

A COMMENT ON DANIEL KLEIN'S "A PLEA TO ECONOMISTS WHO FAVOR LIBERTY"

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It is somewhat hard to respond to a paper, which in general is in accord with your own reasoning but nevertheless disagrees with you firmly on a least one important point. I think an ambitious young economist would be well advised as a career move to engage in at least some efforts to improve policy by publishing or speaking to a non-economic audience. Klein disagrees.

He goes to the extreme of suggesting that anything of this sort be deliberately removed from the ambitious young economist's vita. I don't think that that is good advice but I would suggest that such papers or speeches be segregated. Thus the vita would contain a list of "scholarly" articles and a second list of "education", "public relations", or "policy relevant" papers. This is not only worthwhile; it is more honest than putting all in one long list. Your vita would still have the same number of pages, but the reader would realize that not all of them involved as much technical knowledge as the others. I think most employers would regard policy interest as a plus even if not an overwhelming plus. Further, as I pointed out in that part of my speech Klein quoted, it is fairly easy to become an expert on such a subject. The expertise does not have to be original or profound. It is aimed not at the professional economist but at the voter or perhaps the Congressman or local government.

The above is substantially my only difference with Klein. I do think the well-intentioned economists who spend some time attempting to improve economic policy by addressing the common man or even the government official will benefit the world and will not injure his own career. He may actually benefit it. Normally, however, these articles, speeches, and even letters to the editor will not help him much in his career. I regard this as a serious criticism of the economic profession. The only reason for economics is to improve policy, mainly political policy but to a minor extent policy followed by businesses.

To take an outstanding example, Henry Hazlitt was for many years the Economic Correspondent for the *New York Times*. During all this period, the *New York Times* opposed minimum wages. No doubt, this was an example of his influence. When he retired, it became an advocate of minimum wages. Granted the influence of the *New York Times*, it seems likely that Hazlitt did more to improve economic policy than any five full professors of economics during this period. Nevertheless, he would not have been regarded as suitable for appointment in any leading university. No doubt he did

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well financially, and for that matter his popular books sold well. Nevertheless, the economic aristocracy never recognized him.

The rather low status of teaching, particularly elementary economics teaching, is indicative of the problem. Since, to make an embarrassing confession, I myself am not fond of teaching, I benefit from my high status. At my rank, I have few classes, and in general the students are good and interested. It is quite a different matter for those people teaching gigantic elementary courses. Nevertheless, from the standpoint of influencing future policy, the elementary teacher is more important than I. I hope that my work will trickle down to the elementary teacher and through him to the large number of potential voters, potential Congressman, and potential newspapermen in his class. This is however merely hope. I don't actually do anything to make that more probable. It is true that my writings are, generally speaking, much more accessible to the ordinary person than most economic writings. This may help somewhat.

Nevertheless, the present situation is in my opinion very undesirable. Economics is a policy science and we should be trying to influence policy. The vast output of the average economic journal contains little that would influence policy. By coincidence, I received my copy of the *American Economic Review* while preparing this comment. The general quality of the articles was, of course, excellent. Since they publish only about eight percent of the articles sent to them, that would be expected. Further, most of the articles have at least some policy relevance. The policy relevance, however, is usually small and to a considerable extent negated by the difficulty a layman would have in reading them. Indeed, I would suspect that most teachers of elementary economics would regard the labor input from reading them as greater than the value they would derive.

From the standpoint of all-wise governments, subsidizing this kind of research would be worthwhile. It would be particularly so if reinforced by further research suggesting changes in policy, or in other cases, indeed far more frequent cases, suggest that the existing policy be continued. Government, however, is not all wise. This information will never reach the existing governments. Only if it is digested and simplified would it affect policy. Further, we have additional problems here. In addition to reading the *American Economic Review* I read *The Washington Post* and *The Washington Times* to get two views of political developments. Both of them not only carry many columns but also direct news. Some of the columns and some articles present correct economics. Others, however, tend to mislead policymakers. It is not obvious that the policymakers can tell them apart.

Economic Affairs presents good economics but other sources in the economic bookshops tend to present plausible errors. On the whole, the average economist would have more policy influence writing letters to editors than articles for the *American Economic Review*. Unfortunately, as Klein points out this would have a lower payoff under present circumstances. I do not think it would have no payoff but I do agree the payoff would be less. Nevertheless, the cost of producing them would also be less.

Currently, there is a debate at the popular level about various aspects of economic policy. I would like to have a much better debate. This means more articles of this sort and better quality. In this case, I cannot argue that the economic profession

has failed because editorial decisions in the popular press are not made by economists. Here again, the failure is at an earlier level.

I regret to say that although I have thought about it carefully I have no practical suggestions. Perhaps we could do something about the appointment and promotion process in the universities. The problem here is that any effort to change things is likely to get immediate bad publicity. Further, experienced people in this area normally are pessimistic. In a number of cases, money has been put to establish new schools or endow chairs, but the educational status quo has been too powerful.

So far I have mainly agreed with Klein. I now wish to turn away from him and then disagree with him in areas where he is more conventional than I. The articles that he refers to as "rigorous", in my opinion frequently are not. There two areas here, one is statistics and the other mathematics. Let me begin with statistics.

I was a student and friend of Karl Popper. Therefore I am strongly in favor of testing. Further, in economics this frequently means statistical testing. Popper did not confine himself to that type of testing but he also approved of it and made contributions to statistical theory. I follow him in this area although my contributions to statistical theory are pretty trivial.

There are two basic problems here. The first of these is the significance test. Suppose, for purposes of illustration we use .05 as our test. If we take a large number of potential bodies of data, at least 1 in 20 should show significant relationships. This is true whether there is any true causal connection. What .05 means is that there is only a 1 in 20 chance that random data would have this close a relationship. Granted the number of tests performed and the fact that those that do not show significance are not even submitted to a journal, it is likely that those submitted to journals and published have a much higher probability than 1 in 20 of being chance relationships.

Richard Palmer [1999] has an article¹ on the subject in the current *American Naturalist*. He emphasizes that the published articles are frequently a subset of articles that were significant. Articles that do not have enough significance cannot be published, and an even lower-level problem is that the researchers will stop and shift to another topic if they do not get significance. Thus the random occurrence of significant correlations should be much higher than 1 in 20. This is not the whole problem however.

There is then the sheer accident that correlations programs are not identical. Most researchers simply use the one that is in their computer. Normally this makes little difference but sometimes it does. More important, there is data torture. As Ronald Coase says, "if you torture the data long enough it will confess".² The young researcher, convinced he knows the truth will make changes in his specifications and very likely produce significant results. In some cases this is correct; his original specification was wrong and his new one is right. Nevertheless, this procedure reduces the significance of the significance test.

There is another problem—the data itself. Levy, and Feigenbaum³ [1990] have attempted to duplicate many statistical tests. In a few cases, the data seems to be either misreported or in some cases invented. The basic problem they discovered, however, is that it is very hard to duplicate many studies.

The reader is referred to their article [1993] in *Social Epistemology* and to the more than 50 pages of comments on it (including one by this author), which follow it. Notably, no one denies the difficulty of duplicating statistical articles. Surely this raises questions as to whether they should be called rigorous.

Although this lowers the value of individual articles, I think that in bulk they support the theory. In a way, if 80 percent of the articles' statistical tests are correct, then the general theory is probably correct and hence the correctly derived theoretical deductions in the other 20 percent are probably right even if the statistical test is an artifact.

If the reader wishes to have his doubt of statistical research reinforced, I suggest he read the lengthy debate set off by Card and Kruger. This is partly theoretical, but mainly a squabble about statistics. Once again, it raises questions about the use of the word "rigorous" in discussing statistical work.

Let me now turn to mathematics in economics. Here I have no questions about the accurate nature of the work. Occasionally, there are errors but they are rare. The problem with mathematics in economics is it is so largely decorative rather than useful. Indeed on it is the opposite of useful since it makes articles that are basically simple hard to read.

Let me turn to an experience of my own in connection with my first book, *The Politics Of Bureaucracy*. Anthony Downs had been reading the manuscript and making helpful suggestions. In one part of the book, I discussed the tendency of information to be degraded as it goes up or down the administrative pyramid. For this purpose I used the compound interest formula. Downs commented that I could use calculus and in fact provided the necessary calculus. Needless to say, his calculus was impeccable. I did not use it because it seemed to me unwise to use more complicated tools than I needed. In essence he was proposing a decoration to my book. Much mathematics in articles on economics is similarly very decorative.

But this decoration by itself would be harmless except for the fact that much of the work is designed to show that the author is right up with most recent and obscure developments in mathematics. Thus, although he makes no mistakes, this means that most readers cannot read the article. Fortunately, in many articles the author begins or concludes by telling you in English what he is doing. Frequently, this is enough so that is not necessary to read the rest. After all, one can be rigorous in English. Archimedes and Apollonius were rigorous in Greek, and English is just as good. Still, a great many readers simply can't follow this math. They are thus prevented from reading things that might have policy relevance.

Although this leads to a good deal of waste paper and hence ought to be opposed by environmentalists, its real cost is imposing a barrier of non-comprehension between people deciding on policy and those people best equipped to advise them. The question is, can we do something about it? Klammer and Colander [1990] did a very interesting and nonmathematical study of graduate education in economics. Perhaps the most important result was that the graduate students didn't like the overwhelmingly mathematical subject matter. Nevertheless, they thought they had to have a mathematical background to get a job. Probably they were right to believe this.

At the same time, undergraduates seem to be moving out of economics courses and hence reducing the demand for teachers. Some American universities have stopped teaching graduate economics. It is hard to say how far the rot will spread. It is, however, clearly a major problem, and I have to admit that I have no solution. One American university is considering changing its graduate program to drop a great deal of mathematics. A couple of other departments do not emphasize math. Whether this effort will be successful, I do not know.

Klein raises the problem but does not solve it. I regret to say that I cannot go further than he has. It's a problem that should attract the best minds in economics. Unfortunately, they are mainly engaged in the type of research criticized by Klein and myself.

NOTES

1. I cite this instead of articles in the economic literature, including some of my own, because it is a very good article and shows we are not alone. Further, the problem in some ways is less severe in biology because they have more data and more control over their data. We use the published data; they can create their own data and expand the data set at will.
2. I have heard him say this several times. So far as I know he has never published it.
3. Feigenbaum and Levy artificially introduced error into a body of statistics and tested its effect. With minor errors the effect was not fatal.

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