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HAUPTBEITRÄGE

Research cultures as an explanatory factor

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Abstract In this article, we explore the potential of culture as an explanatory concept, using the sociology of science as an example. We first argue for a concept of culture that is sufficiently narrow to represent specific factors influencing human actions, and also propose such a concept. We then demonstrate that specific cultural assumptions can be derived from observations of researchers' practices and subsumed to our concept of culture by analysing Knorr-Cetina's comparison of epistemic cultures in high energy physics and molecular biology (*Epistemic cultures: How the sciences make knowledge*. 1999). In a third step, we use our own empirical material to discuss cases in which cultural factors contribute to explanations of researchers' behaviour. We conclude that cultural factors are rarely needed as

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contributions to multicausal explanations of research actions. If they are required, our approach provides a workable solution in the form of heuristic guidance in the search for cultural assumptions, a framework for comparing cultures and a basis for integrating cultural assumptions with other influences on action.

Keywords Research cultures · Sociological explanations · Research practices · High energy physics · Molecular biology

Forschungskulturen als Erklärungsfaktor

Zusammenfassung Das Ziel des Artikels besteht darin, das Potenzial des Kulturbegriffs in soziologischen Erklärungen am Beispiel der Wissenschaftssoziologie zu erkunden. Wir zeigen zunächst, dass ein zu Erklärungen beitragender Kulturbegriff eng genug sein muss, um spezifische Einflussfaktoren auf menschliches Handeln zu repräsentieren, und schlagen einen solchen Begriff vor. Wir benutzen diesen Begriff in einer Sekundäranalyse von Knorr-Cetinas Buch über epistemische Kulturen (*Epistemic cultures: How the sciences make knowledge*. 1999), in der wir aus Beobachtungen der Praktiken von Hochenergiephysikern und Molekularbiologen deren spezifische kulturelle Annahmen rekonstruieren. In einem dritten Schritt nutzen wir Fälle aus eigenen empirischen Studien, um mögliche Beiträge kultureller Faktoren zur Erklärung des Verhaltens von Wissenschaftlern zu diskutieren. Aus diesen Schritten lässt sich die Schlussfolgerung ziehen, dass kulturelle Faktoren nur selten zu Erklärungen des Forschungshandelns beitragen. Wenn sie herangezogen werden müssen, dann bietet unser Herangehen eine Heuristik für die Suche nach kulturellen Annahmen, einen Vergleichsrahmen für Forschungskulturen und eine Grundlage für die Integration von kulturellen Annahmen mit anderen handlungsbeeinflussenden Faktoren.

Schlüsselwörter Forschungskulturen · Soziologische Erklärungen · Forschungspraktiken · Hochenergiephysik · Molekularbiologie

1 Introduction

As in many other subfields of sociology, the sociology of science invokes the concept culture quite frequently. The idea of "research cultures" can be traced back to Fleck's (1979 [1935]) concept of "styles of thought" that are shared by "thought collectives". Several decades later, the constructivist and cultural turn in the sociology of science led to a perspective on science as culture (Pickering 1992), a focus on the "disunity of science" (Galison and Stump 1996) and the comparison of research cultures (Knorr-Cetina 1991, 1999; Galison 1997). Most of these approaches are descriptive and use the concept "disciplinary culture" as a marker for an unspecified or open-ended list of properties of one culture, or of differences between two cultures.

The idea of distinct research cultures has also played a role in the interdisciplinarity discourse, where differences between research cultures are sometimes cited as causes of problems in interdisciplinary collaboration (e.g. Duncker 2001) or in the evaluation of interdisciplinary research (see the contributions in Laudel and Origgi 2006). Again, the concept disciplinary culture is rarely defined in these studies and seems to serve as a container for all differences between disciplines, rather than as a concept.

A second stream of thinking about cultures in science emerged from the observation of national differences between research practices in one field. Analyses of "national culture" identify nationally specific attitudes, behaviour and research practices. For example, Galtung (1981) identified Nipponic, Teutonic, Saxonic and Gallic styles of research. Traweek compared Japanese and US American styles of high energy physics (Traweek 1988, p. 145–156). Differential effects of these cultures on the production of knowledge, if addressed at all, are only speculated on in these studies.

A potentially interesting third approach to research cultures was suggested by Collins (1998) in his study of "evidential cultures". Evidential cultures differ in their expectations in terms of the support of scientific statements by empirical evidence. These can cut across research fields and countries, thus forming a third "layer" of culture besides field- and nationally specific cultures. Unfortunately, Collins only provides descriptions of two evidential cultures in the gravitational wave detection community. Even more unfortunately, the two evidential cultures coincide with a distinction of a national community—all members of one culture are Italian and all members of the other are US Americans. Therefore, Collins's suggestion still needs to be put to the test.

Overall, "culture" appears to serve as a descriptive label for differences between research practices, rather than as an analytical concept. This observation motivates the questions addressed by the current article. Do we really need a concept of "research culture" for our explanations of knowledge production? Can such a concept be more than a summarized description of phenomena that are already captured by existing concepts? How would a concept of research culture that contributes to explanations in the sociology of science look? In this article, we want to explore the explanatory potential of a concept of "field-specific research culture". This involves three closely interwoven tasks. First, we must define the concept, i.e. delineate the phenomena that are to be treated as cultural phenomena. Second, we must identify conditions under which reference to such a concept provides a specific, irreplaceable contribution to causal explanations. Third, we must provide a framework capable of guiding the empirical investigation of research cultures and their comparative analysis.

The remainder of this article begins with theoretical considerations (section 2). We argue for a narrow concept of culture that addresses specific phenomena, i.e. for sacrificing breadth in order to gain explanatory power. We then develop our comparative framework by adopting basic dimensions of culture from Schein's (1985) concept of organizational culture. Specification of these still very generic dimensions is achieved by an analysis of Knorr-Cetina's (1999) comparison of the "epistemic cultures" of high energy physics and molecular biology (section 3). We then change our perspective and relate cultural factors to other factors influencing actions. Our discussion of several situations in which field-specific research cultures influence researchers' actions confirms the applicability of both conceptual considerations and a framework (section 4). As a conclusion, we discuss the limited use of disciplinary culture as an explanatory factor in the sociology of science, as well as the methodological difficulties of empirically identifying these cultures (section 5).

2 Conceptual considerations

Our intention to explore the utility of culture as an explanatory concept requires three preparatory clarifications. First, we must specify what we mean by "explanation". Our approach to sociological explanations follows the recent literature on social mechanisms (see Hedström and Ylikoski 2010 for an overview). Mechanismic approaches consider a phenomenon to be explained if mechanisms can be described that produce the phenomenon when specific conditions occur; namely conditions that are likely to trigger the mechanism and conditions under which the mechanism operates.[1] These conditions are rather complex, which is why social explanations in general (be they mechanismic or not) are multicausal (consider several interacting causal factors) rather than monocausal. We expect culture to play a role as one of these causal factors.

A second necessary clarification concerns the criteria a concept of culture must fulfil in order to serve as an explanatory factor. Based on the considerations above, it follows that, in order to be part of a sociological explanation, the concept of culture must be *specific*, meaning *relatively narrow* and *homogenous*. This demand is motivated by our observation of a trade-off between scope and explanatory power of concepts of culture (Fig. 1).

The wider the scope of the concept, the less it contributes to explanations. The widest possible scope was originally used in anthropology and included the entire way of life of a people (Geertz 1973; Swidler 1986, p. 273). This definition was not used with the intention to "explain" phenomena by culture. Such an explanation is obviously not possible with such a wide definition, because explanandum and explanans belong to the same concept. The same applies to the concept of subculture when it is used in the sense of sub-society (e.g. Macbeth 1992; Bennett 1999; for a critique of this use see Fine and Kleinman 1979). Narrower definitions restrict the concept to shared symbols, beliefs and practices (Swidler 1986, p. 273); sometimes including values and artefacts (Taras et al. 2009). Such narrower definitions are also used for epistemic or disciplinary cultures (Traweek 1992, p. 437–438; Knorr-Cetina 1999; Fry 2004; Fry and Talja 2007). These limit the number of phenomena to which the concept of culture can be applied; however, at the same time, they increase the concept's explanatory power. Culture in this narrower sense may or may not be one of the causes of a phenomenon, because it is a specific condition under which a mechanism may operate. The concept remains, however, heterogeneous; because it includes actions (practices), beliefs, symbols and sometimes artefacts. This is theoretically impractical because the concept "culture" cannot be subsumed to one more general

[1] Following Mayntz (2004, p. 241), we define a social mechanism as a sequence of causally linked events that occur repeatedly in reality if certain conditions are given and link specified initial conditions to a specific outcome (for a similar but less precise definition, see Merton 1968, p. 42–43).

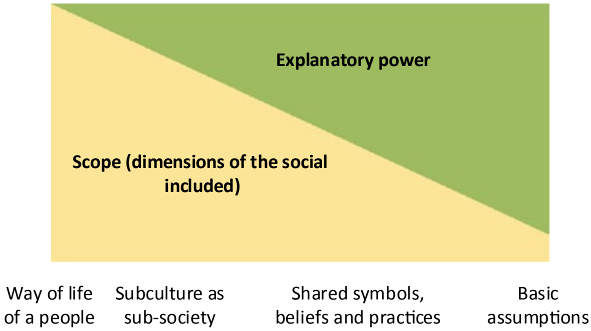

Fig. 1 Trade-off between breadth and explanatory power of concepts of culture

concept and methodologically impractical because the operationalization of the concept inevitably results in a "shopping list" of quite different empirical phenomena.

The considerations above constitute the rationale for wanting to introduce a more specific concept of culture. Our starting point here is the definition of organizational culture by Schein (1985, p. 6) as "the deeper level of basic assumptions and beliefs that are shared by members of an organization, that operate unconsciously, and that define in a basic 'taken-for granted' fashion an organization's view of itself and its environment". In Schein's view, these basic assumptions are learned responses to problems a group has had to face in the past. If repeatedly successful, these are transformed into taken-for-granted assumptions and not questioned anymore in current activities (1985, p. 6).

Schein's conception is not limited to organizations, as he suggests that "the word culture can be applied to any size of social unit that had the opportunity to learn and stabilize its view of itself and the environment around it" (1985, p. 8). Accordingly, research cultures can be national, disciplinary, laboratory or community cultures. Furthermore, according to Schein, culture is expressed in social practices and artefacts, but the practices and artefacts themselves do not belong to the culture.

Although Schein's definition of culture is sufficiently specific and homogeneous, two aspects of his conceptualization must be challenged. First, the idea that culture only emerges from successful problem solving is overly functionalist and theoretically unnecessary. If culture is analytically treated as explanans, no assumptions about the emergence of culture need to be made. If culture is the explanandum, the question of its emergence can be empirically studied. Second, Schein introduces his definition of culture ad hoc, without clarifying how the set of basic assumptions constituting culture is theoretically linked to conceptualizations of actors' assumptions in a theory of action.

Both criticisms can be met by turning to Goffman's concept of "frames" or "frameworks", which are compatible with Schein's definition but do not carry the functionalist limitation. Going back to Goffman (1974), we can define primary frames as principles organizing humans' basic experience of the world. They form a special cognition and simplification, as well as an interpretation of the situation as a basic means for action. Goffman calls these frameworks "primary", exactly because a primary framework "is seen by those who apply it as not depending on or harking back to some prior or original interpretation" (1974, p. 21), a property it shares with Schein's "basic assumptions" (see above). Goffman groups primary frames into specific basic assumptions when analysing culture as "an image of a group's framework of frameworks—its belief system, its 'cosmology'" (1974, p. 27). Although these assumptions can be reflected upon later in situ, they provide a "background understanding" (1974, p. 22) of situations, which operates at such a basic level that it is not seen as an interpretative act by the actors themselves.

With Schein and Goffman in mind, we define culture as *a primary framework that consists of basic taken-for-granted assumptions, is shared by the members of a specific social unit and organizes the recognition and interpretation of the unit and its relevant environment*. This concept of culture is consistent with the notion of an actor's "interpretive schemes" (Schütz 1967; Giddens 1979) and White's definition of culture as "a continuously interacting population of interpretive forms articulated within some social formation" (White 1992, p. 289). The basic assumptions concern the existential conditions of a social unit, its internal integration (including the motivation of its members to participate), typical practices of the social unit and the multiple environments in which it is embedded.

The definition of culture as the primary framework of a social unit, although sufficiently specific and homogeneous to be operationalized, nevertheless leads to four methodological problems for the empirical study of culture.

A first problem concerns the causal attribution of effects to culture, rather than other factors shaping action. Since the practices in which a social unit's culture is reflected are shaped not only by culture but also by a variety of other factors, the question arises under which conditions culture has a specific impact, i.e. makes a difference. Taking our inspiration from Swidler's (1986, p. 278–284) distinction between "settled lives" and "unsettled lives", we distinguish between culture in equilibrium and non-equilibrium situations. In equilibrium situations, the cultural factors exert the same influence (suggest the same actions) as the other elements of the situation. As Swidler puts it with regard to settled lives:

> In settled lives, culture is intimately integrated with action; it is here that we are most tempted to see values as organizing and anchoring action; and here it is most difficult to disentangle what is uniquely 'cultural' since culture and structural circumstance seem to reinforce each other (1986, p. 278).

In equilibrium situations, culture influences action but this influence cannot be empirically distinguished from that of other influencing factors. In non-equilibrium situations, the influence of cultural and other factors diverge. While Swidler limits the notion of unsettled lives to "periods of social transformation" (1986, p. 278), we include all situations—however temporary and whatever their trajectory—in which the influence of non-cultural factors conflicts with the influence of culture. In these situations, the causal role of cultural factors can be identified because they can be distinguished from and compared to other factors influencing action. Our search for examples of cultural influences in research revealed several such non-equilibrium situations (see below, section 4).

A second problem of causal attribution concerns the ascription of observations to a specific culture. Since all people are members of numerous overlapping social units, their practices express the cultures of all these units. Applied to research cultures, this problem can be formulated as follows: If we identify "cultural" assumptions, does this pertain to the culture of the research group, the research organization, the field, the discipline or the country?

The third problem arises from the fact that people are only partially aware of their cultures. This is why the study of culture faces the general methodological problem of empirically investigating a phenomenon of which the subjects of the research are not fully aware. While this problem is not insurmountable, the investigation of such phenomena may require specific methodologies.

A final problem concerns the comparison of cultures. In order to establish a causal role of culture in the explanation of actions, we need to comparatively assess cultural factors. If such a comparison is to lead to more than just a list of differences, it must use a systematic framework of relevant dimensions in which cultures vary. Schein's distinction of basic assumptions provides us with a starting point and a heuristic for developing such a framework. The list includes assumptions about the

- relationship to the environment,
- nature of reality, time and space,
- nature of human nature,
- nature of human activity and
- nature of human relationships (Schein 1985, p. 14).

These assumptions can be specified for each collective whose culture is investigated. In the case of research cultures, the collective whose culture we are interested in—the scientific community—shares a specific work process, namely the investigation of reality that is aimed at producing new knowledge about it. In this sense, reality is the subject matter of the community's central activity and assumptions about reality are closely linked to assumptions about activity. These assumptions exist on different levels of abstraction, because each scientific community investigates a specific part of reality, and its members are therefore likely to share basic assumptions not only about reality in general but also about its particular subject matter.

If we apply these general considerations to the comparative description of cultures of research communities, the basic assumptions about the nature of reality can be specified as follows:

1. The most general assumptions are ontological assumptions about the existence of research objects in a material or social world.
2. Epistemological assumptions concern the opportunity to produce knowledge about research objects and about possible approaches to knowledge production.
3. At a lower level of abstraction, research cultures include assumptions about research objects and their behaviour in investigations.[2]

It is also possible to specify assumptions about human nature, the nature of human relations and the nature of human activity for scientific communities. These are likely to include assumptions about the properties that make a researcher a member of the com-

[2] It is important to distinguish these assumptions from the theoretical and methodological knowledge applied by the community. Scientific knowledge is explicit, formalized to a significant extent and constantly addressed in scientific communication. Only part of it turns into taken-for-granted assumptions about objects and their behaviour.

munity, i.e. assumptions that delineate the "imagined community" (Anderson 1991). Since scientific communities have collective identities that are based on the perception of working with the same knowledge and investigating the same section of reality, the assumptions concerning human nature and activity can be specified as follows:

4. The most general assumptions about the work process concern the objects, practices and outcomes of research (knowledge) that are shared by community members.
5. More specific assumptions about the community's work process include assumptions about what constitutes a scientific contribution and the ways in which it can be produced.
6. Finally, there are even more specific assumptions about what community members do, i.e. about their behaviour in specific situations.

This leaves:

7. The assumptions about a community's environment, which are likely to include two environments in particular; namely other scientific communities and other societal environments.

These assumptions appear to cover the range of basic assumptions suggested by Schein and specify them for the cultures of research fields. However, they still are too abstract to serve as dimensions of a comparative framework. In the following section, we attempt to obtain sub-dimensions that are more specific by analysing Knorr-Cetina's book *Epistemic Cultures*.

3 Dimensions for the comparison of cultures

3.1 Rereading Knorr-Cetina's *Epistemic Cultures*

Knorr-Cetina's book *Epistemic Cultures* (1999) provides a detailed and empirically well-grounded comparison of the "epistemic cultures" of high energy physics and molecular biology. It is difficult to assess the relationship between our concept of culture and the concept used by Knorr-Cetina. Consider the first sentence of the book's introduction:

> This book is about epistemic cultures: those amalgams of arrangements and mechanisms—bonded through affinity, necessity and historical coincidence—which, in a given field, make up *how we know what we know*. (Knorr-Cetina 1999, p. 1, emphasis in the original)

In the later "Culture and Practice" section, Knorr-Cetina clarifies her understanding of the notion of culture as follows:

> *Culture*, as I use the term, refers to the aggregate patterns and dynamics that are on display in expert practice and that vary in different settings of expertise (Knorr-Cetina 1999, p. 8, emphasis in the original).

Both statements lend themselves to interpretation as subculture *and* to interpretation as shared symbols, beliefs and practices, and values and artefacts (see above, section 2). We can therefore assume that Knorr-Cetina employs a wider concept of culture than we use in this article, but cannot tell exactly how much wider her concept is.

With this difference in mind, we "coded" the chapters describing the epistemic cultures of high energy physics (chaps. 3, 5, 7 and 8) and molecular biology (Chaps. 4, 6 and 9). We collected passages from which cultural assumptions could be derived; collated them according to the assumption we believed them to express; formulated the assumption; described the practice in which it was expressed and constructed sub-dimensions that linked the cultural assumption to one of the seven general dimensions. Although we had the list of general cultural assumptions in mind when analysing the book, our coding was a bottom-up process, rather than being theoretically guided. The seven general dimensions provided above are too abstract to guide even an empirical analysis of texts. We used them as "sensitizing concepts" (Blumer 1954) in our search for information about the two cultures.

The major results of our analysis are presented in Table 1. They are unavoidably patchy. Analysing a comparative description that is based on a specific concept of culture with a perspective based on a different concept is unlikely to yield consistent results. We found some of our general dimensions to not be covered by Knorr-Cetina's comparison at all, as well as information about some sub-dimensions to be provided for only one of the communities and cases in which both cultures were described in the same sub-dimension. Table 1 includes only sub-dimensions for which information about both cultures was found. With the exception of the epistemology dimension, our analysis yielded comparable information.

There are only few indirect hints at the "ontological beliefs" of the scientists Knorr-Cetina observed. Nevertheless, it becomes clear from Knorr-Cetina's comparison of the two cultures that both high energy physicists and molecular biologists are realists, who believe that they investigate natural objects that have an existence independent of the researcher's action. Interestingly enough (and this is a cornerstone of Knorr-Cetina's comparison), this belief is based on and expressed in quite different practices. High energy physicists rarely deal with natural objects. They create and analyse traces of objects which they cannot see or handle directly. Only the traces of objects can be accessed. However, the physicists' handling of these traces is based on the assumption that they are *traces*, i.e. that conclusions about natural objects can be drawn from handling them. Both aspects of their work become apparent in the following quote:

> KK: ((How do you measure the W mass?))
> JJ: (()) If one looks at the experimental spectra ((distributions)) of the W mass, one gets an impression of where the W mass lies. But one has to run an MC [Monte-Carlo-Simulation] to describe the data. Because it takes into account not only the decay properties of the W boson, but also *how my detector reacts to it*.
> (Interview with a physicist, Knorr-Cetina 1999, p. 54–55, emphasis in original)[3]

[3] In the transcripts of interviews taken from Knorr-Cetina's book, double parentheses indicate comments by the transcriber (Knorr-Cetina 1999, "A Note on Transcription"). The explanation "[Monte-Carlo simulation]" was inserted by us.

Table 1 Cultural assumptions of high energy physicists and molecular biologists derived from Knorr-Cetina (1999)

Dimension	Sub-dimension	Community	Assumptions	Reflection in epistemic practices	Pages
Ontology	Existence of the research objects	High energy physics	Realism: assumption that the natural world exists independently of researchers	Signals are treated as representatives of material incidents (natural and quasi-natural objects)	48–50, 54–55
		Molecular biology		Manipulation of natural and quasi-natural objects	84–88, 100–106
Epistemology	Ways in which knowledge about research objects can be produced	High energy physics	Research activity is constructive	Signals are treated as results of theoretical and technical decision; experiment is a series of selection processes, self-observation, self-reflection of the scientist's influence in the measurements, constant monitoring, analysis of the monitoring	51–63, 79
		Molecular biology	Complexity of objects is major obstacle to producing knowledge	Effort to standardize research objects and processes (mutants, cell lines etc.), treating objects like machines and rebuilding the original to understand	83, 141–149, 156
Objects of research and their behaviour	Sources, relevance and control of uncertainty	High energy physics	Dependence of results on countless unidentified factors, understanding the measurement process is crucial for obtaining results	Simulation of the measurement process, theory is crucial for interpreting measurements, anthropomorphizing description of the instruments and processes, complete personal absorption (attempt to memorize all the variables), framing individual collider experiments with the results of other experiments	111–126, 128–129
		Molecular biology	Dependence of results on countless uncontrollable factors, manual skills and control of experiments are crucial for obtaining results, marginal role of theories and scepticism about models, importance of cleanliness and hygiene in the laboratory	Failure is part of everyday experience, trial-and-error approaches, application of locally passed on practical/empirical knowledge and visual practical knowledge to interpret results, "blind variation", constant purification and sterilization activities	84, 87–98, 155

Table 1 (Continued)

Dimension	Sub-dimension	Community	Assumptions	Reflection in epistemic practices	Pages
Work process	Timespan for the production of results	High energy physics	Results emerge only after long periods of time	Conference presentations are often status reports on the progress of building the experiments	168–169
		Molecular biology	Shorter periods, which vary widely	Trial-and-error behaviour in experiments, unpredictability of outcomes, close links between experiments and career progress	227–231
Work process	Object of competition	High energy physics	Chances to contribute in experiments	Competition for participation in experiments by advocating one's potential contribution, strategies to keep as many groups as possible in the experiment, authorship and conference presentations decoupled from individual contributions	166–171, 193–201
		Molecular biology	Chances to be the first to produce results	Competition for priority of important results, constant struggle over authorship of publications, sharing information and materials only as exchange	224–240
Work process	Role of collaboration	High energy physics	Success depends on expertise of others	Deliberately coordinated research actions and subordination in the division of labour	130–135, 179–186
		Molecular biology	Success depends on the collaboration/contribution of many	Fragmentation into individual's research projects, exchange of research objects, methods, services within and between research groups	217, 222, 224, 234–239
Work process	Origin of scientific contributions	High energy physics	Impossibility to ascribe publishable results to individuals	Dissertations are defended alone but the paper is published under the name of the collective of the experiment	130–135, 166, 221
		Molecular biology	Limited possibility to ascribe publishable results to individuals	Co-authorship conflicts are common	167, 226, 231, 234

Molecular biologists, on the other hand, directly manipulate natural and quasi-natural objects in their everyday research.

We could also identify "epistemological assumptions" concerning the ways in which information about research objects can be produced, if at all. It became obvious from the practices and statements of high energy physicists that they are constructivists, in that they assume to construct their research findings by technological work, setting parameters, invoking theories, mixing simulations with experimental data and other techniques. In some sense, this assumption is due to the complexity of the *experimental procedure*. In contrast, the dominant assumption of molecular biologists is that their *objects* are complex. Molecular biologists must reduce this complexity in order to produce reliable results; hence the concern with standardization of objects and methods, purification, and avoidance of contamination. The text does not provide enough information for deriving the complementary assumptions, i.e. high energy physicists' assumptions about the complexity of their objects and molecular biologists' assumptions about the complexity of their methods.

A set of "beliefs about research objects and their behaviour" concerned the sources, relevance and control of uncertainty. The description of high energy physicists' research practices demonstrated that they know that, due to the elusive nature of their research object, the success of their research depends on countless factors, only some of which are already identified at any stage of the experiment. High energy physicists perceive their work as a struggle for control over the measurement process (see the quote above) and have little confidence in any individual collider experiment.

> … physicists convey a more urgent lack of trust of single experiments and measurements than we see in other fields, a disbelief that these experiments can stand on their own as they do elsewhere, supported exclusively by the special linkage they have created between themselves and nature. High energy collider experiments stand first and foremost *in relation to each other* ...
> (Knorr-Cetina 1999, p. 74, emphasis in original)

Molecular biologists also have to cope with uncertainties. However, in their case the limited understanding of the behaviour of organisms leads to a struggle over the control of the whole experiment. Contrary to high energy physics, this struggle is fought by trial and error. Theories play a marginal role and models are regarded with scepticism.

> If there is a general strategy molecular biologists adopt in the face of open problems, it is a strategy of blind variation combined with a reliance on natural selection. They vary the procedure that produced the problem, and let something like its fitness—its success in yielding effective results—decide the fate of the experimental reaction. Variation is "blind" in a very precise sense: it is not based on the kind of scientific investigation and understanding of the problem that was so popular among high energy physicists. Confronted with a malfunctioning reaction, a problem of interpretation … or a string of methods that do not seem to work, molecular biologists will not embark, as physicists will, on an investigative journey whose sole purpose is to understand the prob-

> lem. Instead, they will try several variations in the belief that these will result in workable evidence. (Knorr-Cetina 1999, p. 91)

In laboratories, the researcher's practical skills and the prevention of contamination are much more important than theories, not least because too little is known about complex living organisms.

The text also describes sets of "beliefs about the work process". One of these assumptions concerns the time it takes to produce scientific results. High energy physicists are researchers who build an experiment over 20 years. This does not mean that they don't produce results during this time. However, these results are understood as intermediate. The much shorter time horizons of molecular biology research could be derived only indirectly from descriptions of experiments and the competitive behaviour of molecular biologists. Nevertheless, it becomes clear that the biologists studied assume experiments to yield results in the relatively short timeframes of PhD student and postdoctoral positions. The variation of time horizons is partly due to the unpredictable behaviour of biological objects.

Another assumption about the work process concerns the nature of competition in the field. In high energy physics, where very few large experiments are designed and prepared over a period of 20 years, opportunities to contribute to the production of results are perceived as scarce. This assumption is expressed in the competition for participation in the experiment, i.e. for the opportunity to contribute to the work.

Practices of molecular biologists, on the other hand, are shaped by the belief that a researcher must constantly compete for priority of important results. Molecular biologists believe they can, want to and need to contribute important findings; which creates competition between laboratories (for being first with some finding) and within laboratories (for having one's name linked to a specific finding via first authorship). For the same reason, the sharing of information and materials may become problematic if no reward is offered in exchange.

Closely related to the assumptions about competition are assumptions about the actor who creates scientific contributions. High energy physicists assume that it is impossible to ascribe a contribution to an individual because all individuals are part of a large collaboration, on which they depend in their work and to which they contribute. This leads to the paradoxical situation that a PhD student conducts his work in the context of the experiment, submits a thesis at his home university as an individual, and is one of many alphabetically ordered authors when "his" results are published. In molecular biology, the possibility to ascribe findings to individuals is assumed to be limited. However, there are fewer contributors, and a distinction between creative contributions and services is often made (and contested in struggles over authorship).

This analysis of Knorr-Cetina's comparison of two epistemic cultures demonstrates that it is possible to deduce specific cultural assumptions from practices and statements. Therefore, it is possible to specify our comparative framework and to obtain cultural factors that are specific enough to be integrated in explanatory frameworks. Furthermore, this analysis is a first test of the general framework's scope. Since all cultural assumptions we could derive from the text could also be subsumed to one of the seven main dimensions, our framework seems to be exhaustive at the highest level of abstraction. Further tests are of course necessary.

4 Cultural factors as specific causes of behavioural change

Having explored the possibility of using our concept and framework for a comparison of research cultures, we now turn to the possible causal role of cultural factors. For this step we draw on empirical material from two of our own research projects.[4] In the following, we describe non-equilibrium situations by pointing out tensions between elements of culture and another aspect of the situation. We use our comparative framework for describing the relevant dimensions of culture.

4.1 Intercultural conflicts

Non-equilibrium situations can be caused by conflicts between different research cultures, which may arise whenever researchers from different fields collaborate. In these collaborations, researchers working in different cultures produce contributions that must fit. Whenever aspects of these fits are not negotiated because they are taken for granted by one or both sides, contributions might be difficult to integrate and cultural differences become apparent.

Computational Neuroscience (CNS) is a field that aims at modelling neurobiological information processing with the help of computer simulations (for a historical overview, see Bower 2013). To evaluate their computer simulations, CNS researchers usually seek confirmation from neurobiological experiments. Over the last 20 years, the community acquired knowledge that enables simulation of information processing within a wide range of laboratory animals, including rodents and monkeys. More recently, CNS researchers also got involved in psychological studies of the neurobiological foundations of human information processing, such as learning and decision making.

In one of these projects, assumptions of CNS researchers about the behaviour of research objects—which were derived from earlier experiments with animals—implicitly clashed with parallel assumptions of psychologists about humans. The CNS researchers who programmed the computer simulation had previously collaborated with neurobiologists experimenting with animals. In these experiments, animals underwent special training that conditioned them to repeat simple tasks for many hours per day. Stimuli were applied to animals according to a Gaussian distribution. In the course of these collaborations, the "Gaussian distribution of stimuli" had turned into a taken-for-granted assumption of the programmers. However, psychologists, with whom the programmers were now to collaborate, have different assumptions about their research objects and apply stimuli in a different way. Based on the assumption that human subjects perceive simple tasks as boring and monotonous, and that their attention span will decrease rapidly, the psychologists routinely applied the experimental stimuli according to a bi-Gaussian distribution. This approach is

[4] The following examples are taken from a project on visual communication in science funded by the Deutsche Forschungsgemeinschaft (KN 298/8-1) and a project on the development of cyber-infrastructures for science (01UG1005) funded by the German Ministry for Education and Research. We gratefully acknowledge contributions by René Wilke and Sonja Palfner, who conducted some of the interviews. All interviews were conducted in German; quotes were translated by the authors.

recommended in psychological textbooks, but violated a basic assumption underlying the computer model:

> The model assumed that the results that someone sees are Gaussian distributed. But [the psychologists] said, "ah, people find that boring, they learn too quickly" and they replaced it by a bi-Gaussian distribution […] and that practically destroyed the whole model. (CNS researcher)[5]

The conflict between the assumptions held by the collaborators was noticed only after the psychological experiment was completed. As a result, most of the project's data were unsuitable for publication. This case documents the taken-for-granted nature of cultural assumptions, which let the researchers recognize their cultural differences only once it was too late.

4.2 Conflicts between cultures and epistemic conditions of actions

Among the conditions shaping research actions, epistemic conditions of action play a crucial role. Epistemic conditions of action are "conditions produced by the 'technology' (materials, means, and practices) of creating knowledge" (Gläser and Laudel 2004, p. 14). Research practices must be adopted to epistemic conditions, because the success of these practices—i.e. in producing new knowledge that is relevant, valid and reliable according to the standards of one's community—depends on the match between research practices and those properties of objects, knowledge and methods that are not malleable in the current research situation. A community's epistemic conditions of action are reflected in deep cultural assumptions about the nature of reality; the nature and accessibility of research objects and about the possibilities for producing new knowledge about these objects. Practices shaped by these assumptions (and thus expressing them) have repeatedly proven successful.

Given these close relationships between epistemic conditions of actions, research practices and cultural assumptions, cultural assumptions seem unlikely to become misaligned with epistemic conditions of action. Nevertheless, misalignment may occur when a turn to new research objects or methods rapidly changes epistemic conditions of action, while the more inert research culture adapts only with a certain delay.

Such a conflict between epistemic conditions of action and research culture occurred with the introduction of new computer- and internet-based methods in the field of textual criticism, as part of a general movement for creating "digital humanities" (for an overview, see Burdick et al. 2012). The field of textual criticism aims at producing critical and reliable editions of literary works according to scholarly principles such as accuracy, adequacy, appropriateness, consistency and explicitness. Traditional practices of the field are hermeneutic and based on subjective iterative interpretation of intellectual phenomena.

[5] In the quotes from interviews, [text in parentheses] indicates omissions and changes that were necessary to protect interviewees' identity.

The introduction of computer-based methods creates epistemic conditions that are at odds with traditional cultural assumptions of the field. Producing digital scholarly editions of literary works requires systematic approaches to identifying and analysing relevant phenomena in literary studies, and also procedures and objects that can be formalized and applied according to standardized rules. These new epistemic conditions of action (for example software-supported approaches to identify, transcribe, compare, annotate, compile or link text-based sources) are at odds with cultural assumptions about research objects and the ways in which knowledge about them can be produced, as is described by a literary scholar engaged in software development:

> [In the humanities] there is no neutral attitude to something like formalization. On the contrary, the attitude is rather negative. Basic concepts of the humanities are, for example, the individuality of objects, as well as individual and subjective approaches to, e. g., works of art. All this conflicts extremely with the idea of treating 1000 texts in the same manner. (Digital humanities scholar)

Standardization activities and publications about models, methods or tools are not accepted as relevant scientific contributions within the "classical" editorial community, or may even be judged as "rubbishy":

> And there are people who say "You don't follow the pure doctrine of literary and linguistic interpretation. You do informatics instead, which is inferior." (Digital humanities scholar)

The initiatives aimed at promoting the digital humanities methods thus find themselves in conflict with the culture of literary studies in several dimensions. Epistemological and methodological assumptions about research objects and the role of the researcher's subjectivity are incompatible with formalization and standardization. Consequently, the production of standardized outcomes by digital methods is also at odds with cultural assumptions about the nature of scientific contributions (as individualized offers of interpretation).

So far, this conflict mainly affects the status and career prospects of digital methods advocates in their communities. In response to the conflicts described above, some of the advocates of digital methods reduce their "pure" research activities in favour of the development and professionalization of the technical and social infrastructure. Others try to reduce the gap between the classical and the digital editorial communities by focusing on the theoretical and methodological questions related to formalization and standardization in scholarly editing.

This case represents a major clash between new epistemic practices and traditional research cultures in a field. A similar case in the natural sciences, i.e. the introduction of NMR spectroscopy in chemistry, was studied by Reinhardt (2011). It is currently difficult to predict whether the humanities will incorporate the new methods and change their culture, as happened in chemistry.

5 Conclusions

In this article, we explored the potential of culture as an explanatory concept. We started from the premise that in order to be used in explanations, the concept of culture must be defined sufficiently narrowly, to represent specific factors influencing human actions. We used the concept of organizational culture proposed by Schein (1985) and developed a definition that is linked to social theory and that can be specified for comparative and causal analyses. Our analysis of Knorr-Cetina's (1999) comparison of epistemic cultures in high energy physics and molecular biology demonstrated that specific cultural assumptions can be derived from researchers' practices. We then searched our own empirical material for cases in which cultural factors could contribute to explanations of researchers' behaviour. Following Swidler's (1986) suggestion that cultural factors play a more prominent causal role in unsettled than in settled lives, we discussed situations in which a culture contradicts another culture or epistemic conditions of action. In each case, the conflict did indeed change the actions of researchers in whose situation the conflict occurred.

Our study was focused on the link between theory and methodology, and thus does not lead to conclusions about research cultures and their effects. However, we can draw three methodological conclusions from our analyses. First, our comparative framework appears to work, in that it enables a comparison of research cultures and the identification of cultural factors that may, in specific situations, exercise a distinct influence on actions. The fact that our examples of causal effects of cultural assumptions largely drew upon assumptions that were not included in Knorr-Cetina's comparison comes as no surprise. Culture is a complex phenomenon that may resist exhaustive description. In any case, an exhaustive description is pointless, because causal analyses should focus on the factors that do make a difference.

The challenges of empirically identifying cultural assumptions and the difficulties involved in ascribing them to a specific collective make the inclusion of cultural factors in explanations a demanding task. However, our second conclusion is that this task is unlikely to occur too often. The revision of our own empirical material from four very different projects returned only very few cases in which culture was required as an explanatory factor. Most of the situations we investigated represented "settled lives", in which epistemic, economic, sociostructural and institutional factors coincided with cultural ones, and had a direct and stronger impact on actions.

Our third conclusion concerns the methodological problems we anticipated in our theoretical discussion. Our empirical analyses confirmed the prevalence of these problems. In our analysis of Knorr-Cetina's material, we attributed the cultural assumptions to the communities of high energy physicists and molecular biologists. However, Knorr-Cetina sometimes mentioned internal variation (between experiments in high energy physics or between laboratories in molecular biology). Therefore, the specific group to which cultural assumptions can be ascribed did not always become clear, an observation that was supported by the analyses of our own material.

Another problem concerns the inference of cultural assumptions from the empirical material, which also turned out to be difficult in the analysis of Knorr-Cetina's book and in our own empirical cases. Although it is not impossible to deduce cultural assumptions from observed practices or interview responses, a search for cultural assumptions in data that were collected for another purpose faces significant additional difficulties. This is where we see a methodological dilemma. Collecting data about research cultures requires a specific approach, particularly where interviews are concerned. Cultural factors cannot be explored with one or two additional questions in interviews. This is why it seems difficult to include cultural factors as one explanatory factor alongside others in interview-based studies. Ethnographic observations appear to be the method of choice. However, ethnographies are time consuming, which severely limits the number of cases that can be compared (and thus the research questions that can be answered).

Therefore, research cultures are difficult to investigate empirically but rarely required as contributions to multicausal explanations of research actions. If, however, they are needed, our approach to culture provides a workable solution in the form of heuristic guidance in the search for cultural assumptions, a framework for comparing cultures and a basis for integrating cultural assumptions with other influences on action.

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