

# Irrealist Lines of Defense in Econometrics (Work in Progress)

Hans G. Ehrbar  
Economics Department, University of Utah  
1645 Campus Center Drive  
Salt Lake City, UT 84112-9300  
U.S.A.  
+1 801 581 7797  
ehrbar@econ.utah.edu

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## Abstract

This essay explores the mechanisms which allow econometrics to remain an ideology, rather than a science, although econometricians look at data every day.

One cannot expect econometrics to automatically become a science just because it deals with data. According to Bhaskar, such an expectation commits the ontic fallacy, i.e., it assumes that the ontological makeup of the world necessarily generates correct scientific methods. Marx himself made this mistake when he expected that economic crises would “drum dialectics into the heads” of mainstream economists.

The present essay argues in one specific case study why the ontic fallacy is indeed a fallacy. The specific case is the econometrics of the consumption function. In the 1950s, with the permanent income hypothesis, econometrics seemed to move towards depth realism: econometricians recognized that consumers do not react to empirical events (actual income) but to the underlying mechanisms (permanent income).

## 1 From Philosophical Ontology to Sociology of Science

My explanation of this move towards depth realism proceeds in two steps. First I will define *philosophical* (as opposed to scientific) ontology, and then I will show

that the Permanent Income Hypothesis is a switch in philosophical ontology, which makes the distinction between philosophical and scientific ontology very relevant for us:

- The move from actual income to permanent income is a change in philosophical ontology. The question whether the world has depth, i.e., whether permanent income is something real, something that has causal powers and can therefore be a legitimate independent variable in a regression equation, or whether it is just a “hypothetical construct” derived from actual income, which is the only reality that matters, is a question of *philosophical ontology*. Another important question of philosophical ontology relevant for economics is whether *methodological individualism* is appropriate, i.e., whether, as Thatcher said, “society does not exist,” or whether there are emergent powers in society or the economy.
- Questions of philosophical ontology cannot be decided empirically on the basis of looking at a few timeseries. The data will neither confirm nor disconfirm them. But these questions play a decisive role for the interpretation of these data. This is implicit in standard critical realist derivations of philosophical ontology by the question: what must be the case so that science is possible, and in the existence of the ontic fallacy. It also conforms with my teaching experience: the main obstacle preventing people from seeing the true character of capitalism is not that they don’t have the facts but that they are not taught to put these facts together in the right way. As long as people have a false philosophical ontology implanted in their heads, you don’t have to hide the facts from them, because they will not be able to draw conclusions from these facts. This is why a false philosophical ontological perspective is aptly called a “false consciousness.”

From this second point follows: if a researcher switches the philosophical ontology, he or she is not prompted by the data, but must have other reasons. As long as critical realist second-order arguments are unknown to most researchers, the main other reason that must be looked at here is the social function of the science in question. There is therefore a secret complicity between philosophical ontology and the sociology of science.

## **2 The Social Function of Economics**

The following argument is tentative, and in order to indicate this, I will formulate some of my points in the form of questions and answers. I rely in part here on my

own experiences and impressions as a participant in the economics profession. But more research is necessary. This will probably modify my answers, but I hope that the general argument will nevertheless remain applicable.

**Problem 1.** *What are the social functions of economics and econometrics?*

Econometrics and mainstream economic theory obviously have two quite different social functions:

- They are apologies for capitalism.
- They are the basis on which policy decisions are made, i.e., they have the function to coordinate policy.

As the argument unfolds below, an explanation will be given how these two functions can coexist without getting in conflict with each other.

### **3 Policy Dilemmas of the Capitalists in the 1950s**

**Problem 2.** *What are the policy issues faced by economists in the 1950s, when the permanent income thesis was developed?*

During World War II, it was widely feared that the Great Depression would return after the War. In addition, ends of wars are always a risky situation for the capitalist class, since the population is trained to fight, and dissatisfaction with capitalism, which systematically generates wars, is high. Furthermore, the impressive productivity of the war economy belies the myth that the market is more efficient than planning. Therefore it is to be expected that in the immediate post-war time, capitalists will always soft-pedal their thirst for profits, and pretended to be almost socialists (Humphrey-Hawkins bill).

In the 1950s, it became clear that the end-of-war challenges to capitalism had been safely navigated. The Great Depression had not returned, all post-war revolutionary movements had been beaten down, and capitalism was in a world wide boom. Now the capitalist class faced the following dilemma: would they be able to use the economic power of this rapidly accumulating wealth in order to clamp down on the working class and intensify exploitation, or would they still have to restrain their hunger for profits and continue to put on a “human face,” so that the socialist alternative would not be too tempting for their working classes? After all, socialism had become a powerful challenge to the capitalist system; the Chinese revolution had been carried to a successful end, and the Soviet Union had become a nuclear world power, with a rapidly growing economy and a sphere of influence which threatened to expand into India, Indonesia, and Egypt.

**Problem 3.** *If you open an economics textbook, no mention is made of the above dilemma. What gives?*

## **4 The Economists' Discreet Ways of Addressing the Dilemmas of Exploitation**

Economists did not discuss the dilemmas of the class they were serving in these terms. There is a strict taboo against saying things as simple and obvious as the preceding two paragraphs. This is “communist propaganda.” Nevertheless, even if it was never said, it is safe to assume that economists were smart enough to guess the above connections. It is not a coincidence that those nations which had close ties to socialist countries (West Germany, Taiwan) had well-functioning and benign capitalist regimes, and it is also not a coincidence that capitalism has become much more vicious after the collapse of the socialist bloc. These two examples indicate that the capitalist class knows when they have to put on good manners and when they can let their hair down.

**Problem 4.** *How were these fundamental policy questions resolved if nobody discussed them openly?*

Here I am putting a hypothesis forward which needs much more work in order to be verified. Namely I claim that the dispute between Keynesians and Monetarists provided a “scientific” cover for the debate “how many concessions to the working class are necessary to secure the survival of capitalism in the face of the socialist challenge”?

Keynesians argued for a bridled capitalism. But

- instead of saying “we need unemployment insurance in order to prevent the working class from fighting against the system,” Keynesians emphasized that unemployment insurance is an “automatic stabilizer” which smoothens out the “business fluctuations.”
- Instead of saying that capitalism does not really need a rate of profit of 30 percent, it can also survive on 20 percent, they promoted the theory that the economy is not “supply-constrained” but “demand-constrained,” that consumers stubbornly refuse to consume enough, and that therefore government expenditure has to supply the needed demand.
- Instead of arguing that capitalists should welcome unprecedented peace time military expenditures and the tax burden that goes along with them as an investment into a future without the Soviet Union, they argued that “fiscal poli-

cies” were needed to overcome certain technical market deficiencies which “monetary policy” alone would not be able to handle.

Monetarists, on the other hand, did not respond: “don’t worry about the Soviet Union, let’s just do business as usual and become filthy rich”; instead, they said

- government bureaucrats are too corrupt to be entrusted with meddling in the economy,
- policies are ineffective because the economic agents are too smart to fall for the tricks of the policy makers.

Of course, Friedman’s mantra that private entrepreneurs are smart and efficient, that their selfish acts lead to optimal outcomes, while government bureaucrats are corrupt, and whatever they do has a harmful influence on the economy, leaves no doubt about his thinking about socialism. But Friedman did not find it necessary for capitalists to give up any of their profits in order to appease the working class. He thought it was sufficient to feed them TV commercials telling them how good it was to be “free to choose.”

Both sides of the above policy dilemma were therefore represented, but not in the form: we ought to choose A or we ought to choose B, but in the form: B is the only choice because A is impossible, or A is the only choice because B is self-defeating. This debate denies the agency of the capitalist class. The question which course should they take is re-cast as the character of the unique equilibrium the economy is capable of, according to the idealized picture of capitalism which mainstream economics is painting. It is transformed into an apparently scientific debate about the correct theory of the macroeconomy.

**Problem 5.** *How did the capitalists know that this allegedly scientific debate would come up with the best policies in their class interest?*

This outcome is not necessarily best, but at least the capitalists can be assured that the outcome of this theoretical debate will be a feasible route for the system, since a theory can only win out if it has a certain correspondence with some of the facts. And if a global strategic decision of the international capitalist class is promulgated at the only possible truth then coordination is assured: dissenters are not seen as dissenters but as bad economists.

## **5 The Role of the Permanent Income Hypothesis in this Debate**

Monetarists said that fiscal policy was harmful. For this to become credible, Keynes’s theory of the consumption function had to be refuted, according to which con-

sumers do not consume enough, i.e., which holds that the “marginal propensity to consume” is less than 1. It could not be refuted on empirical grounds, the data seemed to confirm it, therefore a new interpretation of the same data was necessary. The basis of such a new interpretation was a change in philosophical ontology. The explanatory variable in the consumption function, Friedman claimed in [Fri57], is not income as it can be observed, but permanent income, i.e., the generative mechanism for income which the consumers are aware of but which are not directly reflected in any observable statistical data.

This change was a frontal attack on a basic aspect of the irrationalism of econometrics, namely its superficial empiricism. But this was a secondary question to the monetarists. Any shift in ontology which would allow them to interpret the data more favorably to the monetarist doctrine would have been welcome to them. Perhaps it was to be expected that this shift would lead away from empiricism; if the starting point is a flat empiricist position, then any change is likely to introduce some depth. The monetarists were also outsiders of the profession at the time, they were considered as lunatics, and they were willing to violate some of the professional taboos to get recognition for their theories.

Their goal was to reconcile the data with a marginal propensity to consume that is equal to 1 instead of smaller than 1. This they achieved by claiming that the too small coefficient is due to the “regression fallacy” or “regression effect.” The fact that this shifted the paradigm into a more depth realist direction was secondary to them.

The finished version of this paper will have here a brief explanation of the “regression fallacy.” Those not familiar with it may want to consult the chapter by this title in my econometrics class notes

[www.econ.utah.edu/ehrbarr/ecmet.pdf](http://www.econ.utah.edu/ehrbarr/ecmet.pdf)

(5 Megabytes). These notes should be mentioned here anyway since they contain tentative critical realist commentaries to some of the laws of probability and statistics.

**Problem 6.** *Did the monetarist position endanger the apologetic function of economics and econometrics, since it pulled the science into a more depth-realist direction, which would ultimately lead to a less flattering picture of the workings of capitalism?*

It is not my view that the monetarists played with a fire that might potentially engulf all of economics. There was no danger of such a fire. The success of the permanent income hypothesis did not cause economists to say, now let’s look for other underlying economic variables which are not immediately apparent; let’s

introduce the rate of profit or the rate of exploitation into our equations too, or let's look for the underlying reality of which prices are the measurements.

## **6 Reductionism as Emergency Brake against a Fall into Depth Realism**

Even in the theory of permanent consumption, the fire did not spread; here it was Friedman's reductionism which saved him from going down the realist road. Instead of seeing the agents as forward-looking centers of causal innovation, he modeled them as passive machines which mechanically extrapolate past experience.

The following argument was used: agents do not know their permanent income precisely, they can only make an estimate of it, i.e., the regressor is *expected* permanent income, not permanent income itself. In forming these expectations, the consumers use their experience of actual past incomes as one of the aids. And the econometricians model this formation of expectations by putting the same past incomes as proxies of permanent income into their equations.

Had these economists known critical realism, they would have seen immediately that this is a fallacious argument. It is an example of reductionism. Instead of recognizing that the agents' decisions form an emergent layer they reduce the consumption decision to a (small subset of) its prerequisites.

One will see also in the other examples below that whenever one aspect of the fundamental irrationalism of economics and econometrics is challenged, other irrational motives jump in as emergency brakes protecting its ideological function. This is the "advantage" of being embedded in a well elaborated edifice of irrational social sciences, in a society which does not want to know about itself.

## **7 Hyperdeterminism in Econometrics and its Unsurprising Consequences**

The sixties were a time when econometrics engaged in shallow hyperdeterminism. For depth they substituted the sheer multitude of surface relations, hoping that computers would allow them to run enough regressions so that they could find the constant conjunctions of events which they thought had to exist.

The models fitted during the sixties used some versions of Keynesian or Permanent Income consumption functions and other relations (investment functions, money demand functions) each of which has its own ad-hoc theory filling the papers of the professional journals, in the hope that this patchwork of theories would miraculously come together to a coherent theory of the economy. This is the il-

lusion of the empiricist who does not recognize that the many partial regularities he observes cannot be responsible for the order he or she sees around him or her, but that such an order can only be created by mechanisms active deep below the surface.

The more regressions they ran the more different results they got, results that were plausible if looked at in isolation, but which were inconsistent with each other.

Econometricians had various reactions to this. Phil Mirovsky suspected that the error distribution belonged to a family of distributions which had so many outliers that mere averaging or regression, which is a weighted averaging, will never reveal the central tendency of the data. His position was that econometrics was not possible.

Other econometricians explained the variety of results by the lack of discipline on the part of the researchers. [DHSY78, pp. 662] propose

that any new model should be related to existing “explanations” in a constructive research strategy such that previous models are only supplanted if new proposals account (so far as possible) for previously understood results, and also explain some new phenomena.

This is a sound realist principle. But after two sentences the same authors say

Unfortunately, much existing economic analysis relates to hypothetical constructs (for example, “permanent income”) . . .

i.e., here they take an empiricist stance which refuses to look beneath the surface. Here is another example where a partial movement in a realist direction is cancelled out by other movements away from realism. They rule out an important explanation of the diversity of estimation results, namely, that the surface data they measure are only very indirect reflections of those hidden forces which really drive the economy. The authors were experienced econometricians. Did they never suspect that the data themselves, as they come, are only very imperfect and sometimes perverse reflections of these mechanisms?

One of the main criteria applied by these econometricians was whether the estimated coefficients were stable. Coefficients which were derived from empirical relations in 1958–1970 were only then deemed correct if these same coefficients could help predict the empirical relations in an obviously different economic regime 1970–75. Again a Critical Realist can easily spot their error. Instead of distinguishing depth relations which remain unchanged from the empirical expressions of these underlying mechanisms which vary from regime to regime, they were looking for *empirical* relations which remain unchanged through the regimes.

These researchers placed high emphasis on scientific rigour. Unfortunately, doubts about the integrity of the scientific endeavor are often put forward by those

who have the least intuition about the philosophical ontology presupposed by the various methods which they find objectionable. Such uncounscious critiques did not make a dent in the ideological functions of econometrics; on the contrary, they helped it because they created the illusion that scientific standards were important for econometrics.

## 8 Stagflation in the 1970s

A much more serious challenge to econometrics arose when the large-scale econometric models were suddenly no longer able to predict. As long as econometrics takes a hyperdeterministic view instead of attempting to identify the real underlying mechanisms, such predictive failure must be expected as soon as new situations arise to which capitalism has to adapt. This happened with stagflation in the 70s. This breakdown re-kindled a number of debates in econometrics.

Monetarists argued that instead of large structural models, one needed small models with only few variables, with the money supply being the most important one. This is again the debate between bridled and unfettered capitalism. The proponents of unfettered capitalism argued that it was unnecessary to go through all the details which the large econometric models do, because all these details are simply the intermediary stages through which the market works out its efficient outcome. They argue therefore for emergence: the money demand function is the emergent outcome of various transmission mechanisms which one does not need to know in detail.

A different response to the predictive failures of the 1970s was to go back to the aborted realist self-criticism of econometrics in the 1950s, and to release the reductionist emergency brake which had been applied then. Muth [Mut60] had undermined the econometric practice of his contemporaries as early as 1960. He explored the conditions under which adaptive expectations were optimal, in order to come to the conclusion that they were usually *not* optimal. With a delay of 16 years, Lucas in [Luc76], the famous “Lucas critique,” took the logical next step: one cannot assume that economic agents engage in such suboptimal behavior. The pattern how people form their expectations is not a structural invariant which policy makers can rely upon for their policies to be effective. This was again a step in a realist direction: just as the Permanent Income Hypothesis attacked the lack of depth of modern economics, the Lucas critique attacked its reductionism.

Again, as in the 1950s, the philosophical ontological perspective was modified. However, this time, the ball, once it started rolling, was not stopped agan right away, but continued to roll. It is easy to see why: in the 1950s, a small quibble among more greedy and more cautious capitalists was at stake, in an environment

of economic stability and rapid growth. In the 1970s, competition was fiercer, the economic conditions much more turbulent, and economic policy makers truly did not know what to do. Therefore the debate in econometrics was more radical.

The Lucas critique did not imply that there are *no* stable structural invariants, it merely said that the consumption function is not one of them. The next question is then: which *are* the stable invariants? This is where [Hal78] comes in, which says: these invariants are consumers' optimization, rationality, and access to information. This is a depth realist explanation if there ever was one, but its emergency brake was that it anchored itself fully in methodological individualism. Hall's conclusion from these kinds of arguments was that consumption should be a martingale, i.e., that changes in consumption cannot be predicted from anything known at the time the changes were made, otherwise consumption could not have been optimal. This is a completely different kind of econometric question asked than before, and it generated a lot of enthusiasm. However it turned out that this thesis was empirically not tenable.

After monetarist models with few variables, vector autoregressive (VAR) models were promulgated, which tried to explain a group of timeseries by its own past alone. This "measurement without theory" was irrealist but fertile, because it liberated econometrics from the dead weight of false economic theory. The next step after this was the unit root hypothesis which, despite cointegration, places econometrics seriously at odds with received equilibrium-oriented economic theory. Instead of a closed system which gravitates towards equilibrium, this paradigm conceives the economy as an open system.

Much more can be said here, but the point I want to make is that questions of philosophical ontology played an important although unrecognized role in the debates of econometrics.

## **9 Conclusion: What Difference does Critical Realism Make?**

One observes a lively debate in econometrics, which is fueled not by different data, but by different perspectives in philosophical ontology. It is my conjecture that the purpose of these debates is not to find the truth about the capitalist economy, but to coordinate capitalist policies and strategies. Switches in philosophical ontology are introduced whenever received policies no longer work, or when competing policy proposals have to be decided upon. However every turn towards more realism was always accompanied by several irrealist emergency brakes.

As long as philosophical ontology has to rely on the common sense of the researcher, this switching back and forth is a possible maneuver by which an undertaking which does as much empirical work as econometrics can be adjusted to

the ideological needs of the capitalist class. The researcher may spontaneously discover certain aspects of the prevailing irrationalism, but it is unlikely that he or she will see all of them without a systematic theory. Critical Realism is such a systematic theory. It could conceivably make it more difficult for the capitalists to mould econometrics according to their class interests.

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