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Mainstream economics – the poverty of fictional storytelling

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Introduction

Mainstream economists usually content themselves with stating something like "economics is what economists do" and "economists are those who deal with economics." No deeper philosophizing is considered necessary.

However, this is an unsustainable and not particularly enlightening attitude. Scientific, philosophical, and methodological analyses of economic science are both important and necessary. Methodological knowledge serves as a 'road map'. When we discover that we are not on the road, we must occasionally look at the map or model for scientific progress that we all carry with us, consciously or unconsciously. Economic theories *always* build -- consciously or unconsciously -- on methodological positions. The question is therefore not whether economists should deal with methodology or not, but rather *how* it should best be done. Methodological analysis is both desirable and inevitable in economic science. Not least, it can have a critical function by making economists aware that the fundamental flaws of economic theory may be due to the fact that the concepts, theories, and models they use are incompatible with the very object of investigation. The tools borrowed mainly from physics and mathematics were constructed to solve completely different tasks and problems and risk contributing to a non-correspondence between the structure of economic science and the structure of economic reality. This, in turn, can lead economists to dubious simplifications and generalizations. Predictions and explanations based on such unauthorized 'bridges' between theory and reality have limited value and reliability.

The starting point for my criticism of mainstream economics is critical realism, a scientific and methodological direction primarily developed by the philosopher of science Roy Bhaskar. In *A Realist Theory of Science*, Bhaskar (1997) laid the foundation for a scientific theoretical stance that primarily means that the basic ontological question -- how reality must be constituted for us to be able to know something about it -- is prioritized over the epistemological question of how knowledge is possible. Against *naive realism*, critical realism asserts that we must focus our attention primarily on the underlying causal mechanisms of reality rather than just studying empirically observable events.

Economic methodologists try to develop and apply criteria for evaluating different theories (coherence, correspondence, clarity, simplicity, etc.). They often differ in their views on what constitutes reliable sources of knowledge (observation, experiment, theoretical reflection), what theories should look like (descriptive, analytical), how they should be tested (econometrics, case studies, randomized controlled studies, etc.), and whether they should be mathematically formulated or not. But the most fundamental question economic methodologists try to answer is what the purpose of economic analysis is. Although opinions are many, it can be said that there are two different fundamental views. One is that the goal of economic analysis is to better predict what happens in the economy and to develop better instruments for steering the economy in a certain direction. The other -- to which a critical realist methodology must be counted -- asserts that economic analysis must first and foremost aim to *understand* and *explain* how the economy works.

The inadequacy of instrumentalism

Realism as a methodological approach has been remarkably neglected or even unnoticed by mainstream economists. To the extent that it has been addressed, it is largely in connection with the debate over Milton Friedman's (Friedman, 1953) defense of the thesis that the realism of economic theory's assumptions is irrelevant. Against this background, it is perhaps not surprising that economists have recoiled from arguments presented from a realist perspective.

Friedman's instrumentalism is a particular version of empiricism according to which the goal of science is to formulate theories that can predict empirical events. Theories should therefore not be judged based on their explanatory power, but on how good instruments they are for delivering correct predictions. Theories and theoretical concepts should be understood as useful fictions that facilitate our predictions. If there is a 'real' world behind the theories we use, the instrumentalist is not concerned. Science cannot function as a bridge between the observable and the non-observable.

This 'fictionalist' perspective leads to the perception of the assumptions of economic theory as 'as if' assumptions, whose value lies in the fact that we can construct models that simplify complex contexts and make them analyzable and understandable.

For a scientific realist, Friedman's approach is unsatisfactory. If a theory proves to be useful, this is not a coincidence but depends on the nature of the object of knowledge. For a realist, it is necessary to try to find assumptions in theories that are practically relevant and that work and are consistent even in other contexts and with other known knowledge and practices. Theories should be *robust* and able to explain studied events by indicating what caused them, not just logically deducible.

Explanation and prediction of economic phenomena require theory construction, and seeking event correlations is therefore not enough. One must simply 'get beneath the surface' and see the underlying structures and generative mechanisms that explain the economic system.

Instead of explanations and predictions based on events and experiences, the primary object of investigation for critical realism is the *generative mechanisms* in the 'domain of reality' that cause events. The fact that economics has had such difficulty predicting events is not primarily because the theory would be incorrect, but because the underlying methodology assumes that the world consists of event regularities. Such regularities exist only if we assume that the world is *closed* -- which is a deeply unrealistic assumption.

Mainstream theory has its own solution to the problem -- it introduces a *ceteris paribus* clause. However, this is done at the expense of the fact that economics studies a theoretical and closed construction of the economy rather than the real economy, which is open. A paradoxical fact associated with mainstream theory is that it is said to be a theory of free individual choices in the market. But to predict events in such a model, choices must be 'closed' and preferences must be given and stable -- in short, the individual must be described as lacking real choices. They try to explain an event in an open social world (Y) from assumptions about what happens in a model that only applies in a closed world (X). But the explanation cannot be transferred from X to Y.

The atomistic fallacy

Methodological individualism -- explaining social institutions and change as a result of individual actors' actions and interactions -- has always had a strong position in mainstream economics.

But social and economic phenomena cannot be adequately explained by breaking them down into their smallest components or underlying strata without losing important emergent properties. Against the mainstream theory's reductionist view of 'macro explanations' as redundant once the micro level is mapped, critical realists object that social and economic contexts cannot be reduced to the individual level.

In economic theory, attempts are often made to build theory 'from the bottom up,' primarily expressed in the project of creating 'microfoundations' for macro theory (cf. Syll 2016). Sometimes this is done through careless aggregation, which is guilty of the so-called *atomistic fallacy*. The problem is that all the effects of microeconomic phenomena cannot be captured by simple aggregation. Although mainstream economic theory considers the mutual interdependencies of the parts, it often misses the more important emergent fact that the parts themselves can change and are partly constituted by the whole. The whole is not just a sum of parts but also something that affects and changes the nature and functions of the parts. From a critical realist view of social structures as real and independent entities, the entire micro-based project does not seem particularly fruitful.

When the model becomes the message

Economics is a science that relies heavily on the use of models. In economics textbooks, the use of models is generally presented as simplified descriptions of reality with which one constructs a kind of thought experiment and tests various hypotheses. The simple supply and demand model is a typical example. Mainstream economists admit -- usually -- that reality does not look like economic models, but that models have a justification as a kind of simplified *benchmark* with which the economist can describe and understand how economic mechanisms work.

But if the model does not reflect reality, *how* can we benefit from it? Is 'simplicity' and the ability to lead to 'clear conclusions' the model's most relevant aspect? How can we be sure that the model abstracts the 'essential' features? There is something deceptive about the evasive justification. It gives the *appearance* of qualifying the use of models in economics, but at the same time, it leaves open the question of how and to what extent models can contribute to our understanding of economic reality.

Sometimes it is argued that the use of formal models is good because they can guarantee logical consistency, force explicit assumptions, enable a concentrated representation, and facilitate communication among researchers. This may be true, but one should not forget that these advantages often lead to simplicity and mathematical elegance replacing explanatory power, that the models give rise to counterintuitive and paradoxical results, and that the desire for formalization can become an end in itself.

Model simplifications can be *harmless* or *restrictive* (depending on whether they affect deductions made within the theory in a decisive way or not). If the simplifications are of the restrictive type, the theory/model cannot be adequately used to explain or predict real events. If the restrictive assumption is removed, we cannot be sure that the relationships established in the theory/model remain.

The Iron Cage of Mathematics

Since the mid-20th century, mainstream economics has increasingly meant examining the world using the tools of mathematics. However, instead of uncritically adopting a mathematical representation form, economists should ask themselves what conditions the real processes and objects must meet for mathematical representations of them to be relevant. When the economic relationships or objects we are measuring or representing undergo qualitative changes, we cannot, just because a mathematical representation for 'tractability reasons' often requires it, model them as if they were given constants.

The precision and 'clarity' that mathematical language brings are no guarantee that the models are 'true.' Like any other models, they must be confronted with empirical observations and theory to determine whether they are adequate representations of reality.

Mathematics *can* be an excellent tool for model construction. But it must not become an end in itself. The fact that economic science is more quantitative than other social sciences is partly due to the fact that its objects of study are largely naturally quantitative (money, accounts, salaries, profits, etc.). However, this cannot be a defense for driving the mathematization trend *in absurdum* or for failing to ask what the mathematical models and quantitative measures are models of and measures of.

An exaggerated emphasis on the strength of mathematics and quantitative methods is typical of mainstream economists' deductivist approach. The dominance of the deductivist method in economic science is largely due to its suitability for mathematical modeling and its dominance in natural science (cf. Syll 2023). Since the end of the 19th century, neoclassical economics has seen natural science as a model for developing a supposedly more 'scientific' economic discipline.

The price paid for the mathematization of economics is that the subject has had to abandon the real world in order to focus scientific aspirations on proving things about imaginary worlds. Instead of accepting that a lower degree of certainty is inevitable, one engages in axiomatic and rationalistic model constructions that enable secure knowledge. If the goal is knowledge of the real world, the value of these is at best unclear.

No, there is nothing wrong with mathematics per se. No, there is nothing wrong with applying mathematics to economics. Mathematics is one valuable tool among other valuable tools for understanding and explaining things in economics. What is, however, totally wrong, are the utterly simplistic beliefs that

- "math is the only valid tool"
- "math is always and everywhere self-evidently applicable"
- "math is all that really counts"
- "if it's not in math, it's not really economics'
- "almost everything can be adequately understood and analyzed with math"

Mainstream economists have always wanted to use their hammer, and so have decided to pretend that the world looks like a nail. Pretending that uncertainty can be reduced to risk and that all activities, relations, processes, and events can be adequately converted to pure numbers, have only contributed to making economics irrelevant and powerless when confronting real-world financial crises and economic havoc.

Mainstream economic theory today is still in the storytelling business whereby economic theorists create mathematical make-believe analog models of the target system -- usually conceived as the real economic system. This mathematical modeling activity is considered useful and essential. To understand and explain relations between different entities in the real economy the predominant strategy is to build mathematical models and make things happen in these 'analog-economy models' rather than engineering things happening in real economies.

Without strong evidence, all kinds of absurd claims and nonsense may pretend to be science. But -- math never has, and never will, be able to establish the truth value of facts.

We have to demand more of a justification than rather watered-down versions of 'anything goes' when it comes to the main postulates on which mainstream economics is founded. If one proposes 'efficient markets' or 'rational expectations' one also has to support their underlying assumptions. As a rule, none is given, which makes it rather puzzling how things like 'efficient markets' and 'rational expectations' have become standard modeling assumptions made in much of modern macroeconomics. The reason for this sad state of 'modern' economics is that economists often mistake mathematical beauty for truth.

How do we put an end to this intellectual cataclysm? How do we re-establish credence and trust in economics as a science? A couple of changes are absolutely decisive:

- Stop pretending that we have exact and rigorous answers on everything. Because we don't. We build models and theories and tell people that we can calculate and foresee the future. But we do this based on mathematical and statistical assumptions that often have little or nothing to do with reality. By pretending that there is no really important difference between model and reality we lull people into thinking that we have things under control. We haven't! This false feeling of security was one of the factors that contributed to the financial crisis of 2008.

- Stop the childish and exaggerated belief in mathematics giving answers to important economic questions. Mathematics gives exact answers to exact questions. But the relevant and interesting questions we face in the economic realm are rarely of that kind. Questions like "Is 2 + 2 = 4?' are never posed in real economies. Instead of a fundamentally misplaced reliance on abstract mathematical-deductive-axiomatic models having anything of substance to contribute to our knowledge of real economies, it would be far better if we pursued "thicker' models and relevant empirical studies and observations.

- Stop pretending that there are laws in economics. There are no universal laws in economics. Economies are not like planetary systems or physics labs. The most we can aspire to in real economies is establishing possible tendencies with varying degrees of generalizability.

- Stop treating other social sciences as poor relations. Economics has long suffered from hubris. A more broad-minded and multifarious science would enrich today's altogether too autistic economics.

- Stop building models and making forecasts of the future based on totally unreal micro-founded macro models with intertemporally optimizing robot-like representative actors equipped with rational expectations. This is pure nonsense. We have to build our models on assumptions that are not so blatantly in contradiction to reality. Assuming that people are green and come from Mars is not a good – not even as a 'successive approximation' – modeling strategy.

Model and Reality

Mainstream economists often believe that models and theories will approach the 'truth' through 'successive approximations' by incorporating more and more factors and relaxing the idealizing assumptions. However, using 'isolation' and 'successive approximations' as a heuristic method is not convincing. For the method to work well, the factors studied in 'isolation' must be real causal factors or tendencies, and the effects of the studied factors must be treated separately and mechanically added and interacted. However, rationality and maximization assumptions in mainstream theory do not represent real causal relationships or tendencies, but often only misleading fictions. It is difficult to imagine that complex economic phenomena could be adequately treated by considering them as if they existed separately or in isolation from each other. Economic contexts are fundamentally contextual and open. Therefore, the possibility of treating the economy as if its causal factors could be mechanically combined is generally excluded. It is not clear what kind of interesting and relevant conclusions can be drawn about our economy using 'successive approximations' from assumptions about all-knowing and infallible individuals and companies. If the starting point is wrong, so is the end product. The analysis does not contribute to explaining the generative mechanisms and forces of society and the economy. It only leads us astray.

Economics is more model-oriented than any other social science. There are many reasons for this - the history of the subject, ideals (drawn from the natural sciences), universal claims, the desire to explain as much as possible with as little as possible, rigor, precision, etc.

The approach is fundamentally *analytical* -- the whole is broken down into its components so that it is possible to explain (reduce) the aggregate (macro) as a result of the interaction between the parts (micro).

Mainstream economists typically base their models on a number of core assumptions (CA) -- which fundamentally describe actors as 'rational' -- as well as a number of auxiliary assumptions (AA). Together, (CA) and (AA) constitute what we could call the base model (M) for all mainstream models. Based on these two sets of assumptions, one tries to explain and predict both individual (micro) and societal phenomena (macro).

The core assumptions typically consist of:

CA_1 Completeness -- the rational actor is always able to compare different alternatives and determine which she prefers
CA_2 Transitivity -- if the actor prefers A to B and B to C, she must prefer A to C
CA_3 Non-satiation -- more is always better than less
CA_4 Maximization of expected utility -- in situations characterized by risk, the actor always maximizes expected utility
CA_5 Consistent economic equilibria -- different actors' actions are consistent, and their interaction results in an equilibrium.

When describing actors as rational in these models, *instrumental* rationality is meant -- the actors are assumed to choose alternatives that have the best consequences given their given preferences. How these given preferences have arisen is generally perceived to be outside the scope of the concept of rationality and therefore not part of economic theory as such.

The picture one gets of the core assumptions ('rational choices') is a rational actor with strong cognitive capacities, who knows what she wants, carefully considers her alternatives, and given her preferences, chooses what she believes has the best consequences for her. By weighing the various alternatives against each other, the actor makes a consistent, rational choice and acts accordingly.

The auxiliary assumptions (AA) specify spatial and temporal aspects of the type of interaction that can take place between 'rational' actors. These assumptions often provide answers to questions such as:

AA_1: Who are the actors, and where and when do they interact?
AA_2: What are their goals and aspirations?
AA_3: What interests do they have?
AA_4: What are their expectations?
AA_5: What kind of agency do they have?
AA_6: What kind of agreements can they make?
AA_7: How much and what kind of information do they possess?
AA_8: How do their actions interact with each other?

The basic model for all mainstream models is thus a general picture of what (axiomatically) constitutes optimizing rational actors (CA), and a more specific description (AA) of the situations in which these actors act (which means that AA functions as a restriction that determines the intended application domain for CA and the deductively derived theorems). The list of assumptions can never be complete because there are always unspecified 'background assumptions' and unmentioned omissions, often based on some kind of negligibility and application considerations. The hope is that this 'thin' set of assumptions will be sufficient to explain and predict 'rich' phenomena in the real, complex world.

In the extreme case, the theorems turn into non-testable tautological thought experiments with no other empirical ambitions than to tell a coherent fictional 'as-if' history.

Not clearly distinguishing between (CA) and (AA) opens up all sorts of attempts to 'save' or 'immunize' models from criticism by unfairly 'sliding' between interpreting the models as empirically empty deductive-axiomatic analytical 'systems' or as models with explicit empirical aspirations (cf. Albert 2012). In ordinary cases, flexibility may be seen as positive, but in a methodological context, it is rather a sign of a problem. Models that are compatible with everything or come with unspecified application domains are worthless from a scientific point of view.

Economics -- unlike logic and mathematics -- should be an empirical science, and empirical tests of 'axioms' should obviously be relevant to such a discipline. Even if mainstream economists themselves (implicitly or explicitly) claim that their axioms are universally accepted as 'true' and without the need for proof, this is obviously not a reason for others to simply accept them.

When mainstream economists' deductive 'thinking' is used, it usually results in the construction of 'as-if' models based on some form of idealization logic and a set of axiomatic assumptions from which consistent and precise inferences can be made. The beauty of this, of course, is that if the axiomatic premises are true, the conclusions necessarily follow. However, although the procedure is successfully used in mathematics and mathematically derived axiomatic-deductive systems, it is a poor guide for understanding and explaining systems in the real world.

Most theoretical models that mainstream economists work with are abstract and unrealistic constructions that are used to construct non-testable hypotheses. How this can tell us anything relevant and interesting about the world we live in is difficult to see. Faced with the massive empirical failures these models and theories have led to, many mainstream economists retreat and choose to present their models and theories as merely a kind of thought experiment without any real aspirations to tell us anything about the real world. Instead of ‘bridging’ the model and reality, they simply give up. However, this type of scientific defeatism is completely unacceptable. It can never be enough to prove or deduce things in a model world. If theories do not -- directly or indirectly -- tell us something about the world we live in, why should we waste time on them?

Open and closed models

In all science, the search for regularities and laws has been an essential feature. The goal has often been to make predictions based on these regularities and laws. Although natural science has been partly successful in this regard, economics has encountered major problems (which perhaps testify to all the failed economic forecasts). Some have drawn the conclusion that economic science is still underdeveloped, and if given enough time, it should certainly be possible to achieve the same predictive reliability as the natural sciences. From a critical realist perspective, however, the economic science's lack of predictive ability is mainly a result of its research object's inherent structure and character.

The critical realist researcher investigates the conditions that must be met for regularities and law-like regularities to be said to exist (cf. Lawson 1997). One condition is that there must be no changes or qualitative variations in the object that determines the causal relationships, i.e., the underlying mechanisms must be stable (the condition of *internal closure*). The internal structure of the system must be constant over time. One must try to find an analysis unit that is invariant over time and that exhibits a constant pattern of behavior. This makes it possible to postulate a stable and identifiable relationship between a set of conditions X_1, X_2, ..., X_n and the value of a variable Y.

Another condition for regularities and law-like regularities is that the relationship between the causal mechanisms and the mechanisms in the environment that influence them must be constant (the condition of *external closure*). If we are to identify empirical regularities, we must be able to isolate the system from non-constant external influences. This means that only explicitly treated conditions X_1, X_2, ..., X_n have a systematic, non-constant impact on Y. The purpose of this condition is to exclude the impact of unspecified conditions on Y. If a system cannot be physically isolated from the environment, the environmental factors must either be internalized in the system, or one must ensure that they have a constant and unchanging influence on the system. If both closure conditions are met, we have a closed system that meets the conditions for regularities to be produced. But most real systems we encounter do not meet these conditions. They are *open* systems whose possible regularities are more transient, local, or approximate.

The pitfalls of econometrics

As an example of the problems that the deductivist approach leads to, econometrics is a good example. This part of economics can be said to aim to establish probabilistic regularities consisting of a dependent variable Y being functionally related to a set of independent variables X, so that changes in the latter can be shown to give rise to predictable variations in the former. To date, no universal laws have been found - judging from the existing literature. Today, we are still as surprised as Trygve Haavelmo was in the 1940s that the estimated relationships break down at the same rate as new data becomes available.

From a critical realist perspective, however, it is hardly surprising that an approach that assumes a closed system fails when applied to an open system such as an economy. According to the so-called ‘Lucas critique’, the problem with econometric models is that the identified relationships are unstable, while the underlying theory is based on the assumed existence of stable parameter relationships. Therefore, the models do not allow for more far-reaching predictions.

The starting point for econometric analysis is partly that social and economic relations can be modeled using mathematical functions, and partly that it is possible to obtain knowledge about the parameters of these functions. However, it is difficult to confirm the validity of these assumptions. Such confirmation would imply that the econometrician not only had a statistical model but also a 'true model', a mathematical function that connects the different variables. Therefore, researchers often assume that the true model exists but is unknown. Instead of trying to estimate how well the econometric model coincides with the 'true' model, researchers are content to analyze how well the econometric model specifies a particular theory. It is difficult to see how this could contribute in any interesting way to explaining real events and structures.

The pre-requisite for predictions based on econometric equations to have any greater value is, of course, that the relationships represented in the equations are stable over time and space. If the mechanisms through which the relationships operate do not remain constant over time, reliable predictions cannot be made. In the economy, it is a pervasive characteristic that behavioral patterns do not exhibit the required invariance. The causal forces and structures that shape the relationships are not constant but constantly evolving and transforming. Consumption and investment patterns fluctuate continuously, and this is not solely due to relevant variables being omitted or mis-specified in the econometric model. The relationships themselves change due to variations in the underlying mechanisms. The error in this type of generalization lies simply in treating temporary and transient relationships as universal and invariant natural laws.

The very basic assumption for applying probabilistic logic to real systems is that the uncertainty exhibited is not of the type that we usually call *genuine uncertainty* (where we cannot practically assign any numerical values to future expectations). Instead, all uncertainty is assumed to be reducible to *calculable risk* (such as coin tossing and roulette). That this would be a sustainable assumption for understanding real economic systems and relationships is simply a belief for which there is no other foundation than the belief itself.

Trying to reduce the risk of having established only 'spurious relations' when dealing with observational data, statisticians and econometricians standardly add control variables. The hope is that one thereby will be able to make more reliable causal inferences. But — as Keynes showed already back in the 1930s when criticizing statistical-econometric applications of regression analysis — if you do not manage to get hold of *all* potential confounding factors, the model risks producing estimates of the variable of interest that are even worse than models without any control variables at all.

Conclusion: think twice before you simply include 'control variables' in your models!

'Kitchen sink' econometric models are often the result of researchers trying to control for confounding. But what they usually haven't understood is that the confounder problem requires a causal solution and not statistical 'control.' Controlling for everything opens up the risk that we control for 'collider' variables and thereby create 'back-door paths' which gives us confounding that wasn't there to begin with.

Causality can never be reduced to a question of statistics or probabilities unless you are — miraculously — able to keep constant all other factors that influence the probability of the outcome studied. To understand causality, we always have to relate it to a specific causal structure. Statistical correlations are never enough. No structure, no causality.

Explanatory fictionalism and the experimental hype

One of the limitations of economics is the restricted possibility to perform experiments, forcing it to mainly rely on observational studies for knowledge of real-world economies.

But still — the idea of performing laboratory experiments holds a firm grip on our wish to discover (causal) relationships between economic 'variables.' If we only could isolate and manipulate variables in controlled environments, we would probably find ourselves in a situation where we with greater 'rigour' and 'precision' could describe, predict, or explain economic happenings in terms of 'structural' causes, 'parameter' values of relevant variables, and economic 'laws.'

Galileo Galilei's experiments are often held as exemplary for how to perform experiments to learn something about the real world. Galileo's heavy balls dropping from the tower of Pisa, confirmed that the distance an object falls is proportional to the square of time and that this law (empirical regularity) of falling bodies could be applicable outside a vacuum tube when e. g. air existence is negligible.

The big problem is to decide or find out exactly for which objects air resistance (and other potentially 'confounding' factors) is 'negligible.' In the case of heavy balls, air resistance is obviously negligible, but how about feathers or plastic bags?

One possibility is to take the all-encompassing-theory road and find out all about possible disturbing/confounding factors — not only air resistance — influencing the fall and build that into one great model delivering accurate predictions on what happens when the object that falls is not only a heavy ball but feathers and plastic bags. This usually amounts to ultimately stating some kind of ceteris paribus interpretation of the 'law.'

Another road to take would be to concentrate on the negligibility assumption and to specify the domain of applicability to be only heavy compact bodies. The price you have to pay for this is that (1) 'negligibility' may be hard to establish in open real-world systems, (2) the generalization you can make from 'sample' to 'population' is heavily restricted, and (3) you actually have to use some 'shoe leather' and empirically try to find out how large is the 'reach' of the 'law.'

In mainstream economics, one has usually settled for the 'theoretical' road (and in case you think the present 'natural experiments' hype has changed anything, remember that to mimic real experiments, exceedingly stringent special conditions standardly have to obtain).

In the end, it all boils down to one question — are there any Galilean 'heavy balls' to be found in economics, so that we can indisputably establish the existence of economic laws operating in real-world economies?

As far as I can see there are some heavy balls out there, but not even one single real economic law.

Economic factors/variables are more like feathers than heavy balls — non-negligible factors (like air resistance and chaotic turbulence) are hard to rule out as having no influence on the object studied.

Galilean experiments are hard to carry out in economics, and the theoretical 'analogue' models economists construct and in which they perform their 'thought experiments' build on assumptions that are far away from the kind of idealized conditions under which Galileo performed his experiments. The 'nomological machines' that Galileo and other scientists have been able to construct have no real analogues in economics. The stability, autonomy, modularity, and interventional invariance, that we may find between entities in nature, simply are not there in real-world economies. That's a real-world fact, and contrary to the beliefs of most mainstream economists, they won't go away simply by applying deductive-axiomatic economic theory with more or less unsubstantiated assumptions.

By this, I do not mean to say that we have to discard all (causal) theories/laws building on modularity, stability, invariance, etc. But we have to acknowledge the fact that outside the systems that possibly fulfil these requirements/assumptions, they are of little substantial value. Running paper and pen experiments on artificial 'analogue' model economies is a sure way of 'establishing' (causal) economic laws or solving intricate econometric problems of autonomy, identification, invariance and structural stability — in the model world. But they are pure substitutes for the real thing and they don't have much bearing on what goes on in real-world open social systems. Setting up convenient circumstances for conducting Galilean experiments may tell us a lot about what happens under those kinds of circumstances. But — few, if any, real-world social systems are 'convenient.' So, most of those theories and models, are irrelevant for letting us know what we really want to know.

To solve, understand, or explain real-world problems you actually have to know something about them — logic, pure mathematics, data simulations or deductive axiomatics don't take you very far. Most econometrics and economic theories/models are splendid logic machines. But — applying them to the real world is a totally hopeless undertaking! The assumptions one has to make in order to successfully apply these deductive-axiomatic theories/models/machines are devastatingly restrictive and mostly empirically untestable– and hence make their real-world scope ridiculously narrow. To fruitfully analyze real-world phenomena with models and theories you cannot build on patently and known to be ridiculously absurd assumptions. No matter how much you would like the world to entirely consist of heavy balls, the world is not like that. The world also has its fair share of feathers and plastic bags.

Most of the 'idealizations' we find in mainstream economic models are not 'core' assumptions, but rather structural 'auxiliary' assumptions. Without those supplementary assumptions, the core assumptions deliver next to nothing of interest. So, to come up with interesting conclusions you have to rely heavily on those other — 'structural' — assumptions.

In physics, we have theories and centuries of experience and experiments that show how gravity makes bodies move. In economics, we know there is nothing equivalent. So instead, mainstream economists necessarily have to load their theories and models with sets of auxiliary structural assumptions to get any results at all in their models.

So why then do mainstream economists keep on pursuing this modelling project?

The way axioms and theorems are formulated in mainstream economics often leaves their specification without almost any restrictions whatsoever, safely making every imaginable evidence compatible with the all-embracing 'theory' — and theory without informational content never risks being empirically tested and found falsified. Used in mainstream 'thought experimental' activities, it may, of course, be very 'handy,' but totally void of any empirical value.

Some economic methodologists have lately been arguing that economic models may well be considered 'minimal models' that portray 'credible worlds' without having to care about things like similarity, isomorphism, simplified 'representationality' or resemblance to the real world. These models are said to resemble 'realistic novels' that portray 'possible worlds'. And sure: economists constructing and working with those kinds of models learn things about what might happen in those 'possible worlds'. But is that really the stuff real science is made of? I think not. As long as one doesn't come up with credible export warrants to real-world target systems and show how those models — often building on idealizations with known to be false assumptions — enhance our understanding or explanations about the real world, well, they are just nothing more than just novels. Showing that something is possible in a 'possible world' doesn't give us a justified license to infer that it therefore also is possible in the real world. 'The Great Gatsby' is a wonderful novel, but if you truly want to learn about what is going on in the world of finance, I recommend rather reading Minsky or Keynes and directly confronting real-world finance.

Different models have different cognitive goals. Constructing models that aim for explanatory insights may not optimize the models for making (quantitative) predictions or deliver some kind of 'understanding' of what's going on in the intended target system. All modelling in science has tradeoffs. There simply is no 'best' model. For one purpose in one context model A is 'best', for other purposes and contexts model B may be deemed 'best'. Depending on the level of generality, abstraction, and depth, we come up with different models. But even so, one could argue that if we are looking for 'adequate' explanations, it is not enough to just come up with 'minimal' or 'credible world' models.

The assumptions and descriptions we use in our modelling have to be true — or at least 'harmlessly' false — and give a sufficiently detailed characterization of the mechanisms and forces at work. Models in mainstream economics do nothing of the kind.

Coming up with models that show how things may possibly be explained is not what we are looking for. It is not enough. We want to have models that build on assumptions that are not in conflict with known facts and that show how things actually are to be explained. Our aspirations have to be more far-reaching than just constructing coherent and 'credible' models about 'possible worlds'. We want to understand and explain 'difference-making' in the real world and not just in some made-up fantasy world. No matter how many mechanisms or coherent relations you represent in your model, you still have to show that these mechanisms and relations are at work and exist in society if we are to do real science. Science has to be something more than just more or less realistic 'storytelling' or 'explanatory fictionalism.' You have to provide decisive empirical evidence that what you can infer in your model also helps us to uncover what actually goes on in the real world. It is not enough to present epistemically informative insights about logically possible models. You also, and more importantly, have to have a world-linking argumentation and show how those models explain or teach us something about real-world economies. If you fail to support your models in that way, why should we care about them? And if you do not inform us about what are the real-world intended target systems of your modelling, how are we going to be able to value or test them? Without giving that kind of information it is impossible for us to check if the 'possible world' models you come up with actually hold also for the one world in which we live — the real world.

Keynes' critique of econometrics

Mainstream economists often hold the view that Keynes' criticism of econometrics was the result of a sadly misinformed and misguided person who disliked and did not understand much of it. This is, however, nothing but a gross misapprehension (cf. Nasir & Morgan 2023). To be careful and cautious is not the same as to dislike. Keynes did not misunderstand the crucial issues at stake in the development of econometrics. Quite the contrary. He knew them all too well — and was not satisfied with the validity and philosophical underpinning of the assumptions made for applying its methods.

Keynes' critique of the 'logical issues' regarding the conditions that have to be satisfied if we are going to be able to apply econometric methods, is still valid and unanswered in the sense that the problems he pointed at are still with us today and largely unsolved. Ignoring them — the most common practice among applied econometricians — is not to solve them.

To apply statistical and mathematical methods to the real-world economy, the econometrician has to make some quite strong assumptions. In a review of Tinbergen's econometric work, Keynes (1939) gave a comprehensive critique of Tinbergen's work, focusing on the limiting and unreal character of the assumptions that econometric analyses build on:

- Completeness: Where Tinbergen attempts to specify and quantify which factors influence the business cycle, Keynes maintains there must be a complete list of all the relevant factors to avoid misspecification and spurious causal claims. Usually, this problem is 'solved' by econometricians assuming that they somehow have a 'correct' model specification. Keynes was, to put it mildly, unconvinced.

- Homogeneity: To make inductive inferences possible — and be able to apply econometrics — the system we try to analyse has to have a large degree of 'homogeneity.' According to Keynes most social and economic systems — especially from the perspective of real historical time — lack that 'homogeneity.' As he had argued already in Treatise on Probability, it wasn't always possible to take repeated samples from a fixed population when we were analyzing real-world economies. In many cases, there simply are no reasons at all to assume the samples to be homogenous. Lack of 'homogeneity' makes the principle of 'limited independent variety' non-applicable, and hence makes inductive inferences, strictly seen, impossible since one of its fundamental logical premises is not satisfied.

And then, of course, there is also the 'reverse' variability problem of non-excitation: factors that do not change significantly during the period analysed, can still very well be extremely important causal factors.

- Stability: Tinbergen assumes there is a stable spatio-temporal relationship between the variables his econometric models analyze. But as Keynes had argued already in his Treatise on Probability it was not really possible to make inductive generalisations based on correlations in one sample. As later studies of 'regime shifts' and 'structural breaks' have shown us, it is exceedingly difficult to find and establish the existence of stable econometric parameters for anything but rather short time series.

- Measurability: Tinbergen's model assumes that all relevant factors are measurable. Keynes questions if it is possible to adequately quantify and measure things like expectations and political and psychological factors. And more than anything, he questioned — both on epistemological and ontological grounds — that it was always and everywhere possible to measure real-world uncertainty with the help of probabilistic risk measures. Thinking otherwise can, as Keynes wrote, "only lead to error and delusion."

- Independence: Tinbergen assumes that the variables he treats are independent (still a standard assumption in econometrics). Keynes argues that in such a complex, organic and evolutionary system as an economy, independence is a deeply unrealistic assumption to make. Building econometric models from that kind of simplistic and unrealistic assumptions risks producing nothing but spurious correlations and causalities. Real-world economies are organic systems for which the statistical methods used in econometrics are ill-suited, or even, strictly seen, inapplicable. Mechanical probabilistic models have little leverage when applied to non-atomic evolving organic systems — such as economies.

Building econometric models can't be a goal in itself. Good econometric models are means that make it possible for us to infer things about the real-world systems they 'represent.' If we can't show that the mechanisms or causes that we isolate and handle in our econometric models are 'exportable' to the real world, they are of limited value to our understanding, explanations or predictions of real-world economic systems.

- Linearity: To make his models tractable, Tinbergen assumes the relationships between the variables he studies to be linear. This is still standard procedure today, but to Keynes, it was a 'fallacy of reification' to assume that all quantities are additive (an assumption closely linked to independence and linearity).

And as even one of the founding fathers of modern econometrics — Trygve Haavelmo — wrote (Haavelmo, 1944, 6): "What is the use of testing, say, the significance of regression coefficients, when maybe, the whole assumption of the linear regression equation is wrong?"

Real-world social systems are usually not governed by stable causal mechanisms or capacities. The kinds of 'laws' and relations that econometrics has established, are laws and relations about entities in models that presuppose causal mechanisms and variables — and the relationship between them — being linear, additive, homogenous, stable, invariant and atomistic. But — when causal mechanisms operate in the real world they only do it in ever-changing and unstable combinations where the whole is more than a mechanical sum of parts. Since statisticians and econometricians — as far as I can see — haven't been able to convincingly warrant their assumptions of homogeneity, stability, invariance, independence, and additivity as being ontologically isomorphic to real-world economic systems, Keynes' critique is still valid. There are strong reasons to remain doubtful of the scientific aspirations of econometrics. Especially when it comes to using econometrics for making causal inferences, it is still often based on counterfactual assumptions that have outrageously weak grounds.

In his critique of Tinbergen, Keynes points us to the fundamental logical, epistemological and ontological problems of applying statistical methods to a basically unpredictable, uncertain, complex, unstable, interdependent, and ever-changing social reality. Methods designed to analyse repeated sampling in controlled experiments under fixed conditions are not easily extended to an organic and non-atomistic world where time and history play decisive roles.

Econometric modelling should never be a substitute for thinking. From that perspective, it is really depressing to see how much of Keynes' critique of the pioneering econometrics in the 1930s-1940s is still relevant today.

Randomization tools

Since a couple of decades, we have seen a new trend in economics, where there is a growing interest in experiments and — not least — how to design them to possibly provide answers to questions about causality and policy effects. Economic research on discrimination nowadays often emphasizes the importance of a randomization design, for example when trying to determine to what extent discrimination can be causally attributed to differences in preferences or information, using so-called correspondence tests and field experiments.

A common starting point is the 'counterfactual approach' developed mainly by Neyman and Rubin, which is often presented and discussed based on examples of randomized control studies, natural experiments, difference in difference, matching, regression discontinuity, etc. Mainstream economists generally view this development of the economics toolbox positively. However, I — like, for example, Nancy Cartwright and Angus Deaton — am not entirely positive about the randomization approach.

A notable limitation of counterfactual randomization designs is that they only give us answers on how 'treatment groups' differ on average from 'control groups.' Let me give an example to illustrate how limiting this fact can be:

> Among school debaters and politicians, it is often claimed that so-called 'independent schools' (charter schools) are better than municipal schools. They are said to lead to better results. To find out if this is really the case, a number of students are randomly selected to take a test. The result could be: Test result = 20 + 5T, where T=1 if the student attends an independent school and T=0 if the student attends a municipal school. This would confirm the assumption that independent school students have an average of 5 points higher results than students in municipal schools. Now, politicians (hopefully) are aware that this statistical result cannot be interpreted in causal terms because independent school students typically do not have the same background (socio-economic, educational, cultural, etc.) as those who attend municipal schools (the relationship between school type and result is confounded by selection bias). To obtain a better measure of the causal effects of school type, politicians suggest that 1000 students be admitted to an independent school through a lottery — a classic example of a randomization design in natural experiments. The chance of winning is 10%, so 100 students are given this opportunity. Of these, 20 accept the offer to attend an independent school. Of the 900 lottery participants who do not 'win,' 100 choose to attend an independent school. The lottery is often perceived by school researchers as an 'instrumental variable,' and when the analysis is carried out, the result is: Test result = 20 + 2T. This is standardly interpreted as having obtained a causal measure of how much better students would, on average, perform on the test if they chose to attend independent schools instead of municipal schools. But is it true? No! If not all school students have exactly the same test results (which is a rather far-fetched 'homogeneity assumption'), the specified average causal effect only applies to the students who choose to attend an independent school if they 'win' the lottery, but who would not otherwise choose to attend an independent school (in statistical jargon, we call these 'compliers'). It is difficult to see why this group of students would be particularly interesting in this example, given that the average causal effect estimated using the instrumental variable says nothing at all about the effect on the majority (the 100 out of 120 who choose to attend an independent school without 'winning' in the lottery) of those who choose to attend an independent school.

Conclusion: Researchers must be much more careful in interpreting 'average estimates' as causal. Reality exhibits a high degree of heterogeneity, and 'average parameters' often tell us very little!

To randomize ideally means that we achieve orthogonality (independence) in our models. But it does not mean that in real experiments when we randomize, we achieve this ideal. The 'balance' that randomization should ideally result in cannot be taken for granted when the ideal is translated into reality. Here, one must argue and verify that the 'assignment mechanism' is truly stochastic and that 'balance' has indeed been achieved!

Even if we accept the limitation of only being able to say something about average treatment effects there is another theoretical problem. An ideal randomized experiment assumes that a number of individuals are first chosen from a randomly selected population and then randomly assigned to a treatment group or a control group. Given that both selection and assignment are successfully carried out randomly, it can be shown that the expected outcome difference between the two groups is the average causal effect in the population. The hitch is that the experiments conducted almost never involve participants selected from a random population! In most cases, experiments are started because there is a problem of some kind in a given population (e.g., schoolchildren or job seekers in country X) that one wants to address. An ideal randomized experiment assumes that *both* selection and assignment are randomized — this means that virtually none of the empirical results that randomization advocates so eagerly tout hold up in a strict mathematical-statistical sense. The fact that only assignment is talked about when it comes to 'as if' randomization in natural experiments is hardly a coincidence. Moreover, when it comes to 'as if' randomization in natural experiments, the sad but inevitable fact is that there can always be a dependency between the variables being studied and unobservable factors in the error term, which can never be tested!

Another significant and major problem is that researchers who use these randomization-based research strategies often, in order to achieve 'exact' and 'precise' results, set up problem formulations that are not at all the ones we really want answers to. Design becomes the main thing, and as long as one can get more or less clever experiments in place, they believe they can draw far-reaching conclusions about both causality and the ability to generalize experimental outcomes to larger populations. Unfortunately, this often means that this type of research has a negative bias away from interesting and important problems towards prioritizing method selection. Design and research planning are important, but the credibility of research ultimately lies in being able to provide answers to relevant questions that both citizens and researchers want answers to.

Believing there is only one really good evidence-based method on the market — and that randomization is the only way to achieve scientific validity — blinds people to searching for and using other methods that in many contexts are better. Insisting on using only one tool often means using the wrong tool.

The 'true model' assumption

Most work in econometrics is — still — made on the assumption that the researcher has a theoretical model that is 'true.' Based on this belief of having a correct specification for an econometric model or running a regression, one proceeds as if the only problem remaining to solve has to do with measurement and observation.

When things sound too good to be true, they usually aren't. And that goes for econometric wet dreams too. The snag is that there is pretty little to support the perfect specification assumption. Looking around in social science and economics we don't find a single regression or econometric model that lives up to the standards set by the 'true' theoretical model — and there is almost nothing that gives us reason to believe things will be different in the future.

To think that we are being able to construct a model where all relevant variables are included and correctly specify the functional relationships that exist between them is not only a belief without support but a belief impossible to support.

The theories we work with when building our econometric regression models are insufficient. No matter what we study, there are always some variables missing, and we don't know the correct way to functionally specify the relationships between the variables.

Every regression model constructed is mis-specified. There is always an endless list of possible variables to include, and endless possible ways to specify the relationships between them. So every applied econometrician comes up with his own specification and 'parameter' estimates. The econometric Holy Grail of consistent and stable parameter values is nothing but a dream.

A rigorous application of econometric methods in economics really presupposes that the phenomena of our real-world economies are ruled by stable causal relations between variables. Parameter values estimated in specific spatio-temporal contexts are presupposed to be exportable to different contexts. To warrant this assumption one, however, has to convincingly establish that the targeted acting causes are stable and invariant so that they maintain their parametric status after the bridging. The endemic lack of predictive success of the econometric project indicates that this hope of finding fixed parameters is a hope for which there really is no other ground than hope itself.

The theoretical conditions that must be fulfilled for regression analysis and econometrics to really work are nowhere even closely met in reality. Making outlandish statistical assumptions does not provide a solid ground for doing relevant social science and economics. Although regression analysis and econometrics have become the most used quantitative methods in social sciences and economics today, it's still a fact that almost all of the inferences made from them are invalid.

Given the usual set of assumptions (such as manipulability, transitivity, separability, additivity, linearity, etc.) econometrics delivers deductive inferences. But the problem, of course, is that we will never completely know when the assumptions are right. Conclusions can only be as certain as their premises — and that also applies to econometrics.

Although 'ideally controlled experiments' may tell us with certainty what causes what effects, this is so only when given the right 'closures.' Making appropriate extrapolations from (ideal, accidental, natural or quasi) experiments to different settings, populations or target systems, is not easy. "It works there' is no evidence for "it will work here.' The causal background assumptions made must be justified, and without licenses to export, the value of 'rigorous' and 'precise' methods used when analyzing 'natural experiments' is often despairingly small. Just to take one example — since the core assumptions on which instrumental variables analysis builds are *never* directly testable, those of us who choose to use instrumental variables to find out about causality *always* have to defend and argue for the validity of the assumptions the causal inferences build on. Especially when dealing with natural experiments, we should be very cautious when being presented with causal conclusions without convincing arguments about the veracity of the assumptions made. If you are out to make causal inferences, you have to rely on a trustworthy theory of the data-generating process. The empirical results causal analysis supplies us with are only as good as the assumptions we make about the data-generating process. Garbage in, garbage out.

Non-manipulability and the limits of potential outcome models

Framing all causal questions as questions of manipulation or intervention runs into many problems, especially when we open up for 'hypothetical' and 'symbolic' interventions. Humans have few barriers to imagining things, but that often also makes it difficult to value the proposed thought experiments in terms of relevance. Performing 'well-defined' interventions is one thing, but if we do not want to give up searching for answers to the questions we are interested in and instead only search for answerable questions, interventionist studies are of limited applicability and value. Intervention effects in thought experiments are not self-evidently the causal effects we are looking for. Identifying causes (reverse causality) and measuring effects of causes (forward causality) is not the same. In social sciences, like economics, we standardly first try to identify the problem and why it occurred, and then afterwards look at the effects of the causes.

Leaning on the interventionist approach often means that instead of posing interesting questions on a social level, focus is on individuals (cf. Goldthorpe 2001). Instead of asking about structural socio-economic factors behind, e.g., gender or racial discrimination, the focus is on the choices individuals make (which also tends to make the explanations presented inadequately 'deep'). Within the manipulation approach to causality you are only allowed to ask certain rather restricted causal questions. A typical example of the dangers of this limiting approach is 'Nobel prize' winner Esther Duflo, who thinks that economics should be based on evidence from randomised experiments and field studies. Duflo *et consortes* want to give up on 'big ideas' like political economy and institutional reform and instead go for solving more manageable problems the way plumbers do (cf. Duflo 2017). Yours truly is far from sure that is the right way to move economics forward and make it a relevant and realist science. A plumber can fix minor leaks in your system, but if the whole system is rotten, something more than good old fashion plumbing is needed. The big social and economic problems we face today are not going to be solved by plumbers performing interventions or manipulations in the form of randomised control trials.

Although, of course, it is possible (more or less, depending on context) to retrofit causal questions into a manipulation/intervention framework, before we are there, we have to agree on having identified the causal problem we try to deal with when recommending different policies. Before we can calculate causal effects we have to identify the causes, and this is perhaps not always best done within a manipulation/intervention framework. One problem is that the manipulation/intervention approach in broader social and economic contexts requires a reframing of the questions we pose, which often means that we get 'well-defined' causal answers, but not necessarily answers to the questions we really are interested in finding answers to. The manipulation/intervention framework is one way to do causal analysis. But it is not the way to do it. Being an advocate of 'inference to the best explanation' I think we also have to more carefully consider explanatory considerations when estimating and identifying causal relations.

A popular idea in quantitative social sciences is to think of a cause (C) as something that increases the probability of its effect or outcome (O). That is:

$$P(O|C) > P(O|-C).$$

However, as is also well-known, a correlation between two variables, say A and B, does not necessarily imply that that one is a cause of the other, or the other way around, since they may both be an effect of a common cause, C.

In statistics and econometrics, we usually solve this "confounder' problem by 'controlling for' C, i. e. by holding C fixed. This means that we actually look at different "populations' – those in which C occurs in every case, and those in which C doesn't occur at all. This means that knowing the value of A does not influence the probability of C [$P(C|A) = P(C)$]. So, if there then still exists a correlation between A and B in either of these populations, there has to be some other cause operating. But if all other possible causes have been 'controlled for' too, and there is still a correlation between A and B, we may safely conclude that A is a cause of B, since by 'controlling for' all other possible causes, the correlation between the putative cause A and all the other possible causes (D, E,. F …) is broken.

This is of course a very demanding prerequisite, since we may never actually be sure to have identified all putative causes. Even in scientific experiments, the number of uncontrolled causes may be innumerable. Since nothing less will do, we do all understand how hard it is to actually get from correlation to causality. This also means that only relying on statistics or econometrics is not enough to deduce causes from correlations.

Some people think that randomization may solve the empirical problem. By randomizing we are getting different 'populations' that are homogeneous in regards to all variables except the one we think is a genuine cause. In that way, we are supposed to be able not to actually have to know what all these other factors are.

If you succeed in performing an ideal randomization with different treatment groups and control groups that is attainable. But it presupposes that you really have been able to establish – and not just assume – that the probability of all other causes but the putative (A) have the same probability distribution in the treatment and control groups, and that the probability of assignment to treatment or control groups are independent of all other possible causal variables.

Unfortunately, *real* experiments and *real* randomizations seldom or never achieve this. So, yes, we may do without knowing all causes, but it takes ideal experiments and ideal randomizations to do that, not real ones. That means that in practice we do have to have sufficient background knowledge to deduce causal knowledge. Without old knowledge, we can't get new knowledge. No causes in, no causes out.

Once you include all actual causes into the original (over)simplified model, it may well be that the causes are no longer independent or linear and that *a fortiori* the coefficients in the econometric equations no longer are identifiable. And so, since all causal factors are not included in the original econometric model, it is not an adequate representation of the real causal structure of the economy that the model is purportedly meant to represent.

Econometrics is basically a deductive method. Given the assumptions (such as manipulability, transitivity, exchangeability, monotonicity, ignorability, Reichenbach probability principles, separability, additivity, linearity etc) it delivers deductive inferences. The problem, of course, is that we will never completely know when the assumptions are right. Real target systems are seldom epistemically isomorphic to axiomatic-deductive models/systems, and even if they were, we still have to argue for the external validity of the conclusions reached from within these epistemically convenient models/systems. Causal evidence generated by statistical/econometric procedures may be valid in 'closed' models, but what we usually are interested in, is causal evidence in the real target system we happen to live in.

Guy Orcutt once – according to (Leamer 1983, 31) -- said that "doing econometrics is like trying to learn the laws of electricity by playing the radio" Advocates of econometrics want to have deductively automated answers to fundamental causal questions -- but to apply 'thin' methods we have to have 'thick' background knowledge of what's going on in the real world, and not in idealized models.

Econometric forecasting

As Oskar Morgenstern noted in his classic *Wirtschaftsprognose: Eine Untersuchung ihrer Voraussetzungen und Möglichkeiten*, (Morgenstern, 1928) economic predictions and forecasts amount to little more than intelligent guessing. Making forecasts and predictions obviously isn't a trivial or costless activity, so why then go on with it?

The problems that economists encounter when trying to predict the future really underline how important it is for social sciences to incorporate Keynes's far-reaching and incisive analysis of induction and evidential weight in his seminal *Treatise on Probability* (Keynes, 1921).

According to Keynes, we live in a world permeated by unmeasurable uncertainty – not quantifiable stochastic risk – which often forces us to make decisions based on anything but 'rational expectations.' Keynes rather thinks that we base our expectations on the confidence or 'weight' we put on different events and alternatives. To Keynes, expectations are a question of weighing probabilities by 'degrees of belief,' beliefs that often have preciously little to do with the kind of stochastic probabilistic calculations made by the rational agents as modelled by 'modern' social sciences. And often we "simply do not know.'

How strange that social scientists and mainstream economists, as a rule, do not even touch upon these aspects of scientific methodology that seem to be so fundamental and important for anyone trying to understand how we learn and orient ourselves in an uncertain world. An educated guess on why this is a fact would be that Keynes's concepts are not possible to squeeze into a single calculable numerical 'probability.' In the quest for measurable quantities, one puts a blind eye to qualities and looks the other way.

So why do companies, governments, and central banks, continue with this more or less expensive, but obviously worthless, activity?

A part of the answer concerns ideology and apologetics. Forecasting is a non-negligible part of the labour market for (mainstream) economists, and so, of course, those in the business do not want to admit that they are occupied with worthless things (not to mention how hard it would be to sell the product with that kind of frank truthfulness). Governments, the finance sector and (central) banks also want to give the impression to customers and voters that they, so to say, have the situation under control (telling people that next year x will be 3.048 % makes wonders in that respect). Why else would anyone want to pay them or vote for them? These are sure not glamorous aspects of economics as a science, but as a scientist, it would be unforgivably dishonest to pretend that economics doesn't also perform an ideological function in society.

Econometric testing

Debating econometrics and its shortcomings one often gets the response from econometricians that "ok, maybe econometrics isn't perfect, but you have to admit that it is a great technique for empirical testing of economic hypotheses." But is econometrics — really — such a great testing instrument?

Econometrics is supposed to be able to test economic theories. But to serve as a testing device you have to make many assumptions, many of which themselves cannot be tested or verified. To make things worse, there are also only rarely strong and reliable ways of telling us which set of assumptions is to be preferred. Trying to test and infer causality from (non-experimental) data you have to rely on assumptions such as disturbance terms being 'independent and identically distributed'; functions being additive, linear, and with constant coefficients; parameters being' 'invariant under intervention; variables being 'exogenous', 'identifiable', 'structural and so on. Unfortunately, we are seldom or never informed of where that kind of 'knowledge' comes from, beyond referring to the economic theory that one is supposed to test. Performing technical tests is of course needed, but perhaps even more important is to know — as David Colander (2019, 340) puts it — "how to deal with situations where the assumptions of the tests do not fit the data."

That leaves us in the awkward position of having to admit that if the assumptions made do not hold, the inferences, conclusions, and testing outcomes econometricians come up with simply do not follow from the data and statistics they use.

The central question is 'how do we learn from empirical data?' Testing statistical/econometric models is one way, but we have to remember that the value of testing hinges on our ability to validate the — often unarticulated technical — basic assumptions on which the testing models build. If the model is wrong, the test apparatus simply gives us fictional values. There is always a strong risk that one puts a blind eye to some of those non-fulfilled technical assumptions that actually make the testing results — and the inferences we build on them — unwarranted.

Haavelmo's probabilistic revolution gave econometricians their basic framework for testing economic hypotheses. It still builds on the assumption that the hypotheses can be treated as hypotheses about (joint) probability distributions and that economic variables can be treated as if pulled out of an urn as a random sample. But as far as I can see economic variables are nothing of that kind.

It is still difficult to find any hard evidence that econometric testing uniquely has been able to 'exclude a theory'. As Renzo Orsi (1993, 365) put it: "If one judges the success of the discipline on the basis of its capability of eliminating invalid theories, econometrics has not been very successful."

The econometric illusion

The processes that generate socio-economic data in the real world cannot just be assumed to always be adequately captured by a probability measure. And, so, it cannot be maintained that it even should be mandatory to treat observations and data — whether cross-section, time series or panel data — as events generated by some probability model. The important activities of most economic agents do not usually include throwing dice or spinning roulette wheels. Data-generating processes — at least outside of nomological machines like dice and roulette wheels — are not self-evidently best modelled with probability measures.

When economists and econometricians — often uncritically and without arguments — simply assume that one can apply probability distributions from statistical theory to their own area of research, they are skating on thin ice. If you cannot show that data satisfies all the conditions of the probabilistic 'nomological machine,' then the statistical inferences made in mainstream economics lack sound foundations. The rigour and precision sought with mathematical statistical instruments has a devastatingly important trade-off: the higher the level of rigour and precision, the smaller the range of real-world application.

Statistical — and econometric — patterns should never be seen as anything other than possible clues to follow. Behind observable data, there are real structures and mechanisms operating, things that are — if we really want to understand, explain and (possibly) predict things in the real world — more important to get hold of than to simply correlate and regress observable variables.

Using formal mathematical modelling, mainstream economists sure can guarantee that the conclusions hold given the assumptions. However, the validity we get in abstract model worlds does not warrant transfer to real-world economies. Validity may be good, but it is not enough.

Mainstream economists are proud of having an ever-growing smorgasbord of models to cherry-pick from (as long as, of course, the models do not question the standard modelling strategy) when performing their analyses. The 'rigorous' and 'precise' deductions made in these closed models, however, are not in any way matched by a similar stringency or precision when it comes to what ought to be the most important stage of any economic research — making statements and explaining things in real economies. Although almost every mainstream economist holds the view that thought-experimental modelling has to be followed by confronting the models with reality — which is what they indirectly want to predict/explain/understand using their models — they then all of a sudden become exceedingly vague and imprecise. It is as if all the intellectual force has been invested in the modelling stage and nothing is left for what really matters — what exactly do these models teach us about real economies.

No matter how precise and rigorous the analysis, and no matter how hard one tries to cast the argument in modern mathematical form, they do not push economic science forwards one single iota if they do not stand the acid test of relevance to the target. Proving things 'rigorously' in mathematical models is not a good recipe for doing an interesting and relevant economic analysis. Forgetting to supply export warrants to the real world makes the analysis an empty exercise in formalism without real scientific value. In the realm of true science, it is of little or no value to simply make claims about a model and lose sight of reality.

To have valid evidence is not enough. What economics needs is sound evidence. The premises of a valid argument do not have to be true, but a sound argument, on the other hand, is not only valid but builds on premises that are true. Aiming only for validity, without soundness, is setting the economics aspiration level too low for developing a realist and relevant science.

Conclusion

Contemporary economics -- still -- focuses on studying what happens in abstract and unrealistic models. A deeper study of underlying causal mechanisms in the economy could make economic science more realistic. However, when faced with the monumental gap between empirical data and models, mainstream economists often resort to one of their four favorite strategies to immunize the models against facts:

1. Treat the model as an axiomatic system, which necessarily gives rise to pure logical tautologies.
2. Use unspecified *ceteris paribus* assumptions, which give each model assertion an unlimited 'alibi'.
3. Limit the applicability of the model to spatio-temporal problems where the assumptions/axioms are valid.
4. Leave the application of the model open-ended, making empirical falsification of the model impossible.

From a scientific standpoint, the value of these types of rescue actions is equal to zero. The real challenge for an economic science worth its name should be to confront reality as it is, instead of conjuring away all kinds of 'problems' through the formulation of models and theories that are nothing more than sterile deductive-axiomatic systems.

Science should not be reduced to substanceless fictional storytelling. This has been going on for far too long in economics.

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