Preliminary Note: I wrote quite extensive comments on your interrogations this week, but there are some specific issues raised in some of the interrogations that I skipped because I couldn't think of anything sensible to say. In part I think this was because I had not reread all of the pieces for this session and therefore did not have freshly in mind some of the details in the pieces. You should feel free to bring up in the seminar anything which I skipped over.

1. Matt Desmond

I have two lines of questions for this week. First, can concepts be appropriated outside of their conceptual framework? If we side with Pawson's idea that concepts are only understood relationally (p. 227), then borrowing a concept from a set of theoretical relations is incredibly problematic. "Scientists are confident in their usage of particular terms," Pawson writes, "not because they have some one-and-for-all conceptual anchorage, but because they are entranced in formal networks of concepts" (p. 236). If this is the case we cannot, as Wright demonstrates, analyze class without also taking into account class formation, class struggles, and class consciousness (pp. 28-31). Because of "theoretical constraints," then, we cannot treat a theoretical framework like a cafeteria bar, scooping out whatever we please to fit our analysis. However, if you side with Weber and see concepts as ideal-types, which can serve their purpose of "ordering reality" embedded in networks of concepts or free-standing, the cafeteria approach to concepts is not problematic. Many social scientists take the cafeteria approach. Take Bourdieu, for example, whose concepts of "habitus" and "cultural capital" have been appropriated by many scholars but almost always outside of their conceptual framework (few ever utilize "capital" in conjunction with "field" and "habitus" even though through a Bourdieuian analysis one concept makes no sense without the other). Although I have not fully thought this through, I am more apt to side with the relational view of mobilizing concepts, as the cafeteria approach tends to dilute analytical clarity and explanatory power, and more importantly, concepts exist only through relationships, and not taking these into account empties the concept. **[Can't one, perhaps, modulate between these two modes of conceptualization? There are a number of possibilities here, I think. One is that theoretical frameworks are overlapping rather than hermetically distinct: Marx and Weber, for example, share quite a few elements in the relational configuration of class analysis, and this partial overlap allows for borrowing. A second sort of possibility is that theoretical frameworks have areas of tight interconnection and loosely coupled conceptual structures, and that one can easily borrow concepts into the loosely structure regions of a theory. Also: if we see theories as theories about specific causal mechanisms, then one can important concepts from the cafeteria for the inclusion of mechanisms outside of a given**

theories coherent structure. Thus Marxists can important gender concepts for the study of the interaction of class and gender, since Marxism does not itself contain a theory of gender. It is only if theories attempt to be totalizing, fully integrated and complete that there would be no room for the menu even if one sees concepts as relational.]

Second, are concepts ends or means? To Weber, concepts are means. They are "primarily analytical instruments for the intellectual mastery of empirical data" (p. 106). They are not hypotheses, but offer "guidance to the construction of hypotheses" (p. 90). On the other hand, Stinchcombe claims that concepts are ends; or rather more precisely, that the aim of science is to constantly refined its concepts to "adequately represent the phenomena of the world" (p. 40). To him, all concepts are hypotheses, whether this is explicitly recognized or not. Similarly, in Wright's chapter, the concept of "contradictory locations" seems to be an end: it attempts to solve a problem. The concepts-as-means view teeters more toward instrumentalist/utopian philosophical underpinnings, while the concepts-as-ends view favors realism. Several question flow from this basic distinction: If concepts are means, where do they come from? Could we ever have a "biography of a concept" through an idea-typical framework? Additionally, must concepts be one or the other? Can we not fathom a social science that utilizes concepts as both the ends and the means? **[This is the view I would have: there are a variety of different *kinds of concepts*, and it would impoverish our explanatory potentials to refuse to allow certain types into our research. There are both concepts as ideal-types and concepts as realist abstractions – both are needed and serve different purposes.]** Finally, if a concept is useful, must it be empirically-grounded (I have in mind psychoanalytic concepts here)?

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2. GOKCEN COSKUNER
1) How objectivity is attained in social sciences? Weber suggests that "objectivity" of social sciences depends on the fact that the empirical data are always related to the evaluative ideas which alone make them worth knowing and the significance of the empirical data is derived from these evaluative ideas (p. 111) he also says that the objective validity of all empirical knowledge rests exclusively upon the ordering of the given reality according to categories which are subjective in a specific sense. Weber's view of objectivity is similar to those of feminists who argue that the consciousness and critical attitude of one's stand point results in objectivity. Is this a widely accepted view of objectivity in social sciences, are there alternative views? **[It seems to me that the stipulation of value-relevance is the way of assuring that social science is <u>meaningful</u> and worth doing, but not that it is "objective". The fact that observations are inherently value-laden does not imply that they acquire they objectivity through value-laddenness. I think "objectivity" has more to do with some criterion for evaluating the success of the scientific claims in identifying real mechanisms/causes/explanations.]**

2) The second set of questions I have is on Weber's notion of ideal type.

- Weber talks about the selection of the traits which are to enter into the construction of an ideal-typical view but does not really provide an explanation on how one selects these traits. **[I think the principle is this: (a) given the value-concern that motivates the ideal-type, you then (b) logically order the traits on some principle. Thus, for example, the traits of the concept "rational-legal bureaucracy" are derived from the logical requirement of an administrative procedure being rational.]**

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- Is the ideal type a totally subjective construct formed by the researcher or does it rest on some type of social consensus? **[An ideal type is an idealization of some process or phenomenon. A frictionless plane is an ideal type plane within Newtonian physics. No actual plane is ever frictionless, but the idealization enables us to conduct some thought experiments and think through theoretical the implications of various causal processes. As a "thought experiment" it is something that exists in the mind and not in the world, and in that sense it is "subjective." As a *meaningful* thought experiment within a community of scholars it reflects a social consensus of some sort about what sorts of thought experiments need to be conducted.]**

- Weber defines ideal-type as an attempt to analyze historically unique configurations or their individual components by means of generic concepts (p. 93). Can we talk about ahistoric (non historic) concepts and their analysis? **[This is very much like the medical metaphor I used in class: when you diagnose a particular patient's symptoms you are investigatihng an "historically unique configuration" but using diagnostic tools (concepts in the present context) which are not historical unique in their relevance.**

- Does Weber's ideal type notion take into account the "emic" meaning/s?

Let me clarify what I mean through an example. The term "metrosexual" was coined by Mark Simpson. The original definition was as follows: "The typical metrosexual is a young man with money to spend, living in or within easy reach of a metropolis — because that's where all the best shops, clubs, gyms and hairdressers are. He might be officially gay, straight or bisexual, but this is utterly immaterial because he has clearly taken himself as his own love object and pleasure as his sexual preference. Particular professions, such as modeling, waiting tables, media, pop music and, nowadays, sports, seem to attract them but, truth be told, like male vanity products and herpes, they're pretty much everywhere." When mark Simpson first wrote about metrosexuals his intention was to criticize the capitalist system that created itself a new consumer. But later on the term took on rather a different meaning: "A dandyish narcissist in love with not only himself, but also his urban lifestyle; a straight man who is in touch with his feminine side."

My question in relation to Weber's ideal type is then if one wants to formulate an ideal type of "metrosexuality" does he/she take into account the definition of the word used in popular culture, does he/she need to take into account how men who would be classified as metrosexuals by marketers think of the term and whether these men see themselves as metrosexuals? **[Note that in the original statement the term used is "typical" not "ideal typical". This suggests that the concept was meant more as a way of identifying a <u>modal</u> type or, perhaps, an <u>average</u> type, but not an ideal type. To count as an "ideal type" the concept must be specified within a theory in which it is possible to specify the logical component of some element of the theory. It isn't clear from what you've said whether the theory of marketing and sexuality is developed in a way that uses ideal-types. One other point: when a word has a popular culture meaning there is always a challenge in giving it a <u>technical content</u> for purposes of scientific investigation. To do so requires that there be some compelling theoretical reason to construct a logically coherent, precise concept. I am not sure that is the case in this instance.]**

3. Fabian Pfeffer

Does the interplay of theory and empirical reality in the process of concept formation cease to take effect and is it replaced by pure empirical testing once this process has been completed?

Wright expresses the interplay of theory and empirical reality in the process of concept formation with the idea that the "production of scientific concepts operates methodologically under both theoretical and empirical constraints" (p.20). He illustrates vividly what Hempel tries to express when he rejects "operationism" (which basically equates scientific concepts with "operational definitions") and calls for "systematic import" (p.98) of theoretical context. As Wright's own past struggle between internal theoretical consistency and external empirical consistency of his class concept shows, this method helps to avoid two traps: 'Theoreticism' (liberation from empirical constraints), as e.g. committed by Pawson going to bat for formal models / formal reasoning or Weber when constructing his ideal types. And on the other side 'empiricism' (liberation from theoretical constraints), as e.g. committed by Stinchcombe when putting forward his version of operationism.

I have no doubt about the superiority of the method put forward for producing a new scientific concept. That is why my question goes beyond this process and asks for the role which theory and empirical reality play in judging the final product of concept formation: the concept itself. Here, I assume that the constant "back and forth" between theoretical and empirical constraints has a natural endpoint.**[In a sense there is no "endpoint", at least if by that one means a stable equilibrium in which one knows for sure that no further iterations are needed. There may be a kind of pragmatic endpoint – a point at which one says, "this is the best I can do, let's get on with business" – but that is always a provisional statement, and concepts can continue to**

get revised in light empirical results and theoretical reconstructions of using the concept.]

More exactly, two possible endpoints seem conceivable:
a) The method described above yields adaptations of the concepts which finally make it empirically consistent. Its architect rubs his hands and leans back.
b) Even after adaptations the concept fails to meet the demand for empirical consistency. This "ultimately leads to a process of transformation of the more general theoretical framework" (Wright, p.24). Here we leave the field of concept formation and turn to the field of theory formation.

As the latter case cannot be regarded as a success in concept formation, we are left with the former case. **[There is a sense in which provoking a process of theory reconstruction can be viewed as a "success" of the effort of concept formation. That is, the point is to advance knowledge, and provoking theory transformation is part of that process. It may be that the repeated attempts at concept formation and reformation are necessary steps in the provocation of theory reconstruction. It can also be the case, of course, that the theory so reconstructed might provide a more comfortable home for the concept that provoked the theory reconstruction. That is, the results of the tension between a given concept and the theory within which it functions may be to reconstruct the theory in ways that are more compatible with the concept. Concept formation may be the leading edge of theory reconstruction.]**
One might therefore conclude that at the very endpoint of concept formation it is only the empirical constraints that decide about the success of a concept. Inner-theoretical consistency and elegance do not play a role anymore and the former symmetry between theoretical and empirical constraints makes way for pure empirical testing. Correct?

If so, another question arises: This check against empirical reality will in most cases be carried out 'competitively', i.e. competing concepts* will serve as a yardstick. But which ultimate empirical measure decides then about the winner? Wright's or Goldthorpe's class concept - only a question of explanatory power? Of precision of measurement? Or all the same: unsolvable theory adjudication - and thus "the curtain falls, all questions open"? (*Bert Brecht, "The Good Person of Szechwan"*)

* Here I refer to concepts stemming from different theoretical frameworks. The choice among concepts of the same theoretical framework might potentially be solely based on theoretical arguments. **[The competition among alternative definitions of the same "concept" *within* a theory are just as likely to be resolved empirically as theoretically. Theory may underdetermine the definition of a concept – i.e. alternative definitions may all be consistent with the theory – but the theory may suggest specific empirical tests of the concepts thus allowing adjudication. It may, ion contrast, be impossible to adjudicate between rival concepts in rival theories because the rival concepts may be specified with respect to different explanatory projects.]**

4. Wayne Au

At the core of the readings this week, for me at least, seems to be a dialectical tension between universal/specific or the abstract/concrete in concept formation. Clearly it revolves around the "exactness" of measurement and how that relates to our conceptual development.

It seems assumed or implied that in mathematics and the "hard" sciences (physics, chemistry, etc.), experimental measurement and calculation are exact methods that we use to understand the world. This assumption seems faulty to me for a few reasons. Within math and science, nothing is totally exact. We do not have a complete calculation of pi, for instance, yet we can apparently calculate the area of a circle. Likewise in science, we are constantly having to essentially gloss over "scientific error" of greater or lesser magnitude (yet we still agree with their truth claims), and we still can't pin down particles in basic, mechanical motion – the best we can currently do is provide quantum probabilities about where a particular particle is going to be and its trajectory (yet we believe they are there and that they are indeed moving). This issue is dealt with extensively in the readings in the discussion surrounding measurement of lengths. While we may have the one metre stick in France or where ever, it is impossible to exactly reproduce that for common measurement elsewhere. Instead we use our own rulers and measuring tools that at best are close approximations. This means that, generally, measurement and the concept of length are 1) socially defined and that 2) we have essentially and tacitly agreed that a certain amount of conceptual imprecision (lack of concreteness) is okay/acceptable to us. **[I wouldn't quite put the issues this way. There is no <u>conceptual</u> imprecision in the measurement of length, there is only practical imprecision in the application of the concept to a concrete object. The concepts themselves are perfectly coherent and precise. I also don't think that it makes sense to say that the concept of length is "socially" defined, except in the trivial sense that all definitions, since they occur in language, are social. The degree of *acceptable* error in measurement for given purposes is certainly socially defined, but that is different from saying that measurement as such is socially defined. Perhaps I am not quite understanding your point here.]**

This to me is at the core of concept formation for the social sciences then. **What level of imprecision in concept formation is acceptable and upon what criteria do we set that acceptability?** It seems that if we relegate ourselves to working with every last specificity and facet of something we run the risk of 1) never actually completing any research and 2) falling into postmodern relativism because everything becomes so specific that we can draw no conclusions and make no truth claims. **[Precision and logical coherence of concepts does not mean specific in the sense of detailed-concrete. Thus, we can have a precise, logically coherent concept of capitalism without implying that any concrete society is perfectly capitalistic. The concept is precise, but the concrete world is always determined by a multiplicity of mechanisms, and thus a given concept is not adequate for a full description of that world.]** Likewise, if we give in to remaining too broad and over-general in our concept formation, we end up making truth claims that are, frankly, not true or at least not grounded in the material world. **[Claims about capitalism can be true, and relevant to**

a concrete situation, even if it is also the case that many other concepts are needed to understand the case. This is like saying is the chemical compound H2O a valid concept for analyzing the properties of Lake Mendota even though the Lake contains thousands of other compounds and for some questions those compounds may be of considerable importance in explaining something. No Lake in the world consists only of H20, but this does not impune the realist character of the concept or its explanatory power. If you want to answer the question: will Lake Mendota freeze if the temperature is -20 degrees Fahrenheit for three weeks, then all you need to know is that it is H20. But if you want to know, will I get sick if I drink it, you need to know other properties as well.

5. Elizabeth Holzer

Pawson offers a relational approach—the formalist strategy—as a seemingly nice way out of the debate between operationalists and hermeneuticists over the nature of concepts. Operationalists think that we can root concepts in the empirical world so deeply that we might make scientific terms with clear, precise, and closed definitions. Hermeneuticists argue that language intervenes—language is so mutable and unclear that no one could ever craft closed concepts out of it. But concepts, Pawson argues, need to be understood not in isolation as theorists in these two schools do, but as part of a broad network where "connections are made between relatively speculative ideas and certain other concepts which are understood well enough to control and measure" (1989:225). This sounds nice, especially if the pragmatic "well enough" attitude translates into frequent reexaminations and developments of these certain other concepts. (Though I do think that formalists would have to make an effort to avoid Stinchcombe's stultifying road to scientific advancement via "progressively redefining its concepts until they accurately represent the phenomena of the world" (1968:40)—it seems like this could flow pretty naturally out of the formalist tradition).

But what I don't get, and what I would like to discuss in class, is what Pawson's fascination with mathematics is all about. What does he mean by mathematics (he seems rather saddened by probability), how exactly does he want sociologists to use it, and why does he think that it is superior? **[I think Mathematics is given special status in Pawson because of its capacity to provide clear criteria for the coherence of theoretical constraints on the production of concepts. He develops this idea through his gentle critique of my efforts to specify logical constraints on the concept of class that are imposed by Marxist theory. I do this through what he calls "ordinary language reasoning" – basically discursively identifying a set of theory-based constraints on concepts and describing the ways in which they block certain solutions to the problem of the "middle class." He argues that there is too much slippage between theory and concept in this kind of casual, ordinary language strategy. That "embryonic proto-formalism" slides into "pseudo-formalism." Mathematicization – by which he means formal-logical derivations of conclusions from axioms – avoids this and thus establishes much less arbitrary ways of evaluating concepts with respect to the theories in which they function.]**

On a separate note, if anyone would like to argue about the value of "eclecticism," I'd be up for that—Erik defines it as "the refusal to worry about theoretical coherence. Old concepts are modified and new concepts are adopted from various theoretical frameworks in an ad hoc manner with out regard to their compatibility or their integration into a general framework" (1985:23). I think standpoint epistemologists who chose not to privilege one particular standpoint could be considered eclectics—but I don't hold it against them. Rorty (I think) presents theoretical frameworks as tools, good for different functions. (If I had a clearer sense of how Weber envisioned the relationship between ideal types and theories, I'd bring him in too, but alas, it's a bit fuzzy for me). Given that the social phenomena are infinitely complex and infinitely varied (as Weber would say) and researchers are limited in what they can study (as in that elephant-and-the-blind-men metaphor), why is theoretical compatibility a feasible goal in explaining complex social phenomena? **[Nicely posed question. I think my position is the following: the optimal scientific *community* is one that contains both eclectics and coherentics (pardon the expression). Theoretical Eclecticism, means you pick and choose from different theoretical frameworks and traditions without worrying how things fit together: a little bit of rational choice, feminism on gender, Marxism on class, status attainment for socialization and mobility, etc. This strategy, I think, is likely to be more successful if it occurs in an intellectual environment in which some scholars are not eclectic, where they believe that pushing for increasing coherence and conceptual integration into systematic frameworks is the way to advance understanding/knowledge. This means that we should value *collective pluralism* but this does not mean it is optimal for participant to be uncommitted to a particular perspective. (This is like saying we want theory to take an explicit standpoint and systematically develop it in a coherent way, but for this to occur in a community of multiple standpoints where some people will draw from multiple standpoints in making their arguments.)**

6. Ana Cristina Collares

This week we have several approaches for the nature of scientific concepts and the relationship of these concepts with theory (can I say that we have "conceptualizations of scientific concepts"?). Pawson summarizes these accounts in four "theories of meaning", employed in different sociological attempts to create concepts. One of my questions is about Pawson's own position towards these theories, and his understanding of how sociological investigation can create scientific concepts.

Pawson classifies himself as a critical realist. He is therefore asking himself about the possibility of producing closed systems of empirical experimentation in sociology, which he concludes that is impossible, as seen in this passage where he criticizes Boudon's model of social stratification:

"Firstly, there is the problem that the model is (necessarily) probabilistic and so makes essentially qualitative claims about the relationship between variables. Secondly, the business of the empirical confirmation of these models cannot be a matter of

manufacturing direct physical replicas of mathematical models since we can only weakly approximate the conditions of experimental closure." (p. 251-52)

Given this perspective, he asserts that he prefers formal reasoning than descriptive detail. What is not clear to me is <u>on which grounds this formal reasoning is to be developed</u> and, if the answer for this is the 'abstract calculus' that links networks of concepts, <u>how these concepts are produced in the first place</u>? If we are to take the example of Boudon's model building, how are we to escape from making theory that resembles the "procrustean bed" mentioned by Weber, i.e., one that forces reality into our abstract categorization? **[The of "forcing" reality into "abstract categories" is only a problem if there is no mechanism or process for transforming the abstract categories as a result of the analysis of the resulting observations within the theoretical framework containing those categories. That is, if there is self-reinforcing closure in which: the theoretical framework generates the abstract categories which generate precisely those observations ("forced into the categories) needed to reinforce the existing categories and the larger theoretical framework. But, in general, there is no automatic closure of this sort in science. The abstract categories can generate observations that contradict the framework within which those categories are located.]**

The second question relates to the "path" followed by science to consolidate scientific concepts, as suggested by Stinchcombe and by "The Biography of a concept". Stinchcombe seems to suggest the following sequence:

* common sense knowledge => causal theory => concepts => measurement mechanisms => "empirical" tests => re-elaboration of concepts. *

Hence, science is created by abstract reasoning (causal theory) that, nonetheless, is grounded in experience (common sense), but it develops and becomes more precise through the mediation of experience (experimental testing), that refines the concepts.

Erik Wright, however, argues that concepts can come from two different kinds of framework: common sense and elaborated theory.

In both schemes (Stinchcombe's and Wright's) the "refinement" of the concept is empirically mediated by the use of data gathering and measurement techniques. My question here is about this mediation (and is suggested by a comment made by Fabian Pfeffer in a brownbag last week): <u>Given the role of empirical mediation in concept formation, can these measurement mechanisms be considered neutral instruments that can be used in different theoretical frameworks, to test different kinds of hypothesis</u>? **[I don't image that the theory behind measurement is any more "neutral" that the theory behind the causal processes the measurements are supposed to reveal. That is: every theory excludes some things and includes others, which makes the theory non-neutral in some sense. But I don't know if this has any profound implications for the relevance of observations/measurements, understood within some measurement/observational theory, for particular theoretical problems.]** Or measurement and data gathering techniques have also a "causal theory" behind them that make them fit to only very specific uses within specific theoretical frameworks? **[While it is true that there must be a theory behind any measurement technique – that is, a**

theory of how the observations we call "measurement" are generated byan underlying phenomenon – it is not obvious that this implies that a given measurement technique necessarily applies to only certain theoretical frameworks. Of course, it is certainly the case, that certain kinds of data are only relevant for certain kinds of questions. Survey data is inappropriate for studying the effects of the Federal Reserve Bank's interest rate policy on home mortgage rates. But I am not sure why the theory of the causal processes that generate particular kinds of measurements implies appropriateness for only certain theoretical frameworks assuming that the data wewre relevant to the questions generated by those frameworks.]

7. Brett Burkhardt

Pawson and Hempel both address the idea of a conceptual network as a way of understanding and explaining phenomena. Pawson's discussion of core concepts emphasizes the role that they can play in generating theories and knowledge. One particular way of increasing our knowledge is through "the application of the same formal system to new substantive fields (237)," or the "displacement of concepts" (to use Schon's phrase). Another is through "the extension of the formal system by mathematical innovation (237)."

I am unclear on whether these different uses of formal systems are mutually exclusive, and this fact may bear on my real question for this set of readings. When we discuss the application of formal systems (or of core or knot concepts) to fields outside of their originally intended application, are we discussing mechanisms or metaphors? Does "concept displacement" or extension of mathematical models provide us merely with a metaphor or heuristic for helping us think about different phenomena? Or do we make claims about real mechanisms when we extend our concepts or models to new realms?

A particularly confusing example of this ambiguity is found in Pawson's discussion of Boudon's claims of exponential decay in survival rates in education systems (250-251). If there is in fact a common phenomenon of exponential "decay," are we to understand this as a metaphor used by Boudon (and Pawson) to help readers think about the data, or as a claim that there are phenomena in social systems (in this case, education systems) that actually mimic in some real way mechanisms that are at work in decay in physical systems? **[This is an interesting set of issues, and I don't have a clear answer. Clearly the extension of the idea of "exponential decay" from physics to sociology does imply – at least initially – the use of a metaphor: in physics the exponential decay description of a phenomenon is tightly linked to a mechanism that explains that specific trajectory of change. I suspect in sociology it is used to describe a trajectory that has the same basic appearance, but probably not such a determinate generative mechanism, and thus it is meant to be suggestive. But, in principle, it could be the case that there is some social process that has the same mathematic form and is generated by a mechanism that entails this form, and thus the metaphor could become a proper realist concept. The relationship between**

metaphors, concepts, and mechanisms is interesting – sometimes it seems that we muddle through with language that gives us apparent insights and metaphors help us with this, but then nailing down the insight requires transforming the metaphor into something else.]

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8. Dan Warshawsky

This week, a second, and equally important critique of positivism has been focused upon by the various authors. Is separating science into specific assertions whose validity is dependent upon definitions and logic outside the realm of experience a valuable exercise? This question seems to be one of this week's readings' central questions.

During our initial week's analyses of positivism, we criticized their lack of interest in the researcher as a component of the process of interpreting and analyzing surrounding phenomena. As we have moved through various epistemologies, such as critical realism and standpoint epistemologies, the level of focus on us, the social scientist, has increased significantly. Although some of our class members might differ on issues of positionality, reality, and self-reflexity, our general consensus is that the researcher is an integral component of the research process. **[Nice summary statement of the trajectory through the first three sessions]**

Within the context of these previous week's readings and epistemologies, I would emphatically state the answer to the original question is a resounding, "No." However, the idea of 'concept formation' is a central issue for philosophy of social science that has not been directly analyzed as of yet in our discussions. We need to determine what the roles of specific terms are in concept formation. Are their proto-terms and proto-concepts that are then utilized for our specific interrogations in academics and in daily life? Again, I believe this a central question for this week's discussions.

Throughout our readings, each author seems to emphasize the role of social construction, although in different ways. Most interesting to me is Hempel's analysis:

> Not every term in a scientific system, therefore, can be defined by means of other terms of the system: there will have to be a set of so-called primitive terms, which receive no definitions within the system, and which serve as a basis for defining all other terms. This is very clearly taken into account in the axiomatic formulation of mathematical theories. In each of the different modern axiomatizations of Euclidean geometry, for example, a list of primitive terms is explicitly specified, and all other terms are introduced by chains of stipulative definitions that lead back to expressions containing only primitive terms. (Hempel 88)

Although I understand the idea that most analytic concepts are dependent on some other idea conception, I am not certain if proto-terms or proto-concepts actually exist. And, if they do exist, what is their connection to social construction. Thus, in contrast to

my original emphatic "yes" to the first question (first paragraph), I am hesitant as to how to proceed with the second question (proto-terms, proto-concepts, and their relationship to social construction).

Hempel, among other authors, continues to explain that concept formation and theory formation must be strongly interrelated. This implies that one's initial position in the research process is integral to the concepts they refer to and utilize in their analysis. Secondly, theory formation is not only directly connected to the concepts one utilizes, but it is these concepts that construct theory. Therefore, the role of positionality, concept formation, and theory formation are inextricably connected.

A third important concept presented throughout the readings is the role of 'operations.'" What is the role of operations as they relate to concept formation? Are they synonymous with conception formation? I believe there cannot be a synonymous relationship between operations and concept formation. Why? Although there are many reasons, it seems that there can always be several possible criteria of application for a term based on different set of operations. There are only 'partial interpretations' possible with each set of operations. Hempel adds some clarity:

> Here, then, the conception of the terms of a theory being individually interpreted by a finite number of operational criteria has to be abandoned in favor of the idea of a set of bridge principles that do not interpret the theoretical terms individually, but provide an indefinite variety of criteria of application by determining an equally indefinite variety of test implications for statements containing one or more of the theoretical terms. (Hempel 100)

In all, I have three main questions. Is separating science into specific assertions whose validity is dependent upon definitions and logic outside the realm of experience a valuable exercise? **[I don't think that there are important scientific *explanations* whose validity is *solely* dependent upon definitions and logic outside the realm of experience, but it is possibly true that every scientific assertion requires at least *some* definitions and reasoning that is "outside the realm of experience."** Are there proto-terms and proto-concepts that are then utilized for our specific interrogations in academics and in daily life? **[I don't quite remember what "proto-terms" and "proto-concepts" refers to – I didn't reread the readings (always a mistake!) for this week. If a proto-concept is a term used in a description or explanation that is not fully elaborated or given technical precision within a conceptual framework, then these are absolutely indispensable in social science. The descriptive demands in research go far beyond our technically anchored menu of concepts.]** What is the role of operations as they relate to concept formation? (Are they synonymous with conception formation?) **[I would distinguish concept formation from operationalization of concepts. Concept formation concerns the elaboration of definitions that logically specify the interconnection of a specific concept in a network of concepts or a conceptual framework or a theoretical framework (take your pick – these are similar formulations). Operationalization concerns the practical task of how to generate observations relevant to a particular concept. Operationaliztion is also a conceptual task – i.e. the formulation of the specific criteria that will be used to**

specify some abstract concept – and it implies a causal theory about how the observations based on those criteria are generated. But it is distinct from the demands of formulating the formal definitions of the concepts themselves and their logics-of-interconnection.]
Although these questions are strongly contested among academics, I believe we can bring some clarity and possible consensus to these issues in our upcoming discussions on concept formation.

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9. Mark Cooper

I think it would be useful to briefly review the four "theories of meaning" that Pawson explores before his discussion of formalism. It seems that each of these four theories contain within their description of a concept a set of assumptions that roughly corresponds to the general perspectives discussed in the last three sessions. While Pawson mounts a critique of both operationalism and reconstructionism, it is less clear to me why he necessarily rejects contestabilism in favor of formalism, except for its capability to offer a more systematized or practical description of scientific discourses. This relates, on a different level, to Wright's discussion of adjudication between rival concepts including his statement that, "there is no guarantee…that a satisfactory concept *can* be produced within the constraints it faces." The lack of satisfactory concept formation here does not, however, seem to be the position that contestabilism rests upon. Wright's discussion of "double adjudication" and the existence of empirical constraints offer a practical means of escaping contestabilism, yet again, it seems that there is a difference between what Wright calls "eclecticism" and the contestabilist position. **[I think what Pawson dislikes in contestabilism is not simply the fact that there are no guarantees, but that it implies inevitable *arbitrariness* in the definition of concepts. That is, when one uses informal discursive reasoning – what he calls ordinary language reasoning – to specify concepts, rather than formal deductive reasoning, there are always multiple possible derivations all consistent with the broader framework. This is his objection to my specification of the theoretical constraints on the concept of class derived from Marxism – there are too many different ways that one could specify these constraints and thus the specific conceptual choices/definitions are not really "derived". Another way to put this is that the definitions of specific concepts are underdetermined by the theories within which they function. This is what opens the possibility of contestation at the theoretical level (as opposed to contestation over empirical implications of agreed-upon concepts). I suppose I would agree that if it were possible, formalism is a better way to produce concepts – better in the sense of less arbitrary. But in social science contexts you may have to give up to much to get there.]**

Second, I would like to explore whether or not there is a fundamental difference between the process of concept formation in experimental research and empirical (non-experimental) research. Stinchcombe approaches this question in the section on the conceptualization of variables, though it is unclear if the statement that, "our measurement of any concept improves as our theories of its causes and effects improve"

is a given or a contingent process. **[I think that this is a general point, applicable in both experimental and nonexperimental contexts, it is just that it may be harder to know when our "theories of its causes and effects improve" in the nonexperimental settings. But it would always be the case that our capacity to mobilize evidence/observation (i.e. measurement) of a concept is better when we have a good theory of the effects of the concept than we have a weaker theory of those effects, since to measure X depends upon the belief that the observations we take to indicate X are produced by X. The stronger are the grounds for that belief, the better will be our observations.]**

10 G.C.

 "…I am not arguing that class structures define a unique path of social development. Rather, the claim is that social structures constitute the lines of demarcation in trajectories of social change. There is no teleological implication that there is a 'final destination' toward which all social change inexorably moves"(Wright, Classes p.32).

What, precisely, is the theoretical element involved if one *were* to argue teleologically? Weber states that

"…the causal relationship between the historically determinable idea which governs the conduct of men and those components of historical reality from which their corresponding ideal-type may be abstracted, can naturally take on a considerable number of different forms. The main point to be observed is that *in principle* they are both fundamentally different things"(Weber, "Objectivity" in Social Science p.95).

He goes on to say that "those 'ideas' which govern the behavior of the population *of a certain epoch*"(ibid., my emphasis) can be formed into an ideal type and gives the example of Christianity in the Middle Ages. It is one thing, however, to define an idea-, or belief-related cause-and-effect relation that is a past event; it is another thing when the historical process under examination is unfolding in the present. In Classes, favor is given, in explanation of class struggle, to definition in terms of the nature of the agents in conflicts, over effects definitions and objectives definitions (32-33). It seems to me that this sort of definition is distinguished in another way, which is related to the teleological aspect of explaining social change: an account of the *nature* of agents, of protagonists in struggles, allows for a much greater degree of further deduction than do accounts of effects of struggles and objectives of struggles (struggles, that is, entirely contained within *past* history, having already yielded up its empirical raw materials). The latter two have a more de facto character; the *nature* of agents, however, could be used to describe what people *would* do in any given historical situation, including our own, present situation. Do accounts of the nature of a protagonist, or groups of protagonists in class struggles permit the inclusion of themes on which it is more difficult to make scientific inference, such as "the perfectibility of man" or the "unsociability of man" found in classical political theory? **[I had a little trouble completely tracking your arguments here. I am not sure why defining classes in terms of the "nature" of the actors-in-**

conflict in any way suggests a teleological way of arguing or a teleological form of theory. I take a teleological theory of history to be one in which history has a known destination, and that this destination helps to explain the trajectory of social change that leads up to it. This is how teleology is used to explain individual behavior: someone accomplishes a goal. We imagine that the as-yet-not-accomplished goal nevertheless existed from the start of the action (in the form on an "intention") and that in this sense the end of the process helps to explain the actions that lead up to it. The mechanism is intentionality. Reasons can be causes, as some philosophers like to argue. But it is much less clear how one can deploy the same argument for longterm macro-structural transformations in society since they are not intended by the actors from the start. I don't see how specifying the nature of people and explaining social change and struggle on the basis of that nature changes this point. That nature need not imply some ultimate end of historical change embodied in that nature from the start. Am I missing something in your question?]

11. Matías D. Scaglione

Concept formation, history of thought and land-owners

1. In Chapter 2 of your book *Classes*, "The Biography of a Concept: Contradictory Class Locations", you proclaim that although you "feel that the theoretical conditions elaborated below are consistent with Marx's general usage…" you "will make no attempt to validate this claim" (27). My first question has to do with the importance of the history of thought in the process of concept formation. Why do you think it is not necessary to demonstrate that we are not misrepresenting the original concepts or theories that we are using to "form" new concepts and theories? **[The history of thought is certainly important explaining why it was in the 1970s – and not the 1950s, for example – that people were worrying about these issues, proposing these solutions to specific conceptual problems, and so on. That is: if you want to explain a conceptual innovation, the historical development and context of the idea-production process is important. Nevertheless, I don't see why the history of thought needs to enter into the interior of the process of concept formation. If I propose a number of theoretical constraints from what I take to be Marxist theory and you feel that this is not faithful to what Marx wrote, that could be a perfectly valid and interesting point about the differences between Wrightian class theory and Marx's class theory. But it in no way demonstrates the superiority of Marx's class concepts. It could be that only by misrepresenting Marx's ideas – the "original concepts" – can they be rendered analytically powerful in explaining contemporary problems. What you would need to show is that specifying the constraints in a different way – according to the "correct reading of Marx" – actually yields a "better" theory *in the context of the double adjudication process I describe*. That is, of course, a very demanding task.]**

2. Throughout the same chapter you specify "conceptual constraints" imposed by the "abstract theory of classes in Marxism" (27). The fifth conceptual constraint says that "The objective basis of these antagonistic interests is exploitation". Although this

assertion is appropriate for the "polarization" capitalist-proletarian, I think that it became problematic once we analyze the context in which such polarization takes place in Marx's own theory. I interpret that the *two classes model* capitalist-proletarian is valid only in the context of *Capital I*, that is, in the analysis of the process of production of capital. Once we move to the circulation of capital (*Capital II*) and to the process of capitalist production as a whole (Capital III) we find another "big class", the *land-owners*. Even assuming that this latter class has disappeared in advanced capitalist nations, I would like to know how this class fits analytically in the fifth conceptual constraint. Is there a *causal relationship* between the affluence of the land-owner and the poverty of the worker? **[The affluence of the landowner has to come from the appropriation of surplus, since the landowner doesn't work. The mechanism may be, of course, rents from the land. But those rents are simply a device for grabbing part of the surplus, and thus this would count as exploitation, albeit from an indirect circuit – although perhaps directly from tenant farmers, or sharecroppers, etc. in some agrarian contexts. If there is no causal relation between the affluence of the landowner – if they are affluent because they live off the land through their own labor – then this would not be exploittion. But that is not the usual circumstance.]**

12. Matt Nichter

Hempel, Stinchcombe, and Pawson agree that concepts are defined and refined as parts of the theories in which they are embedded. Stinchcombe adds that concepts are hypotheses about causal relationships. However, it seems more accurate to say that we use concepts to formulate hypotheses about causal relationships; the concepts are not necessarily themselves causal hypotheses. **[I don't quite get the distinction between a concept being a hypothesis about real mechanisms and a concept be used to formulate hypotheses about the real mechanisms. The terms we use to identify those mechanisms would count as "concepts" and it would be a hypothesis that we have correctly identified the mechanisms. The definition of then relevant concept in this case would be a specification of what defines the particular mechanism as a mechanism. Why isn't this a concept being a hypothesis about a mechanism? What is an example of a concept that is used to formulate hypotheses about mechanisms which is not itself a hypothesis about a mechanism?]**

Pawson contends that "ordinary language reasoning cannot sustain the development of a logically consistent network of concepts." As far as I can tell, the reason would appear to be that ordinary language is vague. But his alternative proposal to construct axiomatic formal models from which various consequences can be logically deduced does not solve the problem so much as postpone it. For once we try to apply the formal models to real-world situations, we find that the real world never quite fits the models: pendulums are not point masses, strings dissipate heat, etc. When these models fail, theorists engage in the same 'post hoc' tinkering that

is the supposed bane of 'ordinary language' theories. **[Good point – formal models do imply a displacement in the problem of muddling through with the vagueness of ordinary language reasoning. Still, it could be the case that this displacement yields a net improvement in precision and non-arbitrariness, since the weaker derivation occurs against the background of a tightly derived logical network of concepts.**

Pawson's critique of Wright, from which the strong conclusion about ordinary language models is supposed to follow, struck me as unconvincing. Pawson is correct that Wrights's self-imposed 'conceptual constraints' are "internally complex verbal propositions which make sense only in relation to a totality of further propositions from Marxist and neo-marxist theory." True, but so what? That's what makes the Marxian concept of class a distinctively Marxian one. Pawson then quibbles about which aspects of Marxian theory are included in these constraints, ignoring the various reasons why one might think 'classes are defined in terms of exploitation' is more central to the Marxian concept of class than is the labor theory of value, let alone the entire Marxian theory of history. **[What I would say in Pawson's defense here is that he is correct that Marxism has such conceptual complexity, than any effort at derivation of constraints via discursive reasoning will not produce the sort of definitive closure of debates that a formal derivation would. This is why people continue to write articles – like a recent diatribe against my class analysis by Resnick and Wolfe in *Critical Sociology* – which claims that the conceptual parameters deviate from the core of Marxism. This means that they challenge my claims about the appropriate constraints. And given the relatively loose quality of any such derivation, it is hard to provide a knock down counter argument. The result is permanent contestation, since probably no concept could be consistent with every possible constraint contained with what is called "Marxism" and many concepts are consistent with the different possible subsets of constraints.]** Then, in the course of his discussion of Wright's models of contradictory class locations, Pawson conflates or otherwise misstates several of these conceptual constraints. Wright's main complaint about his 'domination' model is not that domination is gradational, but that it does not yield objectively antagonistic interests [Pawson writes 'subjectively' on p. 241]; Pawson appears to conflate relationality per se with antagonism. I basically share Pawson's critique of Wright's failure, in his revised model, to stick to the conceptual constraints, but it doesn't follow that this has to do with its lack of formality. As Pawson acknowledges, Wright is perfectly aware of the problems with the notions of organizational- and skill-exploitation. **[And, of course, in my final revision of my conceptual strategy I pull back from both of these ideas and move more in the direction of my ealier formulation.]** In short, I got the feeling that Pawson, by interpreting Wright so sloppily and uncharitably, was fulfilling his own prophecy about the inevitable muddiness of theories constructed in non-mathematical language…

13. Matt Dimick

Concept Formation: Form v. Substance?
        Pawson's main objection to Wright's rules for concept formation (his "logic of concept formation" (p. 240)) is that "they are stated in the absence of any formal calculus which is needed to accomplish them. The result is that Wright's rules are prey to interpretation in dozens of ways, giving the possibility of making hundreds of conceptual transformations to adopt them to a specific circumstance, thus leaving us with not one, but a thousand explanatory networks" (p. 240). Since they otherwise appear to agree on their approach to conception formation by rejecting the narrow "operationalistic" approach and by arguing that concepts need to be embedded in larger theoretical frameworks, it is Pawson's requirement that this framework be formal that seems to be the major point of disagreement. So my first question is, what is necessary to meet this requirement of a "formal calculus"? **[I think what is needed is any procedure by which the derivation of complex concepts from simple elements is determinate – that there is only one specifable concept given the axioms or simpler elements. Mathematical derivations satisfy this. But there may be less mathematicized ways of doing this that still banish arbitrariness in the specification of concepts. That really is the issue here: how to purge arbitrariness from the specification of conceptual terms.]**   Pawson is fond of the physical sciences and their use of geometry and calculus. But he acknowledges that "formal reasoning begins with far less grand ideas than these." So this formal calculus need not actually require mathematics. He also describes Blau's "primitive theory" of social structure as a "general vocabulary of social structure." Is it right to treat the formal structure in which concepts are deployed in some way pre-theoretical and having no explanatory content (not implying Pawson does)? Or is it better to think of it as a set of propositions which *could* explain certain phenomena, but are linked together in a structure so as to tease out their logical (as opposed to empirical) consequences, so when the effort to explain something begins, the formal apparatus can be deployed in a deductive way? **[I think this is what he is talking about. The idea of a formal network of concepts is simply one in which the logical implications of each concept and their interconnections can be fully specified, so that the coherence of the relations of each concept with respect to the others is firmly established.**
        Pawson characterizes a "knot concept" as one "with roots traceable to some elemental social process which ultimately connects to all others" (p. 237) When he evaluates Wright's conceptual transformations of "class" in terms of how they integrate into "the complete network of concepts" (p. 243) he concludes that they don't fit in with Marx's theory of history and that they integrate perhaps more favorably with Weberian class ideas. In light of this evaluation, my next question is: Is possible to have different concepts of the same phenomenon that are used in different theoretical frameworks to explain different sets of phenomena? For example, could one have two concepts of class (Marxian v. Weberian) that are used to explain different things (historical change v. income distribution)? **[This is absolutely possible. The issue, of course, is whether what is in play here is just the common use of the "word" class rather than really two rival definitions of the same concept. This is a messy business to sort out. In the specific case of Marx and Weber what I like to think is that the concept in the**

Marxist tradition is a much more complex concept and that it contains within it the Weberian concept. That is, roughly, the Marxist concept of class links production relations and exchange relations into an integrated complex concept, whereas the Weberian concept is restricted to the simpler causal nexus of exchange relations. For some purposes just focusing on exchange relations may provide better explanations of something that trying to combine exchange and production into the more complex compound concept. Explaining earnings variation might be an example. But note, in this case: the Marxist concept might help explain why the Weberian concept works well in those settings, whereas the Weberian concept cannot explain why the Marxist concept works in other problems.] That Pawson says that knot concepts must be traceable to social processes that connect to all others and that he objects to Wright's class theories based on their alleged inconsistencies with Marx's theory of history would suggest that this is not permissible. On the other hand, the objection that Hempel makes and Pawson repeats, that concepts should be tied to larger theoretical frameworks, is based (at least partly) on the pragmatic principle that we shouldn't needlessly multiply concepts. But having different concepts for different theories is also a pragmatic concern, which would seem to make this permissible. **[I am not quite sure precisely what is meant by the sentence "different concepts for different theories", since much of what makes two theories different from each other is in fact the presence of difference concepts. Theories are made up of concepts, and thus I am not sure we can exactly specific a theory independently of the concepts that make it up.]** Perhaps another question to ask is whether explaining different phenomena requires that different concepts be used. Maybe there will always be competing concepts of class (Marx/Weber) because they are used to explain different things (historical change, income distribution)? **[This is what I argue in the conclusion to my forthcoming book *Approaches to Class Analysis*. The conclusion is called "If Class is the Answer, what is the Question?" and argues that differences between the broad styles of class analysis and class concepts are largely derived from differences in the pivotal questions in which class figures as part of the answer.]**

14 Martín Santos

Societies structurally organized around "contradictory social positions"

What kind of concept formation and theory reconstruction is needed in societies in which social actors are structurally (historically) situated in "contradictory positions" along racial and class lines? For example, in many Latin American societies one can be considered *both* "white" *and* "black", depending of the context; likewise, the development of an *informal* sector within the economy makes it hard to establish *who* occupies *what* "contradictory social (class) locations" in society. Is it enough for such a theory reconstruction to take place, to consider the "theoretical" and "empirical" constraints suggested by Wright? Should it be added an *interpretation of the history* of these societies based upon the concept of "*societies structurally* organized around "contradictory social (class and race) locations"? **[Isn't the attention to "*interpretation***

of the history **of these societies" basically a way of talking about the "empirical constraints" in the specific context of Latin American societies? Empirical constraints vary across time and place, and that is why they produce continual challenges to specific concepts and their associated theoretical frameworks. The notion of "contradictory social positions" can be thought of as a kind of "metaconcept" that could then be deployed to develop specific concepts of distinct kinds of contradictory locations in different historical/empirical settings and linked to different theoretical problems. The meta-concept is basically something like this: In any system of polarized social relations which generate antagonistic relations, there can be specific ways of organizing those relations in which a person is in the dominant/oppressor/exploitative position with respect to one dimension of relations and in the dominated/oppressed/exploited position with respect to other dimensions. Such complexes are called "contradictory social positions." This could then be applied to contradictory locations within racial relations or contradictory locations within a class-race structure of relations, and so on. That meta-concept is rather like the older idea of "role conflict" or "role incongruence": that metaconcept could be specified independently of the content of the roles in question, and then could be used in all sorts of organizational and social settings, each of which would require the production of a specific concept of a type of role concept. Which of these would be relevant to a given problem would depend upon "empirical constraints."]**

Historically grounded context-specific interpretations of social facts and theory construction

I do not understand on what grounds Pawson states that historically grounded context-specific interpretations (necessarily) pose a problem for sociological explanation and theory construction. Why "it is logically impossible to distinguish post-hoc theorizing from a genuine attempt to build a generative model when both rely on specific contextual reasoning to establish the meaning of key elements in the model" (p.250)? I contend that given the historical and context-specific nature of social phenomena (something acknowledged by Pawson, p. 254), the integration of formal modeling, "verbal reasoning" (odd expression used frequently by this author) and historical analysis is necessary in order for the Social Science to better explain social phenomena. **[I think the post-hoc interpretative mode is simply being regarded with suspicion because the human mind is so good at generating post-hoc explanations of everything. This is why functional explanations are always suspicious, and why even in biology evolutionary biologists are always suspicious the just-so quality of functional explanations of adaptationist explanations (in the absence of specific evidence of the evolutionary trajectory). The idea, then, is that when post-hoc historical contextualization is used to generate the theoretical understanding of something, this should be regarded as a hypothesis which should also *logically*-entail some other empirical facts not yet observed (i.e. if the contextualized historical argument is correct than some other things should also be true which have not entered into this post-hoc explanation/interpretation). Making those observations in subsequent research *and not subjecting them to post-hoc historical interpretation*, then, would add credibility to the previous post-hoc historical argument.]**

15 Mara Eisch-Schweitzer

The readings this week were around the development of concepts, principles and the definitions thereof. Am I correct in understanding that, according to Hempel, scientific theory is the simultaneous development of operational criteria (which may become laws), and development of principles through the testing of hypothetical implications (which involve the given criteria but do not interpret or test the criteria)? That these principles become the connecting threads between concepts; the more threads the stronger the evidence for the principle? Pawson then suggests that over time the accumulation of interconnected concepts/laws and principles/threads yield a network of scientific growth. This formalized theory of networked concepts sounds looks like systems theory. Is there a connection here? **[Systems theory is a theory about how society itself should be understood – it is a system of interconnected parts that function like an organism, with feeback loops, integration, regulation, etc. What Pawson is talking about is the system of conceptual elements and how they are connected—through logical derivations and formal reasoning, or more loosely through verbal reasoning. I think these are quite distinct, although systems theory itself is constructed through a network of concepts.]**

The difficulty in applying this process of the develop of scientific theory to the social sciences is that in the natural sciences the operational criteria remain consistent, and evolve into laws with empirical testing. Principles tested empirically reveal a deeper understanding of the natural world. When dealing with the social sciences are there concepts which the social scientist/human actors would desire to remain consistent? It would seem that social scientists/human actors would endeavor to learn from the principles connecting the concepts in an effort to change the outcome; especially if that outcome is perceived as less than favorable. **[Changing an outcome in the world does not mean that you necessarily change the concepts used to explain those outcomes. A good instance is the theory of the "collective action failure" – the "free rider problem" – which is built around a series of rational-choice concepts. This concept specifies a set of conditions under which people fail to cooperate to produce a common good even when they would benefit from that common good. The theory of the free rider problem bears on all sorts of struggles and can help people and movements figure out ways of "overcoming the free rider problem" and, as a result, change something important in the world. But this would not mean that the concepts themselves would not remain consistent and unchanging.]**

Eva Williams

        "Most of those attempting to construct a scientific sociology have admired the clarity and precision, and thus what they have assumed to be closure, of scientific terms. This guiding instinct has led to calls for operationalism and reconstructionism" (227). This has lead to objections by those (who Pawson terms "contestabilists") who declare that the goal of concept construction is unattainable. (227). This seems to stem from

issues around particular terms and their usage/meanings. (e.g. see his example of the multiple uses of the term 'ball'.) For Pawson this is resolvable. He asserts that "…conceptual certitude, measurement, empirical validity and all the other hallmarks of scientific method cannot be achieved by attention to singular terms, be they relatively empirical or relatively conceptual" (236). Through this he reframes the discussion from one of *contestabilism vs. reconstructionsim* to that of *formalism vs. contestabilism*. He goes on to reveal that when we consider not the isolated cases of usage but look for "the context of the whole set of sentences in which they occur"(239), locating meaning in what Kaplan (1964, p.64, cited by Pawson) terms 'conceptual structure' or an 'horizontal articulation'. This means that we are never taking the term in isolation but rather within the whole of a theorist's work. Clearly this is easy with an example like Bourdieu's use of the term habitus, more difficult with Marx's use of the term of class or exploitation, and pretty near impossible if the example is Kuhn's use of the term paradigm. Which framework for considering these multiple uses works best? **[You give a very good distilled statement of the issues in the alternative strategies of concept clarification in Pawson's book. I don't know which works "best". My guess is that it very much depends upon the problems being studied and the state of the art in the development and elaboration of theoretical ideas.]**

Pawson goes on to challenge the idea of conceptual frameworks such as the box model and the matrix structure (Bourdieu's theory of fields?). These frameworks, according do Pawson, "provide an abstract notation to describe social structures in general can be reworked to encompass and explain a whole range of particular situations"(246). "Sociology faces serious limitations in this respect, and ends up having to make the stark choice, scope *or* precision."(246). Erik O.Wright, who reminds readers that there will likely be rival interpretations of concepts, presents in his discussion of concept formation/revision an argument that suggests one need not choose between scope and precision. "What we need is a balance between theoretical commitment to maintain and strengthen the coherence of given general theoretical frameworks with theoretical openness to allow for concept transformation and theory reconstruction" (24). To accomplish this, it is necessary to consider constraints, "imposed by the *explanatory role* of the concept…(and those)…imposed by the *structural properties* of the abstract concept…"(27). This might seem at first a simple distinction, however, for those who are more comfortable with less abstract discussions, a more drawn out explanation of these two types of constraints would be helpful.**[I think what Pawson is really preoccupied with is the problem of what is precisely meant by my assertion that there are constraints imposed by "the structural properties of the abstract concept". He is worried that unless one can really formalize this, there will be too much arbitrariness in the efforts to specify these constraints, and thus it will not be possible to establish a workable procedure for pushing forward knowledge. If the relationship between theory and concepts is "underdetermined" (i.e. if the constraints as specified really allow for many possible definitions of concepts), then the theory itself may become insulated from empirical failures, since it is always possible to jigger the concepts and pretend that they are still consistent with the overall.]**