Rights and wrongs of economic modelling: Refining Rodrik

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1 Introduction

Time will tell whether Dani Rodrik's *Economics Rules* (perhaps in its next editions) will win a place in the series of honoured treatises on the nature of economics written by practicing economists, next to works such as J.E. Cairnes's *The Character and Logical method of Political Economy* (1875/88), Lionel Robbins's *The Nature and Significance of Economic Science* (1932/35) and Milton Friedman's "The methodology of positive economics" (1953). However it turns out, the timing of Rodrik's book is well-chosen: it comes rather soon after the collapse of the global financial markets had generated some intellectual turmoil amongst the economics profession and various outsiders, prompting serious questions about the capabilities of the academic discipline of economics. It is these questions that Rodrik sets out to address.

There are some commendable features in Rodrik's account. First, it is an attempt to provide a balanced treatment of the capacities and performance of economics. It is critical yet informed, unlike other pronouncements from within economics that manifest more complacency and arrogance than insight, while at the same time it is unlike those commentaries that are inspired by (informed or uninformed) deep discontent with the discipline of economics. Rodrik aspires to develop an image of economics that can be used for understanding the concerns of both sides, and to steer a middle of the road line. This puts his account in the tradition of Alfred Marshall's methodological thinking and J.N. Keynes's *The Scope and Method of Political Economy* (1890). This balance is to be commended, but as we will see, it creates a number of tensions.

Second, unlike many other recent methodological statements by practicing economists, Rodrik's reasoning is to some extent based on at least some consultation of expert work in the specialized research field of methodology and philosophy of economics (cf. Aydinonat 2015). While many others exhibit another version of disciplinary complacency and arrogance by ignoring this expert research, Rodrik seems willing to consider whatever possibly useful tools and observations these experts might have produced. While the book is not written for an audience of scholars in the philosophy of economics, it is definitely an inviting read for learning, critical discussion, and further elaboration amongst such scholars.

I will focus on what lies at the heart of Rodrik's book, issues of modelling in economics, a major theme also in current philosophy of economics (see Morgan and Knuuttila 2012). More specifically, I will take the book's two subtitles (in its two printings by two publishers) as a target of my attention: *why economics works, when it fails, and how to tell the difference;* and *the rights and wrongs* *of the dismal science*. I will provide a framework in terms of which many of Rodrik's insights can be elaborated in a systematic fashion. This will enable exposing them to critical philosophical scrutiny and identifying possible issues for further debate and elaboration to make further progress in our understanding of economics.

2 Don't blame economics, blame economists!

So there are rights and wrongs, successes and failures. In order to please both champions and critics of economics, one attractive rhetorical strategy is to divide the praise and blame between different parts of the enterprise: right here, wrong there. Rodrik suggests that if there is a problem, it is with economists, not with economics. Economics is just fine – or fine enough – but economists may misbehave because they are subject to professional biases and they misunderstand economics, its capacities and their limitations. True economics hails pluralism and humility, while economists all too often and all too strongly adhere to single theories and claim more than they are entitled to. Rodrik affirms he himself is loyal to economics, and he takes the task of teaching others what the practical implications of that loyalty are.

This may be a rhetorically smart strategy, but it relies on *a distinction that is difficult to sustain* – a distinction that involves keeping economics separate from economists' customary behaviour. On many popular accounts, they are not separate. In the extreme, they collapse into one. Indeed, there are those who (seriously or just for the fun of it) reduce economics to such behaviour: "economics is what economists do" as the famous phrase goes. Saying so would undermine Rodrik's divided judgement: if economists misbehave, then, by conceptual necessity, there is a flaw in economics. One should not rush to this conclusion though. This way of defining economics implies a circularity. As soon as one is asked to give some conditions for how to identify economists and their doings, one will have a hard time not citing economics in the answer.

An obvious way to try to avoid the reductionism and circularity of "economics is what economists do" is to use 'economics' as a name of an *academic discipline*, and to take disciplines to consist of various cognitive, behavioural, and institutional components. But this does not help for sustaining Rodrik's divided judgement either. In this case too, one cannot avoid including some ideas about economists' attitudes and behaviour in one's concept of economics. The concept of a scientific discipline is usually considered to contain an idea of regular and regulated behaviour, of disciplinary practice – or at least to cite the conventions or recipes of such behaviour. Academic disciplines don't exist independently of such practices, nor are they reduced to them, such practices rather partly constitute academic disciplines. So if there is a problem with such practices, there is a problem with the respective discipline. It is not possible for the discipline of economics to be just fine if economists regularly misbehave, especially if this regularity is supported by customary attitudes.

One may – and should -- distinguish between *proper and improper practices*, those in line with the ideal conventions of a discipline, and those that violate such ideals. Indeed, this is implied by stating that economists often are "not being true to their own discipline" (178). If improper practices are regular and dominant, we should conclude that there is a problem with the discipline. If they are exceptional – or not very seriously inappropriate – then we might conclude that the discipline is fine, and it is those rare violations that are the problem. Rodrik does not seem to think economists' improper practices are negligible in this way. They are more serious. Not only is the proportion of improper practices too large, but they are also supported by improper conventional attitudes.

One way to remedy Rodrik's divided judgement is to adjust the notion of economics and to make the division *within economics* (rather than between economics and economists' behaviour). One could simply admit that there is a problem with some parts or aspects of economics while claiming that other parts of it are just fine. This would seem to be in line with Rodrik's reasoning. To articulate this idea properly, some elaborations are needed.

Let us see what Rodrik says about the notion of economics. He does not to employ the "substantivist" idea that economics is "a social science devoted to understanding how the economy works" (7). Instead, Rodrik mostly adopts a "method-based" conception of economics: "economics is a way of doing social science, using particular tools […] the discipline is associated with an apparatus of formal modeling and statistical analysis" (7). As one of his more radical formulations has it, "economics is, in fact, a collection of diverse models" (6). So the idea of model is central to Rodrik's idea of economics

There are ambiguities in these formulations as they waver between characterizing economics in terms of *methods* of modelling; *practices* of modelling; and *products* of modelling, that is, models. I presume the intended idea is that the methods and products are fine, while some of the practices are not (although some others are). Economists often misbehave, while the models and modelling methods of economics are basically alright. All of this belongs to the discipline of economics, so the divided judgement is about parts or aspects of the discipline. Rodrik has set out to defend the general principle that models are needed for creating knowledge about the economy and if appropriately interpreted and used, are indispensable for the task -- but he is also prepared to "criticize the manner in which economists often practice their craft and (mis)use their models" (6). Economics not only consists of models and modelling methods, but also of various customary "practices and professional biases" (174). This is in line with the subtitles of the book:

when *economics* works and fails; the rights and wrongs of *the dismal science*. So there are notable flaws in *economics* after all, even on Rodrik's own judgement.

Given this focus on the cognitive instruments of economics – models – let's consider an analogy. It too draws on an issue that is currently being discussed in some countries, and it attracts attention to instruments of action. As a popular argument has it, if there is a problem about people being shot and killed, the problem is not with guns but with how some people sometimes use them. The analogy is that just as the problem with models is not models themselves but their wrong use, the problem with guns is not guns themselves, but their improper use.

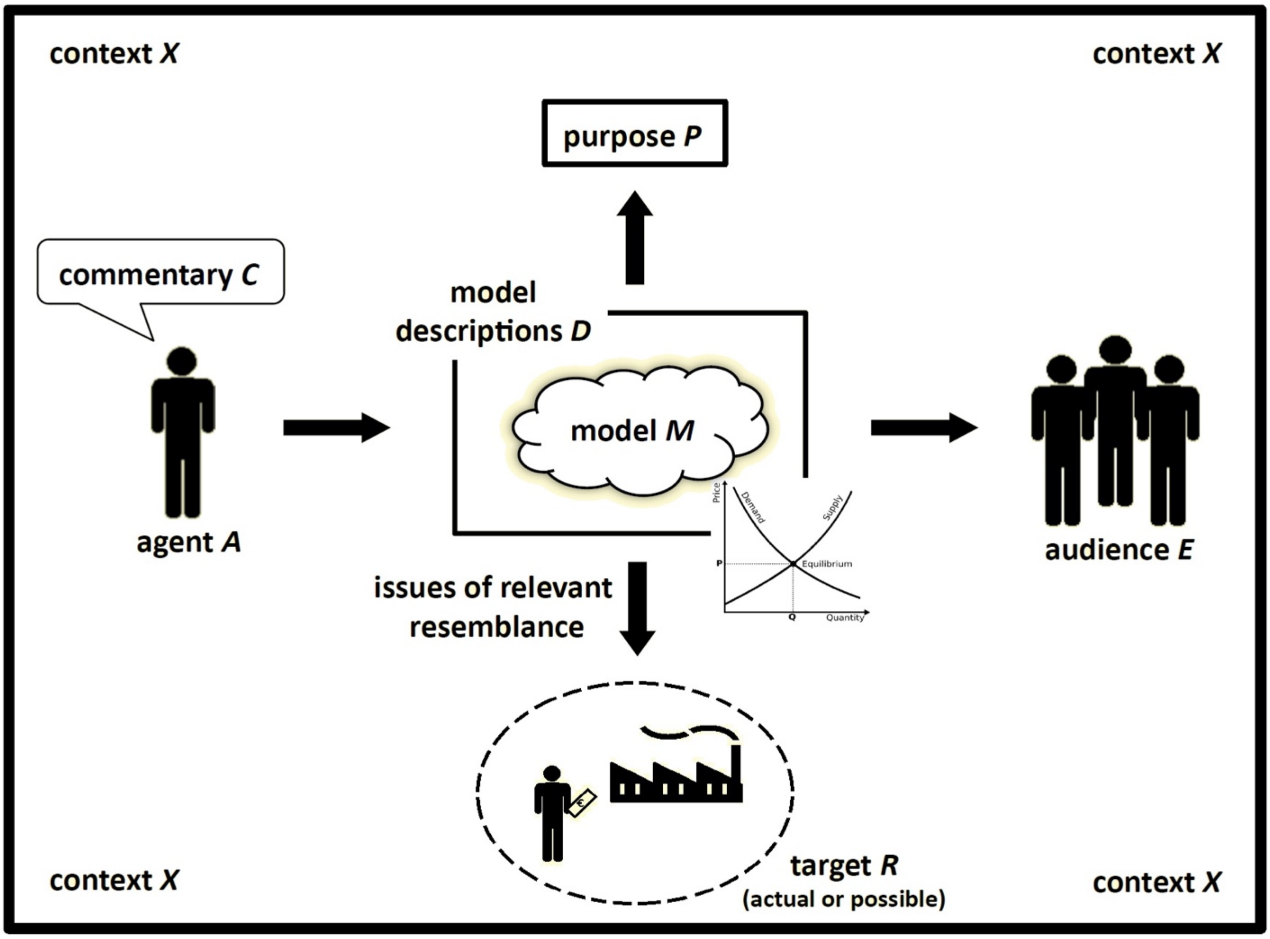
Some critics of economics appear to believe that the root of the alleged problems lies in models, or the method of modelling in general (see Mäki 2013). On this view, modelling just is not an appropriate way of acquiring knowledge about the social world. No (simple) model can do epistemic justice to the (complex) world, so there cannot be a proper use of models that would make the method of modelling justified. Models are the problem. Models distort the world, not people using them. This would be analogous to claiming that guns kill people. Just as a gun-free world would be a better world, a model-free economics would be better economics. This is not Rodrik's line. He believes the problem is not with models but with their bad use.

Of course, the "guns don't kill, people do" argument is open to two, quite different interpretations. One party argues that the availability of guns should be restricted and their use properly controlled -- and in the extreme, a gun-free world would be ideal (excepting perhaps the police, provided the police would behave themselves). The other party argues that the availability of guns should not be restricted – and in the extreme, more guns would make the world safer; this is because there are much more good people than bad people, so by being equipped with guns they could prevent bad people from misusing guns.

Restrict the availability of guns – this is the recommendation by many in the current debates over gun violence. Regarding models, Rodrik's recommendation is the exact opposite: make more models available! Drawing on the analogy further, Rodrik does not appeal – and cannot appeal – to the prevailing statistical distribution of good and bad economists, suggesting that more models will be good for economics because a vast majority economists currently are well-behaving. Any arguments for the proliferation of models would therefore need to be accompanied by normatively motivated measures that will ensure that most economists understand and use economic models sufficiently well. Addressing this issue requires a wider perspective. Indeed, a broadening of the relevant issues – from the ontology of model targets and the semantics of model-world relations to the pragmatics and cultural framework of economic modelling – is needed for having a sophisticated conversation about the rights and wrongs of economics. In the next section I will outline a framework that is hoped to accommodate such a breadth of issues.

3 Nature of model and dimensions of modelling

So what is it to be a model? It is to serve as a *surrogate object*. Scientists often start their investigations by building a model and examining it instead of trying to enter into a direct contact with what the model is about, viz. its *target*. Scientists use models as surrogate objects that stand for and represent the target objects of their inquiries. Modelling thus is an *indirect method* of inquiry. One directly examines the properties and behaviour of models in order to indirectly acquire relevant information about the ultimate target objects. For this to succeed, models as surrogate objects must be appropriately related to their targets. But things are more complex than this, as suggested by the following framework (see Mäki 2009, 2010, 2013, 2017). This framework can be used for organizing much of the analysis and debate over modelling.



Nothing is a model in itself. Modelhood requires a larger structure within which an object becomes a model. This larger structure embodies many of the dimensions that are characteristic of scientific disciplines. Spelling out its constitutive components and relations will therefore also help us see why Rodrik's divisionist assessment strategy (economics vs economists' attitudes and behaviour) is difficult to maintain.

Some *agent A* (individual or group) needs to consider and use an object as a *model, M*. This involves having an idea of the model being a model *of* some *target R*, actual or possible. It also involves some *purpose P* for which the model can be used (such as predicting or explaining a specific fact about some economic phenomenon, or grounding a piece of policy advice). I've suggested that it is also useful to add the further component of an *audience E* that is being addressed by the agent when building and using the model. For an economic model, the audience may consist of other economists in the same research field, economics students, other social scientists, the media, economic policy makers, and so on. Note that Rodrik also suggests that the audience makes a difference, for example when explaining that economists exhibit different degrees of confidence amongst themselves and when addressing non-economists.

In the case of theoretical models that are the focus of Rodrik's own discussion, we can think of models as fantasy worlds created by the modeller, *as imagined simple mini-worlds that are intended to represent some part or aspect of the complex maxi-world in society*. Representation amounts to the model serving as a *representative* of its target, standing for it as its surrogate; and to prompting issues of *resemblance* or similarity between the simple mini-world of the model and some part or aspect of the complex real maxi-world. This seems to be roughly in line with Rodrik's thinking as he says that a modeller "builds an artificial world that reveals certain types of connections among the parts of the whole – connections that might be hard to discern if you were looking at the real world in its welter of complexity" (12). Many models are also – or even only – models of some rich theory, providing a simple version of it. I will invoke this idea later below to point out a puzzle in Rodrik's argument.

Economists and others are often somewhat ambiguous about what models consist of. They often think of them as being made of strings of mathematical symbols, but they also recognize the idea that models are imagined simple systems created in theoretical laboratories as it were, those miniature fantasy worlds I mentioned above (such as the 2x2x2 worlds in international trade). This ambiguity can be resolved by distinguishing between models and their descriptions. Models as imagined mini-worlds are described by *model descriptions D*, such as mathematical equations, verbal characterizations or boxes and arrows on a black board. The idealizing assumptions (of transitivity of preferences, zero transaction costs, infinitely lived agents etc) are among the model descriptions. Thus we can say that in a given model, there are no transaction costs; and also that 'transaction costs = 0' is among the assumptions describing the model. We are imagining a world without transaction costs, and we are using the idealization for describing that world.

Model *commentary C* also plays an extremely important role, also for Rodrik's concerns. Indeed, it is here that we find – and he finds -- much of the deficiency in economic modelling. Model commentary contains and conveys ideas about how the other components in the modelling endeavour play out their roles in coordination with one another. What is the point of using radically unrealistic assumptions? When is unrealisticness alright and when not? What roles exactly can a model play in assertions or hypotheses about the world? Is it about an actual or merely a possible target? What's the proper domain of application of a model? What precise purpose(s) can a given model be used for? What uncertainties are involved in model use? A commentary provides (good or bad) answers to such questions. When Rodrik states that economists often have a limited understanding of some important aspects of modelling, he is implying that economists hold a deficient model commentary. *My reading of Rodrik's book is as an attempt to help develop better model commentaries for economics. This article is an attempt to help improve that attempt.* One of my proposals is to challenge Rodrik's claim that "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." (73) My contention is that *models are not transparent in this way in themselves; that is why a model commentary is needed.* For example, "critical assumptions" don't identify themselves as such; they need to be identified by the agent using an apposite commentary. A good commentary makes as much of the model and modelling process transparent as is possible and needed. This is also consistent with the spirit of Rodrik's book.

The things listed above take place against the background of a larger *context X*. The context includes a wide range of items, such as the professional culture and power structure of the discipline, the academic reward structure, the way economists are educated, the textbook industry, the structure and functioning of the job market, conventions of publishing, network externalities in the intellectual arena, self-image of the economics profession in disciplinary comparison, competition for academic resources, societal status of economics, extra-academic demand and inspiration, ideological and political affiliations of the economics profession, academic geopolitics – and whatever else is relevant to the way models are built and used. Rodrik also has thoughts about the contents of this larger context.

4. Rights and wrongs

The above framework can be used for a number of purposes, but here we are interested in the rights and wrongs of economics – the success and failure of economic modelling. The framework should be useful for this purpose in two ways. It helps us *define or characterize* what success and failure might amount to, and it helps us *identify the sources* of success and failure. Success and failure should be defined in terms of some goal, in terms of whether the goal is achieved or approached. Their sources are the conditions that can be used for explaining success and failure as well as for promoting success by manipulating those conditions so that they better facilitate success; these are also among the tasks of Rodrik's book.

A simple version of *success* is the attainment of *relevant resemblance* between the model and the target. This has two dimensions built into it: model-target relations and model-purpose/audience relations. Resemblance is a matter of model-target relations, while relevance is a function of the pragmatics of purposes and audiences. If the goal is to satisfy the need of a university administration (in a particular culture at a historical point in time) for a reliable but rough (say ±.5%) prediction of the effect on student enrolment of a change by x% of tuition fees, then a model succeeds if this is delivered. If the goal is to reliably alert legislators to some previously unrecognized feedback mechanisms in the functioning of the market for pollution rights, then success requires that the model relevantly resembles parts of a causal structure. Rodrik appears to agree with this; he says that when used well, a model "captures *the most relevant aspect of reality in a given context*" (11).

If success is a matter of attaining relevant resemblance between a model and some target, then success requires that the issue of (whether there is) relevant resemblance has been raised and then settled favourably. We can now see that there are two broad classes of *failure* (Mäki 2009, 2017). One is to fail in achieving relevant resemblance between a *surrogate model* and some target. The issue of relevant resemblance is raised, and efforts are taken to resolve the issue, but one fails because the respects and degrees of resemblance are not found fitting for the purpose and audience. Another sort of failure is deeper, it is not to raise any issues of relevant resemblance at all, but to use a model as a *substitute model* rather than as a surrogate model. Substitute models are examined without an attempt to ascertain how they are related to any target; the imagined mini-world of the model is a substitute for any target in the real maxi-world rather than an attempted pathway of indirectly accessing the latter (for qualifications, see Mäki 2009, 2017). These two classes of modelling failure give us another interpretation of "wrong versus not even wrong" (80-81): surrogate models can be wrong (or right), while substitute models cannot even be wrong about the world (since they are not presented and examined as being about the world).

While *describing models mathematically* is useful in that it may help increase clarity and transparency in cognition, it may also increase the temptation towards substitute modelling. Let us consider these two features in turn. First, I think Rodrik may sound too optimistic about the powers of mathematical model descriptions to guarantee *transparency* in models. "Once a model is stated in mathematical form, what it says or does is obvious to all who can read it." (31) It is not quite enough to say that transparency is dependent on the ability to "read" a model. This ability in turn is dependent on having an adequate model commentary that is informative about what the model "says and does". A more refined version of Rodrik's claim would thus suggest that model transparency is enhanced by mathematical model descriptions, but only if skilfully conjoined with a rich and sound model commentary.

Second, as to the temptation towards *mere substitute modelling*, Rodrik recognizes this, saying "too many economists fall in love with math [...] Excessive formalization – math for its own sake – is rampant in the discipline." (35) In some branches of economics the "reference point has become other mathematical models instead of the real world" (35-36), but he also sees the problem is becoming less severe than it has been (36-37). Below is a passage that can be translated into the claim that accepted practice allows economists to be content with a very contracted model commentary and to exercise substitute modelling, that is, examining models without an interest in learning about real world targets:

> "Asked point-blank, they can state chapter and verse all the assumptions needed to generate a particular result [...] But ask them whether the model is more relevant to Bolivia or to Thailand, or whether it resembles more the market for cable TV or the market for oranges, and they will have a hard time producing an articulate answer." (172)

In other words, many economists don't quite know what they are doing, and are doing too little. They are capable of providing a commentary about relationships among the various components in a model description. But they don't hold an articulate model commentary that would inform themselves and their audiences about how their models are connected to any external targets and what specific purposes they can be supposed to serve. What is remarkable is that it is *accepted practice* that they don't hold such a commentary: "accepted practice does not require economists to think through the conditions under which their models are useful" (172). This sounds like admitting that the disciplinary conventions of *economics* are flawed – instead of merely *economists* misbehaving in not abiding with the proper conventions.

So economists often fail to understand the models they use, and therefore often misuse them. Model commentary is where such understanding should be expressed. This then means that economists often have a *deficient commentary* of their models and this is a major source of failure in economic modelling. Economists excel in manipulating mathematical model descriptions, but are much less competent in regard to the details of, and coordination between, the other components of the modelling endeavour – targets, purposes, audiences, issues of relevant resemblance – and this shows in their limited model commentaries.

Flaws in model commentary also show in harmful discrepancies between the ways in which academic and extra-academic *audiences* are addressed. "Economists' contributions in public can therefore look radically different from their discussions in the seminar room." (170) They appear misleadingly confident, dogmatic, and unified when presenting ideas to extra-academic audiences, while in intra-academic settings they do not similarly suppress the uncertainties and disagreements involved in the modelling activity (209). Among other things, this may lead to dubious policy recommendations when over-confident economists looking for the attention of the media and government "overlook the fine print" (168) that should accompany the models when applied to policy. This is another failure of commentary, that of dismissing the "warning labels" that would alert the relevant audiences about the qualifications and provisos that should be conveyed together with a model and its conclusions. "In private discussions among ourselves we recognize this complexity, but we don't add the appropriate warning labels to our models when they are discussed in public. There, we pretend we understand more than we do." (Colander 2011, 20).

It seems safe to read Rodrik's key message as one that identifies the root of problems to lie in the pragmatics of modelling, the realm of agents, purposes, audiences, commentaries, larger social context. This means one should not read the following quite literally: "The antidote of a bad model is a good model" rather than "no model" (29). This may sound like a pro-gun person saying there are good guns and there are bad guns. But I suppose that Rodrik wants to say that *models are good or bad relative to some use or purpose, not in themselves*. So if model M_1 is bad for some given purpose P_j, then it had better be replaced by model M_2 that serves P_j better, and M_2 is good in this sense.

The use-relativity of goodness gives rise to intriguing issues. If the goodness of a model is always relative to some use and purpose, then we may ask whether there are bad models at all. An obvious first response would be to suggest that if a given model is not good for *any* purpose, then it is bad, period, but without qualifications, this will not do (see Mäki 2017). This is because for any given model, we can always invent some purpose, however fanciful or eccentric, for which the model is just fine – maybe the model brings idiosyncratic aesthetic pleasure to its builder, or maybe it is effective in fooling students, or maybe it is useful for someone's job promotion, or whatever. The obvious qualification is the idea that a model is bad if it cannot serve any *respectable* purpose (and good if it can). What we start seeing is that we are confronted with a hierarchy of considerations. If our judgement is that model M_i is useful for purpose P_j (that is, has properties in virtue of which it serves P_j well), we next have to ask whether P_j is among the respectable purposes – not just any purpose will do. The set of such respectable purposes must be constrained by principles that dictate the proper goals of economic inquiry. Some of the actually held or implied goals might not be respectable.

Given the above account of goodness and badness of models – that they are always relative to some respectable purpose – we can raise a critical question about Rodrik's idea of *how economics makes progress* in terms of models. Making progress, as well as creating a capacity of making progress, are of course kinds of success. Rodrik suggests that progress in economics is different from progress in natural sciences (71) as economic knowledge does not accumulate "vertically" by rejecting earlier bad models and replacing them with new good models, but rather "horizontally" by expanding the pool of models (67). "The newer generations of models do not render the older generations wrong or less relevant … Older models remain useful; we add to them." (71) Taken literally, this would seem to imply that all economic models ever built are relevant for some present (or perhaps future) respectable purpose. This sounds too radical, it would need some further qualification to be defensible.

Some of the deficiencies in modelling have their sources in the *larger context*. One example of such context is economics education. Economists may not be systematically educated to be attentive to real-world nuances – that is why they are "not always good about drawing the links between their models and the world" (171). The core craft of model selection that Rodrik keeps underlining, has not traditionally been on the agenda of economics education: "Freshly minted PhDs come out of graduate school with a large inventory of models but virtually no formal training – no course work, no assignments, no problem sets – in how one chooses among them." (83-84)

Another source of failure consists of weaknesses in disciplinary self-understanding (which could also be a product of narrow education). Among other things, "Economists often go astray precisely because they fancy themselves as physicians and mathematicians manqué." (45) Narrowness of competences then manifests in the familiar perceptions of disciplinary arrogance: "… economists are an arrogant bunch, with very little to be arrogant about" (Rodrik 2007, 5). This may result in difficulties in developing adequate model commentaries that would incorporate appropriate degrees of humility reflecting the uncertainties that are involved. And it may result in problems in interdisciplinary communication and collaboration that would be useful for recognizing and exceeding the limits of economic expertise.

There may be a blind spot in Rodrik's view of the contents of the larger context. He says, "The authority of [a piece of research] derives from its internal properties – how well it is put together, how convincing the evidence is – not from the identity, connections, or ideology of the researcher." (78) This is not sufficiently attentive to the social power structure within the discipline of economics at different scales, from academic geopolitics to domestic labour markets for economists. As Ariel Rubinstein complains, Rodrik here ignores the existence and often unfortunate influence of the elite of the discipline. As an example, "The job market for junior economists is an illustration of the unfairness associated with the power of the elite." (Rubinstein 2017, 165) This is not inconsequential for the sorts of models that are produced and get published, cited and disseminated.

5 Isolation and unrealistic assumptions

Rodrik appears as an advocate of what philosophers of economics call the isolation account of models: models identify and isolate mechanisms (e.g. Mäki 1992, 2005, 2011; see Aydinonat 2015).

> "The easiest way to understand them is as simplifications designed to show how specific mechanisms work by isolating them from other, confounding effects. A model focuses on particular causes and seeks to show how they work their effects through the system." (12)

Models must exclude many things in order to isolate what is of "the essence" in a phenomenon: a model "leaves many things out so that it can focus on the essence" (179). Rodrik notes that even the artificial world of the perfectly competitive market has this capacity. It leaves many real-world things out of the model world, such as the multiplicity of motives, rationality being overridden by emotion and cognitive shortcuts, some producers behaving monopolistically, and the like. "But it does elucidate some simple workings of a real-life market economy." (14) He also recognizes that theoretical models are similar to laboratory experiments: just as lab experiments "performed under conditions that depart starkly from the real world [models] allow us to identify a cause-effect relationship by isolating it from other confounding factors" (180; cf. 21-25).

Rodrik does not have much to say about exactly how these isolations and exclusions are accomplished, and especially about the role of *idealizing assumptions* in the procedure – other than recognizing in passing that "patently untrue" assumptions "allow us to identify a cause-effect relationship by isolating it from other confounding factors" (180). This is the best justification for unrealisticness in many assumptions, but Rodrik does not pursue its details any further. False idealizing assumptions are vehicles of isolation, they neutralize or remove many things so that analysis can focus on other things. Just as a physicist examines the impact of gravity on falling bodies by removing air pressure, an economist examines markets without transaction costs, and both make these idealizations in order to isolate some important causal or other dependency relation.

Explicitly stated idealizations contribute to the transparency of the models they describe. But much of what is left out of models is *silently omitted*; hence isolation is also accomplished without explicitly mentioning the things that are excluded. Indeed, most of the things that are excluded are not mentioned since they are not believed to have any relevance whatsoever. These beliefs may sometimes be incorrect, and whenever they turn out to be so, the right response is model revision. Anyway, quiet omission is *another source of shortages in model transparency* since such omissions are not expressed in model descriptions (yet the fact that a portion of the exclusions are displayed by explicit idealization may in some situations facilitate making silent omissions transparent by spelling them out).

Naturally, not just any isolation will do. Models may go astray, so their isolations must be revised by de-isolation, adding what was excluded from the previous model. "In some settings, a simple model can be, well, too simple. We may need more detail. The trick is to isolate just the interactions that are hypothesized to matter, but no more." (43) Isolation and de-isolation should be guided by the structure of the world itself, together with the pragmatics of modelling – that is, by considerations of relevant resemblance: "When causal mechanisms interact strongly with each other and cannot be studied in isolation, models do need to include those interactions." (179)

How then to characterize models that ensue as the products of successful and unsuccessful modelling activities? What attributes to use? Economists often say a given model is *useful*, or that it is useful to model some issue in this or that manner. In my view this can only serve as a starter. Without more detailed thoughts about *what a model is useful for*, and about the *model-target relations*, this remains hopelessly vague. The challenge is to amend the close to empty attribution of usefulness to models, and meeting this challenge is among the duties of a richer model commentary. Rodrik, on the other hand, employs the vocabulary of *correctness*, saying that "… the 'correct' economic model is the one that isolates the critical relationships, allowing us to understand what is really causal among all the things going on" (86) It is not clear what 'correct' is supposed to mean in this context, but this phrasing suggests that it might have something to do with truth rather than just usefulness.

So what about *truth*? A popular commonsense view is that models are false by their very nature. I say this is a commonsense view because models indeed appear to be false: there seems to be an obvious discrepancy between any model and what we know about its possible targets. I've argued this is too simplistic and that there is a way of looking at models and model-target relations such that there is a chance for a *model to be true*, on top of acknowledging that there may be *truth in a model* (Mäki 2011). Rodrik seems to be of two minds about whether economic models can be true. On the one hand, he says, "Models are never true; but there is truth in models." (44) On the other hand, he implies that models can be true: a model is "true [if it] captures the most important mechanism" behind a phenomenon (89). The latter idea can be appreciated by taking the pragmatics of modelling seriously enough. Letting the model commentary be specific about what exactly the model is intended to be about (in its target) and what exactly it is intended to be for (its purposes and audiences) determines those specific parts of a model that can be considered candidates for truth – while other parts play just auxiliary roles and are not nominated for truth. If those privileged model parts happen to be true, then we may say the model itself is true – true about what it is intended to capture in the target, not about the whole of it. (Mäki 2011)

Milton Friedman's 1953 methodological statement (F53) is hopelessly ambiguous, so no wonder it can be interpreted either as being in line with what is being suggested here (in this article and in Rodrik's book) about assumptions and unrealisticness, or as conflicting with it (Mäki 2009). Rodrik's account is in agreement with a realist interpretation of the F53 argument for *unrealistic assumptions*. At the same time, he refuses to buy the other part of F53's argument, the one that relies on the *predictive capacities* of economics: "no social science should claim to make predictions and be judged on that basis. The direction of social life cannot be predicted." (184) I won't pursue this complex issue here other than pointing out an observation that appears puzzling in relation to Rodrik's strong skepticism about prediction: if one seeks to provide justifiable policy recommendations (as Rodrik surely does), they must be partly based on some sort of anticipatory capacity. If a conditional policy recommendation has the form, "if you want to achieve X, do Z", then for the recommendation to be justified it must be based on some reasonable degree of predictive confidence about X occurring if has Z occurred: "if you do Z, then X will follow". Prediction may be very difficult as we all know, but all of policy advice (and the whole of social life) depends on at least some minimal success in it, so it is better not to throw it out entirely.

6 Negligibility, applicability, tractability, and "critical assumptions"

The challenges of unrealisticness in modelling are made more complex by *issues of negligibility, applicability, and tractability* (Musgrave 1980, Mäki 2000, 2012, Hindriks 2006). These issues need to be addressed to elaborate on the various functions served by unrealistic assumptions and on the goal of relevant resemblance between model and target.

There is no general principle that would justify all sorts of unrealisticness in all sorts of model components. Rodrik adopts the notion of *"critical assumption"* to demarcate between different sorts. While it is alright for many assumptions to be even utterly unrealistic, this is not alright for critical assumptions. I suggest the notion of negligibility can be used for elaborating this idea: *the unrealisticness of critical assumptions is not negligible and thus not alright.* Consider what Rodrik himself says about critical assumptions:

> "… an assumption is critical if its modification in an arguably more realistic direction would produce a substantive difference in the conclusion produced by the model" (27)

This highlights one aspect of negligibility, viz that of *ontic weight* captured by 'producing a susbstantive difference'. This is not yet to say anything about how large the difference must be to count as substantive. Judgements about this matter are relative to the other, pragmatic aspects of negligibility, viz those related to *purposes and audiences*. This is also implied by Rodrik's statement that "what makes an assumption critical depends in part on what the model is used for" (29).

As Rodrik says, an assumption is not critical when its lack of realisticness "is not of great importance" (28), that is, in our words, when its unrealisticness is negligible. Importance here can be construed as being a matter of both ontic weight (e.g. causal power) and of pragmatic relevance for some purpose (e.g. usefulness for it). No degree of unrealisticness is in itself small enough to be negligible (or large enough not to be negligible), it is only so relative to this or that purpose.

Indeed, the notion of negligibility is deeply pragmatic in that it depends not only on ontic weight, but essentially on uses, purposes, and audiences. Nothing is negligible in and of itself; it is always partly a function of the pragmatic constraints. We have already incorporated this aspect into our account of model, thus negligibility considerations can easily be accommodated. For predicting the effect of raising the tax on cigarettes on their retail price, the unrealisticness of assumptions about the degree of market competition, about whether various cigarette brands are perfect substitutes, and about perfect rationality is negligible, whereas for predicting the effects of price controls on the supply of cigarettes it is not negligible (27-29).

So my suggestion is to define 'critical assumption' in terms of negligibility. An assumption is *critical* if its unrealisticness (of some sort and degree) *is not negligible* for the conclusion drawn. And an assumption is *not critical* if its unrealisticness *is negligible*. Critical assumptions therefore had better be realistic, that is, realistic enough. And claims about negligibility had better be true, and checking them for their truth is part of good modelling practice (see Mäki 2012).

Critical assumptions may remain *not only unchecked but unstated*, and this may lead to serious problems. Rodrik's example is the "frenzy over market liberalization" in the 1980-90s, using models of markets and failing to make explicit that their applicability is dependent on "the existence of various social, legal and political institutions" which then produced disastrous outcomes in post-Soviet Russia for example (97-98). This is an example of *failure in model description*, failure to properly describe critical components in models under the guidance of adequate model commentary.

The example also shows how critical assumptions may be important for specifying the *applicability* conditions of a model. The market models are only applicable provided the assumptions about institutional conditions are realistic (and to check them for their realisticness they must be explicitly specified). This observation makes Rodrik critical of the economics profession the large majority of which (even over 90%) is prepared to rush to agree on generalized statements about rent controls, trade restrictions, fiscal stimulus, and minimum wages even though these only hold under rather stringent conditions that are not regularly met in the real world (149-150). These economists thus tend to ignore the "teachings of economics":

> "What economics teaches us are the explicit conditions – critical assumptions – under which one conclusion or its opponent is correct." (150)

I have two comments on this. First, here we again meet the idea that economics is distinct from economists' attitudes and behaviour – that ideally economics teaches us about explicit applicability conditions while the majority of economists ignore them. Second, applicability and negligibility considerations must go together. It is not that a model is applicable only if certain key assumptions are strictly accurate – that goods are perfect substitutes, that information is perfect, that there are zero transaction costs in the economy. We should be more permissive and say that a model is applicable if such assumptions are realistic enough, that is, if their unrealisticness is negligible for the (respectable) purpose at hand.

The purpose-dependence of negligibility and applicability considerations is imperfectly captured by phrasings like this: "The applicability of a model depends on how closely critical assumptions approximate the real world." (29) Instead of 'how closely' we should say 'how closely for the purpose at hand' or the like. Negligibility is not merely a matter of the "distance" between an assumption and some real fact; it also involves the pragmatics of purposes and audiences.

Negligibility issues should also be brought to bear on the challenges of *tractability* in modelling. There is a general sense in which the whole idea of model and model-based science derive from the need for tractability. The real world out there is far too intractable to be examined directly (because it is too complex, too small, too slow, too far away in space or time, and so on), therefore one directly examines the more tractable model worlds. When we additionally zoom in and look at individual assumptions, we find that many of them are there for tractability reasons, and they may be quite starkly unrealistic.

I have two observations on this. First, many of these tractability-enhancing assumptions are made because the math that is being used requires it. They enhance the mathematical tractability of models. This is not riskless as suggested by the second observation, namely that unrealistic tractability-enhancing assumptions are harmless if they are not critical assumptions, that is, if their unrealisticness happens to be negligible (and in such a case there is no need to relax such an assumption; see Mäki 2012). However, they can be quite harmful if tractability and negligibility do not go hand in hand, that is, if the unrealisticness of a tractability-enhancing assumption is not negligible. Rodrik recognizes this is not at all always the case: "The strategic simplifications of the modeler, made for reasons of tractability, can have important implications for substantive outcomes." (38) And in particular, recent macroeconomic models have such implications: "In trying to render their models tractable, economists neglected many important aspects of the real world." (101) Those aspects were not negligible. The dominance of mere tractability in models may have unfortunate consequences.

7 The model, a model, and principles of model selection

Model selection is a key theme in Rodrik's book. Economics offers a large pool of models, and the task of economists should be to recognize this multitude and to select models that are fit for a context of application. Rodrik subscribes to what might be called the *Keynes-to-Harrod principle* that Keynes famously formulated in his letter to Harrod in 1938: "Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world." Rodrik's wording for the principle goes like this:

> "Rather than a single, specific model, economics encompasses a collection of models. The discipline advances by expanding its library of models and by improving the mapping between these models and the real world. The diversity of models in economics is the necessary counterpart to the flexibility of the social world. Different social settings require different models. Economists are unlikely ever to uncover universal, general-purpose models." (5)

Both Keynes and Rodrik think that economists have difficulties with the art of model selection, which gives rise to major wrongs in economic modelling. Economists do not make full use of the multitude of models and they make mistakes about the applicability conditions of the models they do use. In particular, "Models lifted out of their original context can be used in settings for which they are inappropriate." (172) This may be because economists are too attached to one model and are driven by a universalizing proclivity, or in Rodrik's words, economists "are prone to mistake *a* model for *the* model, relevant and applicable under all conditions" (6). So no general-purpose model is likely to be forthcoming in economics, hence one is mistaken to use *a* model as *the* model that is believed to be universally applicable.

This is one of the key ideas in Rodrik's book. It prompts a number of interesting questions and observations. What is the role of generality and level of abstraction in one's conception of 'a model' and 'the model'? What is it to be a context and what does it mean to take context into account in modelling? How to conceive the notion of diversity and the roles of background theories? If we were to consider economists behaving according to their own theoretical principles regarding constrained choice, what would the implications be about model selection?

"A model, not The Model" is a catchy slogan, but what exactly does it mean? An obvious reading derives from Rodrik's earlier work on globalization and national economic policies in his *One Economics, Many Recipes* (2007). There is no single generic *policy recipe* for all situations, all industries, all cultures, all historical periods – such as 'always rely on the government', or 'always go for a market solution', or the like. Many economists don't appreciate this, which is a serious flaw since "the tendency of many economists to offer advice based on simple rules of thumb, regardless of context (privatize this, liberalize that), is a derogation than a proper application of neoclassical economic principles" (2007, 3). But any such over-extended policy recipe is at most *model-based*, it is not a *model itself*. While the arguments against such context-blind simple-mindedness concerning policy recipes sound reasonable, are there analogical arguments against generally applicable models?

Don't stick to one model, believing it to be universally applicable, be open to many other models, check their applicability in various contexts with care, then select those that are applicable! This principle is riddled with ambiguities and hidden underlying dimensions, and we'd better try to spell them out. One ambiguity is pointed out by Emrah Aydinonat (this issue): it is one thing to make a context-sensitive selection of a model from a pool of models, fit for the purpose and domain at hand; and quite another to make a context-sensitive selection of a set of models that jointly capture the causal contributions of a number of mechanisms to a complex phenomenon. Note that Aydinonat discusses situations in which models are used for explaining singular, particular phenomena. Rodrik's reasoning seems similarly circumscribed. I think there is a need to enrich this picture by incorporating dimensions of isolation and de-isolation, and by asking what role might remain for The Model to play.

Traditionally, the disciplinary cohesion of economics has depended on shared core theories and models that have been used for unifying various types of phenomena and distinct branches of economics (see Mäki 2001). Ability to unify means being widely applicable. Consider now The Model to be the potentially widely applicable and unifying model. Then consider those various applications of The Model requiring that modified *versions* of it be developed and tailored to fit with various contextual specificities. Because they are versions *of The Model*, what happens is not that The Model is replaced by its versions, but rather that The Model prevails and retains its unifying functions through its versions. These specific versions emerge from The Model along two dimensions, those of vertical and horizontal isolation and de-isolation (see Mäki 1992).

One dimension is between levels of abstraction. Movements between them are accomplished by *vertical isolation and de-isolation*, or between abstraction and concretization whereby one either removes or adds specific features of the target. The model of supply and demand in the generic market is very abstract, those of markets for consumer durables (or labour, real estate etc) are less so, and models of supply and demand in the market for mobile telephones in China in 2000 (or for experts in philosophy of economics in Europe in 2018) are quite concrete. We may say that along this dimension, *The Model* here is the simple abstract supply and demand model, and any model of a particular local case of supply and demand is *a model*. We may say such concrete models are versions of The Model since they inherit its components and principles of functioning – while specifying and modifying them guided by the relevant characteristics of the particular context of application. Modifications by such vertical de-isolation may be accompanied by *horizontal de-isolation*, and this happens by amendment, by way of including factors previously excluded by the isolations of the model, but now considered relevant in the particular context. This may happen through enriching a model by de-isolation or by combining it with other models that depict those previously excluded items (the latter is Aydinonat's second case). Two conclusions. First, it is not a model *instead of* The Model, it is both. Rodrik's critique would only hit cases in which The Model of abstract supply and demand were directly applied to a particular case, not through its specific versions. Second, The Model is widely applicable, and it unifies a large variety of phenomena – albeit not directly.

Consider geographical economics, built upon a relatively simple core model of agglomeration of economic activity. This core model is modified and extended so as to give rise to a variety of models that apply at different scales, from cities to regions to the global system, dealing with phenomena of industry clusters, a variety of core-periphery patterns, international trade, economic growth, and development. These targets at different scales are accessed by incorporating various de-isolations and other modifications. The simple core model (let's say The Model) is intended and believed to unify these domains, showing that they are governed by similar mechanisms, and its unificatory power is not supposed to be compromised by the fact that its wide application requires modifications in each domain or "context" of application. (Mäki and Marchionni 2010) Again, it is not The Model contra a model, it is both. Rodrik's criticism would only hit weird non-existent applications of the core model directly to domains at different scales. At the same time, there are perfectly valid criticisms of excessively straightforward applications of geographical economics models that do not pay sufficient attention to the richness of contextual contingencies. But this criticism does not imply that there is no room for The Model that seeks to unify a variety of domains.

So what I am suggesting is that Rodrik's principle "a model, not The Model" be refined by addressing the above image that reclaims the unifying functions of The Model without compromising the context-sensitivity of its wide-ranged applications. On this image, it is The Model that has the task of isolating and representing, in a skeletal manner, a shared mechanism. More specific models then offer enriched versions of the mechanism and put them in context by de-isolating The Model and thereby adding further detail that is needed to capture the relevant specifics of the domains of interest.

Let us briefly consider the issue from the point of view of *the contents of the information about the context that are incorporated in The Model in its specific applications*. At one extreme, no contents are incorporated. *No attention* is paid to the specifics of the context, yet The Model is directly applied to it. No further models or versions are needed. This is the only case to which Rodrik's "a model, not The Model" criticism straightforwardly would seem to apply.

At the other extreme, *all attention* is paid to the context, and The Model plays no role whatsoever. The various contexts are unique, they share nothing or little in common – at any rate they don't overlap by containing the mechanism depicted by The Model. All that matters is case-specific contextual information – and one can have a debate over which items in the context are non-negligible. This would have two intriguing implications. First, strictly speaking, conventional talk about 'context' loses its meaning if all situations are unique and share no common recurring thing, such as a mechanism, for which the rest of the specific situation would serve as a varying context. Second, no unification across domains would be possible, and modelling would be part of some sort of idiographic strategy of inquiry. Rodrik's "a model, not The Model" could be interpreted as a recommendation for such an idiographic approach, but it is unlikely that was his intention.

In between those extremes, *some attention* is paid to the context, and the issue is, how much, how, and to what items in the context. These should be the real issues, and much of the time they are. Indeed, much of the criticism of (the wrongs of) economic modelling argues that it is based on a too narrow conception of context, that it leaves out relevant items and so ignores information about contextual things such as culture, history, power, and mentality in various specific settings. These are negligibility issues, and one can argue that too much of economic modelling does not adequately address them. The key question about any contextual item is whether it is negligible or not, that is, whether it makes a difference that is sufficiently significant for the purpose for which the model is built and used. Once again, there is an obvious source of either ignoring these negligibility issues or rushing to the (implicit or explicit) conclusion that these items are negligible: the items are hard to formalize and measure commensurably with the more traditional items included in economic models; and economists are not always well educated to examine the causal roles of these "softer" items. I am not convinced "a model, not The Model" is able to accommodate these issues, but on the other hand addressing them would seem to be in line with the spirit of Rodrik's argument.

Finally, I have two other critical observations, both inviting refinements in Rodrik's reasoning. Recall that he argues the strength of economics lies in the method of modelling and the availability of a multiplicity of models; and he argues this strength is not terribly well exploited. In order to remedy the situation, economists need to recognize and utilize the multiplicity of models and must become skilled in choosing the ones that are best for a given case. This is his major recipe for improvement in economists' performance. Now the two observations.

First, the multiplicity of models has two dimensions that are easy to conflate: their number and their diversity. It is one thing to have *a large number of models of the same kind*, and quite another to have *many diverse kinds of models* (this distinction is of course a simplification, exaggerating the sharp difference between number and diversity). Models may be of the same kind by sharing a general vision of the causal structure of the world, for example; there are then an endless number of possible models of that kind, different in further details. As we have seen above, they may vary in terms of their level of abstraction: the same or similar causal structure can be represented very concretely, using information about particular cases; or very abstractly, devoid of any concrete contents. And they may vary in terms of what and how much additional contents are incorporated in a shared core model by way of de-isolation or other modifications. None of this is a matter of any deep diversity.

Diversity results from being based on different visions of basic causal structure. Here we don't just have *different versions of a shared ur-model* (based on de-isolations) as above, but *different ur-models* (based on re-isolations). If you just want to magnify the number of models, you could go for different versions of neoclassical models to be adjusted for different domains and uses. On the other hand, if you want diverse models on some subject, you should consult "schools" such as Austrian economics, Marxian economics, Post-Keynesian economics, Minskyan economics, Complexity economics, Computational economics, Network Economics, and so on. These schools have different visions of the fundamental causal structure and dominant mechanisms governing the economy, as well as of how to model them. Within each of them, one can enlarge the number of models.

A way to avoid conflating diversity and mere multitude is to draw attention to *the contents of background theory or theories* that inform the models built and selected (often these models are also models of those theories, after all). Rodrik's account pays little attention to the role of background theories (but see page 40), and this may be a weakness in its capacity to domesticate some critical views. He himself does not recognize any such weakness, he rather appears to be optimistic about the consequences of theoretical diversity, believing that "Insights of these alternative perspectives are, in fact, readily accommodated within standard modeling practices of economics…" (208) I would think a little more detailed persuasion is needed to convince many critics who endorse those alternative perspectives. They need to be convinced that the "standard modeling practices of economics" are not affected by the contents of some disagreeable background theory to such an extent that the accommodation of the alternative perspectives results in their distortion. Just think of the "standard modellings" of Keynes's *General Theory* and how these have been contested as distorting the deeper vision included in Keynes's theory.

The second observation I want to make is about the practicality of Rodrik's repeated prescription that economists should consult a large pool of models and exercise careful judgement about the applicability of each of them to a given case. This rule is difficult in practice especially if the pool exhibits high degrees of genuine diversity. Consider model selection as a challenge of constrained choice. Economists are constrained by various resources available to them, such as time, energy, attention, contents of preceding education, sunk costs due to preceding experience and effort, and so on. All of these tend to be limited in ways that restrict the range of models that economists are able or prepared to peruse and consider as members in the choice set. And since their audiences largely consist of other economists in similar situations, academic network externalities tend to reduce and homogenise the set of models attended, and so their range of diversity tends to be rather narrow. The incentives tend to become weak or negative for economists to obey Rodrik's prescription, and this is reinforced by the larger cultural context of modelling that evolves to support the practice, for example due to publication pressure.

8 Economics fundamentalism?

I will now return to an issue we have addressed from the beginning, that of economics contra economists. As we have seen, Rodrik's idea is that economics is fine, but the attitudes and behaviour of economists often are not. Economists need to be taught about the genuine substance and spirit of economics, and once they have learned it, the problems disappear.

Consider an analogy, all too familiar and perhaps somewhat embarrassing. It is often argued that Religion *Rel* is just fine, respectful for the basic human values decent people are supposed to share, prescribing and supporting virtuous conduct by its adherents. The problem is with *RelF*, a deviant fundamentalist misunderstanding and misuse of *Rel*, pushing fundamentalists towards narrow-minded, excessively dogmatic, intolerant, and even violent attitudes and behaviour. One issue, as we have learnt in the context of religion, is *whether the fundamentalist version is a part of, or implied by, its non-fundamentalist counterpart*. If we think it is, it will be difficult to defend the claim that the latter is just fine and the problem only lies in its fundamentalist counterpart. If we think it is not, then the claim may be justified – even in case a vast majority of adherents and practitioners of *Rel* & *RelF* are fundamentalists.

We have seen that Rodrik makes observations about economists' attitudes and behaviour in ways that also apply to fundamentalism in general, viewing economists as arrogant, over-confident, dogmatic, excessively attracted to simplistic interpretation of the canon, suffering from hubris and overreach, inclined to universalize one narrow doctrine, holding sharp distinctions between in-group and out-group perspectives, prone to hostile responses to criticisms from outside the in-group, under-appreciation for diversity, and so on. I propose such features are characteristics of *Economics Fundamentalism* as an analogue of religious fundamentalism: a misunderstanding and misuse of otherwise virtuous economics as Rodrik sees it.

Economics fundamentalism does not imply *market fundamentalism* but has the latter as a special case that features similar attitudes and behaviour in regard to the applicability of market models and market solutions to social issues. Large segments of economists hold these attitudes, but is this part of economics? Again, Rodrik would seem to be of two minds. On the one hand, he seems to concede that market fundamentalism is included in economics: "Promoting markets in public debates has today become almost a professional obligation." (170) This can be taken to mean that market fundamentalism has (almost) become part of the discipline. Disciplines are the institutional structure that involve definite norms and behavioural conventions; professional obligations are in that family.

On the other hand, Rodrik puts forth the opposing idea in an uncompromised manner. Market fundamentalism is "*not what economics teaches*" (17; italics added); and economists "preaching universal solutions or market fundamentalism […] are, in fact, *not being true to their discipline*. Such economists deserve their fellow economists' rebuke as much as outsiders' reproach. Once this point is recognized, many of the standard criticisms [of economics] are nullified or lose their bite." (178; italics added) This is analogical to claiming that

> [*RF*] Religion *Rel* is fine, but its fundamentalist counterpart *RelF* is not. Adherents to *RelF* in no way exemplify *Rel*, they rather exhibit a misunderstanding of *Rel*. Criticism of *RelF* hence entails nothing about *Rel* itself.

Rodrik would hope the generic version of [*RF*] applies to economics fundamentalism too, such that the sins of economics fundamentalism will not taint economics itself. How could this hope be fulfilled? A popular defence of [*RF*] has two components in it: [i] the principles and practices of *Rel* are virtuous (unlike those of *RelF*); and [ii] the adherents of *RelF* are in a small minority relative to those of *Rel*. Could this work in the case of economics?

As for the analogue of [ii], it is not clear what estimate Rodrik might have of the proportion of fundamentalist attitudes and behaviour in economics. Yet it seems he does not think fundamentalists are just a small minority. He complains that "many" economists "often" violate the principles of economics. It is of course hard to come up with anything but casual observation in support of such estimates, but Rodrik also cites data that show that above 90% of economists hold ungrounded consensus views on issues such as rent controls and free trade, in violation of the appropriate principles of modelling (149). This suggests that it may be hard to fulfil condition [ii]. Obviously, the condition is formulated vaguely in terms of 'small' which creates some flexibility. How small is small enough for the purpose? 90% probably is not.

As for [i], we need two further distinctions. One is between *the very idea and ambitions of surrogate modelling* with the capacity of achieving relevant resemblance between a model and its target; and the various *conditions of surrogate modelling to succeed*, such as holding and applying an adequate model commentary and the incentive structure of the social organization of economic inquiry being in appropriate shape. The other distinction is between *normatively ideal rules and conventions* and the *conventions exemplified by actual behaviour*.

Rodrik keeps telling us that, as a matter of empirical observation, the conventions governing economists' actual behaviour are not in great shape and that the conditions of successful surrogate modelling are generally not met. Hence economics as actually practiced is not so fine. On the other hand, the ambitions and capacities of surrogate modelling are fine, and so are the normatively ideal conventions of model building and model selection. *The ideal discipline of economics is fine, but the actual discipline is not.* Virtue does not lie in the actualities of economics but rather in its ideals and capacities as envisioned by Rodrik. On what conditions might this be sufficient for meeting condition [i] that would help him keep fundamentalism out of his portrayal of the discipline of economics? I wonder if it would be too generous simply to require that there be a feasible strategy of re-designing the institutions of economics, perhaps re-educating the economic profession, imposing an ethical code of conduct, and the like – then allowing him to defend such a non-utopian would-be economics and thereby to expel fundamentalists to the margins?

Let me end with another apparent tension in Rodrik's reasoning. On the one hand, Rodrik's attribution of fundamentalism to his fellow economists is based on the presupposition that there is some authentic, genuine, uncompromised, untainted discipline of economics. Deviant fundamentalists are "not true to" this discipline, and are not attentive to what it "teaches" – the contents of which Rodrik knows. Doesn't this itself exhibit an inclination towards fundamentalism? An obvious characteristic of fundamentalism, after all, is that of believing to be in privileged possession of the one and only correct interpretation of a system of thought, then using this interpretation to argue that anyone holding views or behaving contrary to it is not a genuine advocate of that system – "is not true to the discipline".

On the other hand, Rodrik's views of the contents of the ideal untainted discipline of economics could be taken to relieve him from the charge of being a fundamentalist himself. His insistence of humility is the key. "Economists who remain true to their discipline […] are necessarily humble." (209) This presupposes that there are normative principles characterizing the ideal discipline of economics that are set to discipline economists towards humble behaviour. While arrogant dogmatism – fundamentalism -- is harmful for interdisciplinary interactions, "Humility would also make economists better citizens in the broader academic community of social science." (209)

9 Final remark

There is one major conclusion implied by the foregoing (and Rodrik's book) that should be applauded by experts in the philosophy and methodology of economics. Economists are desperately in need of a better self-understanding, a more adequate portrait of their discipline, including its methods of modelling. The portrait of economics offered by philosophers of economics may sometimes be too refined for practicing economists, but the degree of refinement in the self-understanding (including the model commentaries) currently held by practicing economists is often too low. In securing an optimal degree of refinement, economists would do wisely by consulting philosophers of economics, engaging in collaboration with them, inviting them to give seminars and lecture courses, even recruiting them to their ranks. Thank you for paving the way, Dani Rodrik!

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