

Comments on Hillinger Paper

1. I am sorry that what I will say will sound harsh and critical, but this is a very ambitious paper, and it is therefore not surprising that I have many objections. And as usual in refereeing, I have talked only about the points that bother me, not about the points that I like.
2. The author starts out by saying that the term “ideology” is often used as a rhetorical device to dismiss ideas one disagrees with. He then defines ideology in a more meaningful way. But subsequently he charges his opponents with being ideological, thus doing precisely what he has previously condemned. Too much of the paper sounds like “I am right, and anyone who disagrees with me is ideological”.
3. A much more detailed discussion of the meaning of ideology is needed. There is a large literature on this that the author ignores. Perhaps one should treat ideology as the social-science equivalence of a paradigm in the natural science. Moreover, natural-science paradigms, too, might perhaps be considered ideologies. (I am not a post-modernist, but I can sympathize with a parapsychologist who thinks that science, too, has its ideology.) Or, perhaps one should think of ideology as the practice of evaluating evidence by the (political?) conclusion to which it leads. Furthermore, there have been at least two empirical studies of the extent of ideological reasoning in economics (Fuchs et al, *Journal of Economic Perspectives*, and myself in the *Journal of Economic Methodology*. I suspect that we will learn more from detailed *empirical* studies of the effect of ideology than from denunciations of it. This paper goes part of the way towards the former, but not far enough. For example, in discussing the Impossibility theorem it does not take up alternative explanations of why it was given so much credence. Perhaps its cold-war connection was much less important than its innovative use of symbolic logic, or its extension of the scope of economics. I don't know if this was actually the case, but it needs to be considered. In general, the paper shares an undesirable feature of the Chicago school. Becker, etc, show that rational behavior can explain a certain phenomenon, and then instead of asking whether this explanation is more plausible than alternative ones just assume that it is because it is economics. This paper does the same thing for cold-war explanations.
4. A recent trend in the philosophy of science has been to argue that each field has its own legitimate methodology, and that, say biologists and economists should not necessarily try to copy physicists. I suspect that the author can deal with this objection, but it should be discussed.
5. The paper makes a lot of assertions about matters such as economic interests or cold-war issues being behind the ideology of Chicago (and other) economists, assertions that need to be backed up. For example, the Chicago school was vehemently pro-market already in the 1930s. The author subsequently refers to

this early Chicago school, but in his initial discussion of it treats it as a cold-war phenomenon

6. The author does not give enough credit to the progress that economics has made in recent years in paying attention to empirical evidence. For example, many more papers now provide robustness tests. We are still a long, long way from physics, but we are slowly digging ourselves out of the mud.

7. Some more attention to details may be in order. Marshall's first name was Alfred, not George.

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