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CHALLENGES TO GROUNDED THEORY
The specificity of grounded theory as a social science theory

The following two schemes are drawn from a comparison of the various meanings given to the term “theory” in social science (Mjøset 2001, revised Mjøset 2004). Table 1 challenges the persistent dualism invoked in discussions on the philosophy of the social sciences. A distinction between three practical philosophies of social science — common attitudes (habits of thought) among groups of researchers — are offered as a more productive alternative. Very briefly

Any scholar who has done social research for some time knows that a large group of social scientists conduct research and legitimate what they are doing in ways that refer to routines similar to those employed in various natural sciences (experimental designs, mathematical modelling techniques, etc.). We also know that there is another group of scholars who proceed in ways that remind us of work in the humanities (interpreting texts, reconstructing culturally significant events, reflecting on the existential challenges of our time, etc.). I do not mean that they rigidly copy these other fields of science, but that their inspiration is mainly drawn from models and philosophies of science relating to these fields, and they communicate well with the two other camps respectively. But let us think of a third group, one which is not usually distinguished. This group simply behave as social scientists, with no “external” inspiration. This is the third category that I propose, simply because I have found it highly rewarding to distinguish two different groups within what large and heterogeneous cluster of scholars that conventionally — but not very precisely — have been labeled as “anti-positivists”.

Table 1. Six notions of theory

<table>
<thead>
<tr>
<th>Practical philosophy of social science</th>
<th>Standard dualism</th>
<th>“Positivism”</th>
<th>Critique of positivism</th>
</tr>
</thead>
<tbody>
<tr>
<td>Model of science</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Notion of theory</td>
<td>Middle range theory</td>
<td>Idealizing</td>
<td>Explana- based</td>
</tr>
<tr>
<td>Main empirical procedures</td>
<td>Variables oriented, cases treated in variables-terms</td>
<td>Cases, mainly treated as illustrations</td>
<td>Cases as the explanatory basis</td>
</tr>
<tr>
<td></td>
<td>Social s.</td>
<td>Social-philosophical</td>
<td>Transcendental</td>
</tr>
<tr>
<td></td>
<td>Humanities</td>
<td></td>
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</tbody>
</table>

Table 1 further shows how there are two notions of theory involved by each attitude. I provide a detailed comparison in Mjøset 2004 and some of the nuances will become evident in the discussion below. Table 2 provides a basic overview with reference to levels of analysis (the distinction between low, middle and high levels of theory referring to the case level at the most concrete (“low”) end of a continuum that has decontextualized general theory at the other end) as well as to the basic gap between theory and empirical research (assuming that
low and middle level theory will be substantive, while at the high level, theory is non-empirical).

**Table 2. Notions of theory and levels of analysis**

<table>
<thead>
<tr>
<th>Attitude Notion of theory</th>
<th>Standard Theory oriented</th>
<th>Idealizing Idealia- oriented</th>
<th>Pragmatist Explanation-based</th>
<th>Critical Critical theory oriented</th>
<th>Social-philosophical Transcendental</th>
<th>Deconstruction Deconstructionist</th>
</tr>
</thead>
<tbody>
<tr>
<td>High level</td>
<td>X</td>
<td></td>
<td>X</td>
<td>•</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Middle level</td>
<td>X•</td>
<td>X</td>
<td>X•</td>
<td>•</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Low level</td>
<td>X•</td>
<td>X•</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
</tr>
</tbody>
</table>

*Note:* X marks the main level which the notion of theory relates to, • marks an ambition to move upwards to the next level, • marks an ambition to move downwards.

With reference to this framework, the programme of discovering grounded theory can be specified as an example of an explanation-based type of theory: Grounded theory, we see from Table 2, is less ambitious (or heroic) than high-level notions of theory such as the idealizing models of rational choice theory, and the search for “general presuppositions” among social philosophers.

Grounded theory generates knowledge at the middle or low level, which means that it applies within more or less broadly specified contexts. But it differs from notions of “middle range theory” by not striving to move towards the high level. It is thus a programme of gaining as general knowledge as necessary (given the research problem), without loosing context.

Referring to the very lowest level, grounded theory is a more ambitious notion than that explored by deconstructionists, who take sociology of knowledge skepticism to its extreme by doubting any knowledge beyond the level of specific cases.

What then, distinguishes grounded theory from critical theory? Both can be understood as expressions of a pragmatist or participatory attitude (or practical philosophy of social science), one that relies solely on the experience of doing social research. From Table 1 we see that in grounded theory, theory is buildt from any kind of case as an explanatory basis. Critical theory relates to a specific set of cases, that is “contemporary cases of legitimate claims for social change”. The notion of critical theory is here closely connected to social movements whose claims for change are legitimate in a way that puts the social scientist in a cross-pressure situation. While a researcher discovering grounded theory will normally remain within the relative autonomy of the social science research collective, a critical theorist may — in the extreme case — be drawn out of that community, becoming some kind of “organic intellectual” of the movement involved. There are many examples of intermediate positions: Social science scholars working in NGOs, psychologists or social workers relating to individual clients...)

Based on this framework, this paper adresses a number of challenges facing the programme of discovering grounded theory. In a sense, the question here posed is whether grounded theory can be defended with reference to state of the art philosophy of science. I shall concentrate on three kinds of criticism waged against that programme.
In this version of the paper (which is shorter than the one I had planned, I deal with two types of criticism: one from the vantage point of the standard attitude, another one which combines social philosophical and pragmatist impulses.

A standard criticism of grounded theory

John Goldthorpe’s criticism of grounded theory comes as a side effect of his attempt to defend his preferred synthesis of variables-oriented empirical studies and rational choice theory. The synthesis is legitimated with reference to a set of ideals drawn from the standard practical philosophy of social science.

First, his idea of testing of a theory stems from the experimental ideal, codified in the nomological-deductive (or covering law) model of explanation. Goldthorpe prefers not to use this notion, but talks about “one logic of inference” (see the specification of the law-oriented notion of theory in Mjøset 2004).

Second, the empirical basis of social research stems from large-scale data-sets, data being samples from larger populations, analysed by means of variables-based/probabilistic models.

Third, his notion of theory is the idealizing one, as found in rational choice theory (Goldthorpe prefers the term “rational action theory”).

His criticism of grounded theory mainly relies on the two first ideas. In the postwar history of the social sciences, these two ideas yielded the law-oriented notion of theory. This is an empirical approach to theory: there exists a more or less elaborated set of findings that have withstood attempts at falsification. Within various empirical fields, researchers develop implications of these theories. This yields hypotheses that are tested as the researcher collects material that is appropriate for such testing. Appropriate means that indicators are based on valid measures that are also reliable (successive measures are strongly correlated).

The criticism of grounded theory, then, is as follows:

— the programme of grounded theory allows successive modifications of the hypotheses formulated at the start of the process of empirical research. Goldthorpe — like many others — attacks the “extreme inductivism” and the “adhocery” of grounded theory.

— grounded theory, as related schools such as symbolic interactionism and ethnomethodology, do not rely on indicators. Instead (as in Blumer’s formulation about “testing concepts”), researchers engage in excessive conceptualization, defending this as “sensitivity to context”. Goldthorpe claims that this makes convergent conceptualization impossible.

— as a result of these two preferences, grounded theory escapes the testing of theory. There is no clear distinction between context (situated action) and regularities.

1 Goldthorpe 2000 is the main reference. I here only give the main points. In a revised version of this paper I will provide a more detailed discussion with specified page-references.
However, Goldthorpe’s notion of theory is not the law-oriented one, it is, as his third point shows, the idealized notion of theory. At one point in his work on social mobility, Goldthorpe realised that the statistical methods themselves could never yield explanatory theories about the processes he was interested in. In research on social mobility, Blau & Duncan (1967) was a pioneering work, incarnating the programme of moving from correlations to causes by means of sophisticated probabilistic models such as those developed by Simon, Wold and Blalock.

But at one point in the late 1980s or early 1990s, it was discovered that this programme failed (Abbott 2002, Ch. 3; Goldthorpe 2000, Ch. 7). Among researchers holding standard views, the notion of theory was reoriented with neoclassical economics as the main model. Although Goldthorpe emphasizes that he is skeptical of the full-fledged neoclassical thinking (the kind of “economics imperialism” promoted by scholars such as Gary Becker), the way he legitimates his move is not in principle different from what Kydland and Prescott (1996) argues when they favour the “computational experiment”.

The main idea is this one: Variables-based statistical methods do not yield causal explanations, but they yield “sophisticated descriptions”. Theory is developed as a thought experiment. A number of stylized assumptions about context and motives are made, and deductive chains yield a simulation of the empirical phenomenon selected for explanation. Kydland and Prescott emphasizes that this differs from the earlier approach in econometrics, where parameter values would be estimated from empirical data to yield what was claimed by be theoretical explanations.

In Goldthorpe’s discussions about theory and method, this is formulated as a polarizing distinction between “theory and history”, with history as the “theoretically unexplained explainer” (1997: 129). He notes that a historical narrative inherently is specific in time and place, contrasting this with “the applicability across time and place that is required of theory (even if subject to some delimitation of scope).” (1997: 128). This sentence is highly ambivalent, since it both requires theory to be applicable across time and place, but at the same time requires “some delimitation” of scope. This is where the notion of the standard practical philosophy of social science is useful: it is in Goldthorpe’s scientific habitus to move upwards, to go for the very general relations that are applicable across time and place.

If he was to start to thinking about how to delimit the scope of his theory, he would violate his basic polarization of history and sociology. Grounded theory, in contrast, does not aim to generalize to a broader population. The aim is to develop “representative concepts”, and as Corbin & Strauss (1990: 421) writes: “ultimately to build a theoretical explanation by specifying phenomena, in terms of the conditions that give rise to them, how they are expressed through action/interaction, the consequences that result and the variation of these”.

Generalization takes place through the research process, and it is required that grounded theory at the same time “specifies the conditions under which a phenomenon has been found in this particular data”. The use of knowledge, based on the process of learning, is not simply a subsumption of cases, it is a process of contrasting, discovering the extent to which new situations can be understood through earlier knowledge. “A grounded theory is reproducible in the limited sense that it is verifiable. One could take the propositions that are made explicit or left implicit (whatever the case may be) and test them. However, probably, no theory that deals with a social psychological phenomenon is actually reproducible insofar as finding new situations or other situations whose conditions exactly match those of the original study, though many major conditions may be similar. Unlike a physical phenomenon, it is very
difficult to set up experimental or other designs in which one can recreate all of the original conditions and control all the extraneous variables that may impinge upon the phenomenon under investigation. When testing hypotheses derived from the propositions of a grounded theory, the investigator would have to specify the conditions under which the hypothesis(es) was being tested and make adjustment in the theory to fit those conditions, (if they did not match those originally specified in the theory)” (Corbin & Strauss 1990: 424).

Grounded theory is a way to work towards somewhat broader range without loosing the link to contextual explanations. Goldthorpe, in contrast, polarizes theory and context. He has taken great care to specify this dualism. The problem with singular statements is not to explain by means of them, he claims, but to see them as something that sociology should explain! Thus, for macrosociology, unique or contingent historical phenomena may very well play important roles as explanatory factors, and it is often so also in his own work on social mobility, he states (1997: 129). But they should never be the explananda of sociology.

Let us bracket the latter statement, which is a part of Goldthorpes attempt to distance himself from recent broad trends in comparative macrosociology. If he means that statement as a norm, it is certainly clear that many sociologists have violated it. (Cf. section on implications below, rejecting the nomological/ideographic-dualism.)

By putting his faith in rational choice theory, Goldthorpe seemingly devalues all his empirical work into mere description. Goldthorpe admits to explain with reference to history, historical particulars may enter into the explanation as contextual factors. But at the same time, he emphasizes that history is about time/space-specific events, and demands that this be strictly separated from theory. So how can there be an empirical imput to Goldthorpe’s theory?

The answer is this: Although Goldthorpe has put his bet on the idealizing notion of theory, an element of the law-oriented notion of theory remains with him: The sophisticated descriptions provided by advanced variables-oriented statistics for some reasons have priority. He argues that since these findings reappear in a number of different contexts, they require a general explanation, thus defending his view of theory being “not sensitive to context”.

Rather than a context/no-context-dualism, the question is here about a dense or a crude delimitation of context. In Goldthorpe’s favourite example, this explanation is in terms of a rational choice model implying the very crude contextualization of a working class and service class, where differential school/work-preferences (by parents/youngsters) explains the constancy of a class bias in educational attainment. This is a paradox: Goldthorpe has worked

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2 Referring to his work with Erikson, *The Constant Flux*, Goldthorpe writes: “Perhaps the most notable finding of this work was that when intergenerational class mobility was considered net of all structural influences — or, that is, as ‘social fluidity’ — rates and patterns showed high stability over time within nations and, further, a large measure of similarity across nations. Such a degree of invariance clearly underlines the need for general theory. For hypotheses on the causal processes capable of producing temporal constancy and cross-national commonality of the kind that our quantitative analysis revealed will have to be derived from a theory of considerable scope: that is, from a theory which is precisely *not* ‘sensitive to context’ — unlike the theories of national ‘exceptionalism’ in regard to mobility that our results called into doubt — but applicable to societal contexts widely separated over both time and space. And in this respect, I should say, Erikson and I were able to make only a very modest beginning.” (Goldthorpe 2000: 62).
for most of his life on class structure and social mobility, but in his writings from the 1990s, he actually admits that he has not come far in terms of theoretical explanation.

Empirical work comes first, but no theory is to be gained “from below”. Theoretical modelling explains afterwards. Now is that testing? Goldthorpe has a choice of several empirical findings that he may try to explain across time and space. He does not discuss the criteria for his choice (problem-orientation? participation?) Similarly, there is probably a number of formal models that may simulate the constant relationship he is eager to explain. If he had more than one successful model, which one would he chose?

It even seems that he tries to deflect attention from problems connected to his own rational choice explanation by attacking grounded theory. But we have indicated that grounded theory represents a non-heroic pursuit of contextual generalization, while Goldthorpe — with his polarization context plus explanation versus theory — seems to have some problems, involving both empirical description and testing of theory.

Goldthorpe’s dilemma seems to be the following: he admits that a theory must have “some delimitation of scope”. But he has chosen an empirical regularity, produced by complicated statistical modelling, and he has chosen to explain it by a highly decontextualized theory. His standard “style of research” implies that contextualization is not an art he needs to care about, at least not when developing theory. Whatever contextual explanations of particular cases and periods of social mobility that he has himself presented (using history as “Unexplained explainer”), have no theoretical implication to him. But since some kind of contextualization is necessary, he is not able to explain until he has provided some kind of context. A decontextualized theory is hard to test, since testing requires a specification of contests: theories are only competing if they relate to the same context.

Goldthorpe defends his approach as a productive alternative to research that tries to explain with reference to national exceptionalism. It is not hard to agree that claims to exceptionalism based on studies just of one country are not very productive. But the real challenge to Goldthorpe’s programme is a systematic comparative study. As for Goldthorpe’s own topics, it would seem that the programme of studying “varieties of capitalism” might be one promising start, but it is only in its infancy.

A more worked out proof of a viable alternative is Stein Rokkan’s contribution to political sociology, which can clearly be presented as grounded theory. Here the population of

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3 While it is surely disappointing, it should not be thought a disgrace for sociologists to admit that they have not been able to develop a theory that will adequately account for their empirical findings. This is so because one cannot expect effective theory, in the sense I intend, to be produced at will, nor by following specified procedures or guidelines — as would appear the case with ‘grounded theory’. I do not find it accidental that it is in case-oriented, qualitative sociology that the rather absurd use of ‘theorize’ as a transitive verb has become most common: i.e. it can, apparently, be demanded that a topic be ‘theorized’ in the same way as it can be demanded that the kitchen be cleaned or the shopping brought home. This confirms me in my belief that, typically, no more than (re)conceptualization is in fact involved.” Goldthorpe 2000: 62.

4 This is the interpretation in Mjøset 2000. In Park & Burgess’ Chicago-school textbook, *Introduction to the Science of Sociology*, 1921, they wrote: “As soon as historians seek to take events out of their historical setting, that is to say, out of their time and space relations, in order to compare them and classify them; as soon as historians begin to emphasize the typical
and representative rather than the unique character of events, history ceases to be history and becomes sociology”. The standard interpretation has always been that sociology generalizes towards law-like regularities, but Rokkan’s work shows that there are other ways to establish general theory: “compare” and “classify” are the crucial statements here: we return to this towards the end of the paper.
Western European political regimes is stratified into typologies, and there is a historical sensitivity to institutions and institutional complementarities. Rokkan’s many typological maps reflect his efforts to generalize without loosing context, whereby he was able to proceed quite far in terms of explaining European varieties of political modernization. Unlike the social philosophers, who take the existential situation of modernity as their starting point, Rokkan decomposes varieties of modern political transformations.

Rokkan used both qualitative and quantitative sources, but avoided Goldthorpe’s strange implication that certain variables-oriented methods should give more sophisticated descriptions than other (qualitative or quantitative) methods. Specific regularities had no privileged status in terms influencing the research problem. In fact the research problem was given more or less before the empirical work: what explains the variety of political systems in Western Europe.

Similar examples of viable alternatives would be the work of Esping-Andersen and Korpi on welfare states and comparative political economy, both of which are much better accounted for by a pragmatist attitude leading to explanation-based theory.

A social-philosophical, critical theory-based criticism of grounded theory

Goldthorpe’s criticism of grounded theory is basically that it does not live up to the ideals of the standard attitude. Burawoy’s criticism is the exact opposite: grounded theory is rejected as positivist and non-reflexive.

Burawoy’s criticism combines elements of the social-philosophical and the pragmatist attitude.

The social philosophical element is the idea that basic social science theory must be based on a set of pre-empirical (transcendental) notions about action, structure and knowledge. Most social philosophers (take Habermas, Giddens or Luhmann) relate their (ideosyncratic) set of notions to the broad historical context of modernity, with particular attention to how this context forms patterns of knowledge and perception. Burawoy pursues a Marxist interpretation in which the underlying social structures (“the anatomy of capitalism”), relations of production, is what determines knowledge (this is a “critical realism” for which Bourdieu was a sophisticated spokesman).

The pragmatist element stems from Burawoy’s preference for ethnographic and anthropological methods of participant observation, which he interprets in line with the critical theory notion in Table 1 above.

Burawoy (1991, Ch. 13) discusses four approaches to the development of “theory out of data collected through participant observation”. He classifies them by means of the two major challenges that are always pointed out in standard criticisms of case-studies: (1) the problem of the significance of a study of singular cases, (2) the level of analysis, macro or micro. This yields the following simple scheme:
In a later paper (1998), Burawoy distinguished two models of science. The positive one is what we have termed the **standard** attitude, while the reflexive one is his preferred synthesis of the social philosophical and pragmatist attitudes. This enabled him to suggest another classification of grounded theory, with reference to two different methods of empirical research: interview versus participant observation:

<table>
<thead>
<tr>
<th>Micro</th>
<th>Particular</th>
<th>General</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ethnomethodology</td>
<td>Grounded theory</td>
</tr>
<tr>
<td>Macro</td>
<td>Extended case method</td>
<td>Interpretive case method</td>
</tr>
</tbody>
</table>

Although, as the first scheme shows, grounded theory joins the extended case method in being able to analyse micro/macro-interaction, it is criticized as a standard case: he claims grounded theory strives “toward greater generality: that is, the inclusion of more phenomena under a single covering law” (1991: 8). It is an approach to the analysis of material from participant observation, but it is not reflexive. Let us challenge that claim.

A closer specification leads him to two objections (see 1991: 303, fn 5):

1. That grounded theory, with its preference for discovery of new theory, does not reconstruct theory,
2. and that grounded theory cannot “reach out”, its case-studies are “unextended”. — Both objections must be rejected:

Re (1): Despite his critical stance towards the “positive model of science”, Burawoy explicitly states that his notion of theory fits with the Popper/Lakatos-tradition. But he emphasizes the “act of falsification”, whereby the theory that was the starting point is “reconstructed” to account for a particular case that proved puzzling in the light of the original theory. But this attention to the particularities of cases is not in principle different from the discovery of grounded theory. Burawoy himself (1991: 303, fn) is well aware that Strauss (1987: 13 f, 306 ff) did not imply that theory should be discovered from scratch in every new field work, but required that the earlier theory one should relate to should in itself be grounded.

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5 “The adoption of our approach to theory reconstruction does not imply the use of the extended case method. It is quite possible to reconstruct theory on the basis of discovered anomalies without extending out, instead confining oneself to the structure and dynamics of the micro situation. On the other hand the extended case method, since it depends on constructing social phenomena in their particularity, relies on existing theory that highlights the situation as anomalous. To put it another way, the extended case method cannot be pursued by applying the principles of grounded theory. Discovering theory through generalization across social situations necessarily abstracts from time and space and brackets the determining macro context. Whereas the extended case method identifies a particular phenomenon as the product of historically specific causes, grounded theory systematically removes particularity” (Burawoy 1991: 303, n. 5).

6 For a discussion of the generalizing/specifying-ambivalence in Popper’s understanding of social science, see Mjøset 2001: 15642.
Burawoy’s problem is that he has a very undifferentiated notion of theory: when he talks about “reconstructing theory”, theory sometimes means quite specific (some would label them “grounded”, others would call them “middle range”) theories linked to specific local research frontiers, at other times it means very broad theories of modernity, globalization, system/life world (that according to our taxonomy would be transcendental social-philosophical theories). As long as the theories Burawoy are reconstructing are low or middle level kind of theories, the difference from grounded theory is hard to detect.

But if he is thinking of high-level social-philosophical theories (Habermas’ system/lifeworld-interpretation of the present is an example he refers to), the problem is that such theories are highly decontextualized, so that the link to particular cases becomes very loose. Note that this whole discussion has not made any reference to the extended case method, since Burawoy emphasizes that his “approach to theory reconstruction” does not require “extending out”, one can confine “oneself to the structure and dynamics of the micro situation” (1991: 303, n. 5).

Re (2): The extended case method (originating in the Manchester school of anthropology) is a more limited approach, which sees “a particular phenomenon” as the “product of historically specific causes”, Burawoy contrasts to grounded theory, which in his view “systematically removes particularity” (1991: 303, n. 5). This claim is similar to the one quoted above, that grounded theory strives towards covering laws through inductive procedures. This is simply wrong! Burawoy himself notes that Glaser and Strauss (1967: Ch. IV) distinguished between substantive and formal grounded theory.

A substantive theory would be an explanation that — at the present local research frontier — is accepted by the research community as a valid account with reference to a specified context, such as e.g. the Nordic welfare states in the 1990s. A formal theory, in contrast, would be a module (some prefer the term “mechanism”) that can apply in various contexts (e.g. “the strength of weak ties” in network theory), but one that is not explanatory in and of itself. It only becomes explanatory when used in a context.

Substantive grounded theory is theory discovered in specific case-studies, turned to contextual generalizations in specific fields of research through the process of theoretical sampling. This yields contextual generalizations that are capable of explaining (cf. the example of Rokkan’s typological maps, above). Formal grounded theories may be useful as components in such an explanation, but formal theory does not explain without context. Thus, substantive grounded theory, as Glaser and Strauss emphasized, has priority.

Now, if this is so, the real difference between grounded theory and the extended case method should be the interpretation of “historically specific causes”, where substantive grounded
theory might yield only a very “local” causal constellation, whereas the extended case method extends out to “the determining macro context”. But that would seem to restrict the extended case method to a quite limited set of research questions. Is the macro-context always the determining one? Is that not a question of the research question posed? Are there not many good pieces of social research that does not relate causal chains to a “determining macro context”? Furthermore, we have already seen that Burawoy allows us to reconstruct theory with reference solely to the micro-situation.

But let us again consider the possibility that we have an extended case study that reconstructs social-philosophical theory. The peculiarities of that case would then be related to a “determining macrocontext” conceived in terms of grand, despecified periodizations such as e.g. globalization, modernity, capitalism. Now, given the social-philosophical roots of such notions, they must be seen as ungrounded. When Glaser and Strauss launched the notion of grounded theory, however, that programme was never restricted to the micro level. Grounded theory was possible at any level, and they discussed several macro-studies (cf. Glaser/Strauss 1967: Ch. VI). This is why a focus on the macro-context is in no way in opposition to doing grounded theory, in fact quite the opposite: in order to establish a specification of the “determining macrocontext” that is relevant to the specific field of research and the research questions asked, Burawoy needs a grounded theory at the macro level!

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Some important implications

Goldthorpe is very eager to commit researchers to “one logic of inference”, but our discussion shows that there are at least three logics. One is surely the logic of deduction and testing. But such a logic works only within a context (and if we want an empirical context, we cannot accept the rational choice solution of establishing this in the form of arm chair assumptions), and the deductive logic is often compressed or substituted by mechanisms (or formal grounded theory).

Thus, the second is the logic of context. This is the logic of types and comparisons. How thoroughly we need to do this, and at what level, depends on the research question.

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8 That same dilemma runs through the whole of C. Wright Mills’ argument in his classic The Sociological Imagination (1959). Most of the elements of Burawoy’s criticism of grounded theory is already contained in Mills’ book: one only needs to replace with “the sociological imagination” with “the extended case method” and “liberal practicality” with “grounded theory”. I shall specify this convergence in a later version of this paper.

9 I am quite convinced that this link is not always very solid in the various chapters of Burawoy’s edited volumes (1991, 2000). But space and time prevents me from demonstrating this in this version of the paper.

10 Note again C. Wright Mills, who (in 1959) was one of the first to search for the peculiarities of “the post-modern period”.

11 This point is parallel to our counterposing of Rokkan’s grounded approach to Goldthorpe’s ungrounded approach!
Mechanisms is the third logic: here very complicated chains of action, interaction, including complicated aspects such as feelings, limits on knowledge, etc. are compressed into stylized processes.

Researchers with a standard attitude invoke the necessity to specify the scope within which their formal patterns emerge as “lawlike”, but only substantive grounded theory gives meaning to a notion of the scope or context within which the theory applies. Importantly, this is not a question of testing, since it is only within a set of scope conditions that one can actually test competing (explanatory) theories. Neither can scope conditions be deduced from more general theory, they must be established pragmatically, in relation to the participation of knowledge-production in society, since scope must be a function of what we want to know — that is — our “knowledge interests”. We realise the difference from explanation by laws: we rather have explanation by singular causal factors, they establish the context within which we may enrich the analysis by our knowledge of formal grounded theories.

Pragmatist philosophy always opposed dualisms, and throughout our discussion we have questioned the generalizing/specifying dualism.

Generalization is not the opposite of explaining particulars (as both Goldthorpe and Burawoy are inclined to think), it is a careful careful accumulation of knowledge from explained cases chosen according to a sampling logic (which is not that of testing, but of creating the necessary context given the research problem asked).

The understanding/explanation-dichotomy is here irrelevant. The analysis of constellations of singular causal factors are at the core of the more pragmatic sciences such as psychology (as therapy), law and history.

Formal grounded theory most similar to transcendental theory of action and the kind of generalized theory required by standard scholars! But the pragmatist position is that none of these are explanatory outside of a context! Thus its notion of general theory is not linked to formal theory, but to substantive theory. This implies that general theory is not converging, there maybe several general theories, uniting various local research frontiers. It follows that formal theory can not be regarded as the most general theory (something that both Goldthorpe and Burawoy seem to assume). Rather, general explanatory theory must be general summaries of contextual factors that maybe of value to several local research frontiers. Good typologies as the most general knowledge you can get, relevant to a number of local research frontiers! Such general theory is not necessarily high level theory! It is general in terms of “providing context” for a large number of specialized studies (local research frontiers): but not for all. There may be several, non-convergent general substantive theories. Still they are more general than the chaos of “hypednthed sociologies”...

Rokkan’s maps and Korpi/Esping-Andersen’s families of welfare states are exactly general in this sense, they aid the researchers in the complicated task of establishing context, whether the research is on industrial relations, on welfare states, on political development, of ethnic mobilization, etc. If social theory is about interpreting the present, then these studies are indispensable, and as we have seen, there are even reasons to regard them as among the most general theories that are of relevance to anybody who try to interpret the present within which they are themselves situated...

This implication is probably most relevant for studies at the macro-level. Here most of the generalizations will be substantive ones. At the micro-level, I am less sure. Maybe the notion
of formal grounded theory will prove to be relatively more important in micro-level studies, but this implication cannot be further explored here!

Conclusion

Goldthorpe cannot establish theory (and thus not explain) without some kind of context. His contribution to accumulated empirical knowledge is hampered by his standard attitude!

Burawoy is — in the end — unable to provide convincing arguments that his own “extended case method” can in significant ways be distinguished from grounded theory. His ability to reflect on ethnographic research is hampered by a social-philosophical attitude (more specifically, a Marxist variety of that attitude). A successful pursuit of the “extended case method” requires that the “determining macro-context” is analysed in a grounded way.

Both the standard and the social-philosophical attitude pursues high-level notions of theory (Table 2). Our discussion supports a broader conclusion, namely that high level notions of theory tend to hamper the accumulation of empirical knowledge in the social sciences.

If the pragmatist attitude is one which also standard and social-philosophical social scientists will always be forced to accommodate in some sense, why not consider it — and the low/middle-level notions of theory it implies — in its own right?

References