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Veröffentlichungsversion / Published Version
Zeitschriftenartikel / journal article

Empfohlene Zitierung / Suggested Citation:
Knoblauch, H. (2021). Reflexive Methodology and the Empirical Theory of Science. *Historical Social Research*, *46*(2), 59-79. https://doi.org/10.12759/hsr.46.2021.2.59-79

Reflexive Methodology and the Empirical Theory of Science

*Hubert Knoblauch**

Abstract: »*Reflexive Methodologie und die empirische Wissenschaftstheorie*«. Inspired by hermeneutics tradition, qualitative as well as quantitative social research has realized that the subject matter of the social sciences is always interpreted by the actors studied. Social studies of science have also demonstrated that science itself depends on preinterpreted knowledge. In recent decades, it therefore became increasingly clear that any scientific methodology needs to account for the positionality of researchers and their methodology. In addition to the abstract armchair methodologies of scientific "reasoning," reflexive methodology has been proposed as an approach to empirical study of more than just the procedures and methods of the very researchers who are doing research, such as the videography of videography. It is one goal of this paper to stress that reflexive methodology is not a self-contained method describing and analyzing the practical methodology of one's action. To the degree that the analysis of the "real live" method is intended to guide research, reflexive methodology has also normative implications. As these normative implications have been referred to by the label *theory of science*, the second goal of this presentation is to delineate the idea of *empirical theory of science*.

Keywords: Sociology of knowledge, science studies, philosophy of science, reflexive methodology, videography, video analysis, communicative constructivism, qualitative methods, positionality, knowledge society, social sciences.

1. Introduction

In our current times, there seems to be little doubt that sciences in general must be "useful," and even opponents of the instrumentalization of science demand that science needs to help people and take their sides (obviously without doubts about whose side these people are on). As much as the use of science may legitimize it to society, it is quite an insufficient and some may even say a misleading criterion for science (Habermas 1970). I want to argue here that one of the most decisive features of science is reflexivity. As some

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Historical Social Research 46 (2021) 2, 59-79 │ published by GESIS
DOI: 10.12759/hsr.46.2021.2.59-79

may contend that reflexivity is a feature of any human action, I need to add that it is the explication of reflexivity which makes science distinct. One major aspect of scientific reflexivity concerns the methods. Methods as the ways in which knowledge is being produced are what is or needs to be made explicit in science. Methods do not only describe how knowledge is produced; they prescribe how knowledge should be produced. In this sense, they are normative.

While the normativity of how science should be done has been the subject matter of the *philosophy of science* for a long time, it has been the recent contribution of *science studies* to demonstrate empirically that much of the methods used in science as a social practice often go unnoticed. It is the goal of the *empirical theory of science* (short: ETOS) to link these two approaches to science in a way that avoids both the idealism of the philosophy of science and the relativism of science studies.

Without elaborating here on the basic notion of reflexivity as an aspect of any social and communicative action,[1] I want to focus more specifically on the reflexivity of science and, after this introduction (1) make the following argument: (2) Although *science* is often treated as a distinct institutional field or a subsystem clearly set apart from other systems, the extension of what came to be called the *knowledge society* has led to an increasing dissemination of scientific knowledge; as its (socio-)logical consequence, this dissemination of scientific knowledge, practices, and technologies contributes additionally to the dedifferentiation of science with respect to economy, politics, education, or religion. The dedifferentiation tendencies of science do not only concern it as an institutional field on the macrosocial level, but also affect scientific knowledge and therefore scientific action and practices.

Consequently, it poses the question of what science is in a new manner. While the philosophy of science used to address this question in a theoretical manner, the *social studies of science* approached it empirically. The ETOS (3) suggests linking both the empirical studies of how science is done and the normative reflection of how it should be done. As both relate to methods, that is, the ways how scientists should and/or do act when doing science, it is methods which allows scientists to link both parts. Both empirical studies and the reflection of methods in science is what we call *reflexive methodology* (4). Reflexive methodology (RM) is, so to say, the empirical side of ETOS. I suggest distinguishing between three kinds of reflexivity, the three levels of reflexivity specific to the reflection of methodology: the institutional (discursive) level, the level of interaction (and practice), and the level of the subjective knowledge and action. Due to the constraints on space, I cannot exemplify all three types of reflexive methodology here. On the background of the

[1] The text draws on and uses parts of earlier publications (Knoblauch 2014, 2020; Knoblauch and Schnettler 2012). For comments, suggestions, and corrections of the text, I am grateful to Antje Kahl, Severine Marguin, Jörg Niewöhner, Cornelia Schendzielorz, and Elisabeth Schmidt.

hegemonic non-subjectivist theoretical approaches in much of science studies, I want only (5) to shortly draw on an empirical case of RM, which indicates the constitutive role of (a nonessentialist) understanding of subjectivity when doing science. By way of conclusion (6), I want to suggest that these arguments must not be restricted to the social sciences; given the breadth of research in science studies, we can assume that they can also be extended to the natural sciences as well.

2. Scientification and Knowledge Society

Already in the 19th century, Auguste Comte had indicated the increasing relevance of "positive" scientific knowledge. However, with the decline of the modern industrial society, the industrial working force, and the massive expansion of education in the 20th century, we have seen the advent of what came to be called the knowledge society.[2] This applies above all to the transformation initially observed in Western societies, from an industrially dominated production of goods to knowledge work, which is characterized on the economic side by de-industrialization and, in socio-structural terms, by the rapid growth of the knowledge class. (In many non-Western societies, the knowledge society has developed without any prior industrialization, which causes significant differences in mentalities.) The fact that we are talking about a knowledge society has to do with the massive changes in knowledge transfer. Just take the so-called "educational revolution" (Parsons 1971), which, since the Second World War, has led, first in the USA and later elsewhere, to an ever-increasing number of people spending 10-20 years in education systems, if not through lifelong learning in seminars, courses, or even digital learning. With this massive increase in education, the formal criteria of the education system have also become increasingly prevalent: Work in a qualified sense is no longer possible, even in the craft trades, without some kind of academic qualifications. University degrees and other formal qualifications now even represent a transnationally valid *symbolic capital* (in Bourdieu's sense). Because of these formal distinctions, there has been talk of a knowledge class since the 1960s. Although the term has not become widely used, it proves still useful in the analyses of Fligstein (2008) who accounts for the rise of the new knowledge class as a reaction to the result of the massive extension of higher education in Europe.

Knowledge society is also characterized by the fact that more and more people are concerned about the knowledge of others: Knowledge *as* knowledge becomes the object of human communication, human labor, and thus also a

[2] The notion, popularized in the late 1960s, goes back to the pioneering work of Fritz Machlup (1980 [1962]), based on concepts developed in the sociology of knowledge (Schütz, Scheler).

commodity. However, on the macrosocial institutional level of society, knowledge society does not only refer to the transfer of scientific knowledge in the educational system; rather, the production of knowledge as knowledge plays a central role – and so does, consequently, science. If we consider modern society as characterized by its high degree of differentiation (be it in terms of institutions, institutional fields, or functional subsystems), we can therefore speak of a *scientificization* of society.[3] This scientification can be seen in the increasing penetration of formerly non-scientific areas of social function (business, politics, religion, media, sport, and so on) by scientific knowledge. Some authors see in this scientification the reasons for societies' gradual transformation, for example into a knowledge economy, knowledge policy, and so on (for example, Stehr 1994).

As much as scientifically legitimated knowledge is a key to professions and thus allows access to the core of other social systems – that is, to leading positions in the economy or in religion – it would be inappropriate to regard science as the dominant social subsystem in the knowledge society.[4] There is no doubt that science legitimizes the symbolic capital of knowledge. This can be seen relatively simply in the fact that the value of knowledge increases with its scientific nature. If we only look at the degrees between physician assistants and female doctors or secondary school students and doctoral candidates, for example, it is easy to see that the value of science increases with its scientific nature: The closer the acquired knowledge comes to science, the more it seems to be worthwhile. However, as important as science may be in bestowing legitimacy in knowledge, the influence of other institutions on science should not be overlooked. Apart from the fact that, for example, politics depend increasingly on formal knowledge qualifications, science cannot be regarded as a leading institution, neither economically nor politically. Rather, we find a number of massive influences of other systems on science.

If we look only at the relationship between economy and science, we are not just talking about the huge area of research and development, and more recently innovation; rather, economic logic has entered science, as has been demonstrated by the introduction of *new public management*, competition, and reward procedures (Ward 2012). However, politics also has an influence on science by its allocation of research funds, state funded scientific research and teaching, and the agenda-setting of scientific research topics (from sustainability and digital society to artificial intelligence), not to mention the mass media and the new media, which are now so well integrated into science

[3] The idea of differentiation has been coined by Spencer and Durkheim and elaborated by Parsons as subsystems (for example, 1971) or, for example, by Bourdieu as social fields.

[4] The move from the dominance of politics as a subsystem to economy as subsystem has been considered as one of the major features for the social change from modern to late modern or postmodern societies by, for example, Zygmunt Bauman or Anthony Giddens.

that it seems almost impossible to draw clear boundaries between scientific and non-scientific knowledge.

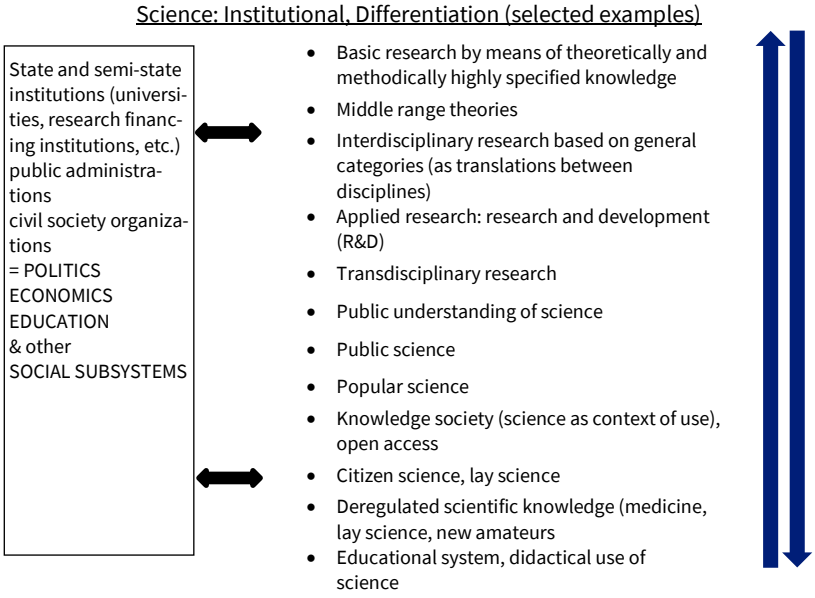
The *scientification of society* thus exhibits a Janus face, for it necessarily and logically goes hand in hand with a dissolution of the boundaries of science to society, which is presupposed in most sociologies of science (Weingart 1993). Without being able to go into the specific organizational contexts here, it is impossible to overlook that politics (state, parties, foundations), business (private research institutions, financing), the churches (theological faculties, research institutes), the media (science journalism), and many other institutions have entered into the very workings of science, affecting its research topics and determining the "usefulness" of scientific knowledge. So, we are dealing with a number of tendencies that are often overlooked when science is seen as a system: The promotion of interdisciplinarity, which has been very much enforced by organizations from outside the scientific field, is extended by transdisciplinarization, that is, the combination of scientific and "practical knowledge."

This combination results in new forms of knowledge. From the point of view of the difference between systems or fields, the result of which is that the boundaries between science and politics are becoming as blurred as those between scientific and, let us say, dance or other artistic research and the increased fluidity of their structures, for example, through projectification (Baur, Besio, and Norkus 2017). From the observation of such developments then arises, for example, the postmodern diagnosis of a new social form of science, called "Mode 2" by Gibbons et al. (1994).

As a result, the forms of science multiply so that the order of knowledge in science has been diagnosed as "fractal" (Abbot 2001). As fragmented as the situation of scientific knowledge may appear, if we start from differentiation theory, we can discern a growing tension between the two poles indicated in diagram 1. While pure research continues to exist, new hybrid forms have been emerging, which not only communicate science to the outside world, but overcome the distinction between both, ranging from public understanding of science to public science to lay science. Due to the spread of neoliberal methods of public management in science organizations, we are observing the dissemination of the most diverse formats and forms from economics into science: evaluation via economic criteria and monetary incentives for "excellence," the consumer evaluation of teaching performance, and, last but not least, the basically economic forms of presentation of knowledge such as PowerPoint or science slams, which are now to be found at the heart of scientific communication (cf. Knoblauch 2013; Wilke and Hill 2019). The spread of new funding models for scientific publications or the massive expansion of scientific data and texts (for example, in the course of the expansion of national research data infrastructures) and the integration of research management systems into project work are indications that digitization will bring

about further changes that, as far as I can see, are not motivated by science, but are rather taken over from other areas (especially the economic sector).

Diagram 1 "Scientification" of Everyday Knowledge/ Mediatization of Knowledge Society



Source: Diagram by Hubert Knoblauch.

If we consider that universities, for example, which are central institutions of science, also serve as major institutions of education for a large part – sometimes even the majority of populations – it is plain to see that the scientifically oriented education in these institutions cannot aim primarily at producing scientists. Rather, scientifically oriented knowledge is transmitted to actors not included in the (core) institutions of science. On these grounds, the above-mentioned expansion and sometimes inflation of the term *research* is understandable; but it seems very questionable whether science can really still be meaningfully regarded as a closed subsystem or as a "field." This does not mean that we are witnessing the postmodern delimitation of science, scientific knowledge, and practices, but rather that the concept of distinct scientific knowledge is, at least in structural terms, becoming diffuse. Science, it seems, is becoming more and more part of a broader formation of knowledge, in which very different societal orientations or codes other than those of truth are at work. In this process, science increasingly overlaps with other social functional systems and, above all, dominant power interests of various institutional areas (for example, state-funded contract research, the

knowledge interests of large corporations, or even of churches and sports associations). From the perspective of science with its historically developed self-image as a recognized and symbolically highly valued institution, tensions and conflicts also arise in these transitional areas, which certainly follow the logic of the social field – as developed by Bourdieu (1985).[5] However, as Bourdieu (ibid.) himself has already indicated developments towards the dissolution of boundaries in social fields, we are faced with an increasing hybridization of core scientific institutions and their "functions" with other social tasks and, above all, revaluations that raise the question of the field's boundaries – and thus the fundamental question of how we can define science.

3. Boundaries of Scientific Knowledge and Empirical Theory of Science

The question of how we can define science is a logical result of the expansion of science into society. As a *question of the demarcation of science,* it has been at the core of the philosophy of science since Popper (2005) at the latest. How can science be distinguished from non-science? As Popper already referred to scientific knowledge, even in sociological terms this question cannot be reduced to the role of science in the macrosocial institutional order; it also must address knowledge and the actions guided by it. Therefore, we must ask: What is scientific knowledge?

The reference to Popper indicates that the philosophy of science was in charge of determining what distinguishes scientific knowledge. Even if it is sometimes called *theory of science* (or, in German, *Wissenschaftstheorie*), it is typically located in philosophy, and so the subject is consistently called philosophy of science. Philosophy of science refers to the epistemology of science. Thus, it deals with how knowledge is produced by means of scientific methods, more specifically with the role of induction and deduction, the forms of explanation, or the difference between the social and natural sciences. It is an epistemology of scientific methods that necessarily includes the methodology of research. As methods are required, it has an explicit normative component (Mittelstraß 1974). Normativity here only means the norms of society (such as the norms of an open society, which, according to Popper (2013), is favored by science). In addition, and more specifically,

[5] Bourdieu's notion of social field did not only originate in his analysis of religion (and Weber's analysis of it), it is also with respect to religion that he indicated tendencies of the dissolution of the religious field (cf. Bourdieu 1985). As to the relation between fields and epistemic regimes cf. Gläser et al. (2018).

normativity refers to methods as the ways in which science should be done and thus includes scientific methodologies.[6]

However, as much as the idea of normativity is of interest, the philosophy of science from Popper to the *new or critical realism* is mostly based on the individual model of classical epistemology implied.[7] This epistemology starts out from individual subjects who make a cognition, who carry out the conclusions and who communicate these conclusions individually to other individuals. As opposed to this individualist epistemology implicit in many theories of science, science studies and its precursors took a much more sociological perspective in scientific knowledge. It is the sociality of knowledge that is characterizing science.[8] This argument of the sociality of knowledge, originally claimed by the sociology of knowledge, has been spreading massively by the rising field of science studies since the 1960s, including much of the *history of science*, the *sociology of scientific knowledge* (Shapin 1995), *social studies of science*, or also *science and technology studies* (Felt et al. 2017). All these approaches are based on empirical studies of scientific knowledge as a social phenomenon (cf. Niewöhner 2012). This sociologically inspired empirical research on science has shown how little the assumption of a linear progress of knowledge (Comte) or an accumulation of knowledge (Popper) corresponds to the practice of scientists. It has also demonstrated how much science is actively involved in the social construction of scientific facts that are epistemologically presented as independent of it.

As rich and fruitful as these empirical studies of science have been able to demonstrate the relativity of scientific knowledge (Stengers 2000), they are certainly concerned with describing norms. While Merton (1973[1942]) already had identified the social norms built into scientific institutions, later social studies of science demonstrated the rules, implicit discursive knowledges, and practices in science (cf. Sigismondo 2010). As much as postcolonial, Marxist, and ethnomethodological studies have added to these insights of how science is affected by cultural norms, social structures, and power relations – and as much as science studies have demonstrated how implicit knowledge governs scientific practices – many of these studies are said to not only foster epistemological relativism but also to weaken the normative

[6] A prominent example for this is the ethical imperatives of science Merton (1973[1942]) identified, such as universalism, communality or "communism," unselfishness, and organized skepticism.

[7] Critical realism shares certainly the idea to unveil the implicit assumption in doing science especially in its concept of metatheory (cf. Bhaskar 1997). However, it still sticks to an individualistic epistemology, fails to realize social constructivisms' stress on (socially constructed) objectivity and the (life-worldly based) "naïve realism." As to the various forms of social constructivism and their misunderstanding, cf. Knoblauch and Wilke (2016).

[8] Philosophy has taken up this idea, which had been established in the sociology of knowledge for decades as "social epistemology" (Schmitt 1994).

orientation in science and the institutional structures.[9] Even if this may not hold for all, it is safe to say that empirical studies of science do not address the question what science can learn from these studies about how we should act as scientists when doing science.

After its heyday up to the 1980s (for example, with the publication of the *Encyclopedia of Philosophy and Philosophy of Science* by Mittelstraß 1980-1996), the philosophy of science still continues to exist (mostly as analytical philosophy). However, its current situation has been described as "gloomy" (Agassi 2011, 280). Although there has certainly been a rapprochement between both, there are many complaints about the lack of reciprocity between science studies and the philosophy of science. Even insiders bemoan that "exchanges between philosophy of science, history of science and science studies have been rather sparse; in fact, the disciplines have drifted further apart" (Schickore and Steinle 2006, ix). Weingart (2003, 12) notes that the differences between the formal orientation of philosophical philosophy of science and social science research are as great as their institutional distances. Ammon (2011, 1) concludes that the connection between the history of science, philosophy of science, and sociology of science is still a desideratum – not to mention the connection to the sociology of knowledge.

As a solution to this unsatisfactory situation, the proposed idea of the ETOS consists in taking up the normative orientation of the philosophy of science and combining it with the empirical approach of science studies in such a way that the (normative) knowledge of how scientists should act is based on the analysis of how scientists do act when doing science.[10] This is based on the assumption that scientists' ways of acting are linked to their various kinds of knowledge about how the actions should be done. This means that science reflects on what it is supposed to be by observing and determining with its very own methods of criticism what distinguishes it as science.

Admittedly, this suggestion is by no means entirely new. What makes it new is the attempt to link the critical approach of the sociology of (scientific) knowledge with the normative approach of the philosophy of science by an empirical reflection on methodology. It is therefore not a matter of expelling the philosophy of science, but rather of the project of an interdisciplinary science research that not only recognizes that science is a thoroughly social and cultural enterprise, but also that the production of knowledge is socially organized both in the "discovery" and in the "justification" contexts (Schickore and Steinle 2006). This is an empirical theory of science that is not only

[9] As Heilbron and Gingras argue (2015, 8), the description of the fluidity of practices has contributed to the dissolution of the institutional structures, thus eliminating the structural conditions that rendered them possible.

[10] As will become clear with respect to the three levels or reflexivity, this suggestion is much more extensive than Mol's "empirical philosophy," that is, the ethnographic interest of philosophers in implicit, embodied, and embedded empirical knowledge practices (Mol 2002).

concerned with the *observation* of science, but with reflecting the practice of science in order to improve it in the face of its explicit and implicit conventions, norms, and standards.

4. Reflexive Methodology and the Three Levels of Reflexivity

As Lynch (2000) argues, the notion of reflexivity has been used in a large variety of ways. In fact, he provides a list of six different notions, including several variants. The list supports the view that radical, critical, and postmodern forms of reflexivity tend to deconstruct and debunk scientific knowledge, particularly in science studies. This view overlaps with his ethnomethodological perspective on the "reflexive [...] character of [all] accounting practices" (ibid., 33): Since practice is always reflexive, he argues, to call science reflexive does not help when trying to define it. I certainly do agree with Lynch that reflexivity is not a privilege of science, but rather a necessary aspect of any social action. That is to say that any action oriented to someone else must be made understandable by what Lynch calls an "account"; I would add that the account needs to be objectified so as to be perceived by the other. Therefore, reflexivity is always "communicative reflexivity."[11] On this basis we can now formulate a distinct criterion of scientific reflexivity: While reflexivity in everyday life typically is an implicit process, in science, reflexivity itself is being objectified and explicated as a topic on its own,[12] be it in language, discourse, bodily performance, and practice or in institutional settings (such as disciplines, institutes, or seminars in the theory of science).

It is true that we find explicit objectivations of reflexivity also in politics, civil society, and economy (for example, deliberative discourses and institutions in politics, civil society, accountancy, and rational bookkeeping).[13] Therefore, one additional feature of scientific knowledge consists in the fact that the very *methods* of how knowledge is produced and objectified become themselves the subject of objectified reflection. The objectivation does not only refer to the datum or the (induced, deduced, abduced) conclusion from its statement about it or new knowledge concerning it, but also to the explicit accounts of (or legitimations for) how the knowledge has been accomplished,

[11] The notions of objectivation and objectification is elaborated in Knoblauch (2020); for the relation of the theoretical approach to the three levels of analysis with respect to video analysis, cf. Knoblauch 2009.

[12] Using Schutz and Luckmann's terms, one can say that in everyday life, (ethnomethodological) reflexivity is interpretationally relevant, in science it becomes thematically relevant (cf. Schutz and Luckmann 1984).

[13] I have discussed these formats with respect to their relevance for an empirical theory of science in an article in German (Knoblauch in print).

that is, the methods mentioned. The explicitness of the account is again part of a "critique" about the adequacy, usefulness, and/or problems – of methods, that is, their normativity.

It is this kind of objectified explicit reflection of the methods producing knowledge that I call "reflexive methodology" (Knoblauch 2004). As mentioned, this idea formed the background for many social studies of science, which initially focused on the practices in the natural sciences: Authors such as Knorr Cetina (1981) or Latour and Woolgar (1979) have shown that the sciences do not simply explore nature, but that nature is *interactively constructed* in the social practices of science in such a way that it cannot only be understood by other scientists, but also observed by social scientists. Later, the (reflexive) scientific study was extended to the social sciences (for example, Lamont 2009; Camic, Gross, and Lamont 2011).[14]

As successful as ethnomethodologically inspired scientific research on social practices has proven to be, it has been accused of reducing science to the *situational level of interactions* only. Thus, Bourdieu has referred to the ethnomethodologically inspired science studies mentioned above as "microsociologie constructiviste" (Bourdieu 2001, 114). In addition to this interactionist understanding of reflexivity, Bourdieu proposes a more extended idea of reflexivity. In his book *Science de la science et réflexivité* (Bourdieu 2001), he formulated the program of a reflexive sociology in such a way that it is influenced by the classical institutionalist science studies (Merton 1942; Kuhn 1962) as well as by the "Strong Programme in the Sociology of Science" (Mulkay 1979). According to this view, action in science is shaped on the one hand by the position of scientists in the social field of science and on the other hand by their embodied situational action, whereby the knowledge stored in the body takes on a mediating role as habitus. Reflexivity means: "La science sociale est une construction sociale d'une construction sociale" ("Social science is a social construction of a social construction"; Bourdieu 2001, 172). Thus, Bourdieu stresses the *institutional character of scientific reflexivity* in addition to acknowledging the role of interaction. The institutional side has been addressed by the classical sociology of science very frequently, and it is visible in many institutional and organizational forms, such as evaluation panels or journal review processes (cf. Lamont 2009; Hirschauer 2004).

By his proposal for the auto-socioanalysis of the sociologist, Bourdieu, in addition, hints at a third dimension of reflexivity also indicated by Lynch (what he calls "philosophical reflexivity"). The role of subjectivity has also been in the background of the idea of reflexivity from Garfinkel who drew on and opposed at the same time the idea of reflection inherited from Schutz and Husserl. In fact, while Husserl (1982), following the line from Descartes to Kant, had suggested "phenomenological reflexivity" as a method of self-

[14] In the 1980s, the notion of reflexivity was later picked up in anthropology and its "reflexive turn" (cf. Boyer 2015).

reflection of subjective consciousness, it was Schutz (1967) who had studied the sociality of knowledge on the basis of this method of self-reflection: The (mundane) phenomenological analysis of scientists' actions (our understanding of other's actions, and, more generally, our life-worlds) should provide the basis for the claim of how we can understand other's actions (and the life worlds). This "science of the subjective paradigm" as Luckmann (1990) calls it, underlines the need to consider the subjectivity of actors and those who study them as a basic subject matter of the social science or, for that matter, of any kind of knowledge production, including the sciences. With respect to qualitative methodology, a similar stress on reflecting subjectivity has been made by Breuer (2009).[15] By reflexivity, he refers explicitly to the self-reflexivity of the researching subjects. He is less concerned here with reflection guided by a method or a theory, but rather with the "subject/ivity of the researcher," which is to be considered "both in terms of its embedding in the life-world as a ('private') person and in terms of its significance for research interaction" (Breuer 2009, 9). In neither term can subjectivities be reduced to being an effect or result of social processes only. Therefore, I suggest identifying *subjectivity as a third dimension of reflexivity*.

Reflexivity can thus take place on three different levels, and they are typically linked to different methods when investigating the production of scientific knowledge: On the one hand (a) there is a subjective dimension of the researcher as an individuated, embodied, sensual, and knowledgeable actor. As Bourdieu makes clear, (b) the institutional order already plays a role in the socialization of researchers and thus also in their habitus, which therefore certainly needs to be taken into account. The institutional order requires different methods of reflection other than the (rather phenomenological-auto-ethnographic) reflection of subjectivity. This includes, for example, the institutional systems of science (such as universities, economic and political power structures, and the organizational structure of universities, disciplines, and networks). Already at the point of access to science, social inequalities and gender order are decisive in a way that Goffman (1977) called "institutional reflexivity." The (c) interactionist research described above is linked to the first mentioned form of reflexivity, which goes back to ethnomethodology: actors not only perform their actions, but also indicate how they would like their actions to be understood. This notion of reflexivity has been explicitly made an analytical resource in conversation analysis, which uses, for example, recordings of interactions, practices, and social situations.

[15] The term *reflexive methodology* has also been used by Alvesson and Skjoldberg (2017). Their notion of reflexivity is, however, directed at the dependence of the methodological approach, data interpretations, or research design on the respective social theories, although they only distinguish three particular approaches, which they call (post-) positivism, social constructionism, and neorealism.

Although the essentialist ideas of subjectivity have been subject to criticism by many dominant approaches in science studies, post-structuralism, systems theory, and practice theories have taken too drastic measures by denying any role of subjectivity in science. Without a doubt, institutions and practices are certainly decisive for subjects doing science: Individuals are socialized into sciences, subjectivated by scientific knowledge, and, mostly, "disciplined" in a quite particular way by institutions, discourses, and within social situations of institutional asymmetry. After all, the subjects doing science are the product of highly institutionalized and formalized learning processes in which they have acquired highly specialized knowledge about the methods of their work. Additionally, they have acquired these methods not only theoretically and discursively, but also by applying a number of quite ascetic self-techniques, such as reading texts and data for hours on end. In this respect, methods are not only a means by which to produce social knowledge but are also means for the production of special kinds of subjects with their corresponding knowledge, their embodied abilities of sensual experience and action. Scientists not only need to acquire explicit knowledge, but also the ethnomethods of disciplines, the proficiency to work with certain instruments in particular, the ability to see in a certain way, and the ability to become part of an epistemic culture (Knorr Cetina 1999). Yet, as much as science needs to educate its subjects and define them as individuals, it depends essentially on a form of subjectivity that is not determined by, and not subjected to, structures, practices, and discourses.

5. Subjective Positionality in the Video Analysis of Video Analysis

In order to get a sense of what we mean by subjectivity in reflexive methodology, let me turn to an empirical example.[16] Given the restrictions of the text and the subject-forgottenness of many contemporary approaches in empirical science studies, I will focus on one aspect of subjectivity. In the case that is presented elsewhere in more detail (Knoblauch and Schnettler 2012), we analyzed a data session by video in which a research group interprets and analyzes a video of an interaction between an interviewer and an interviewee.[17]

[16] I must stress that the idea for this procedure has been pioneered in a research project on the constitution of data in the social sciences (cf. Luckmann 2003).

[17] Cf. Knoblauch, Tuma, and Schnettler (2014). We distinguish the process of everyday understanding (the actors) and scientifically analyzing actions by means of analytical notions, although in reality both aspects are typically intertwined.

Figure 1a Video Data Session

Before we turn to the subjective level, I should at least indicate that the reflection of the institutional level would focus on the socio-spatiality of the research setting (such as in our case with videolabs and seminar rooms at universities), the objects involved (desks, pens, projectors), the format of the event (for example, training, teaching, research data session), and its meso-sequential order (opening, deepening, finalizing the analysis) as well as the social structure, relations and power structure (for example, professional or academic status differences between participants), and the socially acknowledged distribution of knowledge (invited expert, novice of the method). On the situative level of communicative actions and interactions it would imply the identification of activities in the sequences of interactions, such as introductory talk, open discussion, or data session, which includes various activities such as ascribing an analytical technical category to a video fragment discussed.[18]

[18] For the various "activities" in video data sessions and the difference between applied science and pure science, cf. Tuma (2012).

Excerpt 1b Turning the Leaflets [00:41-01:05min] Transcript and Fragment of the Video Data Session

1 H: ja, aber es is am Anfang auf dem Kopf,
Yes, but it is upside down in the beginning
<<points with the finger to the video still>>

1 bei dem ersten Mal oder gucken sie mal
At the first time or have a look at it

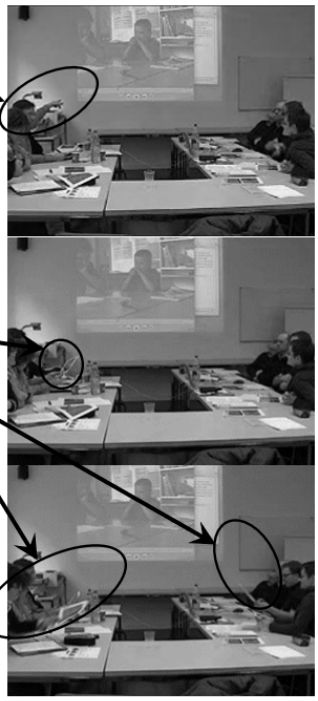
2 C: ähm [Ja
ehm yes

3 I: ja, ja]
yes, yes

4 H: AHA es steht auf dem Kopf aha (-) aha
Indeed, it is upside down, indeed, indeed
<<turns his copy>> (-)
<<other participants are turning their copies>>

5 A: und [da
and there

6 B: noch mal zurück machen bitte ja?]
Please move backwards, do you?



Fragment 7e: analyzing the data

The sequence (1b) we want to turn to starts when H. suddenly points at something on the screen (line 1). On the screen we see the still of the interviewer and the boy focusing on a picture book lying on the table (see also figure 1a). Pointing is part of a sequence consisting of several turns and moves:

While H. is extending his left arm pointing toward the video projection (1), he suggests that the book lying in front of the child in the video is actually upside down adding "have a look at it." The statement is confirmed by C. (2) and, shortly later, by I. (3), who is in fact also the interviewer on the video, while both are looking at the video. After H. has ratified with a certain stress (AHA) he concludes "Indeed it is upside down" (4). What is important to us is what follows now: H. turns the paper copy he is holding upside down – an act that is subsequently mimicked by other participants. It is now on this new basis that he makes an indexical reference ("there") and asks to rewind the video and to continue the analysis.

The turning of the paper copy upside down obviously concerns his spatial perspective. This becomes clear from the spatial references, such as "upside

down," "it," and "there." Note that these references are spatially indexical, that is, their meaning is dependent on the position of the speaker. This is enforced by the pointing gesture, which implies bodily references to the monitor, and since each participant has a paper copy lying in front of them, there is also spatial reference to this copy – accounted for by the turning over of the copy.

The methodological task of understanding what the actors do in the video recordings obviously does not only presuppose understanding the lexical meanings of the words and the spatial references; it also demands an understanding of the indexical relation of the interpreter's body to the paper copy and what is spatially represented on it. Understanding refers on the one hand to the coparticipants in the setting which depends on what Schutz and Luckmann (1984) have called the "exchangeability of standpoints": H. can see the same on the video as each member in the group and that, by means of the copy, they could see the same representation as the child. On the other hand, understanding refers to the establishment of what is the *common object* in the situation represented on the video.[19] In fact, both levels presuppose an acknowledgement of the relation of H.'s subjective body's spatial position towards the object (and its representation).

Even if we take a relational view on space, we need to be aware that it is not only H.'s subjective body that matters; in fact, the coparticipants all take the same position to the object – or rather, turn the object to the same position to themselves. This way, they enact what may be called "subjective positionality" (Knoblauch 2020, 72ff). That means: as much as the interpretation depends on a relation among the actors (interchangeability) and the arrangement between them and the object, each subject's spatial position plays a decisive role (unless they can read inversely). This (relational) subjective positionality becomes particularly relevant if subjects in space are the "object" of study, but it is also involved in any observation. This obviously also holds true for cases in which data are mediated by technologies (such as video recordings, monitors and computers); the social studies of science provide good reasons for assuming that the same principle does not only apply to the social sciences but also for the natural sciences (Amann and Knorr Cetina 1990). In order to produce intersubjective knowledge, everyone who does science must observe its subject matter from their very own subjective bodily perspective and be able to relate it reciprocally to others; scientific knowledge cannot be passively internalized but everyone who does science

[19] The observation supports the methodological distinction between first order construction and second order constructs as suggested by Schutz. Second order, however, must not be considered "superior" but different, depending on scientific methods. While Schutz assumed these methods to rely on "rational" reconstruction by individual scientists, we must assume that rationality requires specific forms of scientific communication. Identifying these forms is an essential task for the ETOS. For a first attempt, cf. Knoblauch in print.

also needs to actively "understand" the explicit epistemology and reproduce at least its most essential categories, including logical relations and reasoning. It is only through their appropriation by subjects in terms of actions that the descriptive findings of reflexive research can turn into norms of action. Moreover, even postmodern, deconstructionist, and relativist scientists must at least implicitly accept that they orientate towards something they have in common, be they (as in some fields of mathematics) concepts, formulas, and other sign-based imaginaries, or be they (as in the empirical sciences) the "objects" that are represented by data. In addition, even the most realist and essentialist scientists must at least implicitly accept that the "reality" they are studying is not the reality we are confronted with in everyday life, but data that is, *per definitionem*, produced by their very actions, the objectivations produced by them, the technologies and media, and, of course, by the very methods of their actions.

6. Conclusion

As I could only address one aspect in one example, I must admit that reflexive methodology certainly needs to be elaborated with respect to more than just the methods of qualitative video analysis in question and their standardized variants (including the creation of codes); it also needs to be developed with respect to other kinds of data, approaches, and disciplines. It is also obvious that the idea of ETOS is still very open and even blank, as, for example, where the relation to ethics of science is concerned. Given the complexity of science studies and the philosophy of science, this vagueness may be an advantage as it allows to take into account other attempts to analyze sciences reflexively. Nevertheless, it should have become clear that the ETOS contains a distinct core, which, as I have argued in the beginning, is gaining relevance in contemporary knowledge society, should science remain to be more than the instrument of social groups, be they states, companies, or institutions of the civil society. The more society becomes enriched by science and the more everyday knowledge is augmented by scientific knowledge, the less clear it is what science may be. Given this situation, it seems quite understandable that in opposition to this transgression, an instrumentalist view of science is becoming popular in and even more outside of science. This view easily tends to exclude "softer" forms of science. In addition, it leaves the decision about what science is or should be to those who usufruct from science. Even if this position seems sound, as in the course of the Corona pandemic, it remains quite short-sighted to assume that science needs necessarily be useful to some social groups. As useful as some scientific results may be, it cannot be defined by its usefulness and therefore not by politicians, managers, and functionaries, even if they are in charge of science and scientific institutions.

In the face of the various kinds of demands on and interests by scientists, they are themselves not directly the ones to decide what makes science and scientific knowledge.

In this situation of two opposing tendencies, the empirical theory of science may constitute a way to refigure the relation between science and society. It maintains that all science is a social enterprise, and at the same time it fosters the specific reflexivity of the sciences. It avoids the relativism of radical constructivism yet does not deliver to the shallow realism of utilitarianism. It remains constructivist by focusing on the methods by which knowledge is produced, and it is materialist by acknowledging the role of objectivations that allow for a grounding of reflexivity. In drawing on everyday reflexivity, it maintains its basis in mundane rationality, while at the same time demanding an expertise in methodology. If science wants to lay claims on the specificity of its knowledge, it cannot rely on the popularity of its knowledge in more or less bounded scientific discourses or the authority of status positions and institutions. It must demonstrate its production and recognition through its methods in the kind of reflexivity sketched above.

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Historical Social Research

All articles published in HSR Special Issue 46 (2021) 2:
Positionality Reloaded.