Understanding and Explaining Political Action

A Mixed-Method Strategy

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The popularity of mixed methods is exploding within the social sciences (e.g. Tashakkori and Teddlie 2003, Bryman 2006b). Also within political science, the use of mixed methods seems to be increasing. However, here mixed methods by many researchers (including those associated to the APSA section on Qualitative and Multi-Method Research) has been synonymous with the combination of large-N analysis and case studies doing process-tracing analyses (Lieberman 2005, Rohlfing 2008, Collier, Brady & Seawright 2004:252-264). Unfortunately, this is a rather narrow conceptualization of mixed method research, and there are several other ways of and reasons for combining qualitative and quantitative methods (se e.g. Creswell et al 2006). Especially, this understanding of mixed methods relies on a rather narrow understanding of qualitative methods as historical case studies, leaving aside interpretive analysis (see e.g. Rabinow & Sullivan 1979, Yanow 2003, 2006, Adcock 2003).

In this paper, I present and discuss a specific mixed-method strategy which is aimed at understanding political practices, and which takes its point of departure in the problem of action as well as the epistemological discussion of *Erklären* and *Verstehen*. Among other things, this strategy presents an understanding of qualitative methods that is more interpretive compared to e.g. Lieberman (2005) and Brady, Collier & Seawright (2004). I begin the paper by reviewing the debate on mixed methods paying special attention to the way in which mixed methods typically are framed towards answering specific epistemological and/or methodological problems. I then proceed to sketch a different problem which, I claim, is also relevant for political scientist as well as for social science in general. In the main parts of the paper, I present a specific mixed methods strategy for understanding political practices. By pointing out the implications for method and research design, and presenting an example from my own research, the paper thus seeks to contribute to the further use, discussion and pluralism of mixed methods within political science.

However, let me first make a brief note of terminology. Throughout the paper, I use the concepts of research strategy, research design and research methods. By research methods, I mean the way data are collected and analyzed, e.g. the collection of survey-data by questionnaires and statistical analyses of these data, or the collection of semi-structured interviews and the narrative analysis of these data. By research design, I mean the logic of the research (de Vaus 2001: 8-10, Bryman 2004: 26-57, Munck & Verkuilen 2005) i.e. the basic architecture of the research that makes it possible to answer the research question. This includes, for instance, decisions on the number of units or cases analysed, the selection of units or cases, the choice of an experimental or

observational design, and the choice of a synchronic or diachronic perspective. These are basic and important choices, and the handling of these choices will influence the validity of the final research.

There is, however, no inherent relationship between research design and research method. Different research methods can be used in different designs, and the distinction between qualitative and quantitative research is a distinction cross-cutting the distinction between different research designs (see also de Vaus 2001: 10). Actually, although we often use the terms of qualitative and quantitative research, indicating an existing and important difference within the social sciences, it seems difficult to point towards one defining element of this distinction. (e.g. Mahoney & Goertz 2006). In this paper, I discuss elements of designing research, which are crosscutting and in many ways broader than what is usually included within discussions of design and methods. Following Bryman (2004: 19-21) I define the term research strategy as "a general orientation to the conduct of social research", and following Blakie (2000: 85-86) I suggest that this will include ontological and epistemological assumptions, the starting point of the research, the use of concepts and theories, the styles of explanation and understanding and the status of the knowledge produced from the research. Thus, the concept of research strategy has a meaning similar to the concept of research paradigm used by Johnson, Onwuegbuzie & Turner (2007).

If mixed methods is the answer – what is the problem?

Discussions on mixed methods reach back to the 1950'ies, where for example Barton and Lazersfeld suggested the use of qualitative methods for the generation of hypotheses (in the context of discovery), whereas quantitative methods were necessary for validly and generally testing these hypotheses (in the context of justification) (Kelle and Erzberger 1999). The combination of research methods was founded upon a division of labour as well as a clear demarcation of the uses of different methods within different phases of the research process. Also, the different methods were positioned in a hierarchy of "scientific value" with quantitative methods as *the* scientific method. Thus, the reason for using of different methods combined in this specific phase-model – and, one could say, the main problem addressed by the mixed method strategies – was primarily the cumulative development of research, where qualitative methods were seen as well suited for the discovery and exploration of anomalies and surprising observations.

Also in the 1950'ies Campbell and Fiske suggested the use of more than one method when constructing research methods: "In order to examine discriminant validity, and in order to estimate the relative contributions of trait and method variance, *more than on trait as well as more than one method* must be employed in the validation process" (Campbell & Fiske 1959: 81). As can be seen

even from this small quote, the use of mixed methods in this strategy is aimed at improving measurement validity (see also Johnson et al 2007). Thus, the basic problem being addressed by mixed methods is the inability of a single method to ensure the validity of new measures. Much in the same way, Denzin later elaborated on how to overcome "the shifting nature of the empirical world and the unique bias that arises from theories, methods and observers" (Denzin 1978: 307). Thus, the basic problem for (sociological) research is how to be sure, that we measure and observe reality, since our theories and methods as well as our nature as human beings will inevitably bias our observations. Using the concept of triangulation, Denzin suggested (among other things) that combining different methods would enhance validity and outweigh weaknesses of the different methods (Denzin 1978: 302), and that this would result in "a convergence upon the truth about some social phenomenon".

This conceptualization of mixed methods as triangulation has been massively criticized, though. Hence, several authors (e.g. Fielding and Fielding 1986, Brannen 1992, Kelle and Erzberger 1999, Yanow 2005) claim that the concept of triangulation presents a naïve picture of the combination of methods, neglecting their different epistemological foundations. The problem is that it may not be the same research problem (or even the same "object") that is studied, since different methods may construct the problem (or, some claim, "reality") differently. Further, the concept of triangulation does not deal with the possibility of conflicting evidence. What happens, if different research methods provide us with different results, and how can we determine which results to trust?

The basic problem of social sciences is thus not only moving research forward or overcoming validity problems, but the basic epistemological problem that social science has different "modes of analysis" as well as different objects. Sometimes we tend to observe and explain entities and behaviours "from the outside", whereas sometimes we try to grasp the meaning of actions and concepts "from the inside", i.e. from a participants perspective (see below for further elaboration on this point). However, the basic suggestions from many of the researchers working with mixed methods seems to be that this is an unsolvable problem, and these authors instead suggest the notion of complementarity as the basis for the combination of research methods. Different methods can highlight different aspects of reality, and the mixed methods research thus gives the researcher an opportunity for covering different aspects of a research problem or showing different sides of a multifaceted reality (Brannen 1992: 14; For further discussions of mixed methods see for example

Mathison 1988, Kelle 2001, Nash 2002, Sale et al 2002, Bryman 2006a, 2006b, Mason 2006, Moran-Ellis et al 2006, Johnson et al 2007, Leech & Onwuegbuzie 2009).

Recently –and especially within political science – a further problem that can be addressed by mixed methods has been added to the ones discussed above, namely the problem of causal inference: "Done well, multimethod research combines the strength of large-N designs for identifying empirical regularities and patterns, and the strength of case studies for revealing the causal mechanisms that give rise to political outcomes of interest" (Fearon & Laitin 2008: 758). This argument is further elaborated by the distinction between data-set observations ("observation in the sense of a row in a rectangular data set") and causal-process observations ("an insight or piece of data that provides information about context or mechanism and contributes a different kind of leverage in causal inference") (Collier et al 2004: 252). Thus, causal inference can be done using these different and complementary sources, and therefore social science needs both quantitative and qualitative methods, perhaps even integrated in a nested design (Collier et al 2004:249).

The concept of the nested analysis has been explicated most thoroughly by Lieberman (2005). Here, the mixed methods design is elaborated to entail different paths and relationships between large-N analysis (LNA) and small-N analysis (SNA), depending on the results of the concrete analysis. Thus, within nested analysis, the problem being addresses is the problem of causal inference as well as the progression of science and the improvement of measurement.

In the instance where the researcher's model is confirmed in a preliminary LNA, she should try to strengthen the causal inference using confirmatory SNA. Specifically, SNA "should be used to answer those questions left open by the LNA –either because there were insufficient data to asses statistical relationships or because the nature of the causal order could not be confidently inferred" (Lieberman 2005: 440). Further, SNA could be used to counter problems on "causal order, heterogeneity of cases and the quality of measurement" (Lieberman 2005: 442). Contrastingly, when preliminary LNA rejects the researcher's theoretical model, she should continue using model-building SNA. In fact, here, Lieberman presents an argument for mixing methods that is very similar to the argument presented by Barton & Lazersfeld, in combination with the argument that SNA also can be used for improving measures used in the analysis (Lieberman 2005: 443).

However, as was the case with classic arguments put forward by Barton & Lazerdsfeld, Campbell & Fiske and Denzin and others, Lieberman does not seem to address epistemological problems of social science as relevant for nested analysis. Thus, as opposed to the arguments from Brannen, Fielding and others, qualitative analysis (or SNA) and quantitative analysis (or LNA) are

not seen as being epistemologically so different that it creates problems for their integration. On the other hand, Lieberman does not forward the idea that LNA and SNA are basically similar, but specifically argues for the integration of these methods based on their "complementary distinctiveness in these two modes of analysis and strategies for causal inferences". In other words, the way in which the methods are perceived to be different is related to the basic problem addressed by the mixed method, namely that of causal inference.

I would argue, though, that Lieberman's argument (as well as the arguments from several other scholars working with causal process observations) is actually related to a central epistemological discussion within the social sciences, namely the problem of the relationship between nomothetic and idiographic elements of social science. However, given the common sense understanding of what kind of research the design is suited for (i.e. the comparative study of country cases), this problem is taken for granted and only addressed implicitly. Lieberman claims in the beginning of the article that although the article solely refers to examples taken from analyses at the country-level, nested analysis should be relevant also for analysing individual behaviors or attitudes (Lieberman 2005: 436). Simultaneously, however, he claims that more often than not LNA will be more suitable for analysing behavior, since "the prospect of explaining the exceptional nature of a particular individual is unlikely to be of intrinsic interest in the way scholars are likely to be interested in the particularities of larger social units, such as nation states" (Lieberman 2005: 436, n. 2). Thus, SNAs are focused upon analysing entities that are intrinsically interesting, i.e. they take on an individualising interest, as opposed to the generalizing interest of LNA. This, however, is not a fruitful conceptualization of small-N or qualitative analysis when focusing on explanations of behavior.

The central point of my argument can be illustrated by going back to the discussion of German philosophy in the beginning of the 20th century during the so-called *Methodenstreit* on how to understand the difference between on the one hand natural science and on the other hand social and human science (*Geisteswissenshacften*). Naturalist (such as the Austrian economist Carl von Menger) claimed the unity of scientific method and the possibility of causal explanation whereas anti-naturalist (such as the German philosophers Dilthey, Rickert and Windelband) claimed that the social and natural sciences were distinct and thus should follow different methods.

The antinaturalist argument was not unitary, though. Windelband conceputalized the difference as nomothetic vs. ideographic science, i.e. sciences aiming at the establishment of universal laws vs. sciences aiming at understanding historically particular events (Mos 1998: 42),

and Rickert further developed this understanding by pointing towards how natural and human sciences are characterized by two different ways of thinking about reality: generalizing thought and individualizing thought (Mos 1998: 51). On the other hand, Dilthey conceptualised the differences as being between *Erklären* and *Verstehen*, focusing on the different objects and the resulting different modes of understanding that pertained to the different sciences: "We *explain* nature, but we *understand* (*verstehen*) psychic life" (quoted in Makreel 1975: 134; See also Harrington 2000).

The debate on the differences within anti-naturalism is long and should not be continued here. The point I wish to make is that even today these differences in how we see and conceptualize the central epistemological problems of social science continue to exist. Thus, comparative historical researcher seem to emphasize the problem of generalizing vs. individualizing thought, or the problem of the universal vs. the particular, a problem that is inherently connected to the problem of causal inference (see for example the debate between James Mahoney (2005) and Bernhard Kittel (2005) and Ragin 1987: 3). This is also the problem implicitly addressed in nested analysis. However, interpretive scholars emphasize the problem of *Verstehen* as it has been discussed and elaborated in the traditions of hermeneutics, phenomenology, critical theory etc. (Yanow 2003). And this problem, I argue, is much more relevant when trying to understand (or explain) actions (or behaviour), i.e. when working at the level of the individual. Let me therefore elaborate on how this problem can be seen in relation to the discussion on mixed methods.

The problem of action and double hermeneutics

As I understand the problem of *Verstehen*, it has to do with the fact that within human and social science, we are faced with the challenge of making sense of what other people are doing. The British sociologist Anthony Giddens has termed this the problem of "double hermeneutics", i.e. the problem of analysing and interpreting human and social life, which in some ways are already interpreted by the actors themselves:

> Sociology, however, deals with a universe which is already constituted within frames of meaning by social actors themselves, and reinterprets these within its own theoretical schemes, mediating ordinary and technical language. (Giddens 1993: 170)

Further, researchers must face the problem of adequacy (Giddens 1993: 155-162), i.e. whether we as researchers can go beyond the understandings of actors themselves, and whether the "first-order" understandings of actors have any epistemological value within social research. In other words,

should we rely on the reasons people give for their actions, when explaining what they doing, or should we rather causally explain people's behaviour independently of what they themselves might think they are doing? (cf. also Pitkin 1972: 264-286). The first option would probably be preferred by researchers working within an interpretive framework whereas the second option probably would be preferred by researchers working within a behavioralist and/or positivist framework. Thus, the problem of *Verstehen* or double hermeneutics is also related to the problem of explaining action and the philosophical discussions of free will. As made clear by Pitkin, talking about actions within social science is inherently referring to the motives, intentions, reasons and meanings of actors (and their creativity and free will) whereas talking about behavior is related to the aim of causally explaining what can be observed (Pitkin 1972).

What is of interest here, though, is not the basic epistemological problem in itself, since this unfortunately tends to produce insolvable dualism. Rather, I seek to connect the problem with the debate on mixed methods. As Hanna Pitkin states:

> Too often the philosopher or social theorist feels it imperative to choose
> between these two perspectives, assuming that there must be a single consistent
> reality […] But it is a mistake to choose between these perspectives at all. We
> need to see at both levels, to be both hedgehogs and foxes simultaneously. In
> the same way, we need the "sociological, or political, imagination" to see
> action from both the perspective of choice and the perspective of causation.
> Only thus will we try to comprehend the nature of action. (Pitkin 1972: 286).

Thus, I will try to point out how, how the basic problem of *Erklären* and V*erstehen* and of actions and behaviour can be bridged. I first discuss this within the context of epistemology, and then, in the remaining parts of the article, I present the methodological implications for a mixed method strategy.

A pragmatic conception of Naturalism

In some ways the division between naturalists and anti-naturalists can help us understand some of the positions within the debate on mixed methods. Thus, a naturalist would typically suggest a hierarchical division of "scientific value" (cf. Barton & Lazersfeld) , whereas an anti-naturalist would be rather sceptic regarding the possibility of combining methods founded within different epistemological and ontological realms (cf. Brannen and Fielding & Fielding). However, the

discussion of naturalism has also moved beyond these traditional positions, presenting more nuanced arguments.

One such argument is presented by critical realism (Bhaskar 1978, 1979). In some of his writings, Bhaskar asks the question "to what extent can society be studied in the same way as nature?" (Bhaskar 1979: 1). Answering this, he on the one hand agrees with naturalism that science indeed has one logic and that the human and social sciences can be sciences in exactly the same way as natural science (Bhaskar 1979: 203). On the other hand, Bhaskar agrees with hermeneutics that social science deals with "a pre-interpreted reality", and therefore must be conducted differently than natural science. It is simply because of the different "object" (meaning, norms, cultures etc.), that social science must employ different methods.

Hence, for critical realism, the problem of explaining and understanding human behaviour is not an epistemological problem, but mainly an ontological problem of "sorting out" causal powers of different entities (societies, cultures and actors) (e.g. Archer 2000: 306-319), and the pre-interpreted nature of social life is something to be reckoned with in the collection and analysis of data. This, of course, implies that qualitative data and methods are an invaluable part of the social research, since they can cover ontologically different social objects. But this does not bring much to the discussion of validity of mixed methods, since the application of any method should be determined by the nature of the object in study.

Thus, if we want an epistemological position facilitating the discussion of the validity of mixed methods, we need to look elsewhere. Hence, my point of departure is a pragmatic understanding of science as the specific "construction of truth" and the reflexive understanding of the difference between this scientifically produced truth and "reality". This implies the attempt to ground science in the intersubjective search for truth and hence in rational arguments and reasoning, as well as empirical investigation utilizing different scientific tools (methods, theories, concepts etc.) (see e.g. Bernstein 1983, 1992, Dewey 1991, Habermas 1999, Bourdieu 2004 [2001]). Within this pragmatic understanding of truth, science "poses questions" to reality and investigates these question in different ways, leaving the answers always inherently constructed by the question. However, this element of construction does not mean that we must slip into relativism, since the rationality of intersubjective reasoning and scientific methods are, although no ultimate foundation, a solid enough ground from which to proceed.

Regarding the question of naturalism, then, one can say that the pragmatic position claims the same basic scientific method for all sciences, namely the posing of questions or problems and the

rational reasoning and empirical investigation of these problems. Also, this version of naturalism claims the legitimacy of posing *any* question and implementing *any* methods as long as rational arguments can be put forward. Further, against the inherent dualism of anti-naturalism*,* it is underlined that human life and nature (e.g. chemical, physical and biological entities) are one, although they may not be directly mutually translatable and therefore possibly will need different scientific perspectives and research strategies. In other words: It may be of no great value (or at least not sufficient) to discuss the poetry of Shakespeare based on an analysis of the chemical reactions in the brain of the readers, even though reading Shakespeare will indeed cause a chemical reaction in the brain. This means that the choice of research strategy can be more or less valid, and thus must be substantiated in a rational argument.

However, I will argue, when it comes to the *social* sciences, the most valid research strategy will be a mixed method strategy! But in order to substantiate this claim, we need to go one step further back to the ontological model underlying these epistemological considerations. For critical realism, the ontological model is clear: Reality consists of three layers: the empirical, the actual and the real (Bhaskar 1978). Hence, critical realists seek to understand any social event by seeking the underlying causal structures and mechanisms, whether these are social, cultural or belong to the realm of individual consciousness. The ontological model underlying the pragmatic conception of naturalism is somewhat different, though. Here, the focus is not so much on the difference between different causal mechanisms as on the difference between spheres of intersubjectivity and spheres of transsubjectivity (Benhabib 1986), life world and system (Habermas 1981), "Gemeinschaft" and "Gesellschaft" (Tönnies 1964 [1912]), or practice and social relations (Bourdieu 1990, 1998).

Thus, the social world is made up of on the one hand a sphere of 'community', interactions and intersubjective understanding. Within this life world, the basic form of social integration is normative, i.e. the coordination of actions is based on mutual understanding (esp. Habermas 1981, II: 171 ff). This is not to say that the life world is free from power and distortion. However, it points to the basic way of "living together" as human beings, sharing a culture and acting on the basis of a "natural attitude" [die natürliche Einstellung] (Schütz & Luckmann 2003).

On the other hand, the social world *also* consists of a sphere of interactions and structures not based on the social or normative coordination of action. As pointed out by Hegel, Marx, Durkheim and others, modern societies develop a division of labour, resulting in the constitution of an emergent "systemic" or "structural" level of society, presenting itself as objective. In this sphere, the logic of action and integration is systemic, i.e. freed from the need for mutual understanding.

Hence, as Habermas argues, we need to see society as on the same time system *and* life world (Habermas 1981 II: 179-180).

Summing up the argument so far, the pragmatic conception of naturalism, as well as the ontological "model" of system and life world, points towards a mixed methods strategy. Thus, the distinction between system and life world relates straightforwardly to the epistemological problems of double hermeneutics and actions vs. behavior. As noted by Benhabib (1986: 31), there is an inherent relationship between arguing that society presents an emergent level of systems or structures, going beyond (or behind) the interactions and comprehensions of actors in their daily life, and arguing that society must be analyzed from an observer's perspective, i.e. from an outside perspective explaining behaviour.

Complementarily, there is an inherent relationship between the concept of the life world and arguing that society must (also) be analyzed from a participant's perspective, i.e. from a perspective within, focusing on "mutual understanding". Conceptualizing society as both system and life world means, then, in terms of epistemology that social analysis must be done both from the outside and from within, both explaining and understanding. The same argument is made by Bourdieu:

> [T]he particularity of the social sciences requires [the researcher] to work […]
> towards constructing a scientific truth capable of integrating the observer's
> vision and the truth of the practical vision of the agent as a point of view which
> is unaware of being a point of view and is experienced in the illusion of
> absoluteness. (Bourdieu 2004/2001: 116; See also Bourdieu 2000: 188-191).

So, the ontological model of critical realism may indeed be true, and social events may indeed be results of complex causal mechanisms (including for example chemical reactions within the brain). However, if we want to fully understand social events and human behaviour/actions, we must be able to reproduce within our research both the causal mechanisms as they are accessible for the "outside observer" *and* the logic inherent in practices as they are conducted by real people and accessible from a life world perspective. In sum, the pragmatic conception of naturalism amounts to saying, that for social science, there are always "two stories" (Hollis and Schmidt 1994).

Praxeological knowledge – a mixed method strategy
As I have shown, a mixed method strategy does not have to be based on the problems of scientific progress, measurement or causal inference. It could also be based on the problem of double hermeneutic and the problem of action.

But how is this done? Moving closer to the practical aspects of designing research, let me devote some attention to the writings of Pierre Bourdieu. In his earlier writings, the double perspective of social science is made very explicit (Bourdieu 1973, Bourdieu et al 1991[1968]). Theoretical (i.e. scientific) knowledge, Bourdieu argues, has basically three forms. First, phenomenological knowledge makes the level of practice visible for the researcher, by describing or reconstructing subjective and intersubjective meanings and experiences. However, this kind of knowledge has no explanatory value, since it cannot question the presuppositions of itself. In other words, phenomenological knowledge is a "translation" of the actor's perspective into the scientists perspective.

Therefore, "structuralism" or "objectivism" can be seen as an epistemological advancement within the social sciences. Objectivist knowledge "constructs the objective relations" presenting a view of the research object (or subject) not accessible to itself.[1] Thus, a "view from the outside", made possible by statistical techniques, access to systematically collected sources of data and to points of comparison not accessible from within everyday life, can contribute with a different kind of knowledge than what is possible from taking the actors point of view. Therefore, analysis of correlations, systematic comparisons, investigations of causal mechanisms or techniques of clustering along with many other methods can contribute to the objective construction of the object.

However, although it is a necessary part of the research process, objective construction runs the risk of hypostatizing itself as "reality". Hence, presenting a "scholastic fallacy" (e.g. Bourdieu 1998, 2000) when constructing their object "objectively", researcher tend to forget that people do not act with the knowledge available to the researcher or with the theoretical models (whatever form they may have) in mind. On the contrary, people most often act in accordance with a "logic of practice", occupied with what they are doing, and considering only a small amount of possible strategies (if any). However, this does not mean that people are "cultural dopes" with no free will, i.e. that some form of structural determinism is put forward. Rather, the point is epistemological, implying that we will tend to see the determination from structures when constructing the object objectively, and see the elements of reason, creativity, and free will when taking the point of view of the actor (see also Bourdieu 2000: 170). But to gain valid knowledge, we need both perspectives!

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[1] Although Bourdieu most often uses these terms, one should not see this as a commitment to structuralism or determinism in any theoretical sense. This would be to confuse the level of sociological theory with the level of epistemology, and the arguments made by Bourdieu regarding "objective construction" is solely epistemological (Bourdieu et al. 1991 [1968]: 30).

Thus, in order to produce what Bourdieu calls "praxeological knowledge" the researcher must make a "second break"[2] and reflexively incorporate the inherent limitations of objectivist knowledge. This implies moving from the *opus operatum*, i.e. the finished structure and the regularities as they are presented in correlation models, charts of causal mechanisms, systemic descriptions etc., to the *modus operandi,* i.e. the principles of production of these regularities inherent in practice. It is important to note, though, that this second move is not a regression to the level of phenomenological knowledge. This means that it is not a question of repeating a "thick descriptions" of people's practices and posing these as a more valid description of what is going on. Rather, it is an attempt to integrate two complementary views of reality that on their own excludes each other (Bourdieu 1973: 54).

As can be seen, praxeological knowledge implies a specific logic of acquiring knowledge, and a specific ordering of the phases of research. First we have phenomenological knowledge, descriptions of the social reality and the common sense of the actors. Second, the researcher breaks with this common sense, using theories and methodological tools in the objective construction of the object. And, third, a second break is made with the objective construction, integrating the phenomenological or practical level of understanding. In the remaining parts of this paper, I discuss in more detail the different logical steps of the praxeological research strategy. Doing this, I refer to an example from my own research, a project on social differences in political participation in Denmark. Obviously, I focus here on the research strategy, leaving aside theoretical and methodological discussions (for these discussions see Harrits 2005).

Breaking with common sense

As a consequence of the problem of double hermeneutics, one of the greatest obstacles for social scientists is their inherent familiarity with their object, creating what Bourdieu calls the tendency to produce spontaneous sociology. This form of knowledge presents the illusion of immediate knowledge, although it is does not have any scientific validity, and hence it is what the social scientist must constantly struggle with and move away from (e.g. Bourdieu et al. 1991 [1968]: 13-15). This point is founded in the epistemological position of pragmatism as well as the tradition following Bachelard and French epistemology, agreeing that "the scientific fact is won, constructed and confirmed" (Bourdieu et al. 1991 [1968]: 11).

This means that the first logical step of a research process is the posing of a research question, not posed by the research object (or subject) itself, since, as Bourdieu claims, "social problems" are

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[2] The first break being the break with common sense made by the objective construction.

never "sociological problems" (Bourdieu et al. 1991 [1968]: 34). Hence, the formulation of the research problem is indeed part of the scientific process, and not, as claimed in the Popperian tradition of "context of discovery", some pre-scientific or private result of intuition, evading the demand of rational arguments. As a result, echoing in many ways what Peirce called abduction, Bourdieu suggests that the *ars inveniendi* of research should be underpinned by different methods for formulating research questions and hypothesis.

In other words, the posing of a research problem and the building of a theoretical model should be informed by empirical analysis, and here qualitative as well as quantitative model-building analyses seems helpful. Thus, for example, the analysis of everyday language and the phenomenological investigation of everyday practice is helpful in producing a systematic description of the research area of interest and further helps to control the spontaneous common sense of the researchers own practice. Further, statistical techniques (and any methods of systematic comparison, really) provide a powerful tool for breaking with common sense, since it contains the inherent contextualization (and possible generalization) of singular experiences and anecdotal evidence. And finally (and most obviously), reviewing theories and previous research gives the opportunity of finding the surprising facts, the overlooked mechanisms and the questions not previously posed. Thus, as Bourdieu states – following the logic of research within physics – "the break with traditional theories and the traditional relation with these theories is simply a particular case of the break with spontaneous sociology" (Bourdieu et al. 1991 [1968]: 28).

In my own project, formulating a research question regarding social differences in political participation resulted in several breaks with common sense. First, the common sense of democracy insists (reasonably) that all citizens are equal and that all have rights, including the right to participate in the democratic process of making political decisions. However, this democratic norm excludes the obvious fact that citizens are *not* equal when it comes to the preconditions (e.g. a specific kind of language or specific amounts of knowledge) of participating in political activities. Of course, this has been addressed, in sociological analysis as well as in philosophical and political discussions. However, these most often focus on *either* the analysis of various factors of social background and the effects on political interest or political participation, *or* on different ways of minimizing within the political system the effect of inequality, e.g. by insisting on the (formal) equal opportunities for access.

Thus, the initial break of the research question presented a break with the one-sidedness of focusing on *either* social resources *or* the political system, insisting in stead on the homologies of

social positions and positions of political resources and practices. Further, the use of concepts such as social space, political field and capital made it possible to evade the linear conception of resources, and instead present a two-dimensional understanding of social differences as well as political practices. And finally, the insistence on focusing on political practices and the underlying practical relationship and conception of politics broke with the tendency of research as well as that of professional politicians to see political practices as solely rational and conscious *decisions* clearly demarcated as being *political*, in stead of looking at the possibility that for some people, politics is often thought of (and acted upon) in non-political terms.

Objective construction

In terms of logical steps of the research process, following the posing of a research question is the objective construction of the object, i.e. the construction of an explanation from an observer's perspective answering the research question. In practice, though, the objective construction and the posing of the research question are inherently related, since the gradual construction of the object, theoretically and empirically, also constitutes a further break with common sense. However, also involved in the objective construction of the object is the insistence on continuously confirming or rejecting the construction made.

Put simple, then, this is what would "normally" be seen as the research process, namely the proposing of hypotheses derived from theoretical propositions and the methodologically adequate testing of these hypotheses by relevant and systematically gathered empirical data. However, Bourdieu insists that following some cookbook recipes cannot take away the demand for "epistemological vigilance" throughout the entire research process. This means that simply proposing hypothesis and testing them in conventional ways will not do.

First of all, objectively constructing an object requires a systematic endeavour to theoretically sort out different concepts and their relation to each other, i.e. the development of a theoretical model. This is so, since only a thoroughly constructed theoretical model can "construct the system of facts among which it sets up systematic relationship" and thus present the fullest "exposure" of theory to the empirical data (Bourdieu et al. 1991 [1968]: 63). So, there is an intrinsic relationship between theory, methods and empirical data, and during the research process this going back and forth between theory and data must be done continuously. Again, this understanding of theory and data relates to the understanding within physics:

An experiment is nothing other than a question addressed to nature;
measurement the record of the answer. But before performing the experiment,
one has to think through, i.e. formulate the question to be put to nature; and
before drawing any conclusions from the measurement, one has to interpret it,
i.e. understand nature's answer. These two tasks are for the theoretician." (Max
Planck 1949, here quoted in Bourdieu et al. 1991 [1968]: 60)

Second, the theoretical model must be tested using appropriate methods designed according to the logic of the theoretical model. Thus, almost any methods contain their own epistemological presuppositions, and we need to be sure that we do not import any "unconscious models of reality" by using the traditional methods or the methods that we have been trained to use. For example, doing multivariate analysis involves presupposing linear causal relationships (if not specifically modelled otherwise) as well as variables with a continuous or linear distribution. So, if the theoretical model presupposes concepts with a two-dimensional structure, one needs to apply statistical techniques suited for testing this model, such as correspondence analysis, multidimensional scaling or latent class analysis.

Third, the same can be said for the use of data. Hence, every collection of data is suited for a specific problem, and as such it must always be thought through whether such data can be redirected towards answering a different problem. One obvious obstacle is survey questions not posed, resulting in the required analyses being impossible to conduct. However, the phrasing of a question, the sampling procedure, the categorization of a variable, or the methods of collecting data can also pose specific problems.

In sum, this means that data "just being there" or methods providing a sophisticated statistical test are only good arguments for applying these data or these methods *given* that they are in accordance with the theoretical model and the basic research question. And, further, data of a lesser quality or methods not as sophisticated may be applied with good reasons, when these are more in line with the way in which the object is constructed theoretically. So, the application of *any* methods will need an argument in relation to the theoretical model, since *no* methods can be seen as epistemologically superior per se.

In this phase of my own project on social differences in political participation, I constructed a theoretical model, connecting the social space and the space of political practices. This involved the proposition of hypotheses regarding the empirical constitution of each of these objects as well as their relationship, which was seen as homologous. More specifically, I put forward the hypothesis

of the social space being structured by economic and cultural capital, with the two basic dimensions being volume of capital and composition of capital. And, further, I put forward the hypothesis of the political field being structured homologous (as opposed to autonomous), excepting to find the same basic dimensions of volume and composition of capital and practices as within the social space.

I then faced the challenge of how to empirically test such a two-dimensional theoretical model. This was done utilizing the technique of correspondence analysis (see e.g. Hjelbrekke 1999, and Blasius 2001), where different maps can be constructed, both testing the distribution and clustering of a set of variables, and the distribution and clustering of a set of different variables given the configuration of the first. This gave me the opportunity to test the distribution of capital within the social space, the distribution of political capital and practices within the political space as well as to compare these distributions and test hypothesis of homology. In this research project I did actually use secondary survey data, collected in connection with different research projects on political participation and citizenship. Although the research questions of these projects involved presuppositions somewhat different from mine, the data did contain enough variables phrased in a reasonable way as to be applied for my purpose. Furthermore, I conducted the analyses separately on two different sets of survey data.

The analysis was conducted in several steps, moving from theory to data and vice versa. This meant that different sets of variables were tested to find out which were better suited for the empirical construction of the correspondence analyses. Further, it led to some surprising results and revisions of the model. For example, the distribution of democratic values was found to be not at all structured in the same way as practices or political capital, since these values show an enormous degree of consensus. This helped to make explicit a hypothesis regarding the democratic doxa, i.e. the silent presuppositions and "rules of the game" underlying the political field and shared by all participants.

Bringing the life world back in

As mentioned above, the first and especially the second logical step of the research process is somewhat similar to a "normal" research process, i.e. posing a research question, constructing a theoretical model, and testing this model against empirical data using appropriate methods. However, moving to the third step of analysis, I present what can be seen as the central point of this paper, namely the integrated combination "explaining" and "understanding".

The point of making a "second break" with the objectivist construction of the object is, then, to recognize the fact that this construction is not "reality", i.e. to recognize the difference between

the model constructed from the outside and the practices as they are conducted and lived by real people. But how is this second break carried out? Reading Bourdieu, two different strategies stand out. The first is mainly at the level of theoretical reflection, namely the effort of the scientist to contextualize her own position as a scientist, involving the recognition of the specific presuppositions involved in her research problem, theoretical model and methodological approach. Basically, this means to reflect upon what it means to be a scientist trained within a specific discipline, theoretical tradition, country etc., and to recognize the results as dependent on these different aspects.

However, I wish to draw attention to the second strategy also presented within the writings of Bourdieu. This strategy can be seen as the recognition of being a scientist per se, i.e. situated within the realm of scientific observation and explanation opposed to everyday practice, and hence the recognition of the need to bring back the life world perspective. This implies the recognition of the fact that in order for our theoretical model to be true, there must be some practices conducted to produce the regularities that we can observe with our theoretical and empirically confirmed model. Thus, as Bourdieu puts it: "Systematicity is found in the *opus operatum* because it is in the *modus operandi*" (Bourdieu 1984: 173).

The third logical step of the research process, then, involves the explicit investigation of the *modus operandi*, i.e. the logic of practices that produce the systematic patterns in actions and events that we observe. At the level of sociological theory,[3] Bourdieu's concept of habitus provides this exact function. Thus, habitus is the translation of structures into dispositions (cognitive, normative, aesthetic and bodily schemes of perception and taste), transforming the "determinations" of structural constraint to the willed actions of the person. So, the habitus presents a generative formula, colouring every practice of the actor. However, more than presenting a causal model of mechanisms (i.e. an ontological model in line with critical realism), the concept of habitus points toward the epistemological double perspective needed to be taken in order to explain *and* understand the social practices of human beings. Thus, as Weininger points out:

> This side of the explanatory project [i.e. the analysis of habitus, GSH] is
> intrinsically *verstehend*; the semi-logical "unity" that connects an array of
> different practices can only be made evident semantically, through the

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[3] By this I mean to underline the fact that the epistemological break with the objectivist construction of the object also can be carried out using other concepts.

> apprehension of a singular "principle" (or "practical philosophy") from which
> they could derive their coherence. (Weininger 2002: 73)

In other words: In order to understand the mechanisms responsible for the systematic patterns in our objectively constructed model, we must incorporate the phenomenological knowledge of practice, and conduct an interpretive analysis of these practices and their logic. Thus, we must combine the objectivist and the life-world-perspective, i.e. the "two stories" of the social world, producing, in the end, a more valid knowledge on what is going on.

Carrying out this second break within my research project on social class differences of political practices, I designed a qualitative study of political practice and habitus. This study was explicitly related to the objective model, which ended up by showing a homology of social positions and political practice. Hence, the goal of the qualitative study was to investigate the practices behind or responsible for this homology. To accomplish this, a theoretical sampling of interviewees in nine different social positions was made, and the interviews were conducted in a semi-structured form, facilitating a focus on the practical understandings of politics, the political practices normally engaged in by the interviewees, and the ways in which the interviewees themselves interpreted these practices (see e.g. Kvale 2003).

With these qualitative data, I was first of all able to show the meaningful practices, i.e. the modus operandi, underpinning the regularities of the model. Here, for example, it became evident how people holding many resources (economic as well as cultural) have an natural attitude towards political practices, easily understanding political language and problems, and seeing themselves as legitimate political actors. This was in sharp opposition to the attitude of people with few resources, which saw politics as a very complicated and "strange" world, and an area of activity definitely not relevant for them to participate in. Further, it was shown how only people in high social positions conceives of and conforms to the specific "rules of politics", whereas others typically makes political judgements based upon a moral or emotional logic. Finally, the interpretative analysis was able to show, how all citizens reproduced a "democratic doxa" of political participation as being matter of choice. Thus, for example, people who were clearly excluded and estranged from the field of politics, most often legitimated this exclusion by saying, that politics was not really of interest to them.

All in all, the interpretative analysis did indeed provide an analysis of the modus operandi, and as such it added "flesh and blood" to the causal mechanisms inherent in the theoretical and empirical model resulting from the statistical analyses. In other research projects this is sometimes

accomplished by anecdotal evidence and stories from the scientist's own practices, but the strategy of praxeological knowledge has the advantage of avoiding the obvious problems of validity connected to such a re-integration of non-systematic common sense knowledge. Further, the strategy of praxeological knowledge enhances the validity of the knowledge produced, since it provides a solution to the problem double hermeneutics. In stead of assuming the truth of either the perspective of the researcher or the actors, the purposive combination of a view from without and a view from within makes these perspectives and their relation empirically evident.

Finally, the combination of explaining and understanding helps to avoid the validity problem of posing a scholastic fallacy , i.e. of projecting a theoretical model as reality. Thus, in my own research project it became evident how the social differences in political participation were not mechanically determined by differences in resources or interests, but rather were translated into different conceptions of the political, different self-images vis-à-vis the political and different modes of acting politically. This amounts to saying that the strategy of praxeological knowledge enhances the validity of the theoretical model, because it makes sure that the mechanisms underpinning the causal explanations are not only empirically corroborated but also in accordance with the "logic of practice" existing within the everyday lives of the actors in study.

However, we have not touched upon one problem of combining research strategies, namely the problem mentioned above, that the results of the interpretative analysis may not comply with our objectively constructed model. Far from being a problem of validity, though, this may in fact be an aspect of reality. Hence, as Bourdieu points out, the practical misrecognition of structural regularities (e.g. inequality of opportunity) may exactly be the precondition for the functioning of practice. So, of course, conflicting evidence can be a sign that the theoretical model is false, or that some of the data and analyses are invalid. However, this is not always so. Unfortunately, though, the only way to deal with this problem is meticulous investigation, theoretical rigour and rational argumentation.

In the research project on social differences, the combination of explanatory and interpretative analysis presented exactly such an example. As mentioned above, none of the practices or understandings of any of the social classes recognized the social differences in political participation, since all interviewees seemed to agree that political participation was a matter of choice. Thus, this was opposed to the results of the explanatory statistical analysis, which clearly demonstrated social differences in political participation. However, the conclusion of the project was that this should not be seen as a problem of validity of the theoretical model or the data, since it

most likely points to the social phenomenon of misrecognition and the workings of the democratic doxa. In other words, inherent in the different social practices of politics is a conception that we act rationally and freely when action politically. And the misrecognition of the obvious social differences thus functions as a precondition for the continuance of politics!
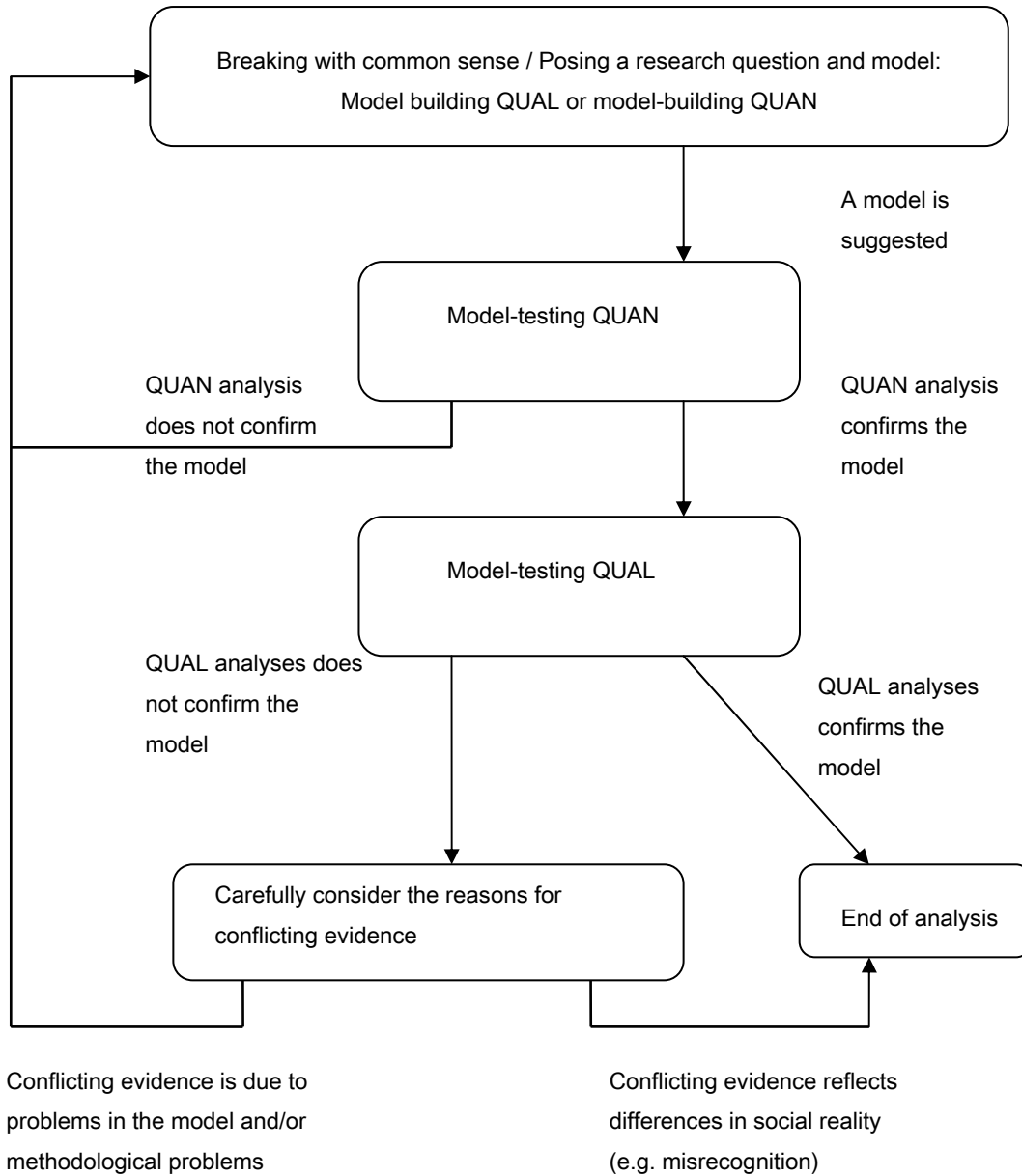
Conclusion: Expanding our reasons for doing mixed method research within political science

Throughout the paper, I have argued that different mixed method strategies are framed in terms of different methodological or epistemological problems. Further, I have tried to show how the dominant conception of mixed methods within political science addresses only the specific problem of causal inference, and hence presents a rather narrow understanding of why and how to use mixed methods. In response to this one-sidedness, I have suggested that the problems of double-hermeneutics and action vs. behavior are more relevant when analysing political practices at the level of the individual. Finally, I have shown how pragmatic conception of naturalism as well as the ontological model of system and lifeworld can provide the metatheoretical foundation for a mixed method strategy producing what Bourdieu class praxeological knowledge. Figure 1 summarizes the argument.

As can be seen from the model – as well as the arguments presented above – the mixed method strategy of praxeological knowledge implies a specific ordering of the different methods. Thus, after the initial model building (using either qualitative or quantitative analysis), the strategy implies the initial use of quantitative analysis followed by qualitative analysis. Further, the model makes clear, that when conflicting evidence is produced by the two methods, it should be carefully considered if this is due to problems in the model or the analysis, or if this perhaps is an interesting result in itself pointing towards specific mechanisms in social reality.

The observant reader will have noticed that I have not touched upon relevant problems related to design and methods. This relates to the fact that questions on methods and design, such as the selection of cases, has been thoroughly covered elsewhere. As described by e.g. Lieberman (2005) and Gerring (2007), there are different ways of selecting cases both randomly and theoretically or purposefully, some more suited for exploratory or model-building analysis and some more suited for model-testing analysis. Similarly, there are different ways of collecting for example qualitative data such as participants observation, everyday language interviews or archive studies, and each of these methods will have advantages and disadvantages with regard to one's research question. Hence, there is nothing in relation to the mixed methods strategy of praxeological knowledge that suggests a specials logic for example for selecting cases or for collecting data.

Figure 1: The mixed method strategy of praxeological knowledge

Breaking with common sense / Posing a research question and model:
Model building QUAL or model-building QUAN

A model is suggested

Model-testing QUAN

QUAN analysis does not confirm the model

QUAN analysis confirms the model

Model-testing QUAL

QUAL analyses does not confirm the model

QUAL analyses confirms the model

Carefully consider the reasons for conflicting evidence

End of analysis

Conflicting evidence is due to problems in the model and/or methodological problems

Conflicting evidence reflects differences in social reality (e.g. misrecognition)

I would like to make one final point, though, namely that my argument is somewhat similar to the concept of "ecological validity" as suggested by Bryman (2004). Ecological validity is "concerned with the question of whether social scientific findings are applicable to people's everyday, natural social setting" (Bryman 2004: 29). This definition, however, poses the problem that judgement regarding this criterion seems to be made outside the scientific realm and not necessarily on rational grounds, since the question of "applicability to everyday, natural social settings" supposedly could be made also on emotional or strategic grounds. Based on the arguments put forward in this paper, then, I suggest a slightly different definition of ecological validity, namely that it concerns the question of reflexively handling the tension between scientific knowledge and everyday knowledge. This definition on the one hand maintains an insistence on relating scientifically produced knowledge to people's everyday lives as well as on taking seriously the rationality inherent in everyday knowledge on the logic of practice. However, the definition on the other hand avoids the idealization of everyday practice and rationality as the primary evaluation of scientific knowledge, and it sustains that scientific rationality is also an inescapable ground from where to judge the validity of our research.

As I have tried to show, this insistence on relating an objective and intersubjective understanding of social practices is highly relevant for political science. In my own research project it provided valuable insight on how to understand patterns of political participation. But the mixed method strategy for praxeological knowledge would probably be relevant in many political science projects, such as voting behaviour, the formation of political attitudes or the practices of public employees and street-level bureaucrats, just to mention a few examples. And surely, other strategies for mixed methods will develop, answering other methodological and epistemological problems of social and political science, adding to the plurality of our research.

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