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When the model becomes the message – a critique of Rodrik

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Introduction

Economics is perhaps more than any other social science model-oriented (see Morgan, 2012; Arnsperger and Varoufakis, 2006). There are many reasons for this – the history of the discipline, having ideals coming from the natural sciences (especially physics), the search for universality (explaining as much as possible with as little as possible), rigour, precision, etc.

Many mainstream economists want to explain social phenomena, structures and patterns, based on the assumption that the agents are acting in an optimizing (rational) way to satisfy given, stable and well-defined goals.

The procedure is analytical. The whole is broken down into its constituent parts so as to be able to explain (reduce) the aggregate (macro) as the result of interaction of its parts (micro).

Building their economic models, modern mainstream (neoclassical) economists ground their models on a set of **core assumptions (CA)** – describing the agents as "rational" actors – and a set of **auxiliary assumptions (AA).** Together **CA** and **AA** make up what I will call the **ur-model (M)** of all mainstream neoclassical economic models. Based on these two sets of assumptions, they try to explain and predict both individual (micro) and – most importantly – social phenomena (macro).

The core assumptions typically consist of:

> CA_1 *Completeness* – rational actors are able to compare different alternatives and decide which one(s) he prefers
>
> CA_2 *Transitivity* – if the actor prefers A to B, and B to C, he must also prefer A to C.
>
> CA_3 *Non-satiation* – more is preferred to less.
>
> CA_4 *Maximizing expected utility* – in choice situations under risk (calculable uncertainty) the actor maximizes expected utility.
>
> CA_5 *Consistent efficiency equilibria* – the actions of different individuals are consistent, and the interaction between them result in an equilibrium.

When describing the actors as rational in these models, the concept of rationality used is *instrumental rationality* – choosing *consistently* the preferred alternative, which is judged to have the best consequences for the actor *given* his in the model exogenously given wishes/interests/goals. How these preferences/wishes/interests/goals are formed is not considered to be within the realm of rationality, and *a fortiori* not constituting part of economics proper.

The picture given by this set of core assumptions (rational choice) is a rational agent with strong cognitive capacity that knows what alternatives he is facing, evaluates them carefully, calculates the consequences and chooses the one – given his preferences – that he believes has the best consequences according to him.

Weighing the different alternatives against each other, the actor makes a consistent optimizing (typically described as maximizing some kind of utility function) choice, and acts accordingly.

Beside the core assumptions (**CA**) the model also typically has a set of auxiliary assumptions (**AA**) spatio-temporally specifying the kind of social interaction between "rational actors" that take place in the model. These assumptions can be seen as giving answers to questions such as:

AA_1 *who* are the actors and *where* and *when* do they act

AA_2 which *specific* goals do they have

AA_3 what are their interests

AA_4 what kind of expectations do they have

AA_5 what are their feasible actions

AA_6 what kind of agreements (contracts) can they enter into

AA_7 how much and what kind of information do they possess

AA_8 how do the actions of the different individuals/agents interact with each other.

So, the ur-model of all economic models basically consist of a *general* specification of what (axiomatically) constitutes optimizing rational agents and a more *specific* description of the kind of situations in which these rational actors act (making **AA** serve as a kind of specification/restriction of the intended domain of application for **CA** and its deductively derived theorems). The list of assumptions can never be complete, since there will always be unspecified background assumptions and some (often) silent omissions (like closure, transaction costs, etc., regularly based on some negligibility and applicability considerations). The hope, however, is that the "thin" list of assumptions shall be sufficient to explain and predict "thick" phenomena in the real, complex, world.

These economic models are not primarily constructed for being able to analyze *individuals* and their aspirations, motivations, interests, etc., but typically for analyzing *social* phenomena as a kind of equilibrium that emerges through the interaction between individuals. Employing a reductionist-individualist methodological approach, macroeconomic phenomena are, analytically, given *microfoundations*.

Now, of course, no one takes the ur-model (and those models that build on it) as a good (or, even less, true) representation of economic reality (which would demand a high degree of appropriate conformity with the essential characteristics of the real phenomena, that, even when weighing inn pragmatic aspects such as "purpose" and "adequacy", it is hard to see that this "thin" model could deliver). The model is typically seen as a kind of "thought-experimental" bench-mark device for enabling a rigorous mathematically tractable illustration of how an ideal market economy functions, and to be able to compare that "ideal" with reality. The model is supposed to supply us with analytical and explanatory power, enabling us to detect, describe and understand mechanisms and tendencies in what happens around us in real economies.

Based on the model – and on interpreting it as something more than a deductive-axiomatic system – predictions and explanations can be made and confronted with empirical data and what we think we know. If the discrepancy between model and reality is too large – "falsifying" the hypotheses generated by the model – the thought is that the modeler through "successive approximations" improves on the explanatory and predictive capacity of the model.

When applying their preferred deductivist thinking in economics, mainstream neoclassical economists usually use this ur-model and its more or less tightly knit axiomatic core assumptions to set up further "as if" models from which consistent and precise inferences are made. The beauty of this procedure is of course that *if* the axiomatic premises are true, the conclusions necessarily follow. The snag is that if the models are to be *relevant*, we also have to argue that their precision and rigour still holds when they are applied to real-world situations. They often don't. When addressing real economies, the idealizations and abstractions necessary for the deductivist machinery to work simply don't hold.

If the real world is fuzzy, vague and indeterminate, then why should our models build upon a desire to describe it as precise and predictable? The logic of idealization, that permeates the ur-model, is a marvellous tool in mathematics and axiomatic-deductivist systems, but, a poor guide for action in real-world systems, in which concepts and entities are without clear boundaries and continually interact and overlap.

Being told that the model is rigorous and amenable to "successive approximations" to reality is of little avail, especially when the law-like (nomological) core assumptions are highly questionable and extremely difficult to test. Being able to construct "thought-experiments" depicting logical possibilities doesn't – really – take us very far. An obvious problem with the mainstream neoclassical ur-model – formulated in such a way that *realiter* is extremely difficult to empirically test and decisively "corroborate" or "falsify". Such models are from a scientific-explanatory point of view unsatisfying. The "thinness" is bought at too high a price, unless you decide to leave the intended area of application unspecified or immunize your model by interpreting it as nothing more than two sets of core and auxiliary assumptions making up a content-less theoretical system with no connection whatsoever to reality.

Seen from a deductive-nomological perspective, the ur-model (**M**) consist of, as we have seen, a set of more or less general (typically universal) law-like hypotheses (**CA**) and a set of (typically spatio-temporal) auxiliary conditions (**AA**). The auxiliary assumptions give "boundary" descriptions such that it is possible to deduce logically (meeting the standard of validity) a conclusion (explanandum) from the premises **CA** and **AA**. Using this kind of model economists can be portrayed as trying to explain/predict facts by subsuming them under **CA** given **AA**.

This account of theories, models, explanations and predictions does not – of course – give a realistic account of actual scientific practices, but rather aspires to give an idealized account of them.

An obvious problem with the formal-logical requirements of what counts as **CA** is the often severely restricted reach of the "law". In the worst case it may not be applicable to any real, empirical, relevant situation at all. And if **AA** is not "true", then **M** doesn't really explain

(although it may predict) at all. Deductive arguments should be sound – valid and with true premises – so that we are assured of having true conclusions. Constructing models assuming "rational" expectations, says nothing of situations where expectations are "non-rational".
Most mainstream economic models – elaborations on the ur-model – are abstract, unrealistic and presenting mostly non-testable hypotheses. How then are they supposed to tell us anything about the world we live in?

And where does the drive to build those kinds of models come from?

I think one important rational behind this kind of model building is the quest for rigour, and more precisely, *logical* rigour. Formalization of economics has been going on for more than a century and with time it has become obvious that the preferred kind of formalization is the one that rigorously follows the rules of formal logic. As in mathematics, this has gone hand in hand with a growing emphasis on axiomatics. Instead of basically trying to establish a connection between empirical data and assumptions, "truth" has come to be reduced to, a question of fulfilling internal consistency demands between conclusion and premises, instead of showing a "congruence" between model assumptions and reality. This has, of course, severely restricted the applicability of economic theory and models.

Not all mainstream economists subscribe to this rather *outré* deductive-axiomatic view of modeling, and so when confronted with the massive empirical refutations of almost every theory and model they have set up, many mainstream economists react by saying that these refutations only hit **AA** (the Lakatosian "protective belt"), and that by "successive approximations" it is possible to make the theories and models less abstract and more realistic, and – eventually – more readily testable and predictably accurate. Even if **CA** & $\mathbf{AA}_1$ doesn't have much of empirical content, if by successive approximation we reach, say, **CA** & $\mathbf{AA}_{25}$, we are to believe that we can finally reach robust and true predictions and explanations. But there are grave problems with this modeling view, too. The tendency for modelers to use the method of successive approximations as a kind of "immunization", implies that it is taken for granted that there can never be any faults with **CA**. Explanatory and predictive failures hinge solely on **AA**. That the **CA** used by mainstream economics should all be held non-defeasibly corrobated, seems, however – to say the least – rather unwarranted.

Confronted with the empirical failures of their models and theories, even these mainstream economists often retreat into looking upon their models and theories as some kind of "conceptual exploration", and give up any hopes/pretenses whatsoever of relating their theories and models to the real world. Instead of trying to bridge the gap between models and the world, one decides to look the other way. But restricting the analytical activity to examining and making inferences in the models is tantamount to treating the models as self-contained *substitute systems*, rather than as *surrogate systems* that the modeler uses to indirectly understand or explain the real target system.

Trying to develop a science where we want to be better equipped to explain and understand real societies and economies, it surely can't be enough to prove or deduce things in model worlds. If theories and models do not – directly or indirectly – tell us anything of the world we live in, then why should we waste time on them?

The economics rules

Dani Rodrik's *Economics Rules* (Oxford University Press, 2015) is one of those rare examples where a mainstream economist – instead of just looking the other way – takes his time to ponder on the tough and deep science-theoretic and methodological questions that underpin the economics discipline.

There's much in the book I like and appreciate, but there is also a very disturbing apologetic tendency to blame all of the shortcomings on the economists and depicting economics itself as a problem-free smorgasbord collection of models. If you just choose the appropriate model from the immense and varied smorgasbord there's no problem. It is as if all problems in economics were conjured away if only we could make the proper model selection. To Rodrik the problem is always the economists, never economics itself. I sure wish it was that simple, but having written more than ten books on the history and methodology of economics, and having spent almost forty years among them "econs", I have to confess I don't quite recognize the picture.

A smorgasbord of thought experiments

Rodrik's describes economics as a more or less problem-free smorgasbord collection of models. Economics is portrayed as advancing through a judicious selection from a continually expanding library of models, models that are presented as "partial maps" or "simplifications designed to show how specific mechanisms work".

But one of the things that's missing in Rodrik's view of economic models is the all-important distinction between core and auxiliary assumptions (on the importance on this distinction, cf. Max Albert (1994) and Hans Albert (2012[1963])). Although Rodrik repeatedly speaks of "unrealistic" or "critical" assumptions, he basically just lumps them all together without differentiating between different types of assumptions, axioms or theorems. In a typical passage, Rodrik writes (2015:25):

> "Consumers are hyperrational, they are selfish, they always prefer more consumption to less, and they have a long time horizon, stretching into infinity. Economic models are typically assembled out of many such unrealistic assumptions. To be sure, many models are more realistic in one or more of these dimensions. But even in these more layered guises, other unrealistic assumptions can creep in somewhere else."

In Rodrik's model depiction we are essentially given the following structure,

$$\frac{A_1, A_2, \ldots A_n}{\text{Theorem,}}$$

where a set of undifferentiated assumptions are used to infer a theorem.

This is, however, to vague and imprecise to be helpful, and does not give a true picture of the usual mainstream modeling strategy, where there's a differentiation between a set of law-like hypotheses (CA) and a set of auxiliary assumptions (AA), giving the more adequate structure

$$\frac{CA_1, CA_2, \ldots CA_n \ \& \ AA_1, AA_2, \ldots AA_n}{\text{Theorem}}$$

or,

$$\frac{CA_1, CA_2, \ldots CA_n}{(AA_1, AA_2, \ldots AA_n) \rightarrow \text{Theorem},}$$

more clearly underlining the function of AA as a set of (empirical, spatio-temporal) restrictions on the applicability of the deduced theorems.

This underlines the fact that specification of AA restricts the range of applicability of the deduced theorem. In the extreme cases we get

$$\frac{CA_1, CA_2, \ldots CA_n}{\text{Theorem},}$$

where the deduced theorems are analytical entities with universal and totally unrestricted applicability, or

$$\frac{AA_1, AA_2, \ldots AA_n}{\text{Theorem},}$$

where the deduced theorem is transformed into an untestable tautological thought-experiment without any empirical commitment whatsoever beyond telling a coherent fictitious as-if story.

Not clearly differentiating between CA and AA means that Rodrik can't make this all-important interpretative distinction, and so without warrant is able to "save" or "immunize" models from almost any kind of critique by simple equivocation between interpreting models as empirically empty and purely deductive-axiomatic analytical systems, or, respectively, as models with explicit empirical aspirations. Flexibility is usually something people deem positive, but in this methodological context it's more troublesome than a sign of real strength. Models that are compatible with everything, or come with unspecified domains of application, are worthless from a scientific point of view.

Pseudo-pluralism

The proliferation of economic models during the last twenty-thirty years is presented by Rodrik (2015:8-17) as a sign of great diversity and abundance of new ideas:

> "Rather than a single, specific model, economics encompasses a collection of models … Economics is in fact, a collection of diverse models that do not have a particular ideological bent or lead to a unique conclusion …

> The possibilities of social life are too diverse to be squeezed into unique frameworks. But each economic model is like a partial map that illuminates a fragment of the terrain …
>
> Different contexts … require different models … When models are selected judiciously, they are a source of illumination …
>
> The correct answer to almost any question in economics is: It depends. Different models, each equally respectable, provide different answers."

But, again, it's not, really, that simple.

Just as Colander, Holt, and Rosser (2004) argued, Rodrik also wants to propogate the view that mainstream economics is an open and pluralistic "let one hundred flowers bloom" science.

But in reality it is rather "*plus ça change, plus c'est la même chose*".

Why? Because almost all the change and diversity that Rodrik applauds only takes place within the analytic-formalistic modeling strategy that makes up the core of mainstream economics. All the flowers that do not live up to the precepts of the mainstream methodological canon are pruned. You're free to take your analytical formalist models and apply it to whatever you want – as long as you do it (Colander 2004:492) "with a careful understanding of the strengths of the recent orthodox approach and with a modeling methodology acceptable to the mainstream." If you do not follow this particular mathematical-deductive analytical formalism you're not even considered doing economics. "If it isn't modeled, it isn't economics." This isn't pluralism. It's a methodological reductionist straightjacket.

So, even though we have seen a proliferation of models, it has almost exclusively taken place as a kind of axiomatic variation -- where the core assumptions (CA) are usually untouched -- within the standard "ur-model", which is always (following an unwritten, but impregnable rule) used as a self-evident bench-mark. Seen from the perspective presented here, that is actually just another variant of theory immunization. When the preferred axiomatic specification fails (we obviously don't have a case of perfect competition (auxiliary assumption AAi)) – just switch from AA_i to AA_j (e. g. monopolistic competition).

In Rodrik's (2015:71) world, "newer generations of models do not render the older generations wrong or less relevant," but "simply expand the range of the discipline's insights". I don't want to sound derisory or patronizing, but although it's easy to say what Rodrik says, we cannot have our cake and eat it. Analytical formalism doesn't save us from either specifying the intended areas of application of the models, or having to accept them as rival models facing the risk of being put to the test and found falsified.

The insistence on using analytical formalism and mathematical methods comes at a high cost – it often makes the analysis irrelevant from an empirical-realist point of view.

Applying closed analytical-formalist-mathematical-deductivist-axiomatic models, built on atomistic-reductionist assumptions to a world assumed to consist of atomistic-isolated entities, is a sure recipe for failure when the real world is known to be an open system where complex and relational structures and agents interact. Validly deducing things in models of that kind doesn't much help us understanding or explaining what is taking place in the real world we happen to live in. Validly deducing things from patently unreal assumptions -- that we all know are purely fictional -- makes most of the modeling exercises pursued by mainstream economists rather pointless. It's simply not the stuff that real understanding and explanation in science is made of. Had Rodrik not been so in love with his smorgasbord of models, he would have perceived this too. Just telling us that the plethora of models that make up modern economics "expand the range of the discipline's insights" is nothing short of hand waving.

No matter how many thousands of models mainstream economists come up with, as long as they are just axiomatic variations of the same old mathematical-deductive ilk, they will not take us one single inch closer to giving us relevant and usable means to further our understanding and explanation of real economies.

Non-transparent user's guides to models

Rodrik (2015:73) argues that "the multiplicity of models is economics' strength", and that a science that has a different model for everything is non-problematic, since

> "…economic models are cases that come with explicit user's guides -- teaching notes on how to apply them. That's because they are transparent about their critical assumptions and behavioral mechanisms."

That is, however, very much at odds with many economists experience from studying mainstream economic models during the last decades.

When, e. g., criticizing the basic (DSGE) workhorse macroeconomic model for its inability to explain involuntary unemployment, its defenders maintain that later "successive approximations" and elaborations – especially newer search models – manage to do just that. However, one of the more conspicuous problems with those "solutions", is that they – as e.g. Pissarides (1992) "Loss of Skill during Unemployment and the Persistence of Unemployment Shocks" – are as a rule constructed without seriously trying to warrant that the model immanent assumptions and results are applicable in the real world. External validity is more or less a non-existent *problematique*, sacrificed on the altar of model derivations. This is not by chance. These theories and models do not come at all with the transparent and "explicit user's guides" that Rodrik maintains they do. And there's a very obvious reason for that. For how could one even imagine to empirically test assumptions such as Pissarides "model 1" assumptions of reality being adequately represented by "two overlapping generations of fixed size", "wages determined by Nash bargaining", "actors maximizing expected utility", "endogenous job openings", "jobmatching describable by a probability distribution," without coming to the conclusion that this is – in terms of realism and relevance – far from "good enough" or "close enough" to real world situations?

It's difficult to see how those typical mainstream neoclassical modeling assumptions in any possibly relevant way – with or without due pragmatic considerations – can be considered anything else but imagined model worlds assumptions that has nothing at all to do with the real world we happen to live in! There is no real transparency as to the deeper significance and role of the chosen set of axiomatic assumptions. There is no explicit user's guide or indication of how we should be able to, as Rodrik puts it, "discriminate" between the "bewildering array of possibilities" that flow out of such outlandish and *known to be false* assumptions. Theoretical models building on piles of *known to be false* assumptions are in no way close to being scientific explanations. On the contrary. They are untestable and *a fortiori* totally worthless from the point of view of scientific relevance.

On maths and models

To Rodrik an economic model basically consists of "clearly stated assumptions and behavioral mechanisms" that easily lend themselves to mathematical treatment. Furthermore, Rodrik (2015:31-32) thinks that the usual critique against the use of mathematics in economics is wrong-headed. Math only plays an instrumental role in economic models:[1]

> "First, math ensures that the elements of a model ... are stated clearly and are transparent… The second virtue of mathematics is that it ensures the internal consistency of a model – simply put, that the conclusions follow from the assumptions."

What is lacking in this overly simplistic view on using mathematical modeling in economics is an ontological reflection on the conditions that have to be fulfilled for appropriately applying the methods of mathematical modeling.

Using formal mathematical modeling, mainstream economists like Rodrik sure can guarantee that the conclusion holds given the assumptions. However, there is no warrant that the validity we get in abstract model worlds automatically transfer to real world economies. Validity and consistency may be good, but it isn't enough. From a realist perspective both relevance and soundness are *sine qua non*.

In their search for validity, rigour and precision, mainstream macro modelers of various ilks construct microfounded DSGE models that standardly assume rational expectations, Walrasian market clearing, unique equilibria, time invariance, linear separability and homogeneity of both inputs/outputs and technology, infinitely lived intertemporally optimizing representative household/ consumer/producer agents with homothetic and identical preferences, etc., etc. At the same time the models standardly ignore complexity, diversity, uncertainty, coordination problems, non-market clearing prices, real aggregation problems, emergence, expectations formation, etc., etc.

Behavioural and experimental economics – not to speak of psychology – show beyond any doubts that "deep parameters" – peoples' preferences, choices and forecasts – are regularly influenced by those of other participants in the economy. And how about the homogeneity assumption? And if all actors are the same – why and with whom do they transact? And why does economics have to be exclusively teleological (concerned with intentional states of individuals)? Where are the arguments for that ontological reductionism? And what about collective intentionality and constitutive background rules?

[1] One might also note that often equations have to be rigged in order to solve in mainstream economics, as Steve Keen and many others have demonstrated.

These are all justified questions – so, in what way can one maintain that these models give workable microfoundations for macroeconomics? Science philosopher Nancy Cartwright (2012:28) gives a good hint at how to answer that question:

> "Our assessment of the probability of effectiveness is only as secure as the weakest link in our reasoning to arrive at that probability. We may have to ignore some issues to make heroic assumptions about them. But that should dramatically weaken our degree of confidence in our final assessment. Rigor isn't contagious from link to link. If you want a relatively secure conclusion coming out, you'd better be careful that each premise is secure going in."

In all those economic models that Rodrik praise – where the conclusions follow deductively from the assumptions – mathematics is the preferred means to assure that we get what we want to establish with deductive rigour and precision. The problem, however, is that what guarantees this deductivity are as a rule the same things that make the external validity of the models wanting. The core assumptions (CA), as we have shown, are as a rule not very many, and so, if the modelers want to establish "interesting" facts about the economy, they have to make sure the set of auxiliary assumptions (AA) is large enough to enable the derivations. But then -- how do we validate that large set of assumptions that gives Rodrik his "clarity" and "consistency" outside the model itself? How do we evaluate those assumptions that are clearly used for no other purpose than to guarantee an analytical-formalistic use of mathematics? And how do we know that our model results "travel" to the real world?

On a deep level one could argue that the one-eyed focus on validity and consistency make mainstream economics irrelevant, since its insistence on deductive-axiomatic foundations doesn't earnestly consider the fact that its formal logical reasoning, inferences and arguments show an amazingly weak relationship to their everyday real world equivalents. Although the formal logic focus may deepen our insights into the notion of validity, the rigour and precision has a devastatingly important trade-off: the higher the level of rigour and precision, the smaller is the range of real world application. So the more mainstream economists insist on formal logical validity, the less they have to say about the real world. The time is due and over-due for getting the priorities right.

The empirical turn

Rodrik maintains that "imaginative empirical methods" – such as game theoretical applications, natural experiments, field experiments, lab experiments, RCTs – can help us to answer questions concerning the external validity of economic models. In Rodrik's view they are more or less tests of "an underlying economic model" and enable economists to make the right selection from the ever expanding "collection of potentially applicable models". Writes Rodrik (2015:202):

> "Another way we can observe the transformation of the discipline is by looking at the new areas of research that have flourished in recent decades. Three of these are particularly noteworthy: behavioral economics, randomized controlled trials (RCTs), and institutions ... They suggest that the view of economics as an insular, inbred discipline closed to the outside influences is more caricature than reality."

I beg to differ. When looked at carefully, there are in fact few real reasons to share Rodrik's optimism on this "empirical turn" in economics.

Field studies and experiments face the same basic problem as theoretical models – they are built on rather artificial conditions and have difficulties with the "trade-off" between internal and external validity. The more artificial conditions, the more internal validity, but also less external validity. The more we rig experiments/field studies/models to avoid the "confounding factors", the less the conditions are reminiscent of the real "target system". You could of course discuss the field vs. experiments vs. theoretical models in terms of realism – but the nodal issue is not about that, but basically about how economists using different isolation strategies in different "nomological machines" attempt to learn about causal relationships. I have strong doubts on the generalizability of *all three* research strategies, because the probability is high that causal mechanisms are different in different contexts and that lack of homogeneity/stability/invariance doesn't give us warranted export licenses to the "real" societies or economies.

If we see experiments or field studies as theory tests or models that ultimately aspire to say something about the real "target system", then the problem of external validity is central (and was for a long time also a key reason why behavioural economists had trouble getting their research results published).

The increasing use of natural and quasi-natural experiments in economics during the last couple of decades has led, not only Rodrik, but several other prominent economists to triumphantly declare it as a major step on a recent path toward empirics, where instead of being a deductive philosophy, economics is now increasingly becoming an inductive science.

In randomized trials the researchers try to find out the causal effects that different variables of interest may have by changing circumstances randomly – a procedure somewhat ("on average") equivalent to the usual ceteris paribus assumption).

Besides the fact that "on average" is not always "good enough", it amounts to nothing but hand waving to *simpliciter* assume, without argumentation, that it is tenable to treat social agents and relations as homogeneous and interchangeable entities.

Randomization is used to basically allow the econometrician to treat the population as consisting of interchangeable and homogeneous groups ("treatment" and "control"). The regression models one arrives at by using randomized trials tell us the average effect that variations in variable X has on the outcome variable Y, without having to explicitly control for effects of other explanatory variables R, S, T, etc., etc. Everything is assumed to be essentially equal except the values taken by variable X.

In a usual regression context one would apply an ordinary least squares estimator (OLS) in trying to get an unbiased and consistent estimate:

$$Y = \alpha + \beta X + \varepsilon,$$

where α is a constant intercept, β a constant "structural" causal effect and ε an error term.

The problem here is that although we may get an estimate of the “true” average causal effect, this may “mask” important heterogeneous effects of a causal nature. Although we get the right answer of the average causal effect being 0, those who are “treated” (X=1) may have causal effects equal to – 100 and those “not treated” (X=0) may have causal effects equal to 100. Contemplating being treated or not, most people would probably be interested in knowing about this underlying heterogeneity and would not consider the OLS average effect particularly enlightening.

Limiting model assumptions in economic science always have to be closely examined since if we are going to be able to show that the mechanisms or causes that we isolate and handle in our models are stable in the sense that they do not change when we “export” them to our “target systems”, we have to be able to show that they do not only hold under *ceteris paribus* conditions and *a fortiori* only are of limited value to our understanding, explanations or predictions of real economic systems.

Real world social systems are 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 being atomistic and additive. When causal mechanisms operate in real world social target systems they only do it in ever-changing and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do it (as a rule) only because we engineered them for that purpose. Outside man-made “nomological machines” they are rare, or even non-existent.

I also think that most “randomistas” really underestimate the heterogeneity problem. It does not just turn up as an *external* validity problem when trying to “export” regression results to different times or different target populations. It is also often an *internal* problem to the millions of regression estimates that economists produce every year.

Just as econometrics, randomization promises more than it can deliver, basically because it requires assumptions that in practice are not possible to maintain.

“Ideally controlled experiments” tell us with certainty what causes what effects – but only 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”. Causes deduced in an experimental setting still have to show that they come with an export-warrant to the target population/system. The causal background assumptions made have to be justified, and without licenses to export, the value of “rigorous” and “precise” methods – and “on-average-knowledge” – is despairingly small.

So, no, I find it hard to share Rodrik’s and others enthusiasm and optimism on the value of (quasi)natural experiments and all the statistical-econometric machinery that comes with it. We are still waiting for the export-warrant. As Jakob Kapeller (2013:210) argues – following the argumentation in Hans Albert (2012[1963]) – is the experimental turn no reason to think that mainstream economics has left its Model Platonism behind

> “Taking assumptions like utility maximization or market equilibrium as a matter of course leads to the ‘standing presumption in economics that, if an

> empirical statement is deduced from standard assumptions then that statement is reliable' ...
>
> The ongoing importance of these assumptions is especially evident in those areas of economic research, where empirical results are challenging standard views on economic behaviour like experimental economics or behavioural finance ... From the perspective of Model-Platonism, these research-areas are still framed by the 'superior insights' associated with early 20th century concepts, essentially because almost all of their results are framed in terms of rational individuals, who engage in optimizing behaviour and, thereby, attain equilibrium. For instance, the attitude to explain cooperation or fair behaviour in experiments by assuming an 'inequality aversion' integrated in (a fraction of) the subjects' preferences is strictly in accordance with the assumption of rational individuals, a feature which the authors are keen to report ...
>
> So, while the mere emergence of research areas like experimental economics is sometimes deemed a clear sign for the advent of a new era ... a closer look at these fields allows us to illustrate the enduring relevance of the Model-Platonism-topos and, thereby, shows the pervasion of these fields with a traditional neoclassical style of thought."

Regarding game theory, yours truly remembers when back in 1991, earning my first Ph.D. with a dissertation on decision making and rationality in social choice theory and game theory, I concluded (Syll 1991:105) that

> "…repeatedly it seems as though mathematical tractability and elegance – rather than realism and relevance – have been the most applied guidelines for the behavioural assumptions being made. On a political and social level it is doubtful if the methodological individualism, ahistoricity and formalism they are advocating are especially valid."

This, of course, was like swearing in church. My mainstream neoclassical colleagues were – to say the least – not exactly überjoyed. Listening to what one of the world's most renowned game theorists, Ariel Rubinstein, has to say on the (rather limited) applicability of game theory (Rubinstein 2012), basically confirms my doubts about how well-founded is Rodrik's "optimism":

> "I believe that game theory is very interesting. I've spent a lot of my life thinking about it, but I don't respect the claims that it has direct applications.
>
> The analogy I sometimes give is from logic. Logic is a very interesting field in philosophy, or in mathematics. But I don't think anybody has the illusion that logic helps people to be better performers in life. A good judge does not need to know logic. It may turn out to be useful – logic was useful in the development of the computer sciences, for example – but it's not directly practical in the sense of helping you figure out how best to behave tomorrow, say in a debate with friends, or when analysing data that you get as a judge or a citizen or as a scientist ...

> *Game theory is about a collection of fables*. Are fables useful or not? In some sense, you can say that they are useful, because good fables can give you some new insight into the world and allow you to think about a situation differently. But fables are not useful in the sense of giving you advice about what to do tomorrow, or how to reach an agreement between the West and Iran. The same is true about game theory."

So – contrary to Rodrik's optimism – I would argue that although different "empirical" approaches have been – more or less – integrated into mainstream economics, there is still a long way to go before economics has become a true empirical science.

The behavioural challenges

How would people react if a renowned physicist, say, Richard Feynman, was telling them that sometimes force is proportional to acceleration and at other times it is proportional to acceleration squared?

I guess they would be unimpressed. But actually, what Rodrik does in amounts to the same strange thing when it comes to theory development and model modification.

In mainstream neoclassical theory preferences are standardly expressed in the form of a utility function. But although the expected utility theory has been known for a long time to be both theoretically and descriptively inadequate, neoclassical economists all over the world gladly continue to use it, as though its deficiencies were unknown or unheard of.

What Rodrik and most other mainstream economists try to do in face of the obvious theoretical and behavioural inadequacies of the expected utility theory, is to marginally mend it. But that cannot be the right attitude when facing scientific anomalies. When models are plainly wrong, you'd better replace them!

As Matthew Rabin & Richard Thaler (2001: 230) have it:

> "It is time for economists to recognize that expected utility is an ex-hypothesis, so that we can concentrate our energies on the important task of developing better descriptive models of choice under uncertainty."

In a similar vein, Daniel Kahneman (2011) maintains that expected utility theory is seriously flawed since it doesn't take into consideration the basic fact that people's choices are influenced by *changes* in their wealth. Where standard microeconomic theory assumes that preferences are stable over time, Kahneman and other behavioural economists have forcefully again and again shown that preferences aren't fixed, but vary with different reference points. How can a theory that doesn't allow for people having different reference points from which they consider their options have a (typically unquestioned) axiomatic status within economic theory?

Much of what experimental and behavioural economics come up with, is really bad news for mainstream economic theory, and to just conclude, as Rodrik (2015:204) does, that these

> "…insights from social psychology were subsequently applied to many areas of decision making, such as saving behavior, choice of medical insurance, and fertilizer use by poor farmers…"

sounds, to say the least, somewhat lame, when the works of people like Rabin, Thaler and Kahneman, in reality, show that expected utility theory is nothing but transmogrifying truth.

To Rodrik, mainstream economics is nothing but a smorgasbord of "thought experimental" models. For every purpose you may have, there is always an appropriate model to pick.

But, really, there has to be some limits to the flexibility of a theory!

If you freely can substitute any part of the core and auxiliary sets of assumptions and still consider that you deal with the same – mainstream, neoclassical or what have you – theory, well, then it's not at theory, but a chameleon.

The big problem with Rodrik's cherry-picking view of models is of course that the theories and models presented get totally immunized against all critique. A sure way to get rid of all kinds of "anomalies", yes, but at a far too high price. So people do not behave optimizing? No problem, we have models that assume satisficing! So people do not maximize expected utility? No problem, we have models that assume …

A theory that accommodates for any observed phenomena whatsoever by creating a new special model for the occasion, and *a fortiori* having no chance of being tested severely and found wanting, is of little real value.

Conclusion

If we cannot show that the mechanisms or causes we isolate and handle in our models are stable, in the sense that what when we export them from are models to our target systems they do not change from one situation to another, then they only hold under ceteris paribus conditions and a fortiori are of limited value for our understanding, explanation and prediction of our real world target system.

But how do mainstream economists react when confronted with the monumental absence of empirical fit of their economic models? Well, they do as they always have done – they use one of their four pet strategies for immunizing their models to the facts:

(1) Treat the model as an axiomatic system, making all its claims into tautologies – "true" by the meaning of propositional connectives.

(2) Use unspecified auxiliary ceteris paribus assumptions, giving all claims put forward in the model unlimited "alibis".

(3) Limit the application of the model to restricted areas where the assumptions/hypotheses/axioms are met.

(4) Leave the application of the model open, making it impossible to falsify/refute the model by facts.

Sounds great doesn't it?

Well, the problem is, of course, that "saving" theories and models by these kind of immunizing strategies are totally unacceptable from a scientific point of view.

If economics has nothing to say about the real world and the economic problems out there, why should we care about it? As long as no convincing justification is put forward for how the inferential bridging between model and reality de facto is made, economic model building is little more than hand waving.

The real economic challenge is to face reality and still try to explain why economic transactions take place – instead of simply conjuring the problem away by assuming rational expectations, or treating uncertainty as if it was possible to reduce it to stochastic risk, or by immunizing models by treating them as purely deductive-axiomatic systems. That is scientific cheating. And it has been going on for too long now.

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