**INTERROGATIONS #1**
**9/9/2004**
**General Perspectives I: Empiricism & Positivism**

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### 1. Fabian Pfeffer

**The covering-law model and the deductive-nomological model.**
**Are they i) inherently contradictory for empiricists and ii) useless for Sociologists?**

One central doctrine of empiricism listed by Benton and Craib is the 'covering law' model. It proclaims that scientific explanation equals derivation from a general scientific law. This is consistent with the deductive-nomological approach Hempel - a logical positivist - has put forward. The most central demand to the DN model is that it has to contain (at least) one law as part of the initial true statements (Hausman, p.289).

i) It seems apparent that *laws* take a prominent place in empiricist/positivist thought. While empiricists show to have no difficulties in defining what a law is (namely a true statement of observed regularities), they - in my eyes - remain relatively mute to the very practical question of how such a law is ultimately developed. That might be due to the fact that this is where empiricist thought runs into a severe problem: Hume's well-known induction problem. It says that "it is impossible to justify a law by observation or experiment, since it transcends experience" (Popper, p.54). The question then is: How does an empiricist get to a (for him indispensable) law without running into Hume's induction problem? Popper presents one path out of the dilemma: the redefinition of laws as 'tentative hypotheses'. As these are open to refutation by observations they satisfy his falsifiability criteria and - miraculously - seem to solve the induction problem. I assume that an empiricist would not consider following Popper's path, nor would he escape to a metaphysical notion of a priori existing laws. **So do empiricists call out loud for laws but don't know where to get them from**? **[I am not sure that much rides on your contrast between tentative hypothesis in Popper's sense and the idea of a covering law. Popper asserts that all scientific claims are tentative – there is never certainty. And thus claims about laws – i.e. claims about invariant regularities – would also have to be tentative claims. But not all hypotheses are about such laws, and thus I don't think that the category "laws" can be treated as equivalent to "tentative hypothesis"]**

ii) Disregarding Hume's induction problem and the doubt it raises whether the construction of laws is possible at all, I would like to turn to the status of laws in Sociology. The question is easy: Where are they? Are there any laws - in the strong empiricist notion - in Sociology; in Social Science at all (what about the law of demand)? If there are no laws to be found in sociology, two conclusions might be drawn from that fact: a) The pragmatic conclusion that covering law or DN models are of no use to current Sociology. b) The adoption of Popper's 'hypotheticism' which calls for the replacement of laws by tentative hypotheses. I claim that any attempt to combine this position with a covering law or DN model is determined to fail. For the explanatory power of these models rests on their deterministic nature. Any alteration of these models towards non-deterministic, statistical probability statements (and Popper's 'hypotheticism' would definitely imply such an alteration) destroys their central explanatory scheme. If sociological statements take - or can only take? - the form of hypotheses and sociological theory the form of a "collection of <u>lawlike</u> statements [and not laws] related to one another" (Hausman, p.297), **why should we bother about the empiricist notion of explanation by laws**? Should we rather be realistic and follow Merton's proposal of middle-range theories, saying goodbye to polished positivist claims about true laws? **[I have always been a bit unclear about precisely what must be true about an invariant regularity to count as a "law." In physics it seems that one can say that gravity is an invariant regularity in the relation between masses, and no side conditions need to be stipuated to describe the "law of gravity." But it would seem that one could perhaps still describe something as a law or a lawlike claim if the invariant relation holds under a set of stipulated conditions. If this is so, then a middle range theory need not reject the relevance of laws, in the sense of systematic regularities that are thought to figure in the explanation of specific events/instance of a phenomenon. Consider the important sociological theory of the 'tragedy of the commons". One could call this the <u>Law of the Destruction of Common-pool Resources</u>. The law only applies when certain conditions are present, including such things as the nature of property rights, resource use, individual rationality, selfishness, isolated individualist decisionmaking, etc. If those conditions exist, then the law would hold. Is this a middle range theory with a social law?]**

<u>Another (provocative) question that popped up while reading but has not been elaborated further:</u>
The role of experience for macro-sociology and specifically historical sociology:
What can be learnt from experience (observations) in an academic discipline where mono-causality never occurs and the same set of conditions cannot be expected to occur more than once? **Much hinges here on the idea of "the same set of conditions". Isn't this a question f the level of abstraction one adopts in trying to explain something? Take again the problem of the "tragedy of the commons" – the destruction of common-pool resources. There is good evidence that this causal process was important in paleolithic times and helps explain the destruction of certain premodern habitats as well as medieval grazing commons and modern corporate fisheries. These are instances of the "same" set of conditions understood in a particular way (short time horizons, prisoner-dilemma type pay-offs for individual strategies, etc.)]**

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**2. Liberty Karp**

Can the acquisition of social knowledge really be broken down into a set of identifiable, concrete steps that a person can and must follow in order to get at the "truth," or is there an element of magic that creates a good social scientist? **[Magic? I am not sure that the real alternatives here would be a formula of universal steps vs "magic" – although I'm not sure what you really mean by the metaphor. In many contexts a distinction is made between codifable knowledge and tacit knowledge – where the latter has to do with capacities that are the result of learnings-by-doing, embedded in the skillful practice of a craft, but not teachable as abstract operations. Perhaps something likethat is operative here.]**

While positivists and empiricists have struggled to distill those fundamental aspects of "hard" sciences which make them "science" and therefore "valid," their critics have pointed out that much of what they would consider to be "real science" actually fails to meet even the criteria they propose. Newton's law of universal gravitation is a frequently-cited example in our readings of knowledge held in high esteem by positivists and empiricists, but which fails the fundamental criterion that it be observable. If such a prodigious discovery in physics was made on the basis of "unscientific" principles, what value is there in holding contemporary inquirists* to haphazard standards in evaluating their means at finding the "truth"? In the discovery of knowledge, what is more important: the means or the end? The obvious objection is that an analysis of the means used to discern supposed knowledge is the only way of determining whether that knowledge might be accurate, but again in the case of Newton's brilliant discovery this approach loses its validity. **[Popper also uses gravity as a pivotal example, but his focus is not on the <u>discovery</u> of gravity but on the <u>justification</u> of the laws of gravitational attraction. Here the example he stresses is Einstein's modifications of Newton and the "bold conjecture" that lead to specific predictions about the behavior of light and gravity. The key issue here is that it was through observable predictions (or implications) of the theory that it could be tested, rather than the stronger claim that ever entity within the theory had to be observable.]**

Consider the possibility that being a brilliant observer of the physical world (or the social world) might be akin to exhibiting brilliance in other fields such as art or music. If we take our philosophical approach to the study of the latter, we might begin by describing what constitutes "good music." We'd research sound frequencies and study their resonance in the human ear canal and we'd theorize about how certain simple frequency ratios (2:1, 5:3) sound particularly pleasing. We could devise a sequence of rules governing the creation of good music:

1. Frequencies with ratios of 2:1 shall be considered "octaves"
2. Octaves shall be divided into 12 semitones
3. Semitones can only be combined in certain ratios

…and so on.

Immediately we might ask: To which music does this apply? Aren't there examples of "good" music in the world which do not use a 12-note scale? Can't there be exceptions? Of course; these critiques are accurate. But even more importantly, we ought to look at the relationship between this set of rules that we have devised and those individuals whom we have come to distinguish as great musicians. Does it make sense for us to judge the music of Tchaikovsky based on our set of rules? Likely not any more than it makes sense to judge Newton using empiricists' criteria. As we attempt to apply this lesson to ourselves and our approach to our work, perhaps the most important question is: Would it make sense for Tchaikovsky to have approached the creation of music by starting out with our particular set of rules?

To bring the conversation back to social science, we might consider Alexis de Tocqueville and <u>Democracy in America</u>. Tocqueville, at the age of 25, sets out to travel around the United States, and produces a work which, while not perfect, manages to posit predictions which prove to be of astounding accuracy over 150 years later. Perhaps in his case the end justifies the means?

In other words: Is it possible that a brilliant craftsperson, regardless of field, more than anything employs generous amounts of genius and intuition? **[Popper would definitely attribute major <u>discovery</u> to genius capable of making leaps beyond existing observation and data – risky, bold conjectures. But he would also insist that the <u>context of justification</u>, in contrast to the**

**context of discovery, can be systematized into a set of logically required criteria and conditions.]**

* Yes, I made that word up, hoping to avoid the term “scientists” so as also to avoid undertaking the assumption that good knowledge can only be ascertained through scientific means

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**3. Ricky Leung**

An interesting issue from Benton and Craib's chapter is that invention of scientific theories is closely linked with imagination and creativity. Seemingly, an inventor might succeed only if she can make creative invention under various constraints. The article has highlighted logical structure and 'relevance' as two constraints of scientific explanations. While these constraints are certainly reasonable, I am inclined to believe that even 'logics' and 'relevance' might be socially constructed. In other words, as criticisms against empiricism, I wonder if Benton and Craib have gone far enough to capture the realistic image of scientific production. Or you think they have incorporated the idea of social construction elsewhere? (Maybe their discussion on the role of interpretation and relativism in science is adequate?) **[I am not completely sure that I understand your point here. Are you suggesting that there are no logical constraints on “scientific” explanations? When you suggest that these might themselves be social constructions, is the implication that if this is so – if logic is a social construction – then it doesn’t count as genuine constraint on the production of scientific explanation? Your point here is not entirely clear, to me anyway.]**

I am not sure how empiricism considers the relationship between scientists' psychology and scientific explanation. **[Does empiricism need a view on this? That is: an empiricist could treat the problem of producing scientific explanations of the behavior of scientists as itself a legitimate problem, and within that problem, psychological processes of scientists would be one of the things one might study. But would this, in a philosophical sense, be a distinctively empiricst theory of scientists?]** One the one hand, it seems obvious that there exist certain linkages between 'psychology' - motivation, imagination and creativity, and the types of explanation a scientist finally arrives. I wonder if Benton and Craib would be interested to make this linkage more explicit. According to our readings, what kind of impression do you have about how scientists' psychology and scientific explanation is related (if at all)? For example, what makes it more likely for a scientist or social scientist to offer functional explanation? Likewise, is it true that scientists tend to go more and more 'micro' in their explanations? If so, what might be the motivations?

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**4. Ana Cristina M. Collares**

According to Samir Osaka (2002), there are no consistent arguments in favor of *scientism* because the so called “scientific method” does not exist, denying the view of science as the unique path to knowledge, or yet its counter-claim that some subjects cannot be explained scientifically (p.125).

Although this may suggest that scientism rests on false assumptions, it does not imply in rejecting the character of science as a myth in the modern world. The choice of a method of investigation has a role in this “mythological” aspect of science. It is heavily embedded in the definition of science itself, and influences what kinds of theories will guide the investigation or will result from it. Therefore, what is the relationship between *the choice of a method of investigation* and the *credibility of the findings*? Or, in other words, can the method of investigation account for the reliability of the results obtained?

In the “soft” sciences this problem become more evident, because it’s object is harder to delimit. Definitions of observables and non observables are “blurred” and the object of analysis reacts differently to different methods (e.g. a survey or an ethnography).

Who can decide, as suggested by Ian and Craib (2001), whether the patterns of suicide detected by Durkheim are a social or individual phenomenon, i.e. whether they are an *observable* social phenomenon (or social fact) made visible through statistics or just a theoretical instrument created by human mind to help explaining suicide? How to distinguish a causal relation from a mere correlation or conjunction of factors (and here comes the old problem of induction proposed by Hume …).

Sociology has been appealing to mathematical instruments to increase the credibility of the knowledge it produces. The statistical instruments are increasingly more elaborated, but our ways to approach the object, i.e., to collect the data, have not received the same attention.

This discussion leads me to think about the recent “field” that has been created in Sociology: the public sociology. Of course any person belonging to a community will make use of any kind of knowledge available to her to make decisions and influence other people. But as the mythological aspect of science is still preeminent in the minds of most people, isn’t it a questionable decision to trust in the sociological knowledge as an indicator of the “good decision”, or the “good explanation, etc”? When I propose this question I think about apparently neutral sociological knowledge such as the one presented in the book “The Bell Curve” that have serious social consequences if taken seriously. **[But the Bell Curve was universally condemned by sociologists as violating the cannons of science. It was pseudo-science, precisely the sort of thing that scientific methiod can show to be false.]**

Therefore, to conclude, I present a last question: do we need to prove that what we sociologists do is ‘legitimate science’ and produces ‘superior knowledge’ than the common sense, to keep doing what we do? I have no answer for it yet.

**[General comment on your interrogation: This interrogation did not really cfocus in on a question from this weeks reading. You should identify some argument or element in the reading about empiricism or positivism and then pose a question about these ideas. What you presented is more a kind of meandering set of comments on issues linked to the readings, but not really a question-posing interrogation.]**

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**5. Matt Desmond**

My main question is this: Does social science possess a unique justification for its existence? Hausman suggests that two debating camps have posed opposite explanations for the purpose of science: “scientific realists,” who see science as a (empirically-supported) profit and an archeologist of “new truths” and “instrumentalists,” who see science as a toolkit for making larger predictions about the world (p. 286). **[I don’t know what you are referring to when you say that scientific realists see science as a “(empirically supported) profit,” or why you**

**introduce the metaphor "archaeologist" to capture Haurmann's claim that realists believe science enables us "to discover new truths." I don't see why the discovery of new truths is appropriately captured by the metaphor of an archaeologist. Your exposition here is not clear to me.]** But a key difference between the laboratory of the natural scientist and the social scientist is that the former studies (primarily) what is and the latter studies what is *and* what can be. So, the goals of social science are a bit more complex than the debate between realism and instrumentalism, as we must, I think, expand our theories past those of explanation and prediction to criticism, and this is where we return to the debate regarding the role of values in science.

Benton and Craib offer three critiques of the empiricist view regarding how morality and values should or should not be incorporated into science: (1) treat moral relativist conceptions as "independently real" and thus objectifiable, (2) argue that scientific claims and cultural values are inseparable, and (3) treat science as a value commitment in and of itself (p. 44). It should be pointed out that what is absent from their discussion is the question of whether or not scientists should evaluate the consequences of their findings from the beginning of the research process or if scientists should only ferret out what they believe to be "scientific" advancements and leave the consequences to others. **[How can a scientist ferret out the moral consequences of findings "from the beginning of the research process", since at the outset of research one does not yet have "findings"?** Einstein's theories, of course, led to the creation of the Atom Bomb, and for social scientists, research on underprivileged populations has sometimes been used against such populations (see the Introduction of *In Search of Respect* by Philippe Bourgois, for a discussion). For this reason, along with many others, it seems to me that the scientist's values must be an integral part of the scientific enterprise from the inception of the research. Specifically in sociology, it would be especially difficult to conceptualize a valueless discipline, as when we make statements about oppression or injustice we are summoning up certain values systems. In my eyes, the best sociology is value-laden and empirically robust, where the values loom large when the sociologist attempts to discuss matters of change. **[Are you saying something more than values should deeply shape the questions we ask, or are you also claiming that values should affect the answers we get from the research? I agree completely that we should ask questions such as "what are the causes of the racial oppression and what sorts of institutional changes would reduce that oippression?", but I am not sure what it means to be "empirically robust" and for values to determine the answers to questions.]**

Perhaps, then, social science's unique purpose is to produce knowledge about the social world that can be used to change it (close to Comte's original idea). And if this is the case, we return to the questions about how the social scientist should interact with the outside world, who gets to define what problems need change and what kind of change is best, how this process threatens the autonomous of the scientific field (ala Bourdieu, for example), and if social science can ever gain the scientific capital the natural sciences possess if it divided so rigidly upon this point or suggests that its enterprise can have multiple purposes (as does Burawoy's model).

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**6. Gokcen Coskuner**

Popper suggests that the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability. He gives examples from natural (Einstein) and social (Marx) sciences.

He asserts that although Marx's theory may have social significance because it is not refutable it is not scientific.
Although I found Popper's falsification method interesting there are several issues that were not clear in my mind and that I found problematic.

1) Is there only one criterion for scientific status; refutability? **[I think that Popper would regard this as the pivotal criterion for what he calls the *demarcation* problem of science vs pseudo-science, but it would not be the only criterion for what constitutes science. Science also involves particular methods of generating predictions from theories (through logical deductions, etc.), for example. But testability is the critical issue in disputes over the status of science, in his views.**
2) How does he suggest one tests social theories? **[I don't think he proposes any specific method of testing. His point is that unless theories are testable they are not scientific – that is, if all possible observations would be consistent with the theory then it would not count as a scientific theory, and this would be true for social science as well].**

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**7. Dan Warshawsky**

Epistemological Consensus: Good or Bad?

After reading Benton and Craib's, Hausman's, and Popper's articles, my intellectual interests in broad questions regarding issues of epistemology and ontology have resurfaced. During my past few years, I have reflected on the nature of knowledge and reality. Of course, grappling with the type of research I want to study and the questions that I want to ask always push me back to the same questions. Am I researching something of value? Are my academic interests, and my specific interrogations worth exploring? I always seem to get different responses from my different academic departments. As an undergrad, I was exposed to many variations of interpretive anthropology, the "classic" scientific method, as well as other common quantitative and qualitative methodologies. In all, the various approaches to academic exploration added more questions, more confusion, and less confidence as I approach my academic endeavors. Among the many questions that I bring to graduate school, and this course specifically, here are my most challenging and most relevant: What are the social sciences, and what is their relation to "science" in general? More specifically, what is the value of interpretive, qualitative, and ethnographic approaches to studying society? I do not intend to a necessary pose these questions immediately. Rather, I reflect on these broad and important questions now to give the class an idea of my reference point as I grapple with the history of science, specifically positivism this week.

Specific to this week's readings, I have one large question to pose to the class, although it is actually one question composed of many small parts. Throughout the readings, issues of realism versus instrumentalist, scientific explanation, induction, and many other concepts are discussed as the authors grapple with their specific issues of inquiry. Yet, I keep wanting to automatically disregard certain initial questions, before even reading their exegesis, as I am tempted to emphasize that each author asks the wrong questions. But eventually, I realize that my disregarding for others' central epistemological questions is commonplace throughout academics, as many scholars focus on issues of knowledge and reality that are specific to his or her own discipline and expertise. So I ask, do academics in all disciplines need to build a consensus to determine what types of research are valid? Do there need to be defined starting points? In other words, should there only be specific epistemological and ontological questions which are "up for grabs." It seems that the authors of these articles and others in the natural sciences assume specific questions are already satisfactorily answered (although different questions are open and closed depending one one's academic discipline), so only certain issues are truly open for discourse. Additionally, is it crucial to have a consensus within the social sciences, within one's own discipline, or even within one's expertise? **["Consensus" might be too strong a condition, if by this you mean something like "complete agreement". But it is hard to see how science can progress in any sense of the word if there is no agreement at all on what distinguishes good work from sloppy work, rigorous thinking from mush, careful use of evidence from tendentious, dishonest use of evidence, etc. And when you look at these dichotomies carefully, to a significant extent they boil down to issues of what renders research valid.]**

I pose this question, and extended questions, because I think it is crucial when one is initially studying issues of epistemology and ontology. Personally, I think that all issues of epistemology and ontology should be contested and open in general academic discourse. However, for ease of academic, research many in the academic community feel content, for the current moment, with putting certain questions aside. This does not mean that those questions won't be raised again one day, it just means that they will be assumed until a new perceived crisis develops. I would enjoy all academics in both the natural sciences and social sciences to continue posing challenging epistemological questions, although I am not sure that a consensus of epistemology and ontology is ever possible or even a good thing. Having "contested discourses" is intrinsic to academics and a lively scholarly and public debate. However, I am quite open to diverging opinions, so I open up this discussion of epistemological consensuses to the class. **[An alternative view on this might be the following: perhaps most of these contests over epistemological and ontological issues are really just diversions from more important debates over substantive disagreements about how the world really works (rather than how we should talk about how it works). Instead of talking about the real mechanisms that perpetuate racial and gender oppression we talk about the meaning of structure and agency and the epistemological status of our knowledge about oppression. Sometimes I feel that much of the metatheoretical discussion actually is a distraction from more important matters.]**

**[One other general comment on your interrogation: You raise interesting and important matters here, but they don't actually engage the reading very directly. You don't pose a question about empiricism or positivism, about Popper's falsificationism or the other commentaries. In a way, the general question you ask about consensus over epistemological problems could be posed regardless of what we read. In general I think it would be better to organize your interrogation more directly around the reading and pose questions that engage them systematically.]**

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**8. Eva Willians**

Positivism, according to Ted Benton and Ian Craib (2001), seeks to elevate the status of the social sciences to that of the natural sciences, "…by extending the methods and forms of explanation which have been successful in the natural sciences" (28). Anti-positives, on the other hand, contend that there are vast differences, (incommensurable ones, perhaps) between studying plants or proteins and studying humans. "These differences," explains Benton and Craib, "include the alleged unpredictability of human behavior, which stems from our unique possession of free will…"(28). However, rather than engaging in this debate, Benton and Craib suggest that the real question is not whether social science should emulate natural science in terms of methodology, but rather upon what form of scientific inquiry (Benton and Craib use the phrase "what account of science") social science is based. **[Another way of putting this issue is: the positivist account of the *natural sciences* is flawed, so while social sciences should not be positivistsic, neither should the natural sciences. One can affirm the "unity of sciences" and reject positivism.]** Is it possible to provide an unqualified answer to this question? Or, is it necessary to select a "best fit" between the subject (or object) and the form of scientific inquiry.

"Theories always face unresolved difficulties. They are too important to be surrendered until alternatives are available, and alternatives are not easily generated." (Hausman, 1992). Regarding the first part of that statement—why? Why is it so important to cling to a theory that feels weak, in the absence of a replacement? Are we like the hapless victim of a loveless marriage, we'd rather stick it out with what we know, even though its not working than venture out into the possible abyss of self doubt and uncertainty? **[nice metaphor!]** Or (switching to the analogy of employment) are the considerations of a more practical nature—paychecks and financial security depend on having some form of employment—even an unsatisfactory job is better than unemployment. In the case of theory, would corresponding practical considerations mean the unraveling of an infrastructure were they to be abandoned? Perhaps both. If the initial statement is true, does the inherent hesitation to abandon theories inhibit the development of new, more plausible ones? **[This is an interesting issue: why is it best to work towards adjudication between rival theories rather than simply rejecting a theory in the face of evidence even without an alternative. I think the idea here is that the best available theory probably has a higher truth content than no theory at all – that is, there is probably some knowledge embedded in it, and thus to simply discard it wholesale because of evidence against it would have the result of throwing out its insights as well as its flaws. The assumption here is that theories are complex combinations of elements and conditions, so total rejection in the absence of some alternative is often too crude a response. There is also the issue that it is impossible to tell if a weak theory is actually "weak", or – in contrast – that the side conditions involved in generating the evidence relevant to the theory are weak – in the absence of a counter-theory. I suppose the presumption here is that the best available theory has itself been generated through a scientific process and thus is better than sheer nonsense. ]** And a somewhat related question, Do the social sciences lean toward the instrumentalist orientation at the expense of a realist perspective? **[I think this is a pretty open issue – there are plenty of realists as well as instrumentalists].**

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**9. Elizabeth Holzer**

Does anyone have any thoughts for tidying up the causality mess that Haussman left us with (pages 295-297)? The problem is as follows: research rests on assumptions of causality but there is no satisfactory explanation of causality (the three main explanations of causality being a total muddle). I was convinced by Haussman's critiques of the first two analyses causation —we can't dismiss causation like Russell does as a "relic of bygone age" because researchers do incorporate it into their explanations; Hume's conventional notion of causation in which causes are defined as "Insufficient but Necessary components in sets of factors that are Unnecessary but Sufficient for the effect to occur" (295) doesn't allow us to distinguish between laws and accidental generalities and excludes simultaneous cause and effect. (Tangentially, Haussman's account of Hume seems inconsistent with Okasha's, in which Hume, as the great empiricist, is said to conclude that causality is a fiction). I was not however convinced that the "counterfactual analysis of causality" is a worthless endeavor—so I'd most like to puzzle through this explanation for a bit (see p. 296 for the description of this explanation). **[I agree with you about the discussion of causality being hard to sort out. Last time I taught the course I did so with Hausman, which was great because I could always turn to him to sort these issues out. We will discuss causation a bit more when we talk about "mechanisms". One way to think about "causes" is to emphasize what it is contrasted with. One standard dichotomy is causation vs correlation: as in the slogan correlation does not prove causation. Invoking a cause, therefore, is a way of distinguishing a mere association with a generative relation.]**

If we'd prefer a more narrow discussion, let's address the first critique that Hausmann raises—it's not clear what is meant by "possible worlds." I read this as a problem of operationalization—how might we operationalize "possible worlds?" Would studying a phenomenon in different historical epochs satisfy the requirement? But on the other hand, maybe I'm being overly empirical, and "possible worlds" are meant to be mental constructs, the imagining of different realities**.[I think this is right – the possible worlds used in counterfactual arguments are meant to be mental constructs, not historical possibilities.]**

I also wonder if part of what makes the discussion of causality so messy is that we're trying to make our notion of causality do too much—perhaps striving for a notion of causality that applies equally well to the relationships of all types of phenomena is positivistic hubris. In the readings, causality seems to be about action—if so, does the intentionality of human actors disrupt it? **[I think that most people who accept the idea of causation would also argue that reasons/intentions can be causes – i.e. they can generate certain effects**.]

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**10. G.C.**

The question I would most like to take up, which arises out of Popper's demarcation drawn between, on the one side, Einstein, on the other side, Freud and Marx, is this: how profound is the difference between objects of scientific "explanation" which are purely physical or material and those which also involve consciousness or consciousnesses? **[Popper's complaint against Marx is not that consciousness figures in the explanations, but that Marx's theory of history is unfalsifiable, that it makes no risky predictions (or at least that the Marxists of his day made no risky predictions of the required sort.) I don't immediately see the relevance of the material vs consciousness issue and the refutation problem.]**

There are myriad problems which appear when one examines science *simpliciter*, as well as the transcendental conditions of knowledge (the two are etymologically closely linked and seem to be of the same line of inquiry). However these problems are dealt with, it may be useful to make an additional demarcation among theories, viz. as regards those which purport to explain something about the physical world and those which purport to explain something about mental phenomena or the individual and social instances of behavior which seem to be, or are taken to be, associated with those phenomena.

The potential for an understanding of the laws of nature may be bound up with what it is to understand, or the way in which understanding "proceeds". Thus, we encounter perplexities, such as Hume's. Yet science still does "something"; Einstien's theory of Relativity does not appear to be a mere tautology. We might say that science tells us about the *way* the world is (here is where logical verificationism is concerned, i.e. with meaning; the further distinction, viz. *what* the world is, creates the conflict over how anyone can ever speak *about* the world – thus, perhaps we can only ever say *how* the world is, and show this by trying to do more – to say what it is - and failing).

This putative nomological character of the world in any case harmonizes with a conception of the world as the *way* it is, was and will be. Thus our theories might strive in themselves to describe future events, and so might be, by their form, testable when the time comes to test them. Thus they have an aspect of "verifiability" or "falsifiability". This is a notion of verifiability of a rather different order than that talked about by the logical positivists (keeping in mind that Wittgenstein's own so-called verificationist phase was brief and only very tenuously associated with the ideas of the Vienna Circle). If we suppose that Popper's arguments do not really engage with the logical positivists, they still present a challenge to anyone who wants to assume the mantle of a scientist.

Thus we ask what it is that science can ever help us to understand. If we are only curious about what the future will *look like*, we would be very critical of a theory that predicted that society and the state would be revolutionized and thus *look* different, when this does not prove to be the case. **[This may not be entirely relevant to what you are addressing here, but I don't think the idea of "prediction" in social science necessarily refers to the "future" in any historical-chronological sense. Prediction is more of the form: If condition X occurs – and certain side conditions are also present – then Y will occur. Marx could be right about this sort of prediction even if his historical predictions about the future of capitalism were false.]** This calls into question how social science arrives at its conclusions. The scientific basis upon which Marx places his social theory has a standpoint-specific application; that is, the principles of Saint-Simon, Owen, and Fourier can only be recognized by the proletariat. **[This is a contentious point in discussions of Marx – and Marxism. One view is that the standpoint of the proletariat matters only because this class and only this class has interests in the truth; ythe bourgeois has interests in mystification. The standpoint of the proletariat, in this sense, is privileged because it allows for an objective understanding of the mechanisms and dynamics of the system. This is somewhat different from the view that different standpoints give you different "lenses" or different perspectives, partial visions.]** This understanding precipitates a conscious overthrow of society. Marx gives a *reason* why people will act in a certain way (in the sense that it is the reason that *they themselves* will be acting upon). What action (in reaction to consciousness) will be taken is necessarily difficult to predict; to try to do so is to take into consideration some of the values, interests &c. that science itself tries to eschew.

It may be transcendentally impossible to explain why people act, even if there were a complete physical cognitive science. Even under the best circumstances it would take an *extremely* sophisticated science to explain why certain mental phenomena occur in conjunction to physical events, e.g. why an "ouch" is followed by certain types of physical brain-states; this is even supposing (optimistically) that "consciousness explained" means something, or means something not disappointingly limited. **[Given Mental states may not correspond to unique brain-states – they may be supervenient on brain states (I think that's the right expression)**

**– and yet they themselves may be explanatory of actions. I am not sure why an intention fails as an explanation of an action – with appropriate side conditions – and thus why you feel it may be transcendentally impossible to explain why people act.]**

Perhaps my question has grown too broad, since it seems to be posing The Question of Class Consciousness – What Is It and "Is it true that the owl of Minerva *only* spreads its wings with the falling of the dusk?" I would like to discuss this issue, though I am sure that I have given an insufficient demonstration that it is relevant.

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**11. Mark Cooper**

Perhaps it is due to my unfamiliarity with positivist sociology, and sociology generally, but it was unclear to me (especially after the Benton and Craib reading) what empiricist/positivist social science looks like. Some of this confusion comes from B&C's use of Durkheim as an example for such, while denying that Durkheim was himself a positivist. (25) Durkheim's project seems to exhibit two characteristics that exclude it from B&C's characterization of positivism.

First, it makes an appeal to the reality of 'social facts.' In my understanding social facts would contradict B&C's third doctrine of the empiricist view of science: ruling out knowledge-claims about beings or entities which cannot be observed. **[I think Durkheim's social facts are generally pretty observable. A collectively shared norm, for example, is observable both behaviorally (the common rules in people's behaviors) and in pronouncements. The social fact comes from the claim that the collective character of the phenomenon is consequential, rather than simply the individual fact of individuals hold given beliefs and attitudes.]** (14) Perhaps I have misunderstood what is meant here by 'observation,' but Durkheim's project seems to have a more realist than positivist character in this respect. I am also not certain if realism and positivism are necessarily oppositional in the same way I imagine realism and empiricism to be. I make this claim though, from my interpretation of B&C's first of four features of positivism: that the empiricist account of the natural sciences is accepted (thus forbidding similar knowledge claims in positivist science.) (23) **[The realism/positivism contrast will be clearer next week since then we read more about realism, and some of the pieces make a big deal differentiating themselves from positivism.]**

Second, Durkheim moves from establishing a statistical relationship between certain social characteristics and incidents of suicide, to hypothesizing that these characteristics cause the act of suicide. It is not clear to me from the readings what the position of such a causal mechanism is in positivism, but the empiricist critiques of causation seems at odds with the conclusions Durkheim draws.

These issues trouble me not because B&C's use of Durkheim is particularly important, but rather because I am unable to imagine how, based on the Popper reading, an empiricist or positivist social science could be imagined using only the covering-law model or the hypothetico-deductive method.

B&C note several times that much of the appeal of positivism comes from a desire by social scientists to capture equivalent legitimacy or authority as that possessed by natural scientists. Although chapter three discusses alternative accounts of science, I would be interested to briefly investigate the origins of this differential legitimacy and ask how such might be explained as either a natural or social condition, and how such authority is reproduced. One option might be to investigate B&C's suggestion that we rely on "our intuitive sense of when an argument is or is not valid." (6)

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**12. Brett Burkhardt**

According to Benton and Craib (2001), one of the four tenets of positivist social research is its potential contribution to everyday human behaviors and interactions within society. "Social problems and conflicts can be identified and resolved one by one on the basis of expert knowledge offered by social scientists…(23)." Such a statement, of course, is intentionally general and probably something of a caricature, but it leads to a number of problems concerning the practical application of science.

Benton and Craib identify two particular problems in positivistic "social engineering" (p. 48-49). The first is that once implemented, social policies may alter society in such a way that the original conditions which were used as parameters for solving a social problem have changed. Consequently, the policy may have an unintended and possibly harmful effect. The second problem of social engineering deals with institutional constraints that exist prior to policy implementation. The assumption of social engineering is that a state will implement policies. But, as Benton and Craib rightly point out, (extra-governmental) economic or socio-cultural forces, as well as government agents or branches, may work against implementation. **[Do you think these are actually cogent objections? It seems to me that these are no more than claims about complexity of the system within which the social engineering is taking place. How does this differ from medicine? There are many unintended consequences of drugs, for example – called side-effects – but this does not mean that drugs cannot cure diseases. I don't see why these objections really have any bite in principle, although of course they do in practice.]**

The question of social engineering takes on a new light when it is addressed to specific historical/societal/governmental contexts. Two hypothetical examples should demonstrate this. First, consider a state with a strong, meritocratic civil service staffed with well-respected "expert" social scientists. In this country, industrial policy is largely governed by a single, relatively uniform body with a known ideological stance. This body also has a strong knowledge of industrial policy. In order to pass industrial policy X, social scientists in this country may have to make their prescriptions for social engineering conform with the known ideological stance, but they will likely not have to "dumb-down" their data, or transform it into easily digestible factoids or sound bytes. Consider a second state which looks to a group of less respected social scientists for policy input. In this government, industrial policy is a minor issue, and authority over it is highly diffused. Policy must be passed through a number of committees, agencies, and branches. In fact, it may also experience a high level of public exposure, which necessitates broad popular support. These social scientists will be forced to shape their policy recommendations in such a way that they are understandable, meaningful, and agreeable to a broad range of people. Similarly, they will have to use techniques and study objects that are seen as legitimate by a broad range of people (who are non-experts).

As I hope these examples suggest, the particular context in which positivist social scientists work will likely force them to formulate their policy prescriptions in different ways. This is a possible outcome even if their research and results are identical. But beyond this, the acceptability (or lack thereof) of certain research methods and topics may dissuade scientists in one institutional setting from pursuing a particular agenda. Why would a social engineer, whose goal is to have scientific policy enacted, pursue research that he or she knows will not be accepted under existing institutional constraints? This is an empirical matter, but there is likely a good deal of evaluation and strategic action involved in policy-related research. If this is true, there seems to be a paradox in a strict positivist position. One the one hand, it encourages application of science to the identification and solution of societal problems. If we assume that extra value is placed on successful implementation of solutions, then we can also assume the sort of "strategic research" described above. Yet, on the other hand, the positivist position seeks to apply empiricist techniques to the social world. Strict versions of empiricism seek to abolish interpretation and subjectivity from research projects. This, of course, is at odds with the strategic work that likely occurs in policy research. **[I am not sure if I entirely get your point here. Are you arguing that the different components of positivism identified by B&C don't actually hang together? Specifically, you suggest that empiricism as a methodological stance does not provide the best model of science for social engineering, which requires a kind of strategic research model that cannot be empiricist in the philosophical sense. Is this right?]**

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**13. Martín Santos**

I have two brief questions for this week:

1-Considering that August Comte had a more complex view of the Positivist Science of Society than the one offered by Benton and Craib, is it possible to enhance our understanding of what "positivism" means ? (for example, what would it be missing in the characterization presented by Benton and Craib in p. 23)

2-Given the ever-present possibility of different interpretations of the same body of evidence (p. 32), is it reasonable to advocate for the *incommensurability* (p. 33) between rival theories or paradigms? (for example, *between* "quantitative" and "qualitative" approaches, or even *within* each of them) Why or why not? If the answer is negative, what are the common characteristics making possible the commensurability between different "paradigms"? **[I don't think the quantitative/qualitative divide has necessarily anything to do with incommensurability of paradigms. Quantitative and qualitative data can both be used within the same theoretical paradigm.]**

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14. *Matías D. Scaglione*

*Scientific concepts, sensory experience and empiricism*

It seems to me that one of the most important failures of empiricism is its inability to accept the scientific status of the discovery of new theories, of the very process of invention of hypothesis and creation of concepts. In a famous letter to Kepler, Galileo admitted that he was not surprised with what he had found with his telescope: "*ut quod mente tenebam in dubium, ipso etiam sensu comprehenderem*", only what the mind has previously configured was apprehended by the senses. It is worth noting that I am *not only* referring to the "complex synthesis of sensory impressions and conceptual ordering and selection" pointed out by Benton and Craig (p. 30), but to the capacity of providing a scientific account of a phenomena when induction lead to partial results, empirical opposites, etc., and not to *the explanation* of the phenomenon (I know that this might sound "strong" in social science, but I am thinking in terms of the example of Galileo). Milton Friedman believes that the process of "construction of hypothesis" must be discussed in "psychological, not logical, categories" (Hausman, p. 301), whereas Dan Hausman argues that "there is little basis for denying that there can be a logic of [scientific] discovery" (p. 302).

Paraphrasing Plato's "Meno", is it true that the process of scientific discovery cannot be taught as long as its nature and causes are unknowable? It occurs to me that if the greatest discoveries in science are results of the kind of the creative, critical and reflective thinking described not only but Galileo but by Einstein, Marx, etc., it follows that the history of thought and the critique of current scientific practices are *indispensables* to "learn" the process of concept creation and, therefore, for future great discoveries. **[This is an interesting problem. Popper obviously draws a sharp contrast between the context of justification and the context of discovery, and feels that the former obeys much more systematic rules than the latter. I suspect that the cognitive leaps that are described as "bold conjectures" in the creative process of discovery is actually a series of distinct processes rather than single giant leaps, and that some of these are deeply informed by anticipatory-justification: that the scientist has in mind the process of justification as alternative potential conjectures are considered and sifted, so that there is a kind of backwards effect of future justification on present discovery.]**

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**15. Matt Nichter**

Not much to say about this week's readings, as they are birds-eye-view peices.

Minor quibble: I found Craib's and Benton's introduction of the H-D model confusing, since as I understand it the H-D model is intended as a model of confirmation, and has nothing to do with the explanation of macro-phenomena in terms of micro-entities per se .

Substantive point: The history of every science is replete with attempts to rule out, on methodological or other a priori grounds, theories that later turned out to be true or otherwise versimilitudinous. All of the extant philosophical analyses of explanation, confirmation, etc. suffer from well-known, seemingly insuperable difficulties (rehearsed by Hausman). This suggests, to me at least, that analytical philosophers' obsessive focus on the formal relations among propositions is misplaced; better to examine the relations among the people evaluating the propositions, in order to see which patterns of interpersonal interaction - i.e. which social institutions - tend to promote scientific progress. Most analytical philosophers seem to fear that shifting the focus in this direction leads invariably to some form of relativism/social constructivism, but I don't see why this need be the case. Just as it is now a commonplace among philosphers of language that 'meanings ain't in the head', scientificity may not be in the heads of individual scientists either. **[This is an interesting proposal, but isn't it, in a sense, somewhat parasitic on having at least partially resolved what it means to have "scientific progress"? That is: in order to answer the question, "which social institutions - tend to promote scientific progress" – you have to have criteria for what counts as progress, and this would seem to require some sort of principle of knowledge-advance, which brings us back to the epistemological issues.]**

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**16. Matt Dimick**

My weekly interrogation raises some issues discussed in the Hausman reading. One reason Hausman finds the deductive-nomological (or DN) model of explanation attractive is the fact that it "show[s] the necessity of the phenomenon to be explained" and fits the intuition that "explanations reduce contingency." What I am wondering is what is Hausman's understading of the relationship between the DN model of explanation, law, causality, necessity, and contingency?

The first problem I have is the relationship between causation and necessity. Are relations of cause and effect forms of *necessary* relationships, so that we can't avoid implicating necessity in any kind of causal or explanatory account? Even if a factor is sufficient but not necessary for an outcome, is there still some necessity in the fact that the outcome was caused anyway? Or is Hausman's understanding of explanation and necessity such that he counts contingent causes as not rising to the level of scientific explanation? **[Contingent causes would certainly count as causes to Hauman. But they still involve a certain kind of necessity with respect to the effects that they produce. The "necessity" in a purported causal relation is between the cause and the effect it produces. A potential problem here is in multicausal systems in which there are interactions and countervailing causes, so that the effects of X are neutralized by the operation of another cause. This is one of the themes of transcendental realism, and leads to the triplet of cause-effect-event, where events are what we observe, not effects.]** I can certainly conceive of causal arguments that are contingent in some sense. For example, one cannot predict the path of biological evolution, nor say that any particular form of life necessarily had to develop and come into existence. Then again, we can cite certain laws (which perhaps should be called statistical regularities, but I'm not sure that matters for this argument) of evolution that "reduce" the contingency, so that we at least know that evolution follows *some* path and we can understand what causes those paths, even if we cannot deduce from those same laws which path will be taken. But in order to explain the evolution of a particular organism, we would have to supply additional factors extrinsic to the "laws" of evolution.

Secondly, what is the status of accounts such as this last one, where a particular organism is explained, using evolutionary laws in the explanations, but also a series of extrinsic factors? For example, I believe some scientists argue that certain geologic events in Africa had an effect on subsequent human evolution. Or we might pitch an explanation in the language of game theory where, according to the "laws" of rational behavior, there may be several stable multiple equilibria, and to explain which equilibrium is selected requires resort to factors other than individual rationality. Are these accounts causal but not explanatory, according the DN model? Are the "other factors" simply other phenomena that could be fashioned in terms of laws and initial conditions? For example could we just state certain laws of evolution, certain laws of geology, supply some initial conditions and have a DN explanation of the origins of human life? **[The DN model always has side conditions of various sorts, and I don't see why the contingent causes couldn't figure in these conditions. The laws of evolution explain why certain mutations become diffused in a population – they diffuse because they increase reproductive fitness – but they do not explain the mutations themselves, nor do they explain the impact of asteroids of earth that change the climate. But this is still – I think – a proper law, with an array of side conditions and other mechanisms.]**

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**17. Natalie Jahr**

Towards my lecture I asked myself at what point one should start to talk about sociological theories and how it differs from philosophical thinking. I wonder if they get separated by their empirical testability. Concerning social patterns, I think it is very hard to establish a theory which could be ranked as scientific.

In the German language we have the word "verrückt" which on the one hand means *crazy* or *mad* on the other hand it means *to displace something* or *to move something out of place*. In my mind this seems very interesting. Referring to the discussion it means that we have a rule, that we expect things to be or to happen. For example it would be a rule or a habit to behave in an quiet and respectful manner during a lecture, whereas if somebody would jump off his seat and cry out loud the course might say that the person is mad, in German *verrückt,* he moved out of place. Which means that there is a kind of rule or custom how to behave in society. You would get punished if you disrespect that rule/norm or at least called crazy.

I was wondering if it is possible to find a theory which will be able to explain social behaviour. That would also be a point to criticise rational theories concerning human behaviour. Because this would reduce it to cause and effect, which might work well concerning natural science but in my mind it would be terrifying if it would be that easy to explain social causalities.

I was thinking about Robert K. Merton's *self-fulfilling prophecies*. Like if you want a certain result you can work on it and get it, but that would lead to pseudo-theories as Popper called them.

For every scientific work there should be logical laws which will be respected. But at the same time, little logical games will show, that logical thinking might fail and conduct to wrong conclusions. So before I can start to work on an question I should be able to pose a question. But already there the problems will begin. Mostly by asking a question there is an answer which I would expect, so the aim would be, to proof that the expectation was right or to find out, that there might be a repetition but not a law. And at least it would require objectivity. **[The fact that you <u>expect</u> a particular answer does not at all mean that you will actually get that answer when you do research. That is, while the self-fulfilling prophecy is a psychological/cognitive process, I don't think it is the case that either in natural science or social science all "prophecies" (i.e. predictions) are self-fulfilling, and thus falsification is a common enough occurrence.**

So I wonder if the magic thing about theories is to prove things which everybody would have expected them the way they are, like puzzlingly obvious things? How far is this possible concerning social patterns? If I would raise a theory, how could a make sure that it is scientific? What is about theories which claim their self as scientific but apparently if you test them, are not at all? **[According to Popper if you <u>can</u> test a theory, then it is scientific even if it turns out to be false. Scientific theories are theories which make predictions about the world which may not hold true. So, the way to make sure a theory is scientific – in Popper's sense – is to formulate it in ways that make "risky predictions."]**

To come back to the German word *verrückt*, is there a rule if you can find an exceptional case? Or are these customs and habits are just repetitions?