Interrogations #10. Grounded Theory vs Extended Case Method
November 11, 2004

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1. Matt Desmond

I began reading the debates between the Extended Case Method and Grounded Theory five years ago when I was a 3rd year undergraduate. Many people feel that this debate centers around the practice of doing ethnography, the core of the fieldwork, and for the most part, they are half right. But what is more important here is the difference between how one enters the field and how one writes up the ethnography. The deductive ethnographer enters the field with a theoretical problem in mind, a certain informed approach to the field, which drives her investigation, whereas the inductive ethnographer is driven more by the operations within the culture and what things strike her as interesting (basically vis-à-vis her criteria of 'interestingness'). While in the meat of the fieldwork, both types of ethnographers use deductive and inductive methods **[Just one slight modification here: Burawoy regards the process of making predictions, finding anomalies, and reconstructing theory as a continual process during the fieldwork itself, not simply a strategy of post-fieldwork analysis. The reconstruction is an active part of the field work itself, and it will lead to a quite distinct fieldwork practice: looking for situations in which the reconstruction leads to new expectations and – perhaps even – new forms of active intervention to prod the situation]** however, upon leaving the field, the deductive scholar uses her data to refine and 'reconstruct' social theory while the inductive ethnography attempts to 'generate' social theory free from "the shackles of existing theory and contemporary emphasis" (Glaser and Strauss, p. 38). I basically side with Burawoy in this debate, and rather using this response to 'hash it out' as is the normal custom when reading these texts, I am instead going to attempt to challenge some of Burawoy's key claims, and accordingly, challenge some of my assumptions about the practice and promise of ethnography.

Burawoy vies for two models of science: positive and reflexive. He sees the former as not living up to its promises, and this is not an empirical problem but a theoretical innateness in the philosophy of positivism. The argument leads to the conclusion that we must reject the fundamental tenants of positivism, but Burawoy does not take this step. Instead, he advances a two-headed science, both with advantages and disadvantages. But if Burawoy is right in that not even the best positivist can live up to the fundamentals of positivism, why keep this way of doing science around? Why not reject it altogether, as would be the logical conclusion stemming from his critique? **[It could be the case that even though the best positivism fails to live up to its promise, the attempt at the best positivism generates knowledge of a form that would not be generated if one abandoned positivism altogether. Perhaps it is like saying that the best markets do not live up to the promise of the perfect market – which is an impossibility – but that nevertheless giving up markets entirely would still be a bad thing. What we need is both market provision and community/state provision of certain things even though both fail to "live up to their promises."]** It is important to note that this does not mean rejecting methods that are associated with positivism, such as statistical large-N studies, but a way of manipulating those statistics according to positivism's demands. One can have a reflexive statistician…and these are usually the best statisticians! **[You need to somewhat expand this: what is a reflexive statistical analysis that isn't also positivistic in some real sense?]**

It seems to me that Burawoy often solves problems by advancing a claim of 'making space.' This is his main argument in his article on Public Sociologies, for example. Not all sociology should be public, he claims; rather, the enterprise of sociology should make a space for public sociology. In the same way, he argues that not all sociology should be reflexive; rather, the enterprise of sociology should make a space for reflexive science. I find these sorts of claims a little on the weak side, and I see two large problems with them. First, vying for space ignores power. If the field of sociology is dominated by a positivist conception, there can be a space for reflexive scientists, but this space may not carry with it any sorts of scientific capital. It is a space in the corner. If positivists hold the means to reproduce their type of science as 'most legitimate,' i.e. by monopolizing editorial seats on flagship journal committees (rather this is the case or not is besides my point), then the question is not space, but how much space and how much power. If one science 'performs' better than the other, at its core, then it should be advanced in the stead of the other. Second, when two diametrically opposed sciences are advanced under the same faculty of sociology, this impinges advancements in the discipline because instead of advancing sociology under a *unified* philosophy of science with *multiple* methods, positivists critique reflexive scientists on the bases of positivism and vice versa instead of critiquing work in accordance to other criteria, such as the sophistication of the model, goodness of fit between theory and data, etc. **[One could still argue that in spite of this, struggling for a hegemonic reflexive sociology that marginalized positivistic sociology would be a bad thing – this would lead to a degenerative reflexivity because it would have to maintain itself through mechanisms of domination that would undercut its virtues. I think it is coherent to argue for the ideal of genuine pluralism – both of technique and methodology (philosophy of science) – on the grounds that this will generate the best creative tension in the community of dialogue. That could be the ideal, and then for pragmatic reasons one could argue for various forms of second-best solutions under differing conditions of power, etc.]**

Now, my claims imply that one type of science is superior to the other, unlike Burawoy who claims that they both have equal pluses and minuses. There is. Reflexive science is superior to positivism because it takes into account its weaknesses—this is the very root meaning of reflexivity, 'to turn the methods of investigation upon themselves'—whereas positivism advances empty claims that cannot be lived up to. What reflexive science must live up to is its ability to reflect upon how issues of power cloud scientific understanding, which seems like a great way to carry out sociological research! This does not imply that the 4 Rs should be tossed to the wayside, but it does imply that the goals of positivism submit to the philosophy of a reflexive science. **[Nice statement – it is a bit like saying tolerance and pluralism are better than intolerance and absolutism, because pluralism can include absolutists in their community of discourse but absolutists can't include pluralists in their's.]**

One last point. I believe that Burawoy's concluding section of the effect of power might be the most important contribution to this article. However, why does he see these 'effects of power' as limitations to reflexive science? The effects are part of the larger social space and part of practice science in general, not effects that are brought on only by *reflexive* science. Reflexive science makes us aware of these effects, and uses these effects as data. When Philippe Bourgois insults a lead crack dealer in *In Search of Respect* by asking the man to read a newspaper clipping, and he discovers that the man is illiterate and is almost beaten to a bloody pulp, he discovers a 'effect of power' here: one of the assuming scientist over the illiterate 'subject.' This was a key insight in his book. The social scientist will never escape these forces of power, but she can be aware of these and use these to draw attention to backdoor effects as well as the limits of research and this is the *power*, not the limitation of, reflexive sociology.

It is only a limitation if you are judging your sociology on the tenants of positivism. **[This might be a good question to pose to Burawoy in our collective discussion. The problem of power is not one that disappears because a reflexive sociologist is aware of it, any more than the problem of the gap between promise and practice in the 4Rs disappears because of self-awareness. Survey interviews are still an artificial situation than defines a distorting context of questioning even if one knows this is the case. So, the point here, I think, is that one cannot purge power from the practice just because one reflexively understands it is there.]**

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2. Gokcen Coskuner

In Reaching for the Global Burroway presents the works of three different school that adopted participant observation: Chicago, Manchester and Berkeley. He talks about the The Polish Peasant one of the example works of the Chicago School. He asserts that in this work,Thomas and Znaniecki sought to locate the subjective, lived experience of the Polish peasant in its widest historical and geographical context but that their presentation of the lived experience missed the class forces and was silent about capitalism. My question is if Thomas and Znaniecki had extensive data about the lived experience of the Polish peasant why did they fail to explain the class forces? If someone was to analyze their participant observation data could he/she point out the class relations as evident in the daily experiences of the Polish peasants? To put the question in a larger frame about general ethnographic research, do the results of an ethnographic research depend on 1) the emphasis on different dynamics when participant observation was done? 2) the focus of analysis when interpreting the data? **[I am not sure precisely what the explanation for the silence on class forces would be in this case, but I imagine that it was at least in part because the ethnographers lacked a theory within which "class forces" figured as an important element, and thus they did not look for the ways in which class might impinge on lived experience. This is especially relevant since "class" is not always a category of lived experience, and thus if one embeds one's**

categories too closely in the lived experiences of the actors one could easily ignore such forces.]

Burroway asserts that although both applying participant observation grounded theory and extended case method differ in the sense that ground theory is concerned to discover new theory from the ground up, the extended case method on the other hand seeks to reconstruct existing theory. He also discusses how the grounded theory attempt to derive the properties of the macro world from the micro observations whereas, extended case method seeks to undercover the macro foundations of a microsociology. The extended case method looks at micro events and tries to explain the macro foundations underlying the dynamics of this event. If it doesn't ever look from micro to macro, and consider the dynamics at the macro level **[do you mean "micro" here?]** that causes reconstruction at the macro how does then the extended case method explain the changes at the macro level? **[The actual data gathered through extended case method participant observation are –- I think – inherently micro insofar as they are gathered through direct interaction with people in some setting. The question, then, is how these observations get deployed as evidence within a theoretical argument of some sort. If the theory within which one is working involves claims about how micro-processes impact on macro-change, then the data from p.o. could certainly be relevant to that. This would be a form of extending from the micro-to-the-macro I think.]**

Burroway highlights the objective of the extended case method as reconstructing theory. In the examples he provides in Ethnography Unbounded we see how the researchers went out in the field, start collecting data and then try to understand the phenomenon in hand in the light of some previous theory. They find an anomaly and then manifest how this extends, reconstruct the previous theory. Then can we talk about totally new theories in social science and if yes, how are they generated? **[I don't think Burawoy has anything explicit to say about "totally new" theory, but I presume this would occur in the aftermath of repeated failures to cope with accumulated anomalies from the reconstruction of existing theory. The accumulation of anomalies provides a stimulus for a more comprehensive imaginative leap, which might constitute something like a "totally new" theory – although in practice, I think, total-newness is unlikely.]**

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3. Wayne Au

My first question revolves around grounded theory. Glaser and Strauss basically define grounded theory as "the discovery of theory from data systematically obtained from social research." (p. 2). They oppose it to "theory generated by logical deduction from *a priori* assumptions." (p. 3). Burawoy (1998) reiterates that "theory is the result and not the precondition of research." (p. 25) **[This is Burawoy's characterization of grounded theory, not his characterization of theory in general. He rejects this formulation in**

the extended case method.] What concerns me about grounded theory generally (and perhaps my concern is shaped by what in my experience has been a sloppy application of grounded theory in educational research) is that it creates the space for the researcher to deny that they came into their research with no **[you mean "any", right?]** preconceived notions. That they just found what they found. **[This is not exactly what Glaser & Straus claim. They write on p33 "To be sure one goes out and studies an area with a particular sociological perspective, and with a focus, a general question or problem in mind." So, you have a *perspective* but not a specific "preconceived theory that dictates prior to research revelancies in concepts and hypotheses." The contrast between a perspective and a preconceived theory is not entirely clear, but in any case they are not saying that research is entirely presuppositionless.]** **Even if researchers have no preconceived theories regarding a particular phenomena, aren't they guided by their own theoretical frameworks as they choose contexts, sites, questions, and samples, for their research? If that's the case, then isn't grounded theory a way creating a false sense of distance between the researcher and the theory they've created from the data?** I do not think that the grounded theory presented by Burawoy, particularly as he frames it within critical reflexive science, slips into this dilemma, but I still sense it generally. **[I am not sure what you mean when you talk about grounded theory "presented by Burawoy". He "presents" Glaser & Straus as a foil for his alternative to grounded theory, but his own approach is not "grounded" theory.]**

Burawoy distinguishes between "inductive generalization" that seeks out "common patterns among diverse cases, so that context can be discounted" and that of the extended case method, which he says uses a strategy of "tracing the source of small difference to external forces." Burawoy goes on to say that the "purpose of the comparison is to causally connect the cases. Instead of reducing cases to instances of general law, we make each case work in its connection to other cases." **Does Burawoy's conceptualization deny the existence (or possible existence at any rate) of generalization? Does this mean that, say I find evidence of a particular process or phenomena taking place amongst every person in the U.S., I cannot draw a generalization from that evidence? Or put another way, do I have to resort to seeing said phenomena as it only relates between/among cases and not as evidence of a process that is happening independently of how individual cases are seeing/experiencing a shared phenomena?** **[I think Burawoy would say something like the following: One of the goals of science is to identify general causes, not just contextually specific cases and their interconnections. But the process of discovering general mechanisms is not one of "generalization", in the sense of gathering ever more cases that empirically demonstrate the same properties. Rather "generalization" is the process of iteratively reconstructing a theory-of-mechanisms that becomes more general as it is strengthened through robust reconstructions. What is rejected is the process of generalization understood in the inductivist way, not the idea of a theory having general relevance or identifying causal processes that operate in many different contexts.]** I will readily admit that I may be fully misunderstanding Burawoy's conceptualization of the relationship between comparative case studies and generalization, but my general concern is the potential lean towards methodological individualism that I see feel may be present here.

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4. Brett Burkhardt

"Can historical research really be reflexive?"

A key element of Burawoy's proposed reflexive model of science is intervention into the subjects' social world. The benefit of intervention is that it "create[s] perturbations that are not noise to be expurgated but music to be appreciated, transmitting the hidden secrets of the participant's world (14)." The benefit is derived (partially, at least) through "dialogue, virtual or real, between observer and participants…(5)." In essence, the researcher adds something new (his or her presence) to the social situation being observed, while at the same time participants react to the new stimulus in novel ways. Moreover, these actions and reactions by observer and participants mutually influence each other. The claim, then, is that reciprocal interaction, initiated by observer intervention, provides insight into social relations and processes, insight that would not be yielded had the observer been invisible and mute.

Burawoy suggests that historical research can be done using the reflexive model. Researchers can "mov[e] with the participants through their space and time. The move may be virtual, as for example in historical interpretation (14)." And as suggested by Burawoy in the previous paragraph, dialogue with participants may be virtual or real. But if crucial insight is gained through the observer acting on an existing social world, and if this intervention is a part of reflexive research, then "reflexive historical research" seems to be an oxymoron. A "virtual" historical dialogue cannot be an actual dialogue, since the researcher can in no way influence what has already happened. He or she can, of course, receive information from the "monologue" provided by the historical record. But the lack of reciprocity between historical figures and researcher would seemingly make historical research a necessarily positive (as opposed to reflexive) research project. **[A couple of comments here: The reflexivity of reflexive sociology is only partially bound up with the intervention-perturbation issue. Remember that this particular element is the counterpoint to only one "R" in Katz's 4Rs – Reactivity. There is also representativeness, reliability , a replicability, and each of these as a reflexive alternative. But of course, you are right that virtual dialogue and real dialogue are quite different, so I guess the question is whether "virtual" dialogue is a genuine methodological process or simply a gesture. I am not sure about this, but certainly one is familiar with historical research in which it does seem like the analyst has gotten a deep grasp of the lived experience of the actors in a way that simulates dialogue and which, when you read, seems quite different from the exposition of self-consciously positivistic scholarship. In any event, the whole edifice of reflexivity does not hinge on this single element.]**

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5. Mara Eisch-Schweitzer

Burawoy takes the position that methodology links technique and theory; that "it is the task of *methodology* to explicate methods of turning observations into explanations, data into theory" (p. 5). Burawoy targets participant observation as a technique of grounded theory and ethnomethodology, but with contrasting aims. Grounded theory makes generalizations from what different social situations have in common to generate new theory. Ethnomethodology examines the non-discursive knowledge that makes social action possible to reveal 'anomalies' that theory fails to explain to so to reconstruct (rather than create) theory. So while grounded theory and ethnomethodology, as methods, link participant observation (technique) with the theorizing of microsociology, they maintain a contrasting relationship to theory.

Burawoy claims that grounded theory "links the macro to the micro on the basis of methodological individualism" (p. 282); whereas ethomethodology (concerned with the particular for microsociology) and extended case method (concerned with the particular for macrosociology) link the macro and micro on the basis of methodological situationism.

Does methodological situationism take as its point of examination the social situation as opposed to the individual? Is it fair to say that methodological situationism then is a wider lens than methodological individualism? **[I am not sure that I would characterize situationism as a "wider" lens – or at least I am not sure what the wide/narrow metaphor is trying to capture here. Situationism involves a substantive claim that the actions of individuals are always situated-within-relations, never atomistic. The situated-action is therefore in a sense the fundamental unit of social life, not the individual as such (even though the actions are performed by individuals). But is this "wider" or "deeper" or just "better"?**

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6. Ana Cristina Collares

Here comes again the discussion about how to define scientific work (as distinguished from, say, journalism, or common sense reports). Sociologists insist in specifying the methods that make sociology scientific, and they also insist in comparing sociology with the so-called "hard" sciences, as if the latter were parameters for the "true scientific practice". Some of the texts for this week's session seem to be guided by this approach, especially *The Discovery of Grounded Theory*.

On one hand, I have been inclining myself lately to adopt a pragmatic point of view such as the one proposed by W.V. Quine (although not so radical) about science, in which there is no possibility of verifying scientific knowledge through the method of deriving empirical evidences from theoretical statements, because the necessary empirical evidences are already implied in the theoretical system from where these statements came from.**[Just one point of clarification here: There is a weak version, a strong version,**

and a super-strong interpretation of your statement (slightly edited) "the necessary empirical evidence is already implied in the theoretical system from where these statements come".

> **Weak version: theoretical frameworks or statements determine the *range of possible* empirical observations.**
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> **strong interpretation: theoretical frameworks determine the actual empirical observations, not just possible observations.**
>
> **super-strong version: theoretical frameworks determines the empirical observations in such a way that those observations will always validate the theory that generates them.**

Now, it is really only the third of these that completely fractures the ability for empirical evidence to "verify" theoretical claims, since theory always generates theory-validating observation. But I don't see why the weak version blocks "verification". (I am taking verification in the soft sense of empirical observation adding credibility to theoretical claims rather than provides absolute guarantees). I am not qite sure exactly what you are claiming here.] Therefore, not even hard sciences can be appropriately "verified". On the other hand, I like Burawoy's approach, according to which there is the possibility of having different conceptions of science coexisting. It is in Burawoy's exposition of the *extended case method* that I would like to center my interrogation.

Burawoy defines ethnography as "writing about the world from the standpoint of participant observation"; and science as "falsifiable and generalizable explanations of empirical phenomena." My first question here is: "Is Burawoy distinguishing *science*, within this definition, from *scientific model*, that, according to him, can be reflexive or positive? I am asking this because, as far as I understood from the text, the reflexive model cannot create falsifiable explanations, because the context of investigation will never be the same. So, how to "falsify" without being able to go back to the original contextual conditions? **[I don't think the thesis of contextualism implies nonfalsifiability of explanatory claims. "Explanation" in Burawoy's approach refers to causal explanations with real mechanisms. The effects of such mechanisms are always context-dependent because any given social mechanism we want to study necessarily operates in open systems, and thus always in conjunction with many other contingently present mechanisms. This makes life difficult for social science, but it is still the case that our claims about mechanisms and their effects are subject to revision and rejection on the basis of evidence. This is where the search for anomalies comes from. I think, therefore, Burawoy is referring to science here not just scientific models.]**

My second question is about the gap between survey interviews and ethnographic work. Neither respondents of surveys nor subjects in ethnographical contexts have full consciousness of the forces leading them to act in a certain way, or to represent their reality in a certain way. Moreover, in both cases the investigator influences the context (reflexivity) and creates "noise", either if this noise is used creatively in the research or just subject to attempts of elimination. Can the connection of research to previous theories be a tentative solution to reduce discrepancies between different accounts? Can previous theory be the key to balance ethnography and survey accounts? **[I couldn't quite follow your point here. Theory, of course, is crucial throughout Burawoy's argument, and theories include explanations of observer effects. Burawoy's interventionist argument about the researcher's use of perturbations of the field-situation generated by the researcher only contributes to knowledge if the researcher has some understanding of how such perturbation is generated – that is, a theory of research interventions. That is a part of the reflexivity of his method, but it also relies on theory.]**

Burawoy claims that "there is something ineffably unique about the ethnographic encounter. It certainly would have been interesting for someone else to repeat the study, simultaneously or subsequently, not as a replication but as an extension of my own study". (p. 11) My questions about this are, first, how to make sense of the different accounts made by different investigators in the same context, if the methods are different and the influence of each researcher will also be different? Can we say that, by putting together different accounts of the same situation, we are gaining in coherence? **[So long as both researchers effectively report and analyze the "perturbations" generated by their interventions, and provide commensurable descriptive accounts, then there would be some chance for cumulative knowledge from a "revisit" to the same site.]**

Finally, I have a last concern about ethnographic research: its ethical grounds. E.g., Burawoy organized a survey about the working and living conditions of Zambian workers, when in reality he wanted to learn about "Zambianization" from the bottom. Was it not unethical to distort the purpose of the investigation to the respondents? In most cases, when our motives are explicitly stated to the subjects of the research, can we achieve an unbiased account of a situation? **[This is a very tricky situation, since there are endless contexts in which if we shared our theoretical perspective or even our core questions we would be denied access to the research site. This is an ethical problem in any kind of fieldwork – in grounded theory it is also a problem since misrepresentation is an ethical issue quite a part from its impact on the interactions themselves. But I do think that such issues are especially salient in Burwoy's framework because misrepresentation has an impact on the dialogic element in the reflexive model of science. How can you have meaningful dialogue if you misrepresent your interest in the communication? Thus, I think, the ethical issue becomes a methodological problem for the extended case method in a way that it does not for other approaches.]**

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7. Mark Cooper

My primary interest in this week's readings is attempting to understand Burawoy's critique of positivist science and his promotion of reflexive science. Burawoy defines science as, "falsifiable and generalizable explanations of empirical phenomena." (6) Despite Burawoy's description of reflexive science, I remain skeptical as to how successful the standards by which it claims to be falsifiable and generalizable are in delineating the existing of a new type of science. Much of the difficulty of this lies in my unfamiliarity with several of Burawoy's central terms, especially context effects and power effects. Additionally, it is unclear how several steps within reflexive science can be described in ways that preserve their falsifiability and generalizability. Both the interpretation of ethnographic data and the aggregation of situational knowledge into social processes seem to rely on the dialogic interaction of observer and observed. Burawoy notes that, "science offers no final truth, no certainties, but exists in a state of continual revision." (16) While this is certainly the case, reflexive science, unlike positivist science does not seem to clearly elaborate how resolution of two competing explanations, developed by separate researchers and similar time and place, might occur. This is similar to Burawoy's mention of Winch, though his answer to this I find unclear. (16) I therefore remain unsure what constitutes a better explanation in the extended case method, much less how such could be falsifiable. **[By "falsifiable" Burawoy means that the explanation you propose is capable of being contradicted by new observations. He does not mean that when such contradiction occurs you reject the theory. His central method stipulates that in the face of anomalies – that is, observations that contradict the predictions/expectations of a theory – you should *reconstruct* rather than *reject* the theory. In this way he rejects Popper's falsificationist doctrine. A theory – or perhaps better, a theoretical framework or research program – could eventually be rejected in the face of accumulations of anomalies that lead to ideiodic ad hoc reconstructions, but not because of specific explanatory failures. Now, when two rival explanations of the same anomaly are proposed – i.e. two rival ways of reconstructing a theory in light of an anomaly are proposed – then I think Burawoy would choose between them on the basis of how well they perform within the overall theory of which they are parts, what novel predictions they make, how well they contend with new anomalies, etc. I don't think this is a special issue for his reflexive science, but very similar to the general strategy of realist, anti-positivism.]**

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8. Matt Dimick

Burawoy states, "Theory is essential to each dimension of the extended case method. It guides interventions, it constitutes situated knowledges into social processes, and it locates those social processes in their wider context of determination. Moreover, theory is not something stored up in the academy but itself becomes an intervention into the world it seeks to comprehend" (p. 21). Whereas, for example, in grounded theory "theory is the result and not the precondition for research" the opposite is the case in the extended case method (p. 25). This key methodological position of theory—where the researcher takes a "kamikaze" stance, looking for a theory's refutations rather than confirmations—would appear to put a high premium on "testable" theory. **[a terminological point (maybe a "precious" one): Anomaly-hunting is not quite the same as "testing" theories, at least as the term "testable" is generally used. Testing is usually used in the spirit of showing whether or not we should reject the theory. Anomaly-hunting is looking for data that forces us to reconstruct and improve the theory. There is thus a high premium on "improvable" theories, or "reconstructable" theories.]**

Since we are not as interested in letting theory "emerge" from the empirical research as we are in confronting already elaborated theory with the facts, we want theory that can be clearly confirmed or disconfirmed. **[Again: disconfirmed is a bit too strong in the reconstructionist approach.]** Recent proposals have offered the use of formal (or "formalizable") theory, including rational choice theory (RCT) and game theory, in combination with "narrative" and/or historical research, to do something of the same thing as the extended case method. I'm thinking of Latin's "tripartite method," the debate on RCT in comparative-historical research started by Kiser and Hechter in AJS, and the *Analytical Narratives* book by Bates, Levi, and others. One of the reasons these approaches have been offered is their purported clarity and testability. Knowing what Burawoy has said in the past about RCT and methodological (or "mythological," I believe were the words) individualism, I'm wondering what he thinks about these recent efforts to combine RCT and "narrative" research in ways that appear very similar to the extended case method (particularly in historical research)? **[I think the issue is the extent to which these tripartite amalgams help to generate focused anomalies which then provoke theory reconstruction. RCT seems pretty impervious to this. The explanatory applications may be reconstructed by anomaly, but I am not sure the "theory" is likely to be reconstructed by empirical anomalies give the intellectual criteria used to define good vs bad RCT.]**

One immediate response would be that, substantively, RCT is just one approach, and if you disagree, again substantively, with RCT, then why use it? My response would be that RCT can still be very valuable as a heuristic device. **[I am not sure what is the status of "heuristic devices" in Buraowy's reflexive methodology. Theories are not treated as heuristics in his argument, for it isn't clear what an anomaly means in such cases.]** One can start with the strong assumption that individuals are self-interested and rational (the "thick" RCT); if people we study in our cases do not act as RCT predicts in this way, we "reconstruct" our theory. **[But we do not reconstruct the heuristic, right? We just combine it with other explanatory models – like habit. This is more like the diagnostic task of the doctor, trying different diagnostic tools, that the theory reconstruction task of the reflexive scientist.]** We can also remove the assumption that behavior is self-interested, but retain the assumption that it is rational, that is, that people's behavior changes as costs/benefits of those behaviors change, regardless of whether one's tastes are selfish or altruistic; again, we can "reconstruct" as needed. In addition, with game theory, where there are "multiple equilibria," even rationality by itself won't tell us which equilibrium is the outcome, which forces us to consider other factors such as structures, institutions, resources, and even "context" (which, Tsebelis conceives in *Nested Games* as one game nested within another). I think what I am trying to say is that RCT is not as substantively demanding as people believe, that it can be used with a variety of substantively different theories (for lack of a better phrase), and that it gives us an opportunity to use its formal properties to help generate better "testable" hypotheses in a way that meshes well with the approach presented in the extended case method.

9. Dan Warshawsky

As usual, I found the readings quite stimulating as both grounded theory and extended case method are relatively new to me as an entering graduate student. My more pressing questions this week focus on three main issues. The first is content clarity. I will describe how I see extended case method, not grounded theory, as extended case method interests me more. Second, I want to interrogate issues regarding the "optimal" research methodology question. Third, I will pose questions regarding the "quality research" issue.

I think Burawoy's extended case theory is quite interesting. I see him making a critical division between positive science and reflexive science. Also, I believe he does not claim superiority of one method over the other; rather, they are more complimentary. **[But don't you feel this is a bit of a rhetorical stance – that in his heart he sees reflexive science as superior to positivist, if only because reflexive science can make use of the results of positivist science whereas the latter cannot make use of the results of reflexive science?]** "Rather than arguing that there is one model of science that is best carried out with reflexive awareness, I propose a methodological duality, the coexistence and interdependence of two models of science - positive and reflexive" (Burawoy 14). So, it seems that he is content not to have a unified approach to science. Instead, each way of approaching situations has its positives and negatives. He says that positive science is limited by "context effects," while reflexive science is limited by "power effects." **[Do you buy this contrast? Isn't positivist science also limited by power-effects, since power is one of the pivotal ways in which contexts impact data?]** His division is intellectually intriguing; however, is it the best way to approach research inquires? Secondly, he uses his reflexive science with ethnography to "extract the general from the unique, to move from the 'micro' to the 'macro'" (Burawoy 16). Is this possible, or is this desirable. This seems to be a question more about issues of methodological individualism: what is the connection between micro and macro? (Maybe this is too much of a digression).

My second and more theoretical issue relates to the question of "optimal science." Is Burawoy's duality between positive science and reflexive science the best way to approach the methodology question? In some ways, it reminds me of the qualitative-quantitative duality. I believe the qualitative-quantitative binary is overused and quite useless. The research question should drive the methodology, not vice versa. **[Arguing for a distinction between these two methodologies, and for their equal legitimacy, is entirely consistent with the claim that the research question should drive the methodology.]** Although Burawoy might just be using a heuristic device to simplify and validate two different yet useful types of research methodologies, he could also be making a statement about methodology before the research question.**[If he is making a "statement before the research question" it is simply that there are two methodologies available for approaching research questions, but he is not affirming a principle that one can decide on methodology before questions.]** Additionally, I am assuming he is describing his extended case study method in detail, just to provide readers and others with clarity over how to use the method as he sees it; however, he is not mandating its use in this way. In this way, he just provides ethnographers with a powerful tool that can be used systematically to provide quality research. Thus, I generally like his extended case study method; however, I need some clarification on some of the assumption that I have made in this paragraph (Am I fair to assume what I have in this paragraph?).

Where does a Geertzian interpretive research methodology fit into his scheme? Geertz might have a problem with Burawoy's binary between positive science and reflexive science as Geertz would probably want Burawoy to dispose of this duality and replace it with his "thick description" and "webs of meaning." I got the feeling that Burawoy is not as "extreme" as Geertz is in his view of optimal research methodology; however, it seems that Burawoy was influenced by Geertz and other post-structuralists. **[Burawoy draws a contrast between the "Interpretive case method" and the "extended case method" in chapter 13 of Ethnography Unbound. Geertz is in the former. The pivot of the contrast has to do with locating the specificity of the particular within broader macro-hsitorical contexts (the extending from micro- to macro-), and I would add the lack of a clear explanatory program in Geertz as well.]**

Lastly, I am interested in the "quality research" question. Would it be appropriate to say that Burawoy is not only providing his dualistic heuristic of positive science versus reflexive science to highlight the strengths, limitations, and positionality of his extended case method, but he is also implicitly providing structure for good research. He describes Katz's four R's of reactivity, reliability, replicability, and representativeness, and how he breaks these four rules when doing his research. But, isn't Burawoy's extended case theory a set of rules too? Stating what is possible as a researcher, however open ended, is still value ridden? **[Burawoy certainly does provide a set of rules or principles of research – indeed, this is precisely why he formulates counterpoints to each of the 4R's – and these rules/principles provide guidelines for the quality of research as much as do the 4R's. I am not sure, however, if this really pivots around the issue of the ways in which research is value-ridden. This is not an issue that Buraowy spends much time talking about. I assume he would say that both positivistic science and reflexive sscience are value-laden; where they differ is in the degree of self-consciousness about this.]**

Hopefully, we can discuss this issue as well in class on Thursday.

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10. Fabian Pfeffer

Grounded theory – why not grounded in data as well as existing theory?

Glaser and Strauss give an attractive appraisal of the possibility and ability of sociologists to construct new theories. That makes hope, yet some of their claims seem unnecessarily restrictive: The call to theorize "from data rather than from the armchair" comes with the apparent challenge or even negation of classical (grand) theory. “The masters have not provided enough theories to cover all the areas of social life […] Further some theories of our predecessors, because of their lack of grounding in data, do not fit, or do not work, or are not sufficiently understandable to be used and there therefore useless in research, theoretical advance and practical application” (p. 11). First, I think ‘standing on the shoulders of giants’ can mean to apply some of their concepts and to remodel them accordingly. **[They reject, of course, the idea of “applying concepts” to problem, if by this you mean taking fully formed concepts from some general theory and imposing them on a case. This is what the mean by the contrast between having a perspective – which is what tells you what questions to bring to a case – and having preconceived concepts and theories – which tells you in advance what is salient about the case.]** What else is the concept of contradictory class locations? Glaser and Strauss mention (in a footnote) that “the researcher does not approach reality as a tabula rasa. He must have a perspective that will help him see relevant data and abstract significant categories form his scrutiny of the data” (p. 3). But they go on to propose a strategy “to first literally ignore the literature of theory and fact on the area under study” (p. 37). Not only grand theory but also what they name formal theory in general seems to be dismissed as a guide to frame research questions and set up empirical investigations. The implications of this approach seem to be the extreme plurality of coexisting and fine-graded theoretical pieces. Glaser and Strauss themselves elevate this to a virtue of their approach when they say that the “generation of theory should aim at achieving much diversity in emergent categories” (p. 37). **[It isn’t so clear what “much diversity” means here – this could just be relative to the existing state of affairs in which very abstract categories are often thought of near universal relevance. In the examples presented in Glaser and Straus – for example, their extended discussion of dying and status loss – they do seem to suggest that there will be cumulative knowledge generated through this process which will be potentially quite robust. Once a robust grounded theory of X is produced, I don’t imagine that they feel it should be ignored in subsequent research. Or perhaps, what they would say is that you should ignore existing theory when you do a case study, but it would be pretty stupid to do a case study on a case for which there is already a fully-elaborated grounded theory <u>for the problems in which you are</u> interested. I don’t imagine that they are for endlessly reinventing the wheel.]** While I can agree to the argument that the complexity of the entities studied forbids a general all-capturing theory, I do not see how such a plurality of coexisting middle-range theories is especially helpful for the advance of our discipline (keeping in mind that in their opinion we should reduce our efforts to test and falsify parts of them). And there is one further trenchant question: If the process of theory construction secures good data fit, the latter ceases to be a criterion for the quality of a theory. The remaining criterion might be the generality or applicability of a theory to a broader range of different phenomena. If my fore mentioned descriptions are correct, I do not see any criteria which helps us to assess the quality of a grounded theory. **[G&S propose the criterion of the extent to which a theory “fits” and “works”. This may be a little vague – perhaps in the rest of the book it is spelled out more fully – but it does not seem empty. “Fitting” seems to be close to the problem of anomalies. “Working” is probably something like “explaining coherently.”]**

Burawoy's extended case method on the other hand leaves me with far more question that might most suitably be resolved in our cyber-talk with him. Just one idea: The core of his interventionistic approach seems problematic. Even if it is the volitional disturbance of the social order which reveals it, I think that a previous undistorted state has to serve as the basis for assessing the changes that had been triggered. How can we assess such a state if in his approach everything seems to be "virtual participation"? **[I don't think everything is "virtual participation" in his approach. I believe he used that term for the specific problem of historical research where you can't really participate. But participant observation is about real participation and interventions which perturb the social processes. Now, if you only participated in a site for one day and only had one shot at intervening, then probably you could not say much about the situation since you would not be able to make any inferences about it from the disturbances you observe. But if you go to the site day after day and reflexively observe your interactions with people and the effects you seem to be having on people in many situations where there are many different sorts of perturbations, then you may be able to say something about the stable underlying processes within which these interaction-events occur. But of course, you need well elaborated theories both of the situations and of the interventions to be able to do this – and to generate the anomalies which provoke reconstructing these theories. One of the big difficulties, of course – and a difficulty Burawoy does not discuss systematically – is that any given anomaly can be an anomaly in the theory of intervention (the theory of the effects of the observer on the situation) or in the theory of the situation, and it is very challenging sometimes to disentangle these. This is parallel to the rpoblem in survey research of the difficulty in disentangling the relationship between the underlying concept and the measures on the one hand, and the relationship between the empirical results and the theory being "tested" – as in the various efforts at comparing my class concept with Goldthorpe's.]** Burawoy's counterargument might be the fluid and every-changing character of social reality which renders the notion of a previous stable state senseless.

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11. G.C.

Burawoy's methodology bears similarities to Popper's: "We begin with our favorite theory but seek not confirmations but refutations that inspire us to deepen that theory"(1998:16). By reducing and traversing the separateness of the scientist and his or her object of study, the distinction between lay and academic theory becomes blurry and seems to lose its significance; that is, the distinction between knowledge as the object of generic curiosity and knowledge as the object of scientific inquiry fades. **[I don't see why this distinction is blurry in Burawoy's methodology. He insists on the search for anomalies – which is hardly what "lay theory" does, since lay people constantly look for illustrative support for their pet theories – and sees the process of scientific progress as the continual reconstruction of theory in light of evidencen-generated anomalies. This is a demanding practice and provides quite systematic criteria for good and bad versions.]** This seems immediately to raise the question with which

Popper was concerned, viz., what is "good" science. The search for, on the one hand, an authentic science or scientific process, and on the other hand, the difference between good and bad science seem to be closely interrelated. The positivist project adduces a sanctified process which, as Burawoy points out, it is categorically incapable of fully implementing. The reflexive model of science, on the other hand, in abandoning positivism's quest for an Archimedean point, at the same time gives up a really clear set of criteria for judging *that* it is good or bad: "The goal of research is not directed at establishing a definitive 'truth' about an external world but at the continual improvement of existing theory"(p.28).**[But the quest for "improvement" can be disciplined into strong quality considerations, in which it is possible to distinguish good from bad science. This is the contrast between progressive and degenerative research programs in Lakatos, which Burawoy affirms. There is no reason that I can see that the impossibility of a definitive truth logically undercuts the standards for judging improvements in truth.]** Is it the case that the quest for "good" science leads to the specification of a process, which under scrutiny becomes untenable, so that one ends up again facing the original question? Which is to say, is the Weber quote at the beginning of "The Extended Case Method" actually is in fact quite accurate? It seems impossible merely to be non-dogmatic; one has to have criteria by which to reject and refine one's earlier conclusions.

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12. Eva Williams

"The *extended case method*," according to Michael Burawoy, "applies reflexive science to ethnography in order to extract the general from the unique, to move from the "micro" to the "macro," and to connect the present to the past in anticipation of the future, all by building on preexisting theory" (5). It is this last point which clearly separates *extended case method* from *grounded theory*. Barney Glaser and Anselm Strauss define *grounded theory*, as theory which is "…derived from data and then illustrated by characteristic examples of data" (5).

The central question therefore is whether or not constructing theory based on collected data—which suggests the absence of preconceived theoretical ideas about what is happening in the environment, which I find questionable—is superior to "building on preexisting theory." Glaser and Anselm argue in favor of the former with a "goodness of fit" argument. "Grounded theory," these authors claim, "can help to forestall the opportunistic use of theories that have dubious fit and working capacity…a tacked-on explanation taken from a logically deduced theory…to give…data a more general sociological meaning, as well as to account for or interpret what [was] found" (4). Yet this process of theory building based on *every new sociological encounter* suggests an eternal space of relativism and moves us away from the larger collective project of macro-level theory building, resulting from adjustment to/of existing theory over time—reconsideration and reconstruction of existing theories that in turn leads to the development of new theory. **[I don't know if this endless repetition is a necessary**

consequence of grounded theory. If you are a scholar who is silly enough to study cases and problems which have been thoroughly and successfully studied already and which has produced good grounded theory, then you will never be able to publish the results of your research – you will be accused (correctly) of "reinventing the wheel". The injunction by Glaser & Straus to enter the field with no preconceptions does not mean that a sociologist should avoid reading the hopefully vast number of brilliant case studies which generated good grounded theory that are relevant to the interests of the sociologist. With that acquired knowledge, then, you decide what are the possible case studies relevant to your broad interests. If there have been many studies of patients dying of cancer in many kinds of hospitals in many places in the world, and these studies have converged on a consolidated grounded theory of this process, then it might be a waste of time to do another such case study. One should pick something else if one is interested in (for example) "unpredicted status transitions". So, I don't think G&S's method necessarily implies relativism and no cumulative knowledge.] "Rather than always starting from scratch and developing new theories," posits Burawoy, "we should try to consolidate and develop what we have already produced" (26).

What remains for me is the question of how much does an existing theory need to explain to be considered good enough? What combinations of theories are acceptable vs. problematic? (For some reason "pharmaceuticals" strikes me as an apt metaphor here! Both in terms of potential toxicity when combined and in terms of the research and development aspect)

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13. Elizabeth Holzer

What according to the extended case method is a *generalization*, and how would theories constructed under this understanding of generalization lend themselves to interventions? [good, crisp question – we should directly ask Michael this].

Katz's (1982, p.136) says that qualitative and quantitative researchers use different *strategies* to achieve generalizations. That's probably true, but what's more to the point is that researchers have different notions of what generalization means. A positivist would say that an ideal theory is general in that it is (1) predictive, and (2) resilient in the face of testing with empirical evidence. **[Is this "general" or "robust"? Maybe they are the same thing. Is a theory of US politics that does a pretty good job of predicting elections count as a "general theory" even if it cannot predict French elections? Is a "theory of democratic elections" <u>more general</u> than a "theory of US elections"? Or is it just more <u>abstract</u> (i.e. capable of predicting more abstractly described characteristics of cases)?]** I couldn't quite catch what Buroway's stance on the predictive capacity of social scientists, but on the question of how to string discrete empirical evidence together to make a general statement, he shares with Katz (1982,134) a belief in the virtue of negative cases that I don't think is shared by positivists.**[Well, Popper also is very big on Negative Cases, since these constitute the basis for "falsification", which is his key criterion for science. Burawoy is a reconstructionist**

rather than falsificationist, but I am not sure that positivists are hostile to the search for negative cases.] So what then makes the extended case method produce generalized theory? In describing the stance of the extended case method folks, Buroway says that, "Instead of reducing cases to instances of a general law, we make each case work in its connection to other cases" (Burawoy 1998, 19). Would there ever be a case—say an ethnography of a factory in South Africa in 1999—that didn't "work" in connection to other cases. Or would the researcher be obligated to fit it in somehow—in other words, must (and can) all evidence be put to cause of generalization? What does it mean to be generalized? **[I am also not completely sure about this issue. Perhaps the contrast is between "reducing cases to instances of a general law" and "explaining effects in cases in terms of the operation of a general mechanism", where "general mechanism" means a mechanism that occurs in many different cases. Thus the mechanism of "hegemonic factory regimes" can occur in Sweden, the US and Japan, even though these are not instances of some general law.]**

On the second front, it'd be nice to take these two notions of generalization, the positivist's and the one endorsed in the extended case method, and see which kind of generalization gets us further towards the end of positive interventions in social life (à la public sociology or *phronesis).* Towards this end, let me offer an Empirical Example. It seems to me that social science research really fed into Clinton's welfare "reform," (not our fault, it was the economists) which highlights some of the troubles. On the one hand, the traditionally positive generalizations on welfare dependency (wasn't it Moffitt and those folks?) informed the policy debate, unlike, the **far superior** ethnographies like Edin and Lein's (er, maybe they came after)—but their intervention sure wasn't positive. So it really breaks down into two issues: which kind of generalization has more legitimacy in a political or public climate and which actually tells you about how the social world works accurately enough that the positive intended consequences of policies based on your generalizations can be trusted to outweigh the negative unintended consequences.

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14. Matías D. Scaglione

Although Michael Burawoy begins his article "The Extended Case Method" with two *strategies* to deal with the "ethnographic condition", i.e. the "positive approach" and the "reflexive model of science" (p. 5), it is soon clear that he treats such "strategies" as **two coexistent** and **interdependent "models of science"**. The difference between these two models of science lies not in their "ontological foundations", but "in the *relation of scientist to object*" (p. 14, emphasis added). Moreover, throughout the article *the author regards this binary typology of "model of science" as* ***exhaustive***: we have a traditional "positive model of science", which "proposes to insulate subject from object", and the coexistent and interdependent "reflexive model of science", which "elevates dialogue as its defining principle and intersubjectivity between participant and observer as its premise" (*ibid*.).

Burawoy defines *science* as "*falsifiable* and generalizable *explanations of empirical phenomena*" and *scientific model* as that which "lays out the *presuppositions* and *principles* for *producing science*" (p. 6, emphasis added). Therefore, since for Burawoy (*i*) the "scientific model" *antecedes* the production of knowledge, in the sense that it provides the "presuppositions and principles", and (*ii*) science *is tantamount* to falsifiable and generalizable explanations of empirical phenomena, I believe that *Burawoy's methodological duality is not able to reproduce and/or justify an important "group" of social concepts and theories*. Marx's conceptual distinction between necessary and surplus labor is a prominent exponent of such group.

Burawoy favorably quotes Marx's "treatment of production", according to which the workers "simultaneously produce their own means of existence (necessary labor) and the basis of profit (surplus labor), that is they reproduce the worker on one side and the capitalist on the other" (p. 18). My position could be summarized as follows:

(*a*) *Scientific status*. Since the concept of surplus labor does not conform to the empiricist–Popperian requisite of *falsifiability*, it follows that it is not scientific in Burawoy's terms. However, we can relax this requirement and assume a *weak definition of science* without *falsifiability*, in order to explore Burawoy's "models of science". **[The claim is not that <u>concepts</u> are falsifiable, but that explanations using concepts are falsifiable. Thus, when you use the concept of surplus labor (as part of a broader theory) to explain capitalist crisis, that could, in principle, be falsified in the specific Burawoyian sense of there being anomalies with respect to that explanation. The challenge then would be to reconstruct the theory in order to deal with the anomaly without rendering it a patchwork of exceptions and ad hoc adjustments. The partition of total labor into surplus labor and necessary labor is not "falsifiable" because this division can be specified by a definition.]**

(*b*) *Scientific discovery (positive model).* It is not clear that Marx was engaged in a "positive model of science". Although we can agree that Marx's concept of surplus labor is the result of a *"positive model" in Burawoy's sense* (Marx is physically detached from the phenomena, he is writing in the British Museum), it is not clear what does it mean in this context to interrogate the subjects "through intermediaries" (p. 5). Marx's concept of surplus labor is a result of a *theoretical development* of categories such as commodity, money and capital in which *direct empirical "positive" research has no role at all*.
[I agree with you: this is a logically deduced category which is part of a conceptual apparatus of logically connected concepts. But I am not sure that this renders them inappropriate for either of Burawoy's models of science, since there is no claim that all concepts have to be derived in any simple, direct way from empirical observation]

(*c*) *Scientific discovery and theoretical reconstruction* (*reflexive model*). Although Marx's concept of surplus labor is obviously a result of empirical observation (i.e. it is not an *invention* of the mind, it refers to a real phenomenon) mediated by theory, it is difficult to imagine how this concept could have been discovered or can be reconstructed, in Burawoy's terms, through the "reflexive model of science". Commodities, money and capital do not seem to be adequate objects of study for this "model of science", at least in the context of the development of Marx's concept of surplus labor (it is worth remembering that such categories are not *things* but expressions of *social relations*). **[I don't see why the fact that these concepts are about relations rather than entities is important in this specific context. Why can't a reflexive model of science contain relational concepts? It might be tougher for Glaser & Straus's grounded theory to come up with this sort of concept, since the inductivist approach to concept formation and category formation would seem to have difficulty with that sort of concept.]**

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15. Matt Nichter

Burawoy writes (Ethnography Unbound, Ch. 13, p. 281), "A generic strategy [such as utilized by grounded theory] looks for similarities among disparate cases, whereas the genetic strategy focuses on differences between similar cases. The goal of the first is to seek abstract laws or formal theory, whereas the goal of the second is historically specific causality."

I'm not entirely clear what the contrast between general laws and historically specific causality amounts to, since one can state perfectly general laws containing conditionals that incorporate contextual variations. **[I think that there are some difficulties in the rhetoric here, especially since if you look at Michael's work as a whole he does invoke some very general mechanisms across widely different settings, even f his account of the effects of those mechanisms will vary with historical context. As I've mentioned in some of the other comments, there is some ambiguity in the distinction between a "general law" and a "general mechanism".]**

Burawoy also writes (Ibid., p. 282), "The extended case method...takes the social situation as the point of empirical examination and works with given general concepts and laws about states, economies, legal orders, and the like to understand how these micro situations are shaped by wider structures."

On the face of it, this seems to be a concession to the idea that the extended case method is, in fact, after laws, however complex and conditional, and that the quest for "societal significance" is not really a distinguishing mark. That the extended case method achieves generality through the "reconstruction of existing theory" does not imply that it cannot also be after laws. Given the raw theoretical material that Burawoy says the method takes as "given," the product could still be (complex, conditional - i.e. more refined) laws.

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16. Martín Santos

Is it possible to dance two dances (criteria of definition and evaluation of what counts as "good" or "bad science") in the science home?

Michael Burawoys argues the possibility of two models of science, each with their own methodology, and most importantly, with their own set of criteria to *evaluate* what counts as "good" or "bad" science (positive or reflexive).

In line with Habermas, Burawoy suggests that there is a historical reason why it should be the case for the coexistence of two models of science: social world is constituted both as external (social forces) and internal (lived local situations) to participants. Social life presents (and works simultaneously) itself both as objectified/reified, and as lived (internal) experience. Social systems and lifeworlds are deeply intertwined and reciprocally influence each other. Although, systems seem to be about to colonize lifeworlds. In this context, Burawoy's narrative suggests, it makes perfect sense to deploy and enact two models of science/methods/techniques to better understand the dual (or multivocal) dynamics of our social world and to overcome the limitations of each of the two models of science. **[This is an interesting reconstruction of Burawoy's argument. I don't remember Burawoy drawing quite so tight a parallel between the two contrasts: (1) social system vs lifeworld and (2) positivism vs reflexive science. I am not sure that he would fully endorse this correspondence, since it suggests that proper explanations of the dynamics of social systems can best be achieved by a nonreflexive science. Does he really suggest this? Do you think it's a sensible thing to say, whether or not it is what Burawoy says? If you are right about this it would certainly help make sense of the claim that both models are equally valid, but that they apply to different kinds of causal processes and explanatory problems.]**

The Social Science field is not a monolithic one. On the contrary, as Bourdieu correctly argues, we can observe a crisis in the principles of vision and division, legitimation and domination, regarding what science is (or should be), what counts as scientific evidence, what counts as "good" or "bad" social science research. Burawoy's ideas about two models of science can, therefore, be located in a broader context of struggles of which postmodernists constitute the extreme case.

Therefore, there are no historical, logical or empirical (practical) reasons voiding *particular* researchers (like Max Weber, strategically cited by Burawoy) to work with different models of science, simultaneously or sequentially, as long as they believe their object of study is marked by a constitutive tension between external forces and internal lived experiences.

However, if the switch back, from the *individual level* (any particular social scientist) to the *collective level* (Social science as a field) we promptly observe that even if we accept the idea of competing principles of legitimation, there is domination and hegemony. This means that some groups will struggle for the *monopoly of principles of* *legitimation* regarding what counts as "scientific work", "scientific evidence", "rigorous scientific work". My thesis is that the "regimes of power" (Burawoy) in Social Science make socially and sociologically impossible the coexistence of two different set of criteria of evaluation regarding what counts as "bad" or "good" "positive" or "reflexive" work. **[Sociologically *impossible* or just difficult and contested. I think in fact that it is possible to have a kind of unstable pluralist equilibrium of reflexive and positive science, hermeneutic and explanatory science, even postmodernism and social science of whichever model. This is sort of contested coexistence rather than peaceful coexistence, but it nevertheless creates a space for both sorts of work.]** I will present and example. Annette Lareau is now a well respected Sociologist of Education. Her recent ethnographic work "Unequal Childhood" has been widely acclaimed in the sub-field of the Sociology of Education, which is dominated by "positivist" sociologists. She, like J.Katz, speaks the language and principles of the "positive" science. She acknowledges that her sample is not "representative" because it did not capture all the variability of black middle class families, that it cannot make causal claims because she couldn't control for different variables of interest, among other things. It is only then, when she proposes possible mechanisms to explain some puzzles in quantative research, that was granted recognition, that was consecrated. What would have happened if she had claimed as legitimate "intervention", "process", "structuration" and "reconstruction" as the defining criteria of a good work? She would not received all the acceptance she has got, I state. **[Maybe not <u>all</u> the recognition, but possibly still considerable recognition. There are certainly writers who reject the positivist idiom whose work is still taken quite seriously and gets lots of praise. Michael Burawoy would certainly be a case. I think it is important to avoid seeing these traditions as more polarized and mutually hostile than they are. What you say may be true for full-blown post-modernism, since it so hostilely rejects and disparages science as such, but reflexive science is more accommodating.]**

In this context, a "Baskar-like" question is crucial: what must be the case for the coexistence of *two equally dominant but different set of principles of legitimation and evaluation* of social science research? Under what conditions this could be the case?

I will finish with a different but related question: what must be the case *for integrating into a unified framework the criteria of "positive" and "reflexive" science*? **[I am not completely clear on what you mean by a unified framework. In some ways this is precisely what Burawoy is proposing: he provides a framework in which the central criteria for good research within each model of science are brought into alignment with each other. For example, by pairing "Reactivity" with "Intervention" he is able to show how each of these principles work, how each helps to constitute a criterion for relevant knowledge and how each involves gaps between promise and practice. This alignment would facilitate a reflexive scientist using data from a survey (by understanding the reactivity problem and the knowledge gap it entails) and also a positivst scientist using data from a reflexive ethnography (by understanding the intervention problem and how this generates knowledge-enhancing perturbations of social processes. Isn't this a kind of unifying framework?]** I think that the systematic struggle against *bias*, which is the reason why positive science prefers to construct the object as external to the observer, should be reconciled with the idea of *intervention* in social reality and the "visibilization" of power structures (Burawoy). We should participate and get involved in the flux of social life we study, but the struggle against bias should be there, even if unfulfilled. This is possible only if we acknowledge that bias has two sources: *power structures* and the *"hypercomplexity" of social life*.