fact, long overdue, in order to avoid the rise of polarization among social scientists that—sometimes inadvertently—hold completely different positions (O'Connor & Weatherall, 2018). Far from any metaphysical reflections, discussing about the former is a pressing need for a group of disciplines that has been called into question for different reasons: from being a self-perpetuating and endogamic community of opaque intellectuals (Sokal & Bricmont, 2003) to being second-class disciplines that should move as soon as possible to integrate within natural sciences (Fitzgerald & Callard, 2015) and to being an interesting yet eccentric way of investing public resources that could be done as long as economic circumstances allow (Campaign for Social Science, 2015). Even if all these critiques could be debated and argued against, there is at least one issue that should invite social scientists to question their current purpose, namely, the publishing game (Gabriel, 2017). This "game"—probably familiar to most readers of this volume—is well expressed by the motto "publish or perish," which implies that every scientist, social ones included, has to publish as much as possible in order to develop a career in academia, i.e., getting and keeping a position, as well as receiving funding for research. While at a first glance this sounds as a noble goal, it is muddied by the current high-impact publishing scheme. This model, as described by Gabriel (2017), implies that public-funded research made by social scientists is given for free—or in exchange of *ad honorem* peer review—to private-owned outlets, which charge hefty fees for granting access to read the published work, fees that are usually paid by the same governments that funded the original research. By participating—willingly or not—in this publication model, social scientists have ended up writing increasing numbers of publications (Fanelli & Larivière, 2016), which are kept behind paywalls, and thus available to a very limited range of readers, mostly academics associated to a large-enough institution. Aside from the questionable economics of this model, the need to publish in high-impact, indexed journals (e.g., Web of Science index) pushes social researchers to adjust their scientific work to the topics, methodologies, and formats that those specific journals—i.e., their editors—accept. Not surprisingly, this publishing game has encouraged social scientists to create knowledge for the sake of publishing rather than giving back something of public value to the social world; thus the increased perception of "losing touch" with the world ("Are we losing touch?", 2018) producing research that could be ultimately irrelevant (CORDIS, 2018a) and even of questionable integrity (Edwards & Roy, 2017). Therefore, having a common understanding of what is that public value is more essential than ever.

While the present chapter is certainly incapable of changing the publishing game described above, it looks to bring forth the discussion of what for are social sciences doing research nowadays. This is done hoping that, in the long run, discussing the latter also makes unavoidable to question the former. In order to open up and organize this discussion, the present chapter develops a conceptual framework that could help its development by outlining and relating existing positions on the direction that social sciences should follow. More specifically, six different positions on what kind of knowledge social sciences ought to create are first presented. As it will be discussed, these stances combine with each other in ways that makes it difficult to neatly distinguish them through the usual theoretical vs. applied lines (e.g., Johnson & Field, 1981; Nafstad, 1982; Roll-Hansen, 2009). Thus it is proposed to group these positions into a single framework composed of three opposite pairs: return on investment vs. value in itself, applied vs. basic social research, and citizen vs. academic relevance. This framework aims not only to identify the orientation of a given set of social science research but also aims to help in finding common ground among these apparently contradictory positions. Finally, I conclude this chapter by sketching ways in which social sciences might reconcile its differences without sacrificing its inherent diversity—emphasizing that doing more research and receiving more funding should be accompanied by being increasingly more reflective on how our own research shapes, impacts, or is irrelevant to the social worlds in which we live.

Return on Investment Versus Intrinsic Value

Despite being typically reduced to one or two lines in the acknowledgments section, funding is an essential and usually painstaking aspect of doing scientific research. Essential as it defines, in very material terms, the scale of the research project that a social scientist might be able to conduct. This ranges from how many research assistants and PhD students could be hired as support, to what kind of materials and instruments are to be available, and especially how large could the scope of the project be in terms of time and participants. Yet the focus here is not the struggles that social scientists usually endure to secure funding but the very natural question that follows from any relation mediated by money: what has to be given by social scientists in return of that money? The question seems especially relevant in a context in which economic analyses reveal that science, in general, seems to be offering "diminishing returns," i.e., making less groundbreaking results despite increasing many times its funding (Collison & Nielsen, 2018).

On this issue, it is possible to identify a perspective for which social sciences and their research projects are understood as government-funded projects, out of which some form of monetary return is expected (e.g., Willis, Semple, & de Waal, 2018). As the nature of social science projects makes clear, this return on investment is not expected in the same way as natural and physical sciences—for which technological innovations and patenting makes that return easily measurable. Instead, the monetary impact of social research could be assessed only through indirect measures, for instance, by measuring how a community development project contributes to lower crime rates, which in turn reduces police and jail costs (e.g., Mocan & Rees, 2005). Not surprisingly, the issue has been largely discussed by natural sciences, even as the subject of a *Nature's* editorial (Macilwain, 2010), under the subject "science economics." Yet the same perspective has only been partially applied to social sciences and humanities, pioneered by the IMPACT-EV project (CORDIS, 2018b). This initiative, aptly named *Evaluation, monitoring and comparison of the impacts of EU funded SSH*[5] *research in Europe*, was funded between 2013 and 2017 by the European Union Commission, aiming to develop a system of "evaluation concerning assessment of the scientific, policy and social impact of SSH research project outcomes" (2018b, p. 1). Besides its scientific impact, it is particularly interesting to note the assessment of both policy and social impacts. Regarding the former, the project declares that "we will focus on EU directives or recommendations, national, regional and local policies" (2018b, p. 1), while by the latter "we understand results of the policies and citizens' actions based on research evidence in relation to the five EU 2020 targets" (2018b, p. 1). In simple terms, from this perspective, any social research project should be able to demonstrate that it is capable of making not only a contribution in terms of increasing scientific knowledge but also in terms of advancing social and policy goals.

[5] "SSH" is the common abbreviation of social sciences and humanities.

In opposition to the former, there is a view that depicts scientific knowledge at large—social sciences included—as something valuable per se (e.g., Burawoy, 2007). By having an intrinsic value, the production of scientific knowledge should be pursued without any worldly hindrance, compromise, or limitation—especially regarding economic aspects. The former naturally implies that funding a research project should not imply any form of retribution other than the knowledge produced through it—for instance, research conducted to assess the impact of human rights (e.g., Friedman, 2018). In other words, this perspective considers social research as "invaluable knowledge," which should not be measured by any standard beyond its own expansion in novel directions—i.e., knowing more about something is sufficient justification for doing (social) science. Far from overstretched, this position represents particularly well the case of social sciences like cultural studies and linguistics, which, in fact, contribute "nothing more" than knowledge. If such knowledge happens to be deemed as less relevant at a particular institution or country due to financial constraints, as it happened at Copenhagen University in 2016 with a series of culture and language programs (see Hedetoft & Hede, 2016), then research and education should cease.

Summarizing up to this point, it is possible to identify one position arguing in favor of making social sciences accountable for their contribution to social improvement. This contribution, moreover, should be measured through some form of quantifiable index—ideally of economic nature—in order to demonstrate that doing research is money well spent. On the other hand, there is a position claiming that social sciences should not be asked to give back anything other than pure knowledge as the expansion of scientific, social knowledge holds an intrinsic value—that goes beyond any money-related concern.

Citizen Versus Academic Relevance

Connected to the former, but pointing into a different direction, is the elusive issue of the relevance of social science research. Elusive as defining what research topics and methodologies are considered relevant is something highly contingent to particular scientific communities (see Lave & Wenger, 1998) and sometimes even to particular scientists (see Polanyi, 1962). This variability is itself expressed on the different disciplines composing social sciences. For instance, how exactly can we decide if a social- or group-level analysis of discrimination is more relevant to hold? Since this issue by far exceeds the scope of this chapter—although it deserves further discussion—here I narrow the discussion to the source of relevance, i.e., *who* should be the arbiter for determining what is going to be considered as relevant social research, rather than discussing *what* should be deemed as relevant and irrelevant. Thus, it is possible to recognize the existence of two main sources of relevance: the local—and global—community of social science scholars that currently review and circulate this work or the communities that are the subjects of study and/or intervention by social scientists.

The first position identified—i.e., placing scientific communities as the ones that should decide what is relevant or irrelevant (social) research—represents the current state of affairs. In fact, declaring this position as the dominant one is something easily verifiable: nowadays, any piece of research that aims to be considered as proper social science has to be published through a peer-reviewed journal or book. Therefore, it is peers, i.e., fellow scientists acknowledged by the community, the ones who deem a work as relevant or irrelevant for that particular community. Here it could be argued that if a given journal does not accept a work for review—due to any reason—there are many more available that could receive it and eventually publish it. While this is correct, it rather reinforces the main point, namely, that scientific communities, through their many different established publishing outlets, are the gatekeepers for what is socially accepted as scientific research—since if no journal editor receives a given work, and peer reviewers accept it, this work cannot properly become a scientific piece. Similarly, most research funds have some form of—typically blind—peer review systems, which makes the argument also applicable to them. Thus, as long as fellow scientists act as *bona fide* reviewers, peer reviewing is a fair system for which it is difficult to think in a better replacement. More importantly, it does help to keep in charge researchers that have dedicated years to study a subject, which also avoid external interventions like political, religious, or other forms of censorship.

The former approach, despite being the dominant one, has nonetheless been criticized as endogamic and authoritarian precisely for the isolation from the social world that provides to scientific communities (e.g., Feyerabend, 1975). In fact, it is not hard to see that placing scientific communities as the only gatekeepers for deciding what is, and what is not relevant for social sciences is quite risky—due to the inherent perverse incentives involved in doing so. Among these risks, it stands out the possibility of communities abusing their privileged positions in order to promote certain topics of methodologies not due to their scientific merit but to put their own work in a good light (see Latour & Woolgar, 1979). Thus, it gives these works ample acceptance and diffusion while keeping its critics out of the spotlight. Whereas the former is borderline with scientific fraud and probably unlikely to happen on purpose, it notwithstanding points to the classic analysis of Kuhn (1962) about how reluctant scientists are to accept radical innovations on their field. Besides the former, there is also the risk of turning social research basically self-centered, i.e., making social scientists focus their work on issues that are plainly irrelevant for the citizens and societies that support their work. Against this, it is possible to think social sciences as the ones providing knowledge on the issues that societies demand to know, for instance, providing empirical and conceptual research that makes possible to understand emerging problems like cyberbullying (e.g., Mishna, McInroy, Daciuk, & Lacombe-Duncan, 2017), fake news spreading (e.g., Vosoughi, Roy, & Aral, 2018), or the impact of widespread use of social networks (e.g., Buglass, Binder, Betts, & Underwood, 2017). Despite the allure of these topics for citizens, they might as well be of no real interest for social scientists due to several reasons—like being virtual iterations of well-known face-to-face phenomena. While this mismatch between academic and citizen relevance does not have to always be the case, it has been a recurring critique towards critical and gender studies (e.g., Deutsch, 2007), as well as research on basic psychological processes (e.g., Epstein, 2016). The main counterargument for this position, as stated above, is the risk of turning this citizen involvement into veiled forms of censorship to the work of social scientists. Yet the opposite case also seems dreary: social research that has lost any touch with the social world, except for extracting information from it, in order to answer questions that matter to social scientists only.

Summarizing this second pair of opposite positions, it is possible to see that the issues of relevance for social sciences could be—in practice—defined in two ways: as the latest trend in the preferred scientific journal or congress and as the latest hot topic hitting news headlines and social media. At the end of the chapter, I will present a middle way between these extremes.

Applied Versus Basic Social Research

Placing this classic tension within social sciences as the last one is no coincidence. On the contrary, I do this to show that the usual debate between applied and basic[6] research, which usually concentrates most of the academic discussion, is in fact only a fraction of the conversation about the knowledge created by contemporary social sciences. It is certainly not a minor part, but it is not the whole picture either. In this context, this tension invokes the long-standing discussion of whether social sciences should aim to ameliorate the ever-increasing number of social problems (from drug addiction to domestic violence) or rather focus on discovering the underlying principles of human and cultural phenomena.

The first view on this holds that social sciences should only conduct applied research, i.e., devoted to create innovative solutions for real-world problems, just like engineering or material sciences do. While this might sound as a cartoonish comparison, it has some historical grounding. In this vein, a notorious example of how social research could be oriented towards the betterment of society is the case of Cora Bussey Hillis, a housewife and mother who, inspired by the child study movement led by psychologist G. Stanley Hall at the end of nineteenth century, advocated for establishing a research station at the University of Iowa devoted to research child welfare (Valsiner, 2017). In brief, "[h]er argument was simple but compelling: If research could improve corn and hogs, why could it not improve the rearing of children?" (Cairns, 1983 in Valsiner, 2017, p. 84). While not a social researcher herself, Bussey Hillis captured the gist of why social sciences should be oriented to conduct applied research above all else. In brief, social sciences ought to provide knowledge that easily translates into viable, efficient solutions to pressing problems. Accordingly, any form of general knowledge derived from applied

[6] Here I chose the term "basic" instead of "theoretical" since, as it will be explained shortly, I consider the latter to be an aspect of the former.

research would be desirable, but neither expected nor crucial—as the development of intelligence tests for selecting US Army personnel clearly shows (e.g., Tuddenham, 1948). Here it is important to distinguish this orientation from the previously described "return-on-investment" position. As noted, the latter is indeed concerned with how social research contributes to the concrete betterment of society, yet this can only be pondered in relation to the money cost involved. Therefore, even if social research is capable of being applied to address real-world issues, this would have to be done in a cost-efficient way that makes it competitive against other forms of social investment.

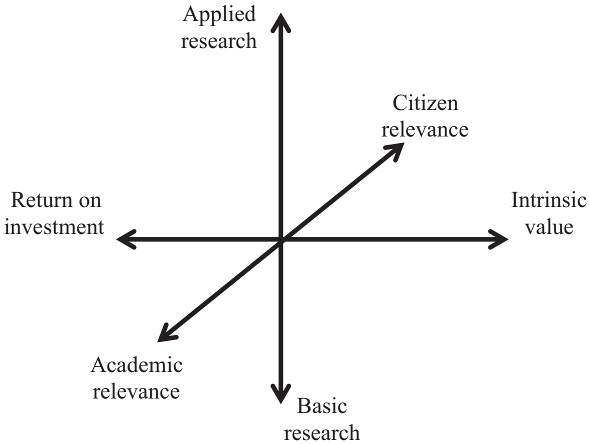
At the same time, it is possible to find the opposite perspective, namely, that social sciences should focus in different forms of basic research. The main rationale for this approach is that social sciences should not necessarily aim to impact the world in the short term but to provide novel ways of looking at and understanding human affairs. In this sense, the argument goes, any applied research that frames an issue inaccurately would ultimately lead to little or short-term impact rather than substantial change. Interestingly, when applied to social research, the term "basic" could be understood in at least two ways. The first of them is centered in raising awareness about current circumstances that, despite not being perceived as such, are in fact relevant social issues (e.g., Foucault, 1961/2006). This kind of basic research, typically conducted through conceptual analysis, looks to unravel situations that are easily assumed as problematic by the society, in order to show what is not being perceived. A case in point is youth delinquency, which at first sight could be seen as a problem of misbehaving teenagers, but upon further analysis it might be seen as even a rational decision when confronted with extremely unequal environments (e.g., Mocan & Rees, 2005). In a similar direction, the other variant of basic research looks for general principles that apply to understand multiple social phenomena (e.g., Luhmann, 1984/1995). Here, of course, it is possible to find multi-volume theoretical treatises that have shaped the understanding of social disciplines (e.g., Geertz, 1973). But, at the same time, it is possible to find works that explore basic human and cultural processes through a combination of theoretical and empirical work (e.g., Werner & Kaplan, 1963) or exclusively through an empirical approach (e.g., Bock, 1966).

In sum, the main positions regarding the applied or basic character of social research are clear: either research is conducted to directly solve pressing social issues, or it takes a full step back from the social world, in order to reflect about it without directly meddling being involved in its events and discussions.

A Common Framework

Having outlined the six contemporary positions on what should be the kind of knowledge that social sciences should give back to the societies supporting their work, it is now necessary to organize them in a single framework in order to avoid leaving them as a plain list. As described above, the aim of this framework is providing a simple

Fig. 2.1 Framework of the six main criteria for social scientific knowledge



tool that helps at recognizing how different subdisciplines and research areas within social sciences are currently pointing towards very different directions regarding their purposes (see Fig. 2.1). This diversity, which is inherent to the manifold guidance (Valsiner, 2012) that social sciences experience, is not a problem in itself—and it could very well a characteristic that reflects the long-standing co-existence of different research traditions (see Cornejo, 2005). Yet it does become problematic when this is neglected by social scientists, thus making them assume that their own particular view on the purpose of social research is the only possible or reasonable view—as it seems to be the case at the beginning of the twenty-first century. In this scenario, it is only natural to find social scientists that consider the work of other social scientists as irrelevant, purposeless, or even worthless. Helping to clarify this kind of misunderstood is one of the purposes of this framework. Yet it is not the only one, since acknowledging that social sciences could gravitate to different directions is something necessary, but not sufficient. Thus, this framework could also be used to go beyond diagnoses, specifically to look for *common ground* among these different approaches. This is acknowledging that the positions identified are extremes of a continuum, and, as such, they could also be complementary rather than mutually exclusive. The last part of this chapter will further discuss this specific point.

When looking at the visual organization of the three opposite pairs described in the previous section, it seems reasonable to go back to the very distinction that this chapter looks to avoid—namely, an "instrumental" perspective (composed of applied research, citizen relevance, and return on investment) versus an "idealistic" perspective (aligning basic research, academic relevance, and intrinsic value). Yet I argue in favor of dividing them into three axes rather than merging them into two neatly separated opposites. The main reason against this traditional separation (e.g., Johnson & Field, 1981; Nafstad, 1982; Roll-Hansen, 2009) is the fact that it does not match the diversity of social science research. Thus, it is possible to find several examples of contemporary social research that simply does not fit in the instrumental-idealistic dichotomy. For instance, action-research projects—as in communitarian psychology (e.g., Carollo, 2012) or social work (e.g., Healy, 2001)—are, by its nature, applied research that operates under citizen relevance. They, however, are prone to reject any association with any form of a return-on-investment mentality, as its value relies on developing local communities. On the contrary, human-computer interface studies, for instance, usually address basic processes (like perception or attention) with the goal of improving the development of different forms of computer software (e.g., Raptis, Iversen, Mølbak, & Skov, 2018). Despite its basic focus, these studies are usually conducted for making software more engaging and intuitive to the user, in order to increase the profits of companies that develop software, thus going against the intrinsic value logic that they should have under an idealistic view. Similarly, social and economic studies regarding the impact of information in decision-making (e.g., Martínez & Dinkelman, 2014) are developed with a strictly applied mentality, as they aim to inform the creation or improvement of public policies. Yet their relevance typically comes from academic rather than citizen environments—since determining at what point in time is information more beneficial is rarely an ordinary concern—which once again breaks the mold of fitting into either instrumental or idealistic alignments.

The former examples, as it is clear, break the classic applied vs. idealistic distinction—and have certainly been picked for showing this. While this does not imply that any form of social research could fit within the classic model, it does intend to make clear that it is an insufficient approach; insufficient for understanding how many different elements combine at the moment of defining the kind of knowledge that social science create. But especially insufficient for finding common ground among different directions, rather than polarizing their differences—as the current basic/applied divide has done. In the following section, I outline ideas for what this common ground could be for social sciences.

Concluding Remarks: Finding Common Ground

In this chapter I have explored the role of contemporary social sciences; more specifically, I have discussed what do these sciences give back to the societies that make their research possible. While the short answer to this debate is "knowledge," this points to a much larger and overdue conversation: what *kind* of knowledge are social sciences expected to craft. Since this dialogue necessarily touches upon the—sometimes hotly debated—science-society relation, I proposed to approach this issue through the concept of guidance (Valsiner, 2012). In brief, this concept emphasizes how the work of scientific communities is constantly tried to be guided by society towards certain directions, while pushed away from others. This guidance is not necessarily an external influence that coerces social scientists—as this could also be done from within scientific communities (Danziger, 1979). This concept, moreover, makes clear that rather than a single direction of guidance, societies usually try to canalize social research towards many different—and even opposite—directions.

Therefore, when thinking through the notion of guidance, it possible to understand that social sciences could have more than one expected outcome—thus making possible to analyze the many kinds of knowledge that social sciences are currently giving back. It is in this context in which the six positions previously described were organized into a single, three-axis framework (see Fig. 2.1). Through the analysis of the three opposite pairs of positions, it became clear that the classic distinction between applied and idealistic research does not meet the diversity of contemporary social science research. Moreover, by presenting a single pair of opposites, it has not contributed to create a common ground in which this diversity could co-exist collaboratively rather than competitively. In the following, I sketch ideas on this.

In order to find how the quite different positions presented could co-exist—and collaborate—it is necessary to look closely at some of the distinctions made, in order to determine whether they are really opposite or possibly complementary. Regarding the first axis presented, return on investment vs. intrinsic value, it is clear that any initiative framing the contribution of social science only in terms of improving numeric indicators, particularly economic revenue, is a certain way to alienate a major group of social scientists from their own work. On the other hand, pretending that social sciences "owe nothing besides knowledge" to the societies that make their work possible is not only questionable but also hardly tenable in practice. For any form of social science, knowledge relates with the social world outside academia either we want it or not; in other words, the days of patronage are largely over. Therefore, the question at stake is how to establish a meaningful relation for social science and society. For this it seems essential to compromise on both ends of the spectrum. On the one hand, there is no need for social scientists to suddenly become experts in calculating the social profitability of research projects, yet it seems necessary to acknowledge the need of defining parameters to assess whether the knowledge created is making the expected impact or not. On the other hand, it is important to keep in mind that, as researchers, crafting scientific knowledge is the gist of our activity. This, however, should not be an excuse for pretending to be locked in an ivory tower—from which knowledge runs downstream to lay people, but never in the opposite direction (cf. Schütz 1932/1967). The former is, interestingly, tightly connected to the second tension discussed. The issue of relevance undoubtedly calls for an open dialogue between scientists and citizens rather than swinging into one of the positions presented, as neither the current academic isolation nor playing the role of social consultant properly fits with social science that produces research not only *about* people but also "with people in it," in the words of Ingold (2000). Here the ACCOMPLISSH initiative,[7] backed by the EU program Horizon 2020, offers—at least on paper—a viable framework for keeping social sciences and humanities grounded into social worlds from they emerge. Finally, the third dichotomy, despite being the most traditionally assumed as such, is not necessarily a contradiction. This is because basic and applied social research could be seen just as different stages in the research process rather than separate—or even

[7] http://www.accomplissh.eu/#accomplissh.

opposed—categories. While academic and administrative organizations tend to promote such separation (e.g., innovation centers), this could be done otherwise. In this sense, working within research networks in which different teams lead different angles of a project—basic, applied, and intervention—could put together apparently antagonistic stances. The former, however, requires that social scientists think beyond their own comfort zone, being open to establish bona fide dialogues with colleagues who have different orientations and interests.

As stated at the beginning of this chapter, its aim is to open a discussion on what kind of knowledge should social science give back to the societies that make their research possible. And thus it would be pointless to close this work by providing an answer that I consider as definitive to this debate. This is why, as I have tried to make clear, my aim has been twofold: making visible the necessary distinctions for holding such a discussion and showing that among the sometimes-contradictory diversity of social science knowledge, it is possible to find ways to reconcile these manifold paths into common ways of development.

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Chapter 3
Vitenskapsteori: What, Why, and How?

Roger Strand

Norway is a small country that remains outside the European Union while in many ways acting as if it were within it. In 1942, during WWII, US President Franklin D. Roosevelt famously exclaimed: "Look to Norway!" Norwegian patriots often interpreted Roosevelt's speech as a tribute to the courage of the Norwegian resistance movement. A more modest and analytical interpretation, however, can be suggested: In order to understand Europe, it may be useful to study its smaller and more peripheral countries. At the periphery, *more is possible*: the solutions may be a bit more exotic, a bit easier to analyse in terms of their historically contingencies, and perhaps even a bit more telling about the mainstream European culture. The emergence of *vitenskapsteori* in Norwegian academia and higher education is such a story. This story displays Norway as a proper part of Europe and at the same time a special one, deeply influenced by interplay between societal sectors, above all between state officials and popular movements, creating opportunities for dialogues without steep hierarchies (Skirbekk, 2018).

The Special Case of Norway

The citizens of Norway have twice declined Norwegian membership to the EU (by referendums in 1994 and 1972, the latter to the EEC). Still, Norway is a member of the inner market through the European Economic Area, which means that almost

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all EU directives apply and they are upheld with a high degree of compliance. Norway is part of the European Research Area (ERA) and participates in the Framework Programmes for research and innovation, and the country is committed to the European Higher Education Area (EHEA) and its Bologna Process. Specifically, this means that Norway has adopted the EHEA Qualification Framework (sometimes called "the Dublin Descriptors"). At the Ph.D. level – the so-called "third cycle" – there are six such descriptors. Three of them describe the qualifications to be expected from carrying out the dissertational work, such as mastery of research methods and the ability to carry out a research project. Two descriptors focus on communication skills and the participation in society. And then there is one descriptor that indicates the legacy of the European *Bildung* tradition, the history of Western philosophy perhaps, and traditional academic virtues: the successful Ph.D. student is "capable of critical analysis, evaluation and synthesis of new and complex ideas".[1] In this way, and through other descriptors, the EHEA underlines that the Ph.D. and the master's degrees are educations in breadth and depth and not just a device to bring manpower to European research.

From the Norwegian perspective, we[2] have implemented the Bologna Process, albeit with national adaptations as was anticipated by the EHEA. Unsurprisingly in a Scandinavian welfare state with a strong planning tradition, a state institution plays a major role in the Norwegian implementation of *Bologna*. The Norwegian Agency for Quality Assurance in Education holds 140 employees as well as the power to grant accreditation to Norwegian higher education institutions and programmes. Indeed, the Norwegian State enjoys a monopoly on higher education in the sense that private universities and university colleges can only award bachelor, master's, or Ph.D. titles if they have been accredited by the mentioned Norwegian Agency. Without such accreditation their study programmes are actually illegal.

The yardstick against which Norwegian study programmes are measured is our national Qualification Framework – our own adoption of Bologna and mandated by law. At the Ph.D. level it includes 11 descriptors, which are not very different from the EHEA original though somewhat more comprehensive. The descriptor equivalent to "capable of critical analysis" is found already in the first of the set of 11. It states that the successful Ph.D. candidate is:

> *i kunnskapsfronten innenfor sitt fagområde og behersker fagområdets vitenskapsteori og/eller kunstneriske problemstillinger og metoder.*[3]

[1] See e.g. http://www.ehea.info/media.ehea.info/file/WG_Frameworks_qualification/71/0/050218_QF_EHEA_580710.pdf

[2] I will write "we" in this chapter to signify a series of ever more narrowly construed subjects: Norway, the Norwegian university sector, Norwegian *vitenskapsteoretikere* – practitioners of *vitenskapsteori*. The author of this chapter belongs to and cannot help represent each of these subject positions.

[3] The official document is found on the server of the Norwegian government: https://www.regjeringen.no/globalassets/upload/kd/vedlegg/uh/utbyttebeskrivelser_kvalifikasjonsrammeverk_endelig_mars09.pdf

which will be difficult to translate; indeed, this difficulty is why we at all are looking to Norway in this chapter. The semi-official translation is in need of considerable explanation:

> *in the forefront of knowledge within his/her academic field and masters the field's philosophy of science and/or artistic issues and methods.*

The strange passage on "artistic methods" is easy to explain. It is simply the result of a regulatory patchwork as it became possible to obtain a Ph.D. in performing arts, in which the dissertational work is not necessarily research as such but rather a fine arts, music, or design project. For a non-Norwegian the passage on "the field's philosophy of science" may seem even stranger, though. Why in the world should all Ph.D. students in Norway, say, within biology, master the philosophy of biology? At this point, someone like me – a Norwegian, a Norwegian university teacher, a Norwegian teacher of *vitenskapsteori* – would intervene: no, they are not supposed to master philosophy of science, by all means. What they should learn is *vitenskapsteori,* to the extent that most Norwegian Ph.D. programmes include mandatory courses, typically 5, 10, or even 15 ECTS credits, with this subject. *Vitenskapsteori* may include some philosophy of science but is definitely not the same. It is something different, just as Norway is different from the European Union.

What Is *Vitenskapsteori*?

It is almost unethical to keep the reader of this chapter in such a suspense. What is *vitenskapsteori*, then? State the definition! This should be an easy task, in particular for the present author, who has worked at a *Senter for Vitenskapsteori* for 25 years and was appointed professor in *vitenskapsteori* 13 years ago. Alas, even at our centre we had an internal seminar series for years called "What is *vitenskapsteori?*", with heated, never-ending discussions.

German and Dutch readers will recognise the word itself. In German, there is *Wissenschaftstheorie.* In Dutch, there is *Wetenschapstheorie. Wissenschaft, wetenschap, vitenskap, vetenskap* (Swedish), and *videnskab* (Danish) are all words for science, that is, with an inclusive definition of science. In the verbiage of Germanic languages, sociology, philosophy, theology, the study of law, history, and anthropology are all *Wissenschaften* – sciences, as well as the natural and medical sciences, of course. Hence *vitenskapsteori* means "theory of the (diverse set of) sciences". This allows us to complete the next iteration to the question of what Norwegian Ph.D. students are supposed to master. They should learn the "theory" of their academic discipline or field. What kind of theory? Duden, the excellent German dictionary, defines *Wissenschaftstheorie* as follows:

> *Teilgebiet der Philosophie, in dem die Voraussetzungen, Methoden, Strukturen, Ziele und Auswirkungen von Wissenschaft untersucht werden.*[4]

[4] https://www.duden.de/rechtschreibung/Wissenschaftstheorie

which we might translate as follows:

> *Subfield of philosophy, in which the presuppositions, methods, structures, goals and impacts of science are examined.*

Indeed, the German Wikipedia page of *Wissenschaftstheorie* describes it as a subfield of philosophy: philosophy of science, or even simply epistemology. It is an activity mostly performed and owned by professional philosophers. Important parts of that activity include the rehearsal of the debates over realism and constructivism ("Are scientific theories true? Do theoretical concepts correspond to real-world entities?") and the debates about the unity of scientific method, the logical structure of explanations, the styles of scientific reasoning, etc.

In Scandinavia – Norway, Sweden, and Denmark – however, the term *vitenskapsteori* is used in two quite distinct ways. One usage is equivalent to *Wissenschaftstheorie* and denotes philosophy of science, in that broad definition of science as explained above. It is an activity that philosophers own and that quite a few scientists and citizens also like to engage in, discussing a canon that includes authors such as Karl Popper and Thomas Kuhn (whom they are likely to have read) and many others such as Otto Neurath, Rudolf Carnap, Imre Lakatos, Larry Laudan, and Paul Feyerabend (whom they are less likely to have read). The second and main usage, however, of *vitenskapsteori* denotes a heterogeneous and interdisciplinary academic field. The Danish Wikipedia page on *videnskabsteori* is radically different from the German one (and Norwegian one, which follows the first usage). I have translated its opening below[5]:

> Videnskabsteori *is an interdisciplinary area that has the science itself as its object. In Denmark, one also talks about "research on research", and sometimes the looser term "science studies" is used.*
>
> *Classically,* videnskabsteori *is divided into these areas:*

- *Philosophy of science*
- *History of science*
- *Sociology of science*

In Norway, a national conference in 1975 (at Jeløya, South of Oslo) gave an even broader definition which included the economics, anthropology, pedagogics, and psychology of science together with science policy studies, research ethics, and the study of ethical aspects of science (NAVF, 1976). The Jeløya conference was highly influential in structuring the field in Norway, and in genealogical terms there goes a straight line from the conference to the mandatory requirement of *vitenskapsteori* in the Norwegian Ph.D. qualification framework.

Since 1975, the universe of "research on research" has changed. Following Kuhn and Feyerabend, philosophy of science has moved somewhat in the direction of empirical studies, in what Werner Callebaut dubbed the "naturalistic turn", with philosophers such as Nancy Cartwright and Ian Hacking doing original historical work and historians such as Peter Galison and Lorraine Daston making profound philosophical contributions. Furthermore, science and technology studies (STS)

[5] https://da.wikipedia.org/wiki/Videnskabsteori

have grown and developed into a very real academic field at many universities, with its own centres and departments and study programmes. Its history began in the 1960s with the formation of radical political awareness and criticism of the role of science in society (when STS still meant "Science, Technology and Society"). A period of intellectual radicalism followed, with the SSK – Sociology of Scientific Knowledge – movement that often claimed strong if not extreme positions on the social construction of knowledge. Since then, STS matured, institutionalised, and grew in volume and academic prestige at the expense of philosophy of science. STS concepts and methods have gained influence in a variety of social sciences and humanistic research fields. To quote Zia Sardar, STS has changed from a low church community to high church (Sardar & van Loon, 2011). The influence of STS is clearly seen in a recent job advertisement at Gothenburg University in Sweden, which has a section for *vetenskapsteori* in one of its multidisciplinary departments. In the advertisement, *vetenskapsteori* is defined as:

> *[…] part of a post-Kuhnian tradition and has a distinct orientation towards empirical study. Research and teaching is primarily focused on epistemological and social aspects of the production and use of scientific knowledge. Differences in epistemological and methodological presuppositions across disciplines and fields of research are accentuated and analyzed, with the humanities and social sciences as well as medical and natural sciences being objects of study. Theories and methods are usually drawn from the field of science and technology studies (STS).*[6]

So we may conclude this step of iteration as follows: *vitenskapsteori* seems to be the name of a Scandinavian brand of interdisciplinary research on research that combines philosophy, history, sociology, etc. of science with STS, science policy studies, and research ethics and research on ethical aspects of science. And science is to be taken in its broadest sense, including the humanities and social sciences. Two mysteries remain, however: If this is how it is, why did my colleagues at the *Senter for Vitenskapsteori* engage in "heated, never-ending discussions" about the identity of the field? Secondly, again, why in the world should this subject be taught to all Norwegian Ph.D. students? Towards the end of this chapter, we shall see that these two questions are deeply related.

Why *Vitenskapsteori*?

When asked "why *vitenskapsteori*?" we might choose to reply with causes, with historical events, and with institutional structures that can explain the presence of a mandatory requirement in Ph.D. training in the small country of Norway in contrast to other European countries. The Jeløya conference was already mentioned. The relatively strong presence of philosophers at Norwegian universities is another explanatory factor, which again can be explained by the four century–old tradition

[6] https://www.gu.se/english/about_the_university/job-opportunities/vacancies-details/?id=3453

of *examen philosophicum*, a more or less mandatory first year course in Norwegian university education that usually includes a course in the history of philosophy. *Examen philosophicum* is an important part of the Norwegian version of *Bildung* and also, on a more trivial note, an abundant source of employment opportunities for philosophers in this country.

Being a participant rather than a neutral observer to the institution of *vitenskapsteori,* my main contribution to answering "why *vitenskapsteori*?" will not be to offer much more of sociological explanation. Rather, I shall try to state our reasons for believing in the importance of *vitenskapsteori.* I have found it useful to divide them into three groups: Reasons that are bland, reasons that I don't believe in myself, and reasons that I actually believe in. Let us do the tour.

Reasons that are bland point in the direction of what the Dublin Descriptors call "critical analysis". By gaining a theoretical understanding what science is and how science works, the student may be equipped to critically evaluate scientific work, including her or his own. There is nothing wrong with this idea, but it is primarily stating a desirable purpose of *vitenskapsteori* than making an argument about how this purpose may be fulfilled.

Reasons that I don't believe in revolve around the direct utility of *vitenskapsteori* as judged by internal scientific criteria. For instance, this has been argued in support of philosophy of science. If one knows the logical structure of explanations, or the workings of the scientific method, the argument goes, one will be a better scientist who will reason with a higher degree of conceptual and inferential clarity. I have not seen other than anecdotal evidence to support this claim, and the mere heterogeneity of scientific practice speaks against it. What we do see in our teaching practices, however, is that some Ph.D. students find a need for conceptual clarification in their dissertational work, and a course in *vitenskapsteori* may serve that need because it provides an opportunity to work on the concepts in question. In such cases, the course may prove useful also by internal scientific criteria. I shall return to this point later.

Finally, there is a set of what I consider good reasons for *vitenskapsteori* in the training component of the Ph.D. study. Their common denominator was eloquently expressed by Gunnar Skirbekk, the founder of *Senter for Vitenskapsteori* at the University of Bergen:

> *Modern societies are science-based in a variety of ways. Hence it is important to understand what the various sciences can, and cannot, deliver, and to understand how, and why, this is so. More specifically, due to specialization in contemporary research there is a need for* vitskapsteori *both at the universities so they can live up to their name of uni-versity, and in societies in general in order for them to be able to cope with the different professions and experts, each with their specific approach and perspective.*[7]

In order to appreciate Skirbekk's argument, it is useful to know his philosophical outlook. A scholar in the Apel-Habermas tradition, Skirbekk has been interested in how modern societies are characterised by a division of labour that calls for

[7] https://www.uib.no/en/svt/21651/history-centre

specialisation and differentiation of expertise. Modern society has a myriad of sectors, institutions, and tasks, and for each specialised task a particular form of expertise may be required. Now, it is a fact that experts from different fields sometimes disagree. Imagine a controversy, say, about regulations of sick leave from work. The right wing party cites experts from welfare economics who have constructed a model that in that particular case suggests stronger negative incentives, for instance, that the first day of the sick period becomes a waiting day without compensation. The unions emphasise studies by occupational health researchers that show a recent increase in stress due to higher work pace. A sociologist then enters the debate with an analysis of how neoliberal policies and ideologies have shifted the balance between employers' and employees' responsibilities for maintaining a well-functioning workplace and so on. How can society sustain an enlightened debate between these perspectives in its public sphere? Part of Skirbekk's answer is that there is a need to understand "what the various sciences can, and cannot deliver", that is, to understand the respective domains of the various validity claims being made and the underlying theoretical and methodological assumptions that have to be fulfilled for the claims to be valid. What assumptions are being made in the economist's model and the sociologist's theory?

In this vision, *vitenskapsteori* serves democracy by opening up the black boxes of expertise and thereby rendering it accountable. The economist should understand the theoretical assumptions of the sociologist in order to be able to appreciate the latter's knowledge but also the limits to that knowledge and vice versa. Even more important, the economist (or any other specialist) should appreciate the limits to her or his own knowledge, in order to develop the appropriate reflexivity and humility on behalf of her or his own expertise. This is important in society in general but should begin already at the university, in critical and self-critical interdisciplinary encounters.

Gunnar Skirbekk (2018) has traced this argument in the Norwegian public sphere back to the eighteenth century. For historical reasons, Norway entered the Enlightenment era (almost) without nobility but with a class of state officials educated (mainly in Copenhagen) into enlightenment thinking. What is characteristic of Norway's development in the eighteenth and nineteenth centuries, towards political independence in 1905 and towards a new golden era of literature (Ibsen, Hamsun, Undset) and art and music (Munch, Grieg), is *intellectual and political interplay* between these "enlightened" state officials who represented the political power in the country, with a diversity of popular movements that often had a basis in Christianity but nevertheless were pro-Enlightenment. A similar argument was continued after WWII, when the philosopher Arne Næss renewed the *examen philosophicum* university institution with the explicit purpose of promoting clear though and speech and thereby lay the grounds for accountability in the public sphere.

There is no fixed set of tools within *vitenskapsteori* that can open up any black box of expertise. I will give examples of didactics below, but a glance at curricula and learning activities at Norwegian universities shows a variety of resources being used, ranging from philosophical classics to STS to in-depth study of theoretical debates within the scientific disciplines themselves. What should be clear, though, is that Skirbekk's prescription of *vitenskapsteori* implicitly is predicated on a diagnosis of modern university life as not precisely a model of universal pragmatics and research education as not entirely self-sufficient in reflexivity and humility. This diagnosis is quite consistent with what has been learnt in what Gothenburg university called "the post-Kuhnian tradition", above all within STS but in general in the more empirically oriented studies of science. Thomas Kuhn (1962) himself opened his *Structure* with a related accusation:

> *History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed [*sic*!]. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. (p. 1).*

Kuhn was no radical in political terms; his mission was to correct the history and philosophy of science, away from the idolisation of the scientific method (whatever that was) and the imagery of "tourist brochures", to his picture of normal science as a rather gloomy place in which the manpower of science mechanically collected facts that fit the paradigm and swept others under the carpet, "a bunch of selfish, scared puzzle-solvers" in the words of Jerome Ravetz (2009). As for the scientific activity itself, Kuhn intended no criticism: in his view, normal science may be gloomy but definitely efficient. Post-Kuhn, however, the criticism emerged along at least three dimensions. First, along the lines presented above, normal science was seen to be a threat to democracy because its practitioners were socialised to unreflexively accept the confines of the paradigm and accordingly become unable to understand the limits of one's own validity claims. Secondly, in STS and post-normal science, normal science was sometimes seen to be useless or downright dangerous when its knowledge or technological applications were applied to real-world complex systems. Normal science has little resources to deal with the "unknown unknowns," to paraphrase Donald Rumsfeld. Third, even within the classical philosophy of science, normal science was seen as a threat to the scientific ethos in the sense of organised scepticism (Merton) and the scientific attitude (Popper and perhaps Feyerabend). In *vitenskapsteori*, one can find this double motivation, of correcting the common-sense image of the sciences held by scientists and of somehow attending to the problematic aspects of scientific education seen as a process of combined socialisation and cognitive specialisation. While few if any scholars will accept Kuhn's description of science as the Truth, nobody can deny that modern societies are characterised by differentiation of expertise and that the differentiation comes with challenges. The good reason *why vitenskapsteori,* at least in the eyes of the present author, is that it is a tailored approach to take on some of those challenges.

How to Master *Vitenskapsteori*

From the perspective presented in the previous pages, one can deduce a certain approach, or perhaps attitude, to the challenge of teaching *vitenskapsteori*. I owe the reader a warning that this approach is my own; not all Norwegian *vitenskapsteoretikere* would necessarily agree. I will encircle the approach by first stating its opposite. Teaching *vitenskapsteori* is not a matter of delineating a canon and disseminating it. Such a canon might exist, and the Scandinavian thought collective of *vitenskapsteori* might even agree on big names in it. Names such as Popper, Kuhn, Feyerabend, Hacking, Ravetz, Galison, Daston, and Habermas have already been dropped; similar lists of names could be made of authors from STS (for instance, Sheila Jasanoff, Bruno Latour, and Brian Wynne have often been present in our curricula), ethics, and the many specialised literature that deal with the *vitenskapsteori* of particular fields of science. This canon may be important when we train our own kind, for instance, our own Ph.D. students who will take a degree in *vitenskapsteori*. As a mandatory requirement in the Ph.D. study programmes across the university, however, the point is not to educate the students in *vitenskapsteori* for its own sake. The mandate is to help the students opening up black boxes of expertise; to develop critical abilities, reflexivity, and humility; and to better understand particular strengths and limitations of their own expertise at work in society. The canon is mobilised insofar as it is conducive to this learning process. Personally, I have taught entire courses in *vitenskapsteori* without using any of this literature.

The didactics of *vitenskapsteori* teaching accordingly begins with an analysis of the particular challenges of socialisation and cognitive specialisation of the student group (or researcher group) in question. A colleague jokingly had "epistemological therapist" as the profession on his business card. The metaphor of therapy is not entirely inadequate as long as it is applied with the appropriate caution and respect. It begins with a diagnosis of the blind spots of the cognitive specialisation. How does the specialisation reduce real-world complexity? For instance, in part of economics and social sciences, human choices are often modelled as rational choice that maximises individual utility. In part of biology and medicine, phenomena are predominantly analysed as manifestations of a genetic programme. In part of physics, many systems are predominantly analysed as if they were linear. In part of psychology, there is focus on internal "construct validity" of the variables produced by psychological tests rather than the issue of external validity that often will have to remain in the dark. In part of literature studies, the personal biography of the author is not rendered central in the analysis of the text and so on. There is nothing wrong with any of this – focus and reduction of complexity is absolutely necessary to do science. The didactic challenge of *vitenskapsteori* is to understand how the processes of cognitive specialisation and socialisation into the various thought collectives will condition its practitioners and devise a "therapy" to develop their awareness of their own (and others') conditioning as well as develop their cognitive (and emotional) resources to deal with it in a reflexive manner. This is a huge challenge in many cases because it entails a need to understand the actual content matter of the science in question as well as understanding its more or less implicit theoretical and methodological assumptions. Again, drawing on the metaphor of therapy, this is often not a task for the teacher to solve and then present the solution to the student. Rather, a therapeutic dyad is needed in which the *vitenskapsteoretiker* and the Ph.D. student develop the analysis together. At this point, the therapy metaphor will and should break down. The student is not a patient; rather the therapeutic focus is the issue at hand, and teacher and student gather their intellectual forces to deal with it.

Accordingly, in our finest courses at *Senter for Vitenskapsteori,* implemented at the Faculty of Social Science and the Faculty of Humanities, the classroom part is very brief, and then a semester-long supervision process begins in which the Ph.D. student selects an interesting and contentious topic for his *vitenskapsteoretisk essay.* The topic may be a deep conceptual problem in her or his research project, or a worry about problematic societal impacts of the research, or a need to identify and clarify hidden methodological assumptions. This is also how the *vitenskapsteori* sometimes can be useful for the dissertational work. We encourage the students to choose the topic "that they worry about late at night": theoretical, methodological, and pragmatic doubts that perhaps will come up anyway in the viva when the thesis is to be defended, at least if the opponents are real intellectuals and not just operators in the system. So we encourage and supervise essays in which the students sometimes disassemble the assumptions underlying their research or argue that the technology that they are part of developing may be useless or dangerous. We employ the canon, or other relevant literature, as intellectual support in that process, but most of all, the key vehicle is open and engaged discussion in the dyad and in the student group.

It goes without saying that such learning processes can be demanding ones. I briefly mentioned that there may be an emotional aspect, as most academic socialisation processes include the entrance into an in-group that has a high degree of belief in the virtues of their own endeavour. On this, academic cultures vary a lot. Within the more post-modern corners of the humanities and the social sciences, there might even be an excess of reflexivity and self-doubt. In contrast, many young medical scientists do not seem to have been exposed to much organised scepticism. Rather, they have been socialised into in-groups that believe strongly in the validity of their expertise and the moral virtue of their mission. Teaching *vitenskapsteori* at a Faculty of Social Science and a Faculty of Medicine requires very different didactics.

Vitenskapsteori as a Battle Field

Hence, to engage in *vitenskapsteori* is never boring. We challenge the students, and they challenge us back if they think that we as outsiders do not understand their scientific practice. Then there is the relationship to their supervisors and their departments, which can vary from mutual respect and gratitude to a battle in which some supervisors see us as undermining their authority. To quote a former dean at the University of Bergen, in a research education strategy meeting some years ago: "It is OK that Ph.D. students with individual research projects go through these courses, but we do not really want that Ph.D. students hired on prestigious, international research projects come to doubt their own science, do we?" From this perspective, the mandatory requirement of *vitenskapsteori* is not merely a nuisance and a waste of time but may even be impeding and threatening to normal science.

Moreover, some research groups are entirely positive to the idea of developing reflexivity through *vitenskapsteori* but contest the idea that a thought collective of dedicated *vitenskapsteoretikere* holds the relevant expertise to take on this challenge in its generality. Rather, they see *vitenskapsteori* as something that should be owned and practiced by eminent researchers in the various fields, closely connected to and integrated into their own research practice. Sociology and social anthropology are examples of disciplines that value *vitenskapsteori* and have a place for it in their practice.

I also mentioned that an institution such as *Senter for vitenskapsteori* has sustained its own lively discussion about the identity of our field. To a large extent, these discussions have been related to the issues outlined above. One contested element has been that of "double competence": Is it so that a teacher of *vitenskapsteori* for, say, Ph.D. students of biology, should be an expert both in *vitenskapsteori* and biology? What level of expertise within the particular science is required for a practitioner of *vitenskapsteori* to do research on that science and/or teach the students and researchers of that science? As a matter of fact, many of my colleagues do hold elements of double competence – having done a degree in a particular science before they moved to *vitenskapsteori.* This seems also to be the case for many STS scholars. It is a difficult debate that also resembles debates in social anthropology about the virtues and vices of "going native". It is easy to argue for a balance and a dialectic between cognitive and institutional distance and proximity but difficult to agree on the point of that balance. In a similar debate within STS, Harry Collins argued that "interactional expertise" is what is required, which we can think of as the ability to pass the equivalent of a Turing test within the community of experts – not being discovered as an impostor. I agree but would add that the Turing test is not enough. One actually has to understand the subject matter in order to discuss the strengths and limitations of theoretical and methodological assumptions. A degree in that field is neither a sufficient nor necessary condition for that understanding but sometimes it helps.

Another contested element, related to that of double competence, is the question of normativity in *vitenskapsteori.* The classical philosophy of science, up to Popper and logical empiricism, and going back to Descartes and Bacon, was never afraid of making bold claims about what constitutes and demarcates *good science* and the proper scientific method. Post-Kuhnian *vitenskapsteori* has to a large extent abandoned the idea of a universal scientific method and a simple demarcation criterion for science. The only robust result of *vitenskapsteori* since the 1970s is the disunity of science. Scientific communities and cultures are remarkably different, and scientific knowledge is contextual. One does not have to subscribe to any radical social constructivism to agree on this point. During the worst periods of the so-called Science Wars, however, the issue at stake was if validity claims at all are more than a matter of power games. It is fair to say that in the latter decades of the twentieth century, Scandinavian debates on constructivism and relativism created friction between predominantly constructivist STS communities on one side and more philosophically oriented *vitenskapsteori* communities on the other, the latter arguing against relativism and in favour of the possibility of valid normative claims about what constitutes good science. In that sense Norway may feel as a forerunner before the Third Wave debate in STS around 2002 (Collins & Evans, 2002) and definitely now in 2018 that Bruno Latour worries if he contributed to create Donald Trump (Kofman, 2018). In terms of didactics, the question is perhaps less controversial. Just as Andrew Pickering stated that good science is both objective and relative, I believe that no *vitenskapsteori* colleague of mine would teach the blessings of a particular formulation of the Universal Scientific Method but would still engage in the particular theoretical and methodological issues in the student's dissertational research to discuss the validity of assumptions and a responsible use of the knowledge being produced. A highly successful training in *vitenskapsteori* would make the Ph.D. student aware of internal, theoretical, and methodological issues in his research, as well as the external issues related to the societal and political dimensions of her or his knowledge and expertise. Furthermore, it would make him or her aware of the connection between the internal and external issues and so develop a mature, reflexive expert who understands the strengths and limitations of her or his own expertise and is able to exert it in society with a sense of humility and responsibility.

Vitenskapsteori as a Modern Project in a Small Country: Concluding Remarks

This chapter began with the reference to President Roosevelt's speech "Look to Norway". We have followed that request by looking at the possibly exotic Norwegian implementation of the European Qualification Framework and its call for critical analysis and evaluation. I have presented the vision that I most strongly believe in, namely, that of *vitenskapsteori* as a vehicle for democratic development in a modern, differentiated society.

I will end the chapter with a brief reflection on the smallness of Norway. Smallness can improve the chances of nonlinearities and contingencies; in a huge system statistical thermodynamics will inadvertently prevail. However, there is more to the argument. Again, I resort to the historical narrative of Gunnar Skirbekk: Norwegian universities were so small also in the Post-WWII era that colleagues from different departments had to relate to each other. On the most trivial level, they had to go to the same cafeteria (or pub). Universal pragmatics was accordingly not merely a remote theoretical ideal, it was a matter of making sense of each other in the daily discussions at work. This is a different context than that of huge universities in which every department is a universe of its own. Interdisciplinary interaction was the norm. We may recognise the ideal of interdisciplinarity and contact and understanding between forms of expertise already in the 1975 Norwegian consensus report on *vitenskapsteori* and later in the institutionalisation of the subject with the aid of the Norwegian research council (NAVF) in the 1980s and finally the Norwegian Agency for Quality Assurance in Education.

Norway is still small, but since 1975 its higher education institutions have become mass universities enrolled in the so-called knowledge economy. They are still not big by international standards but big enough that many departments have their own cafeterias and more importantly, their own academic life, quite in isolation from the others. As for the pubs, I doubt that today's early career researchers find time to frequent them. The vision of *vitenskapsteori* is at stake in that development; at a *Senter for Vitenskapsteori* life is never boring because our subject and our existence are always contested. We receive praise from one research dean one day; our study programmes are attacked by another research dean the next day. In the midst of this university life which at best is merely going overly neoliberal, the tiny thought collective of *vitenskapsteori* might picture ourselves as modernity's heroic resistance fighters, finding comfort in President Roosevelt's words: "If there is anyone who doubts the democratic will to win, again I say, let him look to Norway".[8]

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[8] The film recording of the speech can be found at https://www.youtube.com/watch?v=YfnnK76nVt0 and https://www.nrk.no/video/PS*285664

Chapter 4
Culture or Biology? If This Sounds Interesting, You Might Be Confused

Sebastian Watzl

Introduction

Which differences between us are *biological* and which are caused by differences in learning, socialization, economics, upbringing, or, as it is sometimes generally called, *culture*? What is due to nature and what is due to nurture? These questions can seem important. They seem to matter for our self-conception – for who we are and can hope to be. They seem to matter for resource distribution – for which kind of research should be funded. And they seem to matter for policy-making – for which kinds of interventions are feasible or promising.

Questions about the role of biology tend to divide those studying or researching the social sciences. On the one hand, there is *biology attraction*. People in this group feel the appeal of a novel, naturalistic paradigm that promises to transform and rejuvenate the social sciences. Biology attraction may be fueled by the hope for a unified framework for understanding the human condition. On the other hand, there is *biology repulsion*. To the biology repelled, the rise to prominence of the life sciences at the university and in societal discourse feels like a hostile takeover that is at once naïve with regard to social science research and aggressive in its aspirations.

Public discourse a bout the relative contributions of biology and culture has a tendency to get politically charged. The biology attracted tend to view the biology repelled as avoiding reality in favor of well-willing ideology, as idealistic, and as driven by political rather than scientific motives.[1] The biology repelled, by contrast,

[1] Cf. Baron-Cohen (2004, pp. 29–34).

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tend to think of the biology attracted as reactionary, as favoring conservative policies, and at worst as playing into the hands of racists and sexists.[2]

What then *is* due to biology and what is due to culture? What is due to nature and what is due to nurture? If you are enthralled by such questions about our biological differences, then you are probably confused – or so I will argue. My goal is to diagnose the confusion.[3] In debates about the role of biology in the social world, it is easy to ask the wrong questions, and it is easy to misinterpret the scientific research. My diagnosis will help to explain why emotions in these matters often run high and why the debate tends to get political.

In the first part of this chapter (section "Psychological Essentialism and How It Thinks of "Biological Differences""), I will draw on evidence that suggests that in the public understanding, reference to "biological," "natural," or "genetic" differences tends to be associated with an essentialist picture of human kinds. The evidence suggests that because of a deeply rooted human psychological tendency called *psychological essentialism*, this picture has an easy grip on us. In public discussions of biology and culture or nature and nurture, it is the essentialist picture that dominates the debate, incites our emotions, and fuels the conflict between the biology attracted and the biology repelled.

The essentialist picture is a serious distortion of what biological research really contributes to our understanding of human social behavior, as I will review in the second part of the chapter (section "Psychological Essentialism Deeply Distorts Biology"): the notion of an essential difference between some human populations (and populations of other organisms) is foreign to biological thinking; traits and behavior that are heritable need not therefore be genetically caused; and if a difference between two groups is genetically caused, this does not mean that the difference is not caused by social structures and that it cannot be changed by learning or social intervention; social mechanisms may be so deeply intertwined with other biological mechanism that it makes no sense to ask about their relative contributions.

What Are "Biological Differences"?

Men on Steroids: An Example

Let us start with an example of how the biology/culture (and the nature/nurture) distinction is sometimes used. The example illustrates how a network of concepts and terms regarding "biology," "nature ", "genes ", "brain ", " hard wiring", "neurosci-

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[2] Some of the debate between the biology attracted and the biology repelled has, for example, played out in the public discussion in Norway following the release of the 2010 documentary "Hjernevask" (Brainwash) co-produced by Harald Eia and Ole-Martin Ihle.

[3] My discussion draws heavily on, summarizes, and connects the excellent work in Fausto-Sterling (2012), Keller (2010), Fine (2005, 2012, 2017), Gelman (2003), Leslie (2013), Richardson (2013), and others.

ence", "innate ", or "essential" tend to operate together in the popularization of biological explanations of social differences.[4]

The example concerns the biology of sex and gender. In a series of papers and popular books, neuroscientist and psychologist Simon Baron-Cohen distinguishes between a female and a male brain type. These types, it is argued, can be traced back to the different levels of testosterone produced by the male and the female fetus. Those fetal testosterone levels influence their brain development.[5] The alleged result is a differentiation between the female brain, which is an empathizing brain "predominantly hardwired for empathy" (Baron-Cohen, 2004, p. 1), and the male brain, which is a systemizing brain "predominantly hardwired for understanding and building systems" (ibid.). Sex-linked genes (e.g., on the sex chromosomes, XX and XY), according to Baron-Cohen, may also be "a major determinant of the male and female brain types" (ibid., p. 198) possibly acting through testosterone secretion. We can thus "be confident that genes controlling empathizing and systemizing will be identified" (ibid., p. 199). Of course, "[g]enetically and/or hormonally based neural systems underlying empathizing and systemizing still require the right environmental input (sensitive parenting, for example, in the case of empathizing) in order to develop normally. But identifying such genes or hormones will help us understand why, despite all the relevant environmental factors, some children are worse at empathizing, or better at systemizing, than others" (ibid.).

The claim that males and females have biologically different brains is argued to be an important part of the explanation of large-scale societal structures: female-brained people "make wonderful counselors, primary-school teachers, nurses, cares, mediators, group facilitators or personnel staff" (ibid., p. 185), while male-brained people "make the most wonderful scientists, engineers, mechanics, technicians, musicians, architects, electricians, plumbers, taxonomists, …., programmers, or even lawyers" (ibid.). This (as Baron-Cohen also points out) fits the contemporary gender distribution in many modern societies: in 2011, in Norway, for example, 89% of nurses and 83% of personal care workers were women, while 97% of machinery mechanics and 99% of building finishers were men.[6] Biological brain differentiation, according to Baron-Cohen, is an important part of the explanation of why men and women end up with different types of jobs. Baron-Cohen (2004) also

[4] I have deliberately chosen a *popular science* account that traces "social" structures and observations, as they are typically studied in economics, anthropology, sociology, and psychology, to their "biological" roots. The reason for this choice is that it is this kind of popular writing that is most likely directly encountered by most social scientists and humanities researchers, the one that is most directly in the public eye when it comes to debates regarding nature vs nurture, and the one where the problems I am interested in are often most pronounced.

[5] Baron-Cohen, Knickmeyer, and Belmonte (2005) and Baron-Cohen (2002, 2004). For an in-depth review of research on the role of testosterone for various types of social phenomena, see Fine (2017).

[6] These data come from the official statistics of *Statistik Sentralbyrå* Norway (Statistics Norway). See www.ssb.no/en/regsys (StatBank table 11411, www.ssb.no/en/table/11411) or the brochure "Men and Women in Norway 2018" available at https://www.ssb.no/en/befolkning/artikler-og-publikasjoner/women-and-men-in-norway-2018.

hypothesizes that the male/female brain differentiation partially explains the persistence of gender differences in math and physics education.[7]

This example illustrates an explanatory scheme. We start with something that seems clearly biological and independent of cultural or societal factors: here these are differences in genes and fetal hormone levels. These biological facts seem objectively measurable and clearly independent of any cultural or social factors (after all, the genetic makeup of an organism is determined at fertilization, and the hormone level differentiation occurs in the fetus already before most mothers, or anyone else, even know the gender or sex of their future child). In a second step, this biological factor is then argued to cause a similarly biological difference in brain development. In this case, there is stronger cross-hemispheric connectivity and activation in female brains and stronger intra-hemispheric connectivity and activation in male brains. The fetal hormonal difference brings about a "hard-wired" difference in the very structure of the brain. Third, these differences in brain anatomy and activation patterns lead to functional differences, i.e., differences in psychological traits and strategies. Here these are strengths in either empathy or systemizing. Fourth, the difference in psychological capacities in turn is supposed to scale up: individual psychological differences have societal consequences. If men and women have different psychological capacities, and use different psychological strategies, this, it is argued, must be part of the explanation for why they, for example, tend to be found in different occupations. Finally, we are offered a deeper evolutionary explanation for why the genetic differentiation with its social and behavioral consequences exists (see Fn. 7). The populations have different genes because they responded to different evolutionary selection pressures. The explanatory scheme thus moves from evolutionary history, over anatomy and physiology, to psychological differentiation and to social patterns.

[7] Baron-Cohen also offers some evolutionary speculation as to why do people have such different brain types (of the systemizing or the empathizing kind): Baron-Cohen suggests that such brains "have been selected [by evolution] as specializations for entirely different goals and niches" (Baron-Cohen, 2004, p. 225). The male, systemizing, brain type was good for "using and making tools," especially weapons, that could, for example, "have been a major advantage in male–male competition" (ibid., p. 203); it was good for hunting, and trading, gaining higher social status (which makes males attractive for females); to acquire and exercise social dominance, is linked to aggression, makes men tolerate solitude, specialized experts, and successful leaders. All these are aspects of the evolutionary niche of the human male. The female empathizing brain type, by contrast, Baron-Cohen suggests, was good for mothering (females, who Baron-Cohen thinks were the principal caregivers, may thus have evolved an empathizing brain); it is also good for making and keeping friends – the kind reciprocal relationships important to females who need community stability given the resources they invest in children and parenting; relatedly, an empathizing brain makes you good at participating in gossip which stabilizes dependable alliances, integrate into novel social groups (like the family of a male partner); it helps a female understand and be compassionate toward her partner and thus provide her with "a better chance of keeping her relationship stable during her offspring's vulnerable years, thus promoting their survival and the spread of her genes" (ibid., p. 223). The empathizing brain is thus hypothesized to be perfect for the female evolutionary niche. Baron-Cohen offers comparatively little scientific support for these speculations. For a powerful critique of such "evolutionary psychology" speculations, see Richardson (2010). Richardson shows how such speculations fall dramatically short of accepted standards in biology. See also Laland and Brown (2011).

This explanatory scheme no doubt, at least to the biologically attracted, appears powerful. Are we supposed to simply reject the evidence that male and female fetuses are exposed to different levels of testosterone? Are we simply supposed to ignore the genetic differences between men and women? Are we supposed to think that psychology has nothing to do with brain activation? Or are we supposed to think the psychological tendencies of individuals play no role in the explanation of which occupations certain groups of people tend to choose? Each step of the argumentative scheme can seem irresistible. In light of the availability of explanatory schemes exemplified here by Baron-Cohen's research, it can seem that one indeed would have to be "brain-washed" to reject that "biological sex-differences must play an important role in explaining why [for example] Norwegian women and men to such a large degree choose "traditional" educations, professions, and career strategies" (my translation) as the biology-attracted sociologist Gunnar Aakvaag (2015) suggests in a recent newspaper article.

What Is a "Biological Difference"?

What are the "biological sex differences" Aakvaag and others are talking about? While public debate about the relative contribution of biology and culture, or of nature and nurture, can be heated, what is at issue is often discussed very little; it is taken as implicitly understood. Glancing reference to the type of writing exemplified by Baron-Cohen, if even that, tends to be all that is felt needed to get the discussion going. But let us step back and ask:

When is a difference between groups of people a "biological" difference?

One way of understanding something to be "biological" is that it is the kind of thing that is studied in biology or in the biological sciences.

It seems unlikely that this is how the participants in the relevant public debates understand the issue. Biology is a multifaceted field with boundaries that aren't clearly delineated. In nonhuman organisms, *any* sex differentiation in (social) behavior, population structures, ontogenetic development, cellular and molecular mechanisms, neuronal processes, evolution, and more would be studied in the biological sciences. Researchers in biology may, when useful, use methods that originated in the social sciences (like game theory, first employed in economics). Further, most lay participants in the relevant public debates will not know where disciplinary boundaries are drawn in the academy and what methods are used where; and most academic participants will be cognizant of the fluency of methodologies and academic disciplines. When "how much is due to biology?" sounds deep and interesting and incites public debate, it is unlikely that it means "how much can be studied in the biological sciences? " It must mean something else.

Another approach is to focus not on what biological differences *are* but on the pragmatic effects of appealing to the "biological."[8] In this regard one might

[8] With regard to human "nature," Maria Kronfeldner (2018) makes this argument in detail.

emphasize, correctly I believe, that the concepts at issue are "essentially contested" (Gallie, 1956), because they mark domains of epistemic authority. The fact that their descriptive meaning is hard to pin down contributes to their contested nature. People are ready to fight vigorously over what about us is nature or what is biological because the use of these terms delineates who counts as an expert in the domain, who gets resources for its study and for changes or "treatment" in the domain, and therefore who gets power with respect to shaping societal discourse, setting agendas, and in the end also in policy-making.

I believe that their pragmatic function to delineate domains of epistemic authority is indeed an important aspect of why disputes about "nature" or "biology" have a tendency to become heated. But it doesn't yet explain what kind of epistemic authority "biology" indicates. Why does it sound more interesting (and more controversial) to ask which of our differences are "biological" than to ask which are "psychological" (and hence make specialists in psychology experts), "physical" (thus falling to the expertise of physicists), or "economical" (thus being the domain of economists)? In other words, why does appeal to the "biological" signal a special epistemic authority, especially when it comes to human differences?

Psychological Essentialism and How It Thinks of "Biological Differences"

Psychological Essentialism

In order to better understand both the attraction and the controversial nature of appeals to the "biological," I will argue in this section that we need to understand how it is integrated into an important aspect of our psychology. I will argue that the idea of "biological differences" and contributions of "nature" fits well and gets quickly incorporated into a highly intuitive (albeit false, I should already now say) tendency for thinking about human kinds. "Biological differences" are intuitively understood as differences in the internal, unchanging, and immutable *essence* of different kinds of people. Much of the public controversy, in my diagnosis, is fueled by the fact that its participants argue or are perceived as arguing about the viability and the reach of essences – often in vague and inarticulate ways. It is the essentialist picture that generates the strong emotions on both poles of biology attraction and biology repulsion.

In this section, I will review some of what is known about this intuitive essentialist way of thinking. In the next section, I will then show how and why "biology" gets co-opted by that way of thinking.

Psychological essentialism is a set of tendencies for how non-experts, including children as young as 3–5 years old, tend to group the members of certain kinds of individuals together on the basis of hypothesized, underlying, though unknown, features (cf. Gelman, 2003). This underlying "nature" is thought to make the individual the kind of individual it is and causes it to normally have its observable properties. Psychological essentialism is thought to be an important aspect of how people tend to think about both natural and social kinds. It describes an important aspect of our intuitive way of thinking about classification; it is what we do unreflectively, quickly, and automatically (and, thus, independently of exposure to real scientific research). Psychological essentialism has been shown to apply to natural substances like water or gold, biological categories like animal and plant types, but also social categories like race and gender (on which more below); it does normally not apply to artifact kinds like types of furniture or tools. Psychological essentialism appears to be a fairly universal aspect of human psychology and has been shown to exist in human communities around the globe (Gelman, 2003 and Heine, Dar-Nimrod, Cheung, & Proulx, 2017 for recent reviews).[9]

The tendencies described by psychological essentialism show up in how people classify individuals, explain, and make predictions. According to psychological essentialism, people implicitly posit an essence for a kind of being; they are said to "essentialize" a kind to the degree to which their intuitive thinking about that kind is governed by roughly the following features (cf. Heine et al., 2017):

> First, essences are held to be substantially and often quite radically *immutable*. Given the immutable essence of the kind, even radical transformations therefore will not change what the individual fundamentally is: a caterpillar that develops into a butterfly remains a member of the same kind, even though its outward appearance has changed radically (Rosengren, Gelman, Kalish, & McCormick, 1991). Importantly, this includes radical changes in the individual's environment, its upbringing, and its social encounters: children believe that a kangaroo will forever retain its kangaroo nature even if it grows up among goats (Gelman & Wellman, 1991).[10]
>
> Second, essences are held to be *internal* and *deep within* the organism. Children hold that essences are normally invisible. Changing the inside of an organism, children believe, is more likely to affect its essence than changes to its outward appearances (Gelman & Wellman, 1991).
>
> Third, essences – while invisible to the naked eye – are accessible to *experts*. As a result, "[c]hildren [, for example,] readily accept experimenter-provided labels, even when such labels are surprising and counterintuitive" (Gelman, 2003). Children hold that an expert like the experimenter knows best how to classify individuals. They defer to the expert's knowledge of essence in their classificatory practices.
>
> Fourth, essences tend to be all or nothing. Essentializing a kind therefore leads to *boundary intensification*. While children readily accept that a penguin is an atypical bird, it is still "definitely" a bird (Gelman, 2003). While for non-essentialized kinds, such as artifacts, people tend to hold that something can be a member of a kind to some degree but not fully (it's sort of a chair, but sort of a sofa too; sort of like a car and sort of like a motorbike); people tend to make fairly extreme category membership judgments about essentialized kinds even when they accept that an individual is an atypical member (Gelman, 2003).

[9] As shown in these reviews, the extent of these essentialist tendencies does vary with a number of other factors, e.g., socioeconomic status, and is more widespread in some populations than in others, e.g., Europeans vs East Asians.

[10] Note though that the essentialist beliefs about possible transformations for an individual are interestingly constrained: already 3-year-olds hold that a smaller animal cannot be a grown-up stage of a bigger animal (Gelman, 2003, p. 65).

> Children may think that penguins are atypical birds, but they do not think that they are sort of a bird and sort of something else.
>
> Fifth, essences *can be transferred* from one individual to another. This is so especially through biological parenthood: Gelman and Wellman (1991) as well as Heyman and Gelman (2000), for example, show that already young children believe that infants (human and animal) inherit some aspects of their essence from their biological parents even when they are adopted and grow up in a different environment.

All of these tendencies for essentializing kinds are present in children long before they learn anything about the biological sciences, are present also in communities that have not been exposed to those sciences, and govern also adult *intuitive* classificatory judgments.

Essentializing occurs especially for kinds we think and talk about a lot. Highly essentialized kinds tend to correspond to our subjectively preferred taxonomy, to what Eleanor Rosch has called the "basic level" of categorization (Rosch, 1978; cf. Leslie 2017 for discussion): the kinds for which names, for example, are learned first or for which we can list the highest number of distinguishing or salient features. Our preferred way of carving up the world appears to be in terms of these basic level highly essentialized kinds.

Why do we essentialize kinds? Psychological essentialism with respect to biological kinds, while deeply mistaken with regard to real biological thinking (as I will argue below), serves useful functions: it allows us to efficiently and quickly draw inferences regarding which appearances, forms of behavior, and other important properties will tend to come together. Because they are psychological essentialists, "[p]eople expect the disparate properties of a species to be integrally linked without having to know precise causal relationships" (Atran, 1998). Such a powerful set of inferences would otherwise be unavailable. Inductive inferences about the unobserved can be made on the basis of a few observations and knowledge of kind membership. By simply taking terms to stand for essentialized kinds, children can draw on community knowledge (or bias!) for their own generalizations since in the very use of the term expert (or guru!) knowledge gets encoded.

On evolutionary time scales, psychological essentialism may have become an important feature of human cognition because it was such a practically efficient inductive tool. It may have been evolutionarily available and beneficial for humans because of how sociality (including information sharing, extensive learning, and teaching), intelligence, and language use co-evolved in the human lineage (cf. Pinker, 2010). As Atran (1998) argues, psychological essentialism may have been biologically adaptive in our thinking about the organismic world, because it increased the efficiency of inferential reasoning in the biological domain at fairly low evolutionary costs: the individual differences between the members of an animal or plant kind often matter much less than what is shared between them. Under most circumstances, it is much more important to know that "lions have manes," that "bugs are disgusting," or that "the hemlock is poisonous" in order to avoid death and disease than to know about the many individual differences in appearance and behavior. By encoding psychological essentialism into the most easily acquired linguistic terms, we were able to make quick and powerful generalizations exactly when it comes to the most striking and – for our community – most important features of the world.

How and when the use of language supports essentializing is not extremely well understood. There is some evidence, though, that suggests that an essentialist understanding is encouraged both by the use of generic sentences and by the use of noun phrases (see Leslie 2017 for a review and further references). Generic sentences are sentences of the form "Fs are G" or "The F is G," e.g., "Lions have manes." They are to be contrasted with explicitly quantified sentences like "Some lions have manes," "Many lions have manes," or "All lions have manes." Evidence suggest that the use of generics contributes to essentializing the kind. Noun phrases are used in sentences like "Simba *is a lion*" and are opposed to the use of verb phrases like "Simba *has fur*." Children will essentialize more if a property is introduced by a noun phrase rather than a verb phrase ("is a carrot eater" vs "eats carrots"). The use of noun phrases and generic language thus can serve as linguistic means for transmitting essentialist attitudes in the community and across generations (Leslie, 2013).

Essentialist tendencies are known to be prevalent also when it comes to kinds of human or social categories.[11] In this case, essences and their consequences are attributed to certain types of people. Among the most essentialized human kinds are gender, race, ethnicity, and disability (cf. Haslam, Rothschild, & Ernst, 2000). On an intuitive level, already young children (but also adults) thus tend to implicitly accept the following ideas about humans:

1. Human individuals come in kinds that differ in their essences (some human kinds have essences).
2. The essence of a kind of human delineates sharp boundaries between groups of individuals (intermediary cases are impossible).
3. The essence of a kind of human consists in an internal feature shared by each individual of that kind (essences are internal).
4. Essences are invisible to the naked eye or casual observation: they are located deep within each individual (essences are invisible).
5. Essences can be known by experts, to whom non-experts will tend to defer when it comes to placing individuals into kinds of human (essences are known to experts).
6. Essences cannot be changed through the life span of an individual: they remain the same through changes in the individual's development or its physical or social environment (essences are immutable).
7. Internal essences causally determine a type-typical outward appearance and behavior. Other (developmental or environmental) causal factors shaping individual appearance and behavior can be separated from the causal role of essences and explain only deviations from the type-typical appearance and behavior (essences are separable causes).

[11] Rothbart and Taylor (1992); see also Prentice and Miller (2007) for a fairly recent overview.

Consider gender (cf. Bohan, 1993; Gelman & Taylor, 2000; Haslam et al., 2000): already young children tend to think that the differences between men and women reflect an underlying difference in internal features that make someone either a man or a woman (Taylor, 1996). Men have one kind of internal essence. Women have another. This essence (what makes someone a man or a woman) is not visible to casual observations. The essentialist child accepts that some men look like women and behave like them. But deeply within a man will always remain a man (and a woman, however much she dresses and behaves like a man, will always remain a woman). While a person's hair color may change through processes like dying and aging or through external factors like exposure to sunlight (since the kind "blond" or "dark-haired" is not strongly essentialized), children tend to think that whether a person is a man or a woman is *not* something that can change through her lifetime (once a woman always a woman). Further, children think that there is a sharp boundary between men and women. While someone can be sort of blond and sort of dark-haired (maybe they have some blond and some dark hairs; maybe their hair color falls in-between in some way), no one can be sort of a man and sort of a woman. Even if they are an atypical man, in many ways behaving and looking much like a woman, they are definitely a man (or they are definitely a woman). The boundary between men and women gets intensified. Finally, already young children believe that the behavior and appearance of a gendered person are partially due to effects of their gendered essence (whether they "really" are a man or a woman) and partially due to effects of how the person grew up and the environment they live in (a man may grow long hair or behave like a woman if he is surrounded by women or socialized in a certain way; but – children think – their nature as a man in the end can be determined by an expert).

This way of thinking about gender – as an essentialized kind – then is something that most of us find intuitive already when we are 5 years old and that we all continue to find intuitive even as adults. It is a reflection of a deeply rooted, evolutionarily old, and adaptive way of thinking about many aspects of the biological and social world.

Interpreting Biology as Concerned with Essences

It is therefore with those essentialist tendencies in them that lay people, but also many academics, hear about (popular representations of) research on our biological differences and approach the question of what is due to nature and what is due to nurture. In this section, I argue that there is good reason to think that they will map the new terminology to that already familiar way of thinking: what is due to biology or nature in a kind of person is understood as what is due to the essence of the relevant kind. By contrast what is due to culture or nurture is what does not spring from this essence.

"Fetal hormone levels," "genes," and "hard-wired" brain structures intuitively are an excellent fit for the role of essences. They are internal, invisible, and known to experts but not lay people; they are biological inherited, are portrayed as unaffected by environmental factors, and have important and intuitively separable effects on observable appearances and behavior. While not all popular writers are as explicit as Baron-Cohen in their claim to uncover "essential differences," I claim that especially lay participants in the public debate about "biological differences" or "nature" and "nurture" often intuitively understand the debate as being about whether to accept an essentialist picture of the relevant kinds of human. In Baron-Cohen's writings, we see, for example, a heavy emphasis on "types" (of brains and people) that are "determined" or "controlled" by fetal hormones and genes. These "types" can develop "normally" or abnormally, and the "right" environmental input is needed to get out the type-typical appearance or behavior, the one that is "supposed to" be the result – given the relevant essence.

In a number of studies, Dar-Nimrod, Heine, and colleagues (cf. Dar-Nimrod & Heine, 2011; Heine et al., 2017) have argued that people associate genes with essences. They argue that people tend to view genes as the materialization of unknown essences and are ready to transfer their intuitive categorization device to this scientific concept. Genes are internally located, can be transferred from parent to (biological) child, are unchanged through development and transformation of appearance and environment, are discovered by experts, and are supposed to explain many outward properties. "Because of this overlap with people's essentialist intuitions, we submit," so Heine et al. (2017), "that when most people are thinking about genes they are not really thinking about genes – they are thinking about metaphysical essences." It is thus no wonder that people are ready to view the power of genes in an almost mystical fashion (Nelkin & Lindee, 1995) and are very quick to explain all kinds of conditions in terms of those "genes": after all, here the experts are speaking about our deep nature that we were attuned to from early childhood on.

The gene-essence association, Heine et al. (op. cit) argue, leads to a number of (mis)conceptions about genes.

Given that genes qua essences are internal and immutable, we cannot change how they affect appearance and behavior. If a condition or behavior is caused by genes, it is therefore thought to be outside our control. Studies show that when non-experts read about the genetic origins of some condition or tendency, they will tend to form fatalistic attitudes toward that condition or tendency, i.e., they will tend to treat it as relatively unchangeable, and less subject to choice: people who read about research describing "obesity genes," for example, tend to eat more cookies afterward, compared to those who read research about how social networks affect obesity or those who read about non-obesity-related research (Dar-Nimrod, Cheung, Ruby, & Heine, 2014). Given that genes are viewed as essences, their effect is thought to be what is independent of environmental (including social and developmental) conditions.[12]

[12] Dar-Nimrod, Zuckerman, and Duberstein (2013), in a related study, focus on the effects of (apparently) learning that one has an "alcoholism gene." They show that this leads participants to experience negative affect and lack of control over drinking. Similar results are found in more complex domains: people who are led to think that learning styles (how someone learns most efficiently) have genetic causes tend to think that they have no control over their own learning style and that learning is best when learning styles are matched between teacher and student (see Heine et al., 2017).

Relatedly, people also have a tendency to think that if a condition has a genetic cause, it does not have another (e.g., environmental) cause as well. Genetic causes dominate other causes and exclude them. If obesity is genetic, people tend to think, then it does not matter how much you eat; either you become obese or not – independently of your behavior.

If someone believes that gender differences are biological, we would therefore expect that they think that these differences spring from the relevant essence of the gendered kind. And if they spring from those essences, they cannot be changed and have to be accepted as a given that is outside human control (just like for obesity). And this is exactly what has been found. Brescoll and LaFrance (2004) tested how subjects reacted to being presented with a biological rather than a cultural or social explanation of gender differences and found that "exposure to biological explanations significantly increased participants' endorsement of gender stereotypes" (p. 515). Similarly, Coleman and Hong (2008) found that an endorsement of a "biological gender theory [was] … linked to [a] stronger gender self-stereotyping tendency" (p. 34) (as reflected by greater endorsements of negative feminine traits and slower reaction time in denying stereotypic feminine traits). They found further that "this relationship holds even when the participants' sexist attitudes were statistically controlled" (ibid.).

Exposure to biological or cultural explanations of gender differences does not only influence people's explicit attitudes (whether they endorse a stereotype), there is also evidence that it affects people's performance on stereotyped tasks (Dar-Nimrod & Heine, 2006): in their experiment, women did a math test, after reading essays that they were told tested for reading comprehension. If those essays argued for a biological gender theory, then women's math scores were significantly lower than when those essays argued for a cultural, experience-based explanation. Indeed, exposure to the biological theory significantly lowered math scores compared to reading an essay on a neutral topic.

If people intuitively associate "biological" explanations as concerned with internal metaphysical essences, we can explain why the acceptance of such explanations leads to a fatalistic attitude with regard to the status quo.[13] What is biological is what cannot be changed through social means like education, and therefore we simply need to accept those biological differences as an immutable given. If it is a "biological" fact that women are bad at math, then – if you are a woman – it is not even worth trying. If sex differences are "biological," then they are essential to who we are, and therefore we must accept their type-typical results. Exposure to claims about a "hard-wired" or "biological" difference between male and female brains, since those claims are interpreted as concerning essences, therefore "quite independently of their scientific validity, have scope to sustain the very sex differences they seek to explain" (Fine, 2012).

[13] Related to boundary intensification, people who view a human kind as largely homogenous, and importantly and fundamentally distinct from other kinds, tend to also view membership in that kind as genetically caused.

Let us then look, returning to our case study, at how someone who is already – and has been from early childhood – a psychological essentialist would encounter Baron-Cohen's writings.

His 2004 book, the most popular exposition of his scientific research, of course is titled *The Essential Difference* (presumably referring to the "male and female brains" of the book's subtitle; but easily understood as holding between men and women as such: the book's Penguin edition cover, after all, showcases not brains but a man and a woman and their "typical" thoughts). Essential differences between the male and female mind (note the generic formulation) are again prominently mentioned at the beginning of the acknowledgments; a contrast is drawn between the claim that some of the observed differences between "the mind of men and women" (note the use of generics again) "reflect … differences in "essence" (p. 157)[14] as opposed to cultural factors. "Biological factors are the only other candidates" (p. 166) other than those cultural factors. Biology, in Baron-Cohen's writings, gets associated directly with the essence of man and woman (and their minds).

It is not only the explicit appeal to "essences" that triggers an essentialist reading of Baron-Cohen's exposition of his research.

Throughout the book, he uses the language of *types* of brains, thus strongly suggesting that population differences in neuroanatomy and neuronal processes can be traced to a difference between two types of brains ("the male brain type" and "the female brain type" or "brain type E" (for empathizing) and "brain type S" (for systemizing).[15] To speak of "types" suggests a deep and fundamental difference "in nature" (or who would speak, unless half-jokingly, of the blond and the dark-haired type of person).

The book is further full of generic language, often speaking of "the male brain" or "the female brain," but also of what women or men generically are like, do, or have evolved to do (in sentences without explicit quantifiers like "some," "all," or "many"). As we have seen, there is evidence to suggest that use of such generics will encourage essentializing the relevant kinds, and that generics are easily accepted on the basis of just a few striking instances, but tend to lead to overgeneralization to a large proportion of the essentialized kind.

Of course, Baron-Cohen also, at various places in the book, emphasizes that he is "only talking about statistical averages" (p. 20; see also p. 27, 185), that not all men have "the male brain type" and that not all women have "the female brain type," and that "your sex does not dictate your brain type." Unlike stereotyping, he stresses, "science recognizes that many people fall outside the average range for their group" (p. 28). But the psychological essentialist easily acknowledges such

[14] Scare quotes around "essence" are in the original. I suspect, though cannot show this, that with the use of the quotation marks, Baron-Cohen here shows some awareness that appeal to "essences" is considered scientifically unacceptable in the biological sciences. But note that he seems to be also happy to quite explicitly take the shortcut to get his readers to understand the distinction he aims to draw.

[15] The latter sounds more scientific than the former, but Baron-Cohen clearly associates them strongly and often combines the terminology, speaking, e.g., of a "male brain type S" (p. 20).

variations: their (implicit) view is an essence plus variations picture. Some members of an essentialized kind might appear and behave quite differently from the kind type, due to unusual circumstances, cover-up, or lack of "the right environment input" (Baron-Cohen, 2004, p. 1999). What all members of the kind share is an internal disposition (the essence) that could but need not manifest itself. To someone disposed to psychological essentialism, talk of "statistical averages" is therefore naturally interpreted as noisy variation around the norm for the "group" which is identified as the separable causal upshot of the type essence. And of course, psychological essentialists are ready to be corrected by experts. They do not, after all, think that essences can be directly predicted based on appearances or outward behavior. What outwardly looks like a man thus might well have inside its skull "the female type."

The psychological essentialist who is exposed to Baron-Cohen's work thus gets ample apparent evidence that the reported scientific research has uncovered the metaphysical essence of males and females (and the male and female mind) and has a ready interpretation for those places where strict boundaries appear to be denied.

Explaining the Controversy

We have seen evidence that psychological essentialism is deeply rooted in human psychology and that humans – including and especially children and lay people – think of differences between some human kinds, such as the difference between men and women, along essentialist lines, long before and independently of whether they have ever been exposed to biological research.

We have also seen evidence that psychological essentialism gets easily co-opted into an interpretation of biological research on differences between human populations. Differences described in terms of "genes," "hormone levels," or those that are "hard-wired" tend to be understood as differences that are due to differences in the essences of the relevant kind. The way we all tend to intuitively understand "biological differences" thus is as differences of essence, while those differences that are not biological (especially social or cultural differences) are the ones that are not due to a difference in the essence between the relevant kinds and in this sense merely accidental. What is biological thus cannot be changed through education or social arrangements, while other differences can be changed through such means.

This explains why appeal to our "biology" carries special epistemic authority. While we may not know exactly what is biological and what is not, it matters deeply what is part of biology and what is "merely" social: what is biological is what carves human kinds along their essential joints. Experts in the "biology" of sex and gender thus are intuitively understood as experts in what makes gendered people the kind of people they are. Since the difference between what is essential and what is not essential is so important and yet hidden from the observation of behavior and appearances, it is going to be highly contested what falls on which side. We can thus explain the contested character of the concept of a "biological difference." The difference between nature (or biology) and nurture (or culture) seems deep and important exactly because it aligns with the difference between essence and accidents that psychological essentialism gives us.

We have also seen that this essentialist thinking has social consequences: since what is essential is immutable, people are not motivated to try to change or counteract what is due to biology (it can after all not be fundamentally changed but only "covered up" in its effect on behavior or appearances). Biological differences are thus seen as differences that form a neutral and objective background against which all policy making or social arrangement must be taking place and not themselves as differences that can be affected or even eradicated by changes in social arrangements.

This way of aligning the essentially unchangeable with what is biological, internal, hidden, and given from parents to offspring, as we have seen, is present already in young children. It is not a scientific discovery that there are "essential differences" between men and women. Children and lay people already believed that there are exactly such differences and stood all too ready to believe that biology unearthed them in its talk of genes, hormones, and brain wirings. Those who defend biological differences between, say, men and women are thus understood as defending an unchanging and unchangeable difference in the essence of the type "man" and the type "woman." If differences in which professions men and women tend to choose are due to "biology," it is thus understood that such differences will not disappear, whatever social arrangements we may come up with (and the same for differences in math performance or empathy).

It is thus no wonder that those with a progressive political view will be opposed to "biology": the more is seen as due to biology, the more about us cannot be changed and thus presumably is not worth trying to change. Those who advocate social change thus won't like "biology." By contrast, those with a conservative political viewpoint will be happy to see the realm of the "biological" increase. After all, it supports their view that certain aspects of how things are should not be subject for attempted changed (after all, they cannot be changed, and so its hubris to *try* to change them).

The essentialist understanding of our "biological differences" thus explains why debates about them get the hearts racing and have a tendency to become political. They are debates about what should be taken as a given background and what is amenable for social change. We have a fairly well-developed psychological explanation for why the nature/nurture or biology/culture debate *seems* deep and important.

Psychological Essentialism Deeply Distorts Biology

Psychological essentialism explains why the question about our "biological differences" psychologically seems to us deep and important. Is the picture of the world of organisms (including humans) provided to us by psychological essentialism even halfway adequate? Do kinds of organisms have essences of roughly the sort psychological essentialism tells us they do? Is the picture of genes, hormone levels, and brain wiring as corresponding to essences a roughly correct picture of how real biological research thinks of them? The answer, I will argue in this section, is no. Essentialism of the kind we have encountered is a serious misrepresentation of biological research.

There Are No Internal Biological Essences

While, as we have seen, appeal to "biological differences" in public discussion and in popular books like Baron-Cohen's tends to be closely associated with differences in "essences," it is almost universally accepted within biology that no population of organisms (no kind of organism) has anything like the intrinsic, internal, and immutable essence psychological essentialists intuitively posit. Ernst Mayr, one of most influential biologists of the twentieth century, famously contrasted "typological thinking," which he found in the philosophical tradition of Plato and Aristotle, with the "population thinking" that characterizes modern biology. He writes that, according to biology,

> [a]ll organisms and organic phenomena are composed of unique features and can be described collectively only in statistical terms. Individuals, or any kind of organic entities, form populations of which we can determine the arithmetic mean and statistics of variation. Averages are merely statistical abstractions, only the individuals of which the populations are composed have reality. The ultimate conclusions of the population thinker and of the typologist are precisely the opposite. For the typologist, the type (eidos) is real and the variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different. (Mayr, 1959, p. 2)

The population thinking that Mayr describes here is diametrically opposed to the essentialist picture. Mayr's view that an evolutionary and biological approach to humans and other organisms is not compatible with essentialism about the relevant kinds is widely shared among biologists and philosophers of biology. With regard to what biology thinks about the idea of "human nature," another influential biologist, Michael Ghiselin, sums the idea up as follows: "What does evolution teach us about human nature? It teaches us that human nature is a superstition" (Ghiselin, 1997, p. 1). The widespread evolutionary consensus against essentialism is not specific to humans; it applies to all biological kinds. With regard to whether biological species – the paradigm of essentialized kinds for those in the grips of psychological essentialism – have internal essences, philosopher of biology Eliot Sober (1994) says "essentialism about species is today a dead issue" (p. 163; cited also in Okasha, 2002, p. 191).

Why is this? As Samir Okasha (2002, p. 196) puts it, "[e]mpirically, it simply is not true that the groups of organisms that working biologists treat as con-specific share a set of common morphological, physiological or genetic traits which set them off from other species." There is a lot of intra-species genetic variation, often more than between species, and while many members of a species share certain genetic features, there is no set of genes that makes an individual a member of that species. Indeed, such genetic variation within all populations is essential for the operation of processes of natural selection, and therefore such variation "is fundamental to the Darwinian explanation of organic diversity" (Okasha, op. cit., p. 197). The Darwinian view of organismic populations has no room for distinguishing essential aspects of a kind of organism from their accidental features.

Biological species, like *Homo sapiens*, are not defined by any essential features shared by all of their members but rather are individuated by reference to their place in the tree of life (they are the result of speciation events) and certain types of interactions that are possible between the members of the species (typical reproduction). Whether an individual is a human thus, biological, has little to do with its intrinsic characteristics, but rather with its historical connection to a certain constantly changing, evolving, population.

While there are heated controversies within biology and the philosophy of biology about how best to think about species and the species concept, what is almost universally accepted is that biological species are *not* individuated by an internal essence that all of its members share. There are no tiger genes located deep within (the cells of) each tiger that make that individual a tiger. There is also no set or cluster of tiger genes. There is no internal, intrinsic property that makes something a tiger.[16] Psychological essentialism thus delivers a deeply wrong picture of species.

What holds for species also holds for other biological kinds. Specifically, it holds for sex differences. Psychological essentialism is also deeply wrong about the kind "male" or "female." Males and females do not have anything like the essences the psychological essentialist posits.

First, it is important to note that sex differentiation is not uniform across the organismic world. It is highly diverse and far from universal. Most organisms, especially the prokaryotes, do not reproduce sexually at all or have more than two sexes; many – especially plants – use sexual reproduction only occasionally; there are a good number of animal species where sex is determined through the environment (in crocodiles and some turtles, e.g., sex is determined by the temperature in which an egg is incubated), in stark contrast to the internal determination of the male or female kind the psychological essentialist believes in. Further, several animal species can and do change their sex within their life span (in many snails but also in some fish), in contrast to the immutability of essences posited by psychological essentialism. Even in those organisms where sex is stable over the life span and more dependent on internal features, there is a variety of sex-determining mechanisms rather than the uniformity of types suggested by the essentialist picture: in

[16] Note that this does not mean that there is *no* sense in which species have essences. It is compatible with the denial of classical essentialism about species (of the kind psychological essentialism appeals to) that species have, for example, historical and/or relational essences: what makes something a tiger is its relationship to other organism, both at a time (with regard to possible biological reproduction) and over time (as a member of a certain biological lineage). See Okasha (2002) for more discussion.

insects like bees and ants (hymenoptera), for example, unfertilized eggs become males, while fertilized eggs generally became females; and even those that use a chromosome-based system use a number of variations. If we look across the organismic world, there isn't anything like an intrinsic and internal male or female essence of the type our intuitive psychological essentialism posits.

Second, let us consider human sex differences specifically. There are, of course, sex chromosomes in humans, where most males have the XY genotype and most females have the XX genotype (though there are exceptions). These sex chromosomes, for those already inclined toward essentializing sex and gender, may appear like natural candidates for playing the essence role of the relevant kinds. They are, after all, located deep within an organism, are already present in the embryo, and seem to draw a sharp boundary between the male and the female "type." Are the sex chromosomes at least well suited to play the essence role for *human* males and females?

The answer is fairly clearly no. The sex chromosomes contain nothing like a "blueprint" for how males or females are "supposed" to develop under normal conditions even with regard to the primary sexual organs or the sex differentiation in fetal testosterone levels we have encountered in Baron-Cohen's work. Human primary sex differentiation is a complex process that involves many aspects of the genome interacting in complex ways (see Dupré, 1986, Fausto-Sterling, 2012; Keller, 2010). Gonadal sex differentiation, i.e., the development of testes and ovaries (which later end up being involved in the production of estrogen and testosterone), consists in a complex interaction of "two active and opposing signaling pathways" (DiNapoli & Capel, 2008, p. 4; cited also in Fausto-Sterling, 2012, p. 20) involving a variety of genes on several chromosomes blocking and enhancing each other. There is simply nothing like a gene that in some sense "stands for" the production of the female or the male type of primary reproductive organs. Because sex differentiation is such a complex multifaceted process, there are no sharp boundaries between the relevant types. And given the complex pathways leading, for example, to various different levels of estrogen and testosterone production, there is no ground for, say, speaking of something "defective" or "abnormal" in a high level of testosterone in an XX fetus. Biological populations simply do not allow us to speak of something like "normal" types.[17]

To sum up, biological population thinking applied to men and women would precisely *not* speak of "essential" differences, or differences in the nature of men and women. Rather, it would speak of the statistical correlation between certain traits, variations within certain parts of human populations, and the complex interacting developmental pathways leading to the development of those traits.

[17] We can, of course, speak of more or less "fit" organisms of a certain genotype, where such fitness would be related to the overall expected number of offspring. But that fitness will always depend on the specific environment and be relative to the overall population. An organism that has relatively little fitness in one natural or social environment might have a very high fitness in a different environment.

Heritability Does Not Imply Genetic Causation

Let us move to another aspect of how psychological essentialism leads us astray. The psychological essentialist interpreter of "biology," as we have seen, thinks of the essences that characterize social and biological kinds as heritable: they are internal features that an individual inherits from its biological parents, and which make it the kind of individual it is. The notion of a "heritable" difference between two groups of people, for the biological essentialist, therefore, is naturally understood as a difference that can be traced to a difference in an internal essence. Since differences in essences are understood as independent of environmental factors, someone in the grip of essentialism therefore intuitively interprets heritable differences as those that are independent of the environment, such as – in the case of humans – social or political factors.

Heritability is, indeed, an important biological notion. Yet, the biological notion of the heritability of a trait, like being good at empathizing or systemizing or showing certain characteristic patterns of brain activation, does not entail that the development of this trait is largely independent of environmental factors. The intuitive grip of psychological essentialism makes it easy to confuse the biological notion of the heritability of some trait with the notion that this trait is genetically determined (or rather determined by those mythical essences with which we tend to confuse genes) (see Lewontin, 1974 or Block, 1995. For some recent debate about how much about genetic causation can be determined by heritability analysis, see Sesardic, 2003, Oftedal, 2005).

The biological heritability of some trait is defined as the ratio of the genetic variation and the total variation with respect to that trait. Heritability is therefore only defined with respect to specific populations of organisms that differ in that trait. We can, for example, ask about the heritability of systemizing abilities in, say, the Norwegian population. But it makes no sense to ask whether, say, *my* systemizing abilities are heritable. The psychological essentialist in us wants to associate the biological notion of heritability, which applies to populations, with the notion that the heritable trait is in some form "given" like a legal inheritance "in the genes" (or essences) from a parent to its offspring. But the biological notion of heritability is completely silent on how the relevant trait is transmitted from one generation to the next.

A high degree of heritability for some trait difference therefore does not entail that the trait is in any sense genetically caused or determined (as I will show in the next section, it is highly unclear whether there in fact *is* a biologically acceptable notion of "genetically determined"). Ned Block (1995) illustrates this with an example closely tied to the gender differences we have been discussing. Suppose that in a certain population almost only women wear earrings. Most of these women will have XX chromosomes (I've briefly touched on some of the complexities of sex differentiation in the last section). In this population, the trait "wearing earrings" is highly heritable: the total variation with regard to the trait can almost all be "traced" to a genetic variation (having XX chromosomes as opposed to having XY chromosomes), and so the ratio of genetic variation and total variation will be close to one. Heritability will therefore be very high. This does absolutely not entail, though, that XX chromosomes in some way, independently of cultural norms or the environment, "determine" that a developing person will wear earrings. Cultural norms may change, and have changed, and as a result, many men may also start wearing earrings. As a result, the heritability of wearing earrings will now drop. In the new population, where it is fashionable for both men and women to wear earrings, the heritability of wearing earrings therefore is very low.

What holds for earrings also holds for systemizing and empathizing abilities. Suppose it were true that in a given population, e.g., today's Norway, most women were much better empathizers than men. Given that most women differ from most men genetically, it would then follow that in this population, empathizing abilities are highly heritable, since most variation in the empathizing trait can be traced to a genetic difference and the ratio of genetic variation to total variation would be close to one. This, though, would do nothing to show that those abilities are in any interesting sense genetically "determined." A change in social structures, such as schooling or parenting, may well eliminate the empathizing differences or lead to their reversal. The biological heritability of the trait in one social setting is compatible with the trait *not* being heritable in a different social setting.

The quick association between heritability and genetic determination suggested to us by our intuitive psychological essentialism is, therefore, deeply mistaken.

Genetic Causation Does Not Preclude Environmental Causation

Psychological essentialism, as we have seen, leads people to treat genes as separable and independent causes of appearances and behavior. When people view obesity as having a genetic cause, for example, they treat obesity as something that would develop independently of any environmental effects on obesity: the essence of obesity is present in all individuals carrying the relevant gene. The environment acts only by either allowing or preventing the "normal" development of the carrier of the obesity gene, or by adding a further layer of statistical variation around the "norm" for that genotype. The essentialist treatment of the gene thus naturally leads to the view of genes as blueprints, in which the finished "types" are already preformed. Lay people therefore easily accept the notion that there are genes "for" a large variety of traits, from blue eyes, and obesity, to empathizing or systemizing abilities.

But this way of thinking about genes is deeply mistaken.

First, it is controversial whether genes play *any special* role in development (cf. Oyama, 1985; Griffith, & Gray, 1994; Oyama, Griffiths, & Gray, 2003). An organism's development is influenced by a large variety of factors; DNA and RNA interact with the various other molecules in the cellular matrix, in ways that are strongly dependent on the environment of each cell, be it temperature, various gradients of growth factors that cross cellular boundaries, to the nutrients in the cell's or fetus' ambient environment. As the fetus grows, the influence of intracellular (e.g., genetic) and extracellular (e.g., environmental) factors becomes even more heavily intertwined. This complex developmental process is fairly reliably replicated from one generation of the organism to the next. Those favoring a so-called developmental systems approach (Oyama, 1985, Oyama et al., 2003) hold that the various factors in the developmental process are on a par (they accept what has been called the "parity thesis"; Shea, 2011). The causal role of genes in the developmental process, according to this approach, is no different from the role of other intracellular molecular factors or environmental factors. For example, we should treat the reliable replication of an organism's environment that is shaped by the parental generation as in principle on a par with the replication of DNA from one generation to the next: a termite embryo, for example, in the same sense "inherits" the symbiotic bacteria that will help it break down cellulose for nutrition, the stable temperature of the termite mound, and the interactions of worker termites and its DNA. Similarly, a human fetus "inherits" in the same sense social conditions, protective structures like houses, the stable temperature of its mother's womb, and the DNA. If the parity thesis advocated by the developmental systems approach is right, then there is nothing – specially not genes – that can play the role of "biological differences" that can in any interesting sense be distinguished from other factors, specifically social factors. We would therefore reject any notion that genes play anything like the role of internal "essences" that "stand for" certain traits that a normally developing organism is "supposed to" develop.

Second, if even the developmental systems approach rejected, and genes are understood as playing a special role in development, they would still not play anything like the role of "coding" for high-level features such as brain structures or systemizing capacities. Genes operate in complex regulatory networks, and – uncontroversially – code for enzymes that facilitate or suppress biochemical reactions, reactions that also depend on the environment, like which nutrients are available. The psychological essentialist in us likes to draw a distinction between those differences between us that are due to genes (the "biological differences") and those that are due to the environment. But genes, through the production of enzymes that facilitate biochemical reactions, always produce their effects through their action on how one cell interacts with others and the environment: genes, as it were, "tell" the cell how to react to certain environments or changes in those environments. The question whether a difference is due to genes or due to the environment therefore makes little sense, as each effect of genes is mediated by environmental variables. Even if we therefore accept that genes are interestingly different from other causal factors, since they are "read' by development (Shea, 2012) and "code' or "stand for" certain things, what they stand for wouldn't be anything like a trait like obesity, systemizing or empathizing abilities or even blue eyes, they would rather be instructions of something like the form "In conditions C1, do X1!" and "In conditions C2, do X2!" Given how genes actually operate, there therefore is no answer to the general question of whether some difference is genetic or environmental (and there certainly is no answer to the question, whether, say, my empathizing abilities are genetic or environmentally caused).[18]

[18] Lewontin (1974, p. 401) explains this point by reference to an analogy: "If two men lay bricks to build a wall, we may quite fairly measure their contributions by counting the number laid by each; but if one mixes the mortar and the other lays the bricks, it would be absurd to measure their relative quantitative contributions by measuring the volume of bricks and of mortar."

It is important not to misunderstand what I have just argued. It is compatible with the claim that genes are not separable causes and that it may be true that in a given environment or in a given range of environments, the difference between people with trait T1 and those with trait T2 can be well explained by a genetic difference between those people. It may well be true that, say, in the environment of contemporary Norway, a large amount of the variation in empathizing abilities is explained and caused by whether a fetus has XX or XY chromosomes (and other differences in sex-linked genes). This is compatible with a different effect of those sex-linked genes in a different environment. The sex-linked genes might, for example, act through the development of primary sexual organs, how caregivers and others react to babies with those primary sexual organs and how they then treat the baby. In a different environment (where caregivers react differently to babies with certain sexual organs), the very same genes might have a very different effect. We therefore cannot conclude from the fact that a certain difference has a genetic cause that its effects cannot be dramatically altered through changes to the social and cultural setting. The fact that a certain difference has a genetic explanation simply does not speak to whether that difference also has a social and cultural explanation.

Summing up this section: unlike the mythical essences with which psychological essentialism lets us identify genes, the causal effects of real genes cannot be separated from environmental factors. The fact that a difference has a genetic explanation does not preclude that it also has a social or cultural explanation.

Hormone Levels Change and Hard-Wired Brains May Be Flexible

As we have seen, the psychological essentialist treatment of biological and social kinds, of biological heredity, and of the causal role of genes presents a deeply distorted picture of biology.

Similar distortions result when statistically significant differences in testosterone and estrogen hormone levels both in the fetus and later in life are interpreted as hormone levels of the male or the female "type." As Fine (2017) shows in a detailed and accessible review, testosterone levels and their production in the gonads, for example, vary greatly both in males and females and are known to depend – in both adult humans and other animals – also on social factors. Androgen hormone levels do influence social factors, but they are also influenced by social factors in turn (see also Francis, Soma, & Fernald, 1993; Oliveira, Silva, & Canário, 2009). The impact of fetal testosterone levels on brain development and on future behavior is further highly complex and multifaceted, as Fine's review shows in detail. There is simply no sense in which hormone levels either as a fetus or later in life can be well described as falling into a male or female "type." The psychological essentialist treatment of hormones as immutable essences that specify the essence of the male or the female is deeply wrong with regard to how androgens operate in development and in how they are involved in shaping behavior.

Similar problems also arise when the term "hard-wired" is used to describe aspects of the human (or other organisms') brains, neurobiology, and also psychology. The general idea behind this metaphor is that just like some aspects of the possible internal processing of a machine (including, but not exclusive to, computers) are wired into the hardware, and this impossible to change once the machine has been fully assembled, e.g., through software change in a computer, so some aspects of brain processing, or psychological processing, are wired into the very hardware of our brains. But on the reflection, the "hard-wiring" metaphor just points us back to the problematic notions of a genetic blueprint. What is hard-wired into the brain is what cannot be changed once the brain (machine) is fully developed. But when is the brain "fully developed"? Certainly not at biological birth. Some aspects of a specific adult's brain might be unchangeable *then* but clearly are the product of learning and contingent brain development during *youth* (consider the acquisition of our native language).

One way to distinguish those aspects of our psychology that are "hard-wired" is to think of them as *innate*. Yet, while the notion of "innateness" figured heavily in the early ethological research of researchers like Konrad Lorenz (1957 [1937]), who thought that innate characteristics could be revealed by deprivation experiments where an animal is supposed to be stripped of all relevant environmental input, the notion was already heavily critiqued in the 1960s so that, for example, one of the most influential ethologists Niko Tinbergen (who had worked closely with Lorenz in the 1940s) came to think that any such deprivation experiment could only show "which environmental aspect was … not to be influential" (Tinbergen, 1963, p. 424) and that the notion of innateness in the end was probably rather "heuristically harmful" (ibid., p. 425). Today the notion of innateness is sometimes used in psychology (see Griffiths, 2017 for a review). The notion of an innate characteristic may here just mean a characteristic that is universal in humans, one that is best studied by biology rather than psychology, one that is not learned on the basis of experience, or one that has been an evolutionary adaptation. None of these notions would imply that the development of an innate characteristic could not heavily depend on the environment and social structures, in contrast to how psychological essentialism thinks about what is "within us." Indeed, a growing number of biologists and philosophers of biology follow Tinbergen and argue that the notion of innateness is problematic, confused, and of little scientific use (Griffiths, Machery, & Linquist, 2009; Mameli & Bateson, 2006; Moore, 2001): arguably it is a remnant of exactly the misleading essentialism that governs our intuitive way of thinking about the biological world that we have already discussed and has no interesting value in real developmental biology and psychology (cf. Griffiths et al., 2009). Whether or not that is true, any useful scientific notion of innate characteristics will be certain to avoid any link to the idea that those characteristics cannot be affected through social change or is essence determining.

Psychological essentialism thus also misleads us about the role of sex hormones as stable and determining characteristics of certain kinds of people, and it misleads us with regard to the distinction between what is innate and what is acquired.

Conclusion

Culture or biology? The question which of what is due to "biological differences" to many seems deep and important. Those who argue for an important role of "biology" in the explanation of human differences often see "the science" on their side. I have argued that this is false – on the interpretation of "biological differences" that is most intuitive and that makes the question appear to be most interesting. Defenders of "biology" have the science against them. What is often called "biology" is a myth: a myth created by an intuitive tendency that grotesquely distorts real biological research.

I have argued that we are intuitively attracted to psychological essentialism, which let us interpret what is biological in distinguishing human kinds as what can be traced to the "essences" of the relevant kinds. On this interpretation, it would be deep and important to know what about, say, the differences between genders is biological: it would correspond to what is essential to being a man or being a woman and be opposed to what is a mere accidental feature that some women or some men have. Yet, I have also argued, the psychological essentialist understanding of "biological differences" is also deeply mistaken about biology. It has the wrong conception of biological kinds, of biological heritability, and of how genes and hormones work.

Does this mean that everything about us can be affected through social changes? Of course not. But instead of confusing the public debate by asking what is due to biology or nature, we should rather directly discuss the complex causal explanation of, for example, how the genders end up in different types of occupation (at a particular time, in a particular culture) and which types of interventions are effective. The answers will probably be complex. We will need a good deal of biological understanding to discuss them productively. And we will need a good deal of social science. But the idea that we can sidestep the complexities by instead asking about nature vs nurture rests on a mistaken conception of the biological world. Responsible research and public debate about biology would avoid any talk of biological difference, of nature vs nurture, of types of brains or people, and probably also of whether there are genes for this, that, or the other.

The unconscious appeal of the essentialist picture contributes an explanation to why we fall so easily for the "mirage of a space between nature and nurture," as Evelyn Fox Keller (2010) has put it. When we start to debate the relative contributions of "nature" and "nurture" or the importance of "biological differences" in the explanation of some social patterns, we most likely have already fallen into the trap that our essentialist inclinations have set up for us. Those on the biology repulsion side of the debate are right that "biology" is associated with an outmoded, false, and socially explosive way of thinking about humans, namely, the essentialist picture. But the only way to move beyond that is biological literacy: we should follow the biological attracted in their appeal for better education in real biological mechanisms and the real science of human evolution. More biology, there is reason to hope, will let us move beyond the misguided debate over our "biological differences."

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Chapter 5
Conditional Objectivism: A Strategy for Connecting the Social Sciences and Practical Decision-Making

Rolf Reber and Nicolas J. Bullot

In 2014, Richard Dawkins triggered a controversy after responding a Twitter user who asked about what to do "if I were pregnant with a kid with Down Syndrome." He responded: "Abort it and try again. It would be immoral to bring it into the world if you have the choice." Later, Dawkins explained his position in a text published on his website. He wrote:

> Given a free choice of having an early abortion or deliberately bringing a Down child into the world, I think the moral and sensible choice would be to abort. And, indeed, that is what the great majority of women, in America and especially in Europe, actually do. I personally would go further and say that, if your morality is based, as mine is, on a desire to increase the sum of happiness and reduce suffering, the decision to deliberately give birth to a Down baby, when you have the choice to abort it early in the pregnancy, might actually be immoral from the point of view of the child's own welfare. (Dawkins, 2014)

Alluding to his adoption of a utilitarian and consequentialist approach to morality (Mill, 1861/1969; Singer, 1979), Richard Dawkins argues from the standpoint of the child's welfare when advocating abortion of a baby with Down syndrome. In contrast to discussions about abortion dominated by religious justifications, current discussions based on consequentialist reasoning focus on arguments that aim to integrate empirical evidence into moral decision-making. For example, the child's own welfare – if well defined – could be examined on the basis of hypotheses integrating measurements of indicators of human flourishing and harm. Similarly, the contributors to the debate presented in the book *Abortion: Three Perspectives*

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(Tooley, Wolf-Devine, Devine, & Jaggar, 2009) take different positions, in part by relying on different scientific evidence to defend their position. Each author presents scientific evidence for their own position. In support of abortion from a utilitarian perspective, Tooley argues that a fetus is not a person before birth, referring his own review of studies (Tooley, 1983). To underpin her feminist pro-choice stance, Jaggar cites research showing that more women die on childbirth than abortion (Dixon-Mueller & Dagg, 2002) and that unwanted children flourish less (e.g., Kubicka et al., 1995). Finally, in favor of their pro-life position, Wolf-Devine and Devine refer to studies on the damaging psychological consequences for women who conduct an abortion (see Fergusson, Horwood, & Ridder, 2006). Although the three perspectives are different, all refer to empirical inquiries into indicators of human flourishing and harm to support their practical and consequentialist recommendations.

The previous cases of use of empirical findings raise three problems of normativity. First, as demonstrated by the different standpoints debated by Tooley et al. (2009), the empirical results that could be used to justify different moral and political decisions are haphazard. The authors tend to rely on findings of their choice instead of comparing the same body of relevant empirical research. Use of empirical results is often not motivated by making a sound decision but by lending the appearance of authority to opinions (see Boswell, 2009; called symbolic use by Amara, Ouimet, & Landry, 2004). Such a state of affairs reinforces the impression that decision processes in policy making are complicated and cannot be based on rational criteria (Albæk, 1995; Lindblom, 1965).

Second, the authors presented their findings as though the empirical finding could justify a recommendation. However, it may seem to their critic that their use of empirical evidence corresponds to post hoc justifications of moral positions already taken. That is, the authors cherry-picked the findings that offered the best fit with their personal viewpoints.

Third, cherry-picked selection of empirical evidence indicates that the authors adhere to values and valuations that are left tacit or are difficult to ascertain. For example, Wolf-Devine and Devine's argument tacitly assumes that the adverse psychological impact on mothers matters more than the well-being of the child. By contrast, Jaggar thinks the opposite (see Tooley et al., 2009).

These concerns raise a number of important questions. First, can empirical evidence provide direct support to decision-making in the domains of ethics and policy? Second, is it warranted to argue that research should be free from value when research is developed in a context where it could have major societal effects, either beneficial or harmful? To illustrate this second problem, consider the case of a scientist who believes that the well-being of children is significantly worse off when living with Down syndrome than when living without this syndrome. Does that view automatically entail that abortion of fetuses carrying the extra chromosome causing Down syndrome is morally and socially justified? Or do some a priori moral arguments outweigh any empirical considerations? Although we do not aim to specify a final answer to these complex questions, we provide in this chapter an analysis in two steps of the role of scientific evidence in such debates.

In the first part of the chapter, we propose *conditional objectivism*, a strategy that aims to guide decision-making in the face of value-laden subject matters of scientific inquiry. To explain the heuristic procedure by means of which the strategy provides practical recommendations, we use a decision tree. In the second part of the chapter, we outline various open problems with making evidence-based recommendations for practice. These include motivated testing, data interpretation, including and weighing values, side effects, intuitive judgment, and relativism. We do not discuss other problems related to value-free social science because they are not directly relevant for making recommendations. For example, a number of authors do not assess the methodological rigor and quality of the studies cited to justify their viewpoint. Neither do they reflect on the theoretical background that could connect a study to the values they advocate. Finally, some researchers do not design a study from purely scientific interest but are motivated by values when elaborating research questions (see Brinkmann, 2011, 2019). In this chapter, however, our focus will cover neither the motivations in the beginning of a study nor the scientist's value judgment in accepting or rejecting a hypothesis (Rudner, 1953). We focus on the recommendations provided as part of the publication process or once its findings have been made public.

How to Derive Practical Recommendations from Empirical Evidence

Theorists in the tradition of David Hume argue that scientists do not have any permission to derive recommendations from their research (see Cohon, 2010). This comes from the idea that no *ought-judgment* (i.e., normative statements about practical matters) may be correctly inferred from premises comprised of *is-judgments* (i.e., true factual statements). However, scientists violate this rule. First, scientists routinely break the Humean rule by making recommendations expressed by practical ought-judgments from factual statements expressed by is-judgments despite advised not to do so. This is illustrated above by Dawkins' recommendation to the Twitter user. Second, if scientists did not derive recommendations from their research, some practitioners and policy makers would do it. Finally, it is desirable that scientific evidence informs judgments in practical fields (e.g., Douglas, 2007).

There are two main ways to derive a recommendation, either from a protected value or from a utility value. This distinction falls in line with two important approaches in Western moral philosophy: deontology (e.g., Kant, 1785/1996) and consequentialism (e.g., Mill's utilitarianism). With respect to deontology, it is possible to derive a practical recommendation for or against abortion from protected values such as the respect for human life as an end in itself (Kant, 1785/1996) or the respect of a woman's bodily and psychological integrity (Thomson, 1971). With respect to consequentialist reasoning about utility, researchers and policy makers may approximately compute evidence-based recommendations when it is well-defined what future suffering for child and parents means. We show below that such computation faces intricate difficulties (see also Simon, 1997).

Drawing on the theory of heuristics applied to scientific decision-making (Gigerenzer & Gaissmaier, 2011; Wimsatt, 2006), our proposal is that the complex linkage between practical recommendations and empirical evidence can be facilitated by heuristics. In this chapter, we focus on heuristic structures by means of hierarchical decision trees. We provide an example of such trees below. Further, we introduce the dichotomy between *conditional statements* and *evidence-based advocacy* in order to address the value-related (axiological) problems faced by contemporary social sciences. What follows is a step-by-step commentary on the decision tree presented in Fig. 5.1.

Protected Values

According to the core heuristic of conditional objectivism (see Fig. 5.1), when researchers are asked to provide value-based recommendations, they may first ask whether they are dealing with values that are protected within some individuals or their cultural group. We use the term *protected values* to denote a set of practical norms that are viewed by advocates within a cultural group as non-negotiable and requiring strict adherence under all types of circumstances (Baron & Spranca, 1997). That is, protected values are understood by their advocates as warranted in all possible contexts of action. Some values are not completely protected because these values, albeit prioritized to some extent, can be challenged and changed by means of a special and exceptional procedure (e.g., compelling empirical evidence about the harm of the value; referendum to change constitutional laws). Although there is a continuum from strictly protected values to defeasible and inconsequential values, we shall present our heuristic with the dichotomy protected values versus defeasible values for the sake of simplicity.

In the context of a debate of the practical implication of empirical evidence, the concept of *protected values* is preferable to the concept of *sacred values* (e.g., Tetlock, 2003). This is because the scope of the former is broader than that of the latter and the concept is less likely to be affected by biases for or against religion. Although some sacred values can operate as protected values, protected values do not need to derive from religious persuasion alone. For example, the strict ban on lethal medical experiments on humans as conducted by Nazi doctors (see Lifton, 1986) cannot only be derived from sacred values grounded in religion but also from non-religious protected values grounded in humanism. If a researcher adhered to a protected value, he or she would not seek scientific evidence to support an argument that relies on this protected value. For example, social scientists do not gather evidence on advantages for public health of lethal medical experiments on humans because the rejection of killing humans for the sake of scientific progress is a protected value for social scientists and society at large. Depending on the cultural context, protected

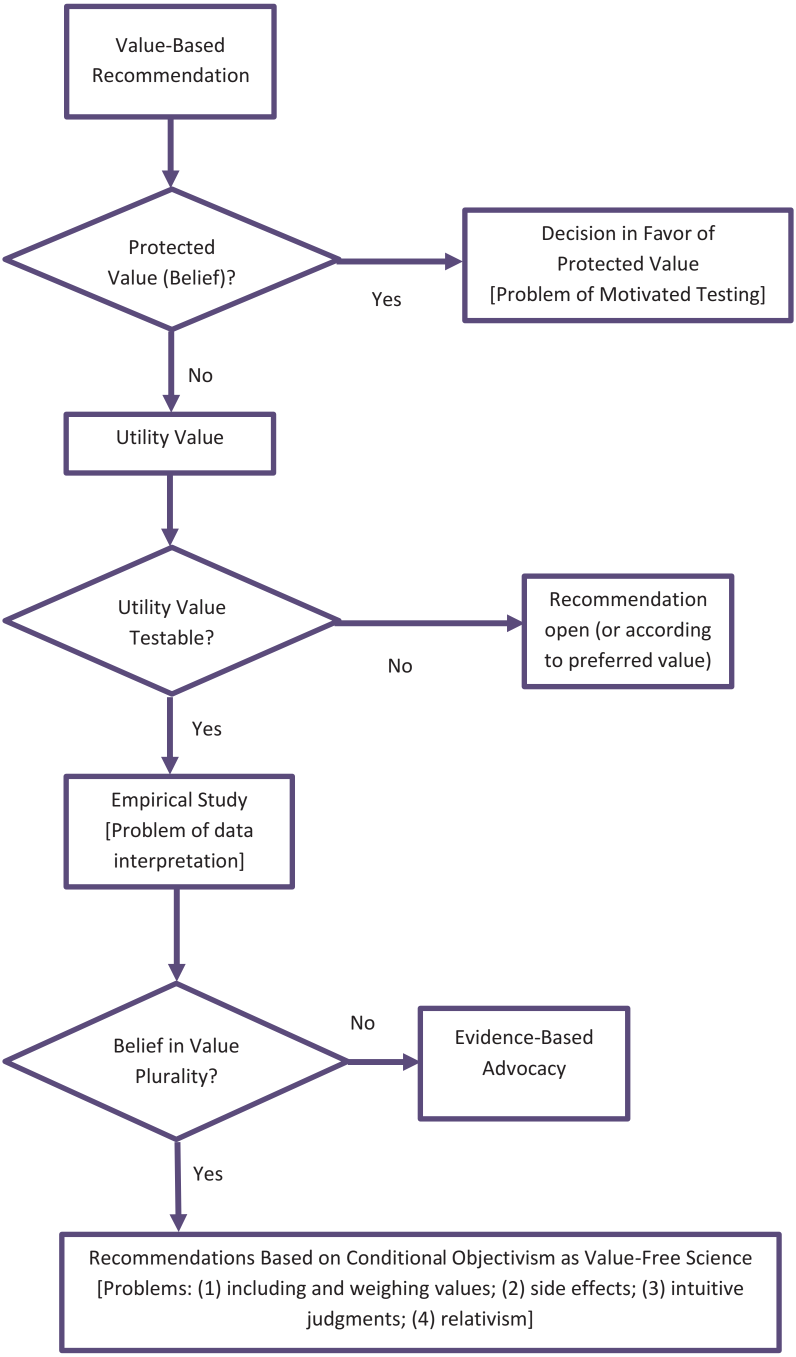

Fig. 5.1 A basic heuristic of conditional objectivism

values may include the protection of life, rituals, gender equality, or protection of the environment. A person's belief that expresses protected values is not defeasible by arguments grounded in empirical evidence.

Consequentialist Test of Utilitarian Value

If there are no protected values or a group or individual does not adhere to it, utilitarian criteria may come to the fore. Utilitarian values derive from superordinate, often implicit values. For example, a pro-choice advocate in the abortion debate may give more weight to the well-being of the mother when evaluating the effects of abortion (Thomson, 1971). We do not discuss where utility values come from because conditional objectivism can be used to look at them from different viewpoints. The origin of the value component in calculating expected utility thus remains a black box. However, we assume that there is agreement on what these values might be but not on how they should be weighted. For example, it seems clear that the respect for life, health, and well-being of child and mother and the financial, social, and cultural prosperity of the state, to name a few examples, are values that provide utility. However, it is unclear how much weight is attached to these values in making a judgment about the acceptability of abortion.

If the expected utility cannot be determined, for example, because there are no methods available to test it, the issue remains either undecided or it is judged by defeasible values. In the abortion debate, citizens may lean toward pro-life or pro-choice attitudes without holding protected value. Thus, these moderate people may be ready to switch their position if evidence became available; their opinion is in line with their value only as long as they do not know of any evidence.

If the expected utility can be tested, researchers can proceed to conduct the test. The outcome of the test decides whether a policy could be recommended or not. For example, several countries banned indoor smoking in restaurants after it became clear that restaurant employees had a higher risk of lung cancer, compared to office workers (see Siegel, 1993). Based on this data, Siegel concluded with the recommendation that "To protect these workers, smoking in bars and restaurants should be prohibited" (p. 490). Similarly, some countries banned capital punishment after it became clear that it is not effective at reducing crime rates.

There are two ways scientists can put forward their recommendations, depending on whether or not they endorse value pluralism.

Plurality of Values

Let us assume that the test provides unequivocal evidence that supports a certain value-based viewpoint. A scientist might now adhere to a certain, presumably preferred viewpoint and neglect that there are multiple viewpoints to consider. In the smoking hazard example reviewed above, Siegel (1993) uses data indicating that working in a restaurant allowing smoking increases risk of lung cancer to recommend the smoking ban. His recommendation is a case of *evidence-based advocacy* because in it the scientist uses evidence to support a single standpoint. By contrast, if a scientist accepts that multiple viewpoints exist and that each of these viewpoints needs to be considered, she should use conditional statements to make clear that she has no vested interest in one single standpoint but looks at the issue impartially. In our smoking hazard example, the scientist could have asked what measures could be taken to protect restaurant workers if indoor smoking were permitted, which would accord with a liberal approach to legislation. Alternatively, the scientist could have considered what would be the best solutions for the country's economy, without concern for the restaurant workers. Analogous to this reasoning, there have been discussions on whether it would be cheaper for the economy to execute criminals instead of imprisoning them for life (which does not seem to be the case; e.g., Spangenberg & Walsh, 1989). A decision based on using conditional statement to critically examine a plurality of values corresponds to a decision complying with *conditional objectivism*.

Conditional Objectivism

The Principles of Conditional Objectivism

The idea underlying conditional objectivism comes from philosophy of history and history of science. Morten White (1965) proposed that it is a fallacy to assume that a historian looking at scientific problems from a certain (political) viewpoint equals its endorsement. Instead, it is appropriate to see a problem from several viewpoints. Historians have to make conditional statements to clarify that a certain finding or explanation obtains provided we look at the problem from a certain viewpoint. The idea is not new; already David Hume advocated what Daston (1992) calls "perspectival suppleness," the ability to assume myriad other points of view, rather than the total escape from perspective implied by the "view from nowhere" (Daston, 1992, 604). This emphasis on perspectival flexibility as a condition of objectivity is also defended by a number of epistemologists, who have developed models of inference to the best explanation and epistemic virtues (see Lipton, 1991/2004).

To address the problem of value freedom and objectivity, conditional objectivism does not make the contention that a neutral viewpoint exists or is possible to adopt. Conditional objectivism posits that cognitive and social scientists can distance themselves from a viewpoint by using *contrastive reasoning* based on comparing conditional statements. This hypothesis extends to the domain of practical decision-making, an idea that is central to recent research on contrastive thinking in knowledge attribution (e.g., Lipton, 1991/2004; Morton, 2013; Schaffer & Knobe, 2012). For example, in the evidence-based practical decision, contrastive statements may be conditional counterfactuals of the form “If we were analyzing this problem from viewpoint x, question y would have to be posed to adequately address problem z” or “If seen from viewpoint x, observation y leads to recommendation z.”

If we apply the logic of counterfactual reasoning to the case of abortion from Dawkins’ standpoint, that position rests on this counterfactual: “If considered from the viewpoint of the well-being of children with Down syndrome compared to genetically typical children, then the evidence indicates that Down children are worse off than genetically typical children. This evidence leads to the recommendation that abortion of embryos carrying the extra chromosome causing Down syndrome is justified from that perspective.” By contrast, seen from Wolf-Devine and Devine, the counterfactual may be expressed as follows: “If considered from the viewpoint of negative psychological consequences for mothers who abort their child, then the evidence indicates that the negative psychological consequences for a mother who aborts a child are worse than those experienced by a mother who does not abort the embryo. Thus, this evidence leads to the recommendation that abortion of embryos carrying the extra chromosome producing Down syndrome is not justified from that perspective.” Conditional objectivism demands the contrastive analysis of several of such counterfactual conditionals.

Why Conditional Objectivism and Similar Approaches Are Needed

In her book *Happiness and Education*, Nel Noddings (2003) argued that if there were no individual differences in personality characteristics, “we could seek and recommend one best way of raising all children” (p. 181). The hidden assumption behind the belief in finding a uniquely optimal way to educate children is that all parents educate them to adhere to the same set of values and to act accordingly. As soon as we acknowledge that parents may convey different values to their children and that humans can live in radically different cultural contexts, there is no longer one best way to educate children, even if personality differences were non-existent (see Reber, 2016). A careful consideration of the literature reveals that many social scientists share Noddings’ lack of pluralism; she simply made this point explicit. Social scientists typically lack Noddings’ care to spell out the values that are distinctive of the cultural context that they assume and start from. They may assume that there is one best way to do things, as practical recommendations in journals ranging from political science to special needs education testify (or, to refer back to an earlier example, Siegel, 1993, when he recommended a smoking ban inside restaurants). Such recommendations are often derived from unarticulated values favored by the authors. These observations show that it is important to make explicit the researchers’ values, be such values about abortion or any other topic in the social sciences.

According to Max Weber (1946/1919, 1949/1917), scientists are part of rational institutions that adhere to scientific values, including value freedom, and promote scientific virtues, such as intellectual humility, disinterestedness, and impartiality (see Paul & Elder, 2002). One may compare scientists and scholars at universities with judges, tax officers, police officers, or health professionals. It is a common assumption in Western societies that judges, tax officers, and police officers would do great harm if they did not serve the citizenry neutrally and introduced bias that undermined the credibility of their host institutions. There is ample research on how biases and partiality in these institutions hurt the functioning of the state and hinder fairness in economic distribution and judicial proceedings (e.g., Rose-Ackerman, 2007; van de Walle, 2007). We predict that the same harm is done in the social sciences when researchers select those variables or even findings that fit their political opinions or worldviews without acknowledging value plurality. Research has just begun to document the political biases at universities (Cardiff & Klein, 2005; Inbar & Lammers, 2012; see Duarte et al., 2015; Klein & Stern, 2009), and there needs to be research that documents the economic or juridical harms carried out by the lack of value neutrality by social scientists. It is conceivable that biased recommendations by social scientists harm the legal system and may incur financial costs. More importantly than these admittedly speculative consequences may be the introduction of policies on the grounds of recommendations where scientific research did not serve to adjudicate between options but to support a preconceived position. In the end, such lack of value freedom may undermine the trust of citizens in science.

In the twenty-first century, psychological and social scientists rarely discuss values. They often present research as value-neutral when in fact it is value-laden (see Brinkmann, 2011). This is a problem because the faculty at social science departments is politically one-sided on the left (Klein & Stern, 2009). Such one-sidedness leads to politically one-sided research and recommendations. Duarte et al. (2015) propose to employ politically more diverse faculty. However, political screening of faculty members would raise other problems (e.g., violation of privacy rights). Conditional objectivism may solve this problem by proposing a procedure that warrants value-neutrality in making recommendations from scientific evidence. Making conditional statements creates a distance between the messenger and the message, or the scientist and the recommendation (see White, 1965). Plurality of values is acknowledged when scientists take the most obvious and relevant values into account for making recommendations. Such an attitude is democratic because it supports pluralism and prevents citizens to look at scientific evidence from a single perspective (mostly the scientist's favored).

Open Problems

In the following, we discuss six problems of practical recommendations based on evidence. These problems pertain to different steps in the decision tree depicted in Fig. 5.1. Note that we do not address problems of research design and statistical interpretation (see Kampen & Tamás, 2014) or systematic reviews (Sherman, 2003). Instead, we focus on points that cannot be solved entirely by improving methods or providing systematic reviews to make better recommendations. The first open problem is motivated testing. The second is the problem of data interpretation that occurs at the end of the empirical study. The final four problems are raised by the task of making recommendations. We listed them inside the conditional objectivism box in Fig. 5.1 because we are interested in open problems in relation to conditional statements. However, some of these problems also pertain to recommendations as evidence-based advocacy. The third problem is that even when researchers adhere to value plurality and put their recommendations in conditional statements, the question comes up which values to include and how to weight concurring values. Fourth, a recommendation may lead to undesired side effects. Fifth, some values depend on intuitive moral values. Such values seem insufficiently underpinned by rational reasons. Finally, conditional objectivism faces the problem of relativism.

The Problem of Motivated Testing

If a person's decision and action are controlled by a protected value, empirical evidence is unlikely to influence that person's decision-making. For example, from a stern pro-life stance, abortion must be strictly forbidden. From a stern pro-choice point of view, it must be strictly allowed. If held absolutely, both views prevent the use of empirical evidence for decision-making on the issue at stake. A decision-maker with protected values would not initiate a research program on the consequences of abortion. This is because the protected value solely controls their decision. In the terminology introduced by Gaston Bachelard (2002/1938), protected values are "obstacles" to scientific inquiry. The observation that protected values serve as obstacles to start a research program is not meant to be taken as a normative statement to the effect that scientists should not conduct research on issues that threaten protected values. Of course, groups for whom a value is defeasible may start a research program on issues that undermine the protective values of others. For example, scientists began to explore medical and social aspects of abortion despite the fact that the protection of unborn life has been a protected value for many individuals and groups (e.g., Porter & O'Connor, 1985). On the other hand, there are limits to challenging protected values, as illustrated in cases of unethical research with humans during World War II (Lifton, 1986).

Decision-makers guided by protected values would only initiate research in order to support their own views if confronted with others who doubt the absoluteness of their value. For example, adherents of the pro-life view do not have pressing reasons to instigate empirical research on consequences of abortion unless they meet adversaries who seek to legalize abortion rights. In the latter case, pro-life adherents may decide to instigate a research program aimed at showing the negative consequences of abortion for physical and mental health. However, this empirical enterprise could backfire. For example, if it had been shown that women who give birth to a child with Down syndrome suffer from more severe psychological consequences than mothers who abort such a child, the pro-life supporters would provide a utilitarian argument that contradicts their protected value. Of course, the same applies to pro-choice advocates. They may not be motivated to conduct research about positive consequences of abortion unless they are motivated to show pro-life adherents that abortion is advantageous. Again, this strategy may backfire; if it had been shown that despite health problems, the subjective well-being of children with Down syndrome is greater than or at least equal to the subjective well-being of healthy children, the research would undermine the point of the adherents of the pro-choice movement.

Although it increases the risk of being proven wrong, initiating research to gather evidence may serve the purpose to persuade the opposite side. We call this kind of testing *motivated testing*, in line with the term *motivated reasoning* that denotes reasoning guided by a partisan viewpoint (see Kunda, 1990). Like motivated reasoning, motivated testing is predicted to lead to biased research outcomes. Adherents to protected values know that other groups and individuals may not entertain the same protected value. Empirical evidence may persuade the undecided or people who entertain defeasible values. In some cases, different people may support protected values that are diametrically opposed to one's own. For example, one individual's protected value of safeguarding an embryo's life under all circumstances may contradict another individual's protected value of warranting a woman's sovereignty over her own body. This may lead the adherent to a protected value to attempt to persuade the opposite party by means of an argument grounded in empirical evidence.

The strategy is not to attack the protected value of the adversary head on; but it aims to show by means of empirical evidence that the adversary's position comes at a cost in terms of expected utility. The strongest argument to undermine the opposite position would consist in showing that the imparted harm directly stems from the protected value that guides the decision-making process and that the harm done violates the protected value.

Let us assume for the sake of argument that more lives were killed when abortion is prohibited because so many women submit to dangerous abortions that the toll of lives – children plus women – would be higher than the toll of legal abortion. This outcome might undermine the goal to save as many lives as possible and would bring the pro-life adherent into a defensive position.

A similar logic applies for the contrary argument. Let us assume it turned out – again for the sake of argument – that women in fact have less choice when abortion is allowed because the pressure to abort an undesired or disabled child afflicts more women than the prohibition of abortion. Again, an advocate who supported the right to abortion with the argument that women should have a free choice would run into problems because such a finding would undermine the central tenet of pro-choice.

In sum, motivated testing means that advocates of a viewpoint instigate a research program to convince others with an opposite viewpoint. Their research question is guided by their vested interest, and they may suppress – by not publishing the research – findings that do not fit their viewpoint. Alternatively, they may be biased when interpreting their data.

The Problem of Underspecification in Data Interpretation

When the data of an empirical inquiry have been analyzed, the researcher has some freedom to interpret them, for example, as supporting or not supporting a certain viewpoint. There is much leeway to infer their moral implications. Even if the results are unequivocal, there are at least three ways in which inferences about practical implications could be affected by underspecification and offer rooms for diverse interpretations.

The first kind of leeway in interpreting the implications of empirical results consists in the strength of the recommendations. Does the finding that children with Down syndrome have worse health outcomes than genetically typical children – if this is taken to be the decisive value – render abortion morally neutral (compared to negative), acceptable, commendable, or imperative? Interestingly, Dawkins argued that it would be immoral not to abort it. This suggests that a scientific finding can be used in at least two ways: either to argue that it would not violate a moral rule to act in a certain way (acceptable) or – more radically – that it would be immoral not to act in that way (abortion would then be a moral imperative). Apparently, Dawkins did not have any empirical evidence to distinguish between the two alternatives. The difference could be seen in the following: when utilitarian arguments overrule deontological arguments – not to prevent life to come into being – the judgment turns from the relative immorality of abortion due to the duty to protect life into a judgment that the act of abortion, at least in this case, is not immoral. By contrast, if one looks at abortion from the viewpoint of a technical procedure without moral implications, then the finding that children with Down syndrome suffer considerably would render the prohibition of aborting the fetus immoral.

Second, and related to the first point, how strong has the quantitative effect to be before researchers can make a recommendation? As everyone with basic knowledge in statistics knows, there are two parameters regarding the difference between two conditions, one that determines the level of certainty with which a difference exists and the other the size of the effect. Examples of the former are the level of significance, credibility in Bayesian statistics, or confidence intervals (but note that we do not use the term “certainty” in a technical sense here; for statistical fallacies around such terms, e.g., Gigerenzer, 2004). Examples of measures of effect size include the correlation coefficient r or Cohen’s d (see Rosenthal, Rosnow, & Rubin, 2000). There is agreement among social and behavioral scientists and statisticians that the effect size but not the level of significance tells us something about the importance of a difference for practical applications. However, there is still leeway to argue that even a small effect supports a policy or that a recommendation needs a large effect size.

Third, even if the effect is large, one need to ask what qualitative difference needs to be evidenced before the empirical finding can be used for making a practical recommendation. When we examine Dawkins’ argument, how much of a difference in the quality of the ailment between healthy children and children with a disability would be needed for him to persuasively argue that prohibition of abortion is immoral? Colorblindness would probably not qualify, although it would put constraints on the choice of a profession. A child with multiple organic and mental deficiencies plus the prospect of severe chronic pain without chance of recovery would probably fall into Dawkins' category of future human beings for whom it would be immoral to prohibit abortion. Yet, children with Down syndrome are somewhere in between colorblindness and the most severe cases of disability. They suffer from higher disability and ailments than healthy children and have more restraints when it comes to future life options (e.g., Schieve, Boulet, Boyle, Rasmussen, & Schendel, 2009). Yet with some help they seem to have the prospect to lead a happy life (Robison, 2000) and are known for their good-natured temper (Blessing, 1959). In general, the severity of the handicap is not categorical but continuous and therefore makes it difficult to set a clear boundary that separates moral or immoral decisions.

Unequal representation of political opinions in social science departments may lead to biased recommendations because of one-sided interpretation of data. Social scientists may use the degrees of freedom to interpret the data to make recommendations that match their own opinion.

Conditional objectivism offers a partial solution to address this problem. The solution is only partial because individuals who entertain protected values may not be willing to make evidence-based recommendations. However, scientists who conduct scientific research to gather evidence often do not resort to protected values, but they may suppress results that are critical to their own viewpoints and principles; they do not publish their findings or publish them selectively. Another strategy is to tweak the interpretation of the data in a way that underpins the scientist's values. Too often, recommendations are one-sided and not reflected because scientists do not take value pluralism seriously. They may make recommendations based on post hoc interpretations of data that favor the researcher's value. This could be the case if a researcher with protected or favored values tries to convince opponents of his or her own viewpoint by presenting empirical data, as outlined earlier. As the research on motivated reasoning shows, identical results could lead to opposite conclusions (Kunda, 1990). As the data in the social sciences often include potential methodological weaknesses (e.g., unmeasured control variables) or yield unclear results (e.g., problems of inferring causation from correlation, etc.), there is not only a temptation but also the possibility to interpret the data in one's own favor.

Therefore, it needs to be specified beforehand which kind of evidence would count as supportive. One solution to this problem could be adversarial collaboration, that is, two researchers who advocate opposite protected values work together to agree on a fair test of their assumptions. However, if scientists adhered to conditional objectivism, they would look at what would be a fair test from both sides, and they may define the range of results that would speak in favor of one or the other side and a middle range where the findings are equivocal.

Maybe the most proper solution would be preregistration where the methods of a study are reviewed and accepted and the result is accepted whatever the outcome (which becomes more and more a requirement in psychology; see Wagenmakers, Wetzels, Borsboom, van der Maas, & Kievit, 2012). This would prevent issues analogous to the case of Regnerus (2012), whose study raised much opposition from progressive scientists because it found differences in the outcome of adults who grew up in intact heterosexual families from adults who grew up in households with homosexual relationships. As these outcomes favored heterosexual parents, they were not deemed politically suitable to argue in favor of same adoption rights for same-sex parents (see Redding, 2013). The same applies for findings that show disadvantageous consequences of daycare in infants (see Belsky, 2003). Such studies receive much more scrutiny – either under the review process (Belsky, 2003) or after publication (Regnerus, 2012) – than studies whose findings are politically less sensitive.

The Problem of Including and Weighting Values

We have presented a case study on evidence-based recommendations for or against abortion. We contrasted two main values in the abortion debate, namely, well-being of the child versus psychological consequences for the mother. However, these are not the only values in the debate, and in principle, an infinite number of values could be considered. For example, societies appreciate low crime rates, and measures to achieve low crime rates would be welcome. Indeed, some observations suggest that legalized abortion in the USA resulted in lower crime rates (Donohue & Levitt, 2001; popularized by Levitt & Dubner, 2006). After legalization, crime rates began to decline at the time the aborted fetuses would have reached the age when they would have been most prone to be criminal. In addition, there was a correlation between number of abortions in a state and its later crime rate, suggesting that higher abortion rates were associated with lower crime rates. Therefore, researchers might use conditional objectivism to state that if seen from the viewpoint of crime reduction, abortion has positive consequences and would be commendable. The issue of including values reappears when we discuss relativism as a danger for conditional objectivism.

In general, we have discussed the simplified case of considering one value to support evidence-based practical recommendations. One could imagine that researchers take into account several values simultaneously that are either additive, multiplicative, or weighted. For example, an abortion decision may be based on the well-being of the child, the mother, or both. However, a more complicated array of values would not change the principles of our approach, at least not at this point.

The Problem of Side Effects

A serious limitation is that actions may have unanticipated side effects that are often undesirable (the classical source is Merton, 1936; see Elster, 2007, for a more recent treatment). Let us come back to the example of Dawkins in the introduction. He based his recommendation to abort an embryo with the extra chromosome leading to Down syndrome on a utilitarian argument about maximization of happiness and minimization of suffering. Apparently, he thought of the future happiness or suffering of the child. However, one of the possible side effects could be that the argument leads to a slippery slope on the way to utilitarian arguments on the value of life that may result in open acceptance or even adoption of euthanasia and eugenics, as it was the case in Nazi Germany (see Friedlander, 1995). Another, related side effect could consist in the pressure on women to abort a baby with a handicap like Down syndrome. On the other hand, arguments on negative psychiatric consequences for mothers who conducted an abortion may support the recommendation to prohibit abortion. However, possible side effects include illegal abortions that jeopardize the health of the mother and stigmatization of women who conduct an abortion. Although there may be evidence for some side effects so that they could be taken into account in outlining a policy recommendation, many side effects will be difficult to predict at the time a researcher makes evidence-based recommendations.

The Problem of Intuitive Judgments

Although we are not examining the genealogy of values in the present chapter, we ought to address one aspect of the process that leads to value judgments. Haidt (2001) argued that a wide range of moral judgments are based on intuition rather than on reasoning, as some ideals of Western moral ethics would prescribe it. In a striking example of this phenomenon, disgust sensitivity has been shown to predict intuitive disapproval of gay people (Inbar, Pizarro, Knobe, & Bloom, 2009). There are at least two ways to deal with intuitive, feeling-based moral judgments. The first option is simply to suppress them, in line with dominant Western thinking. However, feelings may have adaptive functions, and critical reasoning deprived of feelings and emotions may not suffice for optimal (moral) decisions (see Damasio, 1994; de Sousa, 1987; Reber, 2016). Therefore, it may be better to choose an alternative option, which recommends using intuition or feeling-based values in the same way as rationally derived values and use conditional statements to evaluate them. The conditionals may be expressed as “from the viewpoint of the intuitive outrage when confronted with abortion, it would be recommended to prohibit abortion” or “from the viewpoint of intuitive repulsion of hearing that a woman has to give birth to a child that is the result of rape, it would be recommended to allow abortion.” We tend to recommend the second approach to intuitive judgments because suppressing the use of feeling-based values would be a value-based judgment in itself, and feelings have some rational justification regarding decision-making (see Reber, 2016, for a discussion).

The Problem of Relativism

Finally, a serious concern is moral relativism. Indeed, thinking in terms of conditional statements and contrasting such statements do not necessarily distinguish among morally acceptable and unacceptable recommendations. The upside with this kind of relativism is that it broadens thinking about potential consequences and opens understanding for other viewpoints. For example, in order to understand a terrorist, researchers have to see the world like a terrorist does (Atran, 2010). Research on the most effective ways to recruit suicide bombers might be discussed in terms of how to best recruit suicide bombers or how to prevent terrorist organizations from recruiting suicide bombers. Yet, it may lead to a science risking to become an amoral enterprise, and in a sense it should be because scientists ought not to take sides when carrying out their work. Else, their enterprise is no longer value-free science but evidence-based advocacy; for example, this happens when social anthropologists take sides with their host community and become social advocates (see Sanford & Angel-Ajani, 2006). However, three measures that are germane to conditional objectivism could mitigate the potential costs of relativism.

First, universities might welcome not only scientists but also political activists whose role would be to petition views supported by evidence. In the social sciences and humanities, it is difficult to draw a clear line between science and evidence-based advocacy. If evidence-based advocacy was accepted at universities as a standard academic practice, the problem of political imbalance in social science departments that we discussed earlier would reemerge, and it may indeed be optimal (though hardly feasible in practice) to include political balance in recruitment policies.

Second, as discussed earlier, scientists can never cover all aspects related to recommendations. They will always draw on a selection of aspects that are relevant to the most prevalent practices within a community (c.f. Brinkmann, 2011; MacIntyre, 1985). In homogenous communities, scientists may take the prevalent viewpoint; in heterogeneous societies, as most modern democracies, there may be practices that contradict each other and lead to contradicting values, such as the pro-life and pro-choice stances on abortion.

Third, while scientists qua scientists have to be value-neutral, scientists qua citizens may express their opinions as any other citizens. However, they would not do it in the name of science but as private persons. Such scientists would switch from science to evidence-based advocacy if they used scientific evidence to underpin their political opinion; they would switch to (mere) advocacy if they argued from protected values.

Summary and Conclusion

A major problem in the social sciences and philosophy pertains to making practical recommendations derived from empirical evidence and scientific research.

Too often, researchers consider only one value when making their recommendations, most probably their favored value. This is an unfortunate state of affairs because it contradicts one of the main principles of scientific inquiry, namely, value freedom. Conditional objectivism contributes toward resolving this problem. Conditional objectivism argues that researchers have to:

1. Acknowledge value plurality.
2. Consider the relevant standpoints in a debate before making practical recommendations in light of each standpoint.
3. Reason on the basis of counterfactual conditional statements.

These statements are of the form "if viewpoint *x* were to obtain, then finding *y* would suggest that practitioners should follow practical recommendation *z*." By using such conditional statements in their reasoning procedures, scientists can create a distance between themselves or their opinions and the subject matter of their research. Their use of conditionals signals that they do not necessarily endorse the viewpoint they take when making practical recommendations. We used abortion as a case in point to illustrate the tenets of conditional objectivism. Beyond abortion and other moral issues, the heuristic of conditional objectivism could be applied to decisions about artistic value (Bullot & Reber, 2013), rationality and justice (MacIntyre, 1988), and meaning in life (Reber, 2018).

Conditional objectivism offers new solutions to address several problems that have beset the social sciences. First, it prevents the confusion between value-free science and value-laden activism. Scientists can continue to conduct value-free research and nevertheless make recommendations without being suspected of violating the principle of value freedom. If researchers fail to use the contrasting of conditional statements, their recommendations amount to evidence-based but value-laden advocacy. Second, conditional objectivism removes the confusion between taking a certain viewpoint to make recommendations and endorsement of the viewpoint. Third, if scientists were regularly clarifying by means of conditional statements that they do not necessarily endorse a viewpoint, the bias in representation of liberal-progressivist academics at universities would be alleviated. Fourth, conditional objectivism offers a solution to the problem of stigma by making clear that the researcher takes a conditional and context-specific perspective rather than an absolutist and universalizing standpoint. Finally, conditional objectivism may alleviate grief by those who are subject to stigma (Gray, 2002). It makes a difference whether a recommendation is made in absolute terms or embedded in conditional statements because the victim of stigma does not have to assume that the scientist shares the prejudice leading to stigma.

Some significant problems with practical recommendations derived from scientific evidence affect conditional objectivism, among them, motivated testing, the interpretation of data, including and weighting values, undesired side effects, feeling-based value judgments, and relativism. Some of these problems seem almost intractable, like side effects that are often not only undesired but also concealed. For other problems, like motivated testing, data interpretation, and feeling-based value judgments, the context-specific nature of problems in real-word decision-making has to be elaborated, and best practices need to be developed in the future.

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Chapter 6
Towards Reflexivity in the Sciences: Anthropological Reflections on Science and Society

Anna Zadrożna

The chapters on which my contribution to this volume draws reflect on some of the most important questions and dilemmas in social sciences and still manage to complement each other in some unique ways. They address the issues related to the very foundations, methods, and implications of science and speak for the reflexive approach towards scientific inquiry and the relationship between science and society. Reflexivity, which involves researchers developing an awareness of their own disciplines' strengths and limitations, as well as potential implications of academic endeavor per se, plays crucial roles in developing critical thinking and proper understanding of scholarly expertise in society (Strand, 2019, p. 9). However, whereas in some disciplines such as anthropology and sociology, reflexivity is an integral part of academic practice, in others it has been absent (Strand, 2019), and some scientists perceived philosophy of science as disruptive and limiting.[1]

In this exploratory essay, I will try to identify and discuss the main questions that emerge from these four chapters and then take them as an inspiration and point of departure in order to reflect on the role of reflexivity in sciences, the relationship between society and sciences, and the non-dualistic approach to scientific inquiry. In anthropology, the reflexive turn was a long-term consequence of the crisis of the late 1960s, when the discipline received harsh criticism for its contribution to colonialism. Founded during the Western expansion to the non-Western worlds, anthropology originated as the study of others and had been conducted in a context of unequal economic and political relations that were enabled by due to exploitation of natives and abuses of power (Lewis, 1973). The reflective turn in anthropology

[1] Source: https://web.stanford.edu/class/symsys130/Philosophy%20of%20science.pdf ("Philosophy of Science: Part of a Series on Science", educational materials published online by Stanford University, p. 1–14).

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became a “paradigm shift” from a “scientific” to a hermeneutic or interpretative approach (Salzman, 2002). Reinvention of anthropological practice during the reflexive turn by no means ended the debate on anthropological theories and methods, but rather opened the space for different voices, ideas, and constrains. Among the ongoing debates within anthropology are those concerning research methods, including the notion of the “field” (Amelina, Nergiz, Faist, & Glick-Schiller, 2012; Amit, 2000; Gupta & Ferguson, 1992; Kokot, 2006; Okely, 2012), ethical aspects of ethnographic practice (e.g., Campbell, 2010), and even reflexivity itself which received a critical reevaluation (Salzman, 2002).

Today, the notion of reflexivity expands to concern for the positionality of a scholar both as a researcher and as a writer (Hastrup, 1992), as well as the awareness of the socio-political context and institutional environment in which one is situated. Positionality, in this regard, refers not only to a researcher’s relation to the social and political context of the study (Coghlan & Brydon-Miller, 2014) but also to the impact that one’s race, gender, sexuality, class, and place of origin (Kempny, 2012) have on the research process, including the very initial stage of formulating a research question. Being reflexive means “constant awareness, assessment, and reassessment” of the researcher and the impact of the research on its “subjects” (Salzman, 2002: 806) and attentiveness towards relationships of power: within a society, towards research projects, and between the researcher and his or her research “subjects.” Hence, I should from the get-go situate myself as a scholar in terms of my own background and training before I proceed. I settled on anthropology for my doctoral studies after receiving graduate training in applied natural sciences, upon completing a master’s degree in Poland with a thesis on social aspects of wolf (*Canis lupus)* protection. During my undergraduate studies in ethnology and anthropology as well as my doctoral studies, I have conducted field research in Macedonia, Italy, Poland, and Turkey, where I have also lived for 8 years. Furthermore, and to differing degrees, I worked and received academic training in these four countries as well as in the UK, Austria, and Norway where I am obtaining my PhD degree. To my knowledge, I am the only junior and female scholar contributing to this volume.

Between Impact and “Grimpact”: Practical Recommendations to Avoidance of Harm

The relationship between society and social sciences is complex and multidirectional, and social sciences remain grounded in social worlds in various ways. Two contributions in this volume explore the intrinsic relationship between society and sciences: in Chap. 2, David Carre (2019) problematizes manifold ways in which society and science interact with each other, while in Chap. 5, Reber and Bullot (2019) explore complexities and pitfalls of the process of making recommendations based on empirical evidence.

Drawing on the notion of guidance (Valsiner, 2012 in Carre, 2019), which suggests that scientific activity develops in different, sometimes opposite directions while trying to address societal needs, Carre addresses one of the most disputed and divisive issues among scientists, examining of what kind of knowledge social sciences (should) give back to societies supporting their work. The classic division between applied and "basic" social research is, as Carre puts it, only a fraction of a broader conversation about the knowledge created by contemporary social sciences. This question, as he observes, often leads to polarization alongside three opposite pairs: return of investment vs. value in itself, applied vs. basic social research, and citizen vs. academic relevance. Carre notices that different subdisciplines and research areas within social sciences may point towards very different directions regarding their purposes. On the one hand, scientists may wish to address "real-world issues" and create innovative solutions (p. 9). On the other hand, they might become detached from society and create knowledge for the sake of publishing, in order to comply with the "publish or perish" trend in social science. He proposes a common ground among those different approaches and suggests that such positions should be complementary rather than mutually exclusive. He further attempts to bring attention to the existence of manifold ways in which the relationship between society and science unfolds.

One of the ways in which society can influence science, Carre argues, is through administrative organizations that tend to promote certain categories and purposes of research, and frameworks created by national and pan-national programs that fund scientific research. Carre warns that such a trend enforces scholars to adjust their scientific work to the topics, methodologies, and formats of the publishers (p. 5) and consequently increases the risk of diminishing the scientific and intellectual depth and quality of knowledge. On that note, I find it important to address the growing concerns over precarity in academia and the ways in which it affects academic knowledge (Gallas, 2018; Ivancheva, 2015; Pérez & Montoya, 2018). Neoliberalization and projectification of academia affects scholars already at early stage of their career, facilitates inequalities within academia, and pushes academics towards activism and political involvement (Herschberg, Benschop, & van Brink, 2018; Ivancheva, 2015). At the same time, precarity is normalized through the meritocratic imaginary of academia, which, however, does not necessarily reflect the reality (Gallas, 2018). Increasing instability in academic jobs, work overload, and economic dependence on short-term external grants creates hierarchies of knowledge production (Pérez & Montoya, 2018) and makes academic knowledge susceptible to market pressures (Ivancheva, 2015).

What strikes me in Carre's contribution is his sober awareness of the institutional and economic realities that greatly affect contemporary sciences. Establishing a common ground between the needs of citizens and standards of academia appears to be a process of constant negotiations over "(ir)relevance" of social sciences research, in which certain voices become the arbiters who determine what is (not) relevant in social sciences. While Carre's contribution does not directly problematize power relationships, it appears from his discussion of relevance and usability of social sciences that science is not only embedded in social context but also in complex power relations that operate within both sciences and societies. Hence, "society" and "science" should not be seen as separate and homogenous entities. Rather, each one delineates diverse and mutually contested voices, aims, and points of view.

Thus, I suggest that the question of "what social science should give to societies" could be further problematized through explicitly recognizing that scientific knowledge does not necessarily serve common societal or scientific purposes and societies are not homogenous potential beneficiaries of science[2]: behind the common imagination of "a society" loom cultural and social diversity and hierarchies of power. Failing to recognize complexities of a social context, including power relations, may create more harm than good, and there are countless historical examples of how science can serve or justify taking advantage of others, "conquering" others' lands, or exploiting their resources. "Others" here refers not only to different "societies" but also minorities within one's society (Abu-Lughod, 1991) and to non-human living beings (Nibert, 2003; Noske, 1993). The most vivid cases of historical wrongdoings of science come from colonialism (Lewis, 1973) or Nazism (Beyerchen, 1992), or from the academics' involvement in secret services (Boas, 2005; Fluehr-Lobban, 2008), but it would be naïve to believe that science today is free from the impact of political and economic powers or to assume that it necessarily serves a "common" good.

Regardless of the scientists' own intentions, views, and awareness, scientists are politically positioned through their institutional affiliations and funding schemes (as Carre also suggests), because the boundaries between scholarship and engagement, or between research and politics, are continuously blurred (Eriksen, 2009: 28). Not only are the side effects of research hard to predict, but implicit optimism about and purposeful aiming at positive impact of research may even strengthen the inability to predict and deal with negative impact of research, leading to what Gemma Derrick and others (Derrick, Faria, Benneworth, Budtz-Petersen, & Sivertsen, 2018) call "grimpact." The authors suggest that if scientists were held accountable for the impact of their research, this could help prevent "grimpact." However, they do not explain how such accountability could work without further politicization of science. Moreover, what first appears as a positive impact might turn out to have negative consequences over time.[3] As an example, Derrick and others refer to the rather controversial research that falsely suggested a causal relationship between vaccinations and autism. Although the research was later discredited for lacking clear evidence and data falsification, and the publisher retracted the article, the author himself declined to replicate his research or to acknowledge its flaws and keeps on influencing anti-vaccination movements (Derrick et al., 2018). It is unclear whether the researcher intentionally allowed his a priori beliefs to shape his research results or he developed his beliefs after conducting the research. But even innovations with genuinely and purely scientific intentions can have unintended consequences, such as pesticides increasing food production but severely harming bees. As I further suggest in line with other contributions to this volume, such unintended consequences could be more often prevented if scientists become open towards holistic

[2] Such idea of "social wholes" has been overdetermined in social science, and there is a risk that rhetorical wholes will be taken for social entities, which they are not (Thornton, 1988).

[3] For example, some medicines are withdrawn from the market because they caused risk to patients.

perspective (Watzl, 2019) and acknowledge different points of view and values that guide their research (Reber & Bullot, 2019).

The main insight from anthropology may be an invitation to modify or at least precede the question "what knowledge science should produce or give back," with "which harm science should avoid producing or giving back." Because of its colonial past, a lot of attention in contemporary anthropology is devoted to discussing the negative effects of research. Anthropologists should avoid creating suffering and harm (Fluehr-Lobban, 2013), which means that researchers should be aware of potential impact research findings might have, if applied.[4] Advocacy, whenever it is discussed, is, at a minimum, "an ethical position to try to protect and better the lives of the individuals we work with" (Mullings, Heller, Liebow, & Goodman, 2013). Finding a balance between creating no harm and ethical behavior requires deep knowledge about the socio-political context in which the research takes place and recognizing what ethical and harmful behaviors should be (Abu-Lughod, 2002). What I refer to here is neither moral relativism nor its opposite, normative determinism or absolutism. My aim is to emphasize that ethical standards and values can be subject to disagreement or debate within science in different sociocultural contexts and among various fields, which may be confusing especially when the researcher lacks experience.[5]

One of the ways in which anthropologists address these difficulties is through researchers' immersion in their fields, which can ideally transform research into participatory, embodied experiences through which researchers can embrace different ontologies as ethnographers before they further distance themselves from their starting points as writers (Hastrup, 1995; Okely, 2012).

However, immersion in the field (e.g., Okely & Callaway, 1992) or collaboration (Rappaport, 2008) may facilitate understanding of a sociocultural context to the extent in which advocacy becomes inseparable from research (Mullings et al., 2013). What turns out as problematic is whether and to what extent ethnographers can "represent" the problems and issues of the people they worked with (Abu-Lughod, 1991; Fabian, 1990; Marcus & Fischer, 1986) and to what extent they should embrace their values. Such questions are often addressed within universalism vs. relativism debate, and anthropologists take different stands (similar to other scientists, as David Carre observed) that range from advocacy for universal human rights (e.g., Fluehr-Lobban, 1995) and values to, although much less frequently, cultural relativism (see Brown, 2008). The practice of reflexivity helps recognizing own intellectual and moral inclinations, feelings, assumptions and actions, and provides the reader with information necessary to assesing our work (Salzman, 2002).

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[4] Some anthropologists decide to postpone publications (e.g. Verdery, 2012).

[5] The Guidelines for Research Ethics in the Social Sciences, Law, and the Humanities published by the National Committees for Research Ethics in Norway advises reaching out to a broader research community which shall help to clarify which ethical standards apply and what is or is not ethical (NESH, 2006, p. 6). Assessing potential harm, however, is more complex, because it is based on prediction and requires, again, a deep knowledge of the socio-political context.

Sciences, "Societies," and Points of View

Reber and Bullot argue in their contribution to this volume (Chap. 5), that making practical recommendations derived from empirical evidence and scientific research is highly problematic because scholars often prioritize their favored values and make recommendations from certain points of view. For example, they may intuitively position their research in favor of protected values, such as respect for human life, or the respect of one's bodily and psychological integrity, or, as it was in case of physical anthropology and its engagement with eugenics (Kyllingstad, 2017), their research might be embedded in the dominant intellectual trends and policies in a particular historical context. In certain departments, particular political views might prevail, and postulates for achieving political diversity are problematic because it would require political screening.[6] Reber and Bullot argue that the complex linkage between practical recommendations and empirical evidence can be facilitated by heuristics and propose to apply the strategy of conditional objectivism, which argues that researchers should acknowledge value plurality, consider the relevant standpoints in a debate before making practical recommendations, and reason based on counterfactual conditional statements (p. 16). The scholars should also be aware of the difference between taking a viewpoint to make recommendations and endorsement of the viewpoint (p. 17). As Reber and Bullot put it:

> conditional objectivism does not make the contention that a neutral viewpoint exists or is possible to adopt (but rather) posits that cognitive and social scientists can distance themselves from a viewpoint by using *contrastive reasoning* based on comparing conditional statements. (p. 7).

They argue that this by no means equals relativism, or suppressing feeling-based moral judgments, but rather it implies increasing the awareness of the values and political opinions that stand behind certain viewpoints in science, which might even be intuitive or distinctive of the cultural context. Science may reproduce ontologically rooted practices and beliefs, as Reber and Bullot demonstrate it in their discussion of the debate over the well-being of children with Down syndrome, and incentives for or against abortion of such child. Their focus is on how the different viewpoints that scientists hold may affect the recommendations they make. But if one looks at science within its sociocultural context, the very act of establishing norms and categories such as disability and health can be seen as cognitive constructs and, as such, open to redefinition and renegotiation.[7]

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[6] As an example, Reber and Bullot (2019) refer to social sciences, where faculty members tend to be left-oriented and liberal. However, it might be relevant to the Western context, which is the subject of Reber and Bullot's article, but not necessarily to everywhere else in the world.

[7] There have been significant differences between sociocultural attitudes towards children born with "disabilities." As an example, whereas the early Christian Church associated the birth of an "intellectually disabled" child with "sin," the Olmec of ancient Mexico have seen such children as gifted and having religious and superhuman significance (Gaad, 2004).

These arguments reminisce feminist scholars and philosophers of science who identify the sciences as both a source and a locus of gender inequalities, and bring attention to the relationships among science, gender, race, class, sexuality, disability, and colonialism as constructed within and applied by science (Crasnow, Wylie, Bauchspies, & Potter, 2018). Consequently, they question the conventional understanding of science as objective and free of non-epistemic values and opt for conducting research as reflexive and inclusive, preferably interdisciplinary and case-study based (Richardson, 2010). Science has also received criticism for its culturally "western" bias, as being "produced in western nations, by western authors, for western audiences" (Young, 2014: 29 also Abu-Lughod, 2002; Streeby, 2018). Such bias may result in ill-conceived policy practices, as it has been in case of, for instance, initiatives undertaken to empower women in India, where research applied to study women was based on gender categories and theoretical understandings of power relations produced in the West (Jakimow & Kilby, 2006).

Returning to Reber and Bullot, I think that implementing conditional objectivism in both social and natural sciences would greatly benefit them by facilitating transparency and limiting abuses of power in sciences. Conditional statements would require the researchers to include the diverse voices that exist within societies and necessitate that they reflect on their own presumptions and beliefs. Nevertheless, such practice might not be welcomed by those beneficiaries of science who aim at achieving concrete results or at implementing certain political agendas and legitimizing them through science. Moreover, it is not clear how to implement conditional objectivism in interdisciplinary contexts, when different disciplines may aim at achieving mutually contested and at times exclusive goals, or if their practitioners are unwilling to compromise with each other or engage in conditional objectivism on their own. My studies in environmental protection were interdisciplinary, and I experienced that scholars from different disciplines were not eager to reformulate their way of reasoning towards different goals. For example, experts in animal production who aim at increasing production of meat while minimizing costs were oblivious to the need to address the negative impact meat production has on environment. Another issue worth addressing here is that scientists do not only work academia, and although they do not necessarily represent "science" (if understood narrowly within the bounds of academic institutions), they are trained to practice "science," however with different agendas. For example, whereas environmental scientists work towards finding solutions to the climate crisis or to the growing plastic pollution, the focus of scientists employed at food industry is to increase production, and environmental concerns remain often marginal.

The ways in which sciences are organized into humanities and natural sciences and then further into separate disciplines reflect ontological drives behind the Cartesian divide that imputes an opposition between nature and culture (Haila, 2000), and the very need to systematize and categorize. Social sciences have already received criticism for being profoundly anthropocentric (Nibert, 2003; Noske, 1993) and narrowly defined to study human society as if it were separated and independent from or dominant over other species, ecosystem, and environment.

Most attempts at including particular animals or species into scientific inquiry within social sciences tend to be reductionist and consider animals as passive subjects of study (Noske, 1993). Such *speciesism* has been compared to racism and sexism (Nibert, 2003) because it establishes hierarchies between different species, positioning humans on the top. Establishing hierarchies between different species (or "races") can justify practices which otherwise would be condemned as unethical. For example, positioning animals as not having consciousness and as being less sophisticated than humans justifies practices such as testing drugs and cosmetics on animals or killing animals for recreation, meat, and fur. The ontological claims, based on which such practices are endorsed in Western sociocultural contexts, are strikingly different, for example, from "deep ecology", Indigenous science (Streeby, 2018) or the view of animals in Rajasthan in India.[8] But non-reductionist studies of animals and of animal-human relations remain at the peripheries of social sciences. John Law and Marianne Elizabeth Linen maintain that certain practices, including science, create categories and divisive textures that make certain things or beings as passive or active, with or without agency (Law & Lien, 2012).[9] As they argue, it is time to give attention to the textures on the margins and to ontologies as enacted in practice.

Attempts at including animals, plants, and other species into societies may bring up a redefinition of the notion of society itself, and may reposition the question over the proper contribution of scientific knowledge to society. If scientists redefine societies beyond speciesism, should they also consider the potential risks and benefits that their research may create on all species, or even the whole ecosystems? If we agree that science should equally contribute to all parts of the non-anthropocentric, planetary society, what would happen to scientific endeavors and practices that lead to exploitation of non-human others and of natural resources?

These are pressing questions, which yet lack straightforward, satisfactory answers. However, one should highlight the different ontological claims and practices inscribed into the ways we conceptualize society and science, as well as the inequalities, hierarchies of power, and competing interests that often stand behind science or lay at the very core of scientific research. Parallel to focusing on the mutual relationships between science and society, I would suggest conducting a more specific inquiry about potential beneficiaries of academic knowledge and, consequently, power hierarchies involved in its production. Consequently, assessing potential (gr)impact of research, one should reflect far beyond the boundaries of one's own discipline (as suggested by Strand, 2019) and consider one's own positionality, the broader contexts in which science and particular institutions are embedded, and hierarchies of power that exist within science and society, which may stand behind or show interest in particular research outcomes. One should also remain ethical, after a careful consideration of what "ethical" really means.

[8] In Rajasthan, not killing any animals is among the main sociocultural principles.

[9] In their research on salmon farming, Law and Lien explore how salmon is made through different practices oriented towards producing a healthy salmon, juxtaposed to a "nearly salmon" which is otherized and killed in consequence of different modalities of practice.

Towards a Non-dualistic Perspective in Sciences

Contemplating on the reasons why academia needs philosophy of science, Roger Strand posits that the most compelling one is "to understand what the various sciences can, and cannot, deliver, and to understand how, and why this is so." In other words, scientists should not only develop reflexivity and humility on behalf of their own disciplines but also understand theoretical assumptions, limits, and advantages of other disciplines (Strand, 2019, p. 6–7). I find this postulate extremely important, because it allows us to look at studied phenomena as complex while at the same time consider our cognitive skills and abilities as both very particular and limited. "Cognitive specialization," as Strand names it, is essential to the practice of science, and scholars should be aware of their conditioning to "develop their cognitive (and emotional) resources to deal with it in a reflexive manner" (Strand, 2019, p. 9).

Awareness of disciplinary frameworks that have shaped one's knowledge and peculiarities of language used within each disciplinary tradition[10] matters as much, in my opinion, as reflexivity on one's own political, social, and institutional positionality, including gender, cultural background, and beliefs. Understanding peculiarities of various disciplines is also crucial when establishing what is (not) science, that is, what is factually correct, especially in the "post-truth" era, when fake news and pseudo-scientific theories are widespread through the Internet and social media. The debate over whether certain practices are "scientific" exists within every academic discipline and may evoke the discussion over the very nature of science.

Within anthropology, such debate has taken unusual shape when in 2010 the executive board of the American Anthropological Association removed "science" from their mission statement, which triggered a heated debate among anthropologists over definitions of science. The forum discussion published in *American Anthropologist* (Peregrine, Moses, Goodman, Lamphere, & Peacock, 2012) addressed issues such as false dichotomy between humanities and sciences, the relationship between science and anthropology, and anthropological turn in science and concluded with an affirmation that "anthropology is a discipline that embraces multiple perspectives, multiple methods, and multiple ways of understanding humans and human behavior" (Peregrine et al., 2012: 597). Distancing from science, as they argued, may derive from the fact that "too often "science" is far too narrowly reduced to "confirmatory hypothesis testing" (e.g. through questionnaires and surveys); both by "pro-science" proponents and "anti-science" opponents."[11] Instead of

[10] Within social sciences, many terms have proliferated beyond their original usage and their understanding changes across time and sociocultural contexts, not to mention differences between disciplinary practices and traditions. The example can be debates over terms such as "identity," "memory," or "diaspora" and different disciplinary approaches to these terms within social science. Most readers have witnessed at least one conference debate when the discussion evolved over different conceptualizations of specific concepts, and misunderstandings resulted from taking particular terms for granted.

[11] Source: http://cognitionandculture.net/blog/benson-salers-blog/anthropology-is-not-a-science-says-the-aaa/ (Accessed on May 15th, 2019). The quotation comes from an open letter of Professor Eric C. Thompson of the National University of Singapore to AAA.

discrediting or abandoning "science", anthropologists should push for a more expansive rather than reductionist understanding of what we mean by science.[12]

In his contribution to this volume, Sebastian Watzl (2019; Chap. 4 in this book) postulates a non-dualistic approach in sciences. He observes a tendency, in public discussions as well as within academia, to give more credibility to explanatory claims presented as "biological facts", even if they are based on reasoning made with regard to socially constructed norms[13] (Watzl, 2019). He points out that psychological essentialism, that is, a tendency to form an essentialist picture of humankind, leads to ascribing more value to what is seen as "essences" and which is associated with biology. Such tendency characterizes a positivist definition of science, which gives more credit to results obtained though large-scale quantitative research or verification/falsification of data through observation, than to interpretative practices of social sciences. Watzl deconstructs such belief by engaging with the nature-culture debate and proposes that "what is often called 'biology' is a myth: a myth created by an intuitive tendency that grotesquely distorts real biological research" (p. 22). He argues that "biology" has never been free from "culture" and should not be studied as separated and independent from it and from its context. Instead of applying a dualistic perspective in science, he suggests discussing the complex causal explanations of studied phenomena.

While Watzl focuses on the role of "biology" and "culture" in relation to humans, his argument of non-dualistic perspective in science is valid well beyond the study of humans as such and reminds me of current debate on climate change and Anthropocene, which is characterized by postulates for holistic approach. It is almost striking that social science used to have so little to say about "nature" and environmental conditions that have made certain social practices possible (Chakrabarty, 2009)[14] and about the impact humans have on their environment. The common view of social sciences reduces it to translating the knowledge produced by the natural sciences and communicating it to audience. Nevertheless, the role of social sciences in understanding the world goes beyond translation and includes empirical contribution and critical examination of phenomenon. The current debate over the climate change and the Anthropocene is one of such topics that require a look beneath theoretical and disciplinary positioning. In studies on Anthropocene, social sciences contribute, for example, by helping to understand that the ways in which the Earth is conceptualized into models allows political and epistemic claims, in which complex ecosystems are turned into something that can be managed and governed. Predictions made on models often fail, because the models do not represent reality; some quantities are better understood than others, and there are a lot of

[12] Ibid.

[13] During his lecture in Oslo in December 2017, Watzl critically examined the idea of "brain gender," which was popularized after publicizing brain scans that suggested differences between male and female brains. However, as he demonstrated, behind categories applied when designing research and interpreting such scans stood presumptions on gender roles and norms.

[14] Chakrabarty explores the link between exploitation of fossil fuels and freedoms that were made possible through capitalism.

rhetorical work and political assumptions and claims at the very foundation of such models (Mathews, 2017; Steffen et al., 2011).

Social sciences can facilitate critical examination of epistemological and ontological assumptions behind modeling practices, and sensibility to natural sciences as practice of storytelling can enrich scientific analysis (Haraway, 2015). Human activity and subjectivity have always been present in natural sciences, but the genre of science has been created as if humans were autonomous and exclusive agents in the complex world and as if science was distanced and objective (Mathews, 2017). Bruno Latour observes hesitation in science about how to narrate complex phenomena and suggests that natural sciences can greatly benefit, for instance, from feminist scholars who have developed approaches and tools to study transformation through relations. Relationality of phenomena such as change (Steffen et al., 2011) is what the great majority of models fail to capture (Latour, 2014). Furthermore, scientists may better understand processes that happen on the Earth if they agree to look at the Earth as an agent and actor (Latour, 2014), or if they include other imaginations of the world and science (Streeby, 2018).

However, are scientists from all disciplines ready for such a level of reflexivity that would have them critically examine the very foundations of their own practices? Would looking at sciences as social construction, one of the possible ways of knowing shaped by beliefs, values, and other existing practices of knowing (de Gialdino, 2009), improve the quality of their practice? If so, how can scientists combine holistic approach with rather reductionist research methodologies (Bergandi & Blandin, 1998)? I posit that stepping out of ontological and disciplinary comfort zones would greatly benefit the practice of sciences. It would encourage interdisciplinary dialogue and make scholars aware of each other's disciplinary limitations and potentials (Strand, 2019). However, this should by no means imply that (natural) scientists abandon their rigorous methods of testing and norms of credibility. I would rather advocate, in line with other contributions to this volume, for awareness that behind sciences are humans, who agree upon axioms and establish practices and norms. Expanding the scope of analysis to take into account hitherto neglected cognitive conditioning as well as acknowledging potential limitations of our disciplines can strengthen the credibility of science. At the same time, cognitive specialization should not prevent scholars from capturing complexity of a studied phenomenon.

Manifold and Uneven Paths Towards Reflexivity

As Roger Strand suggests in Chap. 3, there is something special and unique about the ways in which science is practiced in different parts of the world, as he focuses on Norway and the self-reflective potential of Norwegian academia. In Norway, *vitenskapsteori* is a requirement for obtaining a PhD, and the first attempts to bring *vitenskapsteori* as mandatory into curriculum, together with ethics, date back to 1975, when a consensus report on *vitenskapsteori* was prepared during the national conference in Jeløa. While Strand explores the reasons why *vitenskapsteori* matters for science, and even argues that it can serve democracy, he does not discuss historical contexts of its presence and importance in Norway. Remembering that the origins of reflexive turn in anthropology originated from the critical evaluation of its colonial, 'dark' past, I wondered what happened in Norwegian science before 1975, which brought such a debate and consequently consensus about *vitenskapsteori*.

Although Norway might not be considered as a colonial power in a classic understanding of the term, colonialism is essential in understanding of the formation of Norway through domination over Sami territory and incorporation of Sami into Norwegian society (Greaves, 2018). Colonialism in this context refers to power inequalities and cultural and economic domination over indigenous population and their land, justified by notions of racial superiority (Lehtola, 2015). In Norway, the idea of hierarchy between "races" (which was conceptually intertwined with nationhood) was common in the first half of the twentieth century, when it was linked to eugenics (Kyllingstad, 2017). Although the leading scholars in biology and genetics rejected the ideas of a Nordic master race, the idea of racial segregation was widespread within social and natural sciences (Kyllingstad, 2012), and it has influenced public opinion and politics (Roll-Hanses, 2017: 171). Racial tests, including skull measurements and visual documentation of "racial" features, were performed on Romani and Sami people until 1970s, when they gradually vanished (Kyllingstad, 2017; Roll-Hanses, 2017). In 1970s, Norway had ratified the United Nations' *Convention on the Elimination of All Forms of Racial Discrimination*, and policy of assimilation against minorities was abandoned. The perception or difference and "nationhood" changed from the idea of racial hierarchy towards recognition of "ethnic minorities" (Kyllingstad, 2017) and "ethnic boundaries" (Barth, 1969). Such shift in science and legislation by no means ends the scholarly and public debate on "ethnicity," "race," and minorities. As posited by Thomas Hylland Eriksen, anthropological research and public interventions still tend to reproduce categories of identity politics in the public sphere and lack reflexivity regarding categories such as "Norwegianness," especially when juxtapositioned to "Sami-ness" (Eriksen, 2009, 33-36).

In 2018, the Norwegian Museum of Science and Technology held an exhibition titled "Folk—from racial types to DNA sequences," which was a result of a research project funded by the Research Council of Norway under the similar title. Led by Jon Røyne Kyllingstad, the project (and the exhibition) explored the science of human genetics from 1945 to 2012 and addressed epistemological, historical, ethical, and political questions about genetic science and the role of genetic information in society.[15] Although eugenics seem to belong to "dark past," technological advancements in genetics resulted in using genetics to produce data on populations and in common fascination with topics such as DNA tests performed in order to

[15] The project website "From racial typology to DNA sequencing" can be accessed at https://www.ethnicityandrace.com/

establish one's "ancestry." On the project website, it states that: "in this project genetic data are neither understood as a simple representation of nature nor as a mere product of social and political interests. Instead, we will elucidate how society shapes the production of scientific knowledge in human genetics, and how scientific knowledge influences the social sphere." Although the project may not reflect all stands taken in contemporary science in Norway, it reflects existing openness towards critical debare and readiness to reflect on history, foundations and implications of science. It also serves as a reminder that the paths towards reflexivity and plurality of voices are rarely easy, but rather manifold and unever, and that the journey towards them has no end.

Concluding Remarks

The scope of the four chapters discussed in this contribution goes beyond humanities and social sciences and delves into the very nature of science and scientific research, offering readers inspiring and unique cognitive lenses through which we can examine science . The four contributions on which I have drawn remind us about the most crucial issues for science, such as its socio-political positioning, divisions, disciplinary boundaries and hierarchies within science, or normative presumptions that may stand behind certain practices and views. At the same time, they speak to and complement each other and have certain features in common. All chapters derive from the Western tradition of scientific practice and from secular philosophical thought, and have been authored by male scholars. They are also rooted in Scandinavian academia, in which philosophy of sciences is recognized as essential for scholarly practice.

Philosophy of science invites scholars to reflect upon foundations, methods, and implications of science. I suggested in this essay that anthropology can contribute to this with its long tradition of practicing reflexivity as a part of scientific inquiry. Anthropological reflexivity includes acknowledgment that science is shaped and practiced by humans who are themselves socioculturally and institutionally positioned. Being transparent about one's own positionality, research methods, and processes contributes to the quality of research and helps others comprehend what kind of science we practice, what is the source of our data, and the positions from which we speak as scholars.

I also implied that discussion of the relationship between science and society (Carre, 2019; Reber & Bullot, 2019) would greatly benefit from problematizing and contextualizing both concepts. In problematizing the notion of science, the ideas of scientificity, credibility, and usability are as much important as the question of ethics and responsibility for potential impact of research: its process and further publication of outcomes. In line with contributions to this volume I suggested that although disciplinary boundaries and cognitive specialization (Strand, 2019) are extremely useful and essential to the practice of science, explaining complex phenomenas requires a broader scope of multidisciplinary collaboration

(Watzl, 2019). It may also entail critical rethinking of the world, society, and 'science(s)'. Regarding society, instead of applying rhetoric that reproduces societies as wholes, I proposed to recognize existing diversity with its stratifications and hierarchies of power. Following the presumptions of non-dualistic approach, it may be worthwhile to rethink the common understanding of societies as limited to humans, becasue we cohabit and correlate with other species and the environment. Furthermore, perceiving non-humans, including the Earth, as agents rather than passive subjects of study can enrich our explanations and expand the scope of our understanding of the impact of our research. Finally, the boundaries between society and science are blurred, especially but not exclusively in the case of social sciences.

One of the conclusions that emerge from the chapters is how crucial it is to find a balance between diversity of voices and practices in science while remaining committed to academic standards. Awareness of strengths and limitations of own discipline, of our cognitive conditioning (Strand, 2019), held viewpoints and beliefs (Reber & Bullot, 2019) can positively facilitate the quality of science and lead towards a holistic approach towards studied phenomenon (Watzl, 2019). Philosophy of science can aid such process and facilitate dialogue between different disciplines (Strand, 2019) and worldviews. Being reflexive in science means giving more attention to the principles of academic practice, rather than to the current fashions, political expectations, or administrative frameworks that (try to) shape the ways we do science.

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Part II
Philosophies of Explanation in the Social Sciences

Chapter 7
Explanation: Guidance for Social Scientists

Raino Malnes

What is a nail? *Webster's New Collegiate Dictionary* says it is a slender and usually pointed and headed fastener. Explanations like this, which answer a what - question, are *constitutive*. Here is another illustration: an institution is "a public system of rules which defines offices and positions" and "specify certain forms of action as permissible, others as forbidden" (Rawls, 1999:47–48). The intention in both cases is telling what something is like, spelling out the nature of a certain phenomenon, laying bare its constitution.

Other explanations answer a why - question. Why, for example, is this nail sunken into a piece of wood? The answer may well be that it was pounded with a hammer. For a more exiting example, take Gordon A. Craig's (1982:22) explanation of the surge of respect for political authority in German states towards the end of the seventeenth century. It came about, he says, because survivors of the Thirty Years' War were "willing to submit to any authority that seemed strong enough to prevent the recurrence of those terrors."[1] This is *etiological* explanation. It tells how something came to be as it is. "Etiological" derives from the Greek αἰτιολογία, which

[1] For a case of confusion, see Alexander Wendt (1998:105), who adduces two questions: (i) "How was it possible for Stalin, a single individual, to exercise so much power over the Soviet people?" (ii) "How is it possible for a gas to have temperature?" Both questions are, he says, about "what it is that instantiates some phenomenon, not why that phenomenon comes about" (ibid.:105). But (i) is not. Asking how it was possible for Stalin to exercise so much power is tantamount to wondering why he became powerful. The explanation lies in the course of events that lead to this state of affairs. Suppose, contrary to fact, that Stalin regularly got his will because he wanted people to do what they were intent on doing anyway. If so, was he powerful or not? This question, unlike (i), calls for a constitutive explanation of power – an account of what it is to be powerful, in particular, whether being powerful is conditional upon the need to overcome opposition.

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means “giving a reason for.” But αἰτία also means “cause,” and I would like to suppress the latter meaning, because causal explanation is best seen as a subspecies of etiological explanation.

What follows is an exercise in constitutive explanation. It is an attempt to explain what etiological explanation is and, more specifically, what causal explanation amounts to when it is properly done.

Statistical Explanation

Some poetry first. In Samuel Taylor Coleridge’s *The Rime of the Ancient Mariner*, a sailor relates what happened on a ship that set out for a faraway destination. Caught by a storm, it ended up in Antarctic waters, where it fastened in the ice. The situation became critical.

> The ice was here, the ice was there,
> The ice was all around:
> It cracked and growled, and roared and howled,
> Like noises in a sound!

Then an albatross appeared and landed on the ship, and soon the weather shifted, the ice receded, and the journey resumed. The albatross accompanied the ship, but not for long.

> With my cross-bow
> I shot the albatross

So says the sailor. Why did he do that? Why kill the bird that had come to the rescue of the ship? “The only answer,” writes Barbara Everett (2003:10), “is that mariners did: these heroic New World discoverers killed and culled everywhere they went.” The shooting of the albatross is, allegedly, a case of the kind of capricious violence that people of a certain category were wont to engage in. The explanation Everett offers may be spelled out so that it conforms to a general formula:

(a) Something, Y, took place on a certain occasion. (In the case at hand, Y is the killing of the albatross for no apparent reason.)
(b) When Y took place, certain circumstances, X, obtained. (X is the fact that the killer was a New World discoverer.)
(c) Events like Y tend to go together with circumstances like X. (Killing for no apparent reason is common among New World discoverers.)
(d) The correlation between X-type circumstances and Y-type events is, to the best of our knowledge, non-spurious (that is to say, there is no known circumstance, Z – say, something about the nationality of New World discoverers – such that the correlation between X and Y comes and goes depending on whether or not Z obtains).
(e) By virtue of (c) and (d), (b) explains (a).

Everything hinges on (c), which states that a correlation exists, and (d), which underwrites the robustness of the correlation. Unless these propositions have something to say for them, the explanation has nothing to say for it. It flies only if the statistics are right, which is why we may call it statistical explanation. Is it an adequate formula for etiological explanation?

Causal Explanation

According to another formula of etiological explanation, one explains why Y by invoking a connection between Y and something else, X, which is such that the occurrence of Y is ensured by the occurrence of X. We may call such connection an *ensuring*. (I borrow the term from Armstrong, 2012:39). One kind of ensuring comes readily to mind, viz., *causation*.

Causation is commonly conceived as a relationship of making happen and being made to happen – a productive relationship. Let Y be the nail that sits in a piece of wood and X the pounding. A causal explanation of Y is a claim to the effect that X made Y happen, that is, the pounding made the nail retreat into the wood. A cause ensures an effect by way of producing it.

Some hold that statistical explanation, too, is a formula for causal explanation. Indeed, some argue that it is the only usable formula for etiological explanation. Whenever we talk about causation and invoke a causal explanation of something, all we have come upon, and all we are entitled to talk about, is the existence of regularity. David Hume is credited with pioneering this line of argument. For the sake of clarity, however, I shall hold on to the simple distinction between statistical explanation and causal explanation.

There are two modes of causal explanation. First, one *discerns* a causal connection if one finds out that something brings about something else. Second, one *articulates* a causal connection if one discerns it and, on top of that, offers an account of how cause and effect (purportedly) hang together.[2] Compare:

(a) The shortening of the day caused the leaves of the tree to change from green to yellow.
(b) The reabsorption of chlorophyll by the boughs and trunks of the tree caused its leaves to change from green to yellow.

The first is an example of discernment and the second articulation. Some argue that an explanation is in good order only if it articulates a causal connection. Nicholas Rescher (1970:14) says: "one needs to know not only *that* things are connected in a certain way, but *how* these connections function: one need some understanding of the *modus operandi* at issue." According to Alan Ryan (1970:14), "a causal account of human behavior must seek to fill in the details of the sequences between cause and effect, i.e. to offer us an account of the mechanism through which the causal sequence operates." And Jon Elster (2015:14) denies that an assertion that just "cites" a cause is explanatory and says that the "causal mechanism must also be provided, or at least suggested." The thesis is, in brief, that there is no proper explanation without articulation. Is this thesis valid?

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[2] I adopt the terms "discernment" and "articulation" of causation from Newlands (2010:472).

Criteria of Explanatory Success

In order to answer this question as well as the question that concluded the next to last section, we need a standard of explanatory success. What does it take to come up with an adequate answer to a why question? I shall suggest two criteria.

When we ask why something is thus or so, we want to understand how it came to be so. Explanation is successful only if it helps with this. It should contribute to rendering the gestation of whatever we wonder about intelligible, preferably taking us all to the way to the experience of "getting it."

Some give pride of place to this criterion, leaving the impression that it is the only pertinent standard of success. Thus, in Don Herzog's view, "we explain such events by setting them in a narrative frame that shows how they were intelligible responses to preceding events and the local context" (Herzog, 1989:20). In the same vein, Charles Taylor (1971:17) says that the "norm of explanation … is one which 'make sense' of the behavior, which shows a coherence of meaning." And James Farr (1985:1092) likens "understand[ing] and explain[ing] human action" to "forg[ing] intelligibility out of confusing patterns of human interaction." What they all fail to appreciate is the explanation succeeds only if it respects the truth. In David-Hillel Ruben's (1992:210) somewhat cumbrous formulation, "[e]xplanations work, when they do, only in virtue of underlying determinative or dependency structural relations in the world." Elster (2007:24) says that:

> Causal explanation must be distinguished from *storytelling*. A genuine explanation accounts for what happened, as it happened. To tell a story is to account for what happened as it *might* have happened (and perhaps did happen). … Why would anyone want to come up with a purely conjectural account of an event?

But the contrast between pure conjecture and telling it like it is simplifies things. These are extremes and between them is the immensely important category of *justified* conjecture, i.e., an assertion that is conjectural, but credible – a well-grounded and promising candidate for truth. Typically, the best we can do when we try to find out whether things are thus or so is to come up with something in between a story and truth revealed, in other words a justified and credible conjecture.

This calls for a brief digression on justification. It is a question of going after truth by apt means, prominent among which are the senses and the faculty of reasoning. First, empirical evidence, in particular perceptual evidence obtained by one of the five senses, can serve as a direct pointer to truth. Say one sees and hears that the dog is barking at the terrace door. This is direct evidence for a belief about what the dog is doing. One learns something right away from perceptual experience. One can also learn things from introspection, which is a source of empirical evidence about one's own inner life. I shall come back to that below. Second, one may reason one's way from one belief to another – for example, from the belief that the dog is barking at the terrace door to the belief that there is an intruder in the garden. No intruder has been observed and there is no direct evidence for the latter belief, but it has an inferential justification: it is the best explanation of what one observes. Let *epistemic consideration* be a common denominator for evidence and reasoning. Justified conjecture distinguishes itself from storytelling by being backed up by epistemic considerations.

The digression allows for a precise formulation of the second criterion of explanatory success. Ultimately, what matters is getting at truth, but in practice, what we care about is whether an explanatory account rests on epistemic considerations and thereby constitutes a promising candidate for truth. Careful justification does not guarantee success, but it is our best bet.

On the one hand, an explanatory account that has no justification to show for it – no evidence or reasoning that renders it credible – is wasted. But justification does not suffice for explanatory success. Asking why something came to be the case is asking for more than enlightenment about the circumstances of the happening. One is after enlightenment of a particular kind – the one that does away with mystery and ushers in the experience of "getting it." Hence, only justification together with understanding will do.

An Argument for the Inadequacy of Statistical Explanation

Recall Coleridge's poem and suppose we are presented with the first known instance of purposeless killing committed by a New World explorer. Then it clearly will not do to answer the question "why did he do it?" by "because he is a New World explorer." Do things stand differently after the observation of many like instances? Well, if the first instance is inexplicable, so presumably is the next, and the one after that, and every subsequent one. Mystery does not wither upon repetition; it rather multiplies. This, it seems to me, is a forceful argument.

Placing something that has mystery about in the context of other, equally mysterious phenomena makes us no wiser. To be sure, we realize that the puzzle is not a one-off incidence. There are others like it and we had better become used to things like that. But being used to something is not tantamount to understanding it. We do not understand why the sailor killed the albatross by being told that he was one of those who were wont to kill and cull. If the grip we can get on a curious phenomenon consists only in subsuming it under a pattern of similar phenomena, it is beyond our ken.

The argument rhymes with the thesis that there is no explanation without articulation. Correlation does not tell how things hang together and this arguably dooms statistical explanation. As we shall subsequently see, however, this is not the last word on the usefulness of statistics for explanatory analysis.

Articulating Causation

David Hume contends that we never "discover any power or necessary connexion" between phenomena (Hume, 1777/1976:63). We observe correlation high and low – nails regularly retreat before a hammer, green leaves regularly turn yellow in the fall, and so on – but we never observe any causal mechanism. Thus, "when we … suppose, that this connexion [between two types of phenomena] depends upon an efficacy or energy … we have really no distinct meaning, and make use only of common words, without any clear and determinate ideas" (Hume, 1740/1985: 162).

Is Hume right? Not if we are to believe John R. Searle, who says: "When, for example, I raise my arm, part of the content of the experience is what makes my arm go up, and when I see a flower, part of the experience is that this experience is caused by the fact that there is a flower there" (Searle, 1983:123). In cases like these, Searle avers that he "directly experience[s] the causal relation, the relation of one thing making something else happen" (ibid.). He often decides to raise an arm and sometimes (at least) he is introspectively aware that the decision brings about the movement of the arm. Not only does he discern causal connections among his states of mind on the basis of introspective evidence; sometimes he is in a positon to articulate how the cause brings about the effect. Some "cases of Intentional causation are special" in that he is "directly aware of the causal nexus," as "there is a 'logical' connection between cause and effect" (Searle, 1983:135).

What, more specifically, is the object of awareness? Let me venture an illustration. Suppose I am about to set sails for the open sea and then notice some clouds in the horizon. I recognize them: they are clouds of a kind that portends bad weather. Realizing that the weather may change for the worse, I pause and break off preparations. What happens has two aspects. There is a normative side to it. I have a belief whose propositional content is (a) clouds portending bad weather are piling up in the horizon. A little later, I form a belief whose content is (b) the weather may change for the worse. Finally, I make a resolution: (c) better break off preparations. The propositional contents of these mental states stand in a normative relation to each other: (a) lends support to (b), which lends support to (c). One who believes (a) has a reason to believe (b), too, and one who believes (b) has a reason to think (c). There is also a psychological side to what goes on: a succession of mental states. Presumably, there is also a physiological side to it: cellular and molecular activity in the brain and the central nervous system. The various aspects of the process presumably run in parallel. What takes place on the normative side matches up with what takes place on the psychological and physiological sides, and the match is hardly coincidental.

In view of this, one may venture the hypothesis that my decision about what to do is not just rationalized by my belief about the weather; it is dependent on this belief and the dependence is likely to be causal. The belief about the risk that the weather may well change – together with some other states of mind, e.g., a desire not to suffer shipwreck – makes me decide to break off preparations. John Cottingham (2002:347–348) says: "it seems hard to resist the suggestion that our rational adoption of certain beliefs, based on rational grounds, must involve certain mental states being not just inferentially, but also causally related to other mental states."Call the causal mechanism I am hinting at *responsiveness to reasons*. Explanation that refers to responsiveness to reasons is a staple of social science. It is the natural resort when we look around for an answer to the question why someone carried out a certain action. When a person, P, performs an action, A, we survey the situation she found herself in for something that may have served as her reason to do what she did. Reasons of action are out there; they figure in a situation before an actor picks them up, as it were.

Explanation that invokes responsiveness to reasons renders action intelligible. We readily understand why people do what they do when there is reason to believe that they act for reasons. One criterion of successful explanation is, accordingly, fulfilled with a vengeance. What about the other criterion, justification? What kind of epistemic consideration bolsters explanation that invokes responsiveness to reasons? The brief answer, suggested by Searle's argument, is introspective evidence.

Now, introspection is private. Only the person who has the experience that one state of mind brings about another has direct empirical evidence of what goes on. Is this to say that introspection of mental causation is usable only for biographical purposes? Not if we are to believe Thomas Hobbes (1651/1973:2), who contends: "whosoever looketh into himself and considereth what he doth when he does think, opine, reason, hope, fear, etc., and upon what grounds; he shall thereby read and know what are the thoughts and passions of all other men upon the like occasions." Hobbes hints at a formula that each of us may employ in order to arrive at causal explanation of what other people do. All of us have direct evidence of our own responsiveness to reasons. We are aware that some of our actions are brought about by our reasons for doing this or that. Suppose you observe another person perform an action and you consider that, given the situation she finds herself in, she has reason to do what she does. If you were in her shoes, you would have done the same for similar reasons. Then it is likely that she performed the action in question for these reasons. Call this *Hobbesian inference*. It is an inference from observation to the best explanation of what one observes.

The inference rests crucially on the premise that there is "similitude of the thoughts and passions of one man, to the thoughts and passions of another" (Hobbes, *op. cit.*). The assumption is, more specifically, that human beings share rational responsiveness to reasons. We are largely alike when it comes to the way we make up our mind about what to do and anyone can arrive at credible hypotheses about other people's motivation by informing himself about their situation. It is not, of course, an indisputable assumption. Ludwig Wittgenstein has strong reservations, suggesting that our overall outer resemblance misleads us into thinking that we are like on the inside, too. "The older I grow the more I realize how terribly difficult it is for people to understand each other, and I think that what misleads one is the fact that they all look so much like each other. If some people looked like elephants and others like cats, or fish … things would look much more like what they really are" (Wittgenstein, 2008:450). The more there is to this, the less useful introspection is when it comes to getting at truth about why other people do what they do.

Causal Explanation Without Articulation

What if someone does something that displays no responsiveness to reasons? What, in particular, if someone performs some blatantly irrational action? Can we tell why she does it? A case in point is action spurred by an attitude that is pleasant but clearly unwarranted and owes its adoption to wishful thinking. Say someone resolves to do nothing about her substantial consumption of alcohol because she manages to avert her eyes from the risk it represents. If she was asked why she does not cut down, she would not offer the explanation that she engages in wishful thinking. This, however, is a suggestive explanation.

Elster (2015) suggests that wishful thinking is structurally reminiscent of responsiveness to reasons. More specifically, wishful thinking mimics a particular way of doing what one has reason to do. According to Elster, what happens when someone forms a wishful belief is akin to what happens when someone chooses optimal means to a given end. In cases "when the belief formation is biased by the agent's desires," we have to do with "forms of motivated belief formation [that] are, in their way, optimizing processes: They maximize the pleasure the agent derives from his beliefs about the world rather than the pleasures he can expect from his encounters with the world" (ibid.:42). This, it seems to me, is to substitute a metaphor for an account of a causal relationship. The most we can say is that, during wishful thinking, the mind operates as if it were engaged in optimization. But the analogy between wishful thinking and deliberate optimization is formal only. There is no substantive kinship.

Now there may well be a positive correlation between the level of pleasure someone would derive from believing so and so and the appeal of this belief to him. Suppose there is and we know it. Then action that bears witness of wishful thinking allows of statistical explanation. Earlier, however, I wrote off statistical explanation as unsuccessful on the ground that it leaves the nature of the causal connection unaccounted for. Now I am about to retract a bit. While the existence of correlation says nothing about the way things hang together, it may be evidence – albeit indirect evidence – that they hang together somehow. In other words, correlation can be the basis for discernment, if not articulation, of causation.

Here is the argument: if events of one kind, E, and events of another kind, E∗, regularly occur together and always in the same order – E before E∗ – then a causal connection probably exists between the two. Take the grief of someone who just lost a person he loves. Rarely if ever is such an incidence unaccompanied by such a reaction. The two kinds of event – loss and grief – fall into a pattern. Most, if not all, cases of loss bring grief in their wake and it strains the imagination to think that we are only witnessing one coincidence after another. According to Galen Strawson (2014:24), "it is reasonable (in some perhaps irreducibly vague but profoundly unshakeable sense), given a regular world, to suppose … that there is definitely something about the nature of the world given which it is regular, something which is …not itself just the fact of its regularity." What Strawson hints at is abductive inference. Based on evidence of correlation between loss and grief, one infers that the former makes the latter happen, because the existence of a productive relationship of dependency best explains why loss rarely if ever occurs without grief.

Recall the ancient mariner and the contention that what he did is no mystery because it falls into a pattern. In view of what was said above, the dismissal of this contention seems unjustified. The best explanation of the pattern is that it reflects the existence of a causal relationship of which the shooting of the albatross is an instant.

Now, the inference from correlation to causation is justified only if nothing indicates that the two correlates owe their joint occurrence to something else. It is requisite, in other words, to rule out spuriousness. Constant conjunction between X and Y is evidence of the causal dependency of Y upon X only if no third factor, Z, causes both X and Y. This opens the door to a protest against what I just said. Arguably, articulation of the causal connection between X and Y is needed in order to rule out spuriousness. Only knowledge of how X makes Y happen will justify the proviso there is no third factor in the picture. Hence, there is, after all, no explanation without articulation, or so the protest goes.

It does not stand up to scrutiny. The problem of spuriousness can be dealt with by way of statistical analysis, i.e., control for the effect of factors that might play the disruptive role of Z. If no culprit is found, one may be confident about the inference from correlation to causation. In principle, of course, control for spuriousness can go on indefinitely. After Z factors that come readily to mind have been cleared of suspicion, one can always think of more farfetched candidates. Some 300 years ago, the best and the brightest gave serious thought to the idea that *all* observable correlations are spurious and that statistical patterns exist only because God is continuously interfering with the world in order to ensure that regularity prevails. Surely, however, we are entitled to an inference from correlation to causal connection before we get to the point where every imaginable source of spuriousness has been put to the test and ruled out of court. To come up with a genuine explanation is no more – and no less – than reasoning thoroughly about a rich set of empirical data.

The Semantic Argument

Another case against inference from correlation to causation waits. Say I observe that (a) the onset of warm weather in April is constantly conjoined with the budding of trees. I infer, as the best explanation of (a), that (b) the warmth causes the buds to come out. No spuriousness is suspected. Now, consider the propositional contents of (a) and (b), respectively. On the face of it, they differ. It seems that (b) adds something to (a). It posits a power "that [is] supposed to enforce" the observed pattern of events (Psillos, 2012:132). Where does the extra content of (b) come from? How do we manage to mean more by "warm weather causes trees to bud" than we mean by "warm weather is constantly conjoined with budding trees"?

Some deny that we manage to do so. Thomas Brown may have been the first to elaborate the argument. He says that the "power of A to produce B … are words we use to express our belief that A will always have B for its invariable consequent" (Brown, 1835:20). The phrase "A produces B" seems richer in content than "A always has B as an invariable consequent," but how can it be? Supposedly, the only basis for saying that A produces B is the observation that, time and again, B occurs after A. Brown claims that no word we employ to describe the relationship between A and B – neither "power" nor "produces" – "express the existence of anything which is not itself either A [or] B" (ibid.). To be sure, we intend to talk about causal power and a productive relationship, but expressive intent – what one wants certain words to signify – is one thing and expressive accomplishment – what one manages to mean by these words – is another thing. The problem is not that one may be mistaken in positing a causal connection on the ground only of observed correlation. It is worse: one does not know what one is talking about. Call this the *semantic argument*.

Some, presumably, react to it with disbelief. Surely, the meaning of the claim that X causes Y is clear. Talking about causation is not toying with a word that lacks determinate content. Among philosophers, however, the semantic argument has a reputation for weightiness. Derek Parfit (1998:26) holds that "ordinary causation is mysterious." At the most fundamental level, we have no idea why some events cause others, and it is hard to explain what causation is. Barry Stroud (2011:21) puts it more guardedly: if "we can make no full sense" of the assertions to the effect that X brings about Y, then the relations they hint at is "problematic, even mysterious." To be sure, innumerable causal verbs are in common use: to break, to bolt, to frighten, to comfort, to become intoxicated, and so on. These words are not used to talk about mere regularity. But the fact that their use is frequent does not guarantee that their meaning is clear.

Anyway, the semantic argument has a lot to say for it. Suppose we have made numerous observations of events of a certain kind, X, that are contiguous with, and prior to, events of a certain type, Y. These observations are held to be indicative of something else – some non-observable relationship between X and Y. However, no information about the nature of the underlying relationship is available. We have a name for it, "causation," but what the name stands for, over and above "the relationship behind constant conjunction," is anyone's guess. If so, how, if at all, do we manage to mean anything by "causation" apart from "constant conjunction"? There is a problem of semantic ascent. Can it be resolved?

Say we are aware of (a) how A brings about B and also (b) that C correlates with D. Conceivably, the inference from (b) to the belief that C causes D can be abetted by appeal to (a). The condition is that the general nature of the causal connection between C and D, whose specifics are unknown, is akin to the nature of the connection between A and B, which is known. If so, semantic ascent can be accomplished by way of analogical reasoning.

I shall wind up the discussion by pouring some cold water on this idea. There are limits to how far it will facilitate explanation of action. Consider, again, the case of someone doing something that seems motivated by wishful thinking. A belief ostensibly serves up to a wish and, most likely, the belief is the effect of the wish and we have the basis for an explanation of the ensuing action, right? Only if we account for the extra meaning that talk about causation adds to talk about correlation in this case. The "steps from its being pleasant to think of P, to its being pleasant to think that P, to thinking that P, cover no great psychological distance," Bernard Williams (2002:83) says. All the same, the distance will have to be traveled by way of some mechanism or other, and unless we have an inkling as to how this happens, we do not manage to mean more by "wishful thinking" than "regular conjunction between its being pleasurable to believe so and so and inclination to form the belief." Perhaps we manage to mean more by drawing on the analogy between wishful thinking and responsiveness to reasons, which we are well acquainted with? This will not do. In whatever way wishful thinking comes about, it does not happen after the fashion of reason responsiveness. As regards the latter, normative relations between the propositional contents of mental states drive a mental process that issues in an action or an attitude. Wishful thinking, however it unfolds, has no normative aspect. Acquaintance with responsiveness to reasons gives no clue to the causal mechanism that connects its being pleasant to think of P, to its being pleasant to think that P, to thinking that P.

Wishful thinking is one among countless deviations from responsiveness to reasons. People are irrational in myriads of manners, some of which, like wishful thinking, display regularity that invites explanation based on statistics and analogical reasoning. The semantic argument threatens all such exlanations.First, there is no way one can become acquainted with irrationality because it always operates behind one's back. Second, there is no way one can get a grip on the nature of irrationality by helping oneself to the analogy with responsiveness to reasons. The two brands of mentality are a world apart.

A Humean Morale

Hume, always the skeptic, offers an antidote to the craving for etiological explanation. He admits to being dumbfounded by certain religious sentiments: "it is … unintelligible … why the reciting of a liturgy by a priest, in a certain habit and posture, should dedicate a heap of brick and timber, and render it, thenceforth and for ever, sacred" (Hume, 1777/1975:199). It takes a leap of the imagination to see anything but a heap of brick and timber in a heap of brick and timber and it takes another leap of the imagination – one too large for Hume to make – to understand how anyone can make the first leap. Not that it is unintelligible in the way monsters are. Hume does not deny that some people are actually animated by religious sentiment. The mental phenomenon occurs, but what takes place is incomprehensible to him, perhaps because he finds nothing in his own mind that may inform him about the nature of it. One may charge Hume with narrowmindedness or complement his honesty. I, for one, incline towards the latter point of view.

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Chapter 8
From Causality to Catalysis in the Social Sciences

Jaan Valsiner

Our knowledge is trapped in the discourse about causality. This trap is set by the common language notions of something causing something else and its penetration into scientific domains. When a medieval examining board asks a young aspirer toward becoming a medical doctor "why does opium put one to sleep?" and nods at the scientific response "because it has *virtus dormitiva*" in it, common sense has helped to create a believable (and for practice, sufficient) causal explanation for it. In a similar vein, I can explain my shyness in social gatherings by my personality trait of "introversion" that—I assume—is located somewhere in my "self-system" and causes my discomfort for being in public. Psychology as well as other social sciences is rich in such causal attributions, yet most of these are discursive tricks that cover up the need for further analysis of how particular outcomes happen.

When philosophy of science enters the arena of the social sciences, the discourse about causality needs to give way to that of catalysis (Cabell & Valsiner, 2014). Why so? As Malnes (2019—Chap. 7 in this volume) demonstrates, thinking in terms of causality has been a complex issue already in the pre-social sciences where the difficult issues of the framing of the research efforts by socially normative conventions are not yet considerable. In the social sciences, they are (Strand, 2019—Chap. 3 in this volume). Furthermore, both social scientists and the phenomena they study are made possible by the power of human agency that has led the emergence of all social, economic, political, and psychological phenomena in human lives.

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Social Sciences: Normative Regulation of Human Agency

In contrast to pre-social sciences, in the social sciences, *intentionality* and *temporality* are of high importance. In addition, the focus on open-systemic nature of all phenomena from biological to psychological, sociological, and political ones is of central relevance. While Niels Bohr in the beginning of the twentieth century did not need to consider the possibility that electrons "jump" from one orbit to another based on their "inherent intention" to do so, the explanation of parachuters jumping off from an airplane cannot be considered a random act but is explainable by their goal orientations, intentions for why they jump, and what they are expected to do after they land. Human psychological, social, economic, and political worlds are built on inherent intentionality and goal-directed moving toward the future—while being regulated by the social normativity of such moves (Brinkmann, 2019—Chap. 11 in this volume). Normativity sets the stage on which the intentional actions of personal and social (institutional) actors operate, and direct causal linkages vanish into the background. In a colorful example, Svend Brinkmann reminds us of this contrast:

> Noticing an elderly lady with damaged grocery bags provides (under normal circumstances) a reason for others to help. The relationship between the situation and preferred action (to intervene and help) is wholly unlike causal relationship between say, the weight of the goods in her bag and the ensuing accident when the goods fall on the ground. The latter should rightly be seen within a space of causation. The goods have no reason to destroy the bags and fall on the ground. They simply do this because of blind causal powers involving gravity. (Brinkmann, 2016, p. 4)

Normativity is central for organizing our knowledge construction in basic ways—as the contrast between causal and catalytic models demonstrates (Toomela, 2014; Valsiner, 2014—see also Figs. 8.9 and 8.10). The contrast is fundamental for any science—it coincides with the distinction between elementaristic (associationist) and wholistic (systemic) axiomatic bases of the different sciences. It is not by coincidence that chemistry and biology have advanced rapidly once the catalytic focus in ways of constructing theories overtook the leading role from the causal discourses in the nineteenth- to twentieth-century advancement of sciences. Such transition has not yet occurred in the social sciences where the habit of search for new knowledge in terms of looking for causal relations between isolated features of the complex system ("variables") still prevails. The result is a cacophony of a myriad of claims of one-to-one relations between elements in a complex system. This approach is myopic—the systemic nature, the whole, gets lost in amidst of the elements. It is time for social sciences to say farewell to the "variables"-focused mindset (Valsiner & Brinkmann, 2016) and find new—systemic—ways of arriving at knowledge. It is here that the catalytic approach becomes central for our epistemology in the social sciences. However, here we face *recursive normativity* that complicates the epistemology of the social sciences. Not only are the phenomena studied by the social sciences normatively organized, but that normative organization of the phenomena feeds into the normativity of the mindsets the researchers use in their studies of these phenomena. Such recursive normativity may hinder

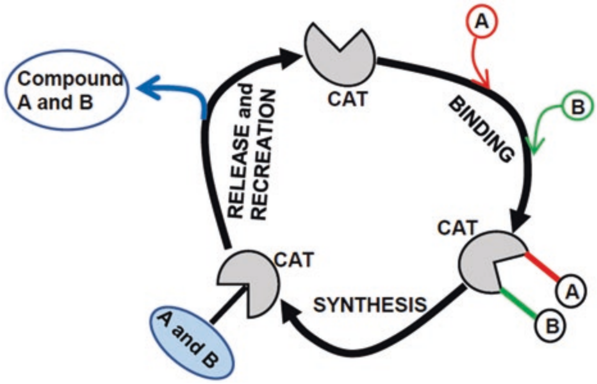

Fig. 8.1 General scheme of catalysis

the boldness of various hypotheses that social scientists dare to set forth for their investigations. *Both* the social scientists and their public—research participants and evaluators of the value of the research—are based on the same canons of normativity. It is therefore not surprising that some of the social sciences—psychology in particular—have been demonstrated to be hindered by *pseudo-empiricism*[1] (Smedslund, 1995) in their research practices.

What Is Accomplished by Thinking in Catalytic Terms? The catalytic approach—started in chemistry in the nineteenth century and overtaking biological sciences in the twentieth—deviates from the classical causal models by focusing on recurrent *reproduction of the system* that produces outcomes ("causal effects" in terms of traditional causality discourses) and giving these outcomes the *status of by-products* of the processes of such reproduction (Fig. 8.1).

Figure 8.1 describes the catalytic process in its generic being. A cycle in the regeneration of the catalysts (CAT) includes three phases—BINDING, SYNTHESIS, and RELEASE AND RECREATION. No discourse of *causality* is present here—the outcome (released synthesized A + B) is a by-product of the catalytic cycle. Two "inputs"—A and B—are binding themselves to the CAT which results in their linking into a new whole (A with B) that is then released from the binding to the CAT. The synthesis emerges as a work of an integrated system—where different parts are integrated into functional wholes.

This form of organization of emergence of new wholes has the advantage over the traditional causality talk. It specifies the self-preserving catalysis system that makes it possible for particular outcomes to emerge (or not). It allows for emergence of synthetic forms that give new structure to the outcome. The compound

[1] Pseudo-empiricism is the research practice in which empirical investigation is undertaken to prove some propositions that already are given in the normative system shared by the researcher and researches. The result is camouflaging the normative implicit knowledge by its "empirical discovery" in the data.

(A with B) is a new form that has Gestalt characteristics beyond its immediate consisting elements (the synthesized unity of A and B). The analysis of the catalytic processes allows for precise depiction of how the new Gestalt emerges. Traditional causality discourse does not afford it—to claim that "the substance CAT *caused* A to become linked with B" would overlook the crucial details of the process of A and B coming together. To make a causal claim here would—in analogy—amount to claiming that the priest who marries a young couple in a church ceremony "causes" them to get married. The priest—and the religious institution it represents—may have a crucial role to play in the organization of the matrimony and family relations, but it is not a causal role. The priest is a catalyst that makes the entrance into a (religious) marriage possible.

Catalytic Models: Overcoming the Limits of *Causality* Discourses

The need for implementing the scheme of catalysis in the social sciences—replacing the traditional causality talk—is clear. The efforts to reduce complexity of the systemic phenomena by attributing causality for these to high numbers of "independent variables" have had its debilitating impacts upon the clarity of theoretical thought (Valsiner & Brinkmann, 2016).

First, the discourse of variables has guided all the empirical activities toward "discovery" of simple causal connections A → B (varying the "independent variable" A causes change in B, the "dependent variable") overlooking the systemic organization of the phenomena. This problem has been recognized in traditional causal discourse (by introducing the notion of "intervening variables") but cannot be resolved within the causal discourse framework where agency (and its counterpart—resistance) is not considered.

Second, the "variables discourse" artificially elevates the researcher into the role of power in working with the phenomena. The researcher is assumed to perform the act of "random sampling" (Valsiner & Sato, 2006) despite the reality that any sampling of human beings depends upon their agreement to be "sampled." The "sampled" persons are at most *invited* (rather than "taken from population") and can counteract the "sampling" by refusing or avoiding participation in research. The possibility of counteraction to "being sampled" leads to new industry of "paying collaboration" where different financial and symbolic incentives are brought to play to guarantee the insatiable need of social scientists for assembling a "large sample."

Furthermore, there is the "illusion of power": the researcher is assumed to have full control over the manipulation of the "independent variables"—which in reality of interaction of the goals-oriented researcher with resisting band divergently oriented "research participants" is a comforting illusion. Adaptation to this limitation is taken to the symbolic level where fixed indexes (gender, socioeconomic status, etc.) become treated *as if* these could be varied at the will of the researchers—statistically, but not in reality. Instead of the glory of control assumed by the researcher, we might be in a more adequate realm if we view the researcher as a beggar—for the data.

Treating the complexity of social phenomena as if it is a matrix of causal "effects" of all kinds of causes and their formal "interactions" (Gigerenzer, 1991) replaces the search for actual organizational principles by attributing causality to an artificially constructed matrix. The social sciences are dealing with phenomena for which the axiom of nonlinearity is appropriate—*all biological, psychological, social, economic, and political phenomena are inherently nonlinear in their organization* (Puche Navarro, 2009). This axiom is built on the observation of the contrast between natural and technological (human-made) objects. The human mind is a system on the border of the two object worlds—its biological substrate (brain) functions as any biological system would (nonlinearly), but the "molding of the mind" by human political and educational systems superimposes a linear order. The latter is the administrative principle of control by homogenized social norms through insisting on the rigor of the classical logic. The human mind of course escapes that administrative control by ways of cognitive heuristics.

The Dynamics of Linearity and Nonlinearity

What does nonlinearity mean? In its simplest explication, it is a curved line. In Riemann-Lobachevsky geometry, all straight lines are actually curved.[2] Nature produces no straight lines, while human technologies are mostly based on organizing their products in linear orders—straight lines, fixed corners, etc. Likewise the methodologies of the social sciences are built upon the notion of linearity—turning complex nonlinear phenomena into artificially "measured" entities (Michell, 1999). By forcing the nonlinear into a process of linearized "measurement" operations creates an irreversible loss of the relevant features of the phenomena.

We assume that the relation between "ugly" and "beautiful" is that of polar mutually exclusive opposites that—if put on a linear scale—can even be turned into quantified indexes (Fig. 8.2b—an equivalent of a rating scale). Yet such quantification of a linear scale is an epistemological impasse (Wagoner & Valsiner, 2005) as it inadequately represents the nonlinearity of the phenomena (e.g., Fig. 8.2a) that it is supposed to "measure." Most objects we seemingly easily rate on a linear fixed scale (like Fig. 8.2b) are complex multifaceted wholes, the Gestalt qualities that vanish in the act of superimposition of the subjective linear order (rating scale). In the example in Fig. 8.2, this vanishing act is exemplified by the inherent ambivalence in the scene—the Biblical personage of Judith after killing Holofernes by cutting off his head, now, is depicted carrying it into the public domain of the viewers of the painting. Her just finished act is deeply ambiguous—she is a murderer, a schemer who purposefully went to the enemy general

[2] Since every straight line can be viewed as a part of an infinitely large circle—where both ends of the straight-looking line eventually meet.

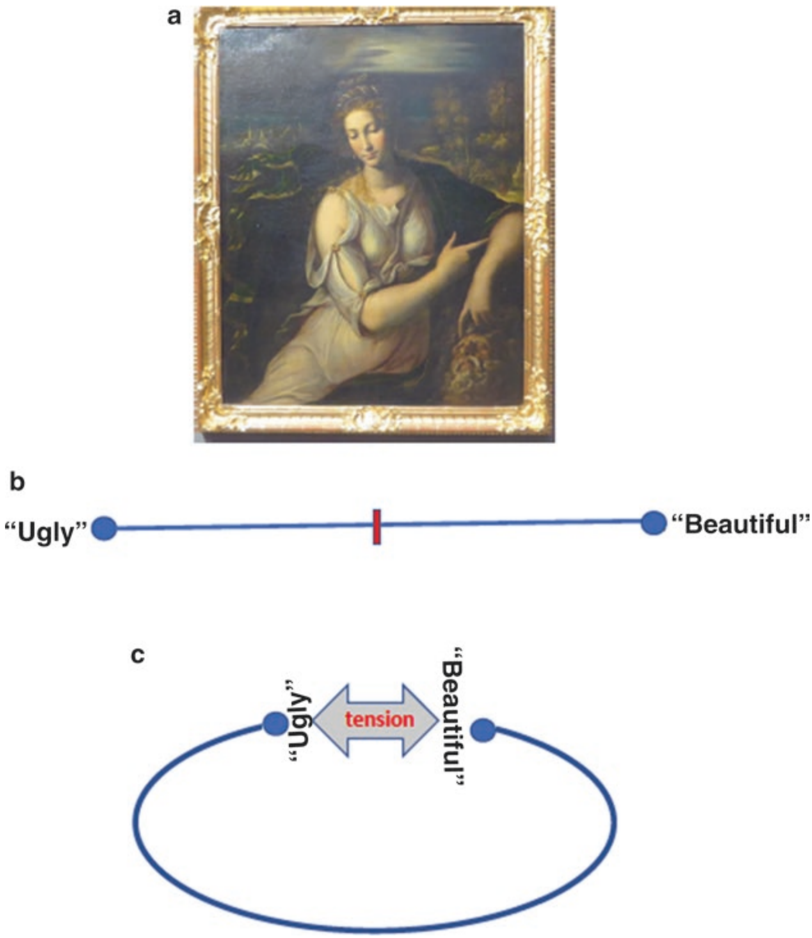

Fig. 8.2 Linear order superimposed on complex processes and its curvilinearization. (**a**) An object of evaluation (Giorlamo da Capri, 1540–50 Judith with the head of Holofernes. Vienna, Kunsthistorische Museum), (**b**) Regular assumption of a linear binary opposite in evaluation (**a** or **b**), (**c**) Curvilinearization of the opposition (from **a** or **b** to bringing **a** and **b** to linking with each other) and emergence of the relation of tension

Holofernes seducing him so as to kill him. At the same time, she is a hero—a woman whose assassination act saves her people from being slaughtered by the troops of Holofernes. The depiction of Judith by the artist does not fail to display her bodily beauty together with the holding of her trophy—Holofernes' head—in her hands.

The realistic process involved in the relating with the object of evaluation differs from the superimposition of the linear order onto the nonlinear phenomenon (as depicted in Fig. 8.2a). It is the adjustment of the psychological system to the nonlinear nature of the object—curvilinearization of the perceiving and appreciating mind—that leads to the self to *create its own* experience *through* the catalytic

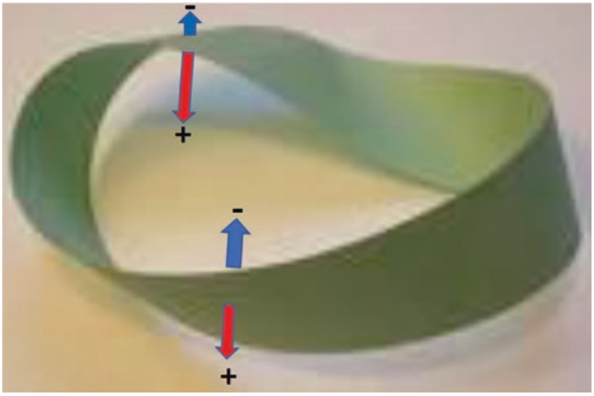

Fig. 8.3 Unity of opposites in perpetual dynamics of regular place change (Möbius strip with forces added)

conditions of the object that is the “target” of evaluation. The flexibility of my *psyche* is being modulated by myself under the conditions of the scene I am experiencing. The psychological tension (Fig. 8.2c) that curvilinearization of the *psyche* brings with it is temporary—it either escalates to a breaking point (and arrives at dialectical synthesis—Valsiner, 2015; Vygotsky, 1971) or de-escalates to a non-tensional state of quasi-linearity.

I posit that this tension is crucial. The dynamics of *linearization* <> *curvilinearization* of the structure of the human *psyche* can be seen as the basic principle of human mind. We become trapped into the insoluble web that unites “love” and “hate” at times (curvilinearized state) in some relation (toward a displeasing political figure or abusive parent) while being completely linear in the relation to subjectively trivial details of daily life (linearization).

An alternative possibility is to see mutually linked opposites as two sides of a Möbius loop—permanently turning into one another (Fig. 8.3). Here the two are permanently together—each + vector is immediately opposed by its counter-vector (−) while their positions on the loop itself fluctuate between front and back positions at every turn of the loop. “Love” turns into “hate” in temporal dominance only to be followed by a next reversal of the dominance relations. This depicts the eternal maintenance of a fixed ambivalence relationship without any possibility of breakdown or breakthrough (synthesis). The tension continues in the eternal cyclicity of reversals of the dominance of opposites.

The Basic Tension: Between Linearity and Spirality I posit (Valsiner, 2019) a very general abstract tension between two orders of any form—linear and spiral (helical). *Triskeleon* (Fig. 8.4a) is the maximum case of curvilinearity. It is a graphic abstraction of the closing of two-dimensional space and opening of the third dimension at the eyes of the spirals (in contrast with the right side, Fig. 8.4b) where the linear abstraction from the center point can expand in two dimensions in all three linear directions, but there is no opening to the third.

This tension is triggered by the settings we are in—the conditions for our activities. We introduce linearity into our human-made natural environments by architectural straight lines—which vanish as the building becomes a ruin and the

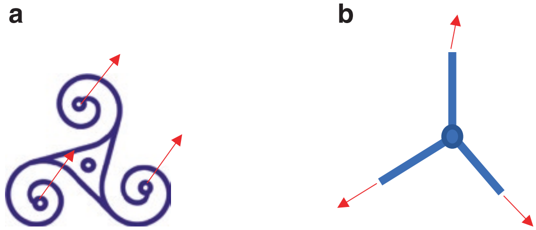

Fig. 8.4 Tension between curvilinear and linear abstract forms

Fig. 8.5 Ceiling decoration in an Egyptian tomb (Goodyear, 1891, p. 90 plate 91)

natural growth takes over. We reproduce curvilinearity in ornamenting spaces of symbolic kinds—such as ceilings of tombs of burial chambers (Fig. 8.5).

The tensions of social living—unity of openness and closedness—are encoded into the forms of environmental decorations during our lives, at the entrance into the "other" world, and in our imagination of the latter.

Ornamentation of any kind is human encoding of meanings into the periphery of our action fields. But what is the "action field" for a dead Egyptian pharaoh whose ceiling of the tomb in which the casket with his embalmed dead body is located under many wrappings? It encodes a generalized and abstracted catalytic orientation for entrance into afterlife.

Ornaments are of catalytic value for our meaning construction acting within our environments (Valsiner, 2019). We create them as decorations (of something—our clothing, our life environments), but while being that, they become catalysts that are present in our agentive actions and provide their meaningful context.

Basic Tensions in Forms and Philosophy of Science

There is something missing in this general picture. Both assumptions—that of a binary opposition (that becomes curvilinear leading to tension between united opposites) and of a Möbius loop—are equally insufficient as they either avoid or fixate time. Even if it is possible to argue that the Möbius loop solution does include time—needed to see the reversal of the back <> front vectors at each turn—the process of the dominance change is turned into a cyclical repetitive loop. Nothing can grow out of the tensions on the surface of the loop despite the tension of the opposites that change their front-back position at every turn.

Philosophy of science is a cruel arbiter for empirical investigations in any science. When the first assumed axiom of a science is wrong, the whole enterprise of a science built on it cannot be adequate—this is the cruelty of empirical efforts in areas where the first axiom was built inadequately. It took chemistry three centuries to escape from the common sense—yet inadequate—assumptions of alchemy. In a similar vein, the social sciences struggle to move beyond reduction of nonlinear complexity into linearized "variables." Linear "variables" cannot generate new of their own kind—but in open systems of biological, psychological, and social orders, the generation of new forms is the starting datum of the systems.

Both models of curvilinear kind (Figs. 8.2 and 8.3) axiomatically rule out the view of phenomena in developmental turns. The latter requires the inclusion of irreversible time. In contrast to their physics counterpart, the social sciences deal with time-inclusive phenomena. If we look at nonlinear opposites in terms of their transformations into novel forms, it is the catalytic organization of self-maintenance and self-transformation that is to be explicated.

How Catalytic Systems Grow?

The primary need for any catalytic account of phenomena is to guarantee their recreation so that they can function in the maintenance of the system (Fig. 8.1). Yet open systems—to which all phenomena of the social sciences belong—do undergo developmental transitions in both directions, increasing their own complexity and annihilating it, at some times.

Innovation of catalytic systems involves catalytic conditions setting itself (Fig. 8.4). Here the fate of the catalytic cycle P-Q-S-? has three potential trajectories at each round, X, Y, and Z.

Figure 8.6 indicates how some by-product of the system takes on a self-catalyzer role (autocatalysis) to enable the system to either eradicate itself (X = "system's suicide"), maintain itself (Y), or innovate its own structure (Z). It is the trajectory Z that is specific for the catalytic processes of the social sciences. Not only maintenance (Y) but the possibility to diverge into trajectory Z allows for living systems to be and to become new. The divergence border of Y and Z is the arena for innovation. The specific combination of autocatalyst (A) and allocatalyst (B) at such junction can lead to a new configuration of the catalytic system -P-Q-S-(?)-.

Phenomena of conversion—to new religious or political orders—give us many examples of such reorganization of the catalytic systems. The major transformation of such kind in European history was the Protestant Reformation. The emergence of self-regulatory internalized religious sentiments (new part depicted by?) led to changed normative practices in daily lives.

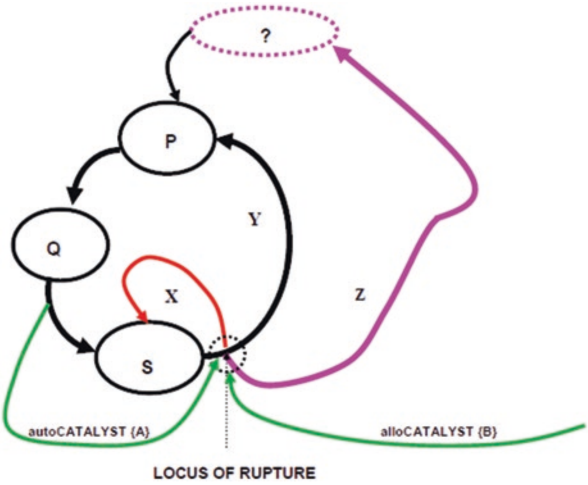

Fig. 8.6 Auto- and allocatalysis in life courses of catalytic systems

Beyond Causal Attributions—To Structures of the Full Field

In our social sciences many causal attributions are made that cannot lead to new *Wissenschaft*, but which nevertheless become consensually accepted. Thus we hear about "financial crisis" as if "causing" some economic outcome, or "gender" causing differences between men and women. In psychology we hear that "culture" causes human beings to act in one or another way (e.g., "my culture causes me to demand that coffee be served in delicate porcelain cup"). In reality the notion culture cannot "cause" anything—it is my personal decision to refuse the cup of coffee offered to me in a plastic cup. I may say "my culture requires that…," but it is I, not the "culture," who makes that statement. Unni Wikan has made the possibilities of culture clear:

> Culture has no agency—only humans and other sentient beings have the power to act… Neither has culture any power—beyond what people attribute to it. (Wikan, 2002, p. 10)

Similarly, attributes like gender, religion, socioeconomic status, and other general idea complexes like that cannot cause anything in society. They are complex catalytic resources that—when utilized by active persons—can unite (or disunite) ideas, persons, and justice systems. They are made functional in the actual conduct by aligning the current action ("I am now doing X") with the wide sign field of catalytic kind ("I can do X, I am a woman and this fits my gender" versus "I can do X, I am a man and this does not fit my gender, but *I want to change gender roles*"). In the case of human beings in any society, the notion of causality may be constrained by intentionality—the move from I WANT → to <u>I WILL → I DO</u> would constitute the human condition of causal action, and the main role for this subjective causal chain is the assemblage of catalytic conditions that make the movement toward set goals possible and meaningful. This narrowed down notion of causality for human actions fits the specific condition of *Homo sapiens*—construction of new environments and their meaningfulness.

This construction of self-sought environments starts from basic features of personal appreciation of relations with environments—making the distinctions between states if silence in contrast to non-silence (Lehmann, 2016). The field of non-silence becomes differentiated into noise (meaningless sounds) and meaningful forms—of speech and music (Klempe, 2016). The latter operates as a catalyst in the scheme of goals-oriented action of a person.

Consider the example of Kurdish 33-year-old refugee from Turkey to Greece. Saber described his relation to the role of Kurdish music in his life (*Kurdish music is very strong because it succeeds in reflecting the everyday life of Kurdish people directly and you experience this immediately*), going on to describe its catalytic role in his personal life:

> …when I work at home or read, I put on quality Kurdish music or classical. But I am a melancholic person, meaning that we come from a certain entity and we have certain roots that we cannot forget. And when I have problems, I try not to avoid them, on the contrary I try to face them, I will put on some melancholic music (pause) I like that and it keeps me alive with the past and the fight [for Kurdish political goals]. (Kadianaki & Zittoun, 2014, p. 198)

In this example we can observe the attributional construction of believed-in causality: Saber sets up the ambience of listening to music as a catalytic condition for the ongoing daily activities, but then presents the music *as if* it has causal properties in relation to him ("*it keeps me alive*"). The music of course does not keep him alive (neither in the literal or metaphoric sense of the word) other than in his belief. This belief in causality reflects the actual role of the music as a catalyst. A similar situation has been described in psychotherapy processes (Valsiner, 1999).

Here we can generalize—the common language attribution by goals-oriented human beings of the form "A causes B" is a projected cognitive illusion that masks the actual role of a catalyst ("A *sets up conditions* for MY(our) achieving B"). The best example is the discourse about "effects" of formal education on human cognitive development. It is habitually presented in causal terms (e.g., "schooling *causes* pupils' transition to use of deductive logic"). In reality "schooling" is a setting—a totality of socially organized education environment—that as a catalytic megastructure *enables* the agentive pupils to master new ways of cognitive functioning. This illusion makes it possible to defocus attention from the agentive role of the person that can play a role for ego-defense functions.

Human beings enter different social institutional settings—religious, educational, political, etc. They join such settings through negotiation of goals of the institutions and their own. Different institutions preemptively set up catalytic conditions to support such "joining in society." Presentations of national history in any society set up conditions for persons to work toward their feelings as citizens of a "nation sate," romanticizing the value of belonging (Lopez et al., 2016, p. 218). Numerous institutionally mandated depictions of hazards of smoking on cigarette packages are to create polarized conditions—supporting the act of nonsmoking (by nonsmokers) and in parallel supporting the act of smoking (for smokers). In both cases the person's initial state of affairs ("smoker" or "nonsmoker") and goal orientations (staying as is versus changing to the other trajectory) are catalyzed by the same ambience. Warnings on cigarette packages do not "cause" smokers to quit smoking (or nonsmokers to start smoking) but set the conditions for the future action of continuity or discontinuity for all. The meaning insertion of dangers into the environment makes all possible relations to the danger feasible—including symbolic ones. It is here where the study of religious sentiments by active human beings in their social surroundings becomes a new—or renewed[3]—frontier for the social sciences. Religion is a social system of spirituality that operates as a catalytic environment for human ways of being (Belzen, 2016). Elsa de Mattos (de Mattos, 2018; de Mattos & Chaves, 2016) has demonstrated how the entrance of a young man into the setting of candomblé guides his transformation into a new form of being. Joining the community enabled the man to reform himself and then establish his autonomous way of living—free from the previous dependences on drugs and alcohol.

Art as Catalyst for Human Affect

What we regularly label as "art" is the result of human creativity over millennia, the functions of which were not for giving their creators aesthetic pleasure but those myriads of reasons that would link the art maker with the world "out there"—in the imaginary domain where ancestors are still alive and where they themselves join them, sooner or later. Being extensions from the meaning-making person on one's own body (e.g., necklaces, beads, etc.), the creation of symbolic forms expanded to that of clothing, surrounding living quarters, and special places for the interchanges with the spirits. The making of symbolic objects—first for specific functions (e.g., masks) that later became "art" (at least in the occidental mindset)—can be analyzed as a massive social practice of creation of catalytic devices for supporting different life problems' solving in the future. A roadside shrine of any religious kind is a semiotic catalytic device once put there by some author, but in its existence over centuries enables the passing-by travelers to feel in some particular ways—rather than others.

The emergence of the genre of pure landscape painting in Renaissance Europe in the sixteenth century and its continuation to our days are an extreme example of creating wholistic scenes of nature which are the result of the painter's imagination (and drawing skills) and came to be of demand by paying collectors of paintings. In contrast to portrait paintings—where the function was to preserve the images of oneself and of one's forebearers for next generations—landscape paintings did not

[3] The psychology and sociology of religious feelings were an honorable research topic in the social sciences in its history, all through to the 1920s. After that it declined in favor of comparative perspective—comparing various religious denominations with one another, without looking into the functions of particular religious rituals in human lives.

Fig. 8.7 Claude Lorrain's *Coastal Landscape with Acis and Galatea* (1657)

have such personal connectivity with the objects painted. Many of the scenes painted were imaginary[4]—in the Netherlandish art of the seventeenth century, scenes of Dutch villages or towns on the foreground, with Italian-type mountain images in the background, abounded. The painters connected the immediately visible with the imagery—resulting in sublime paintings that would keep their new owners fascinated for long times as the paintings would hang in their ordinary living places.

The roots of landscape painting in Europe are in the depiction of imaginary landscapes of no physical referent as the background for depicting Biblical scenes. The nature depicted in the paintings or graphic sheets of how the apple-eating couple of Adam and Eve were expelled from the Garden of Eden was lavish. Step by step in the history of painting arriving in Renaissance, the Biblical figures disappeared or became kept less in focus (see Fig. 8.7), while the nature remained depicted as lavish as before or more.

Together with the avalanche of Protestant Reformation and its iconoclastic vicissitudes against Catholic images, the religious meaning in paintings became clandestine—resulting in the eighteenth-century gnoseognostic ideology of viewing the nature as the ultimate proof of divine creation. Religious figures in paintings

[4] Painters painted mountains without leaving their studios. It was only on rare occasions that they were asked to paint from the nature itself.

were no longer necessary, but exaggerated geological formations acquired their functions in their sign values. Further transformation of the genre into Romantic landscape painting in the nineteenth century (e.g., German version started by Caspar David Friedrich and the Norwegian counterpart of Johan Christian Dahl) grows out of turning the religious feelings into secular-aesthetic enjoyment of the painted landscapes in Romantic terms.

Referring to the paintings by Claude Lorrain (Fig. 8.7) and Jacob van Ruysdael (Fig. 8.8), which were both in Dresden galleries in the beginning of the nineteenth century, Carl Gustav Carus reflected upon them in his classic 1820s sequence of *Nine Letters About Landscape Painting*:

> … before which you and I could never stand without involuntarily drawing a deep breath, filled with the sense of a cheerful, warmer, southern air; but you also remember the waters, both rushing and still, and the grave beech and oak trees, which Ruysdael presents to us with such infinite freedom and truth that our beloved native landscape seems almost to speak to us directly. Here we may say that the artist's inner meaning has assumed objective form; both artists' work convinces us that they had absorbed the life of nature into themselves, in all its beauty and grandeur, and that it pulsed through their veins and sinews, enabling him to speak to us in nature's language, and to reflect its forms in all their pristine beauty. *Hence the feeling of freedom and well-being that overcomes us when we stand before these paintings: we are aware of a beautiful, human individuality that allows us to contemplate its inner essence* reflected in the mirror of the true divine world—that is to say, in the truth of nature—and does so freely and calmly, making no attempt to direct us toward any particular view, but at ease in its own blissful contentment; thereby moving us to lay aside all our petty, one-sided concerns…. (Carus, 2002/1831, pp. 108–109, added emphasis)

Fig. 8.8 Jacob van Ruysdael *Waterfall with a Ruined Castle* (1665–1670)

Two aspects of the landscape paintings are important to emphasize in the present discussion of catalytic process of the human *psyche*. First, the paintings *are second-order* (semiotic) catalysts—they are depictions of real or imaginary views of the nature and its modifications. In contrast with the *first-order* catalysts—the totality of the meaning field of the person who experiences the natural scene—the paintings have limited dimensions (Lorrain's is 102 × 136 cm, Ruysdael's 119 × 180 cm in canvas size). Yet the deep experience that a viewer can generate within oneself is comparable despite this contextual miniaturization. The paintings—like original landscapes—act as catalysts in *the viewer's own creation of their own affective states* in the given setting. The paintings do not cause feelings in the viewers, but the act by the viewers to look at them makes it possible for the viewers to arrive at new feelings.[5]

Where Chemistry Ends and Semiotic Catalysis Begins

The notion of catalysis entered into chemistry over long time—from 1830s (starting with the pioneering work of Jens Jacob Berzelius) to 1909 (Wilhelm Ostwald's receiving Nobel Prize). Its entrance has eliminated the traditional causality discourse from the discipline and opened doors for further innovative thinking in biology. The entrance of the ideas of catalysis into social sciences has taken a further century—the first volume suggesting it for psychology's metalanguage appeared only in 2014 (Cabell & Valsiner, 2014). Its appearance into sociology, economics, history, and anthropology is still to be seen.

One of the most prolific extenders of the catalysis concept in chemistry in the twentieth century—Alwin Mittasch—paved the way to its possible introduction (Fig. 8.9) back in 1938, leaving the psychological side of the pyramid unfilled.

As we can observe from Fig. 8.9, Mittasch had deterministic hopes for psychology as science which neither psychology nor other social sciences can in principle fulfill (as open systems, their phenomena thrive on indeterminacy). The downward regulation idea (from conscious will to unconscious wishes) was also accepted by him. Coming to catalysis notions from semiotic cultural psychology, it is not possible to see as conscious will as determiner of lower-level processes, but as the starting point of further levels of catalytic regulation that reaches out to other levels of organization in society that are covered by other social sciences (Fig. 8.10).

Figure 8.10 integrates Mittasch's efforts to make sense of catalysis in the physical-chemical world with that of the social sciences. In the latter case we axiomatically view catalytic processes as constructed by the intentional human beings. That intentionality is semiotically mediated—a qualitatively new state in the hierarchy of catalysts. Transition to that state is prepared by the catalytic systems at biological level.

[5] Precisely similar role of catalysts is played by fiction—in the reading of a novel or a short story, specific features of the text enable, but do not cause, the reader to arrive at new experiences, including life philosophical hyper-generalizations (cf Valsiner, 2015; Vygotsky, 1971).

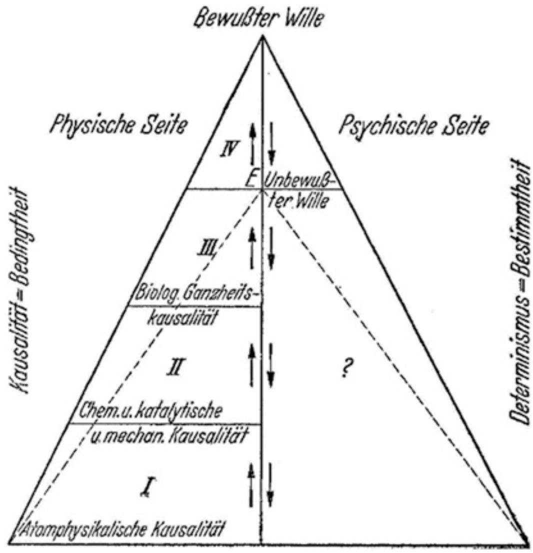

Fig. 8.9 The pyramid of catalytic processes (Alwin Mittasch, 1938)

The Cultural System of Catalysis: Preparing for the Future

Mittasch could not solve the problem of catalysis for the realm of the social sciences. The normative and intentional nature of the latter calls for the higher—cultural—catalytic systems to be conceptualized.

The crucial feature of the phenomena in the social sciences is the flexibility. Our *intentional coordination* of conditions of personal and collective cultures (Valsiner, 2014) with social representations in society is a feature absent at the lower levels of catalysis. This is made possible for the use of sign systems at various levels—personal, communal, societal, economic, and political. We can look at human phenomena as *semiotically catalyzed*. Semiotically,

> Meaning appears only due to a contact between code relations. A contact between (incompatible) codes which activates semiosis, requires a living system. This is because semiosis assumes a mechanism of learning, i.e. a mechanism that can create new codes (therefore to restore and to reproduce) which is just a feature of the living systems. (Kull, 2014, p. 118)

We produce (and reproduce) sign complexes that catalyze our ways of being human. This is possible due to the *double function* of signs we create and use, as we operate with signs on the constantly moving border of the PRESENT in between the FUTURE and the PAST. The primary function of a sign is to grant the meanings of action in here and now. The secondary function of the sign is to provide hyper-generalized meaning field for the future—to be utilized at any moment of need to put into place a catalytic condition.

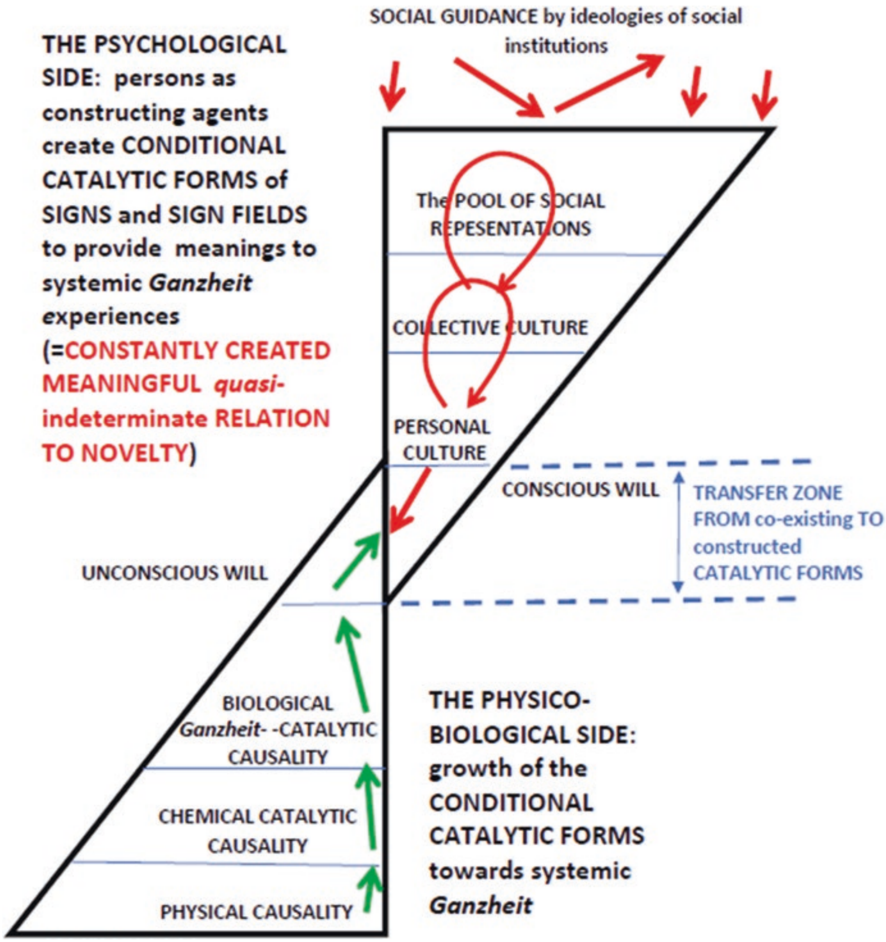

Fig. 8.10 Semiotic catalysis (reconstruction of the Mittasch Pyramid)

A number of interesting features emerge from the notion of double functions of signs. First, the human meaning making in the present is oriented to the future—immediate (here and now giving meaning to the unfolding experience) and indeterminate—setting up anticipatory meaning orientation for possible future conditions (de Mattos, 2018). It is the latter that produce the basis for semiotic catalysts.

Figure 8.11 also illustrates the *aboutness* of the future and the meaning-based borders that semiotic catalysis enables to get introduced. The border between the desired and the non-desired directions (both characterized as zones with non-fixed outer borders) is enabled by the process of hyper-generalized signs. As Alaric Kohler has pointed out,

> Catalysts operate by removing or replacing a constraint on variability. (Kohler, 2014, p. 69)

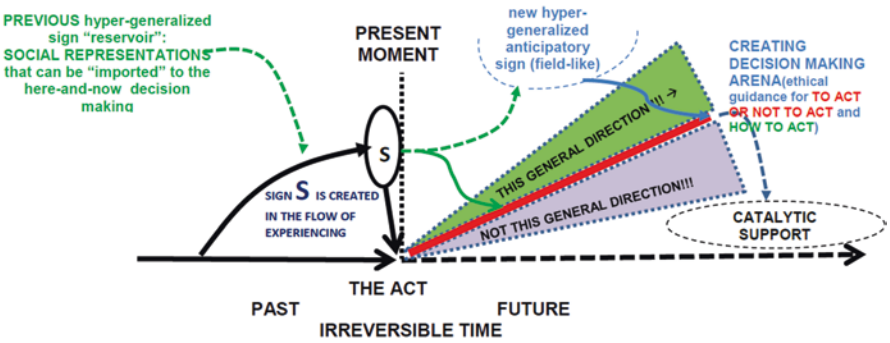

Fig. 8.11 Making of catalysts through double function of signs

Thus, the indeterminacy of the future is precisely the reason for creating catalytic conditions for future events long before they are on the psychological horizon:

> The psychological horizon is the *infinite realm of possibilities ahead of time yet to be semiotized, thus still partially socially unbounded*, that is necessary as a reference point to the person's widening of life space. *The horizon/sign is the specific sign that, once produced, establishes the conditions for the psychological horizon to participate in the production of new psychological phenomena through the co-regulation* of psychological processes. (Tateo, 2014, p. 236)

Here we come to the central issue of all social sciences—handling of the transition of the concreteness of the present toward the inevitably uncertain future. The notion of forward-oriented semiotic catalysis here has theoretical advantages over strict sign-regulated control that becomes possible in the present moment (S in Fig. 8.11).

Guiding the Semiotic Catalytic Roles in the Future The presence of a hyper-generalized sign field as a catalyst projected into the future has the flexibility to be usable in a particular direction when the semiotic agent (person, institution, etc.) needs it. Yet such catalytic fields for the future need to be established in the here-and-now setting. Da Silva (2014) introduced the notion of semiotic catalyst activator—a sign that in the present guides the establishment of the catalytic sign field for the future. Through such activators the future field of semiotic catalysts is directed in desired or expected directions, such as moral self-expectations (Nedergaard, Valsiner, & Marsico, 2015), sensual-religious feelings of temple dancers (Valsiner, 1996), or the hyper-generalized expectation for social revelations of guilt within a civil society (Brinkmann, 2010) or of violence within family (Musaeus & Brinkmann, 2011). The outcomes of such activators are creating meaning-construction atmospheres within a given person or society. Phenomena of witch-hunting, suspicions of espionage by foreigners, expectations for physical and sexual violence from different socially stigmatized outgroups, and much more—all the histories of human societies—are filled with examples of the work of semiotic catalyst activators.

The semiotic catalyst activator signs are activated by the sign maker to guarantee that not every hyper-generalized sign takes on catalytic functions. These are meta-level signs that act upon the directions of field-like signs to guide them either into becoming promoter signs (directly impacting on the meaning construction) or catalytic frames (enabling the work of other promoter signs). A single case example of how a young US college student following at first his father's White supremacist ideology not only overcomes it (negates the inherent racism in society) but develops a new personal life course crossing the race lines in his own marrying life (see Mascolo, 2017, for full description).

Two specific features in the transition of the young White supremacist into a flexible human being who succumbs to the affective attraction across race borders are relevant here to see the semiotic catalyst activator in action. First is the "base line" of deeply embodied interracial feelings of negative kind—not directly expressed. In fact he was socialized to keep his feelings toward other races strictly under personal control. The young man recalled only one different episode—when he was on a wrestling for extra sports credit in high school:

> The only time I got to release my frustration was when I wrestled—especially those Blacks in competition extra curriculum activities at school. I *thought about my people and what their people* were doing to mine. And *I was satisfied at the sound and sight of making their face hit the mat and if I was lucky, drawing blood*. Afterward *I would run for the shower wiping away the filth of the disgusting contact and scent scrubbing vigorously for almost an hour*. They were one and the same and not my people I can give a damn about them. (Mascolo, 2017, p. 232 added emphasis)

The deep—yet externally invisible—interracial separation and dismissal were in place as a result of polite socialization. The opposition "we" <> "they" was the main guidance of relating with others. Yet the strong opposition coming from family socialization had a potential for transformation—through the curiosity of the young man trying to get the glimpse of the "other," even if staying on one's own established ideological position. It took slowly developing affective innovation for the young man to transcend that position.

Love has been powerful in making changes in our mundane ways of living possible. While in college, circumstances brought the young man into joint study task with a Black girl—step by step moving toward deep personal relationship. Again the pre-established internalized dismissal of the other was in place as he tried to avoid the joint assignments and verbal challenges ("go to your people", ibid., p. 233). Yet the joint work did build an attraction (and decision that "she was an *exception to her people*"). While this slow un-racializing interpersonal process was going on, an encounter with a Black male student whom he despised yet became curious about his capacity to enter into interaction with others. Our supremacist decided "to play liberal":

> "Hey man why are you always talking to White girls?" He looked at me conspicuously … He responded "Well it don't look like I got many options at this school. Say man, you wanna give me a hand with this box?". On another day I would have obviously said "hell no" but I needed more answers "Why do you get along with White people?" "Huh?" "You have nothing in common with them… us" I replied calmly. He let out a slight chuckle before replying, "Sure we do, we usually like to have fun and play and watch sports. I mean what has race have to do with getting' along with people?" I gave no expression *not wanting to admit that he had actually made a bit of a poin*t. And even though he was a Black basketball player he was not as dumb as I thought he would be. (Mascolo, 2017, pp. 234–235, added emphasis)

This episode is an example of the agent's ("supremacist") move toward creating a semiotic catalyst activator that would enable him to accept the other race in principle—when his own immediate interaction benefitted from it. The simple doubt ("what has race to do with it?") that produced the "bit of a point" actually led to overcoming of the strict stigmatization "my people" <> "your people" and creating an atmosphere of personal acceptance of openness. It is through regulating the nature of background atmospheres that social systems set the stage for all of the normatively possible and impossible actions—as well as their change.

Cases of structural transformations of normatively regulated developing systems lead to the need for the adoption of new formalizing systems for the social sciences. The axioms of the general linear model do not fit the tensions in linearizing <> curvilinearizing social and psychological processes. New formal models of nonquantitative mathematics are likely to innovate the social sciences. For example, topological innovations allow for making sense of the phenomena of borders in human minds and activities. Borders—in biological sense *membranes*—play crucial role in all systemic perspectives. New methodologies of the study of maintenance and transformation of social borders at all levels—psychological, sociological, economic, and political—are the next horizon toward which the philosophy of social sciences can strive.

Conclusion: Normative Sciences Need Systemic Developmental Epistemology

The issue raised in this chapter is wider than simply a choice between causality discourse and its catalytic counterpart. After all, the causal models can be emulated into the wider catalytic scheme as linearizing mindsets in the field of phenomena that require nonlinear models to maintain the crucial nature of the phenomena in our generalizations.

Historically the story was the other way around—catalytic models emerged in the opposite order—from overcoming the non-systemic focus of common sense and discovering the basic cyclical nature of self-maintaining and developing systems. The systemic-structural nature of catalytic processes is essential for making sense of dynamic complexity (Toomela, 2014). Social systems are nonlinear systems ready to produce unexpected and contradictory outcomes. Such surprises and contradictions are not aberrations of normativity, but a necessary result of societal development where normative constraints are constantly being reorganized.

Still there is the additional feature of normative systems—where psychology takes the lead as it links the biological, psychological, and socioeconomic sides of human action (Brinkmann, 2016, pp. 11–14)—that of *intentionality to resist and reorganize* the normative systems (Chaudhary, Hviid, Marsico, & Villadsen, 2017). Whether it is an adolescent resisting the parents, disadvantaged social groups resisting their status, or dominant groups resisting giving up their power—any systemic account in social sciences needs to include the potentiality of specific resistances into its schemes.

Social sciences introduce a new demand for philosophy of science—to account for the agency of purposeful actors and their co(unter)-actions in any generalized scheme of catalytic processes. This demand is an opportunity that may lead all social sciences toward understanding the dramatic realities of the human condition.

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Chapter 9
How to Identify and How to Conduct Research that Is Informative and Reproducible

Janis H. Zickfeld and Thomas W. Schubert

In 2011, the field of psychology, and social psychology in particular, entered into a state of *crisis* through a series of remarkable events: (1) The work of a renowned social psychologist was found to be largely fraudulent (Levelt, Drenth, & Noort, 2012). (2) After an APA flagship journal published a paper that alleged evidence for psychic processes (Bem, 2011) and the journals' editors emphasized that the paper satisfied the field's standards (Judd & Gawronski, 2011), critiques documented that the papers' statistics and their reporting violated basic assumptions (Schimmack, 2012; Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011). (3) In line with this, common ways of how to collect and analyze data were called out for violating basic tenets of statistics and labeled as *questionable research practices* (QRPs) or also *p*-hacking (Simmons, Nelson, & Simonsohn, 2011). Perhaps because all of these events occurred virtually at the same time, they started a process of change that is ongoing (Nelson, Simmons, & Simonsohn, 2018; Spellman, 2015).

The work of all researchers – young and old – is touched by this process of change in two major ways: We are all both using and producing scientific knowledge. First, you are always building on prior work, be it classic findings, standard paradigms, or recently published findings that you want to replicate, extend, or challenge. In all cases, you have to ask yourself: How reliable are the findings I am working with? For instance, if your goal is to explain how a classic effect comes about by showing a mediation, can you be confident that the to-be-explained effect can actually be obtained? Similarly, if your goal is to show that a recent finding is moderated by another variable, could you replicate the original finding in your control condition? Or, if you want to use a classic paradigm, how reliable is it really?

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Second, it is especially PhD students and postdocs who have become the main labor force of the research enterprise in psychology over the past three decades. While assistant, associate, and full professors are often too busy teaching and writing grant proposals, PhD students and postdocs often perform all steps of the research process, under supervision of course – sometimes more, sometimes less. Much is expected of both groups: In a limited amount of time, they are asked to produce research that makes an *independent contribution* to the scientific literature.

Departments differ in how exactly they define what counts as a contribution. The standard that has developed at many European universities is that several manuscripts that could be publishable are expected or ideally already are published in a peer-reviewed journal or at least submitted to one. The previously common scientific monograph is being phased out. At our own department at the University of Oslo, Norway, the last monograph-based thesis was submitted in 2012, and the typical thesis contains between three and four manuscripts, of which two are often already published. To come up with four manuscripts in 3 years of work is a formidable task. To have two of them published, with editorial processes often taking months, can be challenging. And that is where we get back to the crisis and change in the field of psychology. It is common wisdom now that pressure to publish, combined with reluctance of journals to publish nonsignificant results, results in people taking shortcuts. Such shortcuts can be *p*-hacking, ignoring common wisdom to produce nonsensical results (e.g., for psychic abilities), or even fraud (Bakker, van Dijk, & Wicherts, 2012).

For your role as a creator of scientific knowledge, you can thus translate the current process of change in psychology as this: When you finish analyzing a dataset and decide to write it up for a chapter of your PhD, imagine you would do exactly the same study again, with the same measure and sample size. How confident would you be that you would get the same results – i.e., that you could replicate the finding you are about to submit for publication? And furthermore, how confident do you need to be that you could replicate your own finding in order to publish a finding and thus enter it into the scientific record? Would you ever publish a finding that you are not sure you or others could replicate?

Luckily, searching for the answer to these questions has gotten a lot easier since 2011. Many psychological scientists have risen to the task and started to look at their own work in a whole new manner. We can observe a combination of new work on methods, a strong focus on replication combined with changing editorial practices, and new standards in opening up raw data that are facilitated by new online tools and platforms. In the following, we will provide a summary of the *best* methods to identify solid work that you can build on and produce such work yourself.

We start by identifying possible ways to evaluate published research findings for its credibility and afterwards provide an overview of tools and processes that can be helpful in producing informative research. We should note that our overview is by far not exhaustive and not all tools can be applied equally well to all research designs across the social sciences. However, we think that this chapter could serve as a primer for producing informative and reproducible research. In order to facilitate the informativeness of this chapter, we provide an overview of all major tools and mechanisms in Table 9.1.

How Can We Evaluate Published Research?

Publication Bias

In a survey among more than 2000 researchers, about 50% of respondents indicated only presenting studies that worked (John, Loewenstein, & Prelec, 2012). Similarly, Fanelli (2010) reported the observation that negative results had been disappearing from research reports between 1990 and 2007. Such a tendency to publish only positive results has been labeled as *publication bias* (Dickersin, 1990; Franco, Malhotra, & Simonovits, 2014) or sometimes also the *file drawer problem* (Rosenthal, 1979). The problem of publication bias has been discussed in depth (e.g., Dickersin, 1990). Publishing only studies that *worked* showing positive results provides a skewed estimate of the actual effect. It has been stressed that studies presenting mixed results (i.e., positive and negative findings) are possibly more valid than studies reporting exclusively positive findings (assuming statistical power does not always equal 1, Lakens & Etz, 2017). Some authors have gone so far as to argue that published research might represent the 5% type I errors, finding an effect although there is none, while the remaining 95% of studies are kept unpublished (Rosenthal, 1979) or that most published findings are indeed not valid (Ioannidis, 2005).

In order to review published literature on a certain effect, researchers have typically employed the use of meta-analyses. However, meta-analyses are often able to accumulate only published research and therefore suffer from publication bias (Thornton & Lee, 2000). There have been a number of techniques suggested to account for publication bias in meta-analyses including the *trim and fill* technique (Duval & Tweedie, 2000), meta regression approximations such as *PET-PEESE* (Stanley & Doucouliagos, 2014), or Egger's regression. Recent simulations have recommended the *three-parameter selection model* (Iyengar & Greenhouse, 1988; McShane, Böckenholt, & Hansen, 2016) that seemed to perform best (Carter et al., 2019). However, the performance of such adjustment techniques seems also to depend heavily on the number of studies, the sample sizes, and the heterogeneity of effects (e.g., Carter et al., 2019).

p-*Curve*

Sometimes a researcher wants to evaluate the validity of a single set of studies without performing an extensive literature search or performing a meta-analysis. In such a case one possibility is the use of a *p-curve* (Simonsohn et al., 2014), which targets

Table 9.1 Overview of different tools and techniques to evaluate published research and conduct preproducible and informative research

Main topic	Subtopic	Description	Tool	Source/further reading
Evaluating published research				
Publication bias	File drawer	Repository of (unpublished) replication attempts	PsychFileDrawer	http://www.psychfiledrawer.org
	Meta-analysis	Techniques to account for publication bias in meta-analyses	Trim and fill, PET-PEESE, Egger's regression, three-parameter selection model	Carter, Schönbrodt, Gervais, and Hilgard (2019) Online app: http://www.shinyapps.org/apps/metaExplorer/
	p-curve	Technique to uncover publication bias based on distributions of *p*-values	*p*-curve analysis	Online app: http://www.p-curve.com/app4/ Simonsohn, Nelson, and Simmons (2014)
Statistical reporting	Statcheck	Software to detect misreporting of *p*-values based on statistics and DFs	Statcheck	R-package: *statcheck* Online app: http://statcheck.io Nuijten, Hartgerink, van Assen, Epskamp, and Wicherts (2016)
	GRIM	Granularity test to detect plausibility of means	GRIM	Online app: http://www.prepubmed.org/grim_test/ Brown and Heathers (2017)
Conducting and publishing research				
Disclosure and reporting	Disclosure statements	Acknowledging full transparency in reporting	21-word solution	Simmons, Nelson, and Simonsohn (2012)
	Effect sizes and confidence intervals	Software/spreadsheet to calculate effect sizes and CIs	R-project, jamovi, JASP	R: https://www.r-project.org Jamovi: https://www.jamovi.org JASP: https://jasp-stats.org Spreadsheet (Lakens, 2013): https://osf.io/ixgcd/
	Open data and materials	Guidelines for sharing of research material and repositories	TOP guidelines, data repositories, Open Science Badges	TOP guidelines: https://cos.io/our-services/top-guidelines/ Directory on repositories: http://www.opendoar.org see also O. Klein et al. (2018) for a list of repositories Open Science Badges (Kidwell et al., 2016): https://cos.io/our-services/open-science-badges/ Practical Guide on Transparency (O. Klein et al., 2018): http://psych-transparency-guide.uni-koeln.de

	Preregistration	Templates to facilitate preregistration	osf, AsPredicted	osf: osf.io AsPredicted: aspredicted.org Pre-Registration Template: van't Veer and Giner-Sorolla (2016)
	Registered reports	Overview of journals adopting registered reports	Center for Open Science	Registered reports overview: https://cos.io/rr/
	Power	Software to perform power analyses	G∗power, two factor power, power med	G∗Power (Faul, Erdfelder, Lang, & Buchner, 2007): http://www.gpower.hhu.de/en.html Multilevel Models (Judd, Westfall, & Kenny, 2017): https://jakewestfall.shinyapps.io/two_factor_power/ Mediation Models (Schoemann, Boulton, & Short, 2017): https://schoemanna.shinyapps.io/mc_power_med/ R-package: *pwr; SIMR* (Green & MacLeod, 2016)
	Accuracy	Software to perform accuracy analyses	MBESS	R-package: *MBESS* (Kelley & Lai, 2016)
Accelerate scientific research	Replication	Checklist for conducting replication studies	Replication recipe	Replication Recipe: Brandt et al. (2014)
Methodological techniques	Sequential analysis	Theoretical overview to perform sequential analyses	Sequential analysis	NHST: Lakens (2014) Bayes: Schönbrodt, Wagenmakers, Zehetleitner, and Perugini (2017)
	Falsifying a hypothesis	Software to perform equivalence tests	TOSTER	TOSTER (Lakens, 2017) Online app: https://osf.io/q253c/ R-package: *TOSTER* Tutorial (Lakens, Scheel, & Isager, 2018)
	Bayesian statistics	Software to compute Bayesian calculations	JASP	JASP: https://jasp-stats.org R-package: *BayesFactor*
	Mini metas	Software to conduct meta-analyses	Mini meta, metafor	Mini Meta (Goh, Hall, & Rosenthal, 2016): https://osf.io/6tfh5/ R-package: metafor (Viechtbauer, 2010)

the power of a study rather than its effect size. P-curve attempts to detect selective reporting such as publication bias by exploring the distribution of reported *p*-values from a set of studies. In general, based on the assumption that true effects result in right-skewed p-curves, the obtained p-curve is compared against such a benchmark. If such a right skew is violated substantially, the test expects the studies to be based on selective reporting or publication bias. The authors have provided an online application to easily perform p-curves on any set of studies (http://www.p-curve.com/app4/). Although p-curves have been observed to be rather robust across different situations (McShane et al., 2016; Simonsohn, Simmons, & Nelson, 2015), some studies have failed to find support for the validity of p-curves in the case of observational studies (Bruns & Ioannidis, 2016). Other techniques with similar goals are currently being developed (e.g., Krueger & Heck, 2018; Schimmack & Brunner, 2017).

Checking the Reporting of Statistical Tests

While it is important to evaluate the overall claims of published research, simply checking the reported data can give an idea of the validity of presented research. Nuijten et al. (2016) presented *statcheck*, a tool to test whether the reported *p*-value matches the presented test statistics and degrees of freedom. The application has been found to show good reliability compared with a manual process of checking test statistics. Statcheck helps researchers keep track of rounding errors or misreporting. Unfortunately, the application is only able to recognize the most common tests at the moment. The statcheck package (Epskamp & Nuijten, 2014) can be used in the R environment or online (http://statcheck.io). Another way of discovering errors in reporting has been the GRIM test (Brown & Heathers, 2017). Basically, the test verifies whether a reported mean is plausible based on the reported sample size. Imagine two participants filling out a 5-point Likert-type scale. In such an example, a mean of 2.5 or 3 would be nothing out of the ordinary, but a mean score of 3.33 would be impossible based on the sample size. The GRIM test can point out such anomalies and is available online (http://www.prepubmed.org/grim_test/).

Rules of Thumb When Evaluating Published Research

In general, it is hard to generate any basic rules of thumb to evaluate the veracity or validity of published research in the social sciences as such rules would differ across types of methods and contexts. Considering empirical quantitative studies, there are however a number of aspects that can be kept in mind. First, the combination of small sample sizes and effect sizes that are probably small in the population is often present in psychological research, but should be evaluated with caution as such studies might often be low in statistical power, that is, there is a low probability of finding an effect if the null hypothesis were false (Nuijten, van Assen, Veldkamp, & Wicherts, 2015). Note that the reported estimated effect sizes might actually often be large, and even implausibly large. We will turn to a more detailed discussion of power in a later section, but also want to warn here against overinterpreting observed power on a per-study basis (Cumming, 2012). In addition, one should be cautious of research articles presenting several small scale studies with all statistically significant results (Lakens & Etz, 2017; Schimmack, 2012). Even true effects should generate statistically nonsignificant findings by random error alone with some frequency (Francis, 2012).

Researchers might not only apply the presented techniques in order to evaluate published research but also their own studies and projects. Applying adjustments for publication bias or to check the validity of statistical reporting will help to increase the informational value of one's own research. After providing examples of some tools that might guide the evaluation of published research, we now turn to a summary and discussion of aspects that researchers can employ to increase the veracity and validity of their own projects.

Old and New Lessons Learned on How to Conduct and Publish Research

In recent years social scientists have increasingly engaged in discussion and reflection about methodological and dissemination practices (Nelson et al., 2018). Explicit recommendations have been made to increase the scientific value of research (Asendorpf et al., 2013; Miguel et al., 2014; Munafò et al., 2017) and specific long-recommended changes have been adopted recently (e.g., Kidwell et al., 2016). In this section we introduce and review specific possible ways of how to increase the scientific rigor of research. Based on the classification by Asendorpf and colleagues, we focus on two main aspects. First, we discuss practices to *increase research transparency* including disclosure and reporting practices, sharing of data and material, preregistration of research plans and analysis and registered reports, and finally a priori power analyses and simulations as well as planning for accuracy. Second, we highlight ways to *accelerate scientific research* by conducting replications. In addition, we present possible methodological techniques to increase the informational value of research including sequential analyses (Lakens, 2014; Schönbrodt et al., 2017), meta-analyses (Goh et al., 2016), and how to falsify hypotheses (Lakens, 2017; Wagenmakers, 2007).

We note that the reviewed practices and techniques are not applicable to the same degree in all situations. Sometimes sharing of full data might be problematic due to ethical constraints, preregistration might not be fully applicable because of an exploratory focus, or power calculations are not of interest due to a qualitative methodology. Nevertheless, we argue that adopting as many practices and actions as possible could benefit the integrity and rigor of social sciences.

Increasing Research Transparency

Disclosure and Reporting

In order to provide reliable research reports and allow fellow researchers to draw valid interpretations and evaluations, many scholars have argued that it is vital to provide full disclosure on the so-called basic 4: the sample size determination rule, specific exclusion criteria, all measured variables, and all tested conditions (LeBel & John, 2017; Simmons et al., 2012). Simmons and colleagues have suggested a *21-word* solution that can easily be included in experimental studies: "We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study." Disclosing such basic information about one's study increases transparency and provides a valid description of the intentions and methods (Miguel et al., 2014). Some journals have started to adopt the policy of requiring authors to include a full disclosure statement (e.g., Eich, 2014).

Next to the aspect of disclosing information about the study design, several discussions have targeted the way researchers are reporting statistical results. According to a survey of articles published in the *Journal of Experimental Psychology: General* during 2009 and 2010, Fritz, Morris, and Richler (2012) found that less than half of the articles reported effect size measures and none of them a confidence interval. Scholars have underscored the importance of reporting effect size estimates and corresponding confidence intervals (Cumming, 2012; Fritz et al., 2012; Lakens, 2013; Lakens & Evers, 2014). On the one hand, effect sizes provide information about the practical significance of empirical studies and have been argued to facilitate cumulative science as they can be helpful in power calculations and meta-analyses. On the other hand, confidence intervals provide information on the precision of the estimate. However, popular statistical packages such as SPSS still fail to provide information of various effect sizes (e.g., *t*-tests), which might be one reason for the underreporting of effect size estimates. Open software applications such as *jamovi* (https://www.jamovi.org), *JASP* (https://jasp-stats.org), or *R* (https://www.r-project.org) provide possible solutions for such shortcomings. Similarly, Lakens (2013) has provided several easy-to-use spreadsheets that calculate effect sizes for the most common statistical tests (https://osf.io/ixgcd/). In addition, to aid interpretation of effect sizes, Fritz et al. (2012) have suggested to include statistics such as the *probability of superiority* (*PS*), the percentage of occasions when a sampled member of the one group has a higher mean than a randomly sampled member of the other group (Grissom, 1994), or *U*1, the percentage of nonoverlap of the distributions (Cohen, 1988). Finally, it has been highlighted that researchers should always report additional descriptive statistics including means, standard deviations, or correlation matrices (Fritz et al., 2012). Reporting such information is critical for the inclusion in meta-analyses if open data is not available.

Open Data and Materials

Many scholars have advocated the sharing of study data and materials and have argued that the long-term benefits outweigh the attributed short-term costs (LeBel, Campbell, & Loving, 2017). In fact, openly available information on a particular study could ease replications and extensions and might simultaneously reduce errors as other researchers are able to reproduce calculations and analyses (Miguel et al., 2014). In addition, we believe that it increases trust in the reliability of data and analyses.

Still, recent studies found rather low rates (38%) of researchers sharing their data (Vanpaemel, Vermorgen, Deriemaecker, & Storms, 2015). In a survey among 1329 scientists, lack of time and funding have been described as the major problems related to sharing of data and material (Tenopir et al., 2011). To promote an open research culture, the *Transparency and Openness Promotion Guidelines* (TOP; https://cos.io/our-services/top-guidelines/) have been drafted by the Center for Open Science (COS) to guide journals' decisions on the level of transparency for a number of aspects such as transparency of data, design and analysis, or research materials (Nosek et al., 2015). Open sharing of data, materials, and the research process has become straightforward as there exist hundreds of online data repositories, which differ in their focus and features (see http://www.opendoar.org for a directory on repositories). Finally, to increase transparency of data sharing, an increasing number of journals have introduced so-called Open Science Badge*s* to indicate availability of open data and materials, as well as preregistrations (Kidwell et al., 2016; Lindsay, 2017). An excellent overview of steps to ensure openness of research flow and data has been provided by O. Klein et al. (2018). They also provide a primer on how to implement transparent research management strategies and procedures.

Preregistration and Registered Reports

Preregistrations, the specification and recording of study protocols prior to conducting the study, have become standard for medical trials (Lenzer, Hoffman, Furberg, Ioannidis, & Grp, 2013), but are not the default in the social and behavioral sciences. However, in order to reduce the occurrence of publication bias, the systematically skewed publication of positive results (Dickersin, 1990; Rosenthal, 1979), and *p-hacking*, the flexibility in data analyses to obtain statistically significant findings (Simmons et al., 2011), scholars have called for the employment of preregistrations in the social sciences (Wagenmakers, Wetzels, Borsboom, van der Maas, & Kievit, 2012). Recently, a number of journals have adopted preregistration options and similarly journals publishing only preregistered studies have been launched (Chambers, 2013; Jonas & Cesario, 2016). In general, researchers have distinguished between two forms of preregistration (van't Veer & Giner-Sorolla, 2016):

reviewed preregistrations also called *registered reports* (Nosek & Lakens, 2014) and *unreviewed preregistrations*. For the first type researchers specify a study protocol including method, materials, and proposed analyses, which are then reviewed employing the traditional peer-reviewed system prior to conducting the study. If this preregistered protocol has been vetted, the final results will be published independently of the outcome given that the researchers have adhered to the protocol. In contrast, for the unreviewed preregistration type, the study protocol is not peer reviewed but registered by the researcher. Adopting preregistration increases transparency between a priori planned confirmatory analyses and post hoc exploratory analyses (Nosek, Ebersole, DeHaven, & Mellor, 2018). Contrary to some preconceived notions, preregistration does not restrict exploratory research if it is denoted as such. Many scholars have argued that preregistration is not only able to increase transparency but also to provide a more valid description of actual effects as it distinguishes between *prediction* and *postdiction* (Mellor & Nosek, 2018; Nosek et al., 2018; Wagenmakers et al., 2012). As registered reports are published independent of the outcome, researchers are not pressured to present positive results only. Nevertheless, preregistration is not always applicable to every study design, and most literature has focused on tailoring preregistrations for quantitative experimental research. In addition, envisioning every possible step and outcome of a study might pose issues and uncertainties. Therefore, van't Veer and Giner-Sorolla (2016) have drafted a template including a number of questions with regard to hypotheses, methods, and analyses plan. Moreover, a number of online services such as *AsPredicted* (aspredicted.org) or the *Open Science Foundation* (osf.io) have made it easy to efficiently preregister one's research using predefined templates.

Power and Accuracy

Accordingly, up-to-date psychological investigators are normally expected to include some preliminary calculations regarding power in designing their experiments. (Meehl, 1967, p. 107).

Already several decades ago Cohen (1962) has pointed out the importance of statistical power in the social and behavioral sciences (see also Greenwald, 1975; Ioannidis, 2005). Statistical power refers to the probability of rejecting the null hypothesis when it is false and is therefore only relevant in the context of hypothesis testing. While power is important for planning for rejection of the null hypothesis and explore the direction of an effect, precision or accuracy[1] is relevant for estimating the actual effect and its main goal lies in achieving a sufficiently narrow confidence interval (see Maxwell, Kelley, & Rausch, 2008 for a discussion). Power is dependent on the population effect size and the sample size (and the alpha level),

[1] Accuracy and precision often occur simultaneously. However, while precision refers to a narrow confidence interval, accuracy also provides information that this interval contains the true population value (see Kelley & Maxwell, 2003, 2008 for a discussion).

while accuracy is first and foremost depended on the sample size and the population variance. Thus, both approaches, power analysis and accuracy in parameter estimation, have two different goals, and depending on the context, planning for accuracy might sometimes result in larger sample size recommendations (Kelley & Rausch, 2006). Typically, a researcher wants to perform a power analysis or plan for accuracy before conducting a study, in order to get an idea of how many participants need to be recruited in order to achieve either a certain amount of power or a certain degree of accuracy. Levels of 80% are often discussed as appropriate (Cohen, 1988), though some journals have adopted policies requesting higher values (e.g., Jonas & Cesario, 2016). In general, it should be noted that recent discussions have primarily focused on power and "[...] researchers have not yet made precision a central part of their research planning" (Cumming, 2012, p. 355).

A number of attempts have been made to evaluate the mean amount of statistical power in the social and behavioral sciences. Estimates ranged from about 50% mean power in social-personality psychology (Fraley & Vazire, 2014) to about 35% for psychological research (Bakker et al., 2012) for a medium effect to a median of 21% (Button et al., 2013) or 12% (Szucs & Ioannidis, 2017) to detect small effects. Similarly, evidence has been presented that researchers have problems grasping statistical power and underestimate the sample size needed, especially for small effect sizes (Bakker, Hartgerink, Wicherts, & van der Maas, 2016), with similar findings showing misconceptions and ill-understanding of precision and confidence intervals (Belia, Fidler, Williams, & Cumming, 2005). However, at the same time research in the social sciences often targets primarily small to medium effects (Gignac & Szodorai, 2016). It is therefore not surprising that scholars have called for adequate a priori power analyses or simulations in order to be able to detect possible effects accurately (Anderson, Kelley, & Maxwell, 2017; Bakker et al., 2012; Maxwell, 2004).

In order to conduct a successful a priori power analysis, a researcher would need to know the population effect size, but of course this information can only be estimated. Sometimes researchers select a small, medium, or large effect size based on interpreting standards (Cohen, 1988) and their own estimation of the population effect. On other occasions researchers base the effect size on pilot studies or previous literature. However, Anderson et al. (2017) have warned against such practices, as such effects might often be overestimated due to publication bias and as pilot studies rely on particularly small samples (see also Albers & Lakens, 2018). They advocate the use of adjusting effect sizes for bias and uncertainty (https://designingexperiments.com/shiny-r-web-apps/). In addition, a number of online resources and software applications have been released in order to compute a priori power analyses. One straightforward application is G∗Power 3 including power analyses for the most common tests such as ANOVAs or *t*-tests (Faul et al., 2007; http://www.gpower.hhu.de/en.html). For more complex models such as multilevel regression models (https://jakewestfall.shinyapps.io/two_factor_power/; Judd et al., 2017) or mediation models (https://schoemanna.shinyapps.io/mc_power_med/; Schoemann et al., 2017), online applications exist. While these applications cover the most common models, scholars have advocated to use power simulations for more complex or uncommon situations (Maxwell et al., 2008). Power simulations are a great tool to understand the nature of statistical power and also help researchers to grasp that statistical power should better be understood as a function of various parameters and not an individual fixed value.

In contrast to planning for power, there exist only few possibilities to plan for accuracy. One possibility is the *MBESS* package (Kelley & Lai, 2016) in *R*, which includes accuracy routines for the most common statistical tests. Although the common conception that increasing sample size increases statistical power and accuracy is valid, there are many other aspects of a study that are able to improve power and accuracy such as the type of design (e.g., within subject designs) or the reliability of a measure (Maxwell et al., 2008). Finally, researchers have warned against calculating post hoc or observed power (Cumming, 2012; Hoenig & Heisey, 2001) as it is highly dependent on the obtained estimate and *p*-value and might be misleading.

Given that estimating the effect size is difficult in many cases before running the study, it is useful to think about statistical power in terms of sensitivity. While a power analysis indicates the likelihood of obtaining a significant effect for a given effect size and sample size, a sensitivity analysis outputs the smallest detectable effect size with a given likelihood and sample size. The *Journal of Experimental Social Psychology* (JESP) made such *sensitivity* analyses mandatory. Sensitivity analyses could offer a way around the misleading nature of reporting post hoc power and the issue that a priori power analyses are problematic if no information on a possible effect size outcome exists.

Accelerate Scientific Research

Replication

Many scientists agree that replication, the independent reproduction of a certain finding, constitutes a cornerstone and is vital for the progress of scientific research (e.g., Amir & Sharon, 1990; Lupia & Elman, 2014; Zwaan, Etz, Lucas, & Donnellan, 2018). Still, a systematic survey of published findings in psychology has found a rather low occurrence (~1%) of published replication attempts, although the tendency has been increasing during the last decades (Makel, Plucker, & Hegarty, 2012). It has been repeatedly argued that incentivizing replications can be a cost-effective way to improve the validity of scientific research in the social sciences (Koole & Lakens, 2012). Recently, several journals have become more open to publish replication articles. Replication initiatives, often including many labs, have been flourishing (e.g., R. A. Klein et al., 2014; Open Science Collaboration, 2015; Pashler & Wagenmakers, 2012). In general, researchers have distinguished between *close,* also sometimes called *direct*, and *conceptual* replications (Brandt et al., 2014; LeBel, McCarthy, Earp, Elson, & Vanpaemel, 2018; Zwaan et al., 2018). While close replications attempt to mimic as many aspects of the reference study as possible such as the measures, design, and population, conceptual replications often represent a broader attempt at replicating or extending a theory by using different designs or measures. However, it should be noted that, for example, a replication testing an effect that is thought to be universal across populations would be considered as *close* when differing on the target population from the reference study. Thus, the nature of the original effect or theory guides the interpretation of the replication attempt. Importantly, one should keep in mind that there exist no possible *exact* replications as original studies depend on the specific sample or historic constraints that are impossible to be mirrored – unless the original study and the replication are conducted simultaneously on the same population.

At the same time, replications have been met with criticism and skepticism. Some scholars have argued that replications are distractions for the field, too context specific, or rather a possibility for *unskilled* researchers to publish research (as discussed by Spellman, 2015). Zwaan et al. (2018) have gathered these arguments and responded to them arguing that replications are the only way of ensuring a valid accumulative science. Maxwell, Lau, and Howard (2015) have emphasized the importance of conducting several independent replication attempts as a single replication failure might rather reflect the biased nature of the original study (see also Francis, 2012). Researchers should thereby not conclude on the informativeness of an effect based on one single replication attempt, but rather accumulate evidence such as in the *Registered Replication Reports* format (Simons, Holcombe, & Spellman, 2014) that include many different independent labs to replicate one particular finding (e.g., Cheung et al., 2016; Wagenmakers, Beek, Dijkhoff, & Gronau, 2016). This point is driven home by looking at the reports of individual labs in the many-labs initiatives (e.g., Alogna et al., 2014). Even if an effect is judged to be significant on the basis of the total sample from all labs, it is quite common to see the majority, most, or even all individual labs to obtain nonsignificant results if their individual samples are small. Effects in psychology are often quite small, and large samples are needed to test them with sufficient statistical power. Researchers who are used to reading literatures characterized by publication bias have often misleading intuitions about what statistical power can be obtained with small samples testing small effects (Bakker et al., 2016). Similarly, researchers have warned against the so-called replication paradox, the fact that combining several studies with small sample sizes might decrease accuracy of the estimate (Nuijten et al., 2015). These authors advise to conduct only highly powered replication studies as one solution to address the paradox.

In order to aid researchers with performing replications, Brandt et al. (2014) have drafted a *Replication Recipe* including 36 questions that should be considered when reporting a replication attempt.

Methodological Techniques

Sequential Analyses

As discussed earlier, a priori power analyses are often complicated by the fact that the population effect size is unknown and can be in some cases only guessed or estimated inadequately by underpowered pilot or previous studies. One partial solution to this problem is the employment of *sequential analyses* (Lakens, 2014;

Schönbrodt et al., 2017). Sequential analyses allow the researcher to perform interim analyses during data collection. That is, a researcher might not have the feasible resources to collect all participants suggested by the power analysis and wants to check in between whether an effect of interest emerges or not. Importantly, researchers need to adjust their type I error or alpha level when conducting sequential analyses. Lakens (2014) also highlights the importance of preregistration in order to avoid researcher degrees of freedom. Imagine a researcher wants to test an effect of $d = 0.50$ and a power analysis suggested a final sample of 180 participants in order to achieve 92% power. However, the researcher is interested in analyzing the data, for example, after collecting 80 and 120 participants. For each interim analysis, an adjusted alpha level based on a linear spending function would be applied, as discussed by Lakens (2014). Although sequential analysis can be a fruitful way to save resources, it should be noted that effect size estimates from smaller studies often include wide confidence intervals and less accuracy than bigger samples. Thus, effect size estimates from small studies rarely represent the population parameters adequately as accuracy is rather low. Note that sequential analysis in a NHST framework is not without its difficulties; some have suggested using Bayesian sequential analysis – see, for instance, Schönbrodt et al. (2017).

How to Falsify a Hypothesis?

One inherent issue with null hypothesis significance testing (NHST) is the absence of information for the null hypothesis (see also Rozeboom, 1960). Typically, null hypotheses are rejected, but p-values do not provide information about how likely it is that the null hypothesis is true (Cohen, 1994). Researchers might conclude that the obtained or more extreme data are unlikely given that there is no effect, yet they cannot directly prove the absence of an effect. Still, reviewing published literature has indicated that nonsignificant results are often misinterpreted as providing evidence for the absence of an effect (Aczel et al., 2018). A possible solution for this problem provides the use of equivalence testing in a frequentist framework (Lakens, 2017) or Bayesian statistics (Dienes, 2014; Wagenmakers, 2007). Equivalence testing basically compares the obtained estimate to some lower and upper bound benchmark, or the *smallest effect size of interest*, set objectively or subjectively by the researcher (see Lakens et al., 2018 for a tutorial). Imagine a researcher obtaining an effect of $d = 0.50$. Using NHST this could be tested against the null hypothesis of $d = 0$ but would not provide an indication of the absence of an effect. The researcher could argue that an effect as small as ±0.30 would be of interest to her. Using equivalence testing the obtained effect could then be tested against the lower and upper bound. Based on this procedure the researcher could obtain four different outcomes. First, the effect is not statistically different from zero and is statistically equivalent (lower than the bounds of interest), which could result in the conclusion of the absence of an effect. Second, the effect could be statistically different from zero and statistically equivalent, which might be the case in high-powered studies especially.

Third, the effect could be statistically different from zero and statistically not equivalent, which would result in concluding the existence of the effect. Fourth, the effect might not be statistically different from zero and not statistically equivalent, which would result in an undetermined conclusion. Lakens (2017) has provided an *R* package and an easy-to-use spreadsheet (https://osf.io/q253c/) in order to perform equivalence calculations for a number of statistical tests. In addition Lakens et al. (2018) have presented a comprehensive tutorial (https://osf.io/qzupa/).

Bayesian Statistics

While equivalence testing is one approach used to provide evidence for the absence of an effect in a frequentist framework, Bayesian inference is another possibility (e.g., Dienes, 2014). In general, Bayesian inference takes into account the uncertainty of an effect before testing, which is typically represented as a probability distribution (also known as the *prior*). The prior distribution is then combined with the actual observed data (the *likelihood*), which results in a new probability distribution (the *posterior*). Which hypothesis is favored can among others be finally expressed by the so-called Bayes factor. Based on benchmarks, Bayes factors lower than 1/3 have been described as moderate support for the null hypothesis (Lee & Wagenmakers, 2014). It is beyond the scope of the chapter to give a theoretical and practical introduction to Bayesian inferences. Readers are thus advised to consult a recent special issue of *Psychonomic Bulletin & Review* (Vandekerckhove, Rouder, & Kruschke, 2018) that discusses the benefits and applications of Bayesian inferences in detail (see Etz, Gronau, Dablander, Edelsbrunner, & Baribault, 2018, for a reading list). Recent statistical packages have started to incorporate the calculation of Bayes factors. The free and open software JASP (https://jasp-stats.org) provides Bayesian calculation for the most classical statistical tests.

Mini Meta-analyses

While meta-analyses, the combination of effect sizes from several studies, are already a tool for reviewing published literature, it has been argued that researchers should start performing *internal* or so-called mini metas of their own studies (Goh et al., 2016), that is, combining effect sizes of conceptually similar studies in one paper to derive an overall meta-analytic effect. Goh and colleagues argue that such meta-analyses make the interpretation of findings easier, as sometimes effects are so small that they might go undetected in a single study, but not when combining several attempts. They could therefore help in providing more clear-cut conclusions. Furthermore, combining effects provides increased accuracy as the width of the confidence interval decreases for an increase in sample size. In addition, combining several small studies can be superior than interpreting such studies on its own due to low statistical power and accuracy. Goh and colleagues show that mini meta-analyses can be performed on as few as two different studies. They provide a spreadsheet for conducting such "mini metas " (https://osf.io/6tfh5/). Alternatively, readers are advised to use the *metafor* package in R (Viechtbauer, 2010).

Conclusion

We have discussed several ways to increase the value of scientific research based on reporting practices, ways to accelerate scientific research and methodological techniques. Many of the discussed practices or methods have been recommended and discussed in previous decades (e.g., Cohen, 1992; Dickersin, 1990; Elms, 1975; Kerr, 1998; Rosenthal, 1979; Sedlmeier & Gigerenzer, 1989). However, it seems that only recently the social sciences have started to take such considerations seriously (Moshontz et al., 2018; Munafò et al., 2017). We provide an overview of the reviewed recommendations and suggestions including helpful resources such as guiding templates or online applications in Table 9.1. Note that our summary is not at all exhaustive. We have presented the aspects that have been most prominent in recent discussions on reproducibility and recommendations for the informational value of research (Asendorpf et al., 2013; Lakens & Evers, 2014; Munafò et al., 2017).

The expectations that PhD students face in the social sciences have been increasing steadily. At the same time, the behavioral and social sciences have faced a substantial turmoil questioning the reproducibility of research findings and the way how researchers should perform research. In the present chapter, we have tried to provide guidance on possible ways how PhD students can make sense of this turmoil or "sail from the seas of chaos into the corridors of stability" (Lakens & Evers, 2014). We reviewed recent developments in the social sciences, provided information about tools and methods to evaluate the validity and quality of published research, and offered suggestions on different ways to enhance the informational value of one's own research by focusing on important aspects such as preregistrations, power and accuracy, effect sizes, open science, replications, and methodological techniques such as Bayesian inference. These tools and suggestions cannot replace a good theory or reliable and valid measurement tools, but they can be a first step towards more informative and reproducible social sciences.

Our suggestion to PhD students is to use these insights to evaluate the literatures they read, to produce evidence that satisfies their own standards, and to follow the methodological debates and developments in the field closely. What we were able to summarize here will surely have advanced considerably in a few years. Finally, whenever considering and debating methodology, remember that psychological science, as all scientific fields, is brought about by humans, and thus any change that occurs does so in a social and cooperative way. Many of the problems that accrued in psychology resulted from using the numbers of publications and the impact factors of journals as shortcuts to benchmark the quality of researcher and researchers, which often set the wrong incentives. It would be equally foolish to now confuse replicability indices with the quality of past work. Instead, the replication crisis is an opportunity to redefine what we consider to be a high quality of scientific output.

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Chapter 10
Explaining Social Phenomena: Emergence and Levels of Explanation

Henrik Skaug Sætra

> *The law of causality, I believe, like much that passes muster among philosophers, is a relic of a bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm.*
>
> (Russell, 1912)

Causality is a thorny issue in debates about explanation, and a quote from an introductory textbook on economics may serve as an example on how it is sometimes used. It states that "higher interest rates cause people to save more" (Lipsey & Chrystal, 2004, p. 15). How does higher interest rates *cause* this change in people? Exactly how are we to explain this?

For most people, and even for scientists and philosophers at times, there is actually a world out there. That a stone actually *is* what they perceive it to be is self-evident, and so is the fact that people around them both *exist* and function pretty much like themselves. They can easily describe these people and explain their constitutive characteristics. People drive their cars to work, and if questioned, they'll explain to us that their cars are able to do what they do because they put fuel – or electricity – in them, which is then used to create various reactions in the engine that propels the car forward or backwards. When asked why they go to work, they'll explain that they do so for various reasons, such as the need for money, their love of what they do, the importance of what they do, or perhaps just the fact that they want to get some time away from those that remain at home all day. The funny thing is, once we go deeper into the concept of *explanation* in the social sciences, there is little that remains self-evident.

In order to know how to explain, we must know what an explanation *is*. That we have various philosophies of explanation in the social sciences quickly becomes obvious when we have a look at the three chapters in this section. Explanation as a concept can be explored from various perspectives, and the authors clearly differ in their approach. The distinction between the *linguistic*, *ontological*, and *epistemological*

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questions of explanation is useful for understanding that some of the differences between the various contributions are more about what questions they ask than about fundamental disagreements (Brady, 2009, p. 1055).

A Guide to Causation, Catalysis, Reproducibility, and Informative Value

Malnes opens this section of the book with a guide to explanation for social scientists. However, when we move on to Valsiner's chapter, we quickly see that differing ideas about what constitutes a proper ground for explanation in the social sciences exist. Lastly, we get a glimpse into the state of affairs of much of the social sciences in Zickfeld and Schubert's (2019) chapter, who paint a picture of a scientific community in need of both direction, speed, and informative value.

An important topic is how the concept of *cause* and *causation* is used. The initial impression is that Malnes (2019) employs the idea of causation while Valsiner (2019) rejects it, but this impression might be based on the fact that they are not discussing the (exact) same concept. They both discuss the epistemological level and issues of what kind of explanations we can give and how we can come about them. Valsiner's (2019) contribution is the one that most explicitly discusses the ontological questions of what the social sciences are in fact studying, but these questions are also, at least implicitly, present in the other contributions.

Could it be that, for example, economics, political science, sociology, and psychology are too far removed from each other for them to have a common philosophy of explanation? This is akin to the development described by Pitt and Mischler, where the quest for a general theory of explanation is often abandoned as one starts to focus on individual sciences, as "the particulars of the various sciences called for different accounts of what constituted an adequate explanation in physics and biology as well as chemistry, etc." (Pitt & Mischler, 2017). After discussing the three contributions, I will briefly introduce two topics related to this section. The first is the movement towards a unification of the social sciences. This is the growth of traditional social sciences prefixed with *neuro*. The second topic is concerned with levels of explanation, and in particular the concept of *emergence*.

Explanation, Causality, and Responsiveness to Reasons

What do we *mean* when we use the term *explanation*? One may, for example, explain what a stone is by stating that it is a hard substance composed of some sort of mineral. This would be what Malnes calls a *constitutive* explanation. This is akin to common *definitions*, which lets us put into words the defining characteristics that separate one phenomenon from another. These explanations are useful, but the ones we are really interested in here are the ones that attempt to shed light on *why* something is, or happens. According to Thomas Hobbes everything is motion, so separating *being* from *action* is perhaps harder than it may at first appear (Hobbes, 1946).

Nevertheless, Malnes is interested in explaining *human action*, which takes us right to the social sciences and our current topic of explanation. *Etiological* explanations are the main subject of Malnes' chapter. While the search for *causes* might be part of an etiological explanation, Malnes prefers the term "giving a reason for" as the general description. However, a reason might mean a *cause*, as it is commonly defined as a "cause, explanation, or justification for an action or event" (Reason, 2018). Since *reason* can mean *cause* or *explanation*, and thus lead to a circular problem when understood as something that explains, we might be better off with the concept of giving *cause* for. *Reason* might of course also refer to human beings' capacity for reasoning and rationality, which we will return to later.

One form of etiological explanation is the one of Zickfeld and Schubert (2019) – the *statistical* explanation. Here, action is explained by correlations and regularity. This neo-Humean form of explanation aims at uncovering reasons for what we attempt to explain by examining similar events from the past in order to see which phenomena occur together, so that we may deduce causal relationships. We search for constant conjunctions of causes and effects (Brady, 2009, p. 1).

This form of explanation can provide us with accounts of correlations and symmetric connections. It cannot, however, easily explain the asymmetric connection, unless we examine how some connections where one variable *necessarily* come before another in time, and use temporal precedence in order to determine which factor is the cause and which is the effect (Brady, 2009, p. 1067–8).

This method lets us *discern* a causal connection, but Malnes says we need more. In addition to stating that some phenomenon is causally connected to the action we intend to explain, we need to *articulate* the causal connection. Here he refers to Elster (2015), who demands articulation of *causal mechanisms* and *informative value* from what are to be labelled explanations proper. Elster deviates from Malnes' use of the term *explanation* and states that *explanation* is necessarily *causal* (Elster, 2015, p. 1). This means that what Elster discusses is only a subset of the etiological explanations Malnes provides. Concerning the question of what an explanation *is*, we will shortly see that Valsiner would probably not agree with Elster on the necessity of causality in explanations.

Rational Choice and Methodological Individualism

If I were to explain why people save more when interest rates increase, how would I do that? Malnes suggests *responsiveness to reason* as a good way to start. Here we delve into the water of rational choice, and we assume that people act the way they do, because they had some conscious thoughts that lead them to do so. People are purposive, and consciously so, Malnes suggests.

But how are we to arrive at these reasons? I do not have access to other people's minds, but I do have access to my own. *Introspection*, and the assumption that we are reasonably similar, lets me uncover what reasons. I would have for acting in a certain way, and then hypothesize that these reasons are what causes other people to act.

A plausible reason why a particular person saves more with higher interest rates would be that it is now a more attractive way of maximizing one's profit. We could of course also speak of utility and make profit a part of a person's utility. If interest rates increase, it becomes more attractive than other ways of spending money, so I move some of my expenditure towards saving. When a large proportion of people do this, we get a net effect that let us state that increased interest rates cause people to save more. This is an explanation based on methodological individualism. Malnes, along with Elster, seems to be an adherent to this philosophy of explaining social phenomenon through the actions of the individuals involved.

Human action is the source of causation, and we explain human action by assuming that humans are responsive to reasons and then give our plausible conjectures regarding what these reasons are.

Criteria for Evaluating Explanations

A goal for Malnes is to provide criteria for evaluating explanations. If I were to propose two single-factor explanations for why people save more with higher interest rates – (a) people are profit maximizers that respond to such incentives and (b) a higher interest rate is taken as a sign of economic uncertainty, leading people to put their money in safer positions – how do we evaluate these explanations?

The first criterion is that the explanation must be supported by the facts, and the other that it must facilitate proper understanding of the action explained. Both explanations seem to facilitate understanding, and if we ask people, we might find support for both explanations.

But what about *causes* that aren't *reasons* in the way discussed by Malnes? If I am to explain why I am writing this chapter, I might venture an explanation based on the composition of the neurons in my brain, my personal history and experiences, and some chance encounter that led me to be in this position. I might myself *believe* that the reason is an instrumental consideration of career development combined with a genuine interest in explanations, but could factors below the conscious level be important causes for explaining why I am in this position, doing what I do? The unconscious is certainly a problem for rational choice, if the theory is supposed to fit with the facts *and* be a realistic, and not merely instrumentally useful, way of explaining behaviour. Elster occupies himself with the unconscious and clearly states that he is not convinced that rational choice alone takes us where we need to be (Elster, 2015, p. 188).

Malnes is concerned with separating *storytelling* from *plausible conjectures*. Finding plausible conjectures involves introspection, combined with what empirical evidence we can muster in support of the possible explanations. But what do we do, when we have an infinite number of plausible conjectures? An extreme claim is that "for any set of facts, there is an infinite number of explanations which are consistent with those facts" (Rugg & Petre, 2006, p. 40). How do we, then, choose from these? Luckily, there is also an infinite number of explanations that does *not* fit, so we can exclude a whole lot of possibilities (Rugg & Petre, 2006, p. 41). We must choose the *best one*, and this we usually do by finding out which ones fit the *best* with facts – which one has the best *neatness of fit* (Rugg & Petre, 2006, p. 41). We might "adopt the simplest explanation which maps on to the most facts most neatly", but then we are left with a debate about which one that is (Rugg & Petre, 2006, p. 41). In the end, we have some explanation that we consider the best, but it is nothing more than our theory, unproven. It is our best guess and a most plausible conjecture. The problem of underdetermination is very real when dealing with conscious reasons and evidence, but Occam's razor is one useful way of arriving at a limited set of plausible explanations (Næss, 1966, p. 177–8).

While social scientists are surely interested in explaining human behaviour, some would object to the idea that all can be explained through *individual* human action. While it might be *hypothetically* possible to explain all social phenomena through an analysis of individuals, it is practically impossible and thus insufficient. This is due to our lack of complete knowledge of the causal chains that takes us from individuals to complex social phenomenon, and such things as *emergence*. I return to the limits of methodological individualism and the limits of relying on conscious reasons as causes towards the end of this chapter.

Intentionality and Catalysis

> Philosophy of science is a cruel arbiter for empirical investigations in any science. When the first assumed axiom of a science is wrong the whole enterprise of a science built on it cannot be adequate. (Valsiner, 2019)

In the chapter that follows Malnes' guide, the very notion of causality comes under attack by Valsiner (2019). Social science is built on faulty first axioms. In addition to criticizing scientific discourse based on the notion of causality, he proposes an alternative – that of *catalysis.*

The focus is on the ontological issues of what the social world really is, and this seems like a sound starting point for discussing the philosophy of explanation in the social sciences. What the social world really is will, necessarily, determine what epistemological and methodological questions we must pose in order to arrive at a sensible philosophy of explanation. Valsiner does not explicitly discuss what an explanation *is*, and he does not define a cause or causation. However, we may explicate certain points relating to these two issues.

First of all, a causal explanation is described as a *discursive trick* that obfuscates a real understanding of how a phenomenon arise and occur. While he does not use the term explanation, it seems clear that Valsiner's goal is to prepare the ground for explaining social phenomenon through an understanding of catalytic processes. With regard to *causes*, he seems inclined to share Russell's opinion that the word *cause* "is so inextricably bound up with misleading associations as to make its complete extrusion from the philosophical vocabulary desirable" (Russell, 1912, p. 1).

Valsiner draws our attention to the history of philosophy of chemistry. There the idea of catalysis is said to have displaced the discourse of causality, and he proposes a similar move in the social sciences (Cabell & Valsiner, 2011; Valsiner, 2014, 2019). We are given various reasons for making the suggested move, and I'll attempt a brief summary before dealing with the notion of catalysis.

The first is that human beings are *intentional*. In this respect, Malnes and Valsiner share a focus on people's conscious ideas and purposive action. This means that the social sciences are different from the physical sciences, but not so different that we cannot learn from chemistry. He uses the example that atoms jumping from one orbit to another is different from a human being jumping from an airplane with a piece of fabric to ease his fall. Explaining the latter involves understanding a person's goal orientations, intentions, and future expectations (Valsiner, 2019). An atom, presumably, has no such cognitive processes for us to consider.

Malnes and Valsiner share the need to explain the *subjective* processes that give rise to various phenomena. Valsiner points to the role of subjective causal chains that lead from I *want*, I *will*, I *do*. In order to understand how a desire leads to action, we must understand the catalytic conditions that make the action both *possible* and *meaningful*. This, then is where we find the main difference in focus between the first two contributions: while Malnes is mostly focused on the reasons for our actions, Valsiner is intent on finding a way of describing the conditions in which reasons give way to various forms of action.

This is where we get to *social normativity*. Social norms and pressures affect our actions, and he relates Brinkman's example of a person rushing to the aid of a woman who dropped her bag of groceries (Brinkmann, 2016). The *reason* a person might help is not, for example, that that bag itself dropped, but the various norms relating to reciprocity, helping people in need etc.

This would be akin to Malnes' attempt to explain why a nail is in the wood by saying it was caused by it being hit with a hammer. While technically sufficient, it is very far from satisfactory, because it immediately becomes clear that there are further causes preceding the strike of the hammer. Who did this? Why?

Thomas Hobbes was once involved in a dispute involving an air pump and the concept of vacuum (Shapin & Schaffer, 1989). Of greater importance was Thomas Hobbes' insistence that philosophy had to have a *causal* agenda, in opposition to the experimental science performed by his opponent in this dispute, Robert Boyle of the Royal Society. One problem of causality is that it opens the door to the never-ending race for the *ultimate* causes. The hammer struck the nail, but why? Because a person picked it up and swung it. Why did the person do this? Perhaps he was paid to do so. Why was he paid to do so? And so it goes, on and on. Hobbes stated that the search for deeper causes stopped once he got to an "external cause", but this criterion seems not to cut it. Where *do* we stop? And this is part of the problem with the social sciences. Where do we stop when we attempt to trace the causal chains back to the ultimate causes? Recent developments in many social sciences involve a move very far back in the chain, assisted by neuroscience, and I return to this issue in the final section of the chapter.

Valsiner could be seen as having a dual agenda in his chapter: one positive agenda of explaining the ontology of the social world as the basis for including catalysis in our explanation of social phenomenon and one negative, where he criticizes science based on the concept of causality. I argue that the first undertaking is both important and successful, while the success of the criticism is less certain.

Problems Inherent in the Casual Approach

The problem with the causal approach is portrayed as (a) leading to a focus on simple causal connections, (b) problematic due to the impossibility of random sampling, and (c) suffering from an illusion of power leading to a misattribution of causality.

The first problem is the focus on simple causal connections and thus overlooking the systemic organization of social phenomena. This is a problem because all such phenomena have an "open-systemic nature" (Valsiner, 2019). Every phenomenon "from biology upwards" shares this trait, which makes the search for simple causal connections insufficient (Valsiner, 2019). Here one might object that even in open systems one can search for causal mechanisms, even if we are unable to *discover* their exact nature. Critical realism shares Valsiner's focus on open systems but still claims that experimentational and the search for constant conjunctions and the likes are of interest. However, these "[c]onstant conjunctions are produced not found" (Bhaskar, 1998, p. xii–xiii). Brady points towards mechanisms and the possibility of considering multiple causes in a traditional framework (Brady, 2009, p. 1083). Valsiner dismisses such attempts, as he claims that any endeavour that does not consider agency, and resistance, is doomed to fail.

Secondly, researchers in the traditional sciences are portrayed as beggars of data – unable to find what they desire in forms of random samples. They struggle to find these random samples, so that they can perform statistical generalizations and work their magic, but Valsiner claims that what they search for is *impossible* (Valsiner, 2019). This problem of causal science seems exaggerated. While there are certainly many examples of researchers doing bad science like Valsiner describes, it *is* possible to imagine research where this is not an issue. One possibility, in this age of big data, is to include everyone we are interested in in the "sample", and another possibility is to do sampling in a more serious manner than the one portrayed by Valsiner. At least in certain limited populations, it seems *possible* to do proper sampling from complete population lists. While it may be a problem in practice, it is not a universal or necessary problem with causality as such.

The final objection is the illusion of power and misattribution of causality. Here, again, the problem is one that *may* constitute a problem for researchers involved in explanations based on statistical methods. If, for example, *gender* or *socio-economic* *status* is examined as the independent variable, Valsiner's suggestion that scientists treat them as something that can *actually be changed* and experimented with is, again, more a criticism of bad *practice* than this form of science *in principle*.

Another objection that I will not cover in detail here is that Valsiner considers all social phenomena *non-linear*, while causality is portrayed as necessarily linear. If this is correct, the causal approach is obviously ill-suited for explaining social phenomena. It is, however, possible to suggest that many human inventions, also in the hard sciences such as mathematics, give good causal explanations of non-linear functions, fractals etc.

A Notion of Catalysis Compatible with Causality and Mechanism

The catalytic approach is concerned with the "recurrent reproduction of the system that produces outcomes", and outcomes are considered "by-products of the processes of such reproduction" (Valsiner, 2019). While Valsiner proposes that we discard causality as we know it and *replace* it with catalysis, it seems that a slightly less ambitious agenda might be compatible with his position.

I posit that *causality* is not the culprit that leads people to lose sight of the complexity of social phenomena. It is possible to maintain the notion of causal processes and simultaneously focus on the catalytic forces that *accelerate* such processes. In such an approach, we focus on the systems in which people live, and the various processes that make some actions possible and meaningful – the things that *facilitate* and *accelerate* action. Causality as such need not be discarded, because we will always require a description of the mechanisms that made an action ripe for acceleration – mechanisms that create what we could label potentialities. I want to point out that once again, like with the open systemic nature of social phenomena, Valsiner seems to be aligned with much of critical realism and the search for "aspects of reality that underpin, generate or facilitate the actual phenomena that we may (or may not) experience" (Bhaskar & Lawson, 1998).

At any point in time, I have the capacity for innumerable actions that are explainable by reasons and possible, but only some are *activated.* Understanding why this occurs is of great importance in social science, and as such Valsiner's contribution is important.

The example of paintings and music is discussed, and these are very apt illustrations. Music does not *cause* me to work faster, but I may *attribute* the speed of my work to the music I hear. As such, our attributions of causal factors are of little interest, if we assume that we are very often mistaken, and unable to identify the true causes. My ability to work, and my desire to do so, may be driven by both unconscious and conscious processes, but they may easily be *accelerated* by the catalytic force of music. A painting itself does not *cause* certain cognitive processes, but it can function as a catalytic force that accelerates certain processes and inhibit others.

What, then, is the *actual* causal chain? We rationalize, make explanations that make us appear responsive to reasons, and make explanations that we can understand. Such *accounts*, as I return to later, should never be the end of our search for causes, as they are often the consequence of a lack of understanding of the reasons we behave as we do, along with an incessant desire to feel that what we do actually has meaning, and is reasonable. Any of the myriad of possible reasons we may construct seems better to us than *nothing*. Any explanation that takes the subjective perception of causation to be the complete, and correct, account of the causes involved seems to be vulnerable to overlooking *true* underlying causes.

In sum, I applaud Valsiner's grand vision, but I ask if it may be possible to introduce some of the useful concepts of catalysis to social science *without* abandoning the notion of causality and cause. Firstly, catalysis does not necessarily require the complete abandonment of the notion of causality. Secondly, causality may also have a role in open system, as seen in, for example, critical realism. Finally, while the criticism of traditional social science is important, it reads more as a criticism of science done badly than of the particular form for science in itself. As such, Valsiner's objections to causal social science may just as well lead to causal social science being done in ways that circumvent these objections as to the its demise.

Replicability, Speed, and Informational Value

> The field of psychology has been undergoing a crisis and according change of practices and norms since 2011. Based on these developments, this chapter is a primer for evaluating prior research and producing informative and reproducible research. (Zickfeld & Schubert, 2019)

From a fairly traditional guide to explanation, through the call for catalysis, we arrive at the final contribution from Zickfeld and Schubert (2019). They start out with a description of a "crisis in psychology" – a crisis whose foundations are found in many other quantitative quarters of other social sciences as well. The authors do not provide a discussion of what explanation or causality is, but the chapter is based on an approach to explanation based on statistics. I will thus interpret their contribution as a discussion of the form of etiological explanation that Malnes (2019) labels *statistical* explanation, where discerning causal relationships is the main agenda. Causality is not mentioned, and the aim is to show how we can make our research more *reliable*. While Valsiner (2019) called for a new paradigm, these authors see the crisis as one that can be fixed from within – by being aware of the pitfalls of statistical methods and by using new statistical methods to verify and check the quality of our findings.

As Malnes put forth criteria for evaluating an explanation, Zickfeld and Schubert ask how we can evaluate published research. Criteria are hard to come by, they say, as they vary across "types of methods and contexts". The main argument in the paper, however, seems to be that *replicability* and *informational value* must be goals of science.

Informational Value of Science

Informational value is increased, they argue, by adjusting for publication bias and by checking the validity of our statistical results research (Zickfeld & Schubert, 2019). How we can achieve this is presented in detail in the chapter in question. They make a compelling case for the actions they propose, as they argue that, for example, pre-registering research alleviate the problems that led to the crisis. There are few arguments to be made *against* combatting bias and increasing validity, so I chose to focus on what *informational value* this methodology gives us.

Informational value and the possibility of *explaining* with statistical methods is what connects this chapter with the others. Statistical explanations, Malnes says, are based on correlations – on phenomena occurring together. He separates this from *causal* explanations. Statistical explanations (at most) lets us *discern* a causal connection, while *causal* explanations proper also articulates the causal connection. This involves a demand for an account of *how* something causes an effect, and one way of doing so is through causal mechanisms (Malnes, 2019).

Valsiner, as we have seen, attacked the statistical endeavour with some vengeance. The impossibility of random sampling, the focus on simple causal connections, and the illusion of power were put forth as good reasons to do other things than searching for constant conjunctions.

What, then, is the informational value of statistics? As one of Malnes' criteria for evaluating explanations involves veracity, statistics can surely be used to support *one* aspect of good explanations. The other, however, which involves making causal connections *intelligible*, requires something else. It might be the responsiveness to reason that Malnes proposes or perhaps the description of and insight gained from Valsiner's catalytic systems.

Acceleration of Science

Another subject of the chapter is the *acceleration* of science. This, they propose, is achieved by doing more replication. However, if researchers follow the advice of the authors, the result is surely not necessarily a speeding up of the accumulation of scientific knowledge? While the pace will be slower if more people control each other's work, we may agree with the authors that the long-term speed of a slow and steady pace will take us further – faster – than a rampant scramble for publications with little quality. The latter model takes us somewhere fast, but when we build our progress on shoddy work, we will at times be required to take quite a few steps back in order to recover our bearings. Valsiner calls philosophy of science a merciless arbiter of any undertaking, as errors in any initial enterprise have the potential to wreck everything built upon it. Aristotle had the same idea, when he stated that "[w]hen one begins with an initial error, it is inevitable that one should end badly" (Aristotle, 1995, p. 181).

Slow and steady acceleration, then, but one question remains. In what direction should we accelerate? Valsiner suggested a new direction, but Zickfeld and Schubert seem to ask us instead to first have another look at what we have and whether or not it can be fixed. While replicability is surely a good thing, it seems clear that more is needed in order to provide social explanations that have true informational value. Both Malnes and Valsiner had suggestions for taking us past statistics. The difference, though, is that Malnes considered statistical explanation a potential *part* of a good explanation, while the statistical endeavour seems to be one of Valsiner's main points of attack.

A Challenge from Below and Levels of Explanation

The three chapters in this section may at first seem to be incompatible, particularly because Valsiner explicitly rejects the notion of causality and calls for a move to catalysis instead. This might be taken as an indication that there is not *one* philosophy of explanation for the social sciences, but several. We may, however, be able to find a philosophy of explanation that includes much from all three contributions.

I argue that our inability to trace every phenomenon back to its first causes means that we need to rely on multiple levels of explanation. While it is possible to argue that this is due to the nature of the *social* – something not reducible to the natural sciences – it is also possible to claim that it is due to epistemological issues. One might believe that everything is reducible in theory, but that this theoretical possibility is of little practical interest.

Unifying the Social (and Natural) Sciences

Malnes refers to the political philosopher Thomas Hobbes, which is one of the earliest and most well-known proponents of the attempt to unify the natural and social sciences. Hobbes' agenda in *Leviathan* is to build a philosophy of the state by starting with the smallest possible building blocks. Mechanism, or even atomism, is Hobbes' result, as everything is matter – everything consists of the same materials – and everything adheres to the same rules.

An example of this method is how he first establishes the premise that everything in nature is *motion*. Later on, he employs this premise to explain why men are never satisfied, or at ease. *Felicity*, the best we can achieve in terms of well-being consist in continually achieving the things we desire. It does not, and cannot, consist of getting enough and settling down. Everything is motion, man and his mind also (Hobbes, 1946).

Some will say that this Hobbesian naturalistic ideal is manifested in various modern approaches to the social sciences. One obvious example is how "neuro" is prefixed to all the social sciences, in an attempt to move the explanation of the phenomena in question to a (far) lower level.

Churchland (1989) is a philosopher writing about neurophilosophy. As Hobbes, she sees matter as all there is, and the *mind* is nothing but the *brain*. Neuroscience, then, is of obvious interest, and the book is subtitled *Toward a unified science of the mind-brain*. In her search for a unified and reductionist science of mind, she found that "where one discipline ends and the other begins no longer matters" (Churchland, 1989, p. ix). What about the other social sciences?

Psychology is the discipline in which neuroscience has the strongest foothold. Neuropsychology is "the study of the relation between brain function and behaviour", where the causes of behaviour can be found in the (material) brain (Kolb & Whishaw, 1995). Cognitive neuroscience is a discipline concerned with explaining "cognitive processes in terms of brain-based mechanisms" and is thus less focused on behaviour – at least by definition (Ward, 2006). Watzl (2019) discusses the topic of *culture* vs. *biology* in the fourth chapter of this book and provides an important warning about the dangers of psychological essentialism and what he calls the *biology attraction*. While he is most concerned with the topic of biological *differences*, I mainly refer to neurological explanations of behaviour, and these may very well be compatible with Watzl's point that we should perhaps see species, and groups within species, more as statistical phenomena than as *essentially* different.

Perhaps the most famous classical case study from these sciences is the story of the railroad worker Phineas Gage. While working on a railroad in 1848, an accident led to a metal rod being launched through his skull. The rod was removed, Gage survived, but he had changed drastically. Recently, neuroscientists have reconstructed the case and suggest that he sustained injuries to his frontal lobes, areas that are important for "decision-making, planning, and social regulation of behaviour" (Ward, 2006, p. 331). Gage acted the way he did because of a particular trauma to his brain. The basic idea is that we act the way we do because of how our brains work – traumas or not.

One of the more well-known neuroscientists from a social science perspective is Antonio Damasio, who has written extensively on how *emotions* influence behaviour. This is a topic that is particularly interesting when examining the role of *reason* in guiding behaviour, and whether or not reason can even be considered as completely separate from the human emotional apparatus. He concludes that it cannot (Damasio, 1994, 2003, 2018).

But the neuro-prefix has gone further than psychology. Neuroeconomics is a discipline concerned with finding the "biological causes of our decisions" – and they focus on decisions that have economic consequences (Wilhelms & Reyna, 2015, p. xiii). I'd argue most decision can be argued to have economic consequences, so this discipline has a very wide scope. We also have discussion of neuro*politics*, neuro*sociology*, etc., these days, and the trend seems to suggest that we will get more of this development in time to come.

Before I move on to the *downside* of this development, I want to briefly return to the idea of *accounts* and how people furnish themselves with reasons for their own (and others' behaviour). Scott and Lyman (1968) provide a theory of an *account* – a "linguistic device employed whenever an action is subjected to valuative inquiry". Franks (2010) relays the results from split-brain research that is somewhat related to this concept. Some people have severed the connection between the left and the right side of their brain, and it is possible to give instructions to the right side of the brain (the side without language), that the left side (with language) does not have access to. When the right side is instructed to draw a dog, the left side does now know what is being drawn, until it becomes quite visible. The interesting part is that, when asked, the left side has no problem *explaining* why she is doing what she's doing. While convincing to the person herself, the explanations provided are mere fiction. In another experiment, the right side is told to laugh. It does, and when asked why, the *left* side invents a reason – even if there were none (as the reason was not given to be that she was instructed to) (Franks, 2010, p. 3). This shows why responsiveness to reasons cannot be seen as the sole explanation of behaviour, and why subjective perceptions of reasons are interesting, and can have consequences, but that they lead us astray when confused with *real* causes.

A final point to note, regarding these attempts, is the possibility that true explanations are beyond our understanding, or at least beyond the understanding of all but a select few experts. What will we then say about its informative value and its success according to Malnes' criteria? Explanations are to make phenomena intelligible, but how do we define this? If, say, quantum physics is what explains everything, it may be empirically correct, but will do little in order to make sense of things, for everyone apart from the physicists. If I am unable to grasp the explanation, I may judge it to be a *bad* explanation, even if it is entirely correct.

Emergence, Layers of Reality, and Levels of Explanation

One concept that is of great interest to social scientists, but that is not mentioned in the three preceding chapters, is *emergence.* Emergence is a description of the fact that some order we cannot predict arises from the interaction of building blocks we *do* understand (Barrow, 2007). This has implications for the question of whether or not *one* science will suffice. According to Barrow, "[n]ature seems to create a staircase of increasing complexity so that each significant upward step is not fully reducible to the steps below" (Barrow, 2007, p. 184). Emergence is often seen as the key to understanding the relationship between the individual and social phenomena. Gilbert and Troitzsch (2005, p. 11) illustrate this with Durkheim's claim that "social phenomena are external to individuals" and methodological individualists' outright denial of "society". Could it be that *emergence* could be the key to understanding this conflict?

Railsback and Grimm (2011, p. 10) explain emergence as the "system dynamics that arise from how the system's individual components interact with and respond to each other and their environment". Goldstein (1999, p. 49) defines emergence as "the arising of novel and coherent structures, patterns, and properties during the process of self-organization in complex systems". According to Gilbert and Troitzsch (2005), emergence is the process in which *new* objects arise at higher levels, due to interactions at a lower level; these new objects must require "new categories" of description that is not a (necessary) part of the description of lower-level agents (Gilbert & Troitzsch, 2005, p. 11). They use *temperature* as an example, as atoms have no temperature, but motions and interactions of atoms together create this emergent phenomenon (Gilbert & Troitzsch, 2005, p. 11). The emergence of temperature from the temperature-less atoms is, however, somewhat different from emergence of social phenomena; one of the defining aspects of humans is their reflexivity, which gives rise to what some label "second-order emergence" (Gilbert & Troitzsch, 2005, p. 11). Levin (1998) uses ecosystems as "prototypical examples" of the *complex adaptive systems* I'm here discussing. These are characterized by non-linearity which causes "historical dependency and multiple possible outcomes of dynamics" in addition to emergence on higher levels from interactions and mechanisms at lower levels (Levin, 1998, p. 431).

Sawyer (2004) argues for the ontological reality of "social properties" and states "that once social properties emerge, they have an ontological status distinct from their realizing mechanisms and may participate in causal relations"; he bases his argument on an "emergentist, systemist, and mechanist approach" (Sawyer, 2004, p. 261). One way of arguing the reality of emergent phenomena is attributing causal powers to these phenomena (Davidsen, 2010, p. 76; Goldstein, 1999, p. 60). Sawyer (2004) and Miller (2015) argue that *multi-agent systems* simulation is the best approach for studying complex phenomena with emergent properties (Miller, 2015, p. 179; Sawyer, 2004, p. 262).

Sawyer (2004, p. 266) is not content with the pure reductionist account of emergence and argues "that although only individuals exist, collectives possess emergent properties that are irreducibly complex and thus cannot be reduced to individual properties"; in this, he also refers to critical realism, with its *structured* and *stratified* conception of the social world.

It might be possible to find a "midway" position between methodological individualism and holism, in that individualists are wrong to ignore the independent power of emergent social properties, while holists are wrong to ignore individuals and the micro-level (Sawyer, 2004, p. 266–7). I here refer to the "strong" holist claim that systems cannot be explained simply by aggregating the parts, a view that "postulates new system properties and relations among subsystems that had no place in the system components; hence it calls for emergence, a 'creative' principle" that is contrary to mechanistic explanations (Simon, 1996, p. 171).

The concept of emergence is important, and I argue that it leads to the need for various *levels of explanation.* Short Jr (1998), p. 3 discusses the issue of levels and states that it "influences what we regard as important, the sorts of theories we construct, the research we do, and the social policies that are constructed to deal with crime and other social problems". The idea of levels is not new, but "consensus is lacking as to what these levels are and what it is that is being explained" (Short Jr, 1998, p. 3). Macro-level research may provide useful insight into emergent phenomenon, while micro-level research provides answer to *different* questions that are equally important (Short Jr, 1998, p. 28).

Gazzaniga provides an interesting account of the question of emergence, levels of analysis, and the growth of the neurosciences in his paper *Neuroscience and the correct level of explanation for understanding mind: An extraterrestrial roams through some neuroscience laboratories and concludes earthlings are not grasping how best to understand the mind–brain interface* (2010). Emergence has been known since John Stuart Mill, he states, but some modern scientists refuse to acknowledge the concept. He names neuroscientists as particularly resistant, as they "cling to the idea that an understanding of the elementary parts of the nervous system will explain how the brain does its magic to produce the psychological states we all enjoy" (Gazzaniga, 2010, p. 291). We may get some interesting insight from the micro-level, he states, but in order to understand human beings and the social world, we have to acknowledge that we often only have access to emergent phenomenon that must be examined at the macro-level (Gazzaniga, 2010, p. 292). (See also Smith and Franks (1999) for more on the topic of emergence, reduction, and levels of explanation.)

Philosophies of Explanation in the Social Sciences

In this chapter, we have seen the topic of explanation discussed from various perspectives. Malnes (2019) provided a guide to explanation, with a focus on etiological explanations. Valsiner (2019) provided us with an alternative and a focus on the conditions of human behaviour, before Zickfeld and Schubert (2019) gave some important insight into how to use statistical explanations.

One topic of great importance is that of reductionism and emergence – topics I have briefly introduced towards the end of this chapter. One interesting development in the social sciences is the attempt to use *neuroscience* as our basis of explanation. If successful, such an endeavour might lead to the unification of the social (and natural sciences), leaving us with various disciplines that all resort to the level of neurons when they are to explain the various phenomena they are interested in. Neuroscience provides great insight into why, for example, a strict reliance on subjective *reasons* and *introspection* is insufficient for explaining human action, but it may not take us all the way.

I argue that there are two reasons why we are far away from achieving such a goal. The first is that (a) we have a very limited understanding of the long causal chains that take us from the micro-level of the brain to the various behaviours and social phenomena in question, and (b) in nature, and in the social sciences, there are *emergent* phenomena that cannot be understood by only examining the micro-level. The various social sciences are, to a certain degree, occupied with various levels of human existence, and as such *can* be seen as complementary efforts based at different levels of man and the social.

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Part III
Social Normativity in Social Sciences

Chapter 11
Normativity in Psychology and the Social Sciences: Questions of Universality

Svend Brinkmann

Like every other scientific enterprise, the social sciences are normative activities. I consider this a truism. All sciences are normative in the sense that they strive for truth, validity, reliability, utility, or whatever normative value one claims to be constitutive of scientific practice. However, although it is a truism, it still needs to be emphasized, since quite a few social scientists and psychologists subscribe to the misguided positivist idea that the sciences deal exclusively with facts rather than normative values. In the first part of this chapter, I therefore rehearse the discussion about the relationship between facts and values at some length, and I argue that scientific practices, including those in psychology and the social sciences, are inescapably based on normative values.

A more controversial point, however, concerns not just the practice of scientific research, but the very subject matter targeted by scientific studies. In the second part of the chapter, I argue that the subject matter of psychology and the social sciences is itself constituted by normativity. Some sciences are trivially about normative matters – mathematics, logic, and law come to mind – while others are arguably about causal processes such as chemistry or physiology. The question is where to place those sciences that deal with acting, thinking, and feeling human beings, and I argue that they are rightly placed on the normative side. When human beings are considered as persons, and not, for example, as physical organisms or as clusters of molecules, then any scientific understanding of the lives of persons must begin by acknowledging the normative nature of their doings and sufferings.

Finally, the third part of the chapter considers whether there are dimensions of the social normativity of human life that are universal or if everything normative about social practices is culturally relative. My answer is that there is both a universal and pre-cultural normativity disclosed in the immediate encounter with vulnerable others

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(which the moral phenomenologist Levinas referred to as “the face”) and a universal social normativity inherent in human communicative processes as emphasized by philosophers such as Habermas (1993) and Holiday (1988). I illustrate this with reference to a key contribution to social anthropology, viz., Scheper-Hughes’ (1993) study of cultural practices of grief and bereavement, which at once throws light on cultural particulars while retaining a universalist approach to ethical normativity in line with Levinas.

The Fact-Value Dichotomy in Scientific Practice

Our contemporary discussions about facts and values owe much to David Hume. In his *Treatise of Human Nature* from 1736, he famously observed that an “ought” cannot be logically derived from an “is”: From “God is our Creator,” Hume says, we cannot logically infer that “we ought to obey him” (Hume, 1978, pp. 469–470). Hume’s argument greatly influenced positivists, empiricists, and many later researchers in psychology and the social sciences. Howard Kendler was a recent advocate for a Humean view with respect to psychology: He claimed that no normative moral truths can be derived from factual statements. This claim was based on two observations (the following reworks arguments previously articulated in Brinkmann, 2005): The first is “the failure of is to logically generate ought” (Kendler, 1999, p. 832). Kendler notes that in psychology, it is easy to conflate facts with values, but an “earnest desire to design, execute, and interpret a research project in a manner consistent with scientific objectivity will go far in achieving the desired goal” (p. 833). Kendler rightly notes that “Science is filled with value choices from those that encourage a person to become a scientist to those involved in choosing a criterion to determine the level of evidence that is required to prove a hypothesis” (Kendler, 2002, p. 491). However, he does not think that this has anything to do with the defining *contents* of psychological science. Science, as he says, “by itself, is incapable of converting empirical relationships into moral principles or social policies” (p. 491). Psychology is only competent to “estimate the consequences of different social policies” but cannot “identify the morally correct one” (p. 501). When psychology is concerned with social and applied matters, it must consequently confine itself to means and remain silent on ends, values, and goals, for there are no matters of fact about these to discover.

Kendler’s second main observation is what he calls moral pluralism. This simply means that “a shared moral conception is impossible to achieve” (Kendler, 1999, p. 834). Consequently, psychology is prevented from trying to define the contents of the good life in a pluralistic society: “Human cognitive ability in an open society resists any form of moral monism while simultaneously seeking to expand moral alternatives” (Kendler, 2002, p. 495). However, Kendler’s argument that psychology should not define the good life seems to be a value judgment (indeed a reasonable and correct one) that is entirely compatible with a pluralist conception of values and the good life. Kendler seems unjustified in concluding anything about the objectivity of normative values from his observation that people disagree about the contents of the good life. It may be, for example, that such diversity is morally valuable and that Kendler's argument that psychology should respect this diversity is in fact a good and, yes, objective, value judgment. At this point, we may already notice that it seems quite difficult and even impossible to escape value notions, for, paradoxically, Kendler can only defend his idea that psychology ought to be value-free by invoking values, e.g., objectivity and tolerance toward competing conceptions of the good life.

As indicated above, the contemporary idea of a fundamental dichotomy between facts and values is rooted in Hume's declaration that it is unwarranted to shift from talk of "is" to talk of "ought" in an argument. In Hume's case, it is quite clear that the fact-value dichotomy comes from his representationalist epistemology. What humans ultimately are in contact with, Hume argued (as an early exponent of representationalist psychology), are the mind's sense impressions and ideas. Impressions are similar to what the logical positivists were to call sense data in the early twentieth century, involving actual seeing, hearing, etc. Ideas are on Hume's account less "lively" than impressions and consist of thinking about something, rather than actually experiencing it. Concepts are a kind of idea, and they represent "matters of fact" simply by resembling them. Hume thus has a "pictorial semantics" (Putnam, 2002, p. 15). He thought that concepts succeed in representing the world in virtue of the pictorial properties of ideas. However, he also thought that ideas had other properties: Ideas can be associated with sentiments or emotions (positive as well as negative). And this is what valuation consists of, according to Hume: Valuations are simply sentiments aroused in us when we think about various courses of action. As Putnam says: "Hume does not just tell us that one cannot infer an 'ought' from an 'is'; he claims, more broadly, that there is no 'matter of fact' about right and no matter of fact about virtue" (p. 15). If there were such matters of fact about values, then they would have to be picturable in accordance with Hume's theory of meaning. Seeing that they are not so picturable, Hume is forced to conclude that there are no moral matters of fact. This theory was taken over by twentieth-century positivists and is retained today by defenders of the fact-value dichotomy in scientific practice, who argue that there simply are no facts about values.

The "facts" that are presupposed by Hume's fact-value dichotomy are thus facts about the subject's own sense impressions. The logical positivists originally argued that all factual statements are transformable into statements about immediate experience, expressed in reports of elementary sense data. This was Carnap's idea in *Der logische Aufbau der Welt* from 1928. According to the positivists, there were three kinds of statement: analytic statements (true or false in virtue of meanings, e.g., "all bachelors are unmarried"), synthetic statements (true or false in virtue of being empirically verifiable when confronted with sense data, e.g., "there are more than 5000 oak trees in Oxfordshire"), and cognitively meaningless statements, simply incapable of truth and falsehood (all ethical, aesthetic, and metaphysical judgments). However, the positivists could not have foreseen the developments in the physical sciences that were to come, proving the existence of "unobservables" such as atoms, electrons, gravitational fields, etc. It turned out to be futile and impossible to translate statements about these entities into sense data reports, and Carnap consequently revised his theory. On the revised account, all meaningful (synthetic) statements had to be cast in the language of physics, Carnap now argued (Putnam, 2002, p. 25). So statements about value still fell outside the sphere of the meaningful, since (1) they are not analytic, and (2) they are not intelligible when stated in the language of physics.

The general problem with the revised theory is that if all facts must ultimately be stated in (or be translatable into) the language of physics, then not only do moral, aesthetic, religious, and legal discourse fall outside the realm of the meaningful but also semantical discourse and in principle all normative statements! That is, if the only facts that exist are the facts of physics, then there cannot be facts about what words mean, about what humans say, about how we refer to objects in the world, and so on. Thus, in so far as a physical theory must be stated in a language, it cannot account for its own possibility, given that all facts are facts of physics (Putnam, 2002, p. 106). In this way, the attempt to reduce all statements of fact to the value-neutral language of physics simply undermines itself. In other words, if science is a normative activity, then a completely value-neutral science fails to account for its own possibility.

Furthermore, since Quine's famous demonstration that there is no absolute distinction between analytic and synthetic statements (Quine, 1951), philosophers have become aware that scientific statements cannot be neatly arranged into "empirical facts" (synthetic truths) on the one hand and "linguistic conventions" (analytic truths) on the other. According to Quine's holism, a scientific theory is tested as a whole against the world (including the theory's analytic and metaphysical presuppositions). For if we isolate specific hypotheses and submit them to verification/falsification, then, in the case of an anomaly, it is never clear if we should revise the hypothesis or make adjustments somewhere else in the system. No parts of the scientific system are in principle immune to revision, not even "analytic statements." If that is so, then the notion of an isolated "fact" becomes unclear. It is no longer clear what a singular "fact" is, if the meaning of any factual statement depends on a larger holistic web of meanings, theories, and interpretations.

Accordingly, Hume's distinction between matters of fact (the "pictorial" element of our ideas) and our sentiments toward those matters of fact ("values") breaks down, because ideas can no longer be understood as simply referring one by one to matters of fact. An idea is only the idea it is in virtue of a number of other things, including values (as we shall see). So, as Putnam had made clear, if the dichotomy between matters of fact (synthetic statements) and conventions (analytic statements) has collapsed, this ought to worry those who advocate a similar dichotomy between facts and values, for that dichotomy is dependent on the very notion of fact that has collapsed. At least, we can no longer work with a notion of facts as "untainted" by theories, conventions, and normative values. Indeed, as Putnam argued, scientific facts are only intelligible on the background of correct value judgments. A crucial part of scientific activity is concerned with judgments of coherence, plausibility, reasonableness, simplicity, validity, objectivity, utility, and even beauty (Putnam, 2002, p. 31). Mastering concepts such as these means to be able to apply them correctly to competing scientific hypotheses and theories (which is a rational thing to do if one has the relevant disciplinary knowledge). These concepts are not guided solely by an observer's subjective preferences, but by real, objective, and publicly identifiable properties of hypotheses and theories. And although these concepts pick out real features of the world, they are nonetheless inherently evaluative (since, all things being equal, it is normatively better to go for the more coherent, simple, and relevant theory over the theory less so). These concepts are thus both guided by real properties in their application and normatively action guiding. There is no way of separating the evaluative component from the descriptive component in their meanings (as Hume would have tried), for there simply are no purely descriptive equivalents that pick out the same properties as these concepts. We can only express or translate their meanings by invoking other evaluative concepts.

To anticipate a likely objection, if one came up with the counterargument that although "coherence," "simplicity," and "relevance" may be value concepts, they are not moral or ethical concepts, this would be beside the point (and quite likely wrong as well). This argument is not relevant because it does not address the issue that their application involves normative value judgments that are capable of being objective. What are sometimes called "epistemic values" are *also* values, and if one wants to argue that science should be free from these in order to be objective (itself a value term of course!), then one would have to argue that no value judgment can ever reach a level of be objectivity.

In this first part of the chapter, I have argued against the likes of Hume, the positivists, and Kendler (as a representative of modern mainstream psychology) that science cannot be value-free, because any form of scientific practice presupposes normative values. Thus, in order to arrive at *facts* about a domain of reality, one has to admit the existence of certain *values*. This is a very general point about scientific normativity, but what about the social sciences and psychology that not only presuppose values in the way they are practiced epistemically but also concern a subject matter that is infused with normativity as such?

The Normativity of Human Life

This is the key insight to be defended in this section: that any understanding of the phenomena of human life, i.e., our patterns of thinking, feeling, acting, perceiving, learning, etc., presupposes knowledge about values and norms. I have argued this point elsewhere (Brinkmann, 2006, 2016b) and will merely summarize the argument here (based, primarily, on Brinkmann, 2016b): Although much psychology conventionally presents the discipline as a causal science seeking to uncover laws of human behavior, the argument that psychological phenomena are normative, rather than causal, goes all the way back to Aristotle. Although he understood psychological phenomena such as thoughts, emotions, and motivations in terms of the natural sciences of his times, he did not think that they could be understood fully from this perspective alone. We also need the perspective of the "dialectician" (an equivalent to modern cultural psychologists) in order to grasp it (Robinson, 1989). For only the latter would rightly define, e.g., anger "as the appetite for returning pain for pain, or something like that, while the former would define it as a boiling of the blood" (Aristotle quoted in Robinson, 1989, p. 81). The dialecticians understand that anger (like any other psychological phenomenon) is never just a physiological or neurological happening (like a "boiling of the blood" or some modern neurophysiological equivalent), but always also something done or performed, which is why there is such a thing as justified anger in the face of preposterousness (and there is certainly also unjustified anger). What makes "boiling of the blood" *anger* (in addition to a mere physiological perturbation) is precisely that it is performed in a practical context where it makes sense to question, justify, and state the reason for "boiling of the blood." Anger is thus a psychological phenomenon in so far as it is a normative phenomenon that can be done more or less well and performed more or less correctly and therefore is subject to praise and blame. If anger belonged entirely to the realm of causal happenings, we should confine it to the science of physiology. As Harré (1983, p. 136) once noted, the reason why dread and anger are psychological phenomena (i.e., emotions) but not indigestion or exhaustion – although all have behavioral manifestations as well as fairly distinctive experiential qualities – is that only the former are normative and thus subject to praise and blame, because they belong to the moral orders of human cultures.

The Doing of Psychological Phenomena

We can sometimes say that some psychological process is clearly actively *done* – for example, when someone is trying to perform mathematical operations, which cannot meaningfully be said to happen to the person. But most of our psychological and emotional life lies in a grey area between doings and happenings: For example, we might feel that our grief (which is an emotion I return to below) *occurs* to us after a loss. We are overwhelmed by sadness and think of ourselves as victims or sufferers in such a situation. However, even this kind of emotion is not simply a mechanical reaction that happens to occur like an effect following a cause. Grief is also done or performed by skilled human actors, who can only *grieve properly* if they know their local moral order (Harré, 1983), i.e., know *how*, and *how much*, grief is called for in the social practices of their culture (Kofod & Brinkmann, 2017). This is not to say that grief is an action that can simply be stopped (like playing football with friends, which stops whenever the players become bored with the game or are leaving because of other appointments), but it is to say that grief is not conceivable as a simple mechanical *reaction*, but is rather a *response* to a loss. The loss is also not simply conceivable as a *cause* that mechanically triggers an emotion, but is a *reason* for feeling and expressing grief. This also explains why grief (like other emotions) may be evaluated morally: The person who does not grieve sufficiently is easily seen as shallow or aloof (whether justified or not), whereas the person who is experiencing extreme grief in a situation that does not call for deep mourning can be accused of "overdoing it" (Kofod, 2015). The (often implicit) evaluation of people's grief is directly linked with the various rituals that prescribe a grieving process around the world. In short, for psychological phenomena, there is an internal, normative relation between the reasons and the responses (rather than an external relation between a cause and a reaction). Fundamentally, it is only possible to understand some response as an instance of *grief*, if one acknowledges the normative reason afforded by the loss.

Although I have unfolded the point here with grief as an example, I could have referred to any kind of psychological phenomenon as illustration. All of them are normative in the sense of resting on distinctions between veridical and non-veridical (in the case of perception), logical and illogical (thinking), mature and immature (emotions), competent and incompetent (e.g., problem-solving), etc. That psychological phenomena are normative is not just an insight found in Aristotle and his modern successors (e.g., Harré, 1997), but is also argued in the phenomenological tradition of Husserl. Much of Husserl's work consisted of critiques of *psychologism*, i.e., the philosophical theory that logic can be explained with reference to how humans actually think and reason psychologically (in other words, that logic is founded on psychology). Husserl reacted against this, because it would mean reducing the *normativity* of logic to *causal* explanations of how the psychological system works. And, more generally, there was in Husserl's phenomenology an awareness of the normativity of our experience as such. Intentionality was a key concept in his work, which he took from Brentano. Famously, Brentano had argued that intentionality is the mark of the mental. This means that experience is always *about* something – our thoughts, feelings, perceptions, and actions are always directed at something. But Husserl understood that there is an inner connection between intentionality and normativity. One cannot have one without the other, so to speak, which means that if intentionality is the mark of the mental (which is commonly accepted), then the same goes for normativity (which is less commonly accepted). As Crowell (2009) puts it in his account of Husserl's phenomenology, "intentionality is not simply the static presence of a 'presentation' in a mental experience (*Erlebnis*) but a normatively oriented *claim to validity*" (p. 13). In colloquial terms, this means that what we experience (e.g., grief) can only intentionally be "about" something (e.g., a loss), because there are more and less correct and valid ways of experiencing it (normatively). To take a very simple example, we may see a dangerous snake in the forest, but – on closer scrutiny – it may turn out to be an innocent branch, and our intentional orientation toward the object involves a normative underpinning of trying to "get it right." Experience in general is not a passive happening, causally effectuated, but is rather a striving for normative correctness, as Husserl argued in his phenomenology. Mental life is centrally about understanding the world correctly, getting it right, not just with respect to perceptual processes but also concerning emotional understanding and action.

Sources of Universal Social Normativity

However, a key question then becomes: What are the sources of normativity in social and psychological life? Is all of it relative to the given cultural context or is there a kind of universal normativity in human life? As I see it, there are two valid approaches to universal moral normativity: one building on the track of ideas that began with Aristotle and was continued by later practice-oriented thinkers (MacIntyre, 1985) and also communication scholars such as Habermas and Holiday and another building on the phenomenological track of Husserl, as we have just seen, but especially on Løgstrup (1956) and Levinas (1969).

In a reflection on how cultural psychology ought to approach values (Brinkmann, 2016a), I began to explore these two tracks: the first one with reference to MacIntyre, who argued in his magnum opus on *After Virtue* (which rehabilitated Aristotle's philosophy in relation to ethics) that social practices are based on norms ("standards of excellence" as MacIntyre calls them) concerning what it means to perform a given practice well and therefore demand normative and teleological understanding (MacIntyre, 1985). Without standards of excellence, there cannot be any practice, because then there cannot be a distinction between better and worse ways of performing the given practice, which is a prerequisite for its existence, both in the here and now and as something projected into the future. For MacIntyre, practices are constituted by their normative standards of excellence or by what he calls their "internal goods." These are the goods participants achieve when they excel in the practice. His formal definition of a practice reads:

> any coherent and complex form of socially established cooperative human activity through which goods internal to that form of activity are realized in the course of trying to achieve those standards of excellence which are appropriate to, and partially definitive of, that form of activity, with the result that human powers to achieve excellence, and human conceptions of the ends and goods involved, are systematically extended. (MacIntyre, 1985, p. 187)

We see here how MacIntyre extends Aristotle's (1976) conception of praxis as something done for its own sake, rather than something done in order to achieve something else. Virtues, on MacIntyre's account, are then the human qualities that enable us to achieve practice-internal goods. Although MacIntyre does not himself try to provide arguments in favor of the normative universality of certain practice-internal values, this can be done by invoking the work of Habermas (1993) and Holiday (1988). The latter tried to show that there is a moral necessity inherent in the very practices of language and communication. Without the existence of what Holiday called "core language games" that function to preserve a set of basic moral values, language would lose its communicative force and hence its meaning. In his (much overlooked) work, Holiday identified three such core language games, viz., (1) truth-telling language games that aim to sustain adherence to the truth-telling norm, which "is not itself conventional, but the condition of there being any conventions whatsoever" (Holiday, 1988, p. 93), (2) justice language games, which preserve the distinctions between guilt and innocence, without which "it would not be possible to distinguish harm-attracting activities from safe ones" (p. 93), and (3) ritual language games that protect the integrity of speaking persons, which "cannot be done unless reverence for persons and their rights to speak and be listened to is a prevailing norm" (p. 109). In short, Holiday's argument goes against the view that all moral norms are socially constructed and culturally relative. Some of them might be, but if Holiday is right, we have to admit the intriguing point that there are objective (in the sense of nonconventional) moral values that make language and discourse possible. Some of these values may even concern norms for changing other norms, e.g., as seen in democratic societies where the social negotiation of governing norms is institutionalized in democratic practices. Core language games refer to those discursive practices that serve to preserve and sustain such essential moral values and also the complex social and psychological life made possible by them. Holiday makes this point with reference to the practice-oriented philosophy of Wittgenstein (1953), but it could also have been arrived at by asking (like Habermas, 1993) for transcendental conditions of communication in the Kantian tradition. The argument in favor of the existence of certain universal moral values does not preclude values from developing (e.g., justice comes in many forms), and it also does not prevent other values from being culturally contingent (as many values probably are). One may understand the moral values articulated through core language games as hinges upon which other values may turn.

In addition to this Aristotelian, practice-oriented track, there is also the phenomenological approach to universal normativity found in the works of Løgstrup (1956) and Levinas (1969). I have previously discussed the perspective of the former (Brinkmann, 2016a), so here I shall concentrate on the latter. The key to unlock the often dense and difficult work of Levinas is to understand how he builds upon, but also criticizes phenomenology, in the Husserlian tradition. The problem that Levinas saw in phenomenology and traditional philosophy more generally was that it reduced the other to the same (Levinas, 1969). The post-Husserlian phenomenology of Levinas was meant to respect the otherness of the other as an essential aspect of our experience and not make the other into something that has meaning only in relation to the experiencing individual. In Davis' helpful book on Levinas, he spells out the problem that he (Levinas) saw in Husserl's phenomenology where "consciousness can never meet anything truly alien to itself because the external world is a product of its own activity" (Davis, 1996, p. 19). And, positively about Levinas' contribution, Davis writes that what is at stake in his discussions of intentionality "is the ability of consciousness to encounter something other than itself. If meaning is entirely given by the subject rather than found in the world, then consciousness cannot experience, perceive or learn anything that it did not already contain" (p. 19). Against the philosophical tradition from Descartes to Husserl, Levinas worked toward a conception of subjectivity as "radically turned outwards, maintaining an openness to the non-self which is not subsumed under the categories of representation or knowledge" (p. 20). This was particularly important in relation to ethics, which, for Levinas, rests on the acknowledgement that the other is more than my image or representation of him or her. Ethically speaking, we must therefore not reduce the other to my representation of her. The reality of the other simply surpasses any image I may form of her. This, in a nutshell, is Levinas' great contribution to phenomenology, and it is noteworthy that the subtitle to his grand work on *Totality and Infinity* is "an essay on exteriority." Husserlian phenomenology, Levinas thought, did not take the exteriority of the other sufficiently into account or the otherness of the other.

Levinas is most famous for his concept of the face: This is the ground of ethics in his work, because it is in the encounter with the face of the other that one may understand that *the other is not like me*, on the one hand, but *also not against me*, on the other hand (Davis, 1996, p. 45). Phenomenologically, the face demands infinite responsibility and expresses a prohibition against harming and killing the other. And furthermore, for Levinas, my own subjectivity does not exist prior to my responsibility for the other, but emerges in my encounter with the other. Although it may be hard to grasp if one comes from standard, nonnormative philosophical perspectives, it means that our subjectivity is primordially ethical rather than epistemic or theoretical. Ethics should therefore, as Levinas famously claimed, be understood as "first philosophy." We cannot begin with a neutral metaphysics of the world or of human subjects, for our first understanding is always already constituted ethically. To put it in quite un-Levinasian terms, we can say that the world as we know it is normative and ethical "all the way down." There is nothing below ethics, nothing more primitive from which it emerges, in a subject's understanding of others and the world.

Thus, if Levinas is right, it is from the concrete other – and from one's own responsibility when faced with the other – that the most fundamental normativity in human life appears. Levinas' work has not just been taken up by other philosophers, but interestingly also by empirical researchers like the anthropologist Scheper-Hughes. In her thorough and deeply moving ethnography of life in the northeastern part of Brazil, where people struggle with poverty, hunger, and extreme child mortality, she documents how bereaved mothers, who lose their children, develop practices of mourning that are very different from those found in more affluent parts of the world where the death of children is a rare event. Actually, the mothers display a high degree of indifference when small children die. Scheper-Hughes has returned many times to the same shantytown in Brazil since the 1960s, and in the following passage, she describes an initial experience with infant death in the region:

> Within the first month of my arrival in Bom Jesus, a young mother came to me with a very sick and wasted baby. Seeing the child's condition was precarious, I rushed with him to the local hospital, where he died soon after, the desperate efforts of myself and two clinic attendants notwithstanding. I was devastated and frightened. […] How could I break the news to the child's mother? Would she hold me responsible for the death? Would I be forced to leave my post of duty so soon after my arrival? Selfish concerns, mind you. […] To my great wonder and perplexity, however, the young woman took the news and the bundle from my arms placidly, almost casually and indifferently. Noting my red eyes and tear-stained face, the woman turned to comment to a neighbor woman standing by, […] "Tsk! Tsk! Poor thing! Funny, isn't she?" What was funny or amusing seemed to be my inappropriate display of grief and my concern over a matter of so little consequence. (Scheper-Hughes, 1993, pp. 270–271)

She goes on to describe the funeral ceremony, which is very quotidian without anyone taking much notice, and, throughout the book, Scheper-Hughes comes to an understanding of the moral order of this poor part of Brazil. She learns to see the "apparent indifference of Alto mothers toward the deaths of some of their infants [as] but a pale reflection of the 'official' indifference of church and state to the plight of poor mothers and children" (Scheper-Hughes, 1993, p. 272). But still, in spite of these large cultural differences, and the difficult understanding of a seeming indifference in the face of dead children, Scheper-Hughes invokes the universal ethics of Levinas. Although the shantytown mothers seem to have "suspended the ethical" and their expected motherly love (p. 22), there is a logic to their ways of responding, and we probably need ethical reflection that transcends cultural contexts in order to approach the matter properly. At least, this is Scheper-Hughes' conclusion, and in Levinas she finds an ethics that "is always prior to culture because the ethical presupposes all sense and meaning and therefore makes culture possible" (pp. 22–23). Again, ethics is presented as "first philosophy" – as a pre-cultural condition for the existence of cultural life – rooted in the encounter with concrete others: "the ethical as I am defining it here," writes Scheper-Hughes, "is 'precultural' in that human existence always presupposes the presence of another. That I have been 'thrown' into human existence at all presupposes a given, moral relationship to an original (m)other and she to me" (p. 23). This is also why it makes sense to criticize the debilitating life conditions of people in this part of the world, because there is a pre-cultural source of moral normativity that one may invoke to fight for the alleviation of suffering in a world of "death without weeping" (to quote the title of her book). Scheper-Hughes' work nicely represents a cultural sensitivity that enables readers to understand the (non)responses of the bereaved mothers without judging them but which at the same time articulates a deep moral normativity that should make us wish for a change in the life conditions of the mothers.

Conclusions

We have come a long way from general and quite sterile philosophical discussions of the fact-value dichotomy and to bereaved mothers in the poorest parts of Brazil. What connects such diverse areas is the idea that value and normativity saturate all human experience and that value judgments are essential not only to everyday life but also to the practices of scientific psychology and social science. This does not threaten the objectivity of science, as I have argued, for in so far as value judgments can be objective in a sense, the scientific facts can be so as well. I first developed this argument with inspiration from Hilary Putnam, whose thesis boils down to this: "without values we would not have a world" (Putnam, 1990, p. 141). Without the capacity of humans being to make value judgments about better and worse, we could not know anything at all. I then went on to argue that not only the practice of studying and researching social and psychological life is a normative affair, but also the very subject matter of one's studies – viz., acting and suffering human beings – is saturated with normativity. In psychology and the social sciences, we do not study a value-neutral domain of reality that we then have different evaluative attitudes toward. No, we study a domain of reality that is normative at its very core. Actions, emotions, sufferings, and whole life courses are normative through and through, and if one tries to exorcise the relevant normative values from the field, then one blinds oneself to the nature of these phenomena. Finally, I tried to show that we need not be relativists concerning the basic moral normativity in human life. Both the Aristotelian tradition, which locates the source of normativity in the nature of social practices, and the phenomenological tradition, which locates the source in the encounter with a concrete, vulnerable other, give us reason to think of moral normativity as nonconventional and even pre-cultural. How to articulate and study this idea further after decades of social constructionist relativism is an interesting challenge but one I believe we need to face, if we want to develop our scientific efforts further in a direction that respects the phenomena that we deal with as human scientists, viz., fellow human beings.

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Chapter 12
The Crisis in Psychological Science and the Need for a Person-Oriented Approach

Lars-Gunnar Lundh

Edmund Husserl and Ludwig Wittgenstein were two of the leading philosophers during the twentieth century. They both played a central role in forming the landscape of present-day philosophy, laying the foundations of two of the main philosophical currents of our time: phenomenology and analytical philosophy. Husserl saw as his life mission to develop phenomenology as a "rigorous science," and Wittgenstein had a major influence on the development of analytical philosophy. Both had a strong interest in psychology, and both were highly critical of the turn that this new science had taken.

Husserl kept returning in his writings to what he saw as the "failure" of psychological science. In his last main work, *The Crisis of European Sciences and Transcendental Phenomenology*, written during the years before his death in 1938, he expressed strong admiration for the developments that had occurred in the sciences, including both the natural and the human sciences, with the exception of psychology:

> The scientific rigor of all these disciplines, the convincingness of their theoretical accomplishments, and their enduringly compelling successes are unquestionable. Only of psychology must we perhaps be less sure. (Husserl, 1938/1970, s. 4)

The history of psychology, he wrote, "is actually only a history of crises" (Husserl, 1970, p. 203). The basic reason for this, as he saw it, is that psychology has "let its task and method be set according to the model of the natural sciences" (p. 203), instead of building its own ground, based on an analysis of its basic concepts. The empirical psychology at that time, according to Husserl, was full of conceptual confusion, because it had neglected the importance of a phenomenological clarification of psychological concepts like perception, memory, phantasy, will, etc.

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It is interesting to note that Wittgenstein drew partly similar conclusions about psychology as a science. On the last page of his most well-known work, *Philosophical Investigations,* he expressed a strong skepticism toward scientific psychology in the form that he knew it:

> The confusion and barrenness of psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings… For in psychology there are experimental methods and *conceptual confusion*… The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by. (Wittgenstein, 1953, s. 232)

The purpose of the present chapter is to take these statements from Husserl and Wittgenstein as a starting point for some reflections on whether psychology today is in a state of crisis and if so how that crisis is to be characterized. After a general discussion of possible aspects of a crisis in psychological science, I will then discuss these questions specifically with regard to the area of psychotherapy research. The research paradigm that has dominated psychotherapy research during the last decades has focused on randomized controlled trials (RCTs) to provide evidence for the effects of various forms of psychotherapy. Although this kind of research has probably played an important role in defending the place of psychotherapy in the medical care system, it is utterly incapable of providing a scientific understanding of what makes psychotherapy work. It is suggested that what is needed for further progress in this area is the systematic development of a person-oriented approach to psychotherapy research.

The Crisis in Psychological Science

"Crisis" can mean several different things. The present discussion will first touch on what is discussed today as a *replicability crisis*. The main focus, however, is on what I call a *normativity crisis*, due to a social incentive system that is not conducive to scientific progress, and a *validity crisis*, due to a variable-oriented approach that is not suitable to the scientific problems that need to be solved. Whereas the normativity crisis requires a change in the social reinforcement contingencies operating in the scientific community, the validity crisis requires a change of paradigm from a variable-oriented to a more person-oriented approach.

This differentiation into three sorts of crises does not mean that these are independent types of crisis, but rather has to do with the level of depth of a possible crisis. Whereas the replicability crisis refers to the difficulty to replicate or reproduce previous findings at a purely empirical level, a normativity crisis raises deeper concerns over problematic conduct among researchers and problematic social incentives in the research community. The most severe crisis, however, is when the research methods that are used do not match the scientific problems that are to be solved – in this case, we have a validity crisis.

The Replicability Crisis

It has been widely suggested since the early 2010s that psychology is in a replicability crisis, in the sense that many published findings are difficult to replicate. To address this issue, 270 researchers (Open Science Collaboration, 2015) attempted to directly replicate 100 studies published in leading journals within social psychology and cognitive psychology; the original effects were successfully replicated in only 39% of the cases. A number of phenomena that have been assumed to be well-established (e.g., behavioral priming, ego depletion, the effect of facial expressions on emotions, etc.) have been questioned on the basis of such replication failures. Scientific progress requires that empirical findings can be reproduced by independent researchers, and if this is not the case, it poses a serious threat to psychology as a science.

Various explanations for these replication failures have been proposed. Some writers have argued that the replication failures have been due primarily to methodological factors, such as low statistical power (e.g., Maxwell, Lau, & Howard, 2015) or variation in study designs and measures (e.g., Stroebe & Strack, 2014). If so, they are due merely to methodological artifacts and not signs of any real crisis. (See also Zickfeld & Schubert, 2019—Chap. 9 in this volume.)

Others have argued that the phenomena under investigation are context sensitive, so that they may vary due to time, culture, location, and other contextual factors, and that this may help to explain replication failures (e.g., Van Bavel, Mende-Siedlecki, Brady, & Reinero, 2016). According to this reasoning, replication attempts in psychological research inevitably differ from the original studies, because the context is never the same. In other words, the effects are contextually bound, and an important research task is to find out *how they vary* as a function of differences in context. According to van Bavel et al. (2016), it is a:

> …"mechanistic" mistake in psychological research to assume that "manipulating one variable always and exclusively leads to a specific, deterministic change in another, precluding the possibility of contextual influence." (p. 6457)

McGuire (2004) even went so far as to argue that the purpose of scientific research is not to *test* hypotheses, but to study *under which circumstances* a hypothesis is true, and under which circumstances, it is false. If contextual influences of this kind are at work, this may be a contributing factor to replication failures, without necessarily indicating the presence of some kind of *replicability* crisis (but possibly a validity crisis). More generally, if psychological functioning essentially involves processes in open systems which are profoundly context-bound (Valsiner, 2017), then contextual influences will be ubiquitous in psychology, and we should not expect psychological science to be characterized by any simple kind of replicability.

Other researchers, however, have put forward explanations that imply the existence of a real replicability crisis. Some of these explanations emphasize the role of questionable research practices (QRPs) which tend to produce false positives (e.g., John, Loewenstein, & Prelec, 2012; Simmons, Nelson, & Simonsohn, 2011), and others emphasize the social organization of the world of science (e.g., Romero, 2016). Importantly, these explanations imply that there is not only a replicability crisis but also some kind of crisis in the norms that characterize the research community.

A Normativity Crisis

A normativity crisis, as defined here, means that the research community functions according to *norms that are not conducive to scientific progress*. One example is when the norms at work tend to generate the production of "false positives," that is, concluding that an effect or association exists when it actually does not. Especially detrimental to further progress is when such false positives nourish mistaken beliefs among researchers and in this way turn research into a wrong direction. If this occurs systematically, it may eventually lead people to lose confidence in an entire area of research when such false positives are exposed. As summarized by Simmons et al. (2011):

> First, once they appear in the literature, false positives are particularly persistent. Because null results have many possible causes, failures to replicate previous findings are never conclusive. Furthermore, because it is uncommon for prestigious journals to publish null findings or exact replications, researchers have little incentive to even attempt them. Second, false positives waste resources: They inspire investment in fruitless research programs and can lead to ineffective policy changes. Finally, a field known for publishing false positives risks losing its credibility. (Simmons et al., 2011, p. 1359)

The main culprit, in these authors' analyses, is what they refer to as *researcher degrees of freedom*. During the research process, researchers have to make a large number of decisions concerning data collection and data analysis that are often impractical to make beforehand; as a result, "it is common (and accepted practice) for researchers to explore various analytic alternatives, to search for a combination that yields 'statistical significance,' and to then report only what 'worked'" (Simmons et al., 2011, p. 1359). The effect is a *selection* of those results that pass a certain threshold (e.g., a p value of <0.05) for what might be seen as "statistically significant," and a *disregard* for results that do not pass that threshold, without any correction for the large number of statistical calculations that have been made. These kinds of practices have been variously referred to as "fishing," "p-hacking," "data mining," and "data dredging." John et al. (2012) administered a survey to over 2000 academic psychologists, asking about their use of these kinds of questionable research practices (QRPs), and found that the percentage of respondents who had engaged in them was "surprisingly high" (p. 524). For example, the majority of research psychologists admitted to having engaged in practices such as a selective reporting of studies, a selective reporting of measures, and a description of unexpected findings as having been predicted; in addition, nearly one in ten admitted to having introduced false data into the scientific record.

The Social Incentive System in the Scientific Community These QRPs should be seen in the context of the social incentive system that is at work in the research community. As Simmons et al. (2011) commented, "because it is uncommon for prestigious journals to publish null findings or exact replications, researchers have little incentive to even attempt them" (p. 1359). In view of the fact that publication can mean life or death to a researcher's career ("publish or perish," as it is commonly called), it is easy to understand why researchers may not even consider publishing their nonsignificant findings, once they have understood that it is difficult to get such results published. Why take precious time to write down a manuscript when the chances of having it published are almost nil?

Another example of *publication bias* is that even when a study *is* published, the journal may want the authors to shorten their paper by removing results that are judged to be of less interest – for example, results that did not reach statistical significance. One reason for this is the competition for the limited space in scientific journals, which are run by commercially based publishers and therefore prioritize results with a news value. In this situation, most researchers will probably follow the suggested revision to get their studies published.

These forms of publication bias mean that the published research is not representative for the actual research findings and raise the question to what extent we may actually trust the research findings that are published in scientific journals. This is a well-recognized phenomenon in meta-analysis, where various methods have been developed to detect the likely presence of publication bias and to control for it (e.g., Cooper, Hedges, & Valentine, 2009). Without effective methods to counteract publication bias, the validity of the conclusions drawn from meta-analyses will be uncertain.

Another aspect of this social incentive system is what Lilienfeld (2017) refers to as *the grant culture*, with its "emphasis on external funding as an expectation or de facto requirement for faculty tenure and promotion" (p. 660), in a way that rewards the ability to raise money rather than actual scientific achievements:

> Faculty members routinely receive plaudits for receiving grants but frequently find that their scholarly accomplishments go largely unnoticed. These reinforcement contingencies should strike us as odd for several reasons. First, we do not laud novelists or film producers for securing large contracts for their planned projects, nor should we. Instead, we rightly acclaim them if and when they have generated high-quality artistic work. (Lilienfeld, 2017, p. 661)

If there had been a robust empirical association between the ability to get funding and the quality of the resulting research, this kind of social incentive system might be defendable. However, as Lilienfeld points out, "[t]he correlation between grant funding and citation impact in psychology is low and perhaps essentially zero" (p. 661). One possibility is that the ability to raise grants requires other kinds of values, attitudes, and skills (e.g., social intelligence skills to find out "where the money is" and how to get them) than those involved in scientific curiosity, creativity, and productivity. In fact, we may have to do with two different ways of thinking and writing that need not go together (one intended to get the desired social response and the other intended to arrive at new scientific insights) and where the scientific community primarily rewards the former.

According to Lilienfeld, the grant culture poses a number of hazards for psychological science. For example, it provides clear incentives for engaging in questionable research practices (QRPs): "the lure of grant dollars and the fear of losing them induce powerful incentives to detect positive results by means of *p* hacking, outcome reporting bias, and other QRPs" (p. 662). Although preregistration of hypotheses and analytic plans may help to solve this particular problem, the grant culture also has other consequences that cannot be solved in the same way. One such problem is that it rewards "intellectual hyperspecialization" by canalizing scholars "into specialized lines of thinking for years or decades" (p. 662). Another problem is that it tends to stifle creativity and intellectual risk-taking while reinforcing "conformity to today's 'hot' topics, which may not be tomorrow's hot topics" (p. 663):

> More broadly, the grant culture has almost certainly led many scholars to abandon daring lines of work that are less fundable and to pursue safe lines of work that are more fundable. The same reinforcement contingencies operate for methodologies as well. In much of psychology, functional neuroimaging is now all the rage, and survey data suggest that many investigators feel pressured to incorporate neuroimaging and other biological techniques into grant proposals. (Lilienfeld, 2017, p. 663)

Under the contingencies of reinforcement that are operative in this kind of grant culture, a socially intelligent strategy is to find out which researchers form part of the relevant review panel and what kind of research they value the most and then let one's research proposal take form accordingly.

How to Solve the Normativity Crisis? A normativity crisis may in principle be solved by means of self-correction with regard to questionable research practices and/or a reform of the social incentive system. For example, it has been argued that the problem of publication bias may be solved by a more widespread use of preregistration, so that researchers register their studies, including research designs, hypotheses, and planned analyses, before the research is carried out (e.g., Lindsay, Simons, & Lilienfeld, 2016). The development of electronic journals, with more space for the publication of nonsignificant findings, may also contribute to a solution. Nosek and Bar-Anan (2012) have argued generally for a more open scientific communication, including a full embrace of digital communication, and an open access to all published research (see also Wenaas, 2019—Chap. 13 in this volume). With regard to the grant culture, Lilienfeld (2017) calls for "a thoroughgoing and intellectually honest conversation regarding the negative impact of funding on scientific progress and on potential remedies for diminishing this impact" (p. 663). It remains to be seen, however, if an honest conversation of this kind can lead to a change in the administrative evaluation processes of academic institutions.

A Validity Crisis

A validity crisis is difficult to solve, because it may require a change of basic assumptions related to the research methods that are used. A validity crisis is what would be the case, for example, if Wittgenstein (1953) were correct in his statement about experimental psychological research that "problem and method pass one another by." If the methods do not match the problem, it will be difficult to draw valid conclusions from the results, and then we have a validity crisis. This, of course, is the most serious form of crisis, because it may lead us to question entire research areas, no matter how replicable the results are, or how well the research community has been able to avoid questionable research practices.

A real validity crisis calls for some kind of basic change in conceptual frameworks and basic methodological assumptions. In Kuhn's (1962) philosophy of science, this is described in terms of a change of *paradigm*: A crisis develops when a dominant research paradigm is unable to solve particularly worrying problems, and this leads to an accumulation of *anomalies*. Such anomalies tend to be ignored or explained away as long as possible, but when a number of sufficiently troublesome anomalies have been accumulated, it will be difficult to continue normal science with confidence unless these anomalies are addressed.

A number of researchers have argued that there is a *mismatch* between the methods that dominate present-day psychological research and the nature of human psychological functioning. One type of arguments along these lines, which has already been mentioned briefly, is that psychological phenomena are context sensitive (i.e., may vary due to time, location, situation, culture, etc.) in a way that goes against a mechanistic view of psychological processes.

Another partly similar argument is that dominant research methods, with their focus on *single variables* and their association *at a group level* (i.e., a variable-oriented approach), fail to take into account that the individual person functions according to holistic and interactionistic principles (e.g., Bergman & Andersson, 2010; Magnusson, 1999). According to this reasoning, only the integrated *individual*, and not variables, remains distinct and identifiable over time. The individual should therefore be studied as an integrated indivisible whole; and because developmental processes are idiosyncratic, they must be studied at the individual level. At the same time, research methods also have to take into account that each individual is in constant interaction with the environment and consists of a number of subsystems, systems that interact at various levels. As argued by Magnusson (1999), the neglect of these holistic-interactionistic characteristics of human psychological functioning has led to a fragmentation of research that has had a hampering effect on real progress in psychology as a scientific discipline. There is consequently need for a more *person-oriented* approach.

This critique may be worth discussing with regard to any area of psychological science – and possibly also with regard to other social sciences. In the next section, I will choose to discuss it specifically with regard to one specific area of clinical psychology: psychotherapy research. Are there signs in this research area of a replicability crisis, a normativity crisis, and/or a validity crisis? And what would a person-oriented approach to psychotherapy research look like in this area?

Crisis in Psychotherapy Research

Psychotherapy research during the last decades has focused on demonstrating empirical evidence for specific treatments of specific forms of psychopathology, usually defined in terms of the DSM diagnostic system (e.g., American Psychiatric

Association, 2013). This movement toward evidence-based psychotherapy is based on the assumption that psychotherapy should be evaluated in the same way as medical treatments, by means of randomized controlled trials (RCTs), so that each form of treatment is compared with a control condition, and the participants are randomly assigned to either treatment or control group. After some decades of such research, a number of treatments have been identified as “evidence-based” and have accordingly been included in officially sanctioned guidelines for how to treat various psychiatric conditions. It has also led to the general conclusion that psychotherapy works. But one main problem is that it has not led to any real breakthrough in the understanding of *what* makes psychotherapy work.

Despite the fact that a large number of psychological treatments are now seen as effective, our knowledge about what *causes* these effects is still quite limited. As summarized by the behavior therapist and research methodologist Alan Kazdin, author of classical handbooks of research design in clinical psychology:

> after decades of psychotherapy research we cannot provide an evidence-based explanation for how or why even our most well-studied interventions produce change, that is, the mechanism(s) through which they operate. (Kazdin, 2007, p. 1)

In other words, although RCTs may tell us *that* a treatment works, they have not been able to contribute to knowledge about the mechanisms involved or knowledge about the therapist skills needed to conduct successful psychotherapy. Although this does not necessarily mean that psychotherapy research is in a crisis, it is a clear signal that scientific progress in this field does not proceed in a satisfactory way.

A Replicability Crisis?

There has not been much talk about a replicability crisis in psychotherapy research. As described by Tackett et al. (2017), clinical psychology has “remained insulated from this discussion” (p. 742). Nevertheless, replication problems have figured prominently in some critical analyses of psychotherapy research. For example, Luborsky et al. (1999) pointed out that when two different forms of psychotherapy have been compared (e.g., a behavioral treatment vs. a cognitive treatment or a cognitive treatment vs. a psychodynamic one) and one of them has been found to be more effective, these results have been difficult to replicate by other researchers. Other researchers, in fact, have often even reported the opposite results. Moreover, in a review of 29 studies of such treatment comparisons, Luborsky et al. (1999) found that the effect sizes showed a very strong correlation (r = 0.85) with researcher *allegiance*, that is, the researcher’s preference for a particular treatment. That is, the researchers systematically tended to find more support for the forms of psychotherapy that they believed in. (See also Reber & Bullot, 2019—Chap. 5 in this volume.)

Among the possible explanations for this that were discussed by the authors, two are of special interest here: (1) A researcher’s theoretical allegiance may lead to the "selection of a less effective competing treatment to compare with their favored treatment" (Luborsky et al., 1999, p. 101); and (2) when the outcomes of a study run counter to the researcher's allegiance, there is less motivation to get it published. Both of these possible explanations suggest a normativity crisis. This is also reflected in the possible remedies suggested by the authors, which all refer to various kinds of reform in research practices, such as including a mix of researchers with different therapy allegiances in each study and correcting the results for the impact of researchers' allegiances when analyzing the results.

A Normativity Crisis?

According to the logic of the RCT design, in combination with common criteria for judging what counts as evidence, the best way to provide evidence for a particular form of psychotherapy is to compare it with a suitable control condition that *is not too effective*. In research on pharmacological treatments, this problem does not arise, because here the rule is to use a double-blind condition where the control group receives a placebo, and neither therapist nor patient is aware of which patient receives what. But double-blind designs are not an option in psychotherapy research, because it is not possible to construct a design where neither patient nor therapist knows whether the patient gets psychotherapy or not. For this purpose, a number of more or less "artificial" control conditions have been used by psychotherapy researchers, on the rationale that they should include some presumably inactive elements (e.g., attentive listening and the use of some kind of credible procedure) but none of the presumably active elements of the treatment.

Psychotherapy research with RCT designs has clearly showed that various forms of psychotherapy produce better results than such artificially constructed control conditions. However, as argued by Wampold et al. (1997), when two or more forms of bona fide psychotherapy (i.e., "real" therapies that are described in books and manuals and delivered by trained therapists) are directly compared in an RCT, there is no strong evidence that any one of them produces better effects than any other. And, as already described in the previous section, *when* such differential effects are found in a study, they are difficult to replicate by other research groups and tend to correlate with researcher allegiance.

In a meta-analysis of 79 direct comparisons between different treatments, Munder, Gerger, Trelle, and Barth (2011) reported evidence that researcher allegiance was more strongly associated with outcome when the methodological quality of the study was low. They therefore suggested that researcher allegiance may lead to methodological weaknesses in the control conditions and thereby cause biased results. For example, researcher enthusiasm for one particular treatment may lead to different levels in the therapists' commitment to the two treatments that are compared and differences in how well the two treatments are implemented. Munder et al. (2011) also found that differences in the conceptual quality of the treatments (defined in terms of Wampold et al.'s (1997) criteria for bona fide psychotherapy) mediated the allegiance-outcome associations – that is, researchers with a clear preference for one treatment were more likely to choose a less credible comparative treatment as control condition than researchers with more balanced preferences.

This may be understood against the background of social factors at work in the psychotherapy field. This field contains a number of competing forms of psychotherapy (e.g., psychodynamic therapy, cognitive behavior therapy [CBT], family therapy, humanistic-experiential therapies, etc.), which are based on different theories and even "world views" that therapists and researchers tend to identify with more or less strongly. This means, for example, that for strong proponents of any such orientation, it can become a question of "life or death" to provide the necessary evidence for the particular form of psychotherapy they believe in and which they may have invested in to earn their living. This also means that the discussion climate here is often more reminiscent of debates between different political camps (or even religious faiths) than an expression of scientific knowledge interests.

One possibility is that the primary importance of RCT studies in psychotherapy research is that they have helped psychotherapy survive in a partly hostile environment where biological psychiatry was threatening to take over. Fears have been expressed that psychotherapy is at the risk of being eliminated as an available treatment option within public health care and psychiatry, if it is not able to show the same kind of evidence as pharmacological treatments. RCT studies have clearly established that psychotherapy is effective. For example, on the basis of around 500 RCTs that have examined the effects of psychological treatments of adult depression during four decades, Cuijpers (2017, p. 7) concluded that "psychotherapies are about equally effective as pharmacotherapy, and combined treatments are more effective than either of these alone." With regard to the comparison between different forms of psychotherapy, he concluded that all therapies that have been tested (i.e., various forms of CBT, short-term psychodynamic therapy, interpersonal therapy, and nondirective supportive therapy) "are effective and there are no significant differences between treatments" (p. 7).

In parallel to the expansion of psychotherapy research during the last decades, the landscape of psychotherapy has changed dramatically. From being dominated by psychodynamic forms of therapy, the field of psychotherapy has become increasingly dominated by various forms of cognitive behavior therapy (CBT). One reason for this is that *much more* research has been done on CBT than on other forms of psychotherapy – not that it has been compared with other forms of psychotherapy and found to be more effective.

On the basis of this research, various forms of CBT are now regarded as evidence-based and seen as the treatment of choice for a variety of psychiatric disorders. In particular with regard to anxiety disorders, CBT treatments have achieved a success that has not been matched by any other form of psychotherapy. At the same time, it is important to note that this advantage in favor of CBT is *not* due to studies that have compared CBT with other forms of psychotherapy and found that CBT is more effective. The reason that CBT has a large advantage here is that a large number of published studies show CBT to be effective in all areas of anxiety, whereas there are few similar studies of psychodynamic therapy and an entire lack of such studies for some anxiety disorders (e.g., obsessive-compulsive disorder and specific phobias).

To summarize, it is *both* the case (1) that there is much more evidence for various forms of CBT than for other forms of psychotherapy and (2) that there is practically no evidence that CBT is more effective than other forms of psychotherapy. When proponents of CBT and proponents of psychodynamic therapy discuss these matters, they often choose selectively to attend to only one of these two aspects – the CBT proponents, on the former, and the psychodynamic proponents on the latter, again testifying to the "political" rather than scientific nature of these debates.

A Validity Crisis?

Why should different forms of psychotherapy, based on widely different theories and world views, produce equivalent effects? This is a conundrum in present-day psychotherapy research. How are these results to be explained? First of all, it may be asked whether these results mean (1) that there *are no real differences* in effects between different therapies or rather (2) that there are differences in effects which we are not able to detect because *our research methods are insufficiently sensitive* (Lundh & Falkenström, 2019). Both alternatives present problems for the present research paradigm. If the former is true, then it is just a waste of time and resources to compare various therapies in RCTs. If the latter is true, then it may be asked what is wrong with the present research methods.

Are There No Real Differences in Effect Between Different Therapies? One possibility is that there are in fact no real differences between different forms of psychotherapy, simply because the therapeutic method does not matter. This is called the Dodo bird verdict, after a story in a book by Lewis Carroll, and there are several different versions of this idea. One possibility is that the effects are due to personal characteristics of the therapist, rather than the therapy methods; if the relevant personal characteristics are equally represented among CBT therapists as among psychodynamic therapists and therapists in other orientations, this might explain why no differences in *average* efficacy are found between different forms of therapy. Another possibility is that the effects are to be attributed primarily to the patient's self-healing capacity (e.g., Bohart, 2000), rather than to the method; if the average degree of self-healing capacity is similar in the groups that are compared, this might explain the results.

The most popular version of this idea, however, is the so-called common factors model, which attributes the effects to factors that are common to all kinds of psychotherapy (Frank, 1961; Frank & Frank, 1991; Rosenzweig, 1936; Wampold, 2001). According to this model, these common factors are primarily a good therapeutic *relationship* (alliance), but not as a goal in itself, but as a means for engaging the client in a certain therapeutic *procedure* and to *persuade* the client of a new explanation that gives new perspectives and new meanings in life, as well as new "success experiences." According to this model, the existence of a method is crucial, but the important thing is not what the method contains, but how *credible* it is. Similarly, the client must be provided with a new explanation, although this explanation need not be correct in any scientific sense, but must have the capacity to work as a "myth" to believe in. Proponents of this model have argued that the change that occurs in psychotherapy is functionally equivalent with religious conversions and placebo effects. Again, for these common factors to serve as an explanation, it has to be assumed that they (i.e., the quality of the relationship, the credibility of the therapeutic procedure, and the persuasiveness of the therapist) occur to an equal degree in CBT, psychodynamic therapy, and other treatment forms. (For a more detailed discussion of this model, see Lundh, 2014.)

Are Our Research Methods Too Insensitive? If it is difficult to find significant differences in efficacy between different forms of therapy, this need not mean that there *are* no such differences. It might be that different forms of therapy have quite different effects but that our research methods are too insensitive to detect them. In quantitative terms, this might be due to low *statistical power*; if the differences in effect are small, it might take larger samples to detect these differences than the sample sizes commonly used in psychotherapy research. (See Zickfeld & Schubert, 2019 for more on the statistical power issue.) Or, in more qualitative terms, it may be the case that there are real differences between different forms of psychotherapy in the kinds of changes they produce, but that our *instruments* for measuring these effects are not sufficiently sensitive. In both of these cases, matters may improve without a change of the existent research paradigm, simply by increasing the statistical power or by developing more sensitive instruments to measure therapeutic change.

Do We Need to Differentiate Between Subgroups? Another possibility is that different therapies are *differentially effective with different patients* and that these subgroups of patients are about *equally large*. In that case, such differences will not turn up in the kind of comparisons at the group level that is basic to traditional RCT designs. If so, what is needed is a more person-oriented approach, to identify these subgroups of patients. If such subgroups are reliably identified, it should be possible to improve the treatment effects by matching such subgroups of patients to the form of treatment that is most appropriate to their individual profiles. Some promising results along these lines have been obtained by Huibers et al. (2015), with a personalized advantage index approach. These authors used results from previous research to identify predictors and moderators of outcome in CBT and interpersonal therapy and showed that depressed patients who were randomized to their predicted optimal treatment fared much better than those who were randomized to their predicted nonoptimal treatment.

Although this represents a slight move toward a more person-oriented approach to treatment, it does not represent any basic change in research paradigm. This has a close parallel in *personalized medicine*, which is a medical procedure designed to categorize patients into groups and to tailor medical decisions, practices, and interventions to individual patients based on their predicted response. In other words, it is quite compatible with a medical model of psychotherapy.

There are other characteristics of psychotherapy, however, which clearly differentiate it from most forms of medical treatment. Two of these are (1) the nature of *treatment packages* that are tested in RCTs in psychotherapy research and (2) the role of *responsiveness* in psychotherapy. Both of these characteristics point in different ways to the inadequacy of RCTs in psychotherapy research.

Treatment Packages One important difference between psychotherapy and many forms of medical treatment is that what is evaluated in RCTs in psychotherapy research are large *treatment packages*, which contain numerous technical procedures and personal interactions, usually in the form of weekly treatment sessions during a considerable period time (from around 12–20 sessions in short-term therapies up to 1 year or more in long-term treatments). This raises the possibility that different patients may well respond to different components of the same treatment package; as long as their *average* response to the entire treatment package is the same, however, this will not show up in any analyses at the group level.

This is quite different, for example, from testing the efficacy of a tricyclic antidepressant versus placebo in patients with depression. Here the experimental condition contains one specific component, which makes it easy to know where to attribute the effects. Similarly, if two different antidepressants are compared in an RCT and are found to produce equivalent effects, there is little problem in how to interpret these results. However, in psychotherapy research, if two treatment packages are compared and found to produce equivalent results, there are a large number of possible alternative explanations. Among these are, for example, (1) that some factors were *common* to the two treatment conditions and these were equally effective in both conditions; (2) that *different* treatment components were effective in the two treatment conditions, but they were equally effective at an average for the patients in the two conditions; and (3) that different treatment components were effective for different patients even *within* each treatment condition but that the effect was to produce equal average change in the two treatment conditions.

Responsiveness An additional complication is that the therapist's behavior in each treatment condition is affected by the patient's behavior. That is, the "independent variable" in psychotherapy research (what occurs in the treatment) is in fact influenced by the patient (who represents "the dependent variable"). Stiles, Honos-Webb, and Surko (1998) refer to this as *responsiveness* and point out that this characterizes all kinds of human interaction, including psychotherapy. Examples at the most basic level are that people normally answer each other's questions, stay on related topics, and take turns when they are speaking. Further, each therapist is likely to do this in his or her particular way, which also will differ depending on each particular patient's behavior.

In other words, the therapist does not just *deliver* an intervention, but responds to the client's behavior on a wide range of time scales. This also means that the technical procedures in a given treatment package are carried out in different ways, not only depending on the therapist's personality and professional skills but also as an adjustment to the patient's personality and behavior. Here psychotherapy differs very clearly from medical treatments, which can often be carried out more as one-way *interventions*:

> In suggesting that psychotherapy is responsive, we mean that the content and process emerge as treatment proceeds, rather than being planned completely in advance. Thus, no two clients receive identical treatments, just as no two conversations are identical. Responsiveness may be contrasted with *ballistic* action – action that is determined at its inception and carries through regardless of external events. Ballistic action is insensitive and nonresponsive, not incorporating emerging information. Put this way, no psychotherapy is ballistic. Nevertheless, psychotherapy research often incorporates implicit ballistic assumptions. (Stiles et al., 1998, p. 440)

In other words, the average outcomes measured at the group level in typical RCTs hide an enormous variety of personal, interpersonal, and contextual factors that may be at work in individual therapies, in such a way that the RCT methodology seems utterly inadequate to capture the nature of therapeutic change. It may be argued that, to develop a more detailed empirical knowledge of what actually works in psychotherapy, we need to enter into the actual psychotherapy process as it unfolds in concrete individual treatment sessions over time.

RCTs in Psychotherapy Research: A Case of Pseudo-experimental Research?

RCTs in psychotherapy research are classified as *experimental* designs, and experimental designs are generally considered to be optimal to establish causality, because they prioritize *control* over all possible variables that are involved. This ideally involves a rigorous control both of (1) the experimental manipulation as such (the independent variable) and (2) other possible variables that may have an effect (by using control groups and random assignment of the participants to the experimental group and the control group). The ideal is that the experimental group and the control group should differ in only one way: the experimental group receives a rigorously controlled experimental manipulation, whereas the control group does not receive it.

As has already been illustrated in the previous reasoning, however, RCT designs in psychotherapy research in general suffer from a very low degree of control over the experimental manipulation – that is, what actually takes place in the treatment. The therapies that are tested in RCTs are not described in terms of observable treatments (as is common in other areas of experimental psychology) but in terms of certain *constructs* that are used to label entire treatment packages (e.g., "cognitive behavior therapy," "short-term psychodynamic therapy," "interpersonal therapy," etc.), the principles and procedures of which are outlined in *manuals*. The extent to which a treatment package is implemented as intended is called *treatment integrity* (Perepletchikova, Treat, & Kazdin, 2007) and defined in terms of (a) therapist adherence (i.e., the degree to which the therapist utilizes prescribed procedures and avoids proscribed procedures); (b) therapist specific competence (i.e., the level of the therapist's skill and judgment in carrying out this particular treatment); (c) and treatment differentiation (i.e., whether the treatments that are compared differ from each other along critical dimensions).To ensure an acceptable treatment integrity, the treatment sessions are video recorded and trained observers are set to watch recorded sessions and judge the therapist's adherence to the manual, and their competence in implementing these interventions, on the basis of these observations.

Treatment integrity is one aspect of *construct validity* in this research. For example, if a treatment that is implemented under the name of "cognitive therapy" really fits the construct of "cognitive therapy" as defined in the manual, and if it is also sufficiently differentiated from other therapy constructs (e.g., "psychodynamic therapy"), then this speaks in favor of the construct validity of the conclusions that can be drawn from the results of this particular study (cf. Shadish, Cook, & Campbell, 2002).

High construct validity in this case, however, involves more than just treatment integrity as defined; it also involves the ability to exclude alternative interpretations of what took place in the treatment – for example, interpretations in terms of the therapist's warmth, genuineness, use of empathic listening, validation of the patient's experiences, supportive interventions, the consistency with which the therapist conveyed a clear rationale for the treatment, and the therapist's skill and sensitivity in using such nonspecific factors. Many of these factors are characterized by what Stiles et al. (1998) call *responsiveness*, in the sense that they represent adjustments to the patient's behavior and communication in the therapy session.

Although these potential factors are also, in principle, possible to rate by trained observers while looking at video-taped treatment sessions, this is almost never done. In a systematic review of comparative RCTs of treatments for borderline personality disorder, Lundh, Petersson, and Wolgast (2016) found that existing studies generally included little data that would make it possible to rule out such alternative explanations of the effects. Most of the RCTs that were analyzed even failed to control factors such as the actual dosage of the treatment (number of sessions, length of sessions, etc.), supervision arrangements, the use of medication in addition to psychological treatment, and all aspects of treatment integrity.

As argued by Lundh et al. (2016), construct validity is a neglected topic in psychotherapy research. This is also seen in the publication policies of scientific journals in this area, which seldom require any systematic analysis of alternative explanations of the effects. It is true that the last decades have seen improvements in the reporting standards required of journal articles in connection with psychotherapy research. For example, the Journal Article Reporting Standards (JARS) that are included in the APA manual (American Psychological Association, 2010) require authors to discuss threats to internal validity (i.e., threats to concluding that the effects can be attributed to the treatment) and external validity (generalizability of the conclusions). Nothing, however, is mentioned of the need for an explicit discussion of threats to *construct validity* (e.g., alternative interpretations of the treatment as such).

It is possible that RCTs in psychotherapy research are most adequately classified as a kind of *pseudo-experimental research*. In terms of a three-dimensional ontology

(Lundh, 2018), which differentiates between (1) a material dimension (with observable situations and behaviors), (2) a subjective-experiential dimension (conscious experiences), and (3) a social-constructional dimension (including categories, theories, norms, techniques, etc.), true experimental research belongs to the material dimension. That is, experimental manipulations are defined in terms of observable situations and behaviors. The psychotherapies that are tested in RCTs, however, belong in the social-constructional dimension – they are constructs, referring to principles, strategies, and techniques of treatment found in books and manuals that therapists are trained in. Although the construct validity is checked retrospectively in terms of independent observers rating the therapists' adherence and competence on the basis of observations of video-recorded sessions, this does not guarantee good construct validity.

Moreover, there seems to be no strong evidence that adherence to a manual is associated with treatment outcome; when Webb, DeRubeis, and Barber (2000) carried out a meta-analytic review of RCTs that measured the therapists' adherence and competence in implementing the treatment manual, they found no significant association between these variables and the treatment outcome. This further questions this kind of variable-oriented approach to psychotherapy research.

A Person-Oriented Approach to Psychotherapy Research

A person-oriented approach, as defined by Magnusson (1999), is *idiographic* (by focusing on within-person changes over time), *holistic* (i.e., the individual person is seen as an integrated whole), and *interactional* (by focusing on the interaction between the individual and its environment and between subsystems within the individual). A corollary to this is that a person-oriented approach to psychotherapy research must take into account that there are always at least *two* persons involved in psychotherapy: a therapist and a patient. This therefore requires a "two-person psychology" (e.g., Wachtel, 2008).

If a person-oriented approach is defined as being idiographic, holistic, and interactional, we may conclude that at least a *partial* movement in that direction can be seen in some varieties of psychotherapy research (cf. Lundh & Falkenström, 2019). Most obviously, there are examples of psychotherapy research that are clearly idiographic, by focusing on within-person changes at the level of the individual, but without being holistic or interactional. This is true, for example, of idiographic research that makes use of *intensive longitudinal data* to analyze changes in the patient during treatment, the temporal order of different kinds of changes in the patient, and how these changes are associated over time with various treatment interventions. To exemplify, Boswell, Anderson, and Barlow (2014) carried out an idiographic analysis of change processes in a patient with depression and anxiety who underwent unified transdiagnostic CBT treatment. The results showed, among other things, that changes in mindfulness and cognitive reappraisal preceded changes in depression and anxiety and that the changes in mindfulness and reappraisal were most strongly associated with stages of the treatment where the corresponding skills were trained. The authors suggested that the functional relationships found in this case should be made subject to systematic replication to see whether these results generalize over multiple individuals.

Although this research represents a move toward a more person-oriented approach, it should be noted that the only person who is focused here is the patient (i.e., the person of the psychotherapist is still missing from the analysis). Moreover, the patient is analyzed primarily in terms of single variables (e.g., anxiety, depression, mindfulness, emotion regulation) rather than as a whole individual. Although this kind of research is clearly idiographic, it is not fully person-oriented in Magnusson's (1999) sense because it lacks a holistic approach.

There are at least three more moves that are needed to develop a person-oriented approach to psychotherapy: (1) The patient needs to be conceptualized as a whole person, with a special focus on his/her problems, but also including other patterns of personal functioning. (2) The psychotherapist needs to be conceptualized as a whole person, with a special focus on patterns of professional skills but also including the therapist's more personal way of functioning. (3) The relationship between therapist and patient over the course of treatment needs to be conceptualized in terms of interactions between two persons.

As to the first of these requirements, a partial move in this direction can be seen, for example, in Fisher and Boswell's (2016) arguments for a personalization of psychotherapy, by means of person-specific dynamic assessment and modeling, to identify "person-specific dimensions of psychopathology (Fisher, 2015) that cut across existing diagnostic categories in order to select optimal treatment protocols on a person-by person basis" (Fisher & Boswell, 2016, p. 496).

As to the second and third of these requirements, there is clear evidence for therapist effects – that is, some therapists consistently tend to have better treatment outcomes than others. Most of the research in this area, however, has studied how single therapist variables (e.g., alliance, empathy, goal consensus, positive regard, congruence/genuineness, self-disclosure, attachment patterns, etc.) are linearly associated with outcome, either by means of correlational designs or hierarchical linear modeling (HLM), but without taking context into account. As pointed out by Hill and Castonguay (2017), "some therapist variables are difficult to include in HLM analyses because it is not frequency of the variable as much as timing and quality that matters, and these contextual considerations are much harder to measure and include in statistical analyses" (p. 330).

A person-oriented approach to psychotherapy research that is truly holistic and interactional requires a study not only of how therapists adapt "the choice, dose, manner of implementation, and timing of their interventions to fit clients' moment-to-moment needs" (Hill & Castonguay, 2017, p. 333) but also their ability to integrate these skills holistically. As suggested by Hill and Castonguay (2017), rather than single skills (or a set of single skills), "the integration of skills and other variables may provide a better explanation of therapist effects" (p. 333). A person-oriented approach along these lines presupposes the use of some kind of taxonomy of therapist skills and attitudes, which can serve as a basis for analyzing holistic patterns of professional relating to the patient, in combination with other more non-technical aspects of the therapist's psychological functioning (Lundh, 2017).

Conclusion

The present chapter took its starting point in some comments about the state of psychological science made by Husserl and Wittgenstein during the first part of the twentieth century. Husserl commented that psychology as he knew it seemed to be in a state of perpetual crisis, and both of them argued that psychology suffered from conceptual confusion due its way of imitating the natural sciences. One conclusion from the reflections made in this chapter is that even present-day psychological science, including psychotherapy research, is in a state of crisis.

These problems are partly conceptual (too little work being done on fundamental conceptual and theoretical issues) and methodological (a dominance of variable-oriented research, which ignores the role of the person and the context). But the problems are also partly of a social nature, because the social incentive system in the scientific community tends to reinforce questionable research practices (QRPs), such as selective reporting of results, and tends to favor the social intelligence required to get financial grants over scientific curiosity, creativity, and productivity. Among other things, this has resulted in problems such as publication bias – that is, the empirical results published in scientific journals are not representative of the research actually carried out and are therefore likely to mislead both the public and new generations of researchers. One expression of this is what is today called a replicability crisis, as seen in the difficulty of replicating a number of presumably well-established findings.

If the arguments in the present chapter are correct, today's crisis goes deeper than being merely a replicability crisis. What we have is also a *normativity crisis*, due to a social incentive system that is not conducive to scientific progress, and a *validity crisis*, due to a variable-oriented approach that is not suitable to the scientific problems that need to be solved. Whereas the normativity crisis requires a change in the social incentive system operating in the scientific community, the validity crisis requires a change of paradigm from a variable-oriented to a more person-oriented approach.

In the latter part of the chapter, these topics were discussed specifically with regard to psychotherapy research. The research paradigm that has dominated psychotherapy research during the last decades has been clearly variable-oriented, as focused on randomized controlled trials (RCTs) to provide evidence for the effects of various forms of psychotherapy or on correlational studies to find variables (e.g., therapeutic alliance) that are associated with treatment outcome. Although RCTs have probably played an important role in defending the place of psychotherapy in the medical care system, this kind of research is utterly incapable of providing a scientific understanding of what makes psychotherapy work. Further progress in this area may require a systematic development of a person-oriented approach to psychotherapy research, which is (1) idiographic (focusing on within-person change and therapeutic process by means of intensive longitudinal data), (2) holistic (focusing on both the patient and the therapist as integrated persons rather than on single variables), and (3) interactional (by focusing on the interaction that takes place between therapist and patient during treatment).

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Chapter 13
Open Access: A Remedy to the Crisis in Scientific Inquiry?

Lars Wenaas

The term ‘crisis’ seems to be used often when it comes to science; there is a crisis in the public trust in science, a replicability crisis, and the increase in retractions is denoted as a crisis. There is a validity crisis, a statistical crisis and there is a crisis in scholarly communication. The objective of this chapter is to comment on this framing and connect the notion of ‘crisis’ to normative factors which are central in other chapters in this book and ultimately discuss whether opening up science is a suitable remedy. Open Science is a toolbox designed to improve science, and in particular Open Access is seen as the solution to the crisis in scholarly publishing. The principle of Open Access is that all scientific knowledge should be available for anyone to read and utilize. However, even if Open Access may be attractive for the researcher as a reader, the researcher as an author may hold a different position. A researcher in pursuit of a career needs to take into account prospects of future grants and tenure, and as a result, the choice of publishing outlet seems to be influenced by the incentives that follow journal ranks. The central idea of this chapter is that the choices made when publishing, are constrained by the quest for high-ranking journals, and this is likely the main source of many, if not all, of the crises in the science system. The quest for academic credits affects more than the final step of dissemination; it influences the research process and scientific conduct as a whole. The current arrangement of incentives is also in conflict with Open Science, particularly Open Access in respect to the scientific journal. This means other measures are needed to address the crisis, primarily new ways of research evaluation.

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Problem Versus Crisis

It's not evident that all these alleged crises have earned their reputation as such. For instance, in 2011 it was reported that the number of articles retracted had increased tenfold during the previous 10 years, while the increase in articles expanded only by 44% (Van Noorden, 2011). However, an investigation reveals that only 4 papers out of 10,000 are retracted and the percentage of retracted papers has levelled out since 2012 (Brainard & You, 2018). Fanelli argues that the increase in retractions on the contrary is a sign of integrity; the self-correcting mechanisms in the system works; retractions are a problem being dealt with (Fanelli, 2014). Another example is that the public trust in science is supposedly at a low point. However, according to NORC, an independent research organization at the University of Chicago, confidence in the scientific community in the USA has been stable for decades despite the divide in matters like climate change and food science (Funk & Kennedy, 2017). Similar positive attitudes towards science can be found in the UK, where the public trust in scientists is at record high according to the Ipsos MORI Veracity Index (Stoye, 2017).

It might be debatable what constitutes a crisis and where to draw the line between a crisis and a serious problem, but clearly Ioannidis article 'Why most published research findings are false' (Ioannidis, 2005) is an example of the former if the state of science is in a condition as the title implies. Add this to the 'Sarewitz debate' on the supposedly abysmal state science is in, where 'Science isn't self-correcting, it's self-destructing' by science's detachment from real-world problem (Sarewitz, 2016), and there certainly seems to be room for improvements. In the social sciences, reproducibility in psychology has been under thorough investigation spearheaded by Brian Nosek. The Reproducibility Project is behind the study of the low reproducibility in psychological science where estimates hold that only 39% of studies are replicable (Open Science Collaboration, 2015). Journals of high reputation are not exempt from this effect as shown by a study on the replicability of 21 social science studies published in *Science and Nature*. Of the 21 studies, only 13 are replicable and equally important, with a significantly lower effect rate than in the original studies (Camerer et al., 2018). The scientific community is aware of these problems; 90% of 1576 researchers in a cross-disciplinary survey believes there is a (significant or slight) reproducibility crisis (Baker, 2016). Apparently the crises are recognized by scientists, however, what is the cause and what is the remedy?

Normativity in Science

The replicability[1] crisis is one of the focal points in Zickefeld and Schubert's chapter in this book, where they prescribe sound principles and thorough procedures for solid scientific practices very much aligned with the idea of Open Science

[1] Even if there are differences, replicability and reproducibility are commonly used interchangeably. For a discussion of this theme, see 'Replicability is not Reproducibility: Nor is it Good Science', 2009.

(Zickefeld & Schubert, 2019). Most notably they emphasize the statistical competence and craftsmanship needed for proper scientific conduct. The replicability crisis is also central in Lundh's chapter, together with the validity crisis and the normativity crisis (Lundh, 2019). I believe like Lundh that the replicability crisis is connected and anchored in the normativity in science. Normativity in itself is not necessarily something negative; in his chapter, Brinkmann considers the presence of normativity of science self-evident as the quest for truth, validity, reliability and utility is a natural part of scientific practice (Brinkmann, 2019). In parts of his argument in the conclusion, he holds that value and normativity are integral parts of all human experience and that value judgments are important, not only to everyday life but also to the scholarly practices of psychology and social sciences. This is true, not just because of the inherently normative character of the human and social sciences where humans are the object of investigation. The value judgements and normativity extend to all disciplines and the whole science system as such, because humans are the ones conducting science. This makes the scientific endeavour as such a normative affair or at the very least an affair with normativity and value judgements as one of the main ingredients.

Scholarly Communication and Normativity

There are reasons to believe that our judgements surface in a negative way in how, where and why we publish: we adapt to the incentive system that governs science, we chase the prestige and renommée we ascribe to top-ranking journals and we publish to collect citations. These 'pellets of recognition' have become central in the quest for peer recognition (Merton, 1988) and are now the basic ingredient in research evaluation. At some point, it seems that strategic gaming behaviour has become an integrated part of our analytical methods which are selected to pursue publishing rather than the progress of science (Smaldino & McElreath, 2016). Brian Nosek is more explicit and connects the reproducibility crisis and the crisis in scholarly communication directly. His claim is that the norms of publishing are a chase for novelty and positive results and a natural disregard for negative findings, which instigates research design and analysis primed for positive findings. This leads to an increase in false effects in the scholarly literature (Nosek, Spies, & Motyl, 2012), an effect that seems to be more prevalent in the social sciences. Studies imply that the odds of reporting a positive result was 2.3 times higher in the social sciences than in the physical sciences, an effect ascribed to the relatively fewer constraints to both conscious and unconscious biases in the alleged 'softer' sciences (Fanelli, 2010).

If there is a causal trail between questionable research design and priming of results on the one hand and publishing in high-ranking journals on the other, then the normativity crisis as described by Lundh can be seen as the overarching structure which for a large part is governing most of, if not all, the other crises. The connection lies in a publication culture which through the incentive system fosters effects like hyperauthorship (Ioannidis, Klavans, & Boyack, 2018), honorary and ghost authorship (Vera-Badillo et al., 2016), severe publication bias (Peplow, 2014) and an increase in inflated language and the use of positive wording (Vinkers, Tijdink, & Otte, 2015). This appears to be systemic, the incentives simply make grounds for a natural selection of inferior science and requires 'no deliberate cheating nor loafing—by scientists, only that publication is a principal factor for career advancement' (Smaldino & McElreath, 2016). Where a researcher publishes can be perceived as more important than the content itself (Macdonald & Kam, 2007; Steele, Butler, & Kingsley, 2006). This view is supported statistically; an optimality model for predicting the most rational research strategy favours small studies with a 10–40% statistical power, leading to false and erroneous conclusions in half of the published studies (Higginson & Munafò, 2016).

If this line of reasoning is correct, the normativity crisis in essence is the source of the crisis in scholarly communication and the likely main cause of Ioannidis's claim; poor research design leads to false claims and low reproducibility. So, if we accept this diagnosis, if the scientific system is in a state of disease contaminated with a 'normative virus', what is the cure?

Is Open Access the Solution?

It has been claimed that Open Science is the solution to the problems in research. Open Science proponents prescribe preregistration of studies, arrangements for publishing of negative or non-significant findings, open peer review, open access to both data and publications and openness in every aspect of science (Munafò et al., 2017). Many of these principles are recognized by financiers of science; National Institutes of Health (NIH), with their long-standing commitment for Open Access, has launched a plan for getting science back into 'self-correcting mode' in regard to reproducibility and addresses directly the normative problem of publishing in top-ranking journals (Collins & Tabak, 2014). There is little doubt that Open Science in general prescribes sound procedures for proper scientific conduct, but it is not obvious that Open Science will remedy the normative intrusion in scholarly publishing. On the contrary, it is argued that Open Science practices as a general rule need to be complemented by the adoption of new research practices within the diciplines; adhering to 'open' is simply not enough (Chen et al., 2019).

Open Access is the part of Open Science that deals directly with scholarly publishing, and this will also be the scope of this chapter; in what way is Open Access the solution to the crisis in scholarly publishing and what are the obstacles for Open Access to become the preferred way of dissemination? The argument will start with investigating the problems Open Access is designed to solve, in what way Open Access conflicts with the incentive system governing science and ultimately why Open Access likely is not able to handle the full range of the scholarly publishing crisis.

In the centre of the crisis lie the academic journal and its role as an outlet for research and as the conveyor of academic credits. To understand why Open Access is incapable of being the sole solution, we must come to terms with the different flavours of Open Access and how publishing relates to the norms of science as expressed by Merton. We shall also need to investigate whether arguments against Open Access, primarily gold Open Access, are legitimate, especially viewed through the lens of the current arrangement of incentives.

The Different Flavours of Open Access

Open Access was first formalized through a series of declarations in 2002–2003 now known as the Budapest-Berlin-Bethesda initiative (Chan et al., 2002). The declarations are accompanied by three main implementations of Open Access: gold, green and hybrid. Gold Open Access is research articles published in journals using an appropriate license (Creative Commons or similar), letting the user read, download and text and data mine (TDM) the articles as long as proper attribution is done (Laakso et al., 2011). These permissions apply for the hybrid option as well, although a hybrid article still resides within a subscription-based journal, as opposed to a gold Open Access journal where all articles are free of use. An article is made green Open Access when a version of an article published in a subscription-based journal is deposited in an institutional or disciplinary repository and made publicly available after an embargo of normally 6–12 months (For some journals, the embargo period can be 3 years.) The version that may be deposited is generally not the publisher's version, but usually the (peer reviewed) version denoted as the postprint. Since the publisher's final version rarely can be deposited, the green alternative has traditionally been regarded less attractive by researchers. The arrangement of depositing is seldom accommodated with a proper license other than the publisher granting the rights for the deposit. Due to this, the legal status of TDM is often at best questionable, if not outright illegal. In the literature, one can further find contradictory concepts like 'Bronze Open Access' which is copyrighted material released free-to-read on the publishers website (Piwowar et al., 2018) and 'Black Open Access' being literature found on illegal piracy sites (Björk, 2017a; Green, 2017). Especially bronze Open Access is interesting, since the literatures' availability seems to address the goal of Open Access, but it should be pointed out that since no irrevocable reuse licence is issued, publishers can deny access to these articles at their discretion (Brock, 2018).

Means to What Kinds of Problems?

Open Access primarily addresses the lack of access to research literature for researchers working in institutions with scarce funding. Even at wealthy universities, library budgets are often too tight to accommodate the needs within the institutions. Open Access is further about opening up research to the general public and for exploitation in the private sector, following the principle argument of 'give to the taxpayers what the taxpayers have paid for'. The access problem is also closely related to the economic dysfunctionality in the academic publishing market. Access to scientific literature is achieved through increasingly expensive subscription schemes as the publishing business is dominated by a few large international publishers. Reed-Elsevier, Wiley-Blackwell, Springer and Taylor & Francis are ranking at the top with profit margins ranging from 28% to 38.9% in 2012–2013 (Larivière, Haustein, & Mongeon, 2015), resulting in over ₤900 million revenue for Elsevier (Times Higher Education, 2018). The financial structure and mechanisms have been well documented (Björk, 2017b; Larivière et al., 2015) and underline the journals' importance for the publisher as a very lucrative product. Journals are complementary products that cannot be substituted by one another, and since the publisher is the only supplier of a particular product, a market failure arises, and the prices act accordingly, creating an unsustainable economic situation for the subscribing institutions. The result is a state of oligopoly in the publishing industry (Larivière et al., 2015). So it is clear that Open Access is not only designed to solve suboptimal dissemination of knowledge (Tennant et al., 2016); it is motivated by economic savings and a dysfunctional academic publishing market. However, Open Access is first and foremost about the lack of access to scientific knowledge within the academic community and this is intimately connected to the ideals of science.

The Norms of Science and the Conflict with Incentives

The scholarly publishing system is generally seen as a dissemination cycle. The researchers conduct research, write articles and transfer them to the publisher who publishes journals, which in turn are being distributed through institutions back to the scientific community. Open Access aims at enhancing the step of distribution by making research publicly accessible for all and is consequently consistent with an important norm in science; openness. This central idea is found in the ideals of science as summarized in Merton's acronym CUDOS, where each letter designates the norms Communalism, Universalism, Disinterestedness, Originality and Scepticism. The norm of communalism describes the function where researchers give up their intellectual property rights in exchange for the social recognition of sharing the research and submitting it to the scrutiny of the scientific community (Merton, 1973). It has been argued that Open Access is a direct translation of Merton's communalism (Fecher & Wagner, 2016). Willinsky has formulated this in the following way: 'open access is not just a child of the digital age, but the latest expression of longstanding principles of scholarly publishing having to do with the openness of science' (Willinsky, 2009, p. 53). Openness simply makes better science and better research output quite fitting the norms of science.

However, incentives are integrated into the dissemination cycle, and this interferes with another of the Mertonian norms, the one of disinterestedness. Merton formulated the norm of disinterestedness as conduct for the benefit of the scientific endeavour rather than for personal gain, motivated out of institutional control and sanctions including psychological conflict resulting from internalization of the norm (Merton, 1973). But idealism in itself does not necessarily accommodate tenure; the researcher grants their research to the publishers in a trade-off for the academic credits, not only for dissemination purposes (Steele et al., 2006). This trade-off is primarily connected to the journal title, resulting in credits awarded according to the journals' prestige and rank in the pecking order. The journals' prestige is gained through previously published research and is therefore disconnected from the current research (Migheli & Ramello, 2013). The researchers know how the incentive system works in the quest for grants and tenure; an international survey of 6344 researches representing all disciplines shows that dissemination of research is the primary motivation for publishing. However, almost equally important is the motivation for career advancement and the ability for future funding (Mulligan & Mabe, 2006). In the Nature Publishing Group's author survey from 2015, factors driving the choice of where to submit articles ranked "the reputation of the journal" slightly over "the relevance to my discipline" in the STM-diciplines. In the humanities and social sciences the order was the reverse, by a small margin. (Nature Publishing Group, 2015). The patterns in these surveys are not by coincidence, but arguably a predictable development following Merton's description of the trade-off for social recognition by sharing research. The journal does serve as vehicles of dissemination, but they are also vehicles of prestige, this is essential for researchers in their pursuit of a career. The norm of disinterestedness is consequently under pressure precisely because of the prominent role the incentives have gained in academic publishing. Interestingly enough, in a survey about contemporary support of Communism, Universalism and Disinterestedness among scientist, 'disinterestedness' came last of the three and was the single norm where academics agreed more with disconfirming statements than confirming ones (Macfarlane & Cheng, 2008).

Why Gold Open Access Comes with Limitations

The highly debated 'Plan S' may serve as a lens to the importance of journals and the researcher respectively as a reader and a writer. Plan S is a policy by a coalition of European research councils and funding agencies including the European Research Council, mandating Open Access publishing in all of their funded projects (cOAlition S, 2018). Demands for Open Access are not new in policies from research councils, institutions and governments; the novelty in Plan S is the type of Open Access required to comply. Plan S accepts only gold Open Access journals; all other outlets are deemed not compliant.[2] This has a significant impact on the researcher's choice of outlets where estimates hold that 85% of all academic journal

[2] The criteria for which journals are eligible by the Plan S policy are still not finally settled by the time of writing. Following criticism against Plan S, both hybrid and green open access may comply as a result of the hearing in 2019, then under strict conditions.

titles will not be eligible (Else, 2018). The debate on Plan S is therefore useful as a backdrop to understand how the normative judgements in the publishing system unfold and why researchers protest against the intervention; they are deprived of their favourite publishing outlets and the potential income of academic credits. This is considered a much bigger drawback than the advantages in getting access to the literature. Arguments against Plan S are that it could prove fatal for learned societies (Pells, 2018) and cause trouble for the next generation of researchers (Sveriges Unge Akademi, 2018), and it is unethical and cuts researchers off from the global community and leaves the quality at risk (Plan S Open Letter, n.d.; Schneider, 2018). The view on lower quality in Open Access journals is consistent with an international survey on the trustworthiness and authority of scholarly information where Open Access articles are less trusted by researchers, although the views on Open Access in the research system as such, interestingly, are positive (Tenopir et al., 2016).

Would the lack of eligible journals in Plan S be a problem if gold Open Access journals are of equally high quality as subscription-based journals? This topic has been addressed by Open Access proponents through numerous studies with claims of a citation advantage for Open Access. A list of studies was maintained up until 2015 by SPARC Europe, a Higher Education membership organization advocating Open Access, but is no longer maintained 'since the citation advantage evidence has now become far more common knowledge' (SPARC Europe, n.d.). The large body of literature on citation analysis (Rodrigues, Taga, & dos Passos, 2016) indicates the importance of showing that Open Access publishing is at least equally rewarding as subscription-based publishing. However, since Open Access comes in three main flavours and two of them (green and hybrid) are based on the existing base of subscription-based journals, it is the gold Open Access citation advantage which is important with respect to Plan S. When isolating and investigating gold Open Access journals, studies show a less clear picture. A study by Laakso and Björk concluded, through a journal impact factor analysis investigating all colours of Open Access, that Bronze Open Access articles (which in principle is not Open Access at all) on average have twice as high average citation rates compared to articles in closed subscription journals and three times as high as articles in gold journals (Laakso & Björk, 2013). This is confirmed by the Piwowar study (Piwowar et al., 2018), leading to the conclusion that the 'clear citation advantage' can actually be read as a disadvantage for gold Open Access. The common narrative, as in the case of SPARC Europe, that Open Access as such gives a citation advantage may be correct, but not necessarily for gold Open Access journals specifically. So parts of the argumentation against Plan S do have legitimacy as the version of Open Access that complies with the policy (gold) is lagging behind in terms of citations compared to the versions of Open Access (green, hybrid) that do not comply. Plan S clearly shows that Open Access cause a tension within the current system of incentives and researchers perception of quality in science.

Journals as a Proxy for Quality

As Sovacool, Axen and Sorrel state in their article on appropriate research conduct; 'It is surely a "fool's errand" to try to define quality research in Academia' (Sovacool, Axsen, & Sorrell, 2018, p. 1). Nevertheless, we do need some concept of quality for guidance, both in assessing good research and finding the right journal to publish in. As Merton noted, citations are of high importance among researchers and generally regarded a sign of quality; the more the merrier. This make sense as citations are references to previous research which acknowledges their role in the stock of knowledge. On an aggregated level, citations are an important component in the evaluation of research groups, departments, universities, research proposals, allocation of research funding and personnel. However, even if citations do indicate scientific impact and relevance, there is no evidence supporting that citations indicate anything substantial on other key characteristics of research quality like plausibility/soundness, originality and societal impact (Aksnes, Langfeldt, & Wouters, 2019). Citations also come with its own portfolio of problems, for instance excessive self-citation (Seeber, Cattaneo, Meoli, & Malighetti, 2019), citation rings (Sage, 2014) and researchers citing articles they never read (Simkin, 2003). These are effects which also can be traced back to the normativity in scientific publishing. Eugene Garfield noted 15 reasons to cite a paper (Garfield, 1996) including disclaiming or disputing the work of others; mere counting does not take these into consideration. The different motivations for citing disconnects citations from being a precise indicator of quality, however defined, and warns us on the overall use of citation metrics.

When citation counts are aggregated on the journal level and applied in research evaluation on the individual level, things get more troublesome. The journal impact factor (JIF[3]) is the most popular and well-known method in journal rankings but is a dubious measure for academic impact on the article level. JIF is a calculation invented by Garfield in 1972 and based on the number of citations in journal's articles according to a formula, spanning over the previous 2 years. It was originally intended for comparisons of journals within a specific discipline and was never intended for evaluation on the article level (Garfield, 1972). Since calculating a journals impact factor in a particular year is based on previous published results, there are obviously no substantial claims that can be made for articles published at a later stage. Furthermore, the distribution of citations within a journal shows that the most cited half of articles in a journal are cited ten times more often than the least cited half (Seglen, 1997). JIF is not statistically sound (Seglen, 1992), and there is no connection between an articles' quality and its outlet's JIF. This leads to a clear discouragement of using JIF as a proxy for quality on the article level altogether (Seglen, 1997). Studies argue that this discouragement does not only apply to JIF; all journal metric systems applied in research assessment are simply bad

[3] For a more in-depth introduction to the history and critique of journal impact factor, see Lariviere and Sugimoto (2018) and Zhang, Rousseau, and Sivertsen (2017).

scientific practice (Brembs, Button, & Munafò, 2013). This is further underlined by a study claiming that evaluating two articles by their respective journals' impact factor in most cases equals coin flipping (Brito & Rodríguez-Navarro, 2019). In this respect, it is hardly helpful for Open Access proponents that Open Access journals approach the same academic impact in terms of JIF as subscription-based journals given comparable circumstances like age, discipline and country of publisher (Bjork & Solomon, 2012). Even if Open Access journals are peers to subscription-based journals, Seglen's discouragement of evaluation by using JIF still stands. Whether the journal is open or not, journal metrics has serious limitations when used as sole means in research evaluation. It seems we are in need of something else. As stated by a frustrated researcher: 'we are told that the impact factor should no longer be used, but not told what to use instead' (Tregoning, 2018). Maybe this is not entirely correct.

New Ways of Evaluating Research

The argument so far has been that normativity in academic publishing is the foundation for many of the problems in science and that there are limitations in the remedy of Open Science in general and in Open Access in particular. The challenges lies in both formal and informal systems of research evaluation which are based on the journals' prominent role in the incentive system.

We should perhaps ask what the goal of implementing open practices really is. We could gain better access to the scientific literature by implementing Open Access (and thus conform better to the norm of communalism), and we could change the incentive system to make room for better scientific practice (and thus conform better to the norm of disinterestedness).

The relationship between the two goals can be illustrated by imagining flipping the existing portfolio of subscription-based journals to Open Access. It is hard to see how this would change the incentive system in any way; the journals would still be a part of journal ranks and used for evaluation. This may serve as an illustration of why Open Access in essence cannot deal with the normativity in scholarly publishing; by design it doesn't even try.

Further, adressing the one goal without the other could lead to conflicts since Open Access represents new requirements for researchers that disagree with the standards of the established academic fellowship. As in the case of Plan S, prestigious journals considered of high quality can be regarded non grata by policies. It may take years for journals to obtain high prestige, and many Open Access journals have yet to meet the top standards as perceived by the academic community (Migheli & Ramello, 2013); these new requirements may simply be an obstacle for young researchers in pursuing a career. University departments and research fellowship are encouraging staff to publish in high-ranking and approved journals; a young researcher in the beginning of a career is expected to listen to the senior's advice and 'play the game'. Kam and Macdonald puts it like this: "what a quality journal is does not really matter, the agreement that there are such things matters very much indeed." This leads to 'gamesmanship'; the art of winning without cheating (Kam & Macdonald, 2008). This is the game the aspiring researcher must learn in order to pursue a career.

Open Access won't change normative judgements in scholarly publishing, but normative judgements must change in order for scholarly publishing to become Open Access. Consequently there is a need for ways of assessing research which does not include journal ranks. The San Francisco Declaration on Research Assessment is such an initiative and states that research should be assessed on its own merits and journal metrics like JIF should be disregarded (DORA, 2012). The Leiden manifesto is also suggested as a starting point for responsible metrics (Hicks, Wouters, Waltman, de Rijcke, & Rafols, 2015). Plan S is endorsing DORA, and its signatories intend to incorporate DORA and its requirements in their policies; this is an important contribution to a change in formal evaluation systems.

Neither DORA nor the Leiden manifesto explicitly connects to Open Science, but policies in the EU bridge Open Science, Open Access and research evaluation. EU sees open scientific practices as an important enhancer of innovation in the future. This view is elaborated in policy documents like 'Open Science, Open Innovation and Open to the World' and can be seen as a reinforcement of the social contract between science and society (European Commission, 2016). In this picture, Plan S is a natural policy-development, but for Open Science to happen, there is a need for a change in our evaluative measures. The European Commission has therefore issued a working report with the title 'Evaluation of Research Careers fully acknowledging Open Science Practices' emphasising a whole new range of evaluation mechanisms in the full spectre of Open Science:

> For the practice of Open Science to become mainstream, it must be embedded in the evaluation of researchers at all stages of their career. This will require universities to change their approach in career assessment for recruitment and promotion. It will require funding agencies to reform the methods they use for awarding grants to researchers. It will require senior researchers to reform how they assess researchers when employing on funded research projects. (EU Commission, 2017)

The point that evaluation of research is the keystone in the promotion of Open Science is also the conclusion by an expert group in a EU-commissioned report on the future of scholarly publishing (EU Commission, 2019).

Institutions and governments clearly have an obligation to make sure systemic changes in evaluation are not a drawback for researchers. It is unfair to put obligations on researchers when conflicting rules of conduct directly influence the possibility of career advancement and scientific opportunities. This applies not only locally or nationally but also internationally. In the case of Plan S, European funders have a limited reach globally. Initiatives that call for a change in evaluation schemes must take the international dimension of science into account or risk a division between researchers depending on funding from Plan S signatories and those who do not. There is an additional warning. A global Open Access economy means a change in funding streams that could leave out academics in the global south or at other less funded institutions, due to the principle of pay-to-publish. A Max

Planck white paper makes a strong case for there being enough money in the system globally to convert the entire subscription regime to Open Access, with potentially large savings after a transition (Schimmer & Geshunhn, 2015). However, funds for subscriptions are not easily translated to funds for publishing; a change in streams must address that funds will be distributed disproportionally between institutions and countries and leaves researchers with scarce funding at risk financially. We run the risk of creating a new division, not between who can or can't afford to read scientific articles, but one between those who can and can't afford publishing.

Conclusion

The argument has been that there are serious problems in science, problems that can be traced back to the normative judgements in researcher's pursuit for grants, career and recognition. The pursuit is in itself both natural and commendable, but when incentives become the main target rather than solidity of scientific conduct, we need to adjust the course. There is an imbalance between the journal's function as an outlet for dissemination and its function for the allocation of academic credits. This imbalance is also obstructing the remedy that comes in form of Open Science practices. Open Science comes with limitations; in general it prescribes sound procedures for scientific conduct by opening up the research process and thus submitting it to the scrutiny of the scientific community. More transparency may mean better science, but even if all academic outlets switched from being subscription-based to Open Access, the incentives attached to the journals would still play a negative role if we continue to insist on judging the book by the cover. We run the risk of substituting the scholarly publishing system with a more open version where the Mertonian norm of disinterestedness is still severely challenged. The crucial point is therefore to change the evaluation schemes in science, the DORA declaration, the Leiden manifesto and assessment acknowledging Open Science practices being a good starting point. Incentives should discourage traditional closed practices and reward openness.

All incentive system has the power to change or reinforce behaviour. This is what incentives are designed for, and it should encourage stakeholders to be very careful in the way they are implemented. Researchers adapt to incentives, and we risk goal displacement: scoring high in assessment drills becomes the goal. A final point; we may fall into a trap formulated by Barry Schwartz:

> When you rely on incentives, you undermine virtues. Then when you discover that you actually need people who want to do the right thing, those people don't exist because you've crushed anyone's desire to do the right thing with all these incentives. (Schwartz, 2009)

We started with the framing of crisis, but it's not particularly important nor interesting whether we use the term 'crisis' or 'problem'; what is important is that these issues are dealt with. In this respect, the studies and efforts by Nosek, Ioannidis and many others are clear signs of a self-correcting and self-governing mechanism in science. If anything, these are initiatives that surely should be incentivized.

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Part IV
Social Processes in Particular Sciences: Challenges to Interdisciplinarity

Chapter 14
Fragmented and Critical? The Institutional Infrastructure and Intellectual Ambitions of Norwegian Sociology

Gunnar C. Aakvaag

In this chapter I discuss some challenges relating to the institutional organization and intellectual aims of postwar Norwegian sociology. The main argument can be summarized thus: sociology is (too) fragmented and critical. I take Norwegian sociology as my empirical case. However, although the features I analyze may be particularly salient here, I think the Norwegian case contains important lessons applicable across national and perhaps even disciplinary boundaries.

The approach I take will be empirical, critical, general, and constructive. It is *empirical* because I look at how Norwegian academic sociologists actually go about conducting their sociological business on a day-to-day basis. Thus, I break with a traditional philosophy of science approach that addresses "philosophical" questions on a very high level of abstraction. Indeed, this chapter has nothing to say about such important topics as different research logics (induction, deduction, and abduction), different types of explanations (intentional, causal, and functional explanations; what it means to explain by laws, mechanisms, or thick descriptions), the relationship between science and values (the is/ought distinction), criterion of scientific demarcation (verification, falsification, paradigms, and research programs), or social ontology (realism versus constructivism). Instead, the chapter is mainly a contribution to the empirical sociology of knowledge. Yet, my approach is also *critical* because I intend to assess – and to anticipate and criticize – the empirical patterns I identify and their consequences for the kind of knowledge Norwegian sociologists produce about society. This normative line of investigation is closer to a traditional philosophy of science approach. Furthermore, my approach is *general* because I try to identify some overall institutional and intellectual patterns of Norwegian sociology. Indeed, my main interest is the broad trends and patterns, not the details, and in this case, I think the devil is not in the details. Finally, the chapter

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is *constructive* because I want to suggest an alternative way of doing sociology. For the purpose of intellectual innovation and improving sociology, I believe it is not enough just to be critical and "say no"; one also needs to be constructive and present an alternative way of doing things.

The chapter proceeds as follows. In the first part, I address the most prominent characteristic of the current *institutional infrastructure* of Norwegian sociology, namely, sub-disciplinary specialization. I describe it and give a brief account of its historical genealogy. Then I assess its consequences for Norwegian sociology. In the second part, structured according to the same template, I address the *intellectual ambitions* of Norwegian sociology, namely, to be critical of society: detect and criticize social problems. Finally, in the third and last part of the chapter, I take a more constructive approach and look at an alternative way of doing sociology, based on a combination of synthesizing theory and positive sociology.

Just a few remarks about empirical evidence. The chapter is supported by three types of empirical sources. First, I rely on previous research. Several books, papers, and reports have been written about Norwegian sociology, all of which are taken into account in my analysis (particularly, I rely on Ahrne et al., 2010; Mjøset, 1991; Slagstad, 1998: 371–392; Thue, 1997, 2006). Second, having read much of what Norwegian sociologists have published the last 70 years, I also rely on firsthand contact with the primary documents. Finally, as a full-time member of the sociology community for almost 20 years, I have conducted a long-standing fieldwork in the field of Norwegian academic sociology. Therefore, I know my case from the inside.

Sub-disciplinary Specialization

I will look first at the institutional infrastructure of Norwegian sociology. What are the institutional rules and regularities that govern Norwegian sociology? That is to say, how is academic sociological knowledge production practically organized? At the most basic level, Norwegian sociology is characterized by *sub-disciplinary specialization*. It is split into a large number of sub-disciplines – subsystems within the larger system of academic sociology. The basis for this differentiation is *thematic*: each sub-discipline studies either a delimited social arena, such as working life, education, the health system, religion, and the family, or it studies a delimited social phenomenon, such as class, gender, ethnicity, power, and generations. Furthermore, each sub-discipline is relatively *autonomous*: an enclosed social universe characterized by local and specific research frontiers, debates, data, methods, middle-range theories, seminars, workshops, journals, and book series that altogether demarcate and sustain the sub-discipline's institutional borders vis-à-vis the other sub-disciplines. In this way, each sub-discipline is relatively self-sufficient and lives its own life mostly independent of what goes on in the others. How many such sub-disciplines are there? I know of no official nor unofficial count. Nonetheless, if we start out from the number of sessions of the annual meetings of the European (ESA) and American (ASA) sociological associations (40–50) and subtract a bit because Norway is a small country, we end up with around 25–30, which I think is a fair estimate. It also resonates with my own informal count.

Now a key question pertains to the *interaction* between the sub-disciplines. In particular, are their efforts coordinated or not? Some interaction between the sub-disciplines takes place, as Norwegian sociologists read and from time to time come together to discuss their work across sub-disciplines. For instance, Norwegian sociologists have just (in 2018) debated whether Norway is still a class society, gathering members from across sub-disciplines as both contestants and audience. However, these are sporadic happenings with small consequences for the daily work of Norwegian sociologists. Furthermore, even though Norwegian sociology has a national council ("Nasjonalt fagråd for sosiologi") with one member from each of the sociology departments at the Norwegian universities, not much coordination takes place here. The national council only meets once a year, and it has neither the ambitions nor legal, administrative, or monetary means to govern and coordinate the efforts of Norwegian sociologists. Rather, the governance and coordination take place on the lower level of departments and research institutes where individual or groups of researchers in their capacity as members of local sub-disciplines make decisions about research issues, perspectives, and strategies. Therefore, although Norwegian sociologists at times communicate across sub-disciplinary borders, they communicate much more *within* sub-disciplinary borders in the specific code that structures communication there. In short, at the level where Norwegian sociologists make research decisions concerning topics and perspectives, sub-disciplinary specialization reigns.

Thus, the overall picture is quite clear. With regard to institutional organization, Norwegian sociology has aptly been described as a loosely coupled "coalition of sub-disciplines" (Engelstad, 1996: 246, my translation) consisting of approximately 25–30 self-sufficient and mostly non-coordinated sub-disciplines analyzing 1 delimited social arena or phenomenon of (mostly) Norwegian society.

Let me now illustrate how this sub-disciplinary specialization works. Since 1968, Norwegian sociologists have regularly published new editions of a book titled *The Norwegian Society* (*Det norske samfunn*). This book provides an updated introduction to what Norwegian sociologists know about Norwegian society. In 2016, the seventh and latest edition was published (Frønes & Kjølsrød (eds.), 2016b). It contains 3 volumes with 42 chapters written by 64 authors, adding up to 1200 pages. The book's content and structure very clearly echoes the sub-disciplinary specialization of Norwegian sociology. Each chapter is written by one or more prominent sociologists (and a few by non-sociologists) located in a particular specialized sub-discipline and addresses either a particular arena (such as the economy, politics, religion, art, media, health, education, civil society, transportation, or the family) or phenomenon (such as class, sex, ethnicity, demography, and social movements). Moreover, Norwegian sociology's lack of coordination is displayed by the fact that there is no attempt to synthesize the different chapters into a more totalizing and overall model of Norwegian society. The editors have written only a very brief (2–3 pages) foreword without any synthesizing ambitions (Frønes & Kjølsrød, 2016a). Moreover, the book lacks an afterword that summarizes the overall trends and findings of the book. In addition, the book contains no separate chapter addressing how the different sectors and phenomena described in the book are integrated. Finally, even though the editors claim in the foreword the cross-references between chapters are meant to integrate the book (Frønes & Kjølsrød, 2016a: 10), the cross-references are much too few and random to integrate the chapters into a more comprehensive picture of Norwegian society. As a result, the book, in many ways the flagship of Norwegian sociology, says nearly as much about the fragmented state of Norwegian sociology as it does about Norwegian society (for more, see Aakvaag, 2006, 2017a, 2017b).

The Genealogy of Sub-disciplinary Specialization

Why did Norwegian sociology become subject to sub-disciplinary specialization? To begin with, two general social mechanisms are important. The first is an *inherent intellectual thrust* toward specialization in all academic disciplines. By deliberately splitting up a complex empirical phenomenon such as Norwegian society in its parts and studying them separately, Norwegian sociologists have achieved a more detailed understanding of the entities, processes, and outcomes constituting it. Hence, there is no way around specialization. Indeed, according to Max Weber (2009: 134–136), you are either a specialist or a dilettante. Secondly, as Norwegian sociology expanded throughout the postwar period and intellectual competition for resources, positions, funding, and prestige hardened, there was a growing need for researchers to carve out a *competitive niche* for themselves where they could be the leading experts.

However, intellectual and competitive pressures toward specialization are highly general social mechanisms that must be instantiated in a particular context. Hence, there is also a more local story to tell about the particular *kind* of sub-disciplinary specialization typical of Norwegian sociology and the *actors* who yielded it. To perform such an analysis, I will invoke an analytical sociological framework founded upon two elements. The first is the concept of a *social field*. A social field is a demarcated social arena constituted and regulated by a set of social (shared) rules defining what goes on in the field and what gives actors influence and status (Bourdieu, 1988). Concerning these rules, two types of activities are central. The first is to accumulate, according to the rules of the game, as much as possible of the field-specific “capital” (resources) of the field, such as positions, prestige, and influence in the sociological field. The second is the clash over what the rules of the game are to be in the first place, what, for instance, is to count as “high-quality” and “important” sociology. The second element is the concept of a *generation*. This concept has a demographic dimension, denoting individuals belonging to the same birth cohort. Following the biological life cycle, every 20–25 years, a new generation sees the light of day. However, the concept also has a sociological dimension that will be of more interest here (Mannheim, 1993: 351–398). As young people are shaped – often in confrontational ways – by the society they are born into, they develop generational values, norms, identities, and sometimes even generational projects that they bring to bear on the different social fields they gradually enter. Hence, although not all members of a generation are identical, and even though generation is far from the only significant sociological explanatory variable, the concept is valuable for analyzing overall patterns of stability, conflict, and change in Norwegian sociology, notably in relation to conflicts between "the established" and "the young."

For these reasons, my approach in what follows will be to study Norwegian sociology as a social field in which new generations establish, challenge, and defend particular rules of the game: what sociology should aim to achieve. Doing so, we find three generations that each in their own way have shaped the Norwegian sociological field into a set of specialized sub-disciplines.

The Golden Age Generation: 1950–1970

In 1895, there was an attempt to establish a chair in sociology at the University of Oslo for Sigurd Ibsen, the son of the playwright Henrik Ibsen. If successful, this would have made him one of the world's first sociology professors. However, a conservative committee due to a combination of sociology's lack of scientific merits and its radical political reputation turned him down (Langslet, 2004: 143–181; Slagstad, 1998: 163–167). As a result, academic sociology in Norway is a product of the postwar period (see Thue, 2006 for an overview of the period up to 1945). The founding fathers were a group of young scholars, headed by Stein Rokkan and Vilhelm Aubert, gathering around the philosopher Arne Næss. Inspired by American empirical social science, they wanted to conduct empirical studies of the most pressing issues of the day – and in particular the challenges confronting the liberal-democratic society. Supported with money, organizational skills, and motivational energy by the lawyer Erik Rinde, this group in 1950 founded the Institute of Social Research (Institutt for samfunnsforskning) (Thue, 1997, 2006: chapter 12). The same year the Department of Sociology at the University of Oslo was established. This double organizational anchoring, which marked the beginning of the institutionalization of an academic sociological field in Norway, gave what was no more than 20–30 young sociologists born between 1910 and 1940 (Mjøset, 1991: 150), the so-called golden age generation (Hernes, 2010), the opportunity to create and shape an academic sociological field in Norway during the period 1950–1970.

How did these intellectual pioneers institutionalize Norwegian sociology? In addition to reading much American sociology, the postwar American Marshall Plan gave prominent members of this generation the opportunity to study and make research trips to the USA. In addition, prominent American sociologists such as Paul Lazarsfeld came to Norway to lecture. Consequently, the golden age sociologists were heavily influenced by their American colleges and in particular the functionalism of Talcott Parsons (1951) and Robert K. Merton (1967: part III), dominating American sociology in the 1950s and 1960s (Thue, 2006). Most important in our connection is the conception of society they appropriated from the functionalists, namely, the idea of a system – a clearly delimited territorial, cultural, social, and political unit (usually a nation-state) – consisting of several subsystems (systems within the system) with different manifest/latent and positive/negative "functions" (consequences) for the overall system (Merton, 1967: 114–136). In line with this, the golden age sociologists considered Norwegian society a social system consisting of several subsystems. Moreover, they considered it the main task of Norwegian sociology to study each of them in an empirically informed and theoretically rigorous way (although, as we will see, with a more critical twist than their American forefathers). Accordingly, they divided Norwegian society among themselves and conducted separate studies of the many subsystems composing Norwegian society, such as factories (Lysgaard, 1985), prisons (Mathiesen, 1965), the family (Grønseth, 1966), local communities (Brox, 1966), hospitals (Løchen, 1965), schools (Christie, 1971), and so on. In terms of gathering empirical information, they mostly applied qualitative methods such as participatory observation, interviews, and documentary analysis. In addition, they invoked quantitative survey research. Theoretically, in line with their functionalist inclinations, they applied concepts such as role, norm, value, sanctions, and function to analyze the data and systematize their empirical findings.

The result was the accumulation, more or less for the first time, of a systematic body of theoretically mediated empirical knowledge of contemporary Norwegian society. The result was also the founding of a new academic discipline: sociology. However, the golden age sociologists did not found a very integrated discipline. Norwegian sociology was from the beginning without a center that could coordinate the intellectual efforts of golden age sociologists specializing in analyzing different subsystems. For a brief period, this differentiation was partly counteracted by personal and organizational bonds as the 20–30 golden age sociologists knew each other well and interacted and communicated within the Department of Sociology at the University of Oslo and the Institute for Social Research (also in Oslo). However, these forms of personal and organizational integration lost much of their centripetal force as sociology expanded from a very small Oslo-based discipline in the 1950s to a national mass discipline in the 1960s and 1970s.

In sum, the golden age generation established Norwegian sociology as a loosely coupled set of thematically differentiated and specialized sub-disciplines conducting empirical studies of the different subsystems of Norwegian society. They created no center from which to coordinate the sub-disciplines and integrate their partial knowledge of society into a picture of the whole of society. To be sure, the golden age sociologists considered the question of what kind of society the overall subsystems added up to an important sociological question (e.g., Aubert, 1979: 13). However, due to sub-disciplinary specialization, they were unable to answer it.

The 68ers: 1970–1990

The next generation entering the sociological field were the baby-boomers born in the first postwar years (between 1940 and 1960), also known as the *68ers*, as I henceforth will call them. In connection with the institutionalization of the Norwegian sociological field, I will emphasize two consequences of this generation. To begin with, they transformed sociology from a small discipline to a *mass discipline*. In 1964, there were 40 registered sociology students at the University of Oslo, whereas in 1972 the number was close to 400, a tenfold increase (Førland, 2006: 154, diagram 3). Thus, measured in number of students, the 68ers made sociology one of the biggest and most popular disciplines at the University of Oslo. This was partly a consequence of the educational revolution making tertiary education available to a very large cohort of baby-boomers. Nevertheless, why did so many of them chose sociology?

This brings me to the second consequence: the *totalizing* perspective. In Norway and all across the Western world, 68ers revolted against "established" society. In order to change it, they also needed to understand society and its problems. Hence, many of them chose to study sociology, the science of society. In the sociological field, rebellious 68ers encountered and revolted against the "establishment" of golden age sociologists. One expression of this revolt was the need for a more critical theory of society (of which more later). Another, more significant for my argument here, is that they wanted to overcome the intellectual fragmentation inherent in sub-disciplinary specialization. Why? Because in order to change society, you need an overall conception of society, in particular how the parts (subsystems) interact, and so they invoked one of their favorite terms: "totality" (Jay, 1984).

What kind of totalizing model did they produce? In order to overcome the lack of an institutional and subsequently intellectual center in Norwegian sociology, many 68ers observed society through the elite/people distinction. Hence, they claimed that Norwegian society consists of an elite "up there" that by means of economic, cultural, political, social, and cultural resources dominates – i.e., repress, exploit, and alienate – ordinary people "down here." They laid out the elite/people distinction in several ways. Most common was a Marxist framework in which capitalists dominate workers. Other 68ers chose a more feminist (men versus women), postcolonial (the West versus the Rest), and anti-positivist (technocratic elite versus lay people) framework or some combination thereof. Nevertheless, what united the 68ers was the attempt to overcome sub-disciplinary specialization by producing a totalizing model of Norwegian society based on the elite/people distinction.

Hence, at first look the 68ers seem to contradict my thesis about sub-disciplinary specialization. Indeed, at the level of manifest intentions, what this generation brought to bear on the sociological field was the deliberate attempt to overcome sub-disciplinary specialization by means of a totalizing model of Norwegian society. However, due to inherent weaknesses in their totalizing endeavor, they ended up discrediting the attempt to overcome sub-disciplinary specialization by "grand" or "totalizing" sociology. What I have in mind are three weaknesses that are particularly salient and that became subject to much debate.

The first is that the totalizing models produced by the 68ers were simply *too simple*. A modern society such as Norway is too culturally, institutionally, and psychologically differentiated and complex to be subsumed under the hierarchical elite/people distinction, be that in a Marxist, feminist, anti-positivist, or postcolonial version or some combination thereof. Due to horizontal (functional) institutional differentiation, Norwegian society consists of many basic institutions (the economy, politics, religion, science, education, media, law, etc.) that work according to different logics that are not captured by the simple vertical elite/people distinction (Hagen, 2006: chapters 4–5). Moreover, different basic institutions contain different elites that often disagree on important issues, so there is no unified and coherent "power elite" (Mills, 2000) dominating ordinary people (Guldbrandsen et al., 2002).

Secondly, there is the problem of *ideology*. According to many 68ers, science is always ideological in the sense that it by choice or accident works for the benefit of some groups in society and against the interests of others. Hence, the question is not whether sociology is political but "whose side are you on?" to quote Howard Becker's (1967) influential 1967 paper. According to many 68ers, the answer to this question was easy: sociology should be on the side of "the people." Hence, their attempt to create totalizing models of Norwegian society was often designed as tools in the hand of oppressed workers, women, lay people, or postcolonial countries. This made them look scientifically dubious, having more to do with ideology and politics than science, subject as much to the instrumental standard of political efficiency as the rigorous and disinterested scientific standards aiming at truth such as universalism, disinterestedness, and organized skepticism (Merton, 1973: 270–278).

Finally, there is a problem of *methods*. Many 68ers seemed to be of the opinion that a few master thinkers had penetrated the depths of society and found the ultimate causal factor behind the "surface" of empirical observations that explains more or less all outcomes across society, be that factor capitalism, patriarchy, technocracy, or colonialism. However, these fundamental insights lay scattered around in books and publications often in a somewhat confused form. Hence, instead of an empirical approach, many 68ers preferred in-depth exegesis of the works of, for instance, Marx, Engels, Lenin, Mao, Marcuse, Adorno, de Beauvoir, or Fanon in order to construct a "deep" model of society. Consequently, it was easy to dismiss totalizing sociology as non-empirical and speculative – as "metaphysics," "philosophy," or even "religion."

To sum up, even though the 68ers wanted to overcome sub-disciplinary specialization, they ended up discrediting it by the lack of complexity, ideological bias, and weak empirical founding of their totalizing models. Who did the debunking? Partly they did so themselves. It is notable how effortlessly many of them soon abandoned their totalizing project and became part of the system of sub-disciplinary specialization. However, their totalizing approach was also targeted out as a main object of critique by a new generation of sociologists entering the sociological field.

Generation X: 1990 to the Present

This brings me to what in the literature on generations is often called *Generation X*, after Douglas Coupland's novel by the same name, the generation to which this author belongs (born between 1960 and 1980). In Norway, this generation (sometimes called "the ironic generation") reacted against what they saw as the totalitarian and authoritarian political and intellectual excesses of the 68ers. Most notably, many of the most talented and vocal of them belonged to a Maoist party (AKP m-l) the goal of which was to replace democratic rule of law with a Leninist one-party rule through a violent revolution (Sjøli, 2005).

If we look more closely at the sociological field, Generation X initiated a widespread reaction throughout the intellectual field against the totalizing intellectual models of the 68ers and in particular Marxism. Often inspired by some version of postmodernism or poststructuralism (Foucault, Derrida, Lyotard, Deleuze, Guattarri, Feuerabend, Bauman, Latour, Kristeva, Butler, etc.), the aim was to "deconstruct" the totalizing theories of the 68ers along the lines depicted above (simplicity, ideology, and methodology; in addition it was often pointed to the inherent totalitarian political implications of totalizing models of man, society, and history). The French philosopher Lyotard (1984) summarized the essence of this critique well in his phrase "incredulity toward meta-narratives." Hence, members of Generation X wanted to replace the discredited "grand" and "totalizing" intellectual systems of the 68ers with a multitude of small and local narratives, theories, and perspectives. Much of postmodernism and poststructuralism never got a strong foothold in Norwegian sociology. However, the "credulity toward metanarratives" left an important mark: it discredited the totalizing sociology of the 68ers. It also resonated strongly with sub-disciplinary specialization. Hence, with Generation X we are back where we started: sub-disciplinary specialization.

Dialectic, Circle, or Spiral?

Let me briefly summarize my genealogical explanation of the most salient institutional feature of Norwegian sociology, namely, sub-disciplinary specialization. During the first two postwar decades, the golden age generation institutionalized Norwegian academic sociology as a loosely coupled set of specialized subdisciplines analyzing delimited parts of Norwegian society. The 68ers revolted against this sub-disciplinary specialization, but failed, discrediting totalizing models of society. Finally, Generation X with their "incredulity toward meta-narratives" did the discrediting, embracing a multitude of small narratives. Hence, due to a combination of path dependency and discredited alternatives, the golden age sociologists have laid down the institutional rules and regularities that still rule the Norwegian sociological field.

What kind of pattern does this historical process exhibit? One much used model of institutional and intellectual change is the *dialectical* model: thesis, antithesis, and synthesis (Elster, 1985: 37–48). In the case of the history of Norwegian sociology, a dialectical interpretation would look something like this. First (thesis), sociology was established as an academic discipline by sub-disciplinary specialization providing a scientific basis. Next (antithesis), totalizing models counter-acted the intellectual fragmentation inherent in sub-disciplinary specialization, on the cost of simplicity, methodological flaws, and a speculative bias. Finally (synthesis), sociology develops into a mature discipline that combines empirical specialization with overall models of society (synthesis). However, if my analysis is sound, this is not the case. We find the thesis (specialization) and the antithesis (totalizing models), but not the synthesis. Instead, Generation X's deconstructive "incredulity toward meta-narratives" has brought back fragmentation. Thus, we are back where we started: sub-disciplinary specialization. So perhaps what we have here is an instance of an institutional and intellectual *circle*? Indeed, the circle fits the facts much better than the dialectical model. Yet it cannot accommodate the fact that Norwegian sociologists today are much more theoretically and empirically sophisticated than the golden age generation. In short, there has been progress. For this reason, I think the *spiral* best captures development of the Norwegian sociological field. We *are* more or less are back where we started, only with the benefit of more data, more sophisticated methods, more theories, more researchers, and so on.

What Is Wrong with Sub-disciplinary Specialization?

I now turn from description to evaluation. Sub-disciplinary specialization has important benefits. To begin with, specialization is a significant instrument for generating valid and reliable knowledge of any empirical domain of some complexity, like Norwegian society. Hence, it has made it possible for Norwegian sociologists to produce a vast body of systematic empirical knowledge of Norwegian society, as illustrated by the 1200 pages of the last edition of the collective anthology *The Norwegian Society* referred to above (Frønes & Kjølsrød, 2017). Consequently, Norwegian sociology has been an important part of the Norwegian modernization project, bringing "light" (systematic knowledge) where there used to be "darkness" (lack of systematic knowledge). Indeed, Norwegian sociologists have ended "poetocracy" (Skirbekk, 1970: 7–8). Instead of artists using their artistic intuition and imagination to produce novels and dramas of Norwegian society based mostly on personal experience and anecdotal evidence (not to speak of traditional, mythical, and religious knowledge), sociologists have applied empirical methods and social theory in order to produce a body of much more empirically reliable and theoretically consistent knowledge. Finally, as we will see later, specialized sociological knowledge has also been fed into political reforms in postwar Norway. However, sub-disciplinary specialization also has some costs, of which I will address four.

The first is what I will call the *historical problem*. As any introductory book in sociology will tell you, sociology arose in the second half of the nineteenth century as the academic discipline that put the emerging modern – industrial, capitalistic, bureaucratic, democratic, urban, differentiated, individualized, etc. – society under the microscope. Thus, if modern political ideologies like anarchism, communism, conservatism, fascism, liberalism, and socialism are answers to the *practical* challenges of modern society, sociology addressed the *intellectual* challenge of understanding the social transformation from traditional to modern society. Hence, sociological classics such as Marx, Weber, Simmel, and Durkheim raised the question: What characterizes a modern social order? Answering this question obviously entailed dissecting the parts of society, but it also entailed addressing the more ambitious and totalizing question of what kind of society these parts add up. Here, the classics gave different answers, emphasizing capitalism (Marx), rationalization (Weber), division of labor (Durkheim), and individualization (Simmel). In the subsequent postwar period, postclassical sociologists have provided new answers, emphasizing post-industrialism (Bell), functional differentiation (Luhmann), risk (Beck), social acceleration (Rosa), liquidity (Bauman), deconstruction of metanarratives (Lyotard), reflexivity (Giddens), bureaucratic and capitalist colonization of the life-world (Habermas), and network organization (Castells). Despite different answers, the question has remained the same: What constitutes a modern society? Yet, sub-disciplinary specialization not counteracted by institutional or intellectual integration makes it very hard for Norwegian sociologists to answer it. Hence, due to sub-disciplinary specialization, Norwegian sociologists fail to live up to the disciplinary tasks of sociology as historically conceived. As the golden age political sociologist Stein Rokkan says: "Sociology had aspired to the status of the widest ranging, the most general of the social sciences, but ended up as the most fragmented of them all: the discipline wanted to pose as the queen, it ended up as the clown of the social sciences" (Crawford & Rokkan, 1976: 9).

The second challenge I will call the *epistemic problem*. Implied in our very conception of understanding and insight is the idea of totality. Thus, we understand a phenomenon more satisfactorily if we not only dissect its parts but also address the way the parts interconnect and analyze the kind of totality – emergent or not – they constitute. This is why Kant (1996: 617–636) called totality a "regulative idea," something knowing subjects always strive toward, although they can never fully achieve it. To give an example from biology, we understand an organism much better if in addition to information about its separate organs such as the heart, brain, kidneys, and blood vessels, we also know how these organs work together. The same goes for sociology. We achieve a much deeper and better understanding of a particular society if we in addition to detailed information about its parts also know how these parts interconnect. However, due to fact that Norwegian sociologists only address the separate parts of society, they have little or nothing to say about to what kind of society the parts add up. Hence, sub-disciplinary creates an epistemic obstacle.

Then there is what I will call the *cultural problem*. As already noted, any modern society such as contemporary Norway is highly institutionally and culturally complex. In addition, there is the challenge of the "new obscurity" (Habermas, 1985) created by the transition of the last 30–40 years from the "old" industrial modernity to the "new" postindustrial, postmodern, liquid, reflexive, individualized, etc. social order in the West. In such a novel situation, there is an urgent need for comprehensive intellectual tools to navigate the present. In particular, there is a need for overall models of society to make sense of the present, and this is what Norwegian sociologists due to sub-disciplinary specialization are unable to provide. Hence, they cannot counteract the "fragmentation of consciousness" (Habermas, 1987: 355) – a widespread feeling of disorientation, alienation, and lack of control – that at present characterizes and even threatens Western liberal democracies.

Finally, there is the *political problem*. Many of the most vexing problem Norwegian society currently faces, such as consequences of climate change and other environmental problems, the end of the oil economy, an aging population, integration of non-Western immigrants, increased social inequality, challenges to the welfare state, and so on, are systemic. That is, both their origin and possible solutions relate to how several subsystems of society such as the economy, politics, the family, education, science, law, media, and religion interact. Hence, they call out for exactly the kind of totalizing knowledge of institutional interdependencies that Norwegian sociologists due to sub-disciplinary specialization cannot offer.

If these four arguments are sound, sub-disciplinary specialization creates a need for synthesizing knowledge currently not met in Norwegian sociology.

Critical Sociology[1]

So far, I have presented and criticized the most salient institutional feature of Norwegian sociology. The second characteristic I now want to look at concerns *intellectual* content. That is, apart from breaking the study of Norwegian society apart into demarcated studies of its constitutive parts, what kind of knowledge do Norwegian sociologists want to produce? In one way, the answer is easy: they want to know how social interaction is organized. This answer is inspired by Weber's (1978: 4) influential definition of sociology as the study of social action: as the study of one or more human actors who relate to the past, present, and/or future actions of one or more other actors. Yet as Weber was perfectly aware of, it is impossible to study "everything social," as one would simply drown in complexity. Hence, sociologists need some criterion to pick out which cluster of social actions to study and from which perspective.

Now, the previous point about sub-disciplinary specialization seems to imply that there can be no such criterion or even collective "project" orienting Norwegian sociology. In one sense, I think this is true. As I have argued, there is no institutional center coordinating the efforts of Norwegian sociologists. To quote Yeats: "Things fall apart; the center cannot hold." On the other hand, and to keep quoting Yates,

[1] This presentation and critique of critical sociology is based on a previous Norwegian publication (Aakvaag, 2018a).

the consequence is not that "mere anarchy is loosed upon the world." Rather, I will argue that in Norway, many – definitely not all – sociologists located within operationally closed sociological sub-disciplines have been guided by a common idea of what sociology should be and what kind of knowledge of society it should aim to produce. This is where critical sociology enters the picture.

Before I back up this claim with empirical evidence, I will briefly describe what I mean by critical sociology. I define critical sociology as a general framework for doing sociology that to a greater or lesser extent can influence how sociologists conduct their research. In what follows, I will present a Weberian ideal type (Weber, 1949: 90–101), by which I mean a theoretical construct ("ideal") that stylizes what is distinctive and typical ("type") of critical sociology. Such ideal types have never existed empirically in pure form. Rather, they are a theoretical "harbor" that makes it possible to "navigate in in the vast sea of empirical facts" (Weber, 1949: 104). That is to say, they create a theoretical framework for conducting empirical sociology: selecting, framing, and analyzing empirical phenomena. I develop the ideal type of critical sociology based on the approach Weber himself frequently took when constructing his own ideal types, namely, historical studies of the phenomenon in question.

So, what is critical sociology? In a broad sense, all sociology is – or at least ought to be – critical, namely, by reflexively discussing and criticizing all stages and elements going into the research process, such as the questions one asks; the methods, data, and theories one applies in order to analyze them; and the answer one comes up with. In this chapter, however, I use the term critical sociology in a more restricted sense, namely, as being *critical of society*: identifying and analyzing social problems and dysfunctions. In its ideal-typical form, critical sociology is founded upon three "pessimistic" pillars.

The first is *societal pessimism*. All sociology is based on some preconception of "where the action is" (Goffman, 1969) in society. Critical sociology starts out from the idea that society (typically a modern society) is full of problems: people are being repressed, exploited, alienated, dominated, marginalized, excluded, and disciplined and so on. This is the "synthetic a priori" (Kant, 1996: 55–59) of critical sociology: substantial knowledge of society obtained prior to (empirical) experience. That is to say, critical sociologists "just know" that society is full of problems.

This brings us to the second pillar, *methodological pessimism*. Hence, if society is full of problems, an urgent task of sociology must be to detect and articulate them. To this corresponds a particular methodological ideal, namely, sociological disclosure. It goes like this. Society is full of problems. However, social elites create and sustain ways of hiding, naturalizing, and legitimizing them by means of ideology, hegemony, discourses, epistemes, symbolic power, etc. Critical sociology debunks such legitimating narratives by tearing the veil apart and disclosing how society "really is" – exposing all the problems. (Alternatively, in cases where the elites in order to stay in power foster a culture of fear by exaggerating threats to society, critical sociologists provide evidence to the contrary.) Importantly, methodological pessimism is not pessimism on behalf of traditional sociological methods. On the contrary, critical sociologists apply the full range of qualitative and quantitative methods in order to disclose problems in society. Rather, methodological pessimism is a goal orienting the practice of sociologists, urging them to go about identifying and analyzing social problems and dysfunctions.

This brings us to the third pillar, *theoretical pessimism*. If sociologists are to disclose social problems, they need theoretical tools to do so such as concepts, typologies, classifications, analogies, metaphors, mechanisms, and models. Indeed, anyone familiar with the discipline will recognize that the sociological toolbox is full of such tools: alienation, exploitation, repression, marginalization, exclusion, discipline, patriarchy, panoptical surveillance, governmentality, biopower, anomie, tragedy of culture, cultural contradictions, symbolic power, symbolic violence, greedy institutions, colonization of the life-world, systematically distorted communication, patriarchy, etc.

The Origin of Critical Sociology

So far I have presented critical sociology as a general framework for conducting sociological research. My empirical claim is that in Norway, critical sociology has a strong standing. Even though far from all Norwegian sociologists have been critical of society, many have. Moreover, Norwegian sociologists have not developed an alternative sociological framework. Hence, to the degree that Norwegian sociology despite sub-disciplinary specialization and lack of institutional integration is a collective project, and I will argue that to some degree it is, it is one inspired by critical sociology. To be more specific, what postwar Norwegian sociologists have aimed for more than anything else is disclosing social dysfunctions: to detect, analyze, and expose the problems of Norwegian society. This is a strong claim, and I will seek to substantiate it using the same framework as I used in connection with my empirical analysis of sub-disciplinary specialization, namely, approaching Norwegian sociology as a social field in which members of different generations fight over the rules of the sociological game.

The Golden Age Sociologists

As already pointed out, the golden age generation founded Norwegian sociology as an academic discipline in the first two postwar decades. In addition to institutionalizing sociology as a set of loosely couples specialized sub-disciplines, they also established Norwegian sociology as a predominantly critical discipline. To see why, we need some broader contexts.

The period 1945–1965 has been called the “golden age” also of Norwegian social democracy (Sejersted, 2005: 199). In these years, the social democratic labor party, with absolute majority in parliament from 1945 to 1961, rebuilt Norwegian society after years of class conflict in the interwar period and 5 years of German occupation. The *goal* was egalitarian affluence, i.e., to create a welfare society that would produce wide-ranging economic, cultural, and social opportunities for all members of society, not just the elite. The *means* was capitalist industrial modernization creating wealth combined with an organized labor market protecting workers and a welfare state redistributing the wealth. In addition, the narrative superstructure orienting and legitimizing this project was one of progress and social inclusion, namely, that everyone *should* be included in affluent society and that more and more actually *were* included.

The golden age sociologists reacted against this narrative and produced their own counter-narrative: "things are not going as well as we like to think." Why not? Because not everyone *are* included, and many of those being included face *adverse* consequences. Put differently, the golden age sociologists wanted to identify and analyze groups of individuals that were either not included in the social democratic modernization project or negatively affected by it. This could be workers exploited and alienated in capitalist plants, women still subject to traditional patriarchal gender roles, local communities threatened by centralization, prisoners isolated from society, pupils subjected to discipline in classrooms, class biases in the legal system, etc. In this way, the golden age sociologists founded Norwegian sociology as a critical discipline. Indeed, as research on this generation emphasizes, golden age sociologists conducted "regime opposition in the labor party stat" (Slagstad, 1998: 371–392) and was the "bad conscience of the welfare-state" (Mjøset, 1991: 155). Not for nothing did Vilhelm Aubert (1969: 192–224), the primus inter pares of the golden age sociologists, label this sociological project "problems-oriented empiricism" – the empirical investigation of social problems.

Hence, the golden age sociologists founded Norwegian sociology not only as a specialized but also as a critical discipline. Importantly, however, they were critical not only *of* but also *for* society. That is, they wanted to inspire reforms that would include the groups they discovered into the enabling web of a democratic welfare society.

The 68ers

Not very surprisingly, the highly rebellious generation of 68ers tightened the critical screw (Mjøset, 1991: 187–193). Based on some version or combination of Marxism, feminism, postcolonialism, and anti-positivism, they conceived of society as a system in which an "elite" consisting of capitalists, men, the West, and/or technocrats repressed a "people" consisting of workers, women, the postcolonial world, and/or lay people. Furthermore, where the golden age sociologists wanted to reform the social democratic system from within by incremental improvement, many 68ers viewed Norwegian society as basically flawed. Consequently, radical societal change – perhaps even violent revolution – was needed in order to subvert it. Thus, both the diagnosis and medicine provided by this generation were more radical than that of their predecessors.

Generation X

Finally, Generation X brought the linguistic turn to bear on critical sociology. This manifested itself mostly in two ways. The first is the postmodern/poststructuralist way, inspired by social thinkers such as Foucault, Derrida, Lyotard, and Kristeva. Thus, Norwegian sociologists from this generation frequently looked at language and other symbolic systems ("discourse") as founded upon binary oppositions such as man/woman, west/rest, expert/layman, etc. Such symbolic oppositions, moreover, *rank* people by connecting them to other binary oppositions, such as active/passive, reason/emotion, mind/body, distinguished/vulgar, high/low, etc. Finally, they also lay down rules of normality that subject people to discipline. In this way, there is a symbolic dimension to oppression that infuses even the most ordinary social practice with power and hierarchies. The second way is a more mainstream sociological one, inspired in particular by Bourdieu (1984) and the "cultural turn" in class analysis that he invoked (see Weininger, 2005). The main point here is to analyze how culture and lifestyles draw symbolic boundaries between classes, thus generating "symbolic power" or even "symbolic violence" that is partly an independent form of social domination and partly supports and strengthens other forms of power, dominance, and repression in a seemingly egalitarian welfare society. The practical implication of this generation's brand of critical sociology was typically some version identity politics, that is, fighting for the symbolic recognition of the identities of groups such as women, the disabled, gay people, ethnic and racial minorities, and so on.

What Is Wrong with Critical Sociology?

Let me briefly summarize. Because of the intellectual impulses spread throughout the sociological social field by the three founding generations, the program of critical sociology has a strong standing in Norwegian sociology. Indeed, in so far as we can talk of a common project across the institutionally and intellectually loosely coupled sub-disciplines of Norwegian sociology, it is critical sociology. Thus, many Norwegian sociologists consider it crucial to identify groups of individuals excluded from or negatively affected by the social democratic modernization project, how capitalists, men, the West, and a technocratic elite dominates, represses, and alienates workers, women, non-Western societies, and lay people, and to disclose more hidden and subtle forms of symbolic domination in a supposedly egalitarian Norwegian society. To paraphrase Dr. Relling from Henrik Ibsen's play *The Wild Duck*: "Rob the average Norwegian sociologist of his critical orientation, and you rob him of his happiness at the same stroke."

The framework of critical sociology has in many ways served Norwegian sociology well. It has produced important new knowledge about Norwegian society and its social problems in particular. It has ignited much public debate. It has alerted politicians about social problems and sparked political reform to ameliorate them. It has attracted students and provided funding to sociology. Finally, it has given sociologists a clear identity both internally and externally. Nevertheless, there is also a backside to critical sociology. Here I will address three challenges.

The first has to do with *objectivity*. One crucial aspect of objectivity is truth; that sociological claims about society correspond to how society actually is. However, an infinite number of true sociological propositions can be made about any society (which does not mean that any proposition about a society is true). Therefore, we need to ask *which* truths about society sociologists should seek. Here we encounter an important aspect of objectivity, namely, to provide a *balanced* view of society. To be objective relating to an issue, we need to give equal weight to all sides of the matter (Føllesdal, Walløe, & Elster, 1996: 314). For example, we would not call a judge objective if she/he did not listen equally to both sides in a trial (which does not mean that she/he must give equal weight to all arguments, only that she/he must be equally open to arguments from both parts). Similarly, sociologists need to look at all (important) aspects of society. This is where critical sociology fails. Due to critical sociologists' ambition to detect, analyze, and criticize social problems, they are disproportionally interested in social dysfunctions and leave out much that works well. As a result, the picture critical sociology paints of society and its parts is pessimistically biased. If one only considers social problems, one will portray society in an imbalanced way. This is a general argument against critical sociology, pertaining to whichever society a sociologist puts under her microscope. It is, however, particularly salient in a Norwegian context.

This brings me to a second, related problem of critical sociology, having to do with *historical trends*. The degree to which critical sociology is objective – balanced and unbiased – is partly a function of how well organized the society in question is. Thus, the more the social problems, the more objective is critical sociology, providing an apt and balanced view of society. Vice versa, the more well working a society is, the less objective is critical sociology (although it is still important to detect remaining dysfunctions in functional societies). This brings us to Norway. When the golden age sociologists founded Norwegian sociology in the first two postwar decades, there were many social problems for sociologists to disclose, analyze, and criticize after years of conflicts and social unrest in the interwar period and 5 years of German occupation. (Yet we must add that much was already working well after 150 years of political, economic, social, and cultural modernization.) However, throughout the postwar period, Norwegian society has been through a period of unparalleled development, taking an egalitarian way to affluence. A wide array of opportunities and capabilities have been created and distributed to most of the population. This progress is manifested in indexes which invariably sees Norway perform very well in both absolute and relative terms on such measures as affluence, equal income distribution, educational level, gender equality, social mobility, happiness, democracy, rule of law, human rights, freedom of the press, freedom of expression, lack of corruption, and trust (see Aakvaag, 2018a and Barstad, 2014 for overviews). Hence, in terms of outcomes, Norway is a "successful society" (Hall & Lamont, 2009). Accordingly, Norwegian critical sociology has become progressively less objective as Norwegian society has gotten better. Indeed, a sociological task second to none in importance in my view is to describe and explain the positive development of Norwegian society. Unfortunately, critical Norwegian sociologists wanting to disclose social problems are for systematic reasons unable to do this. As a result, in a time when the rest of the world "looks to Norway" and *The Economist* puts "The Nordic Supermodel" on its front cover, Norwegian sociologists have little else to offer than its pessimistic counter-narrative: "Norwegian society is not as successful as you think." Of course, Norwegian society is not perfect, but it is probably one of the most successful and well-organized societies in the history of humanity, at least if using egalitarian affluence as our yardstick. Norwegian sociologists should have much more to say about this.

Finally, there is also a *policy problem* inherent in critical sociology. Obviously, if one wants to improve society, empirical knowledge about dysfunctional aspects of its institutions is necessary. Why? Because it is hard to solve problems about which you know little or nothing. However, critique it is not enough. To see why, let us take Kant's (2004: 54) famous definition of enlightenment as our starting point. Kant defined enlightenment as "Man's emancipation from self-inflicted immaturity," that is, as increasing individual freedom by means of improving social structures. Beginning with the golden age sociologists, who were inspired both by radical European cultural modernism (Thue, 1997: 180–187) and not least the progressive American vision of a free democratic social order (Thue, 2006), most Norwegian sociologists see themselves as part of this project, wanting to use sociology as a tool for empowering in particular weak social groups. However, the enlightenment project as conceived by Kant has two dimensions: a *negative*, criticizing social structures constraining individual freedom, and a *positive*, analyzing social structures enabling individual freedom. Both are equally necessary. Without "negative" knowledge, we would not know *what* to improve, whereas without "positive" knowledge, we would not know *how* to improve it. Nevertheless, critical Norwegian sociologists have one-sidedly devoted much more time and energy to the critical/negative task than the constructive/positive. Consequently, they have not been able to supplement their critique of social problems with sociological knowledge about which institutions works well, what it means that they work well, why they work well, how they originated historically, how to improve them, and how to extrapolate their enabling mechanisms to other institutions. Norwegian sociologists do not seem to mind this situation and often cherish their role as an "oppositional science" (Slagstad, 1998: 371–392) adversely related to policy makers, "power," and "the system." However, it makes their critique practically impotent – an empty gesture – and leaves the "positive" tasks of social science to other disciplines such as economics, political science, and psychology. In short, it is not enough to be critical *of* society (disclose problems); sociologists must also be critical *for* society (recommend solutions).

Discussion: Is There an Alternative?

This chapter has analyzed important aspects of the institutional and intellectual situation of Norwegian sociology. I have described what I take to be the two most salient characteristics of postwar Norwegian sociology, namely, sub-disciplinary specialization at the level of institutional organization and critical sociology at the level of intellectual ambitions. By conducting a sociological analysis applying a combination of social fields and generations, I have argued that the current situation is the result of the collective efforts of three generations of postwar Norwegian sociologists – the golden age sociologists, the 68ers, and Generation X – competing to define the rules of the sociological game. I have also assessed the two characteristics, pointing to their benefits but even more their intellectual costs, such as the historic, epistemic, cultural, and political problems of sub-disciplinary specialization and the challenges of objectivity, increasing irrelevance, and policy impotency confronting critical sociology.

If this critique is valid, what could be done to improve the current situation? To answer this question, we first need to address the forces that sustain status quo. In my view, two are especially important. The first is *path dependency*: the significance of history and sequencing (Pierson, 2004), that is, the importance of being first and laying down the premises and rules of the game of sociology for subsequent generations. Hence, by being first, the golden age sociologists put Norwegian sociology on a particular path that has been hard to change for subsequent generations due to such mechanisms as career opportunism, academic socialization (golden age sociologists selecting themes of courses and curriculum, being supervisors and role models, etc.), and founding fields of scientific fields of enquiry (structuring their thematic, theoretical, methodological, and empirical assumptions). Not much can be done about this. The past is as it is, although it can be more or less reflexively appropriated and discussed, for instance, by calling attention to its problems, as I have done in this chapter. The other mechanism is a *lack of perceived alternatives*. Here, more can be done. Hence, as a constructive addition to my critique of sub-disciplinary specialization and critical sociology, in this concluding discussion, I will present, in two steps, an alternative. First, I will quite programmatically present two ways of doing sociology that partly supplement and partly challenge sub-disciplinary specialization and critical sociology. Then I will illustrate what such a program might look like in practice. As the main goal of this chapter is to analyze and assess the current situation of Norwegian sociology, not to discuss alternatives, I will be quite brief. Readers interested in more details can look them up in the references.

Synthesizing Sociology: A "Grand Theory of Modernity"

Let me begin with sub-disciplinary specialization. This institutional feature has enabled the production of a vast body of detailed knowledge about the parts of Norwegian society. However, lack of coordination has obstructed the production of knowledge about the interaction of these parts and the kind of societal totality to which they add up. Although institutional problems might seem to call for institutional solutions, I think sub-disciplinary specialization is here to stay – for good and bad. Thus, I will not suggest an institutional reorganization of Norwegian sociology, which is highly unlikely anyway. Rather, I will suggest an intellectual solution in the form of a particular form of sociological theory that transcends and integrates the detailed but fragmented knowledge of sociological sub-disciplines: a *Grand Theory of Modernity* (hereafter a GTM).

A GTM is defined as an empirically founded, theoretically articulated totalizing model of contemporary society (see Aakvaag, 2013). Let me briefly elaborate on the elements going into this definition. Empirical founding means that a GTM is backed up by empirical studies. Due to its comprehensiveness, however, one must often rely on preexisting empirical research covering a vast array of different social fields and phenomena (Rosa, 2005: 56–57). Hence, dialogue with specialized sub-disciplines that provide such knowledge is essential. Theoretical articulation implies the need to develop totalizing concepts, metaphors, analogies, typologies, models, and mechanisms that depict society as a whole – such as functional differentiation, rationalization, and individualization. Put differently, it is not enough to "stretch" middle-range theory developed to analyze the parts of society to understand society as a whole. That a GTM is totalizing is its most salient feature. Instead of describing delimited social subsystems and phenomena, a GTM aims at society as a whole. A GTM is, furthermore, not a 1:1 representation of society. It is a model. Like any Weberian ideal type, it emphasizes what is typical of a particular society, its constituting "structural properties" (Giddens, 1984: 17). Finally, a GTM depicts contemporary society, that is, the society we live in today. For Norwegian sociologists, this typically means current Norwegian society.

In connection with assessing the historical problem of sub-disciplinary specialization above, I argued that creating GTMs has been a central concern of the sociological tradition from the classics up until today. Nonetheless, due to sub-disciplinary specialization, Norwegian sociologists have not contributed much to this tradition, as illustrated by my discussion of the anthology *The Norwegian Society*. Hence, one way to overcome the historical, epistemic, cultural, and political challenges of sub-disciplinary specialization is by means of much stronger synthesizing ambitions. That is, Norwegian sociologists would benefit from cooperating in order to develop a GTNS: a *Grand Theory of Norwegian Society*. This effort would require institutional and organizational backing, such as research groups, funding, journals, seminars, workshops, and the like. However, it is primarily an intellectual task requiring stronger totalizing ambitions from highly specialized Norwegian sociologists.

Positive Sociology

I turn next to critical sociology. I have argued that as far as an overall intellectual goal can be found among Norwegian sociologists, it is to be critical of society: disclose and analyze social problems. Even though this program has been beneficial for sociology, it also exhibits shortcomings: lack of objectivity, increasing irrelevance, and political impotence. Consequently, what can be done to ameliorate the shortcomings of critical sociology? An obvious answer is to develop what I will call a *positive sociology*, which is what we get when we turn the three "pessimistic" pillars of critical sociology on their head (see Aakvaag, 2018a).

Societal optimism In contrast to the societal pessimism of critical sociology, the basic presumption of positive sociology is that in all societies there are some sectors, institutions, organizations, practices, groups, and the like that work well, that is, to the benefit of most members of society. In fact, in an affluent modern welfare society like Norway, it is reasonable to assume that a lot of the social structure works well. This is the background assumption of society regarding "where the action is" that guides sociological research.

Methodological optimism If the presumption of at least some functional social structure is reasonable, then an important sociological task is to find it and study it. Hence, the practical impetus of positive sociology is to *identify* well-working aspects of society, describe what it *means* that they work well, disclose their *historical origin*, work out how they can be *defended*, and look at ways to further *develop* and *extrapolate* them to other arenas of society.

Theoretical optimism Finally, to conduct a sociology of the enabling aspects of society, sociologists need optimistic theory, that is, concepts, metaphors, analogies, typologies, models, and mechanisms, that makes it possible to analyze successful practices, institutions, and societies.

Due to the strong position of critical sociology, such a positive sociology does not yet exist in Norway (see Aakvaag, 2018a for some minor exceptions). As Norway is currently one of the world's most successful societies, I think that for both epistemic and political purposes, it is an urgent sociological task to unlock the key to this success. To amend the situation, I once more think what is needed is not primarily institutional changes but a "positive" reorientation of intellectual ambitions among at least a subset of Norwegian sociologists. Such a positive sociology would make it possible for Norwegian sociologists to play a more constructive role, that is, not only to be critical *of* but also *for* society.

An Illustration: The Democratization of Freedom in Norway

What would such a "grand" and "positive" sociology look like? In order to answer this question, which brings me to the second part of this concluding discussion, I will very briefly outline a *positive grand theory of Norwegian society* based on my own ongoing research. Under the headline of the "democratization of freedom," it is developed to answer three questions that Norwegian sociologists due to institutional fragmentation and critical intellectual ambitions have yet to address properly.

The first question is descriptive: *What does the success of Norwegian society consist in?* Starting from empirical evidence that clearly suggests that Norway is a successful society (see Aakvaag, 2018a and Barstad, 2014 for overviews), my suggestion is the *democratization of individual freedom*. That is to say, in Norway the freedom to be in control of and thus responsible for one's actions and life throughout such important domains as education, occupation, job, spouse, friends, place of residence, worldview, cultural consumption, lifestyle, and politics is no longer an elite privilege. Whereas it used to be reserved for the aristocracy, bureaucratic elite, capitalists, and wealthy farmers, starting in 1814 and accelerating due to the "freedom revolution" (Sejersted, 2005: 518–520) of the last 30–40 years, the basic freedom to live one's own life is now enjoyed by more or less all members of Norwegian society, although, of course, not to the same degree.

The second question is explanatory: *What is the proximate institutional explanation for the Norwegian democratization of freedom?* That is to say, what institutional matrix enables it? My suggestion is a specific combination of three institutional features of the so-called Norwegian (or Nordic) model (see Aakvaag, 2018b). To begin with, there is the "horizontal" principle of *functional institutional differentiation*. That is to say, in Norway several relatively independent basic institutions such as education, religion, science, health, civil society, law, the economy, art, and sports produce collective goods such as education, health services, jobs, consumer goods, opportunities for civic engagement, aesthetic experiences, and so on for members of society. Next, there is the "vertical" principle of *political regulation*. Thus, in Norway a democratic political center upholds and coordinates the efforts of the basic institutions: subsidize and regulate them (although on an arm's length distance), with a particular emphasis on inclusion so that all members of society can enjoy the opportunities and capabilities they produce. Finally, there is the "liberal" principle of *constraints on power*. Hence, in order to protect the individual against the potential authoritarian threat inherent in strong institutions and a strong state, several constraints on power have been institutionalized, such as individual rights, rule of law, and democratic accountability in the form of periodic multi-party elections. As a result, most members of Norwegian society simultaneously enjoy three basic types of freedoms and capabilities (Nussbaum, 2011; Sen, 1999, 2009): formal freedom (civil and political rights), resource freedom (the resources that translate formal opportunities into actual opportunities and capacities), and pluralist freedom (a wide menu of available actions, practices, roles, lifestyles, life projects, and identities).

The third question is also explanatory: *What is the ultimate historical explanation of the Norwegian democratization of freedom?* My suggestion (se Aakvaag, 2017b) is a combination of luck and virtue. Through a lucky historical coincidence, Norway could in 1814 declare independence from its colonial master Denmark, who fought on the losing side in the Napoleonic wars. However, luck must be supplemented with political virtue and collective action. Thus, a liberal bureaucratic elite inspired by the American and French constitutions seized the moment in 1814 and set up a liberal constitution based on rule of law, human rights, and democracy transforming members of Norwegian society from subjects under an absolute monarch into citizens with equal protection under the law. National sovereignty immediately got lost due to a new union with Sweden (lasting until 1905), but the liberal principles of the constitution endured. Consequently, over the next two centuries, three broad social movements could mobilize the democratic power thus created to fight for the social inclusion of new groups through institutional reforms. In the second half of the nineteenth century, the *peasant movement* fought for the "people" against a bureaucratic elite. The outcome was parliamentary democracy: ruled by the people. In the first half of the twentieth century, the *labor movement* fought for the inclusion of workers. The result was a postwar egalitarian welfare state. Finally, from the late 1960s onward, the *women's movement* challenged patriarchy. The result was not only formal but also substantial gender equality (although the debate continues as to how substantial). Thus, in 200 years, a combination of historical luck and political-collective virtue transformed Norway from a backward agrarian elite society into today's liberal, democratic, affluent, egalitarian, and gender-equal welfare society where individual freedom has been democratized.

Final Remarks

This highly condensed and schematic presentation of what an alternative to the institutionally fragmented and intellectually critical postwar Norwegian sociology might look like could be expanded, criticized, and discussed in many ways. There are also other ways to challenge and supplement the institutional and intellectual status quo of Norwegian sociology than mine. However, I will leave these important questions for later. My aim here has only been to illustrate that there in fact *are* alternative ways of doing sociology. Thus, the theory of democratization of individual freedom just presented is both *synthesizing* (it presents an overall grand theory of Norwegian modernity, transcending the thematic borders of the sub-disciplines) and *positive* (it addresses the causes, nature, and consequences of the success of the Norwegian model). By including such "grand" and "positive" elements into the discipline, Norwegian sociologists can improve their ability to produce not only detailed and critical but also totalizing and constructive knowledge of what is obviously a very successful society. Hence, counteracting the institutional and intellectual fatalism and resignation of TINA ("there is no alternative"), as I have done here by providing an alternative, is an important first step for someone who, like me, is critical of the status quo of Norwegian sociology and propagates disciplinary change.

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Chapter 15
How Do Economists Think?

Jo Thori Lind

I argue that most economists pay little attention to *epistemological considerations and* developments in the philosophy of science when doing their research. Consequently, a number of philosophers of science have been critical to the status of knowledge in economics. Still, there are quite clear thoughts on how knowledge is generated within economics. In this chapter, I explain and discuss how economists think about gaining new insights about the world. I discuss the interpretation of formal economic models as well as the status of empirical research in economics. Finally, I discuss how economics and the other social sciences think about each other regarding the scientific status of the respective fields.

The Field of Economics

Economics is one of the oldest of the social sciences. As "the best use of scarce resources," the defining topic of economics, has followed humankind since its inception, economic thought may be said to be as old as humankind itself (Niehans, 1990: Chap. 3). Indeed, both the Bible and the writings of ancient philosophers including Aristotle contain economic insights. The scholastics of the Middle Ages were the first to reach the insight that there are regularities in economic behavior. This opened the gates to a meaningful scientific study of economics.

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The development of mercantilism in the seventeenth century is the first occurrence of a complete economic doctrine. Their proposition was that the success of a country was given by its trade surplus. Hence, a successful country should export as much as possible and import as little as possible, thereby accumulating wealth. The proponents of mercantilism were typically not researches as we find them in contemporary academia, but individuals having employment in business, banking, or as politicians as their main occupation.

Adam Smith, an ardent critique of mercantilism, may have been one of the first academic economists. Although his writing on economics is still seen as highly influential, he also worked in several other fields, particularly moral philosophy. Through the nineteenth century, the field of economics enters into universities in many European countries and starts acquiring a more academic nature. Still, it maintained close ties to philosophy, law, and public administration. This is also evident from the way economists do their research and express their views during this period. There are few attempts at the quantitative exercises or mathematical modelling that is common in contemporary economics. Distinctions between positive and normative statements are often not made clear either.

The field of economics experienced a paradigm shift starting in the 1930s. Parts of the shift were spurred by the positivist turn in large parts of academia. In this period, economists start using mathematical tools to build theories and models of the economy, largely having physics as a scientific ideal. At the same time, recently developed statistical tools are introduced to study economic phenomena empirically. The anticipation is that together, these tools would make it possible to discover the "laws of economics." This bold endeavor has subsequently been moderated, and contemporary economists do not believe there are any set of laws that can be discovered. Still, the way economists work and think has not seen any revolutions over the last 80 years. Rather, approaches and ways of working have been changed and amended little by little.

Economics has been close to both private business and public administration since its inception. During the post-World War II period, economists gain a particularly strong position in public administration in many Western counties. Countries start compiling national budgets and different versions of economic plans. Public spending and monetary policy is aimed at taming business cycles, inspired by the work of John M. Keynes. For several decades, these policies are successful. Toward the end of the 1960s, the idea that business cycles was a phenomenon that was "solved" was prevalent. During the early 1970s, however, a number of countries saw a large economic downturn combined with high inflation – the so-called stagflation.

The strong association between academic economists and governments has shaped some of the research practices in economics. Most developments in the field have been aimed at producing theories that are useful for understanding and running, or at least influencing, the economy. Consequently, little emphasis has been on developing economics as a "critical science." This is also partially driven by the fact that there is one dominant school of thought within the field, often referred to as "mainstream" or "neoclassical." There are other schools of thought, such as Marxist, Austrian, or post-Keynesian approaches (see Lawson (2006) for details). However, these are small and largely overlooked by the rest of the field. Mainstream economics is less monolithic than many critics claim, though. Ways of thinking and working differ between sub-disciplines. A labor economist and a macroeconomist would theorize and undertake empirical research in quite different ways. The same holds within some sub-disciplines as well, particularly macroeconomics. However, they would all acknowledge the others as mainstream economists.

Philosophy of Science in Economics

According to Fourcade, Ollion, and Algan (2015), there is an implicit pecking order within the social sciences with the economists at the top. Although some would object to this, it is a widespread belief among economists that their discipline is "more scientific" than the other social sciences. Colander (2005), for instance, report that 77% of respondents in a survey of economics students in top US graduate schools agree that economics is the most scientific of the social sciences.

One possible explanation for this high self-esteem could be that economists believe they have a better philosophical underpinning of their field than the other social sciences. However, if we look at the status of the philosophy of science within economics, this does not seem to be the case. Whereas scientific works in sociology and anthropology frequently contain discussions of how knowledge generation should be done with the question at hand, such discussions are non-existent in economics. With a few exceptions, the only discussions of the philosophy of science of economics can be found among heterodox criticisms of the field, such as the work of Caldwell (1994).

Although philosophical underpinnings are not discussed in economic publications, it would be wrong to claim that economists do not have any basis for their knowledge generation. Rather, many economists would claim that the foundations of economics are better than the foundations of the "softer" social sciences. Economists would claim that their field is close to the natural sciences and that generation of knowledge in economics can be based on the same foundation as biology or even physics. These fields are also characterized by an absence of reflection of questions related to their philosophy of science – except for the work of philosophers.

More contemporary turns in the philosophy of science, such as post-structuralism and post-modernism, have in periods seen popularity in many social sciences. Very generally, they have a low standing in economics.[1] Elster's (2012) critique of obscurantism in these approaches resonates well with the thinking of many economists, who would claim that the clarity of their mathematical language clearly surpasses the complicated language of the modern turns. To many economists,

[1] A rare exception is the volume edited by Cullenberg, Amariglio, and Ruccio (2001).

the Sokal affair[2] represents a definitive proof that "fancy language" is actually mostly bullshit and hence a challenge and not an asset for scientific practice.

Hence, it seems economists are mostly confident that their field has a sufficiently strong philosophical underpinning that they do not have to worry about these issues in their everyday work. If we turn our attention to scholars of the philosophy of the social sciences, it may seem that many would not agree. A typical argument would be that society is something different from physics, and although the tools of physics may work in physics, they certainly would not in the social sciences. Some hard liners would claim that it is meaningless to search for any truth, as all science is a reflection of the society it was produces in. But scholars with less extreme views are also critical to the underpinnings of economics.

The absence of philosophical reflection seems to be one justification for criticizing the field of economics. To substantiate the critique, a typical approach is to analyze economics textbooks from a critical point of view. This is a potentially fruitful approach as textbooks represent the part of the field passed on to future generations. If students are systematically thought old school approaches, new ideas would struggle to enter the field. Still, introductory textbooks never contain a complete picture of neither the field nor the way economic researchers think about their work. As introductory textbooks are introductory, they have to focus on the basics. Moreover, as in many other fields, it is not uncommon that economics textbooks are still biased by the thinking in the field a few decades back, particularly when the book is issued in its tenth edition. What is crucial, however, is that economics teaching at a more advanced level pick up newer ideas. To me, it seems that this is the case in most universities.

Moreover, most criticism of economics is based on criticizing textbooks and sometimes published research. Published research has almost without exception been widely presented at seminars and conferences before publication. The vivid discussions occurring in seminars and conferences are often quite different from the final written presentation – and research seminars in economics are more intense than most research seminars with often unstoppable streams of questions and interruptions. In these fora, the question on how a researcher can know what she is claiming to know can be discussed at length – although with few references to philosophy.

To understand better how economists think, we need to dig deeper into how they think. Hence I now turn to explaining the fundamental building blocks of economic research. It turns out to be useful to distinguish between theoretical and empirical approaches.

[2] In 1996, physics professor Alan Sokal published a paper in the journal *Social Text* where he claims that quantum gravity is a social construct (Sokal, 1996a). The paper was a hoax, consisting of a number of quotations and plenty of fancy language, but with no arguments of any sort. Upon publication, Sokal (1996b) revealed that the paper was indeed a hoax and raised strong criticisms against this line of writing.

Economic Models

Almost all theoretical work in economics is based on building and analyzing formal models of economic phenomena. These models are presented in a mathematical language. A clear distinction is made between features that are determined (explained) within the model, the endogenous variables, and features that are determined outside the model (taken as given), the exogenous variables. The researcher is also expected to make clear all assumptions being made. Implicit assumptions are not acceptable.

To be a bit less abstract, consider as an example of an economic model the standard model of how an individual's demand for goods is determined.[3] The variables that are taken as given are the prices of all goods available as well as the income of the individual. The explained variables are the amount purchased of the various goods.

To go from prices and incomes to purchases, a number of assumptions on the decision process are made. First, the individual is assumed to have a set of preferences over different combinations of goods so she can judge whether one combination, or "basket," is better than another or not. These preferences are also assumed to satisfy certain criteria, such as completeness and consistency. Completeness implies that for any two baskets, the consumer can tell whether she prefers any of the two or whether she is indifferent. Consistency means that if a consumer finds basket A better than basket B and basket B better than basket C, then she should also find basket A better than basket C. The final assumption is that the consumer chooses the combination of purchases that satisfies her preferences as much as possible under the constraint that she can actually afford the combination.

In the analysis of the problem, economists would characterize the consumer's preferences as a mathematical function, referred to as the utility function. The task of finding the choice of purchases that fulfills her preferences can then be stated as a maximization problem. This problem is solved subject to the constraint imposed by the requirement that she can afford the purchases – denoted as the budget condition. The solution is a mathematical expression that maps a set of prices and incomes into a specific composition of purchases.

This model can be extended to consider choices over time, savings behavior, labor supply decisions, and so on. Along comparable lines, economists have constructed models of how firms chose their production. These models can also be combined into a complete market of producers and consumers, where prices can be determined within the model. The simplest version of the model is one market modelled through a "market cross" where demand equals supply and more sophisticated versions consider the interactions between markets.

Economists distinguish between positive and normative analyses. The example of the determination of consumer demand discussed above is an example of a positive

[3] Expositions of this model can be found in any microeconomics textbook. See, e.g., Deaton and Muellbauer (1980) for a thorough introduction to the class of models.

approach, where the objective of the analysis is to understand why a phenomenon (the purchase of certain goods) occurs. A normative model would add criteria for what is good and then compare how good different policies are. If, for instance, we were to introduce a sales tax, should we introduce a tax on apples or on oranges? The most common criterion for what is good is the notion of Pareto optimality, achieved when nobody can be made better off without anybody being made less well off.

The economists' distinction between positive and normative analyses does not follow the stricter discussion of the normativity of science, as in, e.g., Brinkmann (2019). According to these definitions, both strands would probably be termed normative as the action of the researcher is normative and the research is undertaken in a specific setting. But in my view this is mostly a conceptual confusion and not a challenge to the normative and positive approaches to economics per se.

One common criticism of formal models is the objection that human beings do not maximize mathematical functions when they plan their purchases. Even the most ardent economist would agree with that. But economic models are not supposed to describe reality entirely. As statistician George Box (1979) put it, "all models are wrong but some are useful." A classic example of this is a metro map. As a description of where the stations and rails are located geographically, such maps do a horrible job. For planning a journey, however, they are very useful.

Milton Friedman (1953) presents a similar view of economic models. In his view, the working of the model is completely irrelevant. If a model gives good predictions, it is a good model. If its predictions are unclear or wrong, the model is not good. This implies that to judge the quality of a model, one should study whether its predictions can be verified empirically.

This is clearly an extreme positivist approach. Although some economists would claim this is how economists work, it is a poor description of good modelling practices both today and at Friedman's time. First, it is clear that a number of widely acknowledged models give few strong empirical predictions. The model of consumer demand discussed above, for instance, gives few testable implications unless strong restrictions are put on preferences. Other models simply give wrong conclusion. On example are some of the conclusions from early real business cycle models (Kydland & Prescott, 1982). These models can still be valuable, not because they help us predict the future, but because of their ability to help us understand the working of the economy. A more important criterion than good predictive power of a model is the model's ability to highlight important mechanisms.

A modified version of Friedman's view that is widespread is the "as if" assumption. No economist would believe that people in the real world would solve models when choosing their actions. Some of these models can be so complicated that it takes a computer several days to solve, so this though would be absurd. Rather, it is argued, individuals behave *as if* they had solved this model. The process through which individuals *actually* reach their decisions, down to the actions of the single neurons, is immensely complicated. Most researchers see this process as too complicated to understand fully and hence use a simpler model that captures some key features.

The idea that formal models can help understand economic mechanisms is sometimes ridiculed by other social scientists. A joke goes as follows:

> A physicist, a chemist, and an economist are stranded on a desert island, with nothing to eat. A can of soup washes ashore. The physicist says, "Lets smash the can open with a rock." The chemist says, "Let's build a fire and heat the can first." The economist says, "Let's assume that we have a can-opener..."

If any assumption is allowed when constructing a model, a model can of course "explain" anything. There are even infinitely many ways of "explaining" any phenomenon. This begs the questions whether we can learn anything from models.

To circumvent this problem, the field of economics has a set of rules for the type of assumptions that are suitable for a good model. There are few written sets of rules of modelling. Instead, this knowledge is acquired through a socialization process. Although most economists would agree on many rules, for instance, not questioning an assumption of rationality, there are differing views and traditions. In macroeconomics, for instance, there are two quite distinct modelling traditions often referred to as "freshwater" and "saltwater" economics.[4]

The key rule of a good model is to not assume the conclusion. This means that what is to be explained should be a product of the model. In the words of McCloskey (1990), a good model is a piece of storytelling. The story should make clear what mechanisms are into play, why different agents do as they do, and so on. A natural question is if a model is simply a piece of storytelling is why do we need a complicated mathematical model? Part of the answer is that the story becomes clearer when outlined in formal terms. Moreover, although a good model tells a story and can be seen as a piece of storytelling, it is also something more than simply a story. It is also a proper theory, but a good pedagogical presentation of such a theory can be through a story.

In the same way that a mathematician can see a proof as beautiful, there is a sense of esthetics of models. Some models are considered prettier than others are. Among contemporary economists, a concise model with suitably complicated mathematical formalism is typically an ideal. Some mathematics is used to make the model statement clear. If the model builder needs complicated mathematical tools, sometimes referred to as high-powered mathematics, she has to explain why she does and why she cannot use more conventional tools.

Moreover, the model should highlight an important feature in an insightful way – the reader should learn something from understanding the model. On the one hand, this is a question of the topic studied. On the other hand, it is to add something novel to how we understand the phenomenon.

Probably spurred by the success of applying new mathematical tools, such a fix point arguments, to general equilibrium theory in the 1950, complicated formalism was highly appreciated by most of the economics community during the next couple

[4]The terminology dates from the 1970s where most freshwater economists were working in universities close to the US Great Lakes, whereas the saltwater economists were based on the US east and west coast.

of decades. A humorous illustration is Leijonhufvud's (1973) mock anthropological study of the "Econ," where the mathematical economists are characterized as the priesthood of the tribe. Mathematical economics is still an active field of research today, but does not have the elevated status it had in the 1970s.

There are also some common features of a model that reduces its esthetic appeal. One unappealing characteristic is intractability. Although the analysis of a model may be complicated, it is widely acknowledged that it should be possible to solve and analyze a good model with pen and paper. Models who become so intractable that they can only be analyzed using computer simulations are not beautiful models. In practical policy work, however, such models are common and often seen as useful. Many central banks, for instance, have a version of a big computational model to predict the future of the economy – but not to do academic research. Hence usefulness does not necessarily add to model's appeal.

Another characteristic of unappealing models is application of uncommon or ad hoc assumptions. Assumptions close to "assuming a can opener" are clearly in this group. Assumptions involving psychological factors, such as loss aversion, are more debated. To some, these are seen as unappealing. One argument that is sometimes raised is that allowing for such assumptions in our models is a Pandora's Box, able to explain anything.

Still, a stronger emphasis on psychological realism has been increasingly popular over the last decades under the label behavioral economics. In the 1960s, a few economists started using experimental methods to study the realism of the models in the field. Vernon Smith (1962) famously showed that trading in an experimental setting would lead to an equilibrium close to the one predicted by supply and demand, indicating that economic theory has validity. Later experiments showed that other parts of economic thinking were less robust to experimental test. Particularly, individuals tend to care about others and follow moral norms. This led to the development of new theories incorporating features such as other-regarding preferences and fairness considerations of deviations from rationality (e.g., Kahneman & Tversky, 1979).

Some claim this is a new paradigm in economics, in opposition to neoclassical economics. In my view, however, this is not the case. Whereas other heterodox economic approaches, such as Marxist economics or post-Keynesian economics, are absent from the major economics journals and conferences, behavioral and experimental economists are highly present. Their status as "ordinary" economists is also not disputed among other economists. Finally, most behavioral economists would see themselves as mainstream economists simply taking economics a step forward.

Empirical Research

The construction of formal models may be the feature of the field that distinguishes it the most from the other social sciences. Although all economists are trained in analyzing formal mathematical models and have a stronger mathematics background than most other social scientists, not all economists build formal models on a daily basis. Economists working outside of academia would draw on the knowledge acquired by studying models, but would not construct models in their day-to-day work.

Also within academia, only some economists construct models in their research. Empirical investigation have been an important part of economics since its inception. Economists were also among the first social scientists to wield the new statistical tools developed in the first part of the twentieth century following the Neyman-Pearson paradigm (Haavelmo, 1944).

Over the last decades, a large fraction of economists have turned their attention to empirical studies. Angrist, Azoulay, Ellison, Hill, and Lu (2017) analyze all publications in 33 economics journals since 1980 and document a strong increase in the fraction of published papers that are empirical. This also seems to be the case in almost all sub-fields of economics.

Early empirical studies had two main purposes. The first purpose was to test the validity of economic theories. This fits into a classic approach where true theories are maintained and wrong theories rejected. The second purpose was to quantify theories – answer questions such as "by how much does the demand for sugar decline if the price increases by 10 percent." The analysis of both questions would be strongly linked to theory. A typical approach would be to take a theoretical model similar to the ones we discussed above and amend it somewhat to be able to use it for empirical purposes. This would entail specifying a stricter structure on the model, e.g., linearity, and allowing for some random factors being determined outside the model. Then the model would finally be taken to the data.

In recent decades, this approach has been criticized (Angrist & Pischke, 2010). To explore a specific economic mechanism theoretically, it is fruitful to use a theory model where some features are left under-developed. But this imposes severe problems if the model is to be confronted with data. In the real world, we cannot filter out effects that are irrelevant to the mechanism at hand. Hence a good theoretical model can be a disaster in "explaining" the world.

The main problem is that in the real world, very few factors can be said to be determined exogenously, outside the model, as they would be in a theoretical model. The holistic view that everything depends on everything is a better approximation. If an economist tries to investigate say the effect of prices on demand, he would then face the challenge that there are a number of other factors affecting both prices and demand. Hence finding the effect of prices independently of all other factors is challenging. This is typically referred to as the problem of uncovering causal effects.

Causality is a big topic in philosophy, sometimes referred to as the staple of modern philosophy. This literature is mostly overlooked by empirical economists looking for causal relationships. But the philosophical literature is also quite theoretical and not straightforward to apply. One exception is the debate between philosopher Judea Pearl (2000) and economist James Heckman (2008), the former arguing for conceptualization through probabilistic directed acyclic graphical models and the latter through counterfactual analysis.

Philosophical approaches to causality often try to explain phenomena. Malnes (2019) uses the example of whether the hammer causes the nail to sink into the wood. In this example, causality is straightforward. Moreover, it is reasonable to say that the action of the hammer explains the sinking of the nail into the wood. In social phenomena, causality and explanation do not typically go together. It is quite likely that a high union density has a causal effect on compressing wage differentials, but to claim that union density explains wage differentials (i.e., that union density determines union density alone) would be farfetched. Hence we need to think of causality as a study of the effect of changing one feature (union density), keeping everything else unchanged – the ceteris paribus assumption.

Applied empirical economists attempting to overcome the problem of causality have devised a number of techniques. As laboratory experiments are difficult to use in many cases, researchers try to find natural experiments. These are situations where randomization is not done by the researcher as in a laboratory, but by random events. Most of these are based on finding a factor that can be said to be external to the variables at hand. One example is rainfall, which can be predicted but not easily controlled by humans. Strong cutoffs, such as a minimum test scores for entry into a school, can also help to create pseudo-randomness as individuals slightly above and slightly below the cutoff are very similar except for school entry.

This literature is less theory driven than the older empirical literature. As models are simplified descriptions of reality, they are seen as less useful to inform empirical analysis than previously thought. Although it is necessary to make assumptions to be able to estimate of causal effects, those are rather based on other insights than those given by economic theory, such as rainfall not being affected by human actions. This also implies that the estimated results do not correspond to the insights of one specific theoretical model and hence that empirical research cannot reject a specific theoretical model. Rather, the empirical results can reject or corroborate one feature of a whole class of models.

It is probably fair to say that the new "causal" turn in empirical economics has achieved most of the attention in empirical economics over the last two decades. The approach of taking a theoretical model more literally and comparing it to data has not been abandoned, though. Estimation of so-called structural econometric models is still prevalent within some sub-fields (Nevo & Whinston, 2010).

Fruitfulness of the Economist Approach

It is clear that economists pay little attention to the philosophy of science. Although good economists have ideas about how knowledge is generated that is partially transferred to their students, there is little structured thinking and discussion around these questions. The strictest protagonists of epistemological debate would claim that such a field of science is doomed to perish.

However, this does not seem to be the case. Rather, economics and economists are doing quite well both as a science and professionally. One crude measure is that economists are better paid than most other social scientists in many countries, indicating that there is demand for economist. There are of course also other possible explanations for high wages. Economists are also in high demand as consultants both for private and public projects. This indicates that economists possess some knowledge that is seen as useful.

I argued above that economics does not try to be a "critical science" to the same extent some of the other social sciences do. Rather, the field has a tradition for supplying the type of knowledge that is in demand by governments – to be useful. This may explain why economists seem to be in demand. At the same time, this can of course pose a danger. A completely un-critical field of science could fail to ask unpopular questions, hence corroborating unfortunate decisions. Although there are clearly examples where economists have failed to see warning signs and given the wrong advice, it seems to me that a different approach to knowledge creation would not have changed the situation.

A field doomed to perish should also expect to be marginalized by other fields of science. This does not seem to be the case either. Fourcade et al. (2015) show that sociologists and political scientists cite economics quite heavily in academic writing. Economists, however, do not cite sociologists or political science to a comparable extent. Rather, they cite finance, statistics, and mathematics journals. Their pointed formulation is that economics is the "superior" social science. Although this formulation may be too strong, economics seems to benefit from a strong status among social scientists.

A number of economic theories, such as Becker's (1981) theories of family formation, have had quite direct impact on other social sciences. The economist's way of constructing formal models has seen less popularity. Mathematical sociology has been a significant sub-field in sociology, but has remained small. In political science, there are important insights in voting theory with a strong mathematical inclination (Austen-Smith & Banks, 2000, 2005), and game theory is popular for the analysis of international politics. These approaches remain fairly marginal, though.

In quantitative empirical studies, economic methodologies have had a more direct impact. The desire to estimate causal relationships is shared among the social sciences, and the methods devised by economists to analyze these questions are suitable for other social sciences. Due in large part to stronger mathematical background, economists have typically pioneered many empirical techniques, but there is quite active communication between the fields.

Concluding Remarks

Epistemological considerations are not commonplace in the work of economists. I have argued that despite this fact, good practitioners in the field have clear ideas about fruitful and less fruitful approaches to generating knowledge. However, the lack of discussion of these approaches has led philosophers of science, as well as representatives from other social sciences, to take a critical stand on the foundations of the field of economics.

One pertinent question is whether economics would be a better science if they paid more emphasis to the philosophy of science. I have argued that the field is doing quite well as it is, indicating that this is not the case. But as there are a number of synergies between the different social sciences, both regarding theories and methodologies for constructing and analyzing theories and undertaking good empirical analyses, there are possibilities for more communication between the fields which could also inform the economists.

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Chapter 16
General Conclusion: What Can Social Science Practitioners Learn from Philosophies of Science?

Jaan Valsiner

This volume was born in a six-semester collective teaching effort in Norway, but its implications go far beyond the mundane expectations of a *mandatory* course[1] on philosophy of science to any cohort of social science aspirants for Ph.D. degrees. What is at stake in our twenty-first century is the new nature of knowledge construction in the social sciences—where the input from different social power holders into the *kinds* of knowledge our sciences create becomes increasingly immediate. This normative control is put into practice by rapid growth of the administrative structures of universities, the role of which is to exercise control over the actions of researchers via the legitimate guarantees of the rightful expenditure of research grant funds and protection of the rights of the human research participants. Researchers themselves also contribute to this social guidance of their work—by accepting the administrative demands to publish in "Scopus-listed" journals of "high impact factor." The increasing move by funding agencies to prefer to fund mega-size research programs feeds into the vanishing of the creativity of any single researcher and coordination of their inputs to science within large research collectives. None of these conditions were the case in nineteenth- or early twentieth-century social sciences that

[1] The invention of new administrative frameworks for the highest study level in our twenty-first-century—doctoral—"schools" is an indication of administrative takeover of control of the social construction of potential new knowledge that is happening all over the world in universities at our twenty-first century (see Valsiner, Lutsenko, & Antoniouk, 2018). The introduction of *mandatory* units for aspirants toward their highest degrees in university is an example of turning the realm of intellectual inquiries into that of socially enforced transfer of knowledge. Needless to add that the very first act I undertook as coordinator of the seminars in Oslo over the six semesters was to make certain that—aside from the formal "mandatory" status—the seminars were free of such administrative straightjacket.

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were tarnished by the impacts of the European wars and political societal turmoils. The price of progress in the social sciences in the twenty-first century is the loss of autonomy and increasing interdependence with the "literatures" in their research areas. The new social philosophy of the social sciences is meant to reflect upon such new knowledge creators' roles.

Locating the Social Philosophy of Science in the Methodology Cycle

Philosophy of science leads us to reflect upon the whole process of knowledge construction. We developed the Methodology Cycle (Branco & Valsiner, 1997) for restoring the relevance of the generalized aspects of research—axiomatic systems (meta-code) and theories—for understanding of the phenomena in psychology. We felt then—like now—that the empiricist ethos of focusing on the data and their analyses was becoming overwhelmingly dominant and overshadowed the focus on general knowledge. It was clear then and continues to be so now, two decades later, that glorification of the boundless collection of ever new data in the social sciences under the labels of fashionable metaphoric theory labels is not leading to breakthroughs in our understanding of complex human phenomena. Instead of "big data" social sciences need Deep Theories—and it is here where philosophical elaborations come in as necessary.

It is our contention that such Deep Theories can be constructed only if all parts of the Methodology Cycle are put into work. This means careful coordination of the philosophical side (meta-codes) of the Cycle with the researcher's look at the phenomena and with the moves in theory construction. The lower part of the Cycle—methods and data—*follows* that first philosophy-based setup, rather than leads it. In practice—at least in psychology in the last half-century—it is the methods that have begun to dominate theory construction (Gigerenzer, 1991). This fits the general metadigmatic credo of *inductive* generalization on which the belief in empiricism is based. In contrast, our Methodology Cycle prioritizes *abductive* generalization (Salvatore & Valsiner, 2010). The shift from the former to the latter is axiomatic—fully dependent on the researcher's intuitive understanding of how to look at the phenomena.

While insisting upon the unity of all components of the Methodology Cycle, we prioritize the relevance of the basic assumptions (meta-code) that are on the foundation of any research effort, as well as a control of the kinds of data adequately derivable in principle from the phenomena under study. This criterion of adequacy is often overlooked in empirical research practices. For example, in psychology the habits of automatic quantification as the primary data derivation routine eliminate most of its phenomena from further consideration as the quality of the data becomes sacrificed to the rules-governed quantitative manipulations. Science loses out at the first moment of its investigative effort—to derive data from the phenomena. This is possible only if the general analysis of meta-codes in relation to theories and methods is not done—and that analysis belongs to the realm of philosophy of science.

A dramatic example of the loss of possible understanding of relevant issues in inter-societal relationships in political, economic, and psychological domains comes from the field of cross-cultural psychology. Since the 1970s the research program of studying "collectivism" in contrast to "individualism" between societies (and persons) has resulted in a myriad of empirical investigations showing that society X is "collectivistic," while society Y is "individualistic"—inference based on some statistically significant differences in summary scores of a questionnaire. The same effort to make inference about the inherent essences of such kinds is carried over from societies to persons—"individualism" and "collectivism" have become treated as personality characteristics. Both meanings of the terms are deeply embedded in the common sense and languages of the persons who are questioned. The whole research stream in this field fits well into Smedslund's diagnosis of most of psychological evidence being pseudo-empirical (Smedslund, 1995). Knowledge of common sense kind does not need empirical investigation since it is already pre-given by the underlying assumptions (meta-code) of the cultural histories of our Occidental societies.

In the flow of all the publications empirically demonstrating how different societies differ in "individualism versus collectivism," there has been only one theoretical article (Sinha & Tripathi, 1994) where the authors point to the co-existence of the seeming opposites within the same system—personal and societal. Each person in every society can be posited to include in one's Self system the mutually opposite and relating forces of insisting upon oneself as autonomous individual and the need to be embedded in societal relationships context. Similarly each society includes social forces toward social homogenization (collectivistic focus) and reliance on the individual initiatives that counter-act any homogenization effort. We are all individualistically collectivistic—or collectivistically individualistic. If this axiom of unity of opposites is accepted as meta-code, the empirical investigations would proceed in the direction opposite to those of the last 50 years—looking for specific ways in which individualistic collectivism works in society (e.g., small Chinese family enterprises succeeding economically in any part of the world) or within a person in the context of family, community, and society.

The choice, evaluation, and change of axioms are supported by philosophies. The traditional philosophy of science concentrates on the investigation into the upper part of the Cycle (red quadrangle in Fig. 16.1). The basic assumptions of causality (Malnes, 2019, in this volume) in contrast to catalysis (Valsiner, 2019b, in this volume) set the stage (via the meta-codes route) to develop matching theories in the given area—be it psychology, sociology, economics, or anthropology. The theories created need to be coordinated with the phenomena. If they are not—any empirical effort based on the given theory leads to empty data that fail to represent the phenomena. The role of traditional philosophy of science in the function of granting adequacy of the data is clear here. We can only repeat Wittgenstein's observation—presented as his opinion—about psychology over half a century back:

> The confusion and barrenness of psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings… For in psychology there are experimental methods and *conceptual confusion*… The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by. (Wittgenstein, 1953, s. 232)

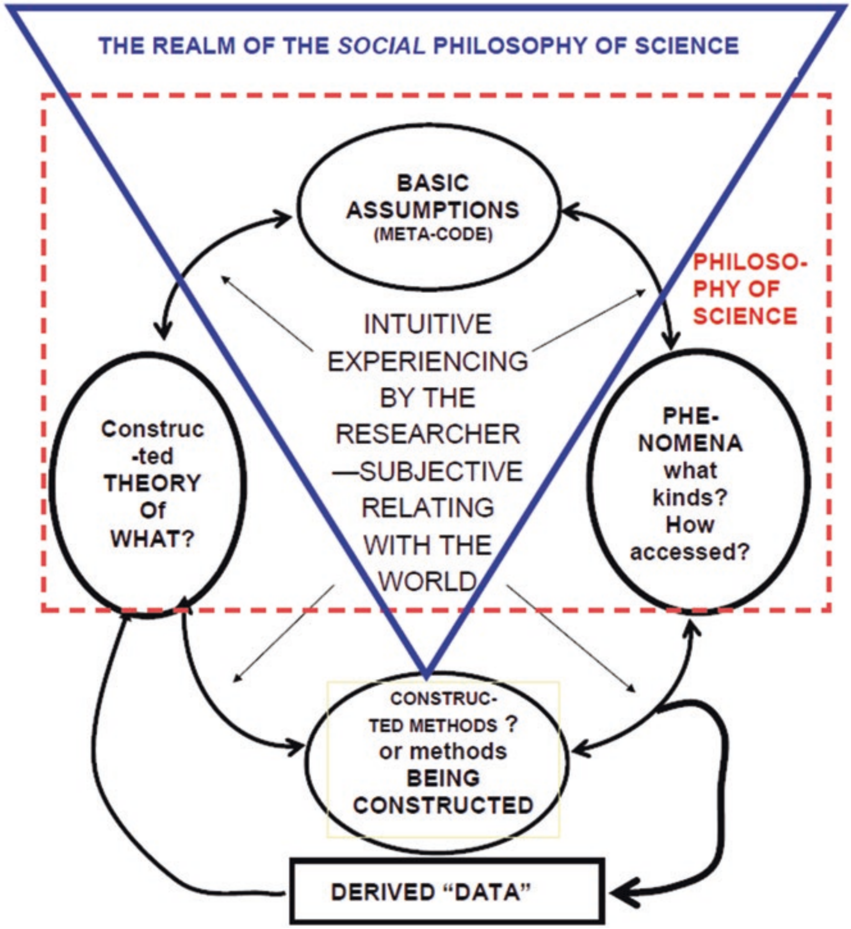

Fig. 16.1 The Methodology Cycle (Branco & Valsiner, 1997) with addition of philosophies

This confusion has increased over the following decades—under the banner of "evidence-based" science. While emphasizing this appealing label—who can question the value of *evidence*?—the social sciences have moved into theory-phobic conditions where the implicit metadigms rule the empirical practices. As Lundh (2019, in this volume) shows remedies are simple—a move in psychology to an explicitly person-centered axiomatic base would set up the stage for productive translation of phenomena into data. Psychology would benefit from small in-depth systemic case analyses—*nanopsychology* (Valsiner, 2018)—rather than from amassing ever larger data sets. Or—as Watzl (2019, in this volume) calls for improvement of our "biological literacy" in making sense of all the complex evidence from neurosciences that arrives in our knowledge bases through journalistic amplifications of the common sense mundane distinctions ("male brain" versus "female brain"). Advancement of science in general—and of social sciences in particular—is counter-acted by the overlook of philosophy of science (Strand, 2019, in this volume). The role of the meta-code in regulating theories <> phenomena relations is obvious and rarely utilized.

The Role of Agency: Researcher and Need for *Educated* Intuition

As an addition to the traditional philosophy of science, the *social* philosophy of the *social* sciences (blue triangle in Fig. 16.1) captures the role of the researcher in the wider societal context of the research act (blue triangle). This was claimed in the Introduction (Valsiner, 2019a, in this volume). Here it can be elaborated—giving primary attention to the role of the scientist in the whole Methodology Cycle. The IDEOLOGIES ←→ META-CODE ←→ INTUITIVE EXPERIENCING relation in the wider context of the classical philosophy of science and the research process becomes focused on. It is here where the relations of metadigms with sociodigms and paradigms (Kuhn, 1962; Yurevich, 2009) become established in the practices of knowledge construction.

It is the personal and subjective side in th e social sciences—the *educated* intuition of the scientist—that here gains the prominent role of linking society, myself, and new knowledge together. This education of intuition is based on the researchers' tuning in with the phenomena they study, with their general assumptions, and with the theoretical constructions they are ready to create and use. The years spent in university studies and doctoral education are the places where such intuition is expected to emerge. It does—in the ways in which students become initiated into research practices. A Japanese advisor to a beginning student in primatology advises him or her to go to the habitats of the monkeys and "get impression" of their lives (Asquith, 2000, p. 170). The focus here is in the development of intuitive feeling-in (*Einfühlung*) into the ecology of the animal. This is supported by the metadigmatic background in Japanese cultural history of not separating the world of monkeys from that of human (Ohnuki-Tierney, 1989). In contrast, Danish young psychology students are asked—after reading 1700 pages of psychology literature—to *apply* 3–6 different theories from these readings to the phenomenon of their choice. The result is a confusion of the role of theories (treated as finished and fixed givens that can be "applied") and the research enterprise (demanding the forcing of the phenomena into the straightjacket of these theories—rather than using the theories to *discover* something new in the phenomena). The education of the intuition in these contrasting cases is different—highlighting the phenomena in contrast to that of the differential dealing with theories as orthodoxic givens.

The role of the educated intuition in science can be dramatically ambivalent. In the social sciences—dealing with issues of the others which mirror those of one's own—the intuitions can develop in personally complex ways:

> (Each society) deals differently with the same psychic material. One represses it, another implements it overtly and may even over-implement it, still another admits it as a permissible alternative, either for all or only for certain overprivileged or underprivileged groups, etc.

> The scrutiny of alien cultures therefore often forces the anthropologist to observe, out in the open, much of the material he himself represses. The experience not only causes anxiety but is, at the same time, experienced also as 'seduction'. It suffices to think in this context of the problems which may confront an anthropologist, obliged to support his aged parents out of small income, who happens to be studying a tribe where filial piety obliges one to kill one's old parents. (Devereux, 1967, p. 44)

Here we see the social normativity of science and that of life in direct tension. Capacity to distance oneself from the personal needs and look at the phenomena with an involved dis-involvement is the stance for productive educated intuition. It is a parallel to the aesthetic look at art—that of interested dis-interest. In this striving for unity our contemporary knowledge construction has deep roots in Renaissance science and philosophy. Back then there were no limits between science and arts—a condition that made it possible for searches after knowledge which we in our century could call "interdisciplinary." Of course these efforts were born in the context of the metadigms of the Renaissance movement in the European societies—hence they give us a glimpse into the social philosophy of science in its recent history.

Organizing Our Understanding of Knowledge Making: Historical Roots

The intellectual origins of the Occidental science are in astronomy, astrology, and alchemy of the Renaissance. Furthermore—these perspectives were closely tied in with one another. Astronomy borrowed productively from astrology—and vice versa. The goals of alchemy in leading people to new discoveries of human mystiques were shared with all other areas of knowledge at that time.

Social sciences build their understandings of the phenomena on the recognition of these basic sciences—at their time—giving leads to the study of social processes. The post-modernist denial of the possibilities of generalization in the case of social and psychological phenomena seriously slowed down development of general theories in the twentieth century (Valsiner, 2009). For example, in sociology in Norway (Aakvaag, 2019, in this volume), the calls for practical actions within the given society made various directions in the discipline to cater for local knowledge of limited practical use. This seems to be a paradox—calls for practical use of knowledge would require such knowledge. Yet it is precisely the generalization potential from knowledge in general that makes sociologists' information practically useful. A precise description of a local community *as it is*—in Norway or elsewhere—does not follow practically usable action plan for the same community *as it could (or should) be*. The missing link here is the theoretical conceptualization of potential development. Honest efforts to improve the given society at the given time may become sidetracked if theory is no longer in focus and empirical enterprise flourishes.

The tension between the general and the particular in *Wissenschaft* has a long history. In the European context, it can be traced back to the Renaissance alchemical interests of practical production efforts (of gold) together with the idea of finding general knowledge about the mysteries of the world. The Stone—Philosopher's

Stone—was to be reached for this effort. Both the starting state and the desired end state of inquiry were that of embedded social context—within which general knowledge could reveal itself to some selected and diligent individuals—artists in their studios and alchemists in their laboratories. Figure 16.2—originating in the late sixteenth-century alchemical searches for knowledge during the reign of Rudolph II of the Holy Roman Empire—gives us an example of the knowledge construction process through superimposition of a sequence of geometric forms onto the everyday unity of human genders.

The road to knowledge in *Epigramma 21* (Fig. 16.2)—emergence of the Philosopher's Stone—was depicted by a series of geometric extensions from the original, unity of man and woman, as an act of data-analytic substitutions of one abstract form by another:

> *Make a circle* out of a man and a woman
> For which a *quadrangular body arises* with equal sides
> *Derive from it a triangle* which is in contact on all sides with around sphere
> *Then the Stone (Lapis) will have come into existence*
> If such a great thing is not immediately clear in your mind
> Then know that you will understand everything
> If you understand the theory of geometry (added emphases)

This seventeenth-century suggestion for generalization about gender relationships via a sequence of geometrical forms may seem naïve in the twenty-first century—yet it had the advantage over our social practices in directing researchers attention to the act of generalization that is needed for basic knowledge. Alchemists in the sixteenth to seventeenth century were serious—even if secretive—scholars whose work was on the foundation of chemistry as science in the nineteenth century (Karpenko, 2016). Chemistry has been close to psychology—at least in the mind of Immanuel Kant who back in the eighteenth century denied the possibility of either to become a *Wissenschaft*. His reason—no way to make them based on mathematics—has been proven wrong for chemistry, but stays put for psychology.

What the alchemists did was to try to use mathematical symbolization in their efforts. The story depicted in Fig. 16.2 is an example of the use of geometrical thinking in generalization—a particular real-life phenomenon is at first highlighted by one geometrical form that by rules of geometrical transformation can be viewed in the frame of another. Such extension of forms opens the door to abstraction—a straight line maybe a special case of a conus (Kepler on forms—Chen-Morris, 2009).[2] The goal—emergence of the Philosopher's Stone—was that of all alchemy,

[2] For Kepler any curved line was a version of the tension of two opposites—zero curve (straight line) and full curve (circle). This *oppositional* unity was elaborated into another kind of unity of subsuming the straight line as a part of the curvature only in the nineteenth century—in the Riemann-Lobachevsky synthesis of geometry—that the unity of linear and curvilinear forms became established by including the less general (line) in the more general (circle). That eliminated the *oppositional tension* that Kepler—based on alchemy's symbolic transformations stemming from opposites—was conceptualizing. In terms of basic meta-codes, the contrast between non-oppositional (monological) and oppositional (dialogical) worldviews remains present in the *Wissenschaft* over the past five centuries providing rise to harmony-based in contrast to tension-based theoretical constructions.

EMBLEMA XXI. *De secretis Naturæ.* 93

Fac ex mare & fœmina circulum, inde quadrangulum, hinc triangulum, fac circulum & habebis lap. Philosophorum.

EPIGRAMMA XXI.

Foemina másque unus fiant tibi circulus, ex quo
Surgat, habens æquum forma quadrata latus.
Hinc Trigonum ducas, omni qui parte rotundam
In sphæram redeat: Tum LAPIS *ortus erit.*
Si res tanta tuæ non mox venit obvia menti,
Dogma Geometræ si capis, omne scies, M 3 PLA-

Fig. 16.2 The way of generalizing in alchemy: geometric substitutions (Maier, 1618)

similarly to our contemporary expectation for arrival at general knowledge. In contrast to our century where most social sciences keep their generalizations limited by the inductive generalization (from samples to populations), the Renaissance alchemists "jumped" beyond the "collection of data." *Epigramma 21* starts from a generic—

already general—image that did not require information about the varieties of men and women, the generic pair was sufficient. The axiomatic direction was the creation of a supra-sexual being—no longer male or female (Chen-Morris, 2009, p. 144). It was the unity of the sexes that was the axiomatic starting point for sixteenth-century alchemists.[3] What followed was the "transformation of the data" by geometric abstraction process by rules that came from the general geometry derived from astronomical analyses of forms of movement of celestial bodies—as Johannes Kepler's astronomical work indicated. This can be seen as an example of deductive synthesis similar to the efforts of Jan Smedslund (1997) in twentieth-century psychology to create a system of theorems of common sense (Lindstad, Stänicke, & Valsiner, 2019). The meta-code of deriving all our psychological knowledge from the organization of the common sense is a generalizing strategy that has led to solutions that can separate the theoretically founded knowledge in psychology from its pseudo-empirical counterpart.

The interdisciplinary nature of the Renaissance knowledge construction was deeply passionate. Rationality of geometrical generalization was not separate from its affective side. Knowing—as the person strove toward understanding of alchemical secrets—was deeply affective. Interestingly the Renaissance alchemy relied on the multi-modal amplification of the efforts to generalize knowledge through understanding geometry.

Generalizing is an epistemological pathway that is socially introduced and may be promoted via persuasive means. Various memory- and belief-inducing techniques can be applied for such promotion. In our twenty-first century, scientific presentations in conferences and in classrooms become increasingly supported by visual materials of pictures or video clips. Yet we are not utilizing other forms of promoting allegiance to our sociodigmatic credos—the various university courses in statistical methods are not (yet?) trying to get their students' interest through chanting or singing a song about how to rotate matrices in factor analysis or to master the performance of the analysis of variance via some form of hip-hop dance.

Interestingly such methods of guidance of knowledge construction were practiced in five centuries ago. Figure 16.3 gives the example of the singing instructions for *Epigramma 21*—an interesting practice that alchemists suggested for self-enforcement of the striving toward the hidden messages in the symbolic universes.

The use of singing would of course fortify the memorization of the suggested alchemical generalization but is also a tool for affective allegiance of the singers to the meta-code established for understanding the world at large (compare the sixteenth-century mantra *if you understand geometry you understand everything* with that of our twenty-first century—*if you understand statistics you understand*

[3] From their axiomatic perspective of maintaining such unity in order to make sense of human beings in general, the twentieth and beyond centuries' practices in psychology of "measuring" gender differences would have made no sense. In their selection of meta-codes, this way the investigators four centuries ago were ahead of our contemporary psychology in their direction of *where* the knowledge construction should proceed. Of course they had no solutions to the question of *how* human beings operate.

Fig. 16.3 The singing of generalization (Maier, 1618)

everything). In both cases the lure of final truth (understanding "everything") is a beautiful promise of intellectual salvation which can never happen in *Wissenschaft*—where behind every horizon we approach is another one that lures us on. Using the familiar form of religious hymn singing in the promotion of generalization tactics via geometric substitutions in the sixteenth century made sense in the metadigmatic context of the European societies. The methods of persuasion of the primary social control—of religious kind—could be emulated in the emerging sciences.

What philosophy of science has contributed to our contemporary scientific enterprise is the focus on the axiomatic bases of the investigative efforts. Yet it has not been clear how such axiomatic bases emerge and how they become accepted in the given sciences as normative starting rules for further thinking. Furthermore what has not been investigated in the traditional philosophy of science is how these axiomatic beginnings may turn into fixed dogmas—viewed no longer as conventions but as absolute truths of no possible doubt. In other terms—repeating Bachelard's point—how strong opinions overtake doubt in the knowledge making process (Bachelard, 2002). This inquiry is in the core of the social philosophy of the social sciences.

Final Conclusion: Main Lessons for the Future

Our volume is a presentation of various aspects of general philosophical and meta-sociological look at the enterprise of knowledge construction. As Strand (2019, in this volume) points out, *vitenskapsteori* would make us increasingly aware of internal, theoretical, and methodological issues that we encounter in our research practices. Added to this are external issues—societal and political conditions—that either enhance our interest in philosophy of science or lead us to exclusion of philosophies from "science proper" (i.e., as it has become socially practiced in increasingly mercantilist science enterprises).

Social normativity saturates all human experience (Brinkmann, 2019, in this volume). Yet this omnipresence is not static—normativity is situation-dependent and open to development in various directions under the regulation of societal communication systems. Lundh (2019, in this volume) sees it as a *normativity crisis*, due to a social incentive system that is not conducive to scientific progress.

The normativity crisis is shared by all social sciences. How to cope with it? Reber and Bullot (2019, in this volume) have a solution—to look at the whole dialogue of normativity and counter-normativity in the context of what they call *conditional objectivism*. The key here is protection of some selected values from easy vulnerability to societal demands. Conditional objectivism leads researchers to recognize plurality of values (including their oppositions) and consider different possible ways of thinking before doing empirical research or suggesting practical applications. It is crucial to pay serious attention to counterfactual conditions. It calls for elaborative reflexivity about the research issue in its own right—and not as an arena for publicly displayed glory of publishing in the "right kind" of journal after using the "right kinds" of methods. The functions of social prestige of some discipline (economics—Lind, 2019, in this volume) can be for societal negotiations of the value of the social sciences (Carré, 2019, in this volume)—rather than vehicles for new knowledge in the field.

The general message of this volume to its readers—practicing social scientists and philosophically oriented general readers—is simple:

STOP (for a moment) and THINK (deeply)

about the social practices in your field of science. It is through generalization of our existing knowledge, based on doubts and ambivalences involved in all efforts toward new knowledge, through which our sciences proceed. Philosophy is not dead, but re-emerges as a lighthouse for orienting our sciences through the dangerous fjords of unlimited empiricism that different social power holders in societies expect.

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