A plea for reorienting philosophical attention from models to applied economics

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Criticism of traditional economic theory

In the past, conventional economic models have been criticized for their inability to explain and predict, as well as for the difficulties in applying them to specific economies. It was assumed that these shortcomings were due to the "unrealistic" nature of some of their assumptions, and for decades this reason has been advanced to dismiss their practical utility and the possibility that they could be true. Musgrave (1981) makes an important contribution to this debate showing that many of the statements used in the formulation of economic models – which were considered false when interpreted in a too literal sense – properly understood mean something completely different that may also be true. A similar approach is advocated by Lipsey & Steiner, 1981.[1] Recently, new arguments have been offered that improve our understanding of the role of different classes of stylized assumptions (i.e., idealizations, distortions and omissions) in the construction of economic models (Mäki, 2002, 2008, Weisberg, 2007). These contributions have been successful in addressing the old-fashioned "realistic" attack on conventional economic models, showing that they cannot be dismissed as carriers of relevant information about the world on the basis of these reasons, and there is nothing inherently wrong in the practice of *mainstream* model building[2].

However, these arguments relate only to the question of the truth-status of economic models, not to their practical utility. Their nature is, so to speak, *negative*: they are arguments designed to criticize the arguments of the critics. Although they are able to defeat the rather naïve objections of the old-fashioned critics, they fail to dispel the concern that many practicing economists feel regarding the questionable performance of economic models. Since the beginning of the XVII century, many economists of different orientations have expressed their dissatisfaction with the course adopted by economic theory and their concern about its regular procedures. It is not a coincidence that Mill's influential defense of the scientific character of economics focused on its "abstract" nature, taking as granted that it could not be successful in the domain of real world ("concrete") phenomena.

Apparently, this discomfort regarding the merits of economic theory is still felt, despite the efforts of current *mainstream philosophy of economics* (MFE), which attempts to exhibit the rationality of mainstream economics' practices and provide epistemic legitimacy to standard

[1] "Consider a theory that assumes the government has a balanced budget. This may mean that the theorist intends that theory to apply only when there is a balanced budget; It may *not* mean that the size of the government's budget surplus or deficit is irrelevant to the theory"(Lipsey and Steiner, 1981, p.24).

[2] Maki's rejection of traditional criticisms of economics asserts that economic models, even those that isolate ideal mechanisms, *can* be true. This is a *possibility* because in fact he makes no claim that these models are indeed true. Maki's arguments are presented in the form of "even-if arguments", stating that, even if certain conditions in the formulation of a model are met, leading us to say that such models are false, they may still be true (see Mäki, 2008). Statements of this kind are the argumentative core of Mäki's minimal realism. They offer a critique of the most common objections to the traditional way of modeling, and in this sense they are just *negative* arguments in the sense intended here. To the extent that these arguments are successful, they only show that the reasons usually directed against the epistemic relevance of traditional economic models are not good for that purpose.

models. See, for example, the words used by Mäki at the beginning of his *Facts and fictions in economic theory*:

> Fact or fiction? Is economics a respectable and useful reality-oriented discipline or just an intellectual game that economists play in their sandbox filled with imaginary toy models? Opinions diverge radically on this issue, which is quite embarrassing from both the scientific and the political point of view... Economics is a contested scientific discipline. Not only are its various theories and models and methods contested but, remarkably, what is contested is its *status as a science*.... Suppose we take one of the characteristics of science to be the capability of delivering relevant and reliable information about the world. Suppose furthermore that this is not just a capability, but also a major goal and actual achievement of whatever deserves to be called by the name of 'science'. How does economics do in this respect? This question is about as old as economics itself (Maki, 2002, p. 3).

His stance is rather odd, because these questions do not arise in the case of other sciences. Who might seriously doubt that physics or biology do provide "relevant and reliable information about the world"? Therefore, it seems that there is a particular difficulty for making sense of theoretical practice in economics. In particular it has not yet been properly clarified in what sense it is applicable to real economies, although this has been the focus of much debate for more than two centuries. Apparently, *negative* philosophical reasons suffice to get rid of the usual criticisms of mainstream economic models. But these reasons are not sufficient to help economists feel proud of the concrete results of their models, and for philosophers of economics to provide a convincing justification of the capability of conventional economic models to suggest and underpin economic policies. To achieve these goals additional (this time *positive*) reasons should be offered.

Let me be clear about the nature of the positive reasons that are needed. They belong to two main kinds:

> a) Some of them could be advanced in order to showcase where a particular content of economic models provides understanding of some aspects of real economies. Apart from exceptional cases of very appealing models (like those of Shelling and Akerlof that have been frequently commented on in recent literature)[3] this issue remains largely unclear once the bulk of economics models are considered. Even in the case of those authors that take for granted that economic models offer valuable information about real economies, the nature of this information remains poorly specified and the merits of those models largely unargued. Regarding concrete economies it is still unclear what exactly is learned from models, and why that which is supposedly known through them cannot be learned by other means (venerable traditional theories, common sense or *practical* economic knowledge).

[3] See Akerlof, George A. (1970). The market for 'lemons': quality uncertainty and the market mechanism. *Quarterly Journal of Economics*, 84: 488-500, and Schelling, Thomas C. (1978). *Micromotives and Macrobehavior*. New York: Norton.

b) More importantly, given the problems that assail the empirical testing of economic models and the general problem of external validity, what is mainly required is to show concrete cases of successful economic technologies able to generate unequivocal practical results (I mean, results that cannot be reasonably attributed to other causes than the insights provided by models). If as MFE asserts economic models are the kernel of economic wisdom and the engine of economic progress, philosophical attention must be applied to show where (in which particular cases) and how they contribute to the acquisition of practical results. Physics, biology, genetics and some other undisputed (and reputed) scientific practices are mainly preached not by their successful tests, but by their capability to mold and transform our daily life through related technologies. Can the practice of economics' model building be defended on these grounds?

The insufficiency of positive philosophical arguments to sustain the representational as well as the practical usefulness of mainstream economic models is to a large extent the result of a feature of these models, which relies on a substantial use of a particular type of assumption, called *tractability assumptions* (Cartwright, 1999; Alexandrova, 2006).

This paper examines some of the major solutions that have been proposed in order to avoid the trade-off between the use of tractability assumptions and the external validity of the results that are obtained with their help. It is argued that these contributions have failed both in clarifying what is the usable real world-oriented content that economic models offer, and in showing clear instances of successful applications of economic models (i.e., economic technologies). It is also claimed that these new sympathetic approaches to highly idealized economic models fail to exhibit what exactly are their particular contributions in those applications. Worse, it has been claimed that those solutions rely on a different approach, which enhances the crucial role of background (extra-modelic) knowledge, something that seems to be difficult to accommodate within their shared view of economics as a model-centered discipline. What is needed, we suggest, is to shift philosophical attention from the conundrums of representations to the conditions that must be fulfilled for building a substantial core of successful applied economics.

The problem of "overconstraint"

In "The Vanity of Rigour in economics", Nancy Cartwright examines a special type of economic model, called by Lucas *Analogue Economies*. There is no doubt that these "economies" are *unrealistic* in their construction, as Lucas himself explicitly recognizes: "Any model that is well enough articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently 'unreal'"(Lucas, 1981, p. 271)".

How damaging is this circumstance for the aspiration of applying the results of a model to situations of our world? In the past, Cartwright defended the unrealistic nature of the models within the framework of a taxonomy that recognizes two types of models: those which "establish facts about what is happening in the real economy" and those that "establish facts about stable tendencies". She has argued that the *analogue economies* belong to the second type.[4] We will refer to them as tendency-models.

[4] "....we do not need to assume that the aim of the kind of theorizing under discussion is to establish results in our analogue economies that will hold outside them when literally interpreted. Frequently what

Tendency-models are designed to isolate the action of a single cause and show what its "pure" contribution to the generation of a brute event, which "happens" in real economies, is. In Cartwright's terms, tendency-models do not describe facts at all; they describe the *exercise* of a capacity, not the *result* of this exercise. Now, if the goal is to capture a tendency, "it is essential that models make highly unrealistic assumptions, for we need to see what happens in the very unusual case where only the single factor of interest affects the outcome" (Cartwright, 2007b, p. 219).This result is important because it leads to the conclusion that the reproach of "unrealism", which traditionally has been used to explain the lack of successful applications suffered by conventional economics, is in these cases misplaced.

However, Cartwright's thesis that through isolation it is possible to identify capacities is more a response to the traditional way of objecting to the practice of modeling in economics than a defense of conventional models; because according to Cartwright *it is* possible to direct against *analogue economies* a more sophisticated criticism, showing that *there is* a problem after all in this way of modeling. Although all models distort reality, this effect may be due to two basically different strategies: to *omit* some factors present in the target system or to *add* to the model factors that are absent in the target. Subtracting and adding are very different activities.[5] The use of additions can endanger Cartwright's usual strategy for defending the practical relevance of "unrealistic" economic models, which asserts that they reveal the "pure" contribution of a cause (or a set of causes) to an observable effect in our world. To see where the difficulty lies, it is useful to distinguish two types of "idealizations" in economic theory:

> (a) Galilean idealizations, which omit any possible interference to the action of an isolated cause;
> (b) Non-Galilean idealizations, which are used to incorporate within the model features that do not have any counterpart in the targeted real economies.

The first are beyond reproach, according to Cartwright, since they are necessary to find out "capacities".

Let us call this kind of idealization that eliminates all other possible causes to learn the effect of one operating on its own, Galilean idealization. My point is that the equivalent of Galilean idealization in a model is a good thing. It is just what allows us to carry the results we find in the experiment to situations outside – in the tendency sense. "We need the idealizing assumptions to be able to do this". (Cartwright, 1999, p. 12).

The idealizations of the second type are necessary to reach (deductively) precise and well-defined results. They are, however, a source of problems because they exacerbate the trade-off between internal and external validity. Cartwright offers two arguments to justify why Non-Galilean assumptions are problematic in this regard. The first one focuses on the *amount* of this type of supposition. She holds that economic models "are complex, at least by comparison with physics models doing the same kind of thing: they have a lot of structure. The list of assumptions specifying exactly what the analogue economy is like is very long"

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we are doing in this kind of economic theory is not trying to establish facts about what happens in the real economy but rather, following John Stuart Mill, facts about *stable tendencies*" (Cartwright, 1999, her italics).

[5] It seems counter-intuitive, but a simplified world simpler than the actual one can be represented within a model either by omitting or adding factors. Additions can cause special problems, however. This is particularly true for models of trends, which assume that the objective of the model is to exhibit the "pure" or "natural" capacity of some factor.

(Cartwright, 1999, p. 15). However, the main problem brought out by the incorporation of Non-Galilean assumptions comes from their non-representational nature, which is conditioned by the goal that these assumptions are supposedly helping to reach: precise results by perfectly deductive means. Galilean Idealizations respond to the interest of providing a simplified representation of reality; Non-Galileans Idealizations, on the other hand, introduce rather arbitrary specifications just for the purpose of allowing or facilitating inferences and achieving consequences with deductive accuracy. In the first case factors which are supposed to be present in reality are omitted, while in the second factors which are regularly assumed not to have a counterpart in reality are nonetheless included.

What is then proved in this kind of model is that factors C isolated within them generate a result R in the presence of (exceptional) conditions N, which are posited with the only purpose of reaching R with deductive rigor. But then the contribution of C to the generation of R in the framework of the model does not give us information about what would be the contribution of C to the generation of R in real (concrete) economies, in which N supposedly are not present. It cannot be now assumed that the *capacity* of C, discovered within the model, is an ability that C would still have whenever it is operating out of the model (in real economies):

> What I want to talk about today is a problem that can beset real and thought experiments alike and in both physics and in economics. But it is a particular plague for thought experiments in economics, I shall argue, so much so that it regularly undermines the use of models to establish capacity claims.That is the problem of *overconstraint* (Cartwright, 2007a, p. 73).

The problem with tractability assumptions

As we have just seen Cartwright considered that the phenomenon of “overconstraint” puts into question the applicability of economic models to situations of our world. But it may be thought that this difficulty has only a limited impact and it occurs only in the framework of the ontological assumptions made by Cartwright, in which models are designs aimed at discovering tendencies or capacities. As long as this ontological commitment is controversial, it is important to describe the nature of the difficulty in a more general way, outlining its logical dimension.

Suppose that B describes a relevant and desirable result and A describes a circumstance whose presence in our world is attainable. At the moment, there is no known logical connection between A and B. Now suppose that someone asks what set of additional assumptions would allow one to deduce B from A (just using the ordinary rules of logical construction and derivation). An ingenious individual puts his mind to work and finds that assuming an arbitrary set of assumptions C (which are only restricted by logical considerations) A implies B. Here ends, successfully, the exercise. Conditions C are mere assumptions in the logical sense of the term: they are starting points for the argument. Following Kuorikoski and Lehtinen, (2009) and Kuorikoski et al, (2010), we call them *tractability assumptions*.

What does this demonstration prove in reference to *our* world? More precisely, what is the relevance, if any, of such an argument regarding the applicability of the results thus obtained to real economies? If prior to its construction, we believe that no causal connection between A and B exists in our world, why would the demonstration provided in this exercise contribute to changing our minds? A reasonable response could be that C describes conditions that are *plausible in our world*. But by hypothesis, it is assumed that this is not the case. As a result, the derivation does not contribute to the credibility that a causal link between A and B exists. It seems that we are facing a dilemma. If previous to the logical exercise we do believe in the existence of a causal nexus between the aforementioned factors its rigorous demonstration does not add anything to our conviction; but if at the beginning we do not believe in the existence of a causal connection between A and B the exercise does not compel us to change our mind. What then is gained by the proof of the connection between A and B given that it has been obtained at the expense of extraordinary (can I say "unrealistic"?) circumstances?

The presence of tractability assumptions poses a problem for those who wish to defend the epistemic relevance of economic models. Two types of solutions have been proposed. The first, which we call "internalist", argues that certain operations carried out within models (i.e., inside what Sugden calls the "model world"), particularly derivational robust analysis, may show their epistemic credence[6]. We find this attempt unsuccessful and misleading, but in this paper we will not examine this claim. "Externalist" solutions, on the other hand, argue that in order to acquire epistemic relevance economic models have to be supplemented with some kind of external knowledge. In the following sections three different strategies to sustain this view will be examined: (a) the interpretation of models as parables (Cartwright); (b) the suggestion that what is needed is to train suitable interpreters (Colander), and (c) the concept of models as open formulas (Alexandrova). Despite their differences, all of these views are based on the assumption that economic models including arbitrary tractability assumptions contain reliable and relevant knowledge about our world. The problem, according to these views, is that it is not directly usable: the applicability of economic models to situations of our world crucially depends on the assistance of some kind of background knowledge coming from outside the models themselves.

Cartwright's vision of models as parables

As seen above, Cartwright (1999, 2007a, 2007b) called attention to the problem of the "overconstraint" suffered by economic models of the "analogue economies" type, which generated a trade-off between their internal and external validity. In a more recent paper (Cartwright, 2008) she offers a new interpretation and argues that the trade-off may not take place after all. To reach this conclusion she contrasts two ways of understanding economic models: as fables and as parables. She argues that fables deliver a "message" or "lesson" that is explicitly formulated within the text. Parables, however, shed (or perhaps it would be better to say "suggest") a lesson, that is not contained in the model itself, but must somehow be built from the outside taking into account relevant portions of available background knowledge. This means that models can have a "correct" lesson within them, but it must be partly construed out of the materials provided by the model on the basis of theoretical and extra theoretical knowledge. Models deliver a lesson that despite being abstract in nature may

[6] There are two main versions of this position. On the one hand the autonomist view of Knuuttila who reconsiders the concept of epistemic relevance, untying it from any reference to our world. On the other hand, derivational robustness analysis, as understood by Kuorikoski, Lehtinen and Marchioni (2010), replaces the comparison between a model and its intended target by the comparison between different versions of a basically identical model. In this case the strategy is to build a family of models and claim that the derivational robustness analysis allows to identify existing causal connections in our world simply by comparing the members of the family.

be applicable to the specific economies of our world. Her vision of models as parables can be understood as a new strategy in the broad project of mainstream philosophy of economics intended to "recover the practice" of mainstream model building.

Cartwright's new vision is consistent with an idea that she has advanced before (see for instance Cartwright, 2007b): economic models do provide valuable informative content, and if there is any doubt about what their epistemic relevance is, the problem lies not in the models themselves, but in our difficulties for developing *another* type of knowledge able to reveal to us *how to use* a models' information. This is precisely what happens with parables. To identify their "lessons" and be able to apply them to situations of our world the use of background knowledge is crucially required. Her defense of the epistemic relevance of economic models whose results depend on the discretionary addition of tractability assumptions is unconvincing, however. Let me mention some problems of this vision.

(1) There is no guarantee that such models will deliver a "correct" abstract lesson (i.e., a lesson applicable to the real world). On the other hand, even if models contain materials for building the right lessons out of them, there are no rules for identify them unequivocally. Besides, the lessons that models could suggest, being dependent on the particular state of knowledge which prevails at the moment, may vary according to times and places, and are always subject to revision.

(2) The lessons and applications that models facilitate are no longer based on the *consequences* obtained in the model but on other, more abstract content, which is not deduced from the model, but is "inferred" or "captured" otherwise. Arguably, then, the problem of overconstraint is not resolved, but is rather diluted by changing the reference point: the *consequences* are still over-constrained (since Cartwright is not advocating a change in models, but in their interpretation), but now the focus is placed not on them (or their applicability), but in *lessons* which supposedly do not depend on the set of tractability assumptions.

(3) To spread their message, parables do not need to deliver rigorous proofs, and much less have recourse to the employment of advanced math or heroic idealizations. In fact, it seems that the "lessons" that economic models offer could be obtained without having to impose deductive power within the model by adding strategic tractability assumptions. Why do modelers send messages or lessons through analogue economies? If the epistemic value of models resides at a more abstract level, what is the purpose of over constraining their results (often with many tractability assumptions)? It seems that a more informal argumentation would be enough (and surely the lesson so delivered would be clearer).

(4) A potential problem of Cartwright's shift from methodology and epistemology to literary analysis is that parables, as many everyday sayings, are not only ambiguous in their content, but frequently suggest opposite lessons that contradict each other. One can then choose the lesson that best suits his interests or the particular occasion. This pliability of the parables could certainly explain the ease with which applications for economic models are found and their epistemic relevance taken for granted. I can't tell whether this plasticity should count as a credit or a defect.

Interpreting economic models

In his article "The economics profession, the financial crisis, and method", David Colander focused his analysis on the performance of "the dominant dynamic stochastic general equilibrium macroeconomics model" (DSGE) regarding the global financial crisis of 2007-2008. This crisis was so deep that "the world economy came perilously close to a systemic failure in which a financial system collapse almost undermined the entire world economy as we know it" (Colander, 2010, p. 1). In this case his analysis refers not to "analogue economies", but to a type of model designed to be applied to a particular situation of our economic system. It is then interesting to see how its performance is evaluated. Colander holds a moderate position, pointing out, like Cartwright did regarding analogous economies, that such a model has valuable information about the world which deserves to be considered and elaborated[7].

As usual only shortcomings are explicitly mentioned. In this regard he argues that those who were looking at the world through DSGE's lenses were prevented from seeing that conditions for the advent of the crisis were growing inside the economy, despite that "the possibility that a crisis might occur at some point was becoming evident to many observers"[6]. To some degree one could excuse this failure pointing out that after all predictions are usually unattainable in economics. But Colander emphasizes a rather different point. He asserts that "it did not take a rocket economist to recognize problems in the financial sector as the burgeoning sub-prime mortgage market was bringing in less creditworthy buyers. At some point that process of credit expansion had to end". This observation seems to imply that those who did *not* have the help of DSGE's analytical tools had a *clearer* perception of the situation than those who counted on the model's help. Leaving aside the difficulties of anticipating future events, the fact remains that this model has also been useless to *analyze and understand* the crisis *once it was already present*.

The inadequacy of the model for examining the crisis is explained by the purpose of simplifying its object of analysis, which exhibits a substantial complexity. To be tractable "the DSGE model ruled out meaningful considerations of the financial crises by its representative agent and global rationality assumptions". Colander does not have much hope in the strict adherence to this strategy. In his opinion "mathematical modelers should deal with that complexity head on, rather than focus on models that assume much of that complexity away as we believed the dominant dynamic stochastic general equilibrium (DSGE) macroeconomics model did" (Colander, 2010, p.1). Consequently, Colander rejects the ongoing practice of working on models like the DSGE and advocates for developing more sophisticated models, which are characterized as "highly complex heterogeneous agent, coordination failure models that might have been able to incorporate such events as a crisis of confidence".

It could be thought that this (forthcoming) new generation of highly complex models would finally meet the requirement of epistemic relevance. However, Colander admits that future models *will not provide a firm basis for the implementation of successful economic policies* either. As he points out, "models of complex systems do not, and at our current stage of knowledge, cannot, provide definite policy answers – they simply provide guidance to individuals who have real-world experience and a detailed knowledge of the institutional structure".

[7] As he says, the criticism that DGS has received "does not mean that such abstract modeling should not be done; We strongly supported such basic research" (Colander, 2010, p.421).

Colander's view is convergent with the ones offered by other authors that we examine in this paper, like Nancy Cartwright and Ana Alexandrova, in the sense that they all believe that standard economic models do provide some type of useful and enlightening knowledge, even if this knowledge is not directly linked to situations of our world. Therefore, in all these cases the relevant question is *how that knowledge should be used to obtain practical results and build successful economic policies*. What distinguishes Colander's views from other opinions is the nature of the proposed solutions, which consist in this case in academic and attitudinal changes.

First, it is advised that economists assume more responsibility when the properties and results of the DSGE model are communicated outside the narrow community of experts and model builders. Particularly, published models should include "an explicit warning directed at the non-scientific users of the model. This warning could include a list of what the researchers see as limiting assumptions of the model, and the researchers' beliefs about whether the model can be used to guide policy" (Colander, 2010, p. 424)

Still more important is his indication that expertise in the use of macroeconomic models requires practical knowledge of the economy as well as other kinds of knowledge and skills, which are different from that involved in the practice of modeling. In particular, Colander proposes a crucial institutional shift, which consists in allocating public funds for training economists in the interpretation of models with a view to their applications.

> Currently, most economists are not selected for their ability to, or trained in how to, 'choose' an appropriate model, or otherwise relate a model to policy. Doing this requires knowledge of a wide range of models, historical knowledge, and institutional knowledge. They are trained almost entirely to produce models. The other ability they must learn on their own. By design graduate training has eliminated those courses, such as the history of economic thought, methodology, economic history, or courses surveying literature, that are most relevant for training students to choose among, and interpret models...A potential solution to this problem is to increase the number of researchers trained in interpreting models rather than developing models. This would mean viewing the interpretation of models as a separate skill from producing models. If a funding agency were to provide research grants specifically to interpret models, that problem could be somewhat alleviated. In a sense, what I am suggesting is the creation of an applied science subdivision of the National Science Foundation's social science division. This subdivision would fund research on the usefulness and interpretation of models. (Colander, 2010, pp. 425 – 426)

Colander's proposal is original and interesting, but somewhat understates and dilutes the role which, according to mainstream philosophy of economics, models play in the production of relevant and reliable knowledge about the specific economies. Colander's perspective makes the potential usefulness of this knowledge heavily dependent on the acquisition of other kinds of knowledge whose source and legitimacy was originally contested. In fact, Mill, Senior and many other economists of the past claimed that economic theory was scientific in the sense that it went well beyond the knowledge of economic affairs available to ordinary people, historians of economics and entrepreneurs. But it seems that Colander's perspective reenhances the role of the very kinds of knowledge that were thought superseded by economics' theoretical practice.

Technological use of economic models. Towards a more applied economics.

Recently, Ana Alexandrova has defended a new vision concerning the role that economic models play in the implementation of economic policy. In it she limits her analysis to one particular model that is praised as a paradigmatic case of successful application in the design of institutions, the auction model (Alexandrova, 2008; Alexandrova and Northcott, 2008). Her purpose is to explain what its contribution is in the achievement of this goal. She argues that the main existing rival views about models' applicability are not useful in this case. Alexandrova's approach is a promising way to defend the practical relevance of economic models, suggesting, at the same time, a more general way to appreciate what exactly the applicability of models that incorporate tractability assumptions depend on.

Economic models can be used to represent (and be applied to) a certain target T. According to Alexandrova, there are two main views that seek to give an account of their applicability: the "satisfaction of assumption" account, which is attributed to Daniel Hausman, and the "capacity account" developed by Nancy Cartwright. According to them, a model represents (and is applicable to) T when, respectively, its assumptions are satisfied in T or the causes described in it occur in T. To illustrate her position, let's express it in Hausman's concept: If a model M contains assumptions, some of which are idealizations (we read: tractability assumptions), this fact prevents them from being strictly true in T. But in that case, according to Hausman, it is possible to gradually de-idealize those assumptions until they match the relevant characteristics of the "intended target". De-idealization allows models to be applicable and to acquire empirical content.

Alexandrova (2006) points out that this strategy is only possible in some cases because it is not always possible to de-idealize the tractability assumptions incorporated into a model. She says, for instance:

> In what sense is it more realistic for agents to have discretely as opposed to continuously distributed valuations? It is controversial enough to say that people form their beliefs about the value of the painting or the profit potential of an oil well by drawing a variable from a probability distribution. So the further question about whether this distribution is continuous or not is not a question that seems to make sense when asked about human bidders and their beliefs (2006, p. 183).

Her proposal is then intended to "recover" the practice of model building in those cases in which de-idealization cannot be followed. She tries to give an account of how, despite this limitation, economic models can be applied successfully to obtain desired economic institutions and practical results. To examine her vision let me describe a model M in this sketchy way:

Given C1,..., Cn, a certain characteristic F causes behavior B (1)

A more synthetic way of expressing its content is:

"Under conditions C, F causes B" (1')

Note that M is a *closed* formula, in the sense that all of its assumptions are specified. But one thing is the model and another its use. Alexandrova points out that M can be *used to build a hypothesis* in which only *some* of its assumptions (or *none* of them) are specified. The hypothesis has this form:

Under conditions X (that *may or may not* include conditions C), F causes B (2)

In (2), C has a purely notional presence, since it may be completely undetermined. For this reason she proposes considering models as *open formulas*. Strictly speaking, the content of (2) boils down to the following:

(In our world) There are conditions X, where F causes B (2')

In (2') F and B are conditions whose properties are known and X is the unknown variable whose "values" have to be found. Interestingly, the original model, which suggests the hypothesis (2) and (2'), does not provide any clues for discovering those values. In fact, the model itself contains no hypotheses such as those made in (2) or (2'). They are independent from the model, though inspired by it. From this point of view the model has no real world informative content of its own: it is rather considered as a source of hints, tools and resources for generating hypotheses about the world. In Alexandrova's words an auction model functions as a "framework or heuristic for formulating hypotheses".

But how workable is such a heuristic? Is it really a form of heuristic after all? Suppose that "B" is a desirable outcome and "F" is a state of things, which we can implement in reality. Suppose then that a model M proves that under conditions C, F causes B. C describes a set of conditions that are logically *sufficient* to ensure such an outcome. The epistemic significance (relevance) of the model seems to depend strongly on the feasibility of conditions C. What is then accomplished by rigorously proving that "F causes B" if it is obtained at the expense of introducing arbitrary assumptions, which supposedly describe a situation that is absent in real economic environments? It seems that such a proof contributes nothing to identify what conditions should be found or created in *our world* to get B to guarantee F. We are in a situation that seems to be very close to that of the logical exercise outlined above.

In circumstances like these the technological moment comes to occupy the center stage in Alexandrova's account[8]. Starting from (2') that ensures that there are (unspecified) conditions out of which "F causes B", economists with practical orientation (and a host of other skillful people) can put their hands to work and try to find out concrete conditions C* (other than C), that can be implemented in our world and have the property that once imposed make F

[8] It is important to be clear about the particular type of laboratory experiment that concerns Alexandrova. Her analysis focused exclusively on the role of experimentation regarding *technological applications*, not for the purpose of testing models as this activity is usually understood. This is why she distinguishes between "test" and "testbed". The test of a model consists of creating or finding a situation in which model's assumptions are met, and see if their results are also obtained. In a testbed, on the other hand, it is known or supposed that the assumptions of the model are *not* satisfied. Its purpose is to obtain the same results obtained within the model from different or additional conditions than those referred in the model. Alexandrova's testbeds enhances the role of applied economics and the autonomy of the achieved results regarding the particular conditions described in models. Testbeds are better described as a practice performed by economic engineers than by experimental economists in the traditional sense of the term.

results in B. In fact, the main claim of Alexandrova is that this achievement has already been obtained with remarkable success in the case of the auction model. Maybe Alexandrova is right on this point, but since in her account models merely inspire (2') and do that in an extremely vague form, there remains the philosophical problem of assessing what exactly auction models' contribution is to the solution of the question raised by the hypothesis. Did the economic engineers referred to in Alexandrova's account need rigorous proof like the one provided by the model to find particular conditions C* under which doing F B is obtained in our world? Did they find, at least, a clue in the model to imagine the specific content of the set C*?

If "F causes B" is a desirable conclusion, there seem to be two different research programs concerning this result. One mathematical (logical): search for *any* conditions C under which the result could be deduced. Another, practical: find conditions C* feasibly implemented in our world such that the production of F leads to B. Unless a clear connection between both programs can be exhibited (something that Alexandrova's paper fails to show) to get busy in building models diverts resources from the technological approach of directly "building" in practice the desired result. This construction, it seems, does not need at all any of the solutions offered within the model.

Conclusions

The points of view examined in this paper agree in that actual conventional models that incorporate tractability assumptions provide *some* relevant information, but they must be supplemented with other types of knowledge, skills and practices if such information is to be successfully used in real world economies. Beyond this coincidence these views differ in the type of extra theoretical resources that are needed. For Cartwright models offer "lessons", which have to be extracted using pre-existing backward knowledge coming from outside the models themselves. Colander is more specific arguing that expert interpreters of current macroeconomic models are crucially needed, emphasizing the importance of having historical knowledge, methodological skills and experience in the analysis of particular situations. The economist-engineers portrayed in Alexandrova's account, on the other hand, are men of action. Thanks to the cooperation of other experts not necessarily economists, they can make – by trial and error-creative contributions to the design of institutions invested with economic relevance.

A major success of the perspective of Alexandrova, which in my opinion makes it superior to the rest of the views examined in this paper, is that she relates the epistemic relevance of economic models to their *practical applications*. From this point of view it is the social technology that the models help to generate which gives them credence as tools for achieving relevant knowledge. Indeed, if a discipline provides "resources" (models in this case) that contribute to successful technological devices (institutions, in this case), this is a clear indicator that this discipline brings out relevant and reliable knowledge (and some may feel entitled to apply to it the label of "science"). That is what has happened with physics, biology, and more recently with genetics. Also in the case of economics their practical applications are crucial, and so it is necessary to have a successful associated engineering. The problem is that, unlike what happens with the aforementioned disciplines that undoubtedly have contributed to an enormous amount of successful practical applications, the contribution of economic models to the generation of social technologies is still equivocal and needs to be properly examined. Indeed it is not clear whether there are or are not successful social technologies. But even granting that they can be found in real economies, it remains unclear what exactly the contribution of models with arbitrary tractability assumptions has been in such cases. Philosophy of economics may be extremely helpful on this issue. One major contribution would be to shift philosophical attention from the intricate details of representations (models) to the conditions that have to be fulfilled for building a substantial core of successful applied economics.

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