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**Where do the Actants/ Mangles Come From?**

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## **Zusammenfassung**

Gegenstand dieses Aufsatzes ist das methodologische Problem der Einbeziehung materieller Einflüsse in die vergleichende Untersuchung von Forschungsprozessen. Daß solche Einflüsse berücksichtigt werden müssen, kann angesichts der neueren Befunde des Konstruktivismus (Actor-Network-Theory und Mangle of Practice) kaum in Zweifel gezogen werden. Weitgehend offengeblieben ist aber bisher, wie eine systematische vergleichende Einbeziehung von Materialität erfolgen kann. In dem Paper werden anhand empirischer Beispiele aus vergleichenden institutionalistischen Studien die Schwierigkeiten diskutiert, die in einer vergleichenden Analyse entstehen. Dabei zeigt sich, dass weder Actor-Network-Theory noch ‚Mangle‘ vergleichende Analysen materieller Einflüsse unterstützen. Als Ursachen werden die Behandlung materieller Einflüsse als rein situative Faktoren, das Fehlen einer Methodologie und der Abstraktionsgrad der theoretischen Modelle diskutiert. Als Ansatz für einen methodologischen Rahmen, der die systematische vergleichende Einbeziehung materieller Einflussfaktoren ermöglicht, wird deren Konzeptualisierung als kognitive Handlungsbedingungen vorgeschlagen. Dadurch kann die Verbindung zur Handlungstheorie und zu auf Handlungstheorien beruhenden institutionalistischen Ansätzen hergestellt werden.

## **Abstract**

This paper deals with the methodological problem of how to include material influences in comparative studies of research processes. Both Actor-Network-Theory and ‚Mangle-of-Practice‘ confirm convincingly enough that materiality must be taken into account in empirical investigations of research processes. However, it is not clear how material factors can be included systematically in comparative studies. In this paper, we use empirical examples from comparative institutionalist studies of research processes to discuss the difficulties that hinder comparative analyses. It turns out that neither Actor-Network-Theory nor ‚Mangle‘ support comparative analyses of material influences. As causes for this deficiency, we discuss the treatment of materiality as situationally emergent phenomenon, the absence of methodology and the theoretical models‘ level of abstraction. A methodological framework is proposed that should support systematic comparative analyses. This framework rests on an understanding of materiality as a cognitive condition of action. With the concept of cognitive conditions of action, materiality can be introduced into theories of action and into institutionalist approaches.



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## **1. The problem of comparison**

Our perspective on the materiality - sociality problem is methodological and pragmatic rather than philosophical. We have conducted comparative institutionalist studies of research processes in several fields and in these studies experienced the necessity to account for material influences. Thus, we had no philosophical quarrels with the two current approaches that offer a strategy for including these influences, i.e. Actor-Network-Theory (ANT, e.g. Callon 1986, Latour 1988) and Pickering's Mangle (Pickering 1995). On the most general level, both approaches confirm that materiality must be included in explanations of research processes as an independent factor that can not be reduced to scientists accounts. However, we faced serious problems when we tried to include material influences systematically in our studies. To do so, we firstly must look for them, i.e. find them in written descriptions and ask scientists about them. Secondly, it was necessary to compare material influences in different research processes. This is the problem we will address in our paper: If we accept material influences on social processes as relevant for the latter's explanation, then we must treat them systematically in a way that enables to compare them. Unfortunately, both ANT and Mangle do not offer much support for this crucial tasks.

We will discuss the problems related to comparative treatments of materiality in the context of the sociology of science. However, we think that in other social sciences face similar difficulties. Our attempts to compare material influences revealed three problem we will discuss in this paper. Firstly, we felt the necessity to ask whether and how material influences in different research processes are related to each other, in other words: we asked the question where the different actants or mangles did come from. Secondly, we had to find a way to compare material influences in quite different research processes. Thirdly, we had to include material agency as one explanatory variable among others, in other words: we had to introduce material agency into existing theoretical frameworks. Neither ANT nor Mangel contribute much to this conceptual work. We will hint to causes for this reluctance and explain why we regard the reference to standard theories of action the better solution for including materiality.

## **2. Where do the actants/ mangles come from?**

We begin with an empirical example. In an empirical study on institutional conditions of research collaboration (Laudel 1999a,b), we observed that some collaborations between biochemists and physicists failed and others were successful. In biochemistry, one important task connected with the identification of new proteins is to clarify their spatial structure. Today this is



mainly done by physicists with X-Ray crystallography. To apply this method requires to have the protein as a crystal. Thus, collaborations occur between biochemists who synthesise the protein, clean it and crystallise it, and physicists who apply X-Ray crystallography to the crystallised protein and produce pictures. These pictures are interpreted by both partners together.

But, of course, its not that easy. The crucial problem for biochemists is that proteins crystallise only under very specific physical and chemical conditions that cannot be concluded from a general theory. That is why biochemists start a long-lasting procedure of trial and error in order to find out the conditions for crystallisation. They know these conditions when the protein is crystallised. Naturally, these conditions are found only seldom, and many collaborations fail.

I cannot say why this (collaboration) does not progress. Presumably, it is very difficult to synthesise especially this protein in a larger scale. We always need fairly much. Firstly, it must be extremely pure, secondly it must be stable, and thirdly it must be a certain amount. These three prerequisites can not always be fulfilled. It is possible that it simply does not work.

Kann ich nicht beantworten, weshalb das nicht weiterläuft. Das ist wahrscheinlich auch sehr schwierig, gerade dieses Protein in größerem Maßstab herzustellen. Wir brauchen immer ziemlich viel. Erstmal muß es hochsauber sein, zweitens muß es stabil sein, drittens müssen es bestimmte Mengen sein. Das sind drei Bedingungen, die nicht immer erfüllt werden können. Es kann sein, daß es einfach nicht geht.

Since a protein's structure is a very important contribution to biochemical knowledge, this task is regarded very important by the specialty of protein biochemists. It was even awarded with a nobel prize. To crystallise proteins is attempted all over the world. Usually, several groups compete for being the first who uncovers the structure of a protein.

Consequently, many similar research processes take place in different laboratories.<sup>1</sup> Similar substances are synthesised, similar methods are applied, similar results are produced, and similar resistances are faced by the groups. We can describe this resistance either with actants resisting their enrollment or as initial resistance of material agency that starts the mangle. Despite their conceptual and philosophical differences, both models could be applied, a fact that hints to their fundamental similarities. But how can we explain very similar material resistance in different

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<sup>1</sup> In the cases included in the empirical study, the similarities were not limited to material influences. The research groups' institutional and economical conditions were equal, and the collaborators had similar interests.

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situations that were separated by time and space?

Four different explanations could be applied. Firstly, one could state that we asked the wrong question because the situations were not similar but incommensurable (Pickering 1995: 185-190). The research situations differed indeed in many characteristics such as local knowledge, machines and group size. However, the incommensurability argument is at least not very convincing because both Actor-Network-Theory and Mangle claim nearly universal applicability. Thus, even all the incommensurable research situations must have something in common that enables to apply these general models. More specific, Knorr-Cetina's description of epistemic cultures hints to fundamental similarities of a field's research processes. From a methodological perspective, the incommensurability of research processes seems to be a problem of suitable research strategies and levels of abstraction rather than a principal feature of research situations.<sup>2</sup>

A second explanation could be that similar situations emerge by pure coincidence. There are no trans-situational regularities in local conditions of research processes, and if they seemingly occur, than by chance. Though this explanation can not be rejected, it is at least not very satisfying because it strongly limits our opportunities to explain scientists' behaviour.

A third possible explanation is that the similarity is caused by social rather than material commonalities. Since researchers share their specialty's paradigm, it is quite natural that they approach material objects and explain failures in similar ways. However, the reference to shared paradigms alone requires to attribute them a strong institutional power that produces uniform perceptions, uniform interpretations and uniform reactions. This is a type of explanation the old institutionalism has been criticised for: Actors become puppets here that can do nothing but follow institutional rules.

A fourth possible explanation is that similar parts of the material world interfere with research processes in a similar way independently of time and place. In other words: they produce constraints for research. The specific conditions for a protein's crystallisation are an attribute that shows up every time when crystallisation is attempted. This attribute can be understood as a material constraint to all investigations of protein structures. Because of the local knowledge

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<sup>2</sup> Since the problem of incommensurability seems to be the crucial point for many epistemological and methodological debates, we would like to add here that in our opinion there is no such thing like an incomplete incommensurability, as Pickering suggests (Pickering 1995: 191). 'Incommensurability' is a precise term: Either there are common grounds or not. And it is at least very difficult to read Pickering's (1995: 210) comparison of "similar" hunts for quarks by Morpurgo and Fairbank as an argument for incommensurability.

about conditions for crystallisation scientists respond to these constraints in different ways and treat their proteins differently. Consequently they face different constraints in the course of their research. Thus, the unique histories of resistance and accommodation can be explained by the fact that scientists' different actions confront them with different constraints.

The concept of material constraints presupposes, firstly, that "how the material world is leaks into and infects our representations of it in a nontrivial and consequential fashion" (Pickering 1995: 183). Secondly, this concept rests on the assumption that there are regular patterns in the material world that show up in research processes in a reproducible way and thus can be explored. From this presuppositions one can conclude, thirdly, that it is these regularities that are theoretically reconstructed by scientists as 'natural laws'. There is no principal contradiction between this view and the observation that all scientific knowledge is constructed. But since the constructions must be brought in accordance with both former constructions and material influences in the lab, they reflect in one way or another both former and current material constraints. What the scientists interviewed by us do is to struggle for better approximations of their constructions to the patterns of the real world they observe in their experiment. From this perspective, the dialectics of resistance and accommodation described by Pickering for single research processes can also be applied to scientific research in general. Thus, the dialectics of resistance and accommodation describes the way in which researchers' ideas become approximated to material world's patterns in an endless series of construction processes.<sup>3</sup>

With the concept of real-world constraints it is possible to explain both similarities and differences in material interferences with research processes and to compare them. For these reasons we included it in our empirical approach as well as in our explanatory strategy. As a consequence, we faced new difficulties.

### **3. How to compare different actants/ mangles?**

Again, we begin with an example. Both experimentalists and theorists apply methods to objects which must to some degree be appropriate. The fit between research methods and research

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<sup>3</sup> Pickering's only argument against this way of justifying "correspondence realism" by concluding from his "pragmatic realism" is incommensurability (Pickering 1995: 185). If incommensurability is given up (a step that seems unavoidable to us), there is not much left to defend realism against correspondence. We hasten to add that the "tuning" of researchers' ideas (and, as we claim, specialties' knowledges as well) to materiality never can result in an absolute truth that mirrors materiality. Instead, tuning should produce better fitting reconstructions that are permanently challenged by new occurrences of materiality. For the concept of "tuning", see Pickering 1995: 13-17.

objects is often endangered by the latter's complexity, that is by the number of different attributes and relations between them which must be dealt with. We found two basic types of misfits between methods and objects. In experimental research, some methods can be applied only to simplified model objects because otherwise observations would be blurred. In theoretical research, some methods or models are not applicable to experimental data because the data describe more simultaneous processes than could be integrated in theoretical models. In this case, theoretical models can be applied only to objects of reduced complexity, that is, to theoretically constructed objects and data.

The misfits between objects and methods influenced researchers' opportunities to respond to institutional settings. In the investigation of collaborations it was observed that several planned collaborations were not started. Other collaborations were started but could not be successfully accomplished. Some of these failures were explained by scientists by referring to a misfit between one collaborator's object and the other collaborator's method.

Well, in principle one could look at it with our models, but I believe it is at first necessary to understand it a bit better. Thus, we must firstly understand the simple questions better ... why there has been no opportunity to do something together up to now.

Also im Prinzip könnte man das wahrscheinlich schon mit den Modellen anschauen, die wir haben, aber ich glaube, da muß man erst noch ein bißchen besser verstehen. Also müssen wir erst die einfachen Fragestellungen noch ein bißchen besser verstehen .. von daher gab es bisher einfach keine Möglichkeit, da irgendetwas zusammen zu machen.

Since there was a strong institutional pressure towards collaboration and scientists invested both time and resources in these collaborations, it can be assumed that the scientists actually tried to collaborate and faced resistance (or, again, the actants' refusal to cooperate) that prevented collaboration.

Another empirical investigation was conducted to study how East German basic researchers responded to the institutional changes brought up by German unification (Gläser 1998). Most basic researcher perceived an institutionalised pressure to conduct research that is, although basic, nevertheless somehow useful. This pressure is institutionalised in general societal expectations, organisations' missions and earmarked funding. The researchers' opportunities to respond to this pressure depended partly on the investigated objects and the applied methods.

Researchers conducting experimental research with objects representing practically applied systems could easily prove that their basic research simultaneously contributed knowledge that is useful for applications. Researchers who did theoretical research unconnected to experimental data and researchers who did experimental research with model objects could not adopt to the institutional pressures. Consequently, the latter type of research was found only in institutionally shielded niches.

With these examples, we tried to show that comparative institutionalist studies must include comparisons of material influences in order to judge the effects of institutions. Unfortunately, constructivist studies are not very supportive here. Both ANT and Mangel combine idiosyncratic descriptions of single cases with highly abstract explanatory models. Even Knorr-Cetina's latest book about 'epistemic cultures' (Knorr-Cetina 1999) follows this pattern. It contains holistic descriptions of epistemic cultures that convincingly enough suggest that the cultures are different, but only very few highly abstract comparisons. This follows from the strategy chosen by Knorr-Cetina:

Using a comparative optics as a framework for seeing, one may look at one science through the lens of the other. This visibilizes" the invisible; each pattern detailed in one science serves as a sensor for identifying and mapping (equivalent, analog, conflicting) patterns in the other. A comparative optics brings out not the essential features of each field but differences between the fields. (Knorr-Cetina 1995: 4)

Thus, the comparison is undertaken as a 'grounded theory - strategy' by switching between the two optics provided by the observed fields. The framework is built by a bottom up - strategy. Such an approach is consistent with constructivism's rejection of theoretically informed empirical strategies. It is, however, limited because it produces only what is shown by the "lenses", i.e. by the investigated fields.

The search for a comparative approach to research processes reveals interesting similarities between the three constructivist approaches: They all combine detailed idiosyncratic descriptions of empirical cases with theoretical models on the most abstract level. Nothing is offered in between, and there is no support for comparisons of one actant with another, one mangle with another, or one laboratory with another. The lack of intermediate levels of abstraction seriously hinders comparative studies. Because they lack an analytical framework, Actor-Network-Theory and Mangle are rather complex languages that provide narratives than theories.

#### **4. How to include materiality into social studies?**

The ‘resistance’ of constructivist approaches against comparative research can be explained by referring to the basic dilemma that has been formulated in the chicken debate (Collins and Yearley 1992a, b; Callon and Latour 1992). Pickering put this dilemma in the following way:

As analysts, Collins and Yearley suggest, we have just two alternatives. We can see scientists as producing accounts of material agency, in which case these accounts fall into the domain of scientific knowledge and should be analyzed sociologically as the products of human agents. This is the standard SSK position that Collins and Yearley want to defend. Or we can try to take material agency seriously, on its own terms - but then we yield up our analytic authority to the scientists themselves. Scientists, not sociologists, have the instruments and conceptual apparatus required to tell us what material agency really is. (Pickering 1995: 12)

Both Callon and Latour (1992) and Pickering (1995) reject the presupposition that there are only two alternatives. Callon and Latour propose a semiotic approach that shall enable to think symmetrically about human and nonhuman agents. Pickering criticises this reply as an “retreat, a return to the world of texts and representations that one does not wish to make” (Pickering 1995: 13). Moreover, he doubts that ANT is consistently engaged only with signs of materiality (*ibid.*). His own solution is to take material agency as temporally emergent in practice:

Thus, if we agree that, as already stipulated, we are interested in achieving a real-time understanding of scientific practice, then it is clear that the scientist is in no better a position than the sociologist when it comes to material agency. No one knows in advance the shape of future machines and what they will do, but we can track the process of establishing that shape without returning to the SSK position that only human agency is involved in it. (Pickering 1995: 14-15)

With this approach to materiality, Pickering apparently solves the problem posed by Collins and Yearley and avoids the retreat to texts.

The problem laid out by Pickering and the solutions offered refer to the problem of how to include something non-social into sociological explanations. The three solutions offered in the debate are

- (1) to include only scientists’ socially constructed accounts and to treat them as social facts (the first alternative preferred by Collins and Yearley),
- (2) to include scientists’ socially constructed accounts as accounts of materiality (the alternative rejected by Collins and Yearley), and
- (3) to include materiality in a specific sociological way by using a specific language to describe materiality (the third way proposed by Callon and Latour as well as Pickering).

The first solution must be rejected because it has turned out that materiality interferes in research

processes. That is why it has to be included into explanations. Thus, the observations presented by ANT and Mangle changed the status of materiality: Materiality has been identified as an intervening variable in sociology of science's explanations. The second solution can be rejected on the grounds of symmetry, but there are more methodological arguments than that. To use scientists' accounts of materiality would turn these accounts from a subject matter of science studies into an instrument of science studies. However, it is methodologically impossible to include statements of the natural sciences into sociological explanations. These statements are constructed in a completely different scientific context. To use them, they must always be translated into a language (and, thus, in a context) sociology can work with.

This is essentially the 'third way' proposed by Callon, Latour and Pickering: If we do not want to neglect materiality and do not want (or are not able) to rely on natural sciences' accounts, then we must develop our own language for describing materiality and including it into sociological analyses. ANT and Mangle offer different solutions, that is, different languages that can be used for describing material influences and including them in sociological explanations. Thereby, the problem of describing materiality in a way that is compatible to social studies is solved differently: Callon and Latour give materiality an actor-like status and this way make it possible to describe material influences as actions. Pickering focuses on materiality as resistance to goal-directed human action and this way makes it possible to describe materiality as conditions of human action.

However, as the problem of comparison revealed, both offered languages suffer from a very limited applicability that is due to their idiosyncratic construction. Both approaches treat the dealing with materiality as a theoretical rather than a methodological and methodical problem. Thus, the languages offered by the approaches came along with whole theories that shall supersede their predecessors. Two complete but incompatible theories emerged.<sup>4</sup>

A second problem connected with both languages is their 'resolution', i.e. the opportunities for detailed descriptions of materiality. This resolution is very small in both approaches. A detailed description of scientific practice is combined with a very poor language for a sociological description of materiality (actants, inscription devices, enrollment ... respectively resistance,

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<sup>4</sup> ANT and Mangle are not incommensurable because they share empirical phenomena they want to describe and explain and share methodological assumptions. However, the idiosyncratic languages make it difficult to compare these theories. Actually, one could wonder if the incommensurability theses is a generalised experience of social scientists rather than a result of observing natural sciences.

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acomodation, tuning).<sup>5</sup> The phenomenology of scientific practice is directly reformulated as a theoretical model without detailed theoretical accounts of how materiality works.

To sum up this short discussion, we would explain the combination of idiosyncratic empirical descriptions with non-specific accounts of materiality in both ANT and Mangle by

(1) the emphasis laid on theory and the neglect of methodology in both approaches, and

(2) the attempt to reduce of materiality's complexity for the sake of sociological description.

Because of their non-specific account for materiality, neither ANT nor Mangle support comparative analyses. That is why we prefer attempts to include materiality into traditional theories of action. This can be achieved by treating materiality as one of the cognitive structures that influence research processes.

## **5. From material constraints towards cognitive structures**

To include materiality in our comparative studies, we were forced to develop a conceptual framework that enables to look systematically for material constraints, to interpret their influence on the observed research processes, and to compare these influences. Moreover, influences of the existing knowledge must be included in a similar way. Material influences as well as influences produced by knowledge do not only constrain actions but also enable them. That is why we prefer the term 'cognitive structures' for the general patterns that influence research processes. 'Cognitive' is related here to the research action only, that is to the production of new scientific knowledge. Thus, the term 'cognitive structures' is not limited to scientists' cognitive processes, but describes influences on knowledge production that stem from materiality, knowledge and instruments.

With the concept of cognitive structures, materiality's and knowledge's influences can be integrated into studies of researcher's actions. However, the description of cognitive structures faces the same problem as the other approaches: Materiality, scientific knowledge and instruments must be described not as such, but with regard to their sociological relevance. We understand this task as a challenge for sociology of science that will take at least some decades

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<sup>5</sup> Obviously, ANT offers more concepts than the Mangle. However, none of these concepts enables to distinguish material influences in different research processes. Ultimately, we are left with 'worked - did not work' by both languages.



of "tuning".<sup>6</sup> In the following, we sketch some of our experiences in order to outline how such a framework might look like.

Although it is not our primary concern here, knowledge provides very interesting cognitive structures because it is both a product of social constructions and a 'hard' condition of action. Knowledge is subject to a process of institutionalisation that makes it to an objective reality that cannot be wished away (Sismondo 1993). In words of Berger and Luckmann (1967: 57): "The paradox is that man is capable of producing a world that he then experiences as something other than a human product." That is why we included knowledge: it may appear as hard as materiality and therefore influence research processes in the same way. For example, structure of mathematical theory restrict researchers' choices between mathematical techniques not only in mathematics (Pickering and Stephanides 1992) but in theoretical physics as well (Merz and Knorr-Cetina 1997). Of equal importance are cognitive structures that are produced by general attributes of knowledge. Important examples that have been discussed in the literature so far are the structure of theories, the degree of codification of knowledge, and interdisciplinarity or, more general, the variety of different knowledge systems which must be integrated in the course of a research process. These cognitive structures have been used to describe scientific fields already twenty years ago. However, the discussion of 'restrictedness' and other cognitive structures was not grounded on empirical investigations and therefore speculative to a great extent.<sup>7</sup>

The most important cognitive structures provided by materiality are of course the research objects' unknown attributes. These unknown attributes partly constitute the research's subject matter, that is they must be 'produced' in empirical research and theoretically reconstructed. Because they are unknown, these attributes cause resistances (as described by Pickering 1995) respectively anomalies in research processes (Star and Gerson 1987). Beside these specific attributes related to a research's subject matter research objects have general attributes who influence conditions of action. In our investigations, a research object's complexity (we already dealt with this attribute in one example) and its internal dynamics have played a role. A research object's dynamics influences the time needed for research processes. For example, some elementary particles exist only splits of a second, some microorganisms reproduce themselves in about 20 minutes, and a cloned sheep needs several months to grow.

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<sup>6</sup> As it is the case for natural sciences, there is no absolute truth here. Scientific knowledge and observed materiality will permanently change.

<sup>7</sup> For the most prominent account, see Whitley 1984.

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Material influences on research processes are also produced by instruments. Thus, it is a combination of material factors and knowledge which produces constraining and enabling characteristics of research methods. One of these characteristics is a method's range of applicability. Depending on the effects built into an instrument the method that is based upon that instrument can be applied to a narrower or wider range of different objects. For example, electron microscopy is successfully used in biology, physics and chemistry because the interaction of electrons with matter that is built into the instrument applies to many research objects. Other methods such as Immunoassays are very specific because they can be used only to identify one substance.

The turn from constraints to cognitive structures requires a comment on emergence. We agree with Pickering in that what he describes as resistances is situationally emergent. However, the emergence of situationally unique cognitive conditions of actions does not imply that there are no general cognitive structures. The overlap of different cognitive structures and the researcher's individually specific knowledge, perceptions and strategies create an unique combination of cognitive conditions of action. With our approach, we try to identify important similarities as well as differences within these cognitive conditions of actions, trace them back to general cognitive structures and thus to provide grounds for comparative analyses. By treating materiality as one of the cognitive structures of action, we were able to include it into our comparative studies. The examples we listed above are the first elements of a framework we try to use in order to compare research processes in different fields. We think that such a framework is necessary for institutionalists who try to avoid institutionalism's traditional failure of black boxism by including the micro-level of research processes into their investigations. However, treating material influences as one cognitive structure among others and as intervening variable forces us to depart from Actor-Network-Theory and Mangle and deal with materiality in a way that does not replace old theories but is compatible to them.

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