Department of Economics
Working Paper Series

Honesty and Integrity in Economics

Thomas Mayer
University of California, Davis

Thomas Mayer
University of California, Davis

January 21, 2009

Paper # 09-2

When looked at individually there is little reason to think that economists lack integrity and are dishonest. Yet, when we look at academic papers written by economists we can see biases. This paper tries to reconcile these two observations by arguing that the constraints the profession sets on permitted practices are loose enough to allow economists to maintain their biases while conforming to the mores of their profession. There is little reason to think that economics is worse in this respect than some other fields.

UCDAVIS

Department of Economics
One Shields Avenue
Davis, CA 95616
(530)752-0741

http://www.econ.ucdavis.edu/working_search.cfm

Honesty and Integrity in Economics

Thomas Mayer*

University of California, Davis

(e-mail: tommayer@lmi.net)

JEL Classification: B 41, A 14

Key words: honesty, integrity, culture of economics, significance tests, data mining,

Paper presented at the 2009 San Francisco session of the Association for Integrity and Responsible Leadership in Economics and Associated Professions. I am indebted to Brian Sloboda for helpful comments.

* E-mail address: tommayer@lmi.net

Abstract

When looked at individually there is little reason to think that economists lack integrity and are dishonest. Yet, when we look at academic papers written by economists we can see biases. This paper tries to reconcile these two observations by arguing that the constraints the profession sets on permitted practices are loose enough to allow economists to maintain their biases while conforming to the mores of their profession. There is little reason to think that economics is worse in this respect than some other fields.

Honesty and Integrity in Economics

Thomas Mayer

The essence of scientific method is honesty; compared to the role that honesty plays in the progress of science the issues that philosophers of science argue about, such as falsificationism or the realisticness of assumptions are merely technical details. If scientists cannot rely on the honesty of their colleagues then it is hard to see how science can progress. Economists who want to enhance the scientific status of economics should therefore pay some attention to the seemingly simplistic issue of the honesty of economists, and not focus entirely on such "sophisticated" issues as the role of models and status of unrealistic assumptions.

Since just about all economists agree that honesty is desirable this requires no discussion. What does need discussion is just how honest economists are, and what can be done to enhance their honesty. Given the high pay-off that publication has for academic economists, and the extent to which dishonesty can enhance the prospect of publication in a prestigious journal, a profession that takes as one of its core propositions that people maximize utility should ask whether its members sometimes maximize utility by being dishonest. In trying to answer these questions I will for two reasons focus on academic economists. First, since they unlike most government and business economists publish their research more information on their research is available. Second, I have spent most of my career as an academic economist, and have had only a short and outdated experience as a government economist. And in discussing the extent of dishonesty the limited availability of hard evidence forces one to rely in good part on one's impressions and "feel".

I

One way in which an academic economist can be dishonest is to accept bribes from students to raise their grades. It seems likely that this is extremely rare because so few cases of it have been exposed, even though the risk of exposure is high. A student who has paid a bribe may, perhaps while drunk, tell other students about it, and as these tell still others deans and department chairs hear about it. This danger of exposure is sufficient to explain the rarity of bribes, though, of course, conscience and a sense of integrity provide equally good explanations, and there is no way of choosing between these explanations.

Plagiarism, in its flagrant form of copying some else's work, also seems rare; I can recall reading about only two confirmed cases of it in the almost sixty years I have spent as an economist. Again, risk of exposure provides a plausible explanation. Conscience does so too, though I would guess that fear of exposure is the stronger force. "Soft" plagiarism in the sense of making unacknowledged use of someone else's ideas is probably much more common, both because it is much less likely to be detected (and the punishment if detected is apt to be much less), and because it being less heinous, one's conscience will protest less, if only because the line between acceptable use of the literature and soft plagiarism is fuzzy. Similarly, though it is unethical, one is not likely to seriously damage one's career by failing to mention, or by mentioning only in a dismissive footnote, if in the process of writing one's paper one discovered that the idea is not new. It *should*, however generate pangs of conscience.

There have been many reports recently of physicians accepting pay-offs from drug companies to induce them to prescribe certain drugs. That sort of thing, or something broadly similar to it, is not entirely unknown in academia with respect to the choice of textbooks, but it seems rather minor.[1] And paying to influence the results of academic studies, or inducing prestigious physicians to put their names on papers that were written by company employees, something which is hardly unknown in drug-effectiveness studies (see Harris, 2008), is not a problem in economics.[2] Judge Jack Weinstein concluded that: "The odds are 5.3 times greater that commercially funded studies will conclude that the sponsor's drug is the treatment of choice compared to noncommercially funded studies of exactly the same drug." (cited in Liptak, 2008, p. A-13") The nearest counterpart to that in economics is the strong tendency of papers to reach conclusions that are consonant with those of the author's previous papers, an issue discussed below.

But we should not pride ourselves on so stalwartly adhering to the honorable course. You can reject a bribe only if you have been offered one, and I doubt that there are many potential paymasters who care enough about the outcome of our studies to offer us a bribe. (Government economists, the results of

---

[1] Publisher pays academics to comment on drafts of textbooks, and apparently sometimes select these referees in the hope that this will induce them to adopt the book in their own classes. But the payment a referee receives constitutes a bribe only to the extent that it exceeds the market price for the effort involved, and it is far from clear that this is generally so. And even if publishers select certain referees because there is a large potential market for the book at their schools that is still not bribery unless the payment induces the teacher to adopt that book.

[2] But not entirely unknown. I was once approached by the economist for a firm to produce a study the contents of which he would more or less provide, but that would carry my academic label.

whose studies can sometimes have a substantial influence on the profitability of a firm, are in a different position. But here the costs of being caught are high.)

A greater temptation at least for academic economists is to produce a publishable paper through outright cheating by making up their data or lying about the results they obtained. The high returns to publication in terms of salary increases, chances of tenure, offers from other schools and prestige make this tempting. If caught the punishment is likely to be high. But the risk of being caught is not all that great. While many journals require authors to make their data available, apparently this requirement is frequently ignored. And if subsequent to publication someone asks for the data a cheater could always claim to have lost them, or if someone does obtain the data and is unable to duplicate the author's results, he could always claim that his misreporting of the results is just due to carelessness. When working with small samples a keypunch "error" or the arbitrary omission of a few outliers can work wonders on one's R^2 and regression coefficients. And while there is a danger that an R.A. may blow the whistle, the dependence of graduate students on faculty members' goodwill reduces this danger.

No data on such cheating are available, but my guess is that it is rare, or at least unusual. If it were common one would hear more complaints about the inability to reproduce a previous author's results. Moreover, my general impression of my colleagues is that they would not stoop to such a practice. A related, though not quite as heineous, and hence perhaps less infrequent, practice is suppress unfavorable evidence and important qualifications to the results. More on that later.

For government economists it is also tempting to manufacture results that please their superiors. Despite this our data do not seem to be biased. In part this may be due to the difficulty of manipulating data when there may be many potential whistle-blowers involved in generating them. However, whistle blowing is not such a great deterrence to a sequence of small, individually more or less arbitrary decisions, all going one way that sum to serious distortions. But a law, the Data Quality Act, that makes the government liable to damage suits by users of its data may also help, Beyond that we owe a great debt to government economists for protecting of the integrity of our data.

A business economist who makes up her results to please her clients is sooner or later likely to lose her reputation, though perhaps not soon enough to make this unprofitable. I know of no evidence on how well business economists resist this temptation. In situations where a business economist acts as an acknowledged spokesperson and is expected to present the firm's line, or acknowledges being a hired gun, doing so is legitimate unless it involves making extraordinarily bizarre statements.

II

However, when looked at from another angle there is evidence that suggests dishonesty, or at least a certain lack of integrity, on the part of academic economists. This is that the results that their papers reach are uncannily consistent with the results that they reached in their previous papers. As Don Patinkin (1972, p. 142) wrote during the monetarist-Keynesian debate: " I will begin to believe in economics as a science when out of Yale there comes an empirical Ph.D. thesis demonstrating the supremacy of monetary policy in some historical episode and out of Chicago one demonstrating the supremacy of fiscal policy." If economists let the facts speak freely so that the chips fall where they may, shouldn't the probability that the empirical results of a paper support hypothesis A be no greater for an economist whose previous papers supported A than for one whose previous papers supported the rival hypothesis B?

Not necessarily. One possible explanation is that the results are driven by the econometric methods (e.g. structural models vs. calibration) or types of data, (e.g. time series vs. cross-section) used, and that an author tends to use the same methods and data in all her work on a certain theory. But even if that is so, this does not necessarily mean that economists are unbiased. The reason economist A uses a structural model and economist B calibration methods *could* be, not considerations of econometric theory, but that structural models tend to lead to the results that A favors and calibration methods to the results that B favors.

Another possibility is that all of A's empirical results are based on a certain premise that B rejects, such as wage stickiness, or even on a difference in world views, for example on the acceptance or rejection of what Thomas Sowell (2007) calls "the tragic vision". That may well account for *some* of the consistency between an economist's new and previous results, but only for *some* of it. For example, there was no such dominating premise or world view in important aspects of the monetarist-Keynesian controversy, nor is it an explanation for the consistency with which some economists find that the excess burden of taxes is higher than others do.

This suggests that bias does play a significant role in disagreements among economists. Such a bias could be ideological, or simply a reluctance to admit that one's previous results are wrong, or more generally, the result of what psychologists call confirmation bias.[3] It could also be the result of loyalty to a particular school of thought, a loyalty probably forged in graduate school, or just an attitude of "my mind is made up don't bother me with the facts."[4]

Does such a bias imply dishonesty or at least a lack of sufficient intellectual integrity? This depends on the definition of dishonesty and integrity. If these terms are defined broadly it does so since economists claim at least implicitly that their results are unbiased. But compare the following three cases: In one an economist tells an outright lie, for example she states that her results are robust to changes in the sample period when she knows that they are not. In the second she does not make such a statement, but merely refrains from claiming that they are robust, and does not tell readers that she found that they no longer hold if some regressors, whose validity is hard to determine, are included. In the third case she does not test for robustness because she is afraid of what she would find.

While in the first case she is obviously being dishonest, the second case where she does not say anything about robustness is less clear. The reader is not told an obvious lie, but he is not being told the whole truth when he has a reasonable expectation that he is. It is a reasonable expectation because academic economists and most government economists are supposed to be searchers for the truth, not attorneys making a case. Hence, even if one does not want to call it *outright dishonesty* it is fair to treat it

---

[3] For empirical evidence of substantial ideological bias in economics see Fuchs et al (1998); for evidence of relatively little ideological bias see Mayer (2001b). Neither study tested for a bias in the sense of trying too hard to confirm one's previous results.

[4] Elsewhere, (Mayer, 1998), I provide an example, the debate about the desirability of a stable monetary growth rate rule, that illustrates the pernicious influence of schools in economics.

as a lack of intellectual integrity. The third case (simply not checking for robustness out of fear of what would turn up) is harder to classify. But all that shows is that the line between what is morally right and what is morally wrong is sometimes fuzzy, surely a familiar point.

Similarly, an economist who uses a technique or a line of reasoning that he knows to be wrong is clearly dishonest, but an economist who uses one that he has not bothered to examine critically because it is an accepted procedure does not consider himself – and should not be considered – dishonest. And if my contention in the following section, that some of our accepted procedures allow economists too much leeway, is correct, this could explain the co-existence of individually honest economists and biased results. I do not mean to imply that these procedures allow *every* research project to reach the conclusions the researcher desires; but in many cases they allow us to indulge in our biases.

III

One of these procedures to confuse statistical and economic significance, so that finding that x affects y at the 5 percent significance level seems like an important contribution, even when the size of the effect is trivial. How common is this? McCloskey (2008) and Ziliak and McCloskey (2008) argue that it is very common, for which they have been criticized by Hoover and Siegler (2008a and b). But even if it occurs only infrequently that is too much.

A related illegitimate procedure is the upside-down use of significance tests, that is to imply that if you have shown that a certain claim, e.g., that x affects y is not confirmed at the 5 percent significance level, you have thereby shown that x does *not* affect y, thus waving away the possibility that what is at fault is not the hypothesis but the small size of your sample. Perhaps we should think of significance tests as being just as much tests of the adequacy of sample size as of the validity of the hypothesis. Actually, it is a joint test of both. If a hypothesis is not confirmed at the 5 percent level all we can conclude is that either (or both) the hypothesis is false or that the sample is too small. Hence, to say something about the hypothesis we have to make a more or less subjective judgment about the adequacy of the sample. And that brings back into the subjectivism that significance tests were intended to avoid. One context in which this error frequently occurs is in deciding whether to adjust the data for heterogeneity because the assumption of normalcy cannot be rejected at the 5 percent level. A similar thing applies to adjustments for unit roots. (See Mayer, 2001a; Ziliak and McCloskey, 2008).

However, when applied consistently as seems common, and not only when it supports the author's results, the use of upside-down significance tests does not on average bias the results towards those that the researcher wants to come up with, and therefore cannot account for the frequency with which the results of new studies cohere with those of the author's previous studies.

Another procedure is data mining.[5] If my initial regressions do not confirm my hypothesis I can simply make some changes, such as switching the regression equation from natural numbers to logs, changing the definitions of a variable, e.g., using M-1 instead of M-2 as my regressor, or else I can add or subtract auxiliary regressors, until I get the values of the regression coefficients that I want. In a classic example Edward Leamer (1978) showed how the estimated effect of capital punishment on the homicide rate can be made to vary from strongly negative to positive by selecting particular auxiliary regressors.

[5] For further discussions of these issues see the symposium on data mining in the June 2000 issue (vol. 7) of the *Journal of Economic Methodology*

It is tempting to conclude that this sort of fishing for desired results is dishonest and should be eliminated. But the problem lies in the words "this sort". There is an observationally equivalent type of data mining, or perhaps one should say an observationally equivalent *motive* for data mining that is not only honest, but often necessary. (See Hoover and Perez, 2000.) This arises from the fact that often the theoretical terms of economic theory cannot be translated unequivocally into measureable variables. For example, does the term "money" as used by the quantity theory correspond to M-1, M-2, M-3, or the monetary base?

Similarly, when a theory tells us that something occurs with a lag should we lag the regressor one, four or ten periods? How can we find out what to do? The only answer is to try various definitions, lags, etc., and then to select the ones that give the best fit.

Sometimes there is a simple solution which avoids the permissiveness that results from the standard data-mining practice. If you have large enough sample; break it into two parts, use one to formulate the appropriate form of the hypothesis and test that on the other part. But in macroeconomics we usually do not have sufficient data for that. And in microeconomics where survey data with large samples are frequently available, this procedure, though feasible, is for some reason rarely used. That is hard to justify.[6]

If it is not possible to use separate samples for specifying the hypothesis

[6] Another peculiarity of our accepted procedures, though one that does not increase the author's leeway, is our readiness to accept the assumption that one's data are normally distributed, even when working with a small sample.

and for testing it, the next best thing is to let the reader know about all the variants that you have fitted and the results thus obtained, so that she can decide how much credence to the results. For example, if I am told that the author's hypothesis is confirmed when a particular regressor that is not well grounded in theory is included and when the regression is run in natural numbers but not in logs, I will know better than to accept it. We economists, or at least macroeconomists, tell the world about the importance of transparency, but do not practice it sufficiently.

IV

The culture in which the just-discussed permissive practices thrive is illustrated by what happened when a series of papers pointed out that our widely used software packages are not reliable – when fed identical data there can be a wide divergence in the results they give us. (See Lovell, 1994; McCullough and Viand, 1999, McCulough 2000,) Thus McCullough and Viand report on:

> the failure of many statistical packages to pass even rudimentary benchmarks for numerical accuracy. … Even simple linear procedures, such as calculation of the correlation coefficient can be horrendously inaccurate. … While all packages tested did well on linear regression benchmarks – gross errors were uncovered in analysis of variance routines. [There are] many procedures … for which we found discrepancies between packages: linear estimation with AR (1) errors, estimation of an ARMA model, Kalman filtering … and so on. (McCullough and Viand, 1999, pp. 633, 635, 650, 655)

Since the just-cited paper appeared in the *Journal of Economic Literature* many economists must have read it. One might therefore have expected the journals to carry many short papers (perhaps in a special section for just such papers) reporting on the sensitivity of the regressions they had previously published in that journal. At the least, subsequently published papers should have discussed whether, and if so how, their results differ depending on the computer programs used. Since neither of these things happened (mea culpa) it is hardly surprising that we also tolerate procedures that allow researchers to impose their wishes on the data.

So far I have talked only about econometric practice. But theory, too, has its undesirable short-cuts. One in welfare economics is to ignore the theory of the second best. Another is to confound utility maximization with income maximization. A third is a casual dismissal of any concern with the realism of assumptions. If questioned about this an economic theorist is likely with a superior smile to refer the questioner who committed this faux pas to Friedman's (rightly) celebrated methodological essay, thereby ignoring all the criticisms and qualifications that Friedman's essay has drawn. A fourth short-cut is to employ the basic principle of rational-expectations theory, that *on average* agents have correct expectations, since people do not continue to make the same mistake time after time. But that is a long way from being able to claim that during a specific period, say 2007.1 to 209.I individual errors washed out and on average expectations were rational

V

It is not surprising that our mores show certain tenderness towards researchers by providing loopholes for them. A soft science such as economics is more prone to such permissiveness than is a hard science, because it is in any case much harder to definitively establish or refute hypotheses. Moreover, if a subject as difficult as economics uses criteria that do not provide some loopholes many fewer papers would be published, since many if not most economic research projects would fail. One might then expect an economist at a research university to publish only perhaps one or two papers per decade. Try explaining that to a dean. I am not denying that our journals use tough criteria – I have had too many papers rejected to doubt that. But their criteria are ones that require extensive and difficult work, such as the application of the latest econometric techniques and math, while providing loopholes that allow researchers to claim that they have reached reliable results when their unavoidable use of a flawed procedure, such as data mining, makes that doubtful.

Just what determines which errors are permitted and which not, is hard to say, but certain conventional errors become established, perhaps in some cases by the historical accident that an outstanding economist made this particular error, or by referees believing that this particular error is unavoidable, or perhaps not noticing it.

Suppose a paper with an erroneous procedure, e. g., confusing statistical with economic significance, does get into the literature. This tends to make another economist less leery of also using this procedure; after all, if others think that it is alright, perhaps it is, and even if you still believe that it is not, the protests of your conscience are likely to be less vehement if you can point to others who do the same thing. Moreover, the more a certain questionable procedure is used, the less likely it is that a referee, who perhaps used it himself, will object, or that the reader will form a bad opinion of you. As Alexander Pope put it (Bartlett, 1980, p. 337):

> Vice is a monster of such frightful mien
> As to be hated needs but to be seen
> Yet seen too oft, familiar with the face
> We first endure, then pity, then embrace.

However this does not mean that over time the standards of economics will inevitably decline , because as readers come across a certain mistake more often, there is a greater chance that someone will catch it, and it also becomes a more valuable target for a critical paper. There is therefore also a tendency for Invalid procedures to be eliminated. For example, we are now much more careful about assuming that expectations are invariant with respect to government policies, or that the government does not face a budget constraint, and more cautious in attributing causation to mere correlation, then we were forty years ago. Similarly, while some years ago R^2's of over 0.9 were common, we are now usually aware enough of the dangers of spurious correlation to avoid such claims.[7] Moreover, in recent years economists have become more aware of the danger of data mining, and more papers report on fragility tests, although still to a lesser extent than seems appropriate. I suspect that in the race between errors becoming acceptable and errors being exposed, the latter has won out in recent years. Economics is improving, albeit at a slow rate. even when measured, not by the elegance of our techniques, but by what we know about how the economy operates, But that does not mean that new errors do not from time to time gain a foothold.

VI

Does the use of readily available of loopholes imply that economists are dishonest? In one sense it does not. The typical *economist* is not consciously cheating, and is using procedures that are

[7] Indeed there is now the opposite danger of accepting results based on very low R^2's. A regression that "explains" only 10 percent of the variance of the dependent variable faces the danger that some of the omitted variables that explain the other 90 percent are correlated with one or more of the regressors used, so that the regression coefficients are biased.

sanctified by common practice, and that he usually thinks are valid. But in another sense we economists collectively are cheating, because we accept practices that we would admit are questionable if we were forced to confront this issue outright. Once one takes account of group-think there is no contradiction between individuals behaving honestly, and yet the combination of these individuals being less than honest and forthright, Just another example of the aggregation problem.

VII

Is this problem worse in economics than in other fields? To answer this question adequately would require substantial knowledge of several fields, knowledge that I do not possess. But nonetheless I cannot just ignore this question because it is so basic in evaluating the extent to which economists are honest.

Physicists seem more willing than economists to admit error. That is hardly surprising since their hypotheses can be confirmed or disconfirmed much more definitively than can economic hypotheses. Economics should be appraised by comparison not with the top of the pyramid, but less elevated fields, such as psychology and geology

Ziliak and McCloskey's discussion of the way psychologists use significance tests, as well as some discussion within psychology, suggest that psychologists are just as ready to misuse significance tests as are economists.[8] In biology Peter Lawrence (2003) complains that "it has become profitable to ignore or hide results that do not fit with the story being sold". And in biometric

[8] Indeed, there has been an` albeit unsuccessful` attempt to ban the use of significance tests in the journals published by the American Psychological Association.

research, according to John Ioannidis (2005), “conflicts of interest are very common … and typically they are inadequately and sparsely reported”. In addition, he argues, the bias introduced by commitment to a particular hypothesis may distort reported results and their interpretations. Furthermore, “[p]restigious investigators may suppress via the peer review process the appearance and dissemination or findings that refute their findings.” More specifically, Ioannidis complains, as do Ziliack and McCloskey (2008), about the overemphasis in medical research on statistical significance, and also about the bias that result from repeated testing. All in all, “there is a widespread notion that in medical research false findings may be the majority or even the vast majority of research claims” (Ioannisis, 2005)[9]. And as for philosophers, the lovers of knowledge and wisdom, an outstanding philosopher, Robert Nozick (1974, p. xiii italics in original), reports that:

> One form of philosophical activity feels like pushing and shoving things to fit into some perimeter of specified shape. All those things are lying out there, and they must be fit in. You push and shove the material into the rigid area: getting it into the boundary on one side, and it bulges out on another. … So you push and shove and clip off corners from the things so that they'll fit and you press in until finally almost everything sits unstably more or less in there, what doesn't gets heaved *far* away so that it won't be noticed. (Of course, it's not all *that* crude. There is also the coaxing and cajoling. And the body English.) *Quickly* you find an angle from which it looks like an exact fit and take a snapshot. … All that remains is to publish the photograph as a representation of exactly how things are, and to note how nothing fits properly into any other shape.

[9] For other criticisms of medical research and its popularizations see Goldacre (2008)

VIII

That certain fields are no better in this respect than economics does not imply that we should cease trying to improve the integrity of economics. But how? An obvious answer is to foster in our day-to-day activities a climate of greater humility, to admit that our methods are imperfect and our results less compelling than they seem. This would make us more willing to admit that some of our procedures are open to doubt, and more willing to challenge prevailing practices. It should also make us less prone to group-think.

Turning to more specific remedies, journals have in recent years cut back sharply on the number of critical comments on the papers they publish. The inauguration of an electronic journal, *Economic Journals Watch,* devoted entirely to critical comments on published papers, has taken up only part of the slack, and the resulting reduction in the probability that an error will be caught is undesirable. To be sure, if journals were to go back to publishing more critical comments that would mean admitting that a journal's referees and editor are not omniscient, and also by cutting back on the space available for original papers would tend to reduce the journal's citation ranking, but these prices are well worth paying. It would also help if journals would enforce more strictly the requirement that authors make their data and computer programs available, and if referees would ask authors to do more robustness tests, including tests using alternative computer programs.

More checking of published results would also help to make outright fraud more risky, though its main benefit would probably be more to reduce carelessness than outright dishonesty. Graduate econometrics courses could require students to check a published paper, and this effort could perhaps be underwritten by NSF grants. Data mining could be disciplined to some extent by requiring authors who use time series data to publish after, say 3 years, a brief note discussing whether their results still hold when you add three more years' data. Authors who use data that are subsequently revised, such as GDP data, could be asked to report whether the fit of their regressions improves – or deteriorates when the revised data are used.

How about a code of ethics? I doubt that it would help with the problems of academic research that I have focused on, but it would help government economists and business economists, as well as economists working for think tanks, to stand up to demands that their research generate the preferred answer. Would establishing a set of best practices help? It probably would in the short run, but there is the danger that by setting certain practices in concrete it would it would inhibit progress in the long run.

References

Bartett, John (1980) *Familiar Quotations*, Boston, Little Brown

Fuchs, V., Kruger, A. and Poterba, J. (1998) "Economists' Views about Parameters, Values and Policies: Survey Results in Labor and Public Economics," *Journal of Economic Literature,* 36, September, 1387-1425.

Goldacre, Ben (2008) *Bad Science*, London, Fourth Estate.

Harris, Gardner (2008) "In Documents, Ties between Child Psychiatry Center and Drug Maker*,"* *New York Times*, November 25, 2008. p. A-15

Hoover, Kevin and Perez, Stephen (2000) "Three Attitudes towards Data Mining," Journal *of Economic Methodology,* 7, June, 195-210

Hoover, Kevin and Mark Siegler (2008a) "Sound and Fury: McCloskey on Significance Testing in Economics," *Journal of Economic Methodology*, 15, March, 1-37

Hoover, Kevin. and Mark Siegler, M. (2008b) "The Rhetoric of 'Signifying Nothing', a Rejoinder to Ziliak and McCloskey, *Journal of Economic Methodology*, 15, March, 57-68.

Ioannidis, John (2005)"Why most published Research Findings are False," PloS Med 2(8): e124 dot: 10 237 (journal. pmed.0020124.

Lawrence, Peter (2003) "The Mismeasurement of Science," *Current Biology*, Aug 7, 17.

Leamer, E. (1978) *Specification Searches: Ad Hoc Inference with Nonexperimental Data,* New York, John Wiley.

Liptak, Adam (2008) “From one Footnote, a Debate over the Tangles of Law, Science and Money,” *New York Times*, November 25, p. A-13

Lovell, Michael. (1994) “Software Reviews” *Economic Journal*, 104, May, 713-26.

Mayer, Thomas. (2001) “Misinterpreting a Failure to Disconfirm as Confirmation,” Working-paper, Department of economics, University of California, Davis.

Mayer, Thomas. (1998b) “Monetarists vs. Keynesians on Central Banking,” in Backhouse, R., Hausman, R, Salanti, A. (eds) Economics *and Methodology*, London, MacMillan.

Mayer, Thomas (2001) “The Role of Ideology in Disagreements among Economists: A Quantitative Analysis,” *Journal of Economic Methodology*, 8, June, 253-74.

McCloskey, Deidre (2008) “Signifying Nothing: Reply to Hoover and Siegler,” *Journal of Economic Methodology*, 15, March, 39-56.

McCullough, B. (2000) “Is it Safe to Assume that Software is Accurate?” *International Journal of Forecasting,* 16, 349-57

McCullough B. and Viand (1999) “The Numerical Accuracy of Econometric Software,” *Journal of Economic Literature*, 37, June, 633-65.

Nozick, Robert (1974) *Anarchy, State and Utopia* New York, Basic Books.

Patinkin, Don (1972) “Keynesian Monetary Theory and the Cambridge School,” Banka Nazionale del Lavoro, Quarterly Review 238-58.

Sowell, Thomas (2007) *A Conflict of Visions*, New York, Basic Books.

Ziliak S. and McCloskey, D. (2008) *The Cult of Statistical Significance*, Ann Arbor, University of Michigan Press.

.

.