Significance Redux

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Science depends on entrepreneurship, and we thank Morris Altman for his. The symposium he has sparked can be important for the future of economics, in showing that the best economists and econometricians seek, after all, *economic* significance. Kenneth Arrow in 1959 dismissed mechanical tests of statistical significance. It took courage: Arrow's teacher, the amazing Harold Hotelling, had been one of Fisher's sharpest disciples. Now Arrow is joined by Clive Granger, Graham Elliott, Joel Horowitz, Ed Leamer, Tony O'Brien, Erik Thorbecke, and Arnold Zellner (Arnold is in our minds a Zeus in the matter of economic significance, with Ed Leamer as his Mercury). And we learn here from our colleagues in cognate fields that mechanical tests have been criticized by the best—for decades. It's time to stop the nonsense and get serious about significance in economics.

Why has it taken until now for economists to catch on? In his own paper Morris Altman makes a good case for path dependence. People have believed that mechanical testing for statistical significance is all right because, after all, it's been around for so long---something one might say, too, of the labor theory of value, or protectionism, or belief in séances with the dear departed. As Altman observes, even in psychology---where since the Significance Test Controversy of the early 1970s there has been widespread understanding of the issue by sophisticates---little has changed. Fidler, et al. conclude here, too: "psychology has produced a mass of literature criticizing null-hypothesis statistical testing over the past five decades. . . but there has been little improvement. . . . Even editorial policy and (admittedly half-hearted) interventions by the American Psychological Association have failed to inspire any substantial change." Capraro and Capraro (2004), cited

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in Altman, found that in psychology the number of pages in texts and guidebooks recommending the mechanical use of statistical significance was orders of magnitude larger than the number of pages warning that after all effect *size* is always the chief scientific issue. Our papers show the same to be true for a supermajority of econometrics texts, from the advanced *Handbook of Econometrics* through Arthur Goldberger's latest down to the simplest of introductory textbooks. Students get misled from the beginning. Few see a problem. And even fewer break away.

But the present forum may be the beginning of the end for a silly and unscientific custom in economics. We associate ourselves with the remark by the psychologist W. W. Rozeboom in 1997, quoted by Bruce Thompson here (Rozeboom has been making the point since 1960): "Null-hypothesis significance testing is surely the most bone-headedly misguided procedure ever institutionalized in the rote training of science students. . . . It is a sociology-of-science wonderment that this statistical practice has remained so unresponsive to criticism" (Rozeboom, in B. Thompson, p. 335). Precisely.

How To Deal With Random Error

To unblock the journal referees and editors and break out of what Altman calls "a steady-state low-level equilibrium" we propose asking major economists and econometricians to state publicly their support for the following propositions: (1.) Economists should prefer confidence intervals to other methods of reporting sampling variance. (2.) Sampling variance is sometimes interesting, but is not the same thing as scientific importance. (3.) Economic significance is the chief scientific issue in economics; an arbitrary level of sampling significance is no substitute for it. (4.) Fit is not a good all-purpose measure of scientific validity, and should be deemphasized in favor of inquiry into other measures of importance. Every editor of every major journal will be asked. We think that on reflection most economists and econometricians will agree with these propositions.

Scores of the best statistical investigators in psychology, sociology, and statistics itself have been making such points for a long time---longer even than McCloskey has, who came by her insights honestly, stealing them fair and square twenty years ago from pioneers like Denton Morrison and Ramon Henkel in sociology and Paul Meehl and David Bakan in psychology and Kenneth Arrow in economics (Arrow 1959; McCloskey and Ziliak 1996). Ziliak first learned about the difference between economic and statistical significance in the late 1980s, when he purchased for his job at the State of Indiana Department of Employment and Training Services an elementary book by the two Wonnacott brothers--one an economist, the other a statistician (Wonnacott and Wonnacott 1982, p. 160). But he met puzzling resistance when he argued to the chief in his division of labor market statistics how it was a shame that rates of unemployment among black urban teenagers in Indiana were not being published---and were therefore not being discussed openly and scientifically---merely because their small sample sizes did not attain conventional levels of statistical significance.

Good fit modulo the present "sample" is nice, even "neat." But there is no reason to make fit *the* criterion of model selection. As Arnold Zellner points out in his comments, sometimes of course the fit measured by R^2 is perfect because the investigator has regressed U.S. national income precisely on itself. Y fits Y, L fits L, K fits K. "Fit" in a wider scientific sense, which cannot be brought solely and conveniently under the lamppost of sampling theory, is more to the point. How well for example does the model (or parameter estimate) fit phenomena elsewhere? Are there entirely *different* sorts of evidence---experimental, historical, anecdotal, narrative, formal---that tend to confirm it? Does it accord with careful introspections about ourselves? What could be lost if policymakers or citizens act as if the hypothesis were true? So we remain skeptical that some simple and equally mechanical *refinement* of statistical significance will work. Some of the advanced proposals miss the main point, that fit is not *the same thing* as importance.

Precision is Nice But Oomph is the Bomb

The kind of decision-making we advocate can be illustrated thus. Suppose you want to help your mother lose weight, and are considering two diet pills of identical price and side effects. The one, named "Oomph," will on average take off 10 pounds—but it is rather uncertain in its effects, at plus or minus 5 pounds. Not bad. Alternatively, the pill "Precision" will take off only 3 pounds on average, but it's less of a roll of the dice: Precision brings a probable error of plus or minus 1 pound. How nice.

The signal-to-noise ratio of diet pill Oomph is 2:1, that for Precision 3:1. Which pill for Mother? "Well," say some of our scientific colleagues, "the one with the highest signal-to-noise ratio is Precision. So, of course: hurrah for Precision." Wrong, of course; wrong, that is, for Mother's weight-management program and wrong for the distressingly numerous victims of scientists in the misled fields from medicine to management. Such scientists decide whether something is important or not, whether it *has an effect*, as they say, by looking not at its oomph *but at how precisely it is estimated.* But the pill Oomph promises to shed 5 or 15 pounds. The much less effective Precision will shed less than 4 pounds. Common sense recommends Oomph. The burden of this symposium is: let's return to common sense in science.

The main thing to grasp in the comments gathered here is this: *every one of the commentators agrees with our two main points:*

(1.) that economic significance usually has nothing to do with *statistical* significance

and

(2.) that a supermajority of economists do not explore *economic* significance in their research.

The agreement from Arrow to Zellner on the main points should change research practice. Moreover, the tiny objections the critics raise against us, though significant as sociology of science, in no way undermine the consensus. Economic significance, substantive significance, is the body---not statistical significance unadorned. We all here agree.

Some Reasons Statistical Significance Does Not Select Models

Graham Elliott and Clive Granger agree with our point, but want for some reason to characterize it as "literary" and not "deep." Perhaps it arises from their mistaken belief that if sample means and so forth are somewhere provided in a paper, then "the economic significance can be determined." Set aside that, as they admit, in many cases the papers do *not* provide the data to get beyond a statement that a certain coefficient is or is not "significant." Our main point is not this stylistic one. It is that "significance" itself is something that needs to be argued out in the context of the scientific or policy issue *and cannot be determined on statistical grounds alone*. Our point is not to repeat a matter of style, literary matters, or superficialities of presentation. The economic significance can*not* "be determined" by simply better reporting on conventional statistical tests. The mistake of Elliott and Granger shows in their claim that what would be at issue in cases of bad reporting is the "statistical comprehension skills" of the reader. No. It is the *economic* comprehension skills that matter for economic science: that is our main point. We cannot hand science over to a table of Student's t.

We have learned recently, by the way, that "Student" himself--William Sealy Gosset--did not rely on Student's t in his own work. To the world's gain Gosset's job and passion was to instead learn scientifically how to brew the best Guinness he could brew at the best price the market could bear (see for example E.S. Pearson, *'Student': A Statistical Biography of William Sealy Gosset* [Oxford: Clarendon Press, 1990, pp. 20, 30-31). Student, one might say, put the oomph in Guinness. R.A. Fisher begged Student for his tables of t to publish in Fisher's now hugely influential *Statistical Methods for Research Workers*. Yet Fisher did not as we have shown believe he needed to emulate Gosset's care for *magnitudes* of ingredient effect, and focused on t.

Often we focus on how to interpret the parameters of a specific model. Elliott and Granger agree with us but then focus their critical energies on a defense of mechanically computed statistical significance to separate theory A and theory B. (We believe they mean "model" A and model B, though their comments equivocate.)

We are not persuaded.

Their instance in physics, of the bending of light around the sun as against the Newtonian prediction of less bending, is ill chosen. The physicists making the experiment *did not in fact use statistical tests*. The leader of the historic 1919 expedition to photograph the eclipsed sun off the coasts of West Africa and Northern Brazil (to see the bending light to which Granger and Elliott refer) was Sir Arthur Eddington, the Cambridge astronomer and popularizer of relativity. Eddington, it turns out, had been a teacher of the statistician Harold Jeffreys, and Jeffreys was intensely interested in the results of the expedition. Arnold Zellner has tried with little avail for decades to get economists to read Jeffreys, precisely because Jeffreys believed---against his teacher Sir Arthur---that statements of "existence" are for purposes of hypothesis and model testing useless (Wrinch and Jeffreys 1921; Howie 2002, pp. 92-3; Ziliak and McCloskey, this volume). Size is what matters. The photographic evidence was not at first persuasive; indeed, it is well known in the history of science that it was some years before an error caused by the instrumentation was corrected: Einstein's theory was at first *rejected* by the evidence. And so Eddington reasoned in favor of Einstein on geometric, a priori grounds. Jeffreys and his collaborator Dorothy Wrinch responded with an empirically based criticism of Eddington's defense, and published their piece in a now famous issue of *Nature* in which Einstein considered all the evidence (Wrinch and Jeffreys 1921). Magnitude, size, design of sample, coherence with other stories and other kinds of evidence are what persuaded. No tests of statistical significance, Jeffreys and Einstein agreed, could alone shed light.

Indeed, as we report, in the leading journals of physics such as the *Physical Review* one hardly ever encounters the t, p, R^2, and the like that litter journals of economics, psychology, and medicine. Physicists certainly do test one physical model A against a rival B. But they never hand over the criterion of decision to an unargued level of significance. Ask any physicist. One of us last month for example asked a distinguished physicist who was helping out with the selection of Phi Beta Kappa Awards. Roughly he said in reply, "Of course not. We use statistical models, such as Brownian motion. But never do we `test' at arbitrary levels of significance the way biologists sometimes do."

No wonder. Suppose you were comparing two pieces of silverware, one a spoon, A, and the other a fork, B. Suppose you wanted to know how similar A was to B. The procedures we and the numerous other critics in other fields are complaining about are mechanical "tests" *on the half-inch of pattern on the "handles" of each piece*. The comparison of models is reduced to the comparison of fit in the so-called "sample" on offer. These may turn out to be very similar---imagine the spoon and the fork coming from the same silverware pattern, and so having much the same figuration of the end of the handles. But a fork in its forked end is different from a spoon in its spooned end for use, for science, for policy. You can't stab meat with a spoon. And no amount of mistaken reports on the philosophy of science will induce a thin soup to pool upon your fork. Precision does not pick the model. Oomph, and the scientist's stories about oomph, does.

Elliott and Granger take the view that conventional statistical methods simply *are* the techniques of "empirical methods in all of science." This is factually mistaken, though rather typical of the way statistical methods is taught these days in economics, all handles, and egg on the face. When we---and Elliott and Granger---criticize for example the mechanical use of 5% significance levels we are criticizing a practice that is widespread only in a tiny part of science. Though it *is* a part that Clive Granger could singlehandedly reform if he would!

We are surprised that our old friend Joel Horowitz, who we know agrees with much of what we say, asserts "there are circumstances in which the existence of a phenomenon, not its magnitude, is decisive." Horowitz, unlike us, was trained as a physicist. But here he is talking like a mathematician. We prefer the talk of physicists, such as Richard Feynman, whose great "elementary" textbook at Cal Tech is filled with statements like "are zero, *or can be neglected in comparison with the variations in the other directions"* (II, p. 7-2) or "the fact that there is an amplitude . . . *has little effect* when the two positions have *very different* energies" (III, p. 9-8). Or in his lectures on computation that "Predictive coding enables us to compress messages to *a quite remarkable degree"* (1984-86 [1996], p. 129). Horowitz will be able to tell us what on earth Feynman was talking about so far as the physics is concerned. But what is obvious in Feynman's talk even to an outsider is that it is about *magnitudes*, never about existence in the mathematician's sense. Remember from your math course: a mathematician trying to prove that a number is greater than zero doesn't care a fig whether the number is 10^{100} or 10^{-100} . The physicist does, every time. All right: *nearly* every time. Thus the famous case of Feynman's test with a glass of water during the Challenger inquiry: was a temperature around freezing *low enough* to change the behavior of the stuff used for the O rings *low enough to matter*?

We see the point of Horowitz's example of Cronin and Fitch. But presumably if the effect had turned out to be two orders of magnitude greater than it was in fact, then the surrounding physics would have been greatly altered. So magnitude mattered even in that case of a very faint effect. And as he himself says, economics is not precise enough for tiny effects to be relevant anyway, a point made fifty years ago by Oskar Morgenstern. The problem with Horowitz's "existence" talk---which, we repeat, we do not think even he believes is very important, since on the whole he agrees with us and teaches our point to his students---is that it suggests there must be a "test" for it, *free of any worries about how big is big*. But there isn't. When Horowitz says that "the difference between [0.2 and 0.4] . . . is interesting and important only if we can be reasonably sure that it is not an artifact of random sampling error" he is applying an arbitrary criterion of significance---which after all is the main thing both he and we don't like. The point is that *even if* (say) *95 percent confidence interval contains both 0.2 and 0.4* that doesn't mean there "exists no difference," or that we are justified in thinking there is "no difference" in the predictions of the two theories (say). *It depends on the loss function*. To put it another way, it depends on the significance level one chooses, and *that* (as Neyman and Pearson stressed) is a scientific/social decision, not to be left to convention or ritual disguised as a mere formality. If one just *had* to make some crucial decision, and had got a coefficient of 0.4, though alas from very noisy data, one might have to go ahead and suppose that 0.4 was The Truth.

We have no wish to take random error out of economic or physical or any other account of the world. Noise exists, and sometimes one wants to know how much there is, and distinguish it from some effect of actual interest. Fine. But we are sure Horowitz would agree that *this does not justify using statistical significance to decide on what variables are "important,"* which as we have shown is the usual economic practice. In fact the coefficient of 0.4 in question, from Gary Solon's 1992 study of intergenerational income mobility, passed conventional tests and seems moreover to be the product of a Pareto improved model for extracting income parameters.

We agree with the more radical point of another old friend of ours, Ed Leamer, that economics needs tests of persuasiveness or usefulness, both of which could be called in official philosophical language "Pragmatism." Leamer is correct that tests of significance persist precisely because they do not in fact settle much that persuades scientists intent on *usefulness*. Consider the enormous number of tests of significance done each year *on both sides of every issue* in economics. Would it surprise anyone to assert that they were, let us say, on the order of 10 million? If the tests were in fact as conclusive as their own rhetoric requires, most issues in economics would long have been settled. That's one way of putting Leamer's point. We see some similarity here between Leamer's point and the very interesting argument with which Horowitz ends his paper. In any event we are confident that both Leamer and Horowitz would agree with us that when one wants compare a spoon and a fork perhaps it would be wise to develop other ways of comparing them beyond doing statistical tests on the design on the handles over and over and over again, 10 million times, to no one's enlightenment. We agree with Peter Lunt that R. A. Fisher's intent in the 1920s and for a long time after was "to develop what he hoped would be conventionally agreed, automatic procedures for statistical inference," because judgment is "fallible." We agree by the way with the spirit of Gigerenzer's paper but we do not agree with his reading of Fisher: Fisher, as we show, "invents" the "formal" criterion of statistical significance, for sure by 1925, and doubtless somewhat earlier in personal communications. And Fisher's less well known second thoughts on the matter--that is, his coming to believe correctly that a "rule of 2" and the like is foolish-- were we think wariness caused by a stronger mathematician, Jerzy Neyman, who showed Fisher the clumsiness of Fisher's qualitative, yes/no reasoning and the incompleteness of his approach to error, which was Type I only. Fisher therefore took the Jeffreys inspired "science route"--saying that a mechanical rule is silly and that what scientists really care about is magnitudes of effects and relations, not mathematical "existence." But Lunt understands us to be Fisherians, wishing only to improve the practice that came from Fisher's final victory in practice over Neyman and Pearson. That is mistaken: we are Neymanites, and Jeffreysites, and most assuredly are not modernist positivists (see for example McCloskey 1983, 1985 [1998], 1990, 1994; Ziliak, ed., 2001; Ziliak 2003, 2005). We would be very willing to engage in an epistemological critique of economics, and in fact we have opened that particular Pandora's Box on many occasions, and at length. But on this occasion we are engaging, as Lunt says, in an "internal critique." It seems appropriate. If economists can't get even their mechanical methods right, perhaps they need to consider a broader range of ways of arguing---for example (to again stay within conventional economic method) putting more emphasis on the simulation that has been made so easy by the fall in computation costs. On the other hand, we agree with

Lunt that the analogy of regression analysis with experimental method on which classical econometrics is built may be reaching a crisis.

We agree with Tony O'Brien that the next step is to see how badly economists are doing in their subfields, such as economic history, O'Brien's target, in making this childish mistake in statistical procedure. O'Brien's project is harder to do, of course, because one has to get down deeper into the discourse, to see how the evidence and argument are constructed overall. That is, one needs to see how the rhetoric works. O'Brien believes that childish mistakes do not always have bad consequences. We agree with him that the character of research in economic history keeps the results from depending too much on the mistakes. Exactly as he says, if one really knows a subject---and we are proud to say that economic historians generally know their subjects much better than economists satisfied with manipulating the same old one-in-a-thousand Michigan samples over and over again, or the same old quarterly time series over and over again---one wants to know about oomph. We believe indeed, as O'Brien appears to find inconceivable, that economic historians in fact do "their economics better than the authors in the *AER*." The economic historians published in the *AER* in fact score much higher on our questionnaire than, say, the average macro or finance economist.

Defineability of Economic Significance

Erik Thorbecke does the best job of summarizing our paper in his own words, which makes us think that he grasps it the best. Richard Feynman used to say that if you cannot express your physical argument clearly enough to give a lecture on it to undergraduates you don't really understand it. Thorbecke's diagram in particular is brilliantly illuminating. Like O'Brien, however, Thorbecke is not sure how (or how we want) to proceed. His desire for concreteness, for more examples of Standards of Economic Significance, is understandable. We recommend starting with magnitudes already discovered and offered as "standards." Gary Solon has pushed the quantitative side of the left's argument about the salience of social immobility to a new and challenging level. Any scientist intending to answer the question of social or income class mobility in the United States must at some point confront Solon's estimates.

Look, for another example, at welfare reform. The 1996 Personal Responsibility and Work Opportunity Reconciliation Act was passed in part on the belief that the welfare state had enabled poor people to stay on welfare for life and therefore a great many did. But the historical record shows in fact that the average length of time a family stays on relief, private or public, religious or secular, has not much changed since the 1820s—families stay on relief on average not for a lifetime but for 8 to 13 months (S. Ziliak 2002, 2004). Still, the journals continue to fill with papers reporting uselessly "significant" results on the duration of welfare dependence. Against much lower estimates put forth by the Council of Economic Advisors (CEA), James P. Ziliak, et al. have argued that up to 75% of the 1990s decline in welfare participation was caused by improvements in economic growth (J. Ziliak, et al. 2000). The White House claimed that the new, draconian welfare laws were the main cause, citing the lower figures of the CEA. Complete agreement on the quantitative contribution of economic growth to the decline in welfare rolls may not ever emerge. But the economists in the debate understand that the size of economic and social magnitudes and their overall effects on decision making are what is at stake. *Not* statistical significance. Every macroeconomist is familiar with the standards of economic significance exemplified by "Taylor's rule" and "Okun's law." The point of economic science, as Thorbecke clearly sees, is to discover the magnitudes of relations between economic variables and then argue them out.

We were pleased to find that Arnold Zellner agrees with what we say. His own critique of practice cuts deeper than ours. We honor his and Leamer's Bayesian approach, and note his friendly and non-ideological invitation to classicists to find the unity of the two approaches. If economists did as he has been recommending for decades, testing and estimation would change immensely. Economic research would be about the measurement and meaning of the size of economic effects and economists of all persuasions, experimental and observational alike would, like Zellner and his mentor the Jeffreys, become much more "humble."   No longer would it be possible for an editor of the *AER*, or any other journal, to force an author to use tests of statistical significance instead of likelihood ratios, as one such editor did to Jack Hirshleifer and co-authors Vernon Smith and Yvonne Durham (Hirshleifer 2004)---unsurprisingly, Hirshleifer, Smith, and Durham found that the tests they were forced to make and report rejected at the 1% level *any* null hypothesis of economic interest.

William James in 1907 noted the "classical stages of a theory's career. First, you know, a new theory is attacked as absurd; then it is admitted to be true, but obvious and insignificant; finally it is seen to be so important that its adversaries claim that they themselves discovered it." We certainly hope so.

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