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MORE ABOUT THE RHETORK OF ECONOMICS

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MORE ABOUT THE RHETORIC OF ECONOMICS

Economists have deferred to a dead philosophy of science for too long. They have not, for which heaven be praised, actually carried the philosophers' orders out; economics would have stopped if they had. But an outdated methodological vision has haunted economists nonetheless. They have paid tribute to (without following) the sterile injunction that successful prediction is everything, and the tribute has been the mean and negative one of disdaining (while still using) all the other good honest methods of persuasion. As a result, economists have become less careful and less effective in the business of argument. The remedy is to stop cowering before the lordly demarcations of yesterday's philosophers as to what is and what is not scientific practice, and to return to the more ancient wisdom of the rhetorician. It is time for economics to listen to its own conversation with a critic's ear.

This was the message of Don McCloskey's long and brilliant article "The Rhetoric of Economics" (McCloskey 1983). McCloskey was soon to profess surprise at the warmth of his article's reception (McCloskey 1984), yet it is hard to believe that he did not know how badly thousands of economists wanted to hear what he was going to tell them.

> Economics _is_ science, a successful sort at that. Economics explains as much about business people and resources as evolution explains about animals and plants, for identical reasons. No one who knows the subject will deny it.[1]

In this paper we look first at the methodology which McCloskey is attacking (and its relationship to the methodology he thinks he is attacking). We then look at the benefits which a more rhetorical approach might bring to the subject. Lastly we take up the surprising fact that McCloskey omits to discuss what makes for good or bad rhetoric.

1. McCloskey (1985), p.56

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I

McCloskey, it must be emphasised, is not condemning all philosophers of science. Indeed he claims he is not condemning anything up-to-date in the philosophy of science. "Modernism", as he calls his target, is an outdated church out of which the last philosopher tiptoed some decades ago. But those disciplines whose practice the modernists tried to order around typically have not heard that the orders are cancelled. In economics, especially, those in authority are distilling their all-too-unfrenzied rules of procedure from defunct philosophical scribblers from a good deal more than a few years back.

So what are the tenets of modernism? McCloskey lists them as follows:

1. Prediction and control is the point of science.
2. Only the observable implications (or predictions) of a theory matter to its truth.
3. Observability entails objective, reproducible experiments; mere questionnaires interrogating human subjetcs are useless, because humans might lie.
4. If and only if an experimental implication of the theory proves false is the theory proved false.
5. Objectivity is to be treasured; subjective "observation" (introspection) is not scientific knowledge, because the objective and subjective cannot be linked.
6. Kelvin's Dictum: "When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind."
7. Introspection, metaphysical belief, aesthetics and the like may well figure in the discovery of an hypothesis but cannot figure in its justification; justifications are timeless, and the surrounding community of science irrelevant to their truth.
8. It is the business of methodology to demarcate scientific reasoning form nonscientific, positive from normative.
9. A scientific explanation of an event brings the event under a covering law.
10. Scientists -- for instance, economic scientists -- ought not to have anything to say as scientists about the oughts of value, whether of morality or art.

We will all, no doubt, react differently to these commandments, both on our own behalf and as to how far they represent the views of the "typical other". Has any economist ever believed in all ten? The only flesh-and-blood modernists McCloskey can find are Machlup and the Friedman of the (in)famous 1953 essay. And even this latter piece, McCloskey concedes, finds room for aesthetic

McCloskey (1985) pp.7-8

criteria when choice of a theory is at stake, endorses questionnaires for suggesting (albeit not testing) hypotheses, and invokes the rhetorical community to which the scientist must speak if he is to produce conviction. Perhaps, McCloskey concludes, the fact that the _locus classicus_ of economic modernism contains so much that is anti-modernist "indicates that modernism cannot survive intelligent discussion even by its best advocates." Alternatively, perhaps the fact that the only example of modernism that McCloskey can disinter contains so much that is anti-modernist indicates that modernism never existed in economics!

The full-blooded modernist, in short, inhabits McCloskey's imagination. To call his version a caricature would be to insult what can be a serious, truthful and disciplined art-form. Nor can his version of things be given credibility by arguing that economists practise the modernism which even Friedman only half-heartedly preaches. It is the whole point, or at least half the point, of McCloskey's thesis that economists do not and cannot live up (or down) to modernist prescriptions.

There are, in fact, no more than five cursory references, even in the full-length McCloskey (1985) to the philosopher who has in reality dominated economists' talk of method these past twenty years -- Imre Lakatos. And before showing how un"modernist" _he_ was, it is worth stressing that economic methodologists since 1970 have, to a man and in almost every respect, been _less_ modernist than Lakatos. Take Mark Blaug, whose "fine but wrong" book (Blaug 1980) McCloskey excoriates for its "authoritarian" equation of science with testability. After praising Lakatos for rejecting an ahistorical philosophy of science (modernist commandment no.7), Blaug goes on to judge the resultant attempt "to divorce apprraisal from recommendation, to retain a critical methodology of science that is frankly normative but... capable of serving as the basis of a research program in the history of science" as "either a severely qualified success or else a failure, albeit a magnificent failure." Philosophers of science, in short, face severe problems whether they put in the actual practice of the scientific community or whether they leave it out -- not exactly the conclusion of a paid-up subscriber to McCloskey's "ten commandments".

So, if no recent economist has been more "modernist" than Lakatos, where does Lakatos himself stand? Lakatos, like Popper, starts with the scientist whose observations have clashed with his predictions and so faces three possibilities: his theory is wrong, his observation is faulty, or there is an unrecognised disturbing factor at work, i.e. ceteris are not paribus. A planet whose observed orbit differs from that predicted, for instance, can mean (i) that the existing theory of gravitation and dynamics is incorrect (ii) that a faulty theory of optics has bred an inaccurate telescope or (iii) that there is some disturbing factor which the scientist has not considered (such as an undiscovered planet nearby). In other words the observation has refuted the conjuncture of three hypotheses: the gravitational-dynamic theory, the optical theorfy behind the telescope and the hypothesis that ceteris are paribus on this occasion. They cannot all be true. Which of them is false? The scientist's dilemma might be called the theory-ceteris-telescope problem.

Before going any further, it must be pointed out that the "telescope" part of the problem cannot be avoided by using the naked eye. Whatever the method of observation, we are left with a clash -- or a congruence -- between theories. Theories in science are not contradicted by facts, because the distinction between theory and fact is an illegitimate one. Even direct sensation is impregnated by expectation: there is thus no psychological demarcation between theoretical and observational propositions.

"Methodological falsificationism" was Popper's first attempt to deal with the theory-ceteris-telescope problem. Popper proposed that some well-corroborated theories should, tentatively, provisionally and for the sole purpose of testing other theories, be regarded as unproblematic background knowledge. Thus a methodological falsificationist might decide to use a well-corroborated theory of radio-optics uncritically, as "background knowledge", and thus regard resulting observations as unproblematic "facts". He would thus be isolating the gravitational theory for testing,. and anomalous behaviour by the planets would "falsify" the gravitational theory. "Falsify" must be kept in inverted commas, because the theory has not been disproved. Since the "unproblematic" theory may still be false, the "problematic" theory, or rather the conjunction of the "problematic" theory and the ceteris paribus clause, may still be true.

If, however, this conjuncture is "falsified", the next step is to decide whether the ceteris paribus clause too should be relegated to "unproblematic background knowledge". To decide this, the scientist must specify disturbing factors which would violate the ceteris paribus clause, and test the assumptions that they exist. If many of them are refuted, the ceteris paribus clause will be regarded as well-corroborated and join the category of "unproblematic background knowledge". The problematic theory will now be regarded as "falsified".

Methodological falsificationism requires the scientist to make daring assumptions both before and during his investigations. First he must decide which theories should be taken as giving unproblematic background knowledge : an operation which Popper himself likened to driving piles into a swamp. Then, secondly , if the conjuncture of a theory and a ceteris paribus clause is "falsified", he must decide which to drop. The procedure just mentioned for doing so leaves the scientist lawless and ill-advised at every turn. Only if he finds a precisely quantifiable disturbing factor which revises prediction exactly into line with observation is his path ahead clear. Otherwise his decision between dropping the ceteris paribus clause and dropping the main hypothesis wil resemble the earlier dictatorial decree that this theory is, and that one is not, unproblematic background knowledge.

Sophisticated methodological falsificationism (hereafter SMF) was devised by Popper to reduce the arbitrary element in theory testing. Under SMF a theory is falsified only if there is a second theory which not only explains everything explained by the first one, but predicts novel "facts" (i.e. "facts" improbable in the light of, or even forbidden by, the first theory), some of which have been corroborated. Pareto-optimality has reached the philosophy of science! If T2 explains all that T1 explained and predicts novel facts, it is said to have excess empirical content over T1. If some of the novel facts have been corroborated, then T2 has corroborated excess empirical content and has falsified T1.

SMF, Lakatos says, reduces the arbitrary element in science. Of the two methodological decisions above, the second no longer has to be made ; because now, if faced with an inconsistency, "we do not have to decide which ingredients of the theory we regard as problematic : we regard all ingredients as problematic in the light of the conflicting accepted basic statement and try to replace all of them. If we succeed in replacing some ingredient in a 'progressive' way (that is, the replacement has more corroborated empirical content than the original), we call it 'falsified'."[1] In other words, rational criteria have been supplied for the choice between dropping the ceteris paribus clause and dropping the main hypothesis. Note, however, that there is still an "accepted basic statement" present ; that is the first methodological decision has still to be made. But

1. Lakatos (1970) p. 125

even here, Lakatos says, the conventional element can be reduced ; such methodological decisions can now be appealed against.

How? Let us start with Lakatos' description of the original methodological decision: "Whether a proposition is a 'fact' or a 'theory' in the context of a test depends on our methodological decision...the clash is not between 'theories and facts' but between two high-level theories: between an interpretative theory to provide the facts and an explanatory theory to explain them...the problem is which theory to consider the interpretative one which provides the 'hard' facts and which the explanatory one which tentatively explains them."[1] Lakatos here instances the clash between Prout's theory (that the atomic weights of all elements are exact multiples of the atomic weight of hydrogen) and Stas's "refutation" (that the atomic weight of chlorine is 35.5.) The latter "fact" is fact only by assuming that Stas's chlorine really was pure chlorine i.e. only by taking as correct the theory that, if the purifying procedures used by Stas are applied, the result will be pure chlorine. This theory is the interpretative theory which provides the "facts" ; Prout's theory is the explanatory theory which explains them (or in this case fails to explain them). So long as we chartacterise the two theories this way round, Prout's theory has been refuted by "fact."

But whichever way round the designation is made, we are in reality testing two or more theories for consistency with each other. Should there be inconsistency, the sophisticated falsificationist will "try to replace first one, then the other, then possibly both, and opt for the new set-up which provides the biggest increase in corroborated content." If this set-up includes the original explanatory theory but not the original interpretative theory, the supporters of the explanatory thepory have appealed successfully. This is what actually happened with Prout's theory.

But even this appeal procedure, Lakatos says, can only postpone the conventional decision to take some "fact" as fact. For the appeal is decided with regard to "increase in corroborated content." But an increase in corroborated content involves the prediction of novel facts. And to accept any statement as a "fact" requires a conventional decision about a theory. The appeal court is thus making its own conventional decisions.

Let us now appraise SMF, and Lakatos's interpretation of it, with particular reference to economics. It is often said

1 Lakatos (1970) p. 129

. that, whereas the natural sciences have theories which come with explicit instructions about the conditions in which they will not apply, the social sciences have models whose domain is left unspecified. Such models cannot be falsified but only pronounced inapplicable to a particular case. The standard criticism of economics as a predictive science is that, if you can only pronounce the model inapplicable <u>ex post</u> and in the light of the observations which contradict it, what is the point of the model?

The ceteris paribus isssue, then, is central to economics ; and the relevance of Lakatos' methodology to economics stands or falls with his instructions on what to do with ceteris paribus when theory and observation collide. I shall argue that SMF applies only when the theory or model is making exact quantitative predictions -- and not always then.

Consider a prediction which is, like most economic predictions, purely <u>qualitative</u> -- that a rise in interest rates will reduce investment by some unspecified amount. Interest rates then rise but investment rises too. We can now argue that some disturbing factor is at work, hunt it down, try to conclude something about the strength of its effects and thus explain the "anomaly" away. This of course is what economists do. But it is not scientific progress in the Lakatosian sense. <u>If predictions are qualitative, the discovery of a disturbing factor can never generate a new prediction</u> . The point is a very simple one. Either disturbing factor Y agrees with model X that variable A will go down next week (in which case it cannot explain the "anomaly" that A has gone up) or it disagrees, in which case it simply blurs the prediction. X and Y are pulling A in opposite directions, neither can be quantified, and A could go either way. Excess empirical content, corroborated or not, cannot arise. Where predictions are purely qualitative, allowing for disturbances can only reduce empirical content.

Even where economic models do make exactly quantified predictions, the situation is likely to be no better. We have heard a paper this week which argues, in line with the vast bulk of evidence, that the stock market is an efficient market. This hypothesis leads to various precise predicitons that can be made about share prices. But if it did turn out to be the case that the stock market was inefficient, this could not lead to an alternative quantitative prediction unless the degree of inefficiency could be measured (and measured, of course, by some means other than calculating back from the observed effects.)

This all seems an insuperable stumbling block to a Lakatosian economics. It is worth stressing how central to Lakatos' prescribed method the criterion of corroborated excess empirical content is. The purpose of the criterion is to demarcate "progressive" from "degenerating" research programmes. A research programme, to Lakatos, consists of a hard core hypothesis, mediated and interpreted through a "protective belt of auxiliary hypotheses." (This is simply another recognition of the fact that no theory can stand alone, but must have its conditions of application specified). But, in Lakatos's words, "theories and factual propositions can always be harmonised with the help of auxiliary hypotheses...the problem is how to demnarcate between scientific and pseudoscientific adjustments, between rational and irrational changes of theory." Corroborated excess empirical content is the touchstone. But, when predictions are purely qualitative, auxiliary hypotheses never produce c.e.e.c. Hence the Lakatosian method cannot distinguish between scientific progress and ad hoc quackery.

It is strange, therefore, that McCloskey should go to the trouble of building up a target composed of nine parts 1936 Ayer to one part 1953 Friedman. Economic practice (as he admits, indeed affirms) was never like this : while the methodological proclamations of contemporary economists slope away down the non-positivist track from Lakatos, who himself represents positivism watered down about as far as it is possible to go. Yet, as I have tried to show, there is a fatal defect in Lakatos's methodology too as a guide to good economic practice : it fails to perform the philosopher of science's most basic task, that of distinguishing between a research programme making fruitful progress amd a research programme derfending its failure by spinning convenient epicycles.

McCloskey, then, did not need to resort to his laboured fiction of the "economic modernist" in order to divest economics of its predictive pretensions. He could and (by his own rhetorical standards) should have made his case much more persuasive by attacking the truly prevailing methodology of today.

II

But what would McCloskey put in place of "modernism"? Nothing, is his original answer (McCloskey 1983). If economists would only free themselves from modernism's thrall, they would automatically place more weight upon -- and give more thought to -- the ways in which they actually converse with and seek to persuade one another. Here the removal of modernist commandment no.10 (stick to the is and don't consider the ought) is particularly essential. Under its baneful influence, McCloskey accuses, economists have to pretend to absurdities such as that they are more strongly persuaded that inflation is everywhere a monetary phenomenon than they are persuaded that it is wrong to commit murder. The ban on introspection as a justifying device means that microeconomists must solemnly plough through the higher reaches of mathematical economics and econometrics to try (and even then fail!) to justify the proposition that demand curves normally slope downwards. Remove modernism, and serious argument can start again on value-impregnated issues. Permit introspection and economists can start addressing their own experience to economic problems (which is what every teacher does in his first lecture on elementary demansd theory anyway.)

It is important to realise that McCloskey is not just another "radical critic" of "bourgeois" economics. Indeed he openly states his allegiance to the neoclassical school, though never doing much to explain it. Neoclassical economics, in McCloskey's view, is neither the only or even necessarily the worst offender when it comes to sterile unthinking "modernism". The Marxist view that history is class struggle is, to him, as narrowing as the neoclassical view that history is about interactions between selfish individuals. The downgrading by Marxists of hypotheses which are supposed to reflect "false consciousness" is as impoverishing to free enquiry as is the downgrading by neolassicals of hypotheses which are not falsifiable in the Popperian sense. One wonders whether this consciously even-handed criticism of schools of thought (Austrians get the same treatment too) is itself a rhetorical device, born of the guess that neoclassical economists will take these criticisms on board the more easily if they see other schools of thought being beaten around the head too. Objectively, one might expect Marxists to come less badly out of McCloskey's strictures than mainstream economists. Most Marxists, after all, have reacted to Popper's charge (that their doctrines are unfalsifiable and hence unscientific) by challenging the Popperian scientific/unscientific demarcation as a coercive piece of pseudo-philosophising with no justification outside (or even within!) the terms of reference it itself imposes. This is the McCloskeyite line precisely. McCloskey is also dismissive of the view (Popper, 1945 ; Hutchison, 1938) that an insistence that theories must be in principle falsifiable is a bulwark against Marxist and fascist pretensions to infallibility under the inexorable laws of history.

McCloskey (1988) continues the theme that all economic schools of thought have an equal amount to gain by taking their rhetoric more seriously. The gains, however, are spelt out in more detail than before.

1) Rhetoric in economics consists of "showing to each other whatever numbers and symmetries and metaphors we agree should matter...Ignorance of rhetoric leaves economists unable to confront doubts.Run another regression that no one believes. Deduce another consequence that no one is persuaded by. Adduce another institutional fact that no one else sees as relevant."[1] Rhetoric, in other words, means economists talk directly to one another about what they find convincing.

2) The rhetorical approach will actually raise standards of scientific practice, which under "modernism" are very <u>low</u> (whatever the pretensions). "Is it more difficult for a Chicago economist to produce still another regression 'consistent with the hypothesis' of peasant rationality or... to produce a set of arguments...that can actually persuade an economist from Yale?"[2]

This is all fair enough; one can reject McCloskey's fiction of "modernism" as the ruling methodology and still share his frustration at the way most economists write up their papers: preamble, model, t-statistic, variant on the model, new t-statistic, summary. If economics is a place where arguments are seldom settled but more commonly peter out through exhaustion and boredom, the above method of "argument" has much to answer for. To make such a charge, however, does seem to call for a general analysis of what forms of economic rhetoric do, and what forms of rhetoric should, persuade different kinds of audience. McCloskey, one feels, is so anxious to avoid tarring himself with the brush of methodological authoritarianism which he wields on almost everyone else, that he puts all rhetorical devices on an equal footing.

1 McCloskey (1988) p.287
2 ibid p.289

III

So what are the principal devices of economic rhetoric? McCloskey claims that he found almost all of them on a couple of pages when he opened Samuelson's Foundations of Economic Analysis at random. pp.122-3 of that work feature:

1) Mathematical virtuosity, so that Samuelson can "present himself as an authority. That the mathematics is sometimes pointless, as here, is beside the point. Being able to do such a difficult thing (so it would have seemed to the typical economist reader in 1947), is warrant of expertise."[1]

2) Appeals to authority -- in these two pages Keynes, Hicks, Aristotle, Knight and Samuelson himself are invoked.

3) Appeals to relaxation of assumptions -- Samuelson considers in a purely speculative way what would happen to the demand-for-money function when Hicks's assumption of a zero return on money is relaxed. "Mere speculation of this sort is not (for the modernist) evidence at all"[2] coments McCloskey.

4) Appeals to hypothetical toy economies (e.g. an economy where money does not exist) as simplifying devices.

5) Use of analogy and metaphor.

It is not helpful to put, as McCloskey does, all these devices on the same rhetorical footing. 2) and 5) in particular are forms of inference. We appeal to X's authority on this occasion because we believe him to have been right, and we believe our audience will think he was right, on most other occasions. We draw an analogy between, say, rational expectations and profit-maximisation because we think that if our audience believes in the one form of optimisation we can convict them of inconsistency in not believing in the other. All of this assumes that prior probabilities are being assigned, however implicitly, to the hypothesis we want to defend. (This would conflict with "modernism" as McCloskey defines it, but not with the way economists actually reason.) Rhetorical devices 2) and 5), in short, are justified by the Bayesian probability calculus.

1. McCloskey (1985) p.70
2. ibid p.71

3) and 4) by contrast are simply different stages in the process of model building. A model is a model whether you call it a model or a toy economy ; the relaxation of assumptions is the next stage in Marshall's "method of successive approximation." Presumably these two devices are labelled rhetorical rather than modernist because they aim to convince the reader by means other than empirical testing. But in fact Samuelson's relaxation of the zero-interest-on-money assumption does not seem designed to convince anyone about anything ; it is thrown in as speculation and allowed to hang in the air. Where implications are drawn deductively from simple models of an economy, and then not put to any kind of test, the purpose must clearly be to say to the reader "here are plausible assumptions ; the conclusions follow inexorably ; you had better therefore accept the conclusions." Again, Friedman (1953) is alone in denying the validity of such a method of persuasion.

1) is surely rhetorically illegitimate (and ineffective?) It is the only one of the five which cannot be cast in probabalistic terms. There is no reason why the couching of a message in advanced mathematical language should cause us to raise our prior estimate of the probability that the proposition concerned is true.

So we come back to where we started. All five of Samuelson's forms of argument would be empty "figures of speech" to a thoroughgoing modernist. But such people do not exist in economics. Economists, however implicitly, start with subjective probabilities as to the truth of a hypothesis and then modify them in the light of the arguments they hear. All but the first of Samuelson's "rhetorical devices" listed above are scientifically respectable ways of bringing about such modification. No doubt economists could be more explicit about the probabalistic way they reason : why on earth should they not say "I thought there was a 90% chance expectations were rational until I read Figlewski and Wachtel in the <u>Review of Economics and Statistics</u> ; their evidence is so much against that I've now come down to 70%." In that way the basis of disagreement would be laid bare, and the job of either resolving controversies or agreeing to disagree would be put in hand. McCloskey is right to attack the complacent way different schools of thought talk through one another. He is right to call for more rhetorical self-consciousness. But true self-consciousness would ask when there is and when there is not a logical backbone under the rhetoric.

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