Cold Atom Gases, Hedgehogs, and Snakes

The Methodological Challenges of Comparing Scientific Things

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ABSTRACT

While several "grand narratives" have been developed to account for the impact of scientific things on scientific practice, there is still very little methodological support for comparative analyses of scientific things. The goal of our article is to sketch the methodological challenges involved in comparatively analyzing scientific things and including their properties in middle-range theories of scientific practice. Methodological challenges arise from the necessity to use scientists' accounts of scientific things, the dilemma between depth and breadth of comparative case-study approaches, and from the necessity to compare accounts of scientific things to each other as well as to social conditions of research. Since the dominant approaches to the study of scientific things avoid the middle levels of abstraction, we suggest using an approach based on a theory of action. Two examples from a recent study of conditions for scientific innovations illustrate our approach to the comparative analysis of properties of scientific things.

KEYWORDS

comparative science studies, epistemic properties, interviews, materiality, middle-range theories, sociological explanation

The Challenges of Comparing Scientific Things

Scientific things—which we define as material objects, means, or conditions of human action—tend to resist inclusion in sociological explanations because their materiality involves factors whose morphology and dynamics cannot be sociologically explained or easily integrated with sociological variables in explanatory frameworks. For example, the fact that a certain atomic gas liquefies at specific temperatures and pressures may greatly affect the actions of a scientist. However, the description given above will remain an idiosyncratic factor that is exogenous to a sociological explanation. Similarly, the duration of research processes depends on the *Eigentime* of scientific things—the dynamics of research objects and methods such as the speed of growth of bio-

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logical systems or the frequency of occurrences of phenomena. Again, these dynamics cannot be analyzed sociologically. While science studies have developed general models for describing the relationships between human actors and scientific things, including such things in comparative sociological research aimed at explaining scientists' behavior still poses many theoretical and methodological challenges.

At first glance, the problem we address in this article seems to be very specific: How to compare the impact of one scientific thing on human action (a) to the impact of another scientific thing, and (b) to the impact of actors, institutions, and other social phenomena? This question is phrased in a rather old-fashioned way as a problem of a theory of action and appears to ignore post-humanist approaches to non-human agency. It emphasizes comparisons and thus lies outside the mainstream of single-case ethnographies. Finally, it is most relevant to studies aimed at causal explanations, which is not a major approach in science studies either.

A typical example of this research would be a project aimed at ascertaining how (through which mechanisms, with which effects) specific social structures such as evaluation systems affect the content of research. Answering this question requires an assessment of all factors shaping the conduct and content of research, including factors contributed by scientific things. Since causal ascription is possible only if we compare varying sets of conditions under which effects occur, research processes must be compared, and thus the use of different things.

Comparing scientific things to each other as well as to social factors influencing actions involves methodological and theoretical challenges that are of interest beyond our specific approach to comparative research. The purpose of this article is to discuss these difficulties and our preliminary solutions to them, which will hopefully promote a discourse about the potential of comparative science studies.

We begin by discussing problems of data collection, which confronts us with some of the fundamental dilemmas science studies have been struggling with for several decades. We then turn to frameworks for the comparison of influences by scientific things, arguing that the dominant approaches ("Actor-Network Theory" and the "Mangle of Practice") do not support comparative approaches, and presenting a modest attempt to use a theory of action. Two examples from a recent project demonstrate how properties of things can be integrated into explanations for changes of research practices. As a conclusion, we highlight the preliminary character of our solution to the methodological problems.

Problems of Data Collection

When studying the role of scientific things in scientists' actions, we face the limitations inherent to data collection in social research. We can either collect information about things by handling them ourselves or by letting other actors (in our case, scientists) handle them. In the latter case, we can produce accounts of the role of things by observing these scientists, initiating accounts of things by asking them or using existing texts about things (that is, accounts produced by other actors for their own purposes). In the case of scientific things, relying on our own accounts is often difficult because our use of them is likely to fundamentally differ from their use by scientists. Without specialist training, "handling" an atom or a microscope is either impossible or results in experiences that are fundamentally different from the ones scientists have.

This is why sociologists usually observe scientists' use of things, interview them about it, or analyze written accounts of scientific things. Exploiting one of these opportunities still involves two important methodological choices: Whose accounts are to be used? Which empirical methods are to be applied?

Whose Accounts?

Accounts of scientists' use of things can either be produced by the independent sociological observer or can rely on accounts produced by the observed scientists. The former approach was first implemented as a methodological principle by Latour and Woolgar who conducted their ethnographic observation with the perspective of a "very naïve" version of a naïve observer. Latour describes the naïve investigator's perspective as that of an

> outside observer who does not know the language and the customs of the natives who are not supposed to read what he writes. As Woolgar has pointed out many times, … this is a very naïve version of the naïve observer—a version that is now abandoned in mainstream ethnography and which seems to survive in so called "lab studies". (Latour 1990: 146)

Latour and Woolgar chose this approach because they did not regard prior cognition as a necessary prerequisite for understanding scientists' work, and because they wanted to avoid the danger of "going native", that is, subconsciously adopting the interpretive schemes of the observed scientists (Latour and Woolgar 1986: 29–30; see also Woolgar 1988: 83–96).

This methodological approach was peculiar in that it still aimed at "understanding" scientists' work but disregarded the specific meaning this work had for the ones who performed it.[1] It was criticized by Lynch (1982: 506–509; 1993: 93–102), who described the consequences of the naïve approach for the outcomes of the study: not only is the observation reduced to what the naïve observer finds intelligible, the observers can furthermore record their observation only in their own language. Thus, from the beginning the naïve observers were forced to select events and actions that seemed intelligible to them and to record them in a sociological language and attached conceptual frameworks. Lynch also demonstrated that Latour and Woolgar were unable to maintain this position during their ethnography (Lynch 1982: 507).

Mainstream anthropology and sociology use a different approach, which is based on the premise that it is necessary to understand the meaning given to the observed actions by the observed actors. For science studies, this position requires understanding the scientific meaning the observed research has to the observed researcher. This methodological position informs the mainstream of science studies.[2] More recently, Collins and Evans characterized this approach as the sociologists' acquisition of "interactional expertise" during fieldwork, which is sufficient "to interact interestingly with participants and to carry out a sociological analysis" (Collins and Evans 2002: 254).

If, however, understanding the meaning scientists give to their practices extends to the role of scientific things, we encounter a problem identified by Collins and Yearley (1992): if an independent impact of nature on scientific practice has to be included in sociological explanations, how will a description of this impact be obtained without transferring authority over the explanation to the scientists under study?[3] Collins and Yearley advocated avoiding this loss of sociological authority by treating accounts of material influences on scientific practice as socially constructed and not related to a nature "out there". According to them, ignoring the impact of nature on scientific practice and treating all accounts as mere constructions is the only way to remain completely in the realm of the social.

Using scientists' accounts undoubtedly has the disadvantages pointed out by Latour and Woolgar in their argument for sociologists' independent accounts, as well as by Collins and Yearley in their argument for a radical-constructivist account. Unfortunately, there does not seem to be a viable alternative. Using sociological accounts of scientific things is not sufficient for understanding their role in the practice of scientists because the latter work with entirely different frames.

Radical constructivism excludes effects of scientific things from the analysis. The only remaining source of knowledge about scientific things is the scientific one produced and held by the very scientists whose actions are to be explained. We cannot juxtapose scientists' accounts to those of others (for example, our own), which deprives us of important means for deeper understanding. For example, the status of cassowaries in the taxonomy of the Karam people—where it is not a bird but similar to a man (Bulmer 1967)—is interesting to us only because cassowaries *are birds in our taxonomy.* However, no such difference can be established for many of the statements made by scientists. Only the study of controversies in science provides multiple accounts that can be comparatively studied (Collins 1981, 1982; Simon 1999). In the absence of such controversies, we are left with unchallenged accounts provided by the scientists whose actions we want to understand and explain. Still, using these accounts appears to be the lesser evil.

Ethnographies or Interviews?

Scientists' accounts of scientific things can be obtained either by observing scientists and asking them about the meaning of their actions or by interviewing them. Accounts in scientific publications can serve as an additional source but are usually incomprehensible to sociologists without further explanation because they are written for other scientists.

The predominant approach to collecting data about scientific practice is ethnographic observation. Ethnographies provide the opportunity for researchers to become immersed in the scientific culture they want to study, to collect rich data about this culture, and to ask questions about scientific practice that have not been asked yet. There can be no doubt about the unique suitability of ethnographic studies for many research purposes in science studies.

Another argument that is sometimes advanced in favor of ethnographic observations is that they are more "direct" and provide "more immediate" knowledge about scientific practice than interviews. This is, however, only true if researchers want to use their own accounts of scientific things. In all other cases, observing scientists' practices requires understanding the observed actors in order to make sense of observations. Thus, ethnographies do not offer an escape from the "double hermeneutics" characteristic to the social sciences (Giddens 1976).[4] They do, however, provide more opportunities to probe deeply because scientists can be asked to explain their observed behavior.

There are also limitations to ethnographies, making them unsuitable for comparative studies that include the impact of scientific things on scientific practice. The first limitation is the trade-off between breadth and depth. It exists only for a given amount of resources but must be taken into account because resources for research are always limited.

Conducting an ethnography for studying the impact of scientific things on scientists' practices is unproblematic as long as one or two sites are investigated by a researcher over the course of several years. However, if the impact of social macroconditions such as institutions or national cultures on scientific practice is to be included, and if the effects sociologists are interested in are field-specific, the scientific practice must be compared at many sites—that is, sites belonging to different fields and more than one site from each field. Without these comparisons, we would be unable to distinguish between local effects of social macroconditions and idiosyncrasies. The use of ethnographies for comparative studies of scientific practice multiplies the necessary research time.[5] For example, systematically varying organizational environments and disciplines in order to ascertain the impact of both easily leads to comparisons of thirty or more cases, which makes conducting ethnographies impractical. Comparative ethnographies would also require researchers to be granted simultaneous access to many sites, which—at least in our experience—is difficult to achieve.[6]

Another disadvantage of ethnographies concerns the synchronization of the observation with the observed research processes. Analyzing the impact of macrosocial conditions and scientific things on scientific practice often requires covering the whole research process from the emergence of the idea for a project to the reception of results by the scientific community. These processes can take far longer than an ethnography; in one of our examples, they took from two to more than eight years. The maximum duration was not anticipated by the scientists but resulted from unexpected behavior of scientific things. Under these circumstances, it is difficult if not impossible to conduct ethnographies of whole research processes. While ethnographies can analyze the unfolding of processes and can capture events and activities preceding the ethnography by ad hoc interviews with informants, they can fully capture only those processes that are finished before the observation ends. Complete coverage of a process is only possible in retrospect.

Thus, while ethnographies provide the greatest possible depth for studies of scientific practice, including the effects of scientific things, they cannot provide enough breadth in terms of numbers of cases and time horizons for comparative studies—at least not at the current funding levels for science studies. This is why most comparative studies use qualitative interviews, which offer a compromise between breadth and depth.

Qualitative interviews produce less information about scientific practice than ethnographies. They also make the sociologist dependent on just one type of source of information. There is a danger of "just accepting the interviewee's word for it," that is, using her personal theories of what happened and why instead of developing an independent sociological analysis. The dangers of this approach were emphasized by Gilbert and Mulkay in their analysis of scientists' accounts for scientific error and justification of their own beliefs (Gilbert and Mulkay 1982, 1983; Mulkay and Gilbert 1982a, 1982b). They identified an interpretive repertoire and a high degree of interpretative variability in their interviewees' accounts. From these observations, they drew the conclusion that it is impossible to go beyond interview data and to establish "what is actually going on." Therefore, the question "What is really going on in science?" must be replaced by the question "How do scientists construct their versions of what is going on?" (Mulkay and Gilbert 1982b: 312–314).

This rather pessimistic approach to investigating social reality has not been widely accepted by science studies. Nor has it gained much popularity in social research at large, where the same argument would apply. We think that Gilbert's and Mulkay's negative assessment of interviews overshoots the mark because the variability of responses they observed is partly due to the topics they addressed and partly to the way in which they asked their questions.

The variability of scientists' accounts is itself a variable. It depends both on the subject matter of an account (its specificity) and on constraints on scientists' narrations. The idea of "narration constraints" has been developed by Fritz Schütze in his use of narrative interviews (Schütze 1977; Riemann 2003). He argued that in their extempore storytelling, interviewees feel forced to condense their stories, to provide detail, and to close the structure of their narration (Riemann 2003: [26]). In our interviews with researchers from the natural sciences, social sciences, and humanities, we have observed an analogous phenomenon in interviewees' reasoning about their own behavior or that of members of their scientific communities. They feel forced to follow the "rules of the game" of scientific arguments in their community. Unless they are engaged in a scientific controversy, scientists account for scientific things by using knowledge currently believed to be true within their community and by applying the mode of reasoning used by this community. Responses by interviewees from the same field show characteristic similarities, while they vary systematically among interviewees from different fields.

It is important to note here that this is an argument about structure rather than content. Interviewees do differ in their beliefs and often argue against what they report to be the mainstream of their community. Our point is that none of these arguments can be made up at will. When talking science, interviewees follow the conventions of their respective communities by considering some phenomena as evidence and others not, some statements as facts and others not, and some conclusions as following from premises logically and others not. In our experience, these deeply internalized constraints severely limit the freedom of strategically answering questions even in conversations with outsiders.

Notes on Interview Methodology

The strategies available to researchers investigating science are obviously not different from the ones of social research in general, even though scientists' monopoly on knowledge about scientific things constitutes a specific challenge. We will focus here on obtaining accounts by qualitative interviews. Our discussion includes the preparation by the interviewer, the operationalization of the research question, and the triangulation of sources and methods.

As we demonstrated in the previous section, the dominant position in the sociology of science is that interviewers need to scientifically understand their interviewees' research to some extent in order to be able to analyze it sociologically (see also Laudel and Gläser 2007). When it comes to acquiring this "interactional expertise" (Collins and Evans 2002), interview-based research is at a serious disadvantage compared to observation because the time for becoming acquainted with the field under study and for acquiring interactional expertise by learning from practitioners in the field is very limited. Nevertheless, bringing interaction to the interview is very important "in order to interact interestingly" with interviewees (Collins and Evans 2002) because the level of scientific understanding at which the interview is conducted and thus the specificity of information about scientific things depend on it. This level is implicitly negotiated at the beginning of the interview and partly depends on the degree of scientific understanding signaled by the interviewer (Laudel and Gläser 2007). The more interactional expertise the interviewer has, the more specific information about the properties of things he/she will obtain.

Gilbert's and Mulkay's observation of the variability of accounts and the latter's dependency on the interview context reinforces a general methodological point. The informational yield of interviews crucially depends on the ways in which questions to interviewees are phrased and ordered. Operationalizing a research question means to identify the information to be collected in order to answer that question and to design an interview strategy for collecting this information in a way that minimizes interviewees' opportunities to provide their personal theories (which often include retrospective rationalizations of decisions) rather than specific descriptions of situations and behavior. Thus, instead of asking interviewees how their conditions of work affect their practices one would attempt to ask them about changing practices and conditions in separate parts of the interview.[7]

Of course, there are limits to a strategic separation of topics in a qualitative interview. We still need to generate narrations, which means that an interview cannot be conducted as a long list of detailed questions. In their narrations, our interviewees will routinely destroy our carefully designed order of topics and recombine them at will. Nevertheless, asking specific questions and arranging them in a specific way makes it difficult for interviewees to just present their personal theories about "why things are the way they are."

Standard research methodology recommends using as many different methods and sources as possible, that is, triangulating them. While bibliometric data or direct observations cannot supplant scientists' accounts (Mulkay and Gilbert 1982b: 314), these data can provide additional insights to support the interpretation of accounts. The triangulation of sources also includes scientists. Interviewing as many of them as possible about scientific topics multiplies the material that can be interpreted as well as the number of accounts that can be compared.

Data Analysis and Integration

Data about scientific things must be integrated into explanations of scientists' behavior. This creates three problems that require compatible solutions. First, effects of different scientific things on the same action must be compared to each other and integrated with regard to their synthetic effect on that action. Second, effects of scientific things must be compared to and integrated with effects of the social conditions of action such as institutions, power relations, culture, and others. Third, effects of scientific things must be compared (and their effects integrated) across cases. Accomplishing these three types of comparisons and integrations requires a unitary explanatory framework that renders data about scientific things comparable. We briefly recount the two most ambitious frameworks for capturing the role of material agency—Actor-Network Theory (ANT) and the Mangle of Practice—before we turn to our own proposal to simply use a theory of action.

Frameworks Dedicated to Accounts of Material Agency

It is of course impossible to give a complete account of ANT in this paper. Much and diverse literature on ANT has been produced and the framework has been developed in many different dimensions. Our account of ANT is focused on (and thereby reduced to) the way material agency is accounted for.

The most important contribution of ANT to science studies is its explicit call for the inclusion of scientific things in the explanation of scientific practice. Callon and Latour propose to solve this problem by rejecting the ex-ante distinction between social and natural phenomena. This specific symmetry principle homogenizes the field of observation and turns it as a whole, now including human as well as non-human "actants", into a subject matter for sociologists. What constitutes the "natural" and the "social" depends on processes in actor networks that are heterogeneous in that they are composed of both humans and non-humans.

This approach enables the use of the resources of the social sciences in order to study the natural world (Latour 1994: 791). Characteristic examples of this approach are Callon's description of attributes of scallops (Callon 1986), Latour's reference to the "Microbe as new social actor" in the index of his book (Latour 1988: 272), and the following account of fuel attributes:

> At the start, Diesel [the inventor] ties the fate of his engine to that of any fuel, thinking that they would all ignite at a very high pressure.… But then, nothing happened. Not every fuel ignited. This ally which he had expected to be unproblematic and faithful betrayed him. Only kerosene ignited, and then only erratically.… So what is happening? Diesel has to shift his system of alliances. (Latour 1987: 123)

"Ally", "faithful", "betrayed" are clearly sociological terms ascribing consciousness and intentional action to fuel. This is one of the reasons why the solution provided by ANT is so seductive: With its symmetry principle, all causal influences are described in the same language, and there are no apparent difficulties with the inclusion of scientific things.

Pickering's (1995) approach, the Mangle of Practice, is centered on the concepts of resistance and accommodation. He introduces "material agency" as a source of emergent resistances to the researcher's goal attainment. In order to achieve their goals, researchers are forced to adapt their practices to the resistances emerging in these practices until they reach their goals (which are subject to redefinition in the course of accommodation). The resistance is locally and temporally emergent (Ibid.: 21–22). Consequently, Pickering insists that his concept of resistance is significantly different from the notion of constraints because the latter are restrictions to human actions that transcend time and space (Ibid.: 65).

The suitability of both frameworks for the purposes outlined above crucially depends on their support of comparisons. Do these accounts make the influence of scientific things on actions comparable to one another as well as to social influences?

The answer to this question is somewhat disturbing. While both approaches do include effects of material agency, neither is able to overcome the idiosyncrasies of descriptions of scientific practice. Pickering's approach leaves us with the choice between the real-time description of emergent resistances and researchers' accommodation and the highly abstract but very poor general language of describing materiality, a language mainly consisting of the words resistance and accommodation.

The focus on emergent resistances reduces the account of nature to the unanticipated and not yet discovered impact. It is consistent with this approach that Pickering rejects the possibility of finding general patterns that provide explanations (Pickering 1995: 24). He distinguishes between the level of the concrete, single research process (which cannot be explained but "just happens", see also Ibid.: 206–207), and the general pattern of resistance and accommodation. This pattern only provides a highly abstract model appearing the same for all material influences on scientific practice. Thus, the Mangle of Practice is not designed for comparing material agency (see also Gingras 1997: 330–331).

Similarly, it is impossible to integrate the natural influences (resistances) into a sociological account of scientists' actions within the framework of Mangle. It is not possible for this framework to capture patterns of intertwining material and social influences on scientific practice (an argument advanced by Knorr-Cetina and Merz 1997: 129).

ANT differs from Mangle significantly in that it enables the application of a rich sociological language to nature. This language is based on

> shifts in vocabulary like "actant" instead of "actor", "actor network" instead of "social relations", "translation" instead of "interaction", "negotiation" instead of "discovery", "immutable mobiles" and "inscriptions" instead of "proof" and "data", "delegation" instead of "social roles". (Callon and Latour 1992: 347)

This enrichment does not change the language's sociological nature. As demonstrated above, Callon and Latour cannot avoid the language of intentional actions and social relations.

The sociological description of nature's influences predefines all observable phenomena as something sufficiently explainable by sociological observers in a sociological language. However, there is a price to pay for the achieved sovereignty over nature. This price is, again, idiosyncrasy. Neither can different actor networks be compared to each other beyond a comparison of their successes, nor can the relative strength of human and non-human actors be weighted and their impact be synthesized. Both deficiencies become most visible in a comparison Latour himself provided, namely the analysis of Pasteur's success over Pouchet (Latour 1987: 84; 1989).

Pouchet replicated some of Pasteur's experiments in order to show that, contrary to the latter's account, there is something like "spontaneous generation". He observed microbes growing in media that had been sterilized in accordance with Pasteur's instructions, that is, microbes behaving against Pasteur's predictions. Latour concluded that non-human "allies" (again a sociological term) have to be included in the list of both Pouchet's and Pasteur's allies (Table 1).

The lists of human and non-human allies make obvious why Pasteur won, but do so only because all of his allies are stronger than their counterparts in Pouchet's network. It seems impossible to compare the non-human and human allies' contribution to the victory, for example, the contribution of an absence of ferments after more heat to the contribution by the Academy. Instead, we are left with a choice between idiosyncrasy and extreme abstraction. On the level of the process under investigation, the contributions made by Pasteur's non-human allies are described in great detail, and a reconstruction of

Table 1 ■ Latour's list of heterogeneous allies (Latour 1989: 109)

Pouchet's allies	*Pasteur's allies*	
no supporter	supporters	
accused of atheism	Academy	human
provincial	in Paris	
abstracts only	full articles	
protocols	good protocols	
- - - - - - - - - - - - - - -	- - - - - - - - - - - - - - -	*No dichotomy*
ill equipped	well equipped	
ferments after	no ferments after	non-human
sterilization	more heat	
etc.	etc.	

Symmetric treatment: all the allies are listed, no matter how long and heterogeneous the list

the process is given in the language of ANT. But the description is given in a way that makes it impossible to compare content and strength of these contributions. We can neither compare them to the contributions of human allies in the same network, nor can we compare contributions of different non-humans between networks. On a more general level, we are left with the information that one actor network succeeded and another one failed, but without a method to compare successful or failing actor networks.

Both ANT and Mangle provide the methodological imperative that scientific things have to be included in analyses of scientific practice. Moreover, both approaches supply us with means for describing these influences in a language available to sociological observers. However, neither of them supports a comparative approach to the analyses of research processes that include different natural influences and to the integration of natural and social causes into comparative studies.

Things in a Theory of Action

The frameworks developed in Science and Technology Studies (STS) contexts do not support detailed comparisons of scientific things or the integration of properties of things with traditional social factors shaping scientists' practices. This is why we start with a rather conventional theory of action and treat scientific things as sources of *epis-*

temic conditions of action, that is, as objects about which knowledge is produced, as instruments used to produce knowledge (technical things), and as material conditions relevant to the application of instruments to objects.[8] We assume scientific things to have material (that is, physical, chemical, or biological) properties that cannot be analyzed sociologically, let alone be integrated into sociological theories of any kind. The use of these physical properties by scientists depends on how they are perceived in the frames of their current knowledge. Physical properties may also directly affect scientists' actions and become the subject of retrospective sense-making (for example, if scallops do not anchor or if something explodes in a chemistry lab). Both the use of scientific things and their not-yet-accounted-for impacts create epistemic conditions of research processes that affect scientific practice. Since we had to account for the influence of scientific things in our empirical investigations, we began to empirically derive such epistemic conditions from our data. Our current preliminary list of epistemic conditions includes the following variables (see also Gläser et al. 2010; Laudel and Gläser 2014):

- *Duration:* necessary minimum time for a research process that is either caused by the physical property *Eigentime* of research objects and methods or by the dynamics of the research process itself, for example, if sequences of steps must be taken in order to build up an experimental setting and to achieve a result;
- *Resource intensity:* quantity and quality of resources that are required to achieve a certain goal;
- *Decomposability of problems:* the extent to which a research problem can be disaggregated in a sequence of steps or in parallel steps;
- *Epistemic room of maneuver:* research actions that are possible with the objects and methods available;
- *Strategic uncertainty:* the likelihood that the anticipated outcome does not exist at all (for example, if a new effect is sought that might not exist); and
- *Technical uncertainty:* uncertainties in the experimental design that require trial-and-error procedures or repeated attempts (for example, producing mutants and screening them for the required properties).[9]

These variables are mostly related to individual research processes. However, at least some of them (especially resource intensity and technical uncertainty) can be used to describe fields in the sense of an average, that is, as a property of a typical research process in a field.

The material properties of things and the epistemic conditions of research affect researchers' actions. Material properties of scientific things that are not anticipated by researchers as well as material properties that are taken into account may affect

- the duration of actions;
- the (anticipated and actual) results, particularly the likelihood of success; and
- the resources required by the action,

which we consider linkage variables between properties of scientific things and scientific practice. These basic properties of an action are co-shaped by properties of the actor (frames, interests, capabilities, perceptions) and by the social conditions under which an action takes place. Thus, epistemic conditions of actions (and among them properties of scientific things) have the same "levers" for influencing action as do social conditions of actions. Consequently, the different kinds of influences can be treated symmetrically at and integrated on the level of conditions of actions.

Information about epistemic conditions of action and "values" of linkage variables can be derived from asking researchers about their projects—problems they want to solve, methods they apply, and objects they use. As the following examples demonstrate, these values can be compared to some extent and related to social conditions of action.

Applications

Both our examples are taken from an internationally comparative investigation of conditions for scientific innovations. This investigation identified epistemic properties of selected innovations, derived conditions under which the innovations could be developed, and asked how these conditions were provided in four national science systems. The epistemic properties of innovations included properties of scientific things that modified opportunities for researchers to achieve their goals under the specific social conditions they faced.

Data collection was centered on qualitative interviews, with the same generic interview guideline applied in all four countries. The interview methodology followed the strategy outlined above.

We chose two examples from this study to illustrate our methodological approach (see contributions to Whitley and Gläser 2014 for the main results of the project). The first example shows the translation of thing properties into linkage variables, which enables an explanation of the differential success of Dutch researchers in creating Bose-Einstein Condensates (BECs) of cold atom gases. The second example compares properties of different scientific things in evolutionary developmental biology in an explanation of the varying degrees to which researchers engaged with the innovation.

Physical and Social Conditions under Which Bose-Einstein Condensation of Cold Atom Gases can be Achieved

A Bose-Einstein condensate (BEC) is a specific state of matter that occurs when gases of atoms or subatomic particles are cooled down to near-absolute zero (< 100 Nanokelvin), that is, to a state of very low energy. At this temperature, a large portion of the atoms collapse into the lowest quantum state and quantum effects occur on a macroscopic scale. The occurrence of the phenomenon was theoretically predicted by Bose and Einstein in 1924. The first BECs of atom gases were produced in 1995 by two US-American groups who innovatively combined several recently developed cooling technologies. Manufacturing BECs remained a complex, risky, and demanding task for almost a decade due to the simultaneous impact of scientific things and social conditions of research (Table 2).

The scientific things physicists dealt with were the cold atom gases and an experimental setup for cooling them and measuring their properties. Their properties at the required temperatures were ill-understood at that time. A gas changes its state (becoming a liquid or a solid) at certain constellations of pressure and temperature that are specific to that gas. Producing BECs would have been impossible if atom gases turned into liquids or solids at the densities and temperatures necessary for BECs to occur. Even after the first successes, the behavior of gases of other atoms at the required pressures and temperatures could not be predicted. Thus, the strategic uncertainty about BECs of a specific atom gas remained high for a long time.

Since it had always been clear that the experimental realization of BEC in atom gases depended on achieving extremely low temperatures, experiments aimed at producing BECs started seriously only after a new cooling technique—laser cooling—had been developed. This method had to be combined with another one—magnetic trapping of

Table 2 ■ Physical properties of scientific things and conditions of action for BEC research.

Conditions of action	*Physical properties*	*Epistemic properties*	*Impact on research practices*
atom gases	liquefaction potential Density	high strategic uncertainty high technical uncertainty long duration low decom-posability high resource intensity	unpredictably prolonging research processes delayed beginning or forced abortion due to insufficient access to resources (see below).
experimental setting	integration of three different technologies very low temperatures ultrahigh vacuum		
career position	—	—	delayed beginning due to lack of access to infrastruc-ture and limited time horizon prolonged research processes due to limited resources
decision practices of scientific community	—	—	delayed beginning or forced abortion due to lack of project funding prolonged research processes due to limited resources

cold atoms and with imaging techniques in order to take "shadow pictures" of atoms. All this had to be achieved at ultra-high vacuum conditions (Ketterle 2004).

The physical properties of scientific things and the scientific knowledge about them at that time led to the following epistemic properties of research processes aimed at manufacturing BECs in the 1990s and early 2000s:

- The *strategic uncertainty* was high for each new kind of atom because it was not clear whether the anticipated effect could be produced in the particular atom gas.
- The *technical uncertainty* was also high because several methods had to be combined under new conditions (high vacuum, very low temperatures).
- The *duration* of the research process was long (setting up the experiment took at least two years) and often unpredictable due to the high technical uncertainty.
- The *decomposability of the problem* was low because all methods had to be integrated into one experimental setting and had to be applied simultaneously.
- The *resource intensity* was high because a specific experiment had to be custom-built by combining expensive equipment.

These epistemic properties of BEC research shaped the characteristics of research practices. The duration of experiments for manufacturing BECs was long and unpredictable. The risk of failure within a given time frame was high. The resource requirements were significant and included infrastructure (a laboratory with basic equipment for low-temperature physics), project-specific equipment (for example, lasers) and access to machine workshops for custom-built equipment. At least one researcher needed to work on the project full-time. The research process could have been accelerated if more resources had been available. Due to the low decomposability of the research problem, this acceleration could only be achieved by setting up parallel experiments, which multiplied the resource requirements.

Under these conditions, opportunities to develop BEC research depended on simultaneous control of the research infrastructure at universities, access to project grants, and long-term horizons for research. As we have demonstrated in more detail elsewhere (Laudel et al. 2014b; Gläser et al. 2015), only relatively few researchers have had these opportunities because control of infrastructure at universities is limited to professors, project funding depends on the opinion and decision practices of scientific communities, and the long-term horizons make a permanent position almost a necessity. More specifically, two young Dutch researchers had to delay their move to BEC research because the permanent positions they held were below the professorial level. Their professors did not approve of BEC research and thus did not permit the use of the laboratory for this purpose. Project funding for BEC research was approved relatively late by the Dutch funding agency because the Dutch physics community considered manufacturing BECs impossible before the successes in the US and not interesting anymore immediately afterwards. Project funding was cut again after a brief peak because very little success was visible after three years. In contrast, only a few German researchers below the professorial level (that is, on fixed-term positions) attempted the move to BEC research at all. Professors had better access to project grants because they were approved (albeit with delays) against the majority opinion of the German physics community. BEC research grew rapidly in Germany because recently appointed professors could use start-up packages to create the necessary infrastructure.

The comparative analysis was able to identify the existence of prior research traditions in a national scientific community, the control of infrastructure, national career systems, and decision practices of national scientific communities as the most important factors shaping opportunities to develop experimental BEC research. The specific roles of these factors clearly depended on the properties of the scientific things BEC researchers had to use and on the way in which they shaped epistemic and general properties of research practices.

Comparing Properties of Different Things: Mice or Snakes?

Evolutionary Developmental Biology (evo-devo) is a highly heterogeneous life-science field that evolved around a set of concepts and questions exploring links between the evolution of a species (the subject matter of evolutionary genetics) and the embryonic development of its individuals (the subject matter of developmental biology). Evo-devo can be traced back to the end of the 1970s when it became increasingly obvious that neo-Darwinian theory was unable to account for all empirical findings of evolutionary biology (Müller 2007), particularly the rapid changes in the forms of organisms evident from the fossil record and the origins of traits that did not constitute an adaptation to the environment.

> It became increasingly obvious that these explanatory deficits of neo-Darwinism were due to its treatment of development as a "black box" and the consequent absence of the generative rules that relate between genotype and phenotype. (Müller 2007: 500–501)

The discovery of genes regulating embryonic development (HOX genes) in the 1980s and advances in molecular and genomic techniques made it possible to address specific questions by comparing the development of different organisms, which led to an increased understanding of developmental mechanisms on the molecular level. More recently, this research received a further impulse by a breakthrough in the development of sequencing technologies, which made the sequencing of whole genomes affordable for individual research groups.

Evo-devo provides a new perspective on existing data and a distinct set of research questions. The new questions require comparative experimental research on genetically different organisms, preferably from two different species. The comparative approach and its new criteria for selecting organisms for experiments introduces new things to biological research. Since evo-devo overlays traditional approaches rather than supplanting them, it offers a wide range of opportunities to change research practices based on possibilities to use established or new scientific things.

No Things, Familiar Things, or New Things?

Evo-devo research could be theoretical and focused on conceptual development. Another theoretical form was mathematical modeling in bioinformatics, which was conducted either with biological data or by building more abstract models. In both cases, the research did not use any scientific things beyond generic research technologies such as computers.

The easiest way to engage in evo-devo research was to use familiar things (continuing the traditional evolutionary or developmental biology research) and additionally contextualizing findings in a theoretical evo-devo framework. This "dual use" of familiar scientific things (organisms and methods that were well-established in the researchers' laboratories) did not require any changes in experimental strategies or designs. It only required the acquisition of the evo-devo theoretical framework and concepts as well as looking for possible comparisons of findings to those of others in a more systematic manner. The biological properties of organisms were irrelevant to evo-devo research because it was added after things were used for other purposes.

Alternatively, evo-devo research could be conducted as new, dedicated experiments using different organisms or methods. The empirical strategy chosen mostly affected the resource intensity of research. Using familiar things or not using things required relatively few resources, while the resource demand of experimental evo-devo research with new things could be considerable (see below). The time horizon of the research with familiar and without things was small and predictable.

Established Things or Surprising Things?

The choice of organisms to compare had a strong impact on epistemic properties of research processes. Experimental biologists generally prefer working with a few, well-understood organisms for which the necessary methods are known to work. However, these so-called model organisms are not necessarily best suited for answering evo-devo questions, not least because the evolutionary branch on which they are located constrains the choice of organisms they can be compared to. Therefore, it is often attractive for evo-devo researchers to work with entirely different organisms. Including new organisms in experimental research requires exploring their relevant biological properties, which are often unknown to researchers. This makes breeding them and conducting experiments with them more difficult and often riskier than working with model organisms, that is, it increases the technical uncertainty. Some of the dedicated evo-devo organisms had longer *Eigentimes,* that is, breeding cycles than the model organisms (some of which were also chosen for their easy breeding).

Working with new organisms had a strong impact on both the resource intensity and the time horizon of research processes. In particular, it often got longer and unpredictable due to the *Eigentimes* and the necessity to establish methods for the new organisms. At the same time, the epistemic rewards of this strategy were likely to be higher due to the organisms' suitability for evo-devo questions. Introducing new organisms specifically for evo-devo in a laboratory is a "high risk, high reward" strategy.

One's Own Things or Other People's Things?

Another decision that considerably affected epistemic properties of research practices concerned the ways in which comparisons between species were conducted. The most difficult and thus most resource-intensive and time-consuming way to conduct comparative evo-devo experiments is working with two or more different organisms in one laboratory. In most cases, this approach to comparative research has a higher technical uncertainty and resource intensity because researchers have to establish (to introduce, breed, and understand) a second organism in their laboratories. The advantage of comparative experiments is that they can be designed within the same conceptual and methodological framework, which guarantees the best fit of data.

Costs of comparative experimental research can also be reduced when information about the second organism is acquired from external sources rather than being internally produced by experimenting with several organisms in one lab. This can be done through collaboration with evo-devo researchers specializing in other organisms. An even easier way is to use published data or bioinformatics databases for comparisons. However, both collaboration and reliance on literature or databases reduce a researcher's control of the experimental approach and the data that can be used. The data that are accessible this way may not fit the specific evo-devo question, thereby limiting the potential epistemic rewards of these cost-efficient strategies.

Comparing the Impact of Scientific Things

Evo-devo researchers could make three strategic decisions concerning their scientific things, each of which significantly affected the duration, resource requirements, and outcomes of their research. The combinations of these and other factors—such as the disciplinary background of researchers—created a wide spectrum of possible evo-devo research practices (Table 3, see Laudel et al. 2014a for an extensive account). Depending on the intended use of things (organisms and things used in methods for handling them), evo-devo research could be conducted under a variety of social conditions, required few resources and little time—unless it used scientific things or was conducted as an additional interpretation of mainstream experiments. If dedicated evo-devo experiments were conducted, time horizons and resource requirements varied between those common to experimental research in evolutionary genetics or developmental biology and the long and unpredictable time horizons and extreme resource requirements that even exceeded those of BEC research because they included completely new buildings for breeding facilities.

Thus, evo-devo research could be taken up under most organizational and career conditions. It was often started in the postdoc phase in one of its less demanding forms and was "upgraded" once researchers became professors and had control of a laboratory. We found the most resource-intensive forms of evo-devo research at Swiss universities and German state-funded, non-university research institutes, which were both able to significantly invest in facilities.

Table 3 ■ Comparison of the impacts of different scientific things on evo-devo and BEC research.

Scientific things	*Biological/physical properties*	*Epistemic properties*	*Impact on research*	*Career position in which research could be conducted*
None (conceptual development, mathematical modeling)	—	Low resource intensity Short duration	None	All
Familiar things (for example, mice)/traditional use	Irrelevant to evo-devo research	Low resource intensity Short duration	None	All
Familiar things (for example, mice)/new use (new genetic methods)	Breeding behavior, responses to methods (well-known)	Medium resource intensity Predictable duration	Typical for experimental research in the biosciences	Professor with control of biology lab
New things (for example, hedgehogs)/new use (genetic methods adapted to hedgehogs)	Breeding behavior, responses to methods (unknown)	High technical uncertainty Unpredictable duration High resource intensity	Unpredictably prolonging research processes Beginning depends on significant investments by research organization	Professor with significant infrastructure support
Simultaneous use of different things (for example hedgehogs and snakes)	For each organism: Breeding behavior, responses to methods (unknown)	High technical uncertainty Long and unpredictable duration High resource intensity	Unpredictably prolonging research processes Beginning depends on significant investments by research organization	Professor with significant infrastructure support
BEC (cold atom gases and experimental setting)	Liquefaction potential Density Integration of three different technologies Very low temperatures Ultrahigh vacuum	High strategic uncertainty High technical uncertainty Long duration Low decomposability High resource intensity	Unpredictably prolonging research processes Delayed beginning or forced abortion due to insufficient access to resources	Professor with control of low-temperature physics lab

The translation of physical/biological properties into epistemic ones and of epistemic properties into general conditions of action rendered different scientific things and their actions comparable and enabled the identification of the group of scientists in both fields to develop their different innovations. While evo-devo researchers could freely choose the degree to which they wanted to become involved with the innovation, BEC was an all-or-nothing innovation for a long time. The high-cost forms of evo-devo turned out to be very similar to BEC in that the opportunities to develop these specific lines of research exist only for those who have control of a laboratory, permanent positions, and stable access to grants.

Concluding Remarks

With this paper, we attempted to define a set of methodological problems and choices involved in attempts to include the influence of scientific things on the production of scientific knowledge. These methodological problems are specific to comparative case studies aiming at the development of middle-range theories. Studies aiming at thick descriptions of single cases do not need to compare effects of scientific things and can thus avoid the problems of comparisons.

We discussed three crucial methodological choices and explained the ones we made in our comparative studies. We decided to use scientists' accounts of things (rather than constructing sociological accounts) because material properties of things are difficult to capture in sociological language and because we need to understand the meaning scientists' practice has for themselves and their peers. We preferred qualitative interviews to ethnographies because in comparative studies, we need to strike a balance between depth and breadth. Finally, we applied an action-theoretical framework rather than establishing post-humanist approaches because the reference to actions renders influences of things comparable to each other as well as to social influences.

Our two examples demonstrate the comparative strategy. Translating the influence of material (that is, physical, chemical, and biological) properties of things into epistemic conditions of action renders these influences comparable to each other, which enables an integration of all material influences on one action and the comparison of material influences across cases. Physical properties of cold atom gases and the equipment for producing and identifying BECs could be integrated by assessing the strategic and technical uncertainty, duration, decomposability, and resource intensity of research processes. Properties of the organisms and methods of evo-devo research were rendered comparable, and the specific epistemic conditions of action for different research processes could be compared to each other as well as to BEC research.

Translating epistemic conditions into general properties of action—such as resource requirements, duration, and likelihood of success—enables comparisons of material influences with social conditions of action as well as an integration of both types of factors. In our cases, the impact of epistemic conditions on general conditions of action could be linked to decision practices of scientific communities, career positions, and the resulting access to resources and time horizons for research.

As a conclusion to our essay, we would like to highlight the preliminary character of our solutions by addressing three unresolved issues.

First, collecting comparable information about scientific things requires an elaborate interview strategy, which puts a rather heavy strain on interview schedules. This seems inevitable because necessities of comparative research make interviews the only feasible method.

Second, the link between methodology and theory remains problematic. We derived epistemic properties of research processes from empirical data of several studies but do not have a theoretical framework yet that systematizes and links them to each other. Such a framework can only be built on the basis of dedicated research projects. Until it exists, our growing list of epistemic properties of research actions provides a pragmatic solution.

Finally, our approach to scientific things affecting actions is still rather coarse in that it focuses on the most noticeable influences. A more fine-grained approach would need to include changes in the course of an action caused by specific properties of things. Again, a specific research program for the study of scientific things would be required in order to develop a more detailed framework. Until this is possible, we invite readers to our iterative approach of accumulating knowledge about the role of scientific things in knowledge production.

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Notes

1. In the context of ethnographic methodology, the methodological reasoning provided by Latour and Woolgar had been criticized as an "outsider myth" according to which "only outsiders can conduct valid research on a given group; only outsiders, it is held, possess the needed objectivity and emotional distance" (Styles 1979: 148; for a discussion of different approaches to observation, see Hammersley and Atkinson 1995: 80–123).

2. See, for example, Collins (1984), Knorr-Cetina (1981: 31n64; 1993: 170), Knorr-Cetina and Merz (1997), Lynch (1982; 1985), Merz and Knorr-Cetina (1997: 74), and Traweek (1988: 9–11).

3. Observations and accounts of things by scientists-turned-students-of-science, which are frequent in science studies, do not provide a solution to this dilemma because these observers would still apply either a scientific or a sociological perspective.

4. The same applies to the use of scientific texts about things. Scientific knowledge appears to provide a unique source of accounts of scientific things. However, using this source of knowledge also requires the specialist knowledge of scientists.

5. This is why so far the most sophisticated comparison of scientific practice builds on decades of ethnographies and contributions by more than one researcher (Knorr-Cetina 1999: 17–20).

6. The comparative ethnographies discussed here are different from most multi-sited ones because the latter still investigate a single case (see, for example, Hine 2007). This seems to be achieved by distributing observation time between sites or by conducting ethnographies in teams. For example, a multi-sited ethnography involving three similar cases was apparently conducted by a team of at least six researchers (Heath et al. 1999).

7. Gilbert and Mulkay criticize the practice of passing on the research question to the interviewee when analyzing results by taking statements of scientists as literal descriptions of real events. However, they do not consider the possibilities of a different strategy of questioning or triangulation (Gilbert and Mulkay 1983; Mulkay and Gilbert 1982b: 314).

8. It is important to note that our interest is in theories of a middle range (Merton 1968) that explain scientists' responses to institutional conditions and governance. We use a general theory of action as a background to inform middle-range theories, but refrain from discussing the interesting question of how things can be positioned in a general theory of action.

9. Our use of the concepts *strategic uncertainty* and *technical uncertainty* differs from Whitley's (2000), who introduced the terms in his comparative analysis of scientific fields. He applied the term "technical uncertainty" to all epistemic uncertainties of a field's research and used "strategic uncertainty" to describe the uncertainty of gaining reputation. In our description of research projects, it is useful to differentiate between the uncertainty concerning the possibility of a specific outcome (that is, the existence of an effect) and the uncertainty concerning the way in which an outcome can be achieved.

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