Faith and Heresy in Social Science:
Commentary on “The Seven Deadly Sins”

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I *Preliminary Comments*

At one level, this paper works off of several contrasts. The basic contrast is between positivism (suitably updated, and more on positivism and the updates below in a later section) and postmodernism. 'Postmodern' is never fleshed out, but for the purposes of the paper it serves as a polemical foil in this subliminal way: If we don't get positivism right and if we don't encourage and enforce the right kind of positivist practice postmodern darkness might reign. But, still, better bad positivism than postmodernism.

So the postmodern bogeyman works to focus and limit the discussion and the discussion then is designed to tell us, "This is what good positivists should do". The paper is much more about positivism than it is about "quantitative analysis" and Schrodt, to his credit, never draws a sharp line between quantitative and qualitative analysis.

The problem with this contrast is that it forces a false choice. Stick to basics for a moment: The contrast to "postmodern" is simply "modern", not "positivism". I will leave to one side the possibility of a postmodern positivist but it is not difficult to conjure up a modern non-positivist.

But then another part of Schrodt's argument kicks in: The only science is positivist science or, maybe to be a bit more careful, the only viable and defensible philosophy of science is positivist. And then the point to take away is that modern non-positivists are not and cannot be scientists, strictly speaking. This follows from the argument, implicit but recognizable in the paper, that the only legitimate (philosophy of) science is positivist. Whatever it is that modern non-positivists are doing, it is not *science.*

But why worry about any of this? Let's just get on with the job – that is with scientific research and leave these kinds of issues to the side. After all, if we pause too long on them, we are no longer really scientists doing what scientists do. Or perhaps we can invoke a division of labour – let's let other deal with these issues. That way we can continue on our scientific way and others can write down the descriptions of and ostensible justifications for what we do – justifications, by the way, that presume the truth or pragmatic value (not quite equivalent things) of positivism, justifications that in effect presuppose what they should be expected to demonstrate.

According to the positivist canon, these justifications should be fairly thin. Positivism does not need metaphysics, which is differentiated by positivists from the realm of physics ('physics' is a conceptual placeholder for all of empirical science, used for its contrast with metaphysics) just by distinguishing nonsense and sense, non-meaning and meaning. More strongly, positivism eschews metaphysics. Metaphysics is the refuge of philosophers or, better, the refuge for the *wrong kind* of philosophers and other assorted scoundrels. Now this may look like an outdated interpretation of positivism. I agree that it is outdated in this sense: This kind of argument can no longer be found or recognized in contemporary philosophy of science but that is because contemporary philosophy of science is not positivist. Positivism has not been updated, it has been left behind.

Here is the rub: Positivism is philosophy, it is not science. It might be philosophy without metaphysics or with the thinnest possible metaphysics but, still, philosophy – even an ostensibly metaphysic-free philosophy -- is not science.

Now there is a different possibility here: Perhaps the point of positivism is to obliterate any distinction between philosophy and science. But if this is the point of positivism, then positivism has been largely unsuccessful – science and philosophy are differently-constituted realms or activities. There is a further emendation to this possibility, however: Perhaps the point of positivism is to argue that philosophy should emulate science. But this is to argue, eventually, that there is no meaningful distinction between science and philosophy because they are governed by one shared set of standards and regulative principles.

These are all interesting possibilities, and worthy of far more discussion. Let me put it this way: I concede that I have not shown in these brief remarks that philosophy and science are different activities but, it must be said, neither has Schrodt demonstrated (1) that positivism is metaphysic-free, or (2) that there is no meaningful distinction between science and philosophy or (3) that philosophy should emulate science.

By way of proceeding, I will take for granted that there is a way (method or methods, the singular or plural should not matter) of doing empirical research that matches a philosophy of doing empirical research – that is, a philosophy of science – and that ‘way' or method is positivist. By this token, any method that matches a positivist philosophy of science is a positivist method. There are other ways – other methods -- of doing empirical research that match other philosophies of doing research (other philosophies of science) and these methods are not positivist.

In neither case, does it matter which is written down first (the research results or the philosophy) or how we go back and forth between them. The philosophy might come later as a form of reflection on practices, it might come first as a statement of how to proceed, they might move together as we move between them.

But there are two things we should not do in light of this variety in doing research and describing and justifying what we are doing. First we should not throw in the towel. It is not the case that anything goes, despite the variety. But the second thing we should not do is to allow or endorse the positivist finesse, which works by relegating to pre-scientific or non-scientific status all forms of empirical research that do not match a positivist philosophy of science.

The reason we should not allow this finesse is simple: It is, I argue, manifestly not the case that a positivist philosophy of science is the only game in town. Positivism is no trump card.

But more generally it is not obvious that there are any trumps at all. This is not to say that there are no rules at all. We are going to want to say in some fashion that some hands are better than others (and perhaps that this hand was played better than that hand and perhaps that this hand *could* have been played better).

Such are my preliminary remarks. I have one final observation for this section. This paper (Schrodt's, not mine) works at different levels and that is a virtue. There are (a) comments on method proper, but there are also (b) comments on philosophy of science (or 'meta-theory'), (c) some comments on what I would call the history of ideas, (d) some philosophy proper and (e) a little sociology of science. You could call this, once fully fleshed out and reorganized, a *stratified* approach or argument in which various pieces sit, each at an appropriate level, but linked together. Put differently: We can read Achen on garbage can regressions, we can read all sorts of people on collinearity and non-linearity and heterogeneity, we can read King, we can read *Political Analysis* which, as Schrodt notes, tops the ISI charts in political science. But none of this reading will expose us to what Schrodt does in this paper.

And I think this helps to identify a basic problem in the discipline. Positivism encourages the belief that science can get along without philosophy. This is the basic positivist conceit. All we need is the empirical work, a textbook or two on methods, some software, and an occasional gesture in the direction of philosophy of science and philosophy proper.

This is a little odd, given that positivism is a philosophy, but it is what it is. It might be encouraged by the way philosophy is treated in most political science departments. Philosophy tends to mean normative political theory and the history of ideas. This is not to say that political philosophers do not know a lot about philosophy 'proper' and philosophy of science -- they do – and they increasingly know a lot about empirical research. What could they teach us about research, about philosophy, about *science*? Well, actually, quite a bit if you take the stratified view I proposed above.

This paper begins to right the balance, but the more we move into philosophy of science and philosophy proper, the less compelling is the positivist story about doing empirical research. The basic reason why not all moderns are positivists is that they do not believe positivism and they have good reasons not to believe it.

The difficulty with positivism is not methodological, it is philosophical. By this I mean I can argue that positivism is not philosophically persuasive and therefore reject it, while still maintaining that some (but maybe not all) of the methods habitually associated with positivism have value, keeping in mind that the standard sorts of questions about appropriateness of application and so on of these methods in research would continue to be relevant. There is no contradiction in this position. It might be difficult and take some work but it is not in principle impossible. It is not a contradiction in terms.

I might take this forward in two ways. First, I might as a consequence be in a position to argue that some *methodological* problems are instead *philosophical* problems. These methodological problems are an artifact of philosophical mistakes. And that would be to say that treatments of these problems, or solutions for them, or workarounds, should identify these mistakes and avoid them, which is to say, given the current argument, that there are no treatments or solutions to these methodological problems from within positivism. Positivism is, by hypothesis, the problem not the cure. Second, there might be methods that a positivist philosophy and philosophy of science might not recognize as legitimate that we can accept if we can show that positivism is philosophically unpersuasive.

Now, none of this possible if positivism is like a trump card – if it is fact the only game in town, the only defensible philosophy of science. But, as I say, that is implausible, at least from my reading of contemporary philosophy of science[1].

II *A Further Comment on Positivism*

Schrodt is interested in updating positivism but his paper does begin with a discussion of prediction and explanation in which Hempel plays an important role. So I will allow myself a comment on early logical positivism. I pick out things in early positivism that Schrodt does not. He picks out a putative positivist symmetry between prediction and explanation (more on this later) and sees predictive validity as the positivist criterion that distinguishes science from non-science. I emphasize the positivist commitment to physicalism.

“It is a just demand that Science should have not merely subjective interpretation but sense and validity for all subjects who participate in it. Science is the system of intersubjectively valid statements. If our contention that the physical language is alone in being intersubjective is correct, it follows that the physical language is the language of science.” (Carnap, 1934: 166-167).

This is one statement of the physicalist thesis of positivism. Reworded: Science is an intersubjective enterprise and its intersubjectivity depends on a physical thing language. Implications: (1) The 'physical language' is the universal language of science and it is the only scientific language. (2) The thesis of physicalism underwrites the methodological

1 See for example Strevens (2008); Hacking (1983, 20th printing 2007); Thompson (2007); Manicas (2006).

unity of science. By the latter, the logical positivists wanted in particular to rule out 'empathy', 'empathetic understanding', *Verstehen* as a distinctive method in the social sciences (eg. Neurath, then later Feigl, Nagel). I will leave this specific feature – the antipathy to understanding and interpretation – to one side in what remains because it opens up issues best taken up elsewhere. But I will say that interpretation is unavoidable in the social sciences.

Back to the main themes. There is a lot more variety in social science now and Schrodt says so. We are not just the hardnosed children of updated positivism or the softhearted descendants of *Verstehen* interpretivism.

I think this statement above taken from Carnap and its implications set out the core commitments of positivism. I think this physicalist thesis is far more central to logical positivism than any thesis about predictive validity. But should we accept this starting point today? And can this position be coherently updated to meet some of the objections that could be made to these commitments?

Physicalism in positivism is a linguistic thesis. Essentially, the thesis is that every meaningful sentence, whether true or false, can be translated into physical language (Gates, 2001: 251) – into the physical thing language of Carnap. Here is a test by which to decide whether or not to accept the positivist doctrine of physicalism. I accept this thesis to the extent that it continues to be an important philosophical doctrine. But my problem in accepting it is that the positivist thesis about physicalism has been discredited in a fairly fundamental way.

Physicalism *does* continue to be philosophically important in the philosophy of mind and in the philosophy of action to a lesser extent, and in metaphysics, and there is a realist physicalism in contemporary philosophy of science (and this philosophy of science thus is saying that there is metaphysics *in* science), but this physicalism is not a linguistic thesis. Rather, physicalism is now a metaphysical thesis and metaphysics is exactly what the logical positivists wanted to deny and to avoid.

In other words, the key element of logical positivism has been philosophically updated but in a way that discredits positivism. Hence I am not inclined to try to rehabilitate positivism. I think it is more likely to be the case that what is positive about positivism should be folded into something more intellectually powerful than positivism. (and this is why we need to pay attention to the updates to positivism – the subject of a later section).

Since it was the linguistic thesis that underwrote the positivist argument about the methodological unity of science, the consequence is that positivism can no longer provide support for the unity of science. If I wanted to look for philosophical support for methodological unity, I would now have to look *outside* positivism. But the contemporary metaphysical thesis about physicalism is not a methodological thesis (Stoljar, 2009) and thus it does not require methodological unity.

I have made these points and raised these questions because Schrodt seems to be committed to a hard positivism. There is an awful lot of social science that denies the positivist unity of science. And this is not because some of this work is not mathematical or quantitative. But it is not disciplined by a linguistic thesis of physicalism.

To clarify – the preceding paragraph is *my* application of *my* understanding of positivism. Schrodt's criterion is not physicalism but rather predictive validity. As he argues in the paper, from his point of view, it is not a question of whether an argument is mathematical or not. Rather, does the argument pass the test of predictive validity? And, for Schrodt, it is not, at least not explicitly, a question of endorsing or accepting the methodological unity of science.

These differences in the interpretation of positivism are interesting. I would put the issue this way: The work in social science that denies the unity of science does so in a way that limits or shapes the application of the criterion of predictive validity. But it is not a vicious or methodologically vacuous limitation. Now, I have in mind here arguments in social science whose methodological choices are shaped by the importance they give to endogenous choice in human life. I think the basic point here is this: It might be difficult (and it might not be necessary) to impose methodological unity in the face of endogenous choice in human life. I have more to say about endogenous choice below but first let's now look more closely at updating positivism.

III *Updating Positivism*

These last few points regarding the treatment of positivism are relevant to Schrodt's interest in updating positivism. The update, compactly summarized, amounts to introducing error as a way of correcting for the earlier determinism of positivism. This amounts to allowing for error in previously "deterministic definitions of prediction". The sorts of error are just the sort to expect in the specification of any physical system: specification, measurement and quasi-random structural error. So far, so good. Physicalism is preserved, as is the methodological unity of science.

But there is a further update to positivism, and this modification seems to violate physicalism and the unity of science. This final update is the introduction of "free will". Yet because Schrodt does not acknowledge the physicalist core of positivism nor its core commitment to methodological unity, he does not face the full consequences for positivism of introducing free will. The latter is simply treated as a source of error, on exactly the same footing as the other sources of error characteristic of physical systems.

I can grant to positivism errors related to specification, measurement and complexity – a concession that frees positivism of dependence on strict deterministic definitions of prediction. But I am much more dubious about treating endogenous choice as a source of *error.* And this is what I am being asked to do when I am expected to endorse the compatability of positivism and free will. Endogenous choice is too important to the generation of the social world to be treated as error. And in addition I want to hold positivism to its physicalist-unified science core even if it is updated. Otherwise positivism lacks the edges that make it what it is.

There are a couple of places in the paper where Schrodt makes brief references to "the radical skepticism of Hume" (critically) and to "scientific realism" (positively). I don't want to make that much of these references because he does not dwell on them. There may be, as he suggests, lessons to be learned from scientific realism but it might be worth keeping in mind that scientific realism is better thought of as an alternative in the philosophy of science to positivism. Positivism is anti-realist (Hacking, 1983: 41-57). The currents of metaphysical necessity that run through some scientific realism, the interest in deep structure and in the *generation* of the world (and to some extent the interest in mechanisms) are fairly distinctive features but not of positivism. There is also a (contested) interpretation of Hume that treats him as a skeptical realist about causation rather than as an associationist or barebones nominalist or empiricist[2]. And a skeptical realist is not a radical skeptic.

And a final set of comments for this section: Schrodt uses the criticism of KKV by McKeown (1999, reprinted in Brady and Collier) in his discussion of Bayesian alternatives to frequentism. But he does not dwell on what I took to be the leading edge of this criticism. I thought McKeown's leading edge was this: Consistent positivists eschew the search for anything beyond regularities and associations and this rules out reference to causation. Positivism does not specify causal relations and an explanation that invokes causal relations is not a positivist explanation. This is an argument that positivism is anti-causal. It is a quite arresting argument, given the folk-beliefs in the discipline about positivism. Nor is it an idiosyncratic position. McKeown cites Popper but another philosopher of science, Ian Hacking (very different from Popper but neither are positivist philosophers of science), makes a similar point (Hacking, 1983: 41). Does the update to positivism remove this problem?

It is often taken for granted in social science (but not in philosophy) that positivism and an interest in causal explanation (and thus an interest in problems of causal inference) go hand in hand. Positivism is supposed to be the *scientific* search for casual explanations. This is what McKeown questions. And consider: "Positivists tend to be non-realists, not only because they restrict reality to the observable but also because they are against causes and are dubious about explanations." (Hacking, 1983: 42)[3].

These points also have consequences for Schrodt's discussion of explanation and prediction in Hempel. Recall that Hempel and Oppenheim (1948) provide the classic logical positivist account of explanation for Schrodt. But, in this account, explanations are not causal explanations!

2 See Strawson, 1989, Read and Richman, 2007 and the discussion in Bennett, 2001, vol. 2: 276-283.

3 "Explanations may help organize phenomena but do not provide any deeper answer to *Why* questions except to say that phenomena regularly occur in such and such a way." (Hacking, 1983: 41)

Here is a summary of Hempel's deductive-nomological [DN] account of explanation: "To explain an event whose occurrence is described by the sentence S is to present some laws of nature (ie. regularities) L, and some particular facts F, and to show that the sentence S is deducible from sentences stating L and F." (Hacking, 1983: 48, for an equivalent description of the formal requirements for a DN event explanation see Strevens, 2008: 9) The explanatory relation is not a causal relation. Rather the explanatory relation was in some parts of Hempel's work a "pattern-subsumption relation"; in other parts it was an "expectability-conferring" relation (For these terms, see Strevens, 2008: 10-23, especially at p. 10). But these relations were never causal relations.

Moreover, there is an explanatory asymmetry built into the deductive-nomological account of explanation. Recall that Schrodt argues that there is a basic symmetry in positivism between prediction and explanation.

Consider this counterexample: From the height of a flagpole, a simple physical law about light and the position of the sun I can deduce the length of the shadow cast by a flagpole. Then, following the DN account, I can explain the length of the shadow. But I can also deduce the height of the flagpole from the same law and the same position of the sun and from the length of the shadow. But I cannot explain the height of the flagpole from the length of the shadow. There is an obvious asymmetry: The flagpole causes the shadow to exist but the shadow does not cause the flagpole to exist (This is taken directly from Strevens, 2008: 24).

There is another famous example involving the prediction of the weather by barometer where the asymmetry is also clear, which I first recall reading in Watkins or Scriven some time ago. Here, to simplify, we might say that the barometer predicts but does not explain the weather. "What the barometer does not cause, it does not explain" (Strevens, 2008: 24). These counterexamples are lodged at the heart of the DN model of explanation and they are considered counterexamples because the explanatory relation in the DN model is not a causal relation.

These examples sound trivial, especially for those looking for immediate validation for their empirical research, but only because we always assume that positivism is designed to deliver causal explanations. But the most important formal account of explanation in positivism delivers explanationary relations without causal relations. That is how these counterexamples arise. The larger point here is not to allow positivism to trade on the strengths of scientific realism.

Thus, there is an update to positivism that goes unmentioned by Schrodt and it is a doozy of an update. The unmentioned update is causation! But, really, once you insert causation has positivism been updated or is it being trimmed to fit something more intellectually powerful?

IV *Endogenous Choice*

I have in mind political scientists whose methodological choices are guided by the importance they place on endogenous choice.

Here are three quotations:

"If we accept the premise that politics is the result of making choices, then the entire history of political interaction is one of repeated selection. Put another way, then history is one grand selection model." (Signorino, 2002: 94).

"The conditions under which we live are somehow created by people in pursuit of their ends…We thus must treat the observable world as having been produced by 'us', that is, as having been generated endogenously." (Przeworski, 1995:17).

"Endogeneity is the motor of history." (Przeworski, 2004a, 2004b, 2007).

Now, I am going to make some claims and obervations about these quotations and these authors. I regret that I do have the time here to fully develop what I need to say in greater detail.

1.These are social scientists, not philosophers of history but their statements (particularly the first and third) are (unelaborated) philosophies of history. 2. These statements are far more compatible with some form of scientific realism than they are with any form of positivism. 3. Endogenous choice is not treated as a source of error. (There are qualifications to build into this for Signorino). 4. In neither case is endogeneity a methodological vice. 5. Endogenous choice implies self-selection and self-selection implies endogenous choice. 6. There is not a lot of self-selection in the physical world, whether it is inanimate or animate nature we are thinking of, nor is there a lot of endogenous choice[4].

1. Endogenous choice does not imply that tomorrow will be a blank slate. 2. You are invited to accept a premise in the first quotation. If you decline the invitation, then you are committed to the proposition that the social world is completely determined by exogenous variables. 3. Introducing an error term changes nothing – you are not thereby freed from this commitment to determinism.

V. *Bayes Rule*

An important part of Schrodt's update to positivism substitutes bayesianism and/or folk bayesianism for frequentism. There is an equivocation in bayesianism particularly on the folk bayesian side which suggests that the problem of induction is neither solved nor deflected via bayesianism. As far as I know, the equivocation was first identified by

[4] An aside: One big thing I have not talked about is the importance to human life of genetic and non-genetic mechanisms of transmission and selection. (To me, it is till an open question whether non-genetic cultural transmission is strictly evolutionary). Too far afield from present concerns.

Hacking (1967. For further discussion see Howson and Urbach 1993 and Hacking 2001: 258-260. Note that my presentation below draws directly on the latter).

Suppose I am interested in a hypothesis H and some evidence E.
I have a prior personal betting rate on H, represented by a prior probability Pr(H).
I have a conditional betting rate that E will happen, conditional on H being true. This rate is represented by the probability of getting E if H is true, Pr (E/H), or the likelihood of E given H.
I have degrees of belief like this for various possible hypotheses.
By Bayes Rule my personal posterior probability Pr(H/E) should be proportional to Pr(H)XPr(E/H).

But these probabiliites need to be indexed by time:
For a truly posterior (later) probability at time t*, later than t, where you know evidence E, you want $Pr_{t*}(H)$.
For a truly prior (earlier) probability at time T when you don't know E, you want $Pr_t(H/E)$.

Then you need the following postulate: $Pr_{t*}(H)=Pr_t(H/E)$.

But bayesianism provides no argument for this postulate. It might be added as an additional axiom or maxim (Hacking, 2001: 259) but then Bayes Rule is reduced to a rule of thumb. What this result suggests is that there is an important remainder – an indeterminacy – in the formalization of bayesian learning. We can live with it but we should know it. Better to acknowledge than to ignore. And it is a fascinating issue.

This result from within bayesianism is consistent with the conclusion that there is no solution to the Humean problem of induction that does not beg the question. I wonder as well whether this result is related to the formal requirement imposed on backward induction. Backward induction requires a condition of common knowledge rather than the weaker condition of common belief. In effect, common knowledge does its work in the backward induction by finessing the problem of forward induction.

VI. *The Qualitative Side*
I have made my major comments. What I say now is more of a placeholder for a further argument to be developed somewhere else. Schrodt values the contribution that “qualitative” thinkers have made to this conversation. And so do I. But the virtues of much of this work, I would argue, if you were to put it in terms of philosophy of science, is not rooted in positivism but in a form of scientific realism. The work of Bennett and George, for example their discussion of de facto generalizations and relexivity, some of the work of Ragin (and others), the recurring interest in forms of 'process tracing', the interest in mechanisms, the interest in moving beyond and 'below' statistical associations and empirical regularities in constructing depth explanations, none of this draws its methodological virtues from positivism.

VII *Concluding Remarks*

I would certainly concede that there is much in this commentary that needs further development and defence. But it is a commentary and not a paper, at least not yet.

I admire anyone who tackles these kinds of questions at the many levels in which they are implicated, as Professor Schrodt has done in his paper.

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