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THE USE OF EMPIRICAL MATERIAL FOR THEORY DEVELOPMENT

For social scientists, empirical work is one – if not the only – core activity. At the same time, theoretical knowledge is often seen as the most interesting, valuable and prestigious part of a scientific study. It is also broadly seen as the most difficult element to add. The academic reader of a PhD or a reviewer of a research paper will often find the lack of a theoretical contribution one of the greatest shortcomings. It is not so difficult to produce a description of what people do and say through interviews, observations and other methods, but to continue beyond that and suggest insights, concepts, explanations and other 'deeper' aspects offering a more abstract theoretical understanding that goes beyond the relevance of a particular case or sample studied is not so easy. The empirical and theoretical elements are not always engaged in a productive interplay. This is the starting point for this book and we hope to offer some ideas on how this interplay can be accomplished in a creative, challenging, and novel way.

How can empirical studies contribute to the development of theory? According to the conventional wisdom in social science, two basic approaches are available: deduction and induction. Most researchers would still probably adopt a nomothetic approach, thus emphasizing the importance of the deduction of theoretical ideas from earlier knowledge, the formulation of hypotheses, and the testing of these as key ingredients (c.f. Freese, 1980; see also Popper, 1963; 1972). Empirical tests are used either to verify theories, as neo-positivists would put it, or to refute them, as Popper and his followers frame it. Inductivist approaches such as grounded theory (Glaser & Strauss, 1967; Strauss & Corbin, 1994) and many versions of so-called case study research (Eisenhardt, 1989; Yin, 1984) would emphasize the building of theory based on data. Either way, both inductivist and deductivist approaches share a belief in a clear separation of theory and data and a deep seated trust in the capacity of data to inform and correct theory building. They also share a strong belief in premeditated process and both downplay the subjectivity of the researcher.

The case study approach could serve as an illustration for our point. Although Robert Yin is by far the most well-known advocate of the case study approach we will pay particular attention to Kathleen Eisenhardt's (1989) take on the capacity

for theory development in the case study approach. Eisenhardt suggests that theory development here proceeds through distinct steps: a tentative formulation of research questions (with as little theoretical baggage included as possible); the selection of cases; the crafting of instruments and protocols; an engagement with the field; analyzing within-case data; searching for cross-case patterns (if multiple cases are researched); shaping hypotheses; enfolding literature; and reaching closure. She also stresses that these moves typically follow one another in an iterative fashion. One would expect at lot of travelling back and forth between stages during research and that this movement is critical for the development of new ideas and insights. In addition Eisenhardt stresses the inductive character of the theory development process. Theory emerges through intimate contact with empirical materials, and through the frictions and tension between and within various data sets.

She argues that this also leads to one of the biggest strengths with theory development through the case study approach. Since the case study approach leads to rich and messy data sets, these data sets are rife with contradiction and paradox. This makes it possible to juxtapose conflicting evidence, thus freeing up the curious mind to rethink the relationships between the data points. Eisenhardt argues that this increases the potential for new and creative theory:

> That is, attempts to reconcile evidence across cases, types of data, and different investigators, and between cases and literature increase the like-lihood of creative reframing into a new theoretical vision. Although a myth surrounding theory building from case studies is that the process is limited by investigators' preconceptions, in fact, just the opposite is true. This constant juxtaposition of conflicting realities tends to 'unfreeze' thinking, and so the process has the potential to generate theory with less researcher bias than theory built from incremental studies or arm-chair, axiomatic deduction. (Eisenhardt, 1989: 546)

She also claims that theory generated from case studies is likely to lead to theory that can be measured and be proven false. The idea here is that since theory emerge from empirical settings it always is rooted in concrete realities and eventualities, which means that it in a sense it is already operationalized and also is less likely to be immunized from falsification. Data will keep the speculative mind in check. Eisenhardt believes as well that theory generated from case studies is likely to be empirically valid. Again, the intimate relationship between data and theory almost guarantees that theory reflects underlying realities. Data, or evidence, will provide a compass that can keep the theory generation process on course.

Yin and Eisenhardt make a strong case for using case studies to generate theory. We would agree with this. Case studies, when properly designed, will be helpful for theory-building purposes for a variety of reasons, but most importantly because they provide a strong potential for a certain thickness of description. Properly executed case studies generate an abundance of empirical material that is almost certain to challenge established assumptions and perspectives. We also think that Yin and Eisenhardt are mostly right about the advantages of case studies. However, we would beg to differ on how case studies can facilitate theory generation. It is

clear that both Yin and Eisenhardt have a strong belief in the robust nature of data. This, they both claim, is the big advantage of theory generation from case studies. Data will navigate the process and provide well-grounded and robust theory that has a strong empirical validity. Theory will provide an insight into the complexities and intricacies of empirical reality. In this sense, Yin and Eisenhardt use theory to resolve data, hence the resumed lack of application range.

In this respect inductivists like Eisenhardt and Yin (and Glaser and Strauss), who would claim theory is to be developed through sifting through data, are no different from deductivists who would see theory emerging through the accumulation of verified (or corroborated) hypothesizing. These views of social science are in many ways different, but both rely on data as the central element in social research. Theory is supposed to 'fit' data – either by design, where a lack of fit should lead to rejections or revisions of a theory, or by default, where theory is understood as emerging from data. Theory and data are thus seen as 'external', two different entities that can and should be related while still being recognized as separate.

In this book we shall suggest adopting a different approach. In particular, we would wish to highlight the usefulness of empirical material for theory development through recognizing the fusion of theory and empirical material in the research construction process. We would emphasize the potential of empirical material as a resource for developing theoretical ideas *through the active mobilization and problematization of existing frameworks*. In particular, we shall point to the ways empirical material can be used to facilitate and encourage critical reflection: to enhance our ability to challenge, rethink and illustrate theory. This approach recognizes the constructed nature of empirical material and 'proofs' (Astley, 1985; Denzin & Lincoln, 2005; Shotter, 1993; Shotter & Gergen, 1994; Steier, 1991). It assumes that something is going on out there in reality and there may be better or worse ways of addressing this reality that can be more or less backed up by what might appear to be evidence.

However, it also takes seriously the view that frameworks, pre-understandings and vocabularies are central in producing particular versions of the world. 'Data' in social science are seldom so strong or clear-cut that a researcher can claim to have produced unproblematic knowledge about how complex social reality looks or operates. This is not an excuse for not taking empirical material seriously, but perhaps often to do so in an open-minded and humble way. We would propose a relaxation of the emphasis on 'data' and a greater interest in the contribution of how 'data' are constructed for the benefit of theoretical reasoning (c.f. Sutton & Staw, 1995).

Some time ago, 'empirical' research frequently meant that one could assume an independent reality out there which could be perceived and measured through indications of this reality, i.e. data. Through the careful design of procedures, the collection and processing of data based on this design and the subsequent analysis of these, empirical research could say yes or no to various hypotheses about the chunk of reality targeted for study. Nowadays, it would be seen in many social science camps as old-fashioned, intolerant, and theoretically and philosophically unsophisticated to favour this idea. The label of positivism – as currently broadly defined (or not defined, but used) – invites

all sorts of pejorative comments. During recent times, there have been more varied views on what constitutes empirical research, making the meaning of this activity quite vague. Reading texts of all kinds, for example, could constitute undertaking empirical research for some people.

But typically, 'empirical research' refers to taking a strong interest in gathering or constructing empirical material that says something about what goes on out there – in the social life existing outside of the research practices of academics or available texts. Even the increasing number of people in social science who are skeptical to the possibility of the 'collection' and processing of data in order to say yes or no to various hypothesis and theories will often take an interest in empirical work. In many forms of qualitative studies (e.g. in grounded theory) the assumption is that data, carefully processed, can guide the researcher to understand specific phenomena and to develop theory (Glaser & Strauss, 1967; Strauss & Corbin, 1994).[1] In interpretive work it is assumed that we can access and study social reality through indications of the meanings and symbolic interactions that are viewed as crucial elements in social communities. Even though postmodernism give strong reasons for being more careful and modest about such enterprises than previously, it would be remiss of us not to be interested in what we can learn from empirical work.

The key point of this book is to suggest a framework and vocabulary for thinking about theory development that is inspired by empirical studies and different from conventional views of building on data associated with grounded theory and other 'dataistic' approaches. We would thus emphasize the creative and imaginative constructions of empirical material. Rather than assuming that 'data', like a signpost, point in a specific direction, 'data' read as empirical material make a variety of readings possible and may also make different knowledge results possible. Rather than asking and checking if there is a data-theory fit, we ask and explore if empirical material can encourage the challenging and rethinking of established theory and thus inspire novel lines of theory development.

Questioning the Faith in Data

This great faith in data and empirical inquiry as a cornerstone in knowledge development has been challenged by a multitude of intellectual streams during recent years. A powerful example is what may be referred to as 'non-objective' *interpretivist* perspectives. These put an emphasis on how pre-understanding, paradigm and metaphor can pre-structure our basic conceptualizations of what we want to study. Our approach to, perceptions of, and interpretations of what we experience are filtered through a web of assumptions, expectations and

[1]There are, however, some efforts to develop grounded theory; to move away from neo-positivism and incorporate some ideas of the constructed nature of social inquiry (Charmaz, 2000).

vocabularies that will then guide entire projects and be crucial for the results we arrive at (e.g. Brown, 1977).

Somewhat more far-reaching critiques have been raised by *feminists* pointing at how male domination and masculine standards have influenced the dominant epistemology and methodology in social science (Jaggar, 1989). Male domination has produced a masculine social science built around ideals such as objectivity, neutrality, distance, control, rationality, and abstraction. Alternative ideals such as commitment, empathy, closeness, cooperation, intuition, and specificity have thereby been marginalized. Scientific rationality is thus expressing male domination, rather than superior reason. If one looks at the psychology of researchers and conflicts between different groups, the idea of the distanced and neutral scholar who is rationally oriented towards objective truth becomes peculiar (Bärmark, 1999; Popper, 1976). Researchers can often be very committed to their research and emotionally attached to theories and results. Critique and counter-evidence can then lead to defensiveness rather than a willingness to radically revise a position.

A related point of view has been expressed by *critical theorists* who would emphasize the political, interest- and value-laden nature of social enquiry (Alvesson & Deetz, 2000; Delanty, 2005; Kincheloe & McLaren, 1994). It is argued that knowledge development is grounded in human interests (Habermas, 1972). In social science, it is impossible to say anything of social significance without having some implications for the formation of society – social science is notoriously and inevitably political. Neither the researcher nor the other actors involved in influencing a research process and its outcome (research foundations, research leaders, editors and reviewers, the people studied and the mass media, and others who would guide them in how to think and how to express themselves) can exist in an ideological vacuum. It is seldom possible to identify and sort out the ideological from non-ideological elements when studying families, gender issues, the socialization of children, consumption, our care of elders, voting behaviour, ethnicity, etc. The vocabulary is, for example, not neutral, even though commonly used language will often give the impression of being so. That human interests and cultural, gendered and political ideals can put their imprint on methodological ideals as well as on research practices and results makes it very difficult to see science as a pure activity – neutral and objective in relation to the reproduction or challenging of social ideologies, institutions and interests.

Even more profound are the views of *discursivists* and (other language-focused) constructivists which would deny science any privileged access to the objective truth about the social world outside language and language use (Potter & Wetherell, 1987; Steier, 1991). Language constructs rather than mirrors phenomena, making representation and thus empirical work a basically problematic enterprise, or so it is argued (Denzin & Lincoln, 2005; Gergen & Gergen, 1991). What (possibly) exists 'out there' (e.g. behaviours) or 'in there' (e.g. feelings or motives) is complex and ambiguous and can never simply be captured, but given the perspective, the vocabulary and the chosen interpretation, 'reality' can emerge in a particular way.

Any claim of truth then says as much or more about the researcher's convictions and language use than about the object of study. Foucault (1980), probably the most influential social theorist (broadly defined) at present, claimed that social scientific knowledge was closely associated with power (the regulation of social reality through arrangements and ordering devices) and less with exploring or distorting truth than creating it.

To sum up, it is increasingly common to claim

> that there is no clear window into the inner life of an individual. Any gaze is always filtered through the lenses of language, gender, social class, race, and ethnicity. There are no objective observations, only observations socially situated in the worlds of the observer and the observed. (Denzin & Lincoln, 1994: 12)

The critique of positivism and neo-positivism is massive. However, this does not stop the majority of researchers from doing normal science more or less as if nothing has happened. Questionnaire researchers still assume that the X's put in small squares by respondents make it possible to determine what goes on in the social world. Qualitative researchers still present interview statements as if they were pathways to the interiors of those being interviewed and that observational data via codification and categories will mirror social practice, although it is broadly recognized – also amongst positivists – that data need to be interpreted to say anything. And that the process always involves the (selective and contestable) construction of data, as well as any use of it.

One problem with the critique of approaches having a strong faith in data is that it is perceived as categorical, provocative and destructive, and therefore is neglected. Another problem is that much of the critique addresses philosophical and epistemological issues, while the craft of doing research – for example fieldwork – has received much less attention. This is largely viewed – at least in most textbooks and also in research reports – as a technical matter, separated from theoretical and philosophical ideas about knowledge production, although some change is on the way here. Method (the action-related principles and ideas on how to produce and make sense of empirical material) still largely remains comparatively unaffected by all the work that has tremendous relevance for our understanding of methodological practices. The wealth of insights into problems of developing knowledge and the limitations to social science as a rational project need to be connected to research practices. Many researchers feel that all this philosophy of science and associated critique of traditional research are of limited relevance (e.g. Melia, 1997). We think the challenge is to try to incorporate parts of these into research practice. This rather heterogeneous but rapidly expanding critique of social research and its uncertain relevance for specific methods for doing fieldwork, interpreting and writing poses one context for this book.

The ambition then is to work with empirical material and to take it seriously without giving it a non-motivated robust status, as well as to treat it as if it offers a strong authority for forms of knowledge that are based on claims of being grounded in data and revealing reality. The ambition is also to put critical

ideas about dataism into constructive use, where the possibilities – rather than the problems and impossibilities of empirical research – can be emphasized.

An Honest Account of How Empirical Material is Created and Processed

The aforementioned, more theoretically- and philosophically-based critique of empirical results as a solid building block in the accumulation of knowledge can be supplemented with some consideration of the practical problems of mirroring reality out there in research texts.

A major issue with the (limited) reliability of empirical studies concerns the many (more or less) coincidental and arbitrary ingredients required in the transportation of representations of whatever is supposed to be studied into the final research text. Given the many elements that matter, it is always highly uncertain what exactly the latter report will actually say about the former.

Let us illustrate this with the help of a short vignette. This is Dan's story of how he undertook his thesis:

> I did my PhD thesis on the subject of the role of organizational culture in professional organizations – in my case, in the newsroom at an evening newspaper.
>
> Methodologically, the study drew mainly from anthropological and ethnographic traditions. Consequently, I spent nine months (two or three days a week during the first six months and one day a week during the three subsequent months) participating in and observing every day organizational activities.
>
> I participated in and observed meetings, conversations between organizational members of different ranks and in different contexts and events occurring in a routine or non-routine fashion. I conversed with as many as possible on an informal basis, resulting in more than 100 interactions spanning from five minutes to several hours. I also interviewed 15 members with different roles and positions on a more formal basis, with each interview elapsing roughly one hour. The interviews and most meetings were taped and later transcribed. I also kept a diary where I made field notes on things that attracted my attention during the day.
>
> As a result, I almost drowned in data. I had 250 pages of interview transcripts, roughly 700 pages of transcripts from meetings – I ended up with more than 100 hours of tapes from meetings, with each hour yielding about 20 pages of transcript and, in the end, I decided that it was simply not worth it to transcribe everything – and about 100 pages of field notes. Given my particular subject, some of the data seemed to be easy to organize – key symbols, dominant values, and so on. Other things were more difficult to get a grip on. Mundane meetings, routinized work procedures, non-descript office buildings and hallways didn't exactly provide a self-evident input to symbolic and cultural analysis. Most of my material, it occurred to me at the time, was redundant, irrelevant or unsurprising. Still, there were some discordant themes that nagged at me and that could not easily be dismissed as obvious, absurd or pointless. Why, for example, did there seem to be a

qualitative difference between the two daily meetings between supervisors? Why was hierarchy both embraced and dismissed?

In the end, approximately 5 per cent of the data eventually showed up in the final thesis, mostly in the form of interview excerpts and excerpted transcripts from meetings. I do think that I am presenting at least a somewhat authoritative account of how an organization culture can co-ordinate activity in the newsroom. I am also the first to admit that another researcher may have chosen to put an emphasis on some other 5 per cent of the text – presumably reflecting what people said when interviewed by me or how I thought they acted and talked when observing them – or may have chosen to report another story, with the help of another subset of the empirical material, and or may even have chosen to interpret my particular subset differently. There was, for example, some good stuff on how the media are key players in creating and constituting the contemporary culture that I could have done more with, but my academic affiliation (business administration) possibly made me inclined to pursue this theme. If my advisor at the time – Mats – had had a smaller commitment to cultural analysis this would also likely have affected my choice of frame of reference.

This account of the problems inherent in condensing a great deal of empirical material into a manageable research report is quite typical for qualitative research. Apart from the inherent selective nature of research reports – one always has to reduce complex empirical material down to directly reporting interview citations and observations that will fill no more than 50 pages if publishing a monography, or no more than five to seven pages if publishing the research results in a journal – there are other concerns that will guide the creation of empirical material in the first place. There will be plenty of filtering elements elsewhere, emerging from the researcher, the research community and the society guiding the individual, as well as from those being studied. These concerns will include a researcher's personal and social background; his/her philosophical, theoretical and political commitments; the expectations and interests of the natives who are selectively reporting things (or perhaps modifying their behaviour due to the researcher observing them); the interplay between the people being studied and the researcher; and the problem of ordering the material in a way that makes sense from a theoretical and paradigmatic perspective (Alvesson, 2002).

There are strong social pressures and convention-guided constraints that will put their imprint on the process and outcome. A researcher's work can often be seen as an effect of the various social forces at play and the pressure to show coherent and convincing results. A persuasive study mobilizes and prioritizes empirical material that clearly supports one case. Exercising caution in bringing forward observations, interview statements or artefacts that go in other directions or indicate that it is hard to say something decisive is a wise move here. There is of course the norm that one should exhibit broad and representative empirical material, but one most also do so without giving too much space to material which is incoherent or ambiguous in relation to one's major idea and thesis. To be persuasive and to minimize the reader's doubts give the template for success.

This means that one can not just report a statistical representative of interview statements or observations without facing sanctions, as this would make its readers confused or unconvinced. Conventions for writing (like formatting, style, modes of persuasion and so on) must be adapted. Readers will often want convergence, support for a story, and clear and convincing patterns to be demonstrated. Even if one makes a case for ambiguity or contradiction for example, this should also be demonstrated in such a clear-cut way that the reader don't suspect some 'deeper' pattern behind any inconsistencies and confusions. (See Alvesson, 1993a, for a demonstration of how behind such claims about ambiguity the opposite element – a shared understanding – can be detected.) In practical terms, it is perhaps 5 per cent (or 1 per cent in an article) of all the available data presented that are, presumably in most cases, chosen, ordered and framed so that other people can be convinced that, 'Aha, here we have a valid empirical study'. As Alvesson et al. (2008) argue, it is in many cases the adaptation to and negotiations with the social and political context that are key driving elements behind a finished and published text.

This is not necessarily a bad thing and it is completely unavoidable. The legitimate researcher wants to craft a text that is well argued and convincing and claims a strong contribution, that is inextricably fused with selective reporting, interpretation and framing. Responses to advice from colleagues, editors, supervisors, reviewers; the anticipation of reader responses; commercial considerations about fashionable and hot topics, and so on – all of this will matter here and tend to contribute to an emphasis on coherence and the delivery of clear results. The final result – the text – will inevitably be the outcome of several complex processes, most of which simply couldn't be described in rational and objective ways. There are too many elements involved, which in turn may or may not have a strong impact on the study.

It is likely that another researcher, even if the purpose and object of research are the same, with another gender and political orientation, and having read (partly) other texts on method and theory as well as other empirical studies, would choose slightly different people to study, would use other categories and vocabularies, would encounter other colleagues, editors and reviewers – and so produce quite different research results. Our overall point is that this is the case *not only* because of the effect of the necessity of interpretations and constructions; the inability of language to mirror the world; the political context of social science; and the ambiguity and indecidability of major parts of social reality. It is *also* the case for very practical, down-to-earth reasons that can be associated with assembling research texts and getting them published and distributed to the wider world.

Our argumentation and overall point are illustrated in Figure 1.1. Conventionally, empirical studies would claim that the phenomenon studied (the cloud-like object to the left) is, at the end of the research project, authoritatively represented in the final text along with reliable indicators of that phenomenon (descriptions, interpretations, summarizing results). But one could argue that all the elements (a) to (g) in the figure follow partly different logics than just one of simply reflecting the characteristics of the object of study and that it is an open question

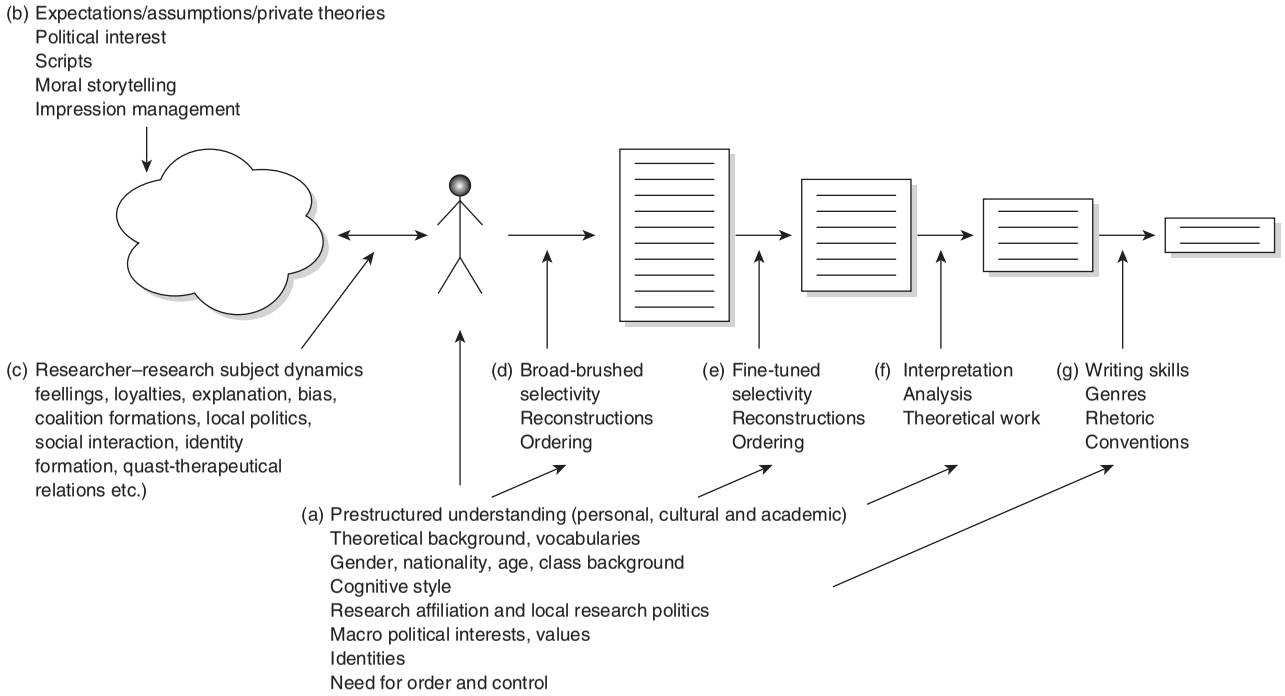

Figure 1.1 The research process: a complex version

as to which of these ingredients would have put the strongest imprint on the final text and its truth claims. Any of these may matter more than the 'objective' characteristics of the phenomenon presumed to have been studied. Taking this seriously suggests that we might consider empirical research in terms of its offering other virtues and contributions than mainly being about the accurate reporting of the phenomenon studied.

Empirical Material as a Critical Dialogue Partner

However, the focus in this book, as has already been mentioned, is not to demonstrate the problems with rationalistic approaches to research or to show what the research process 'really' looks like. Rather than engaging in a generic critique of the difficulties and relevance of doing empirical studies, we aim to put forward a constructive agenda. Thus we see empirical material as the outcome of various constructions – the effects of interpretations and a use of specific vocabularies – and as such it is fused together with theory rather than external to it. In this book we emphasize the potential of empirical material as a resource for developing theoretical ideas through the active mobilization and problematization of existing frameworks. In particular, we point to the ways in which empirical material can be mobilized to facilitate and encourage critical reflection: to enhance our ability to challenge, refute, refine and illustrate theory.

This approach is informed by strong arguments against 'dataism' and a recognition of the constructed nature of empirical material and 'proofs' (Alvesson & Deetz, 2000; Astley, 1985; Shotter, 1993; Shotter & Gergen, 1994; Steier, 1991). It advocates a light or moderate version of constructionism – assuming that something is going on out there and that there may be better or worse ways of addressing things. We propose a relaxation of the emphasis on robust 'data' for the benefit of our consideration of empirical material as a strong but also flexible input for theoretical reasoning (c.f. Sutton & Staw, 1995). From a post-empiricist standpoint, this orientation towards empirical findings offers a viable way forward, especially for those who are interested in what may be encountered in empirical work and who would recognize that all empirical material is constructed, being irredeemably fused with theory and interpretation. This orientation goes against the conventional neo-positivistic criteria favoured by advocates of objectivistic-dataistic qualitative research (a type of grounded theory; see Alvesson & Sköldberg, 2009; Denzin, 1994). However, it does not reject the possibility that some interpretations or constructions may be more empirically supported and qualified than others.

In conventional terms, we shall focus on the discovery (or even better the creation) of theory, rather than its justification. Although we do find novel approaches towards the refinement and justification of theory valuable, we aim for more creative ways of theorizing. Like many others, we would claim that data (or our preferred term 'empirical material') are simply not capable of showing the right route to theory or screening good ideas from the bad. Rather,

empirical material is an artifact of interpretations and the use of specific vocabularies. As will be repeated a few times in this book, data are inextricably fused with theory. Acknowledging this fusion – which is broadly accepted in the philosophy of science (Denzin & Lincoln, 2000; Gergen, 1978; Kuhn, 1970; Rorty, 1979; Wittgenstein, 1953) – has major consequences for how we should consider the theory–empirical material relationship. Empirical material should be opened up rather than viewed as a source of constraint and discipline in research work.

As we see it, the interplay between theory and empirical material is more about seeing the latter as a source of inspiration and as a *partner for critical dialogue*. Empirical material is then *not viewed as a guide to or as the ultimate validator for knowledge claims*. The latter seems to be the preferred root metaphor in method books and in reported empirical studies, but as we outlined above it relies on problematic assumptions. Although fieldwork in social science is typically performed among speaking objects (subjects) one must remember that social reality never speaks for itself. It always speaks through a language that is familiar to and favoured by the speaker; a matter further complicated by the fact that in social science it is not only the subject but also the researcher who will act as mediators of the social world. We must recognize our pre-understanding as researchers (and as subjects targeted for study as well) and our active involvement in construction processes. This opens up the way for more interesting and reflexive constructions that can allow empirical materials to speak back to pre-understandings and theoretical ideas. To engage with empirical material as a critical dialogue partner is thus difficult and challenging. It clearly moves beyond a grounded theory-like codification and the discovery of categories. The very outlook on knowledge creation is fundamentally different.

A key element here is the role of empirical material in inspiring the *problematization* of theoretical ideas and vocabularies. To problematize means to challenge the value of a theory as well as to explore its weaknesses and problems in relation to the phenomena it is supposed to explicate. It also means to generally open this up and to point out the need and possible directions for rethinking and developing it. We consequently attempt to develop a methodology for theory development through encounters between theoretical assumptions and empirical impressions that involve breakdowns. It is the unanticipated and the unexpected – the things that puzzle researchers – that are of particular interest in such encounters. In this sense, our approach attempts to take systematic advantage of what Robert Merton labels serendipity: 'the art of being curious at the opportune but unexpected moment' (Merton & Barber, 2004: 210). Accordingly, theory development is stimulated and facilitated through a selective interest in what does *not* work in an existing theory, in the sense of encouraging interpretations that will allow a productive and non-commonsensical understanding of an ambiguous social reality.

The empirical material, when carefully constructed, thus forms a strong impetus for rethinking the conventional wisdom. However, the ideal is *not*, as in neo-positivist work, to aim for an 'intimate interaction with actual evidence' that 'produces theory

which closely mirrors reality' (Eisenhardt, 1989: 547).[2] The empirical material may be mobilized as a critical dialogue partner – not as a judge or a mirror – which problematizes a significant form of understanding, thus encouraging problematization and theoretical insights (c.f. Ragin, 1987; ch. 9). The dialogue metaphor is not uncommon in contemporary qualitative research. Emphasizing the critical aspect of theory as well as data construction, and therefore involving a careful consideration of alternative representations, frames the enterprise somewhat differently to that found in established views. We think it is important to draw attention to the construction of friction as a potentially productive force rather than to any harmony in the interplay between theory, researcher subjectivity and empirical material.

Rather than displaying a positive process of mutual support, where theory guides empirical work, data modify theory, etc., the theory-impregnated nature of empirical material is emphasized and the value of developing friction and conflict between various possibilities in constructing data is highlighted.

Research as the Creation of Mystery

We have been inspired here by the Swedish sociologist Johan Asplund's stimulating (1970) idea of social science as involving two elements: the discovery or creation of a breakdown in understanding in theoretical interest (the construction of a mystery) and the recovery of this understanding (the resolution of the mystery).[3] Asplund views writing (good) social science as similar to writing a (good) detective story. You create a mystery and then you solve it. In a sense, our project also shows an affinity with Poole and Van de Ven's (1989) suggestion of viewing paradoxes as resources for theorizing, and with Abbott's stimulating (2004) account of heuristics as a method for discovery. However, in contrast to Asplund's and Poole and Van de Ven's strong focus on armchair theorizing, and Abbott's somewhat misguided attempt to be exhaustive, we would pay particular attention here to the interplay between theory and empirical material, thus focusing on how the inconsistencies and breakdowns derived from empirical observation, rather from than (pure) theoretical speculation, may help us to develop theory.

Put succinctly, we aim to take the mystery creation and solving approach to the field and develop it in the context of empirical research, showing how impressions

[2]Neo-positivism (or post-positivism) assumes the existence of a reality that can be apprehended accurately but also imperfectly and probabilistically, where the observer and the observed can be separated, and the data and theory can be treated as separable, although the theory-ladenness of data is acknowledged. The aim is to produce generalizable results (Lincoln & Guba, 2000). Most contemporary quantitative social research and qualitative research like grounded theory (although there are different versions of the latter; see Charmaz, 2000) appear to be based on neo-positivist assumptions.

[3]Asplund (1970) develops two metaphors for creating novel understanding of social reality: the riddle and the crime mystery. In this chapter we use a generalized version of the mystery metaphor as a device for developing theory.

of social reality can be a major source for developing new and challenging insights and, subsequently, for developing theory. Chiefly, our goal is to explore how empirical material can be used to develop theory that is interesting rather than obvious, irrelevant or absurd (Davis, 1971). A key element here is challenging the assumptions within the particular field one wishes to contribute to. This can of course be done through problematizing an existing theory as well as studies in a field via critical scrutiny, combined with efforts to produce alternative ideas (Alvesson & Sandberg, forthcoming), but the objective for this book is to explore how empirical inquiry can be used to challenge assumptions.

It is, of course, not just a question of 'pure' empirical inquiry, as this view of research casts its shadow on the research questions asked, our reading of literature, fieldwork design, tactics, interpretations during and after fieldwork, reinterpretations and writing up research texts. All these elements are important as well as related, and a researcher – having adopted a mystery-view – will go back and forth between different parts of the research process in, at best, creatively stimulating ways. (We must, however, in the space available here focus on empirical studies. We shall try not to do this narrowly or strictly, as we of course will touch upon other related issues as well.)

Theorization may be understood as disciplined imagination (Mills, 1959; Weick, 1989). Empirical material can facilitate theorization because it provides resources for both imagination and discipline. And although empirical material never exists outside perspectives and interpretative repertoires, it nevertheless creates a relative boundary for imagination. Some constructions make more sense than others. Many appear far-fetched or pointless. This is partly a matter of experience of empirical accuracy. Empirical material anchors the process of theorization in specific claims about the object under study, thus prohibiting arbitrary ideas from being put into play. An important element here is to realize that empirical material may feed into rethinking the obvious. This is mainly provided by experiences indicating that the assumptions of conventional wisdom – a tradition, a school of thought, or a framework of theories – are problematic, and where prevailing understandings break down. Breakdowns may appear problematic initially, but they also create spaces where imagination can be put to work.

Consider, for example, Weick's (1993) interpretation of the Mann Gulch disaster – an event that killed 13 firefighters in an explosive forest fire. The central breakdown in this came when a well-trained group of smoke-jumpers ceased to operate as a professional unit and began to disintegrate and panic with catastrophic results. Weick maintained that the main rationale for the disintegration of the organization could be traced to a collapse in sense-making. His argument was that as long as the fire behaved as the team had expected the unit would have operated normally, but when it had unexpectedly turned into an explosive firestorm, a change that was only picked up by the group leader, there was a catastrophic loss of meaning. The other group members had then failed to grasp the reason for the leader's abrupt change of procedure – which included lighting a fire in front of the crew and thus creating a fire escape, all the while urging them to drop their tools and jump into the ash-filled area. This led to a collapse in understanding, which then

led to a sudden disintegration of the organization and thus to panic, which caused the 13 fatalities (the group leader survived). Weick resolved the 'mystery' by pointing out that, contrary to conventional wisdom, panic results from organization disintegration rather than the other way around, and that the organization disintegration was the result of getting stuck in an interpretation of events that suddenly ceased to be justified, thus suggesting that meaning is primary to structure.

Another example here is provided by Aschcraft's (2000) paper on the bureaucratization of personal relationships in a feminist organization. Her study was situated in a shelter for victims of domestic violence, which explicitly embraced diversity, feminist leadership and counter-cultural practices such as 'ethical communication'. Briefly, ethical communication could be viewed as an attempt to create undistorted communication. Members were expected to express themselves authentically and members expected to have sufficient space to articulate themselves. This was taken very seriously at the shelter and was viewed as the centerpiece for enabling empowerment and realizing the feminist ethos. Bureaucracy was viewed with suspicion – a suspicion that Aschraft noted was shared by most of the literature on feminist organizing. Accordingly, one would have expected the shelter to have been a lively place where members and volunteers were unafraid to express themselves and to engage freely in conversation, and where emotions and feelings were vividly at display. Largely, this was also what Ashcraft found, with one highly notable exception. As it happened, concerns had mounted between members on the appropriateness of certain types of relationships at work. In particular, romantic and sexual relationships between affiliates had begun to be constructed as highly problematic, and this had also extended to friendships. The solution to this perceived problem was to institute a highly specific policy on the type of relationships that was allowed between affiliates, a policy that was also largely effectively policed. In effect, the policy prescribed that relationships between affiliates had to be professional and detached, at least in terms of romance, sexuality, and friendship.

How is it possible that an explicitly feminist organization with institutionalized feminist practices could end up with policies that were at odds with a feminist ethos, and could actually embrace the bureaucratic principles that feminism explicitly rejects? The resolution to this mystery could mostly be found, Ashcraft argued, in the intense wariness that existed around the notion of power at the shelter. All of the problems that are supposed to emerge from romantic, sexual and friendship relationships coalesce around notions of the abuse of power. As Ashcraft noted, it was not surprising that this argument had clout in an environment that explicitly fought violence and abuse, although here it took on a neurotic, if not a power-phobic, form.

Exploiting breakdowns is, of course, not new to social science. In particular, in ethnographic work and especially in anthropology the initial difference between the traditions involved (the researcher's and the topic of study) produces breakdowns in understanding: 'A breakdown is a lack of fit between one's encounter with a tradition and the schema-guided expectations by which one organizes experience' (Agar, 1986: 21). The researcher resolves this problem by trying to understand the cultural elements that are causing the breakdown, and

then by adjusting the research schema. Breakdowns will continue to appear until the researcher sufficiently understands the studied culture. This means that ethnography can be described 'as a process of coherently resolving breakdowns' (p. 39). In this sense, ethnography has an in-built propensity towards the type of theory development we outline in this chapter. So this is the case at least if ethnographic inquiry is informed by theoretical ambitions – sometimes the target of ethnography will be thick description, which will fall short of a theoretical ambition.

However, ethnography is far from being the only method that is capable of taking advantage of breakdowns for developing new theoretical ideas. In any kind of study there is always the potential for something that will speak out sufficiently firmly against the assumptions and reasoning that the researcher holds and is engaged in. An example of quantitative studies producing a breakdown is Lincoln and Kalleberg's (1985) piece on job satisfaction and organizational commitment among US and Japanese workers. The result showed higher scores for the former which certainly was surprising, as the general view during this time, when Japanese companies were very successful, was that their workforce had a much stronger work ethic than their US competitors did. The 'mystery' here can possibly be solved by seeing questionnaire responses less as objective measurements of objective phenomena and more as clues to the cultural norms for expressions and following language rules (Alvesson and Deetz, 2000). The questionnaire is perhaps not so much an instrument for accessing people's attitudes than for judging how they tend to follow norms and conventions in expressing themselves. Another example is that of the classical Hawthorne studies, focusing on job performances, human relations and norms of performance among factory workers in the 1920s and 1930s. This research started with experiments on how light could affect performance. The hypothesis was that better light would increase productivity and at the beginning this was indeed confirmed. Increased light increased worker productivity. But this same productivity also continued to rise when the light's strength was decreased. This came as a great surprise to the researchers, who had to rethink their original hypothesis. This led to open-ended ethnographic research that radically explored new ideas about the dynamics of social interactions in the workplace and group norms – a shift that had clearly been encouraged by the empirical material that had challenged the initial research framework (c.f. Schwartzman, 1993).

Our objective in this book is to suggest an approach to theory development that uses theory and imagination to critically open up alternative ways of framing empirical material. Here we are following a large amount of work in methodology, including significant contributions by Asplund (1970), Abbott (2004), Peirce (1978), Mills (1959), Garfinkel (1967), Gergen (1978), Davis (1971), Becker (1996), Weick (1989) and many others in the philosophy of science and interpretive social science. Critical reflection, theory-driven disclosure, and the specific procedure of working with breakdowns and mysteries combine to create an overall

methodology. This systematizes attempts to explore new terrain and develop novel ideas, thus potentially overcoming the inherent conservatism in well-established frameworks. We focus on exploring a maximalist version of breakdown-induced theory development. However, we also address broader strategies for taking advantage of breakdowns for theory development. Our ambition is not to try to colonize empirical research through a specific design, but to provide some overall guidelines and concepts which can be potentially useful for novel theorizing.

Outline of the Book

Apart from this chapter in which we have outlined our overall approach, the book consists of six chapters including this one.

The following chapter deals with constructionist ideas and discusses the role of language and vocabulary, emphasizing the power of a specific research language to (seemingly) control and order the world. Traditional views of language as something that the researcher controls and through which empirical reality is authoritatively captured are critically addressed. How language constitutes the object of study and results also demands consideration. The need to be reflective and to realize vocabularies other than the preferred one – or the culturally dominating vocabulary – are also discussed.

Chapter 3 looks in detail at the norm of exploring patterns in social science. Although identifying patterns is an important exercise in research, it is less helpful for the kind of research we are discussing in this book. Therefore we suggest that theory is likely to emerge through the challenging of established patterns rather than through attempts to put the bits of the jigsaw back together. We introduce five principles – (de-)fragmentation, defamiliarization, problematization, scholarship (broad education or, to use a German term, *bildung* [building]) and reflexive critique – by which to challenge established frames of understanding.

Chapter 4 introduces a five-step guideline, or heuristic (Abbott, 2004), for identifying and constructing mysteries that are suitable for theory development: i) familiarization; ii) enacting a breakdown; iii) elevating a breakdown into a mystery; iv) solving that mystery; and v) developing a resolution. The chapter also discusses some more general ideas on breakdowns and mysteries and how these are cultivated.

Chapter 5 attempts to illustrate the principles outlined in Chapter 3 and the guidelines suggested in Chapter 4 with three empirical cases. These consist of materials on gender structure in the advertising industry; patterns of conversation and identification in a meeting at an evening edition newspaper; and human resource management practices at a management-consulting firm.

Chapter 6 makes a link between our overall methodological framework and specific forms of method/fieldwork. On the whole, the framework can be used irrespective of the type of qualitative or quantitative method, but in qualitative studies one can sometimes mobilize those studied in line with the framework and with a process element that makes mystery creation but in particular mystery

solving easier. We illustrate this with interview research and suggest some ideas on how to address interviewees and conduct interviews, thereby increasing the chances of a productive use of the mystery methodology.

Chapter 7 summarizes our efforts. This is partly done through pointing at our proposed set of metaphors for understanding research and its components, e.g. the view of the nature and role of data/empirical material. The chapter also points to less ambitious ways of applying the ideas brought forward in the book. We here distinguish between breakdown-focused, breakdown-sensitive and breakdown-considering approaches, depending on the level of ambition present when searching for and working with mysteries.