

willing to trade on equal terms, and discussions and collaborations have grown in the recent past with the flowering of journals, academic courses and initiatives – such as the Pisa workshop – devoted to the interrelationship between the two disciplines.

To offer just a few examples of the new research agenda which triggers frequent debates and collaboration between philosophers and economists, it is worth calling attention to:

- 1) the upsurge of studies in the epistemology and methodology of economics, both theoretical and experimental;
- 2) the renewed interest in the ethical dimensions of economic behavior, from the philosophy of happiness to business ethics;
- 3) the growing awareness of the relevance to economics of new findings in psychology, cognitive science and neuroscience.

The papers gathered together in this collection provide insightful viewpoints on some of these subjects. Four are specifically devoted to great economists – John Maynard Keynes, Milton Friedman, Herbert Simon and Ludwig Lachmann – confirming the connection between the history and the methodology of economics; the others deal with topics involving the fundamental concepts, theoretical models and empirical content of economics.

The workshop, the participation of colleagues and students and this publication have given us a unique opportunity to establish and strengthen worldwide connections as well as to promote cooperation in research and teaching among colleagues who live side by side, often ignoring each other's work, within our University and in the wider circle of the Universities and research institutions of Pisa and Tuscany. This is an encouragement to promote new initiatives and enhance the potential of our area as a permanent centre of activity and research in economics and philosophy.

Remarks on Models and Their Truth

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1. Introduction

Economists often seem to hold the belief that it is in the nature of a model that it cannot but be false. Once this view is adopted, any further truth talk in connection to models becomes obsolete or irrelevant. I want to resist this conclusion. I grant that a model may appear to be false in the sense that the world does not seem to be the way it is being represented to be in the model. In earlier work, I have entertained the idea that what appears to be false may be true after all, and I have shown some situations in which there is an obvious way of travelling from one to the other, from the appearances of falsehood to possible truth.

I suggest to distinguish between two strategies of bridging this apparent gap. One is based on revising one's idea of the relevant truth bearers while sticking to some non-epistemic correspondence conception of truth. The other is based on revising one's conception of truth by substituting the non-epistemic conception for some epistemic conception of truth that defines truth in terms of our ways of recognising it. So these are the two options:

- [1] A model only appears to be false because one has misunderstood the relevant truth bearer – the precise claim to which truth (or falsehood) should be attributed – intended when using the model. Once the intended truth bearer is appropriately located, the model will have a chance of being true.
- [2] A model only appears to be false because one has applied a misguided non-epistemic conception of truth in its assessment. Once this conception is appropriately revised in favour of a suitable epistemic conception of truth, the model will have a chance of being true.

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What is offered in this paper is an outline of parts of the reasoning that is needed to spell out the above two strategies of attempting to reveal truth beneath apparent falsehood. The sketches will be not only incomplete but also asymmetric – for I am in favour of strategy [1] and disinclined towards strategy [2].

So we are dealing with models, their truth, and our ways of finding out about their truth. Indeed, in order to see the relevant issues clearly, we must see that the following three themes and questions are involved:

- [A] The nature of models: What is it that makes a model a model?
- [B] The nature of true models: What is it that makes a true model true?
- [C] The criteria and means of recognising true models: What is it that makes a true model recognisably or justifiably true?

I am distinguishing these questions from one another also because there seems to be a notable tendency towards conflating the three in ways that obscure the challenge of aiming at building and recognising true models. Such conflation is exemplified by statements like, “it is just a model, thus cannot possibly be true” (conflation of [A] and [B]); and “this model is consistent with theory and all available evidence, therefore true” or “this model is found persuasive by virtually all economists, therefore it is true” (conflation of [C] and [B]). By distinguishing the three questions from one another I want to highlight the twin ideas that the mere modelhood of a model does not imply that it is false (or true) nor does any successful (or unsuccessful) application of test criteria to a given model imply that it is true (or false). It should also be noted how the two strategies [1] and [2] are connected to the three questions [A]–[C]: strategy [1] deals mainly with questions [A] and [B] and their relations, while strategy [2] focuses on questions [B] and [C] and the way they are linked.

My recommendation hence is that in order to answer the second question [B] – the main question of this essay – we need to be careful in distinguishing it from the other two questions [A] and [C]. My view of how the three questions are related includes this: in order to answer [B] we need to have answered [A]; and in order to answer [B] we must avoid conflating it with [C]. The sections to follow seek to give clues as to why I think so.

2. What is a model?

So what is a model? What is it that makes a model a model? The question is not easy to answer. Not only is there a vast multiplicity of kinds of models, there are numerous rival and complementary philosophical accounts of models – of what they are and how they function (see Hesse 1963; Achinstein 1968; Wartofsky 1979; Morgan and Morrison 1999). What follows is a simpli-

fied summary of how I prefer to view models (Mäki 2001, 2005). I believe this view will help highlight features in models that are relevant to discussing the issue of their truth.

A model is a model *of* something else (some models are models *for* something to be designed and built such as models used in mechanism design). Depending on what that something else is, we have, for example, models of theories, models of the data, and models of real mechanisms. In each such case, a model is a substitute system that serves as the direct subject of inquiry instead of the ultimate target system (theory, raw data, complex reality). The epistemological point of building and using models as such substitute systems is this: one directly examines the properties and behaviour of a model in order to indirectly acquire information about the ultimate target system (from now on we focus on target systems in the real world).

Almost anything can serve as a model of almost anything else, indeed there is quite some flexibility in what models are made of. Rats can serve as models that are directly examined in an attempt to anticipate how humans will respond to certain new kinds of medication. A set of equations may serve as a substitute system that is directly examined to acquire information about the origins of the universe or about the consequences of a rise in the tax rate. A drawing of boxes and arrows can be a model of an important network of causal connections in a social process. Or a model may be an abstract object such as the system of perfect competition (described using equations or drawings or just words) serving as a substitute for real-world market structures.

Another way of putting the idea of models as substitute systems is to say that models are representatives of the target systems. This connects with the idea that models are representations. I take the relevant relationship of representation to have two aspects. One is the *representative* aspect: models stand for their target systems as their representatives. Rats stand for humans as their representatives just as sets of equations and boxes and arrows stand for their respective target systems as representatives. The other aspect of representation is that of *resemblance*. In order for a model to serve as a good representative, it must resemble its target system in relevant respects and sufficient degrees. Rats must be sufficiently similar to people, and the connections between the boxes must be sufficiently like certain causal connections in social reality. An epistemically good representative is one whose direct examination will indirectly produce adequate information about the target system. For this to happen, the two systems must resemble one another. But any claims about resemblance (that it is there, or that it isn't) are characteristically contestable. Using something as a representative of something else gives rise to resemblance as an issue. It is the issue of whether the substitute system and the target system are alike in suitable ways. With this second aspect of representation, the issue of truth will be introduced to bear on the evaluation of models.

One major reason for using models as representatives or substitute systems is the fact that the target systems are often very complex, excessively rich in properties and causal tendencies and their interactions. Models represent the target systems as far simpler, as devoid of most of those properties and causal facts, highlighting or focusing on just a small fraction of them. I have attempted to capture this feature of models by saying (following Marshall and other economists) that they *isolate* a fragment of their target systems. It is part of a long tradition to take it as a major task of economics to isolate what are regarded as the major causes and causal mechanisms behind empirical phenomena and their regularities.

In my work on models as isolations, I have paid attention to what could be called the *experimental moment* of many typical economic models (Mäki 1992, 2005). The central feature of experimentation is control. In an experiment, one controls for a number of ("other") causes and circumstances so as to prevent them from interfering with a relatively simple causal connection or process that is of special interest. The latter becomes thereby isolated from those possible interferences, or disturbances as economists often call them.

Now there are two major kinds of experimental systems and two major ways of effecting the required controls. In an ordinary laboratory experiment, one seeks to *causally manipulate* a set of potentially influential factors so as to neutralise them. Many theoretical models have a similar logical structure. The difference is that in theoretical modelling, instead of causally or "materially" effecting an isolation by controlling for other things, one just *assumes* those other things to be constant, absent, sufficiently weak, and so on. Theoretical models of this kind can be viewed as *thought experiments*. Another difference between "material" experiments and theoretical models as thought experiments is that in the latter, the isolations can be made much tighter – in fact, they can be made as tight as one wishes as there is no limit to the power of assumption.

What material experiments and theoretical models share is that they can both be taken as representations of some real target systems: as their representatives and as giving rise to the issue of resemblance. They are substitute systems that are directly examined with the hope of indirectly acquiring information about the target systems. In this sense they are both models – and they are both experiments, too (Mäki 2005). The experimental moment is present both in experimental economics and in economic theory.

It is useful to notice another feature of models in order to adequately address the issue of their truth. Given that any real target system is immensely rich and complex, it is not surprising that it can be represented by using a multiplicity of models. Different models may highlight different aspects of the target system and may help answer requests for different kinds of information about the target system. This can be made even more obvious by paying atten-

tion to yet another feature of models: their pragmatic aspects, their embeddedness in practices and purposes.

Nothing is a model in itself. Nor is it sufficient for identifying something as a model by adding that it is a model of something else. Whatever is a model is taken to be so by some agent using it for some purpose. Thus, in addition to the notions of representative system and target system, and of the issue of resemblance, we need others such as those of an agent and the agent's purpose. We may then say,

M is a model of R in that subject S builds/uses M as R's representative for purpose P, prompting the issue of resemblance between M and R to arise while keeping it constrained by P.

The relativity of models to subjects and purposes is an important feature of models without which they are hard to understand and appraise. It is obvious that acknowledging this aspect of models has the consequence of increasing our permissiveness about a great variety of models, and types of models. This fortifies our pluralism about models: different models may serve different purposes. While this is indeed obvious and recommendable, it is also clear that the pluralist permissiveness should have limits. Suppose a given model serves its purpose excellently, but the purpose happens to be a matter of, say, "deceiving the general public about the real intentions of the government" or some such. In order not to celebrate models that serve such dubious purposes as "true" or otherwise cognitively praiseworthy, I suggest we need some notion of *cognitively honourable purposes*. Such cognitively honourable purposes do not need to be directly cognitive purposes themselves. They are purposes the pursuit of which does not violate some important cognitive norms. One such norm would proscribe against distorting the facts that are relevant to assessing the truth value of the main truth claims of a model (I will say more on this in the next section). My key point is that the pragmatics of models – the purposes of their creation and use – do not alone determine their cognitive value, including their veritistic properties. Those veritistic properties should in their part contribute to the assessment of the purposes. (Naturally, the purposes of model creation and use must also be assessed in moral terms, but this theme will not be pursued here.)

The reasoning in the above paragraph suggests that the question to be discussed in the next section is not only legitimate but very important – and not very easy.

3. What is it for a model to be true?

In seeking to answer the question about what it is for a model to be true, we are constrained by the concept of model as outlined above. But the constraints are not fully binding, thus our answers will not be determined by

them. Another constraint derives from the conception of truth that we decide to adopt. Here, too, I will leave the constraint somewhat loose. What I do want to rule out, though, are ideas such as the following two. One is based on some coherence conception of truth: a model is true in virtue of being consistent with, or being derivable from, our other beliefs (such as those concerning sound theoretical microfoundations and those concerning empirical data). The other is based on some pragmatist or instrumentalist conception of truth: a model is true in virtue of successfully serving the purposes to which it was designed or put (such as yielding predictions at certain levels of accuracy or grounding certain economic policies). Both of these ideas highlight important features of models, but miss the distinct issue of resemblance. I take truth to be related to this issue.

It is now time to show how strategy [1] works in reasoning from the apparent falsehood of a model to its truth. Recall the idea I put forth in the introduction: A given model may appear to be false because one has mislocated the precise truth bearer – the claim to which truth (or falsehood) should be attributed – intended when using the model. Once the intended truth bearer is correctly located, the model will have a better chance of being true. (See Mäki 2004a)

This general idea can be illuminated by first considering the age old issue of unrealistic assumptions without much attention to the model in which they are involved. When phrasing and listing the assumptions of a model, economists characteristically are not very explicit about the precise truth claim they intend to make. Indeed, what do they mean to claim when assuming that there is no foreign trade, that goods and factors are perfectly divisible, that preferences are transitive, that transaction costs are zero? Assuming is not yet asserting: one can assume that *p* without asserting that *p*. Our question is this: what is the intended assertion or truth claim when assuming that *p*? In many characteristic situations, it is not the claim that, as a matter of fact, there is no foreign trade, that preferences are transitive, and so on. And given that these are not the intended truth claims, one is not justified in criticising them for their falsehood.

Here are just two examples of assertions that may be intended when making an assumption (Musgrave 1981; Mäki 2001). First, the intended assertion may be about negligibility. The assumption that there is no foreign trade may require being paraphrased as the assertion that the actual presence of foreign trade has negligibly small effects on the phenomenon under study. It is the negligibility claim that should be identified as the correct truth bearer, and it is this claim that should be assessed for its truth, and criticised if false. If one were to consider the no-trade assumption as the assertion that as a matter of fact there is no foreign trade, it would in most cases be false. But when paraphrased as the assertion that actual foreign trade has effects that are negligibly small given the purposes of inquiry (such as prediction at a certain level of

accuracy), it may be perfectly true. This is an example of how apparent falsehood can be transformed into truth just by being careful with precisely locating the intended truth bearers. It is also an example that highlights the importance of purposes in judgements of models and their truth: negligibility is a function not only of some causal facts in the real world but also of some pragmatic facts in the world of scientific work.

Second, the intended assertion may be one about applicability. The no-trade assumption is now paraphrased as the assertion that the model involving it only applies to domains with no foreign trade, or to domains regarding which the actual presence of foreign trade has negligible effects given the purposes of inquiry. What first was an assumption about trade now becomes an assertion about a property of a model, namely its applicability to certain domains of phenomena. One seeks to ensure that such applicability assertions are true while the no-trade assumption, if treated as a claim about the world, is false much of the time. Again, an apparent falsehood is transformed into a truth in virtue of relocating the relevant truth bearer.

Let us next consider the role of false idealising assumptions in theoretical models that function as thought experiments seeking to isolate the major causes of phenomena (Mäki 1992, 2004b). Here we do not transform false assumptions into true assertions but we rather pay attention to the constructive role of falsehood in assumptions in facilitating the model user to make true claims about the world. It is very important to understand the function of many unashamedly false assumptions in theoretical models. Their task is to remove or neutralise various causal factors so as to help highlight some key dependencies or causal connections or causal forces in the world, to help isolate the latter from the former.

My favourite illustration is Johann Heinrich von Thünen's model of agricultural land use (see Mäki 2004b for a detailed examination). In his *Der Isolierte Staat in Beziehung auf Landwirtschaft und Nationalökonomie* (1826/1966) Thünen invites the reader to imagine an area that has properties such as these: the area is a plain, i.e., there are no mountains, valleys, etc.; the plain is crossed by no navigable river or canal; the soil in the area is throughout capable of cultivation and homogenous in fertility; all communication between the area and the outside world is cut off by an uncultivated wilderness (hence the "isolated state"); at the centre of the plain there is a town with no spatial dimensions, hence it is represented by a single point; there are no other towns in the area. The list continues with many more "assumptions" of which he and we know that they are false. Thünen is clear about his research question: "What pattern of cultivation will take shape in these conditions?"; and how will the farming system of different districts be affected by their distance from the Town?" (1966: 4.) He then develops the famous image of a resulting land use pattern that has the shape of concentric rings, with each ring occupied by a distinct farming system. "It is on the whole obvious that near

the town will be grown those products which are heavy or bulky in relation to their value and hence so expensive to transport that the remoter districts are unable to supply them. Here too we shall find the highly perishable products, which must be used very quickly. With increasing distance from the Town, the land will progressively be given up to products cheap to transport in relation to their value." (*Ibid.*)

Thünen's research question is directly concerned with his simplest model of isolated state: what is going on in those highly idealised conditions that may be nowhere close to any actual conditions? On the other hand, there is no doubt that Thünen was also interested in real land use systems. The isolated state is built and used as a model of such real systems, a substitute system examined in order to acquire information about the real systems. But what kind of information can he claim to have acquired? What truth claim can he possibly make about the real world? It is not the idealising assumptions that are his intended truth claims. Nor is it the description of the emerging pattern of concentric rings that is intended as true of any real land use pattern. If interpreted as assertions about the real world, both the "assumptions" and the "predictions" of Thünen's model are false. Could the model still be true about real world land use? Yes, but this requires being careful with locating the precise truth claim intended when using the model. The truth of a model is not reducible to the truth of its assumptions nor to the truth of its predictions.

On my reading, Thünen's intended truth claim is the simple claim that distance or transportation costs are a major causal factor that in its characteristic fashion influences actual land use patterns. It is so and it does so both in the model world and in the real world, therefore claims about the model world may be true about the real world systems as well. This is in line with Thünen's own convictions: "The principle that gave the isolated state its shape is also present in reality, but the phenomena which here bring it out manifest themselves in changed forms, since they are also influenced at the same time by several other relations and conditions." (Thünen 1910: 274; my translation.)

This last idea in the above quotation points to the method used, the method of isolation, exhibiting the experimental moment in economic modelling. Thünen's model theoretically isolates the causal force of distance and shows its characteristic way of functioning. It employs idealising assumptions for neutralising those "several other relations and conditions" and thereby for capturing a simple but significant fact about the world. Falsehood has been put to the service of truth. To use J.S. Mill's phrases (perhaps in a somewhat non-Millian way), "truth in the abstract" about the major causal force is also "truth in the concrete" about it, while we may say (perhaps in a more Millian fashion) that the description of the outcome of its functioning in terms of concentric rings is true in the abstract (true about the model world) but false in the concrete (false about the real world).

Let me conclude by linking these thoughts with a recent view of models

and how they are connected to the real world. On this view, models are abstract objects that can be described variously, such as in terms of equations and diagrams, or verbally in words. I think this idea fits with the habit of economists to envision and reason about model worlds that are far simpler than real world systems, such as closed economies, 2x2x2 worlds, one- and two-sector worlds, overlapping generations worlds, representative-agent worlds, state-less economies, zero-transaction-cost economies. Thünenian isolated states, and so on. Economists are comfortable with making claims and reasoning about the properties of and happenings in these representative systems. Those claims are true about the abstract objects, but it is another question how the abstract model world relates to the concrete real world.

Ronald Giere has suggested that models as abstract objects are not candidates for truth since they contain no truth claims (Giere 1988). He thinks abstract objects cannot be true because they are not themselves linguistic, and because only linguistic items such as sentences can be true (or false). He suggests that abstract objects are related to real objects by the different relation of similarity. In addition to models, a theory contains "theoretical hypotheses" about the similarity relation: they are claims about the respects and degrees of similarity between abstract model systems and real systems. Giere thinks that because such hypotheses are phrased in language, they are truth valued, thus either true or false, depending on whether they get the similarity relation right (even though he also believes all truth talk is dispensable at the end). Thus the only possible truth claims are those about the relationship of similarity between abstract models systems and real systems. No truth claims are made about the real systems directly. My response would be to suggest that truth bearers do not need to be linguistic, thus abstract objects of the sort entertained by economists – isolated states, 2x2x2 worlds etc – could in principle serve as truth bearers. And the relationship of similarity could be viewed as a relationship of truth itself. So the difference between Giere and I may be largely terminological. But more importantly perhaps, given what I have suggested earlier, it is often not a good idea to regard an abstract model system as a truth bearer in its entirety. The intended truth claim when using such a model is often much more limited.

Giere's ideas have inspired Daniel Hausman's account of economic models and theories (Hausman 1992). In Hausman's account, a model as such contains no truth claims about the real world, it is rather a definition of a predicate given by the assumptions of the model, and such definitions are not truth valued. Models viewed as such bundles of assumptions define predicates such as "a Keynesian system" and "a general equilibrium system" and economists examine the properties of such predicates in exercising what Hausman calls "conceptual exploration" which makes no claims about the real world. Much like in Giere, "theoretical hypotheses" are truth-valued claims, but in Hausman's account they are about the applicability of the models (understood

as predicates) to real economic systems. Thus, "The Italian economy is a Walrasian system" is one such theoretical hypothesis that is either true or false.

So on this account, it is not models but rather theoretical hypotheses about models that are the relevant truth bearers that can be true or false. My worry about this account is that theoretical hypotheses would not perform any better in truth seeking and truth acquisition than if models were directly considered as truth bearers. They would turn out to be false just as often as models would. The source of this trouble is the same: models play a role in both approaches in their entirety, including all their "unrealistic" assumptions. "The Italian economy is a Walrasian system" is as false as the general equilibrium model described in terms of the usual Walrasian assumptions if taken as a truth bearer in its entirety. Similarly, "This land use system is a Thünenian isolated state" is just as false (or true) as the simple model described in terms of Thünen's idealising assumptions if taken as a truth bearer in its entirety. Given my earlier remarks about Thünen's model, we can now pinpoint a key issue. If the predicate "is a Thünenian isolated state" is defined in terms of the Thünenian idealisations, then there is no improvement forthcoming when using the predicate in a theoretical hypothesis: what appeared to be false still is false. But if a far more limited truth claim (now about the real world directly) is made concerning the powerful causal force of distance and its characteristic way of operation, the chances are better that truth will win out.

4. What is it for a model to be recognisably true?

It is one thing to ask what it is for a model to be true. It is a different thing to ask what it is for a model to be recognisably true. The latter question directs our attention to the ways in which one seeks to find out whether a model is true or false, and to the various methods, criteria and conventions applied in such an undertaking of justifying or criticising belief in the truth of a model. Keeping the two questions separate is to distinguish between the concept of truth and the criteria of truth, or between truth on the one hand, and its discovery and the justification of one's belief in it on the other. The remarks in the previous section are based on this distinction.

Not everybody agrees on keeping the two questions apart. This creates space for various epistemic conceptions of truth. The concept of truth – what it is to be true – becomes dependent on methods and criteria of discovery and justification that help recognise what is true and distinguish it from what is false. So there are two alternative lines that one can take: [a] Keep truth and justification separate. [b] Define truth in terms of criteria of justification.

[a] While distinguishing between the concept of truth – what it is to be true – and criteria of truth – how one goes about in recognising what is true – one

may grant two kinds of connections between the two. The first is a one-way *conceptual connection*: justification is defined in terms of truth. Epistemic justification is a matter of providing justification for a belief in the truth of a model (rather than, say, for its usefulness or its tractability). The criteria of truth are, after all, criteria of truth (rather than of usefulness and so on). The second connection is empirical rather than conceptual. There is a relationship of *correlation* between truth and the criteria of its recognition: truth correlates with satisfying certain criteria. For example, one may hold the view that the truth of a model correlates with having done well in econometric and experimental tests; with having "microfoundations" of a certain kind; with being consistent with, and supported by, other established theories; with serving a useful function in policy advice; with being accepted by a vast majority of economists, and so on. Much more should and can be said about these possible correlations, but let me here just emphasise that the supposed correlation must be made theoretically accountable. A theoretical account should be able to explain the correlation while pointing out further contingencies, exceptions etc.

[b] Epistemic conceptions of truth conflate justification and truth: they define truth in terms of the criteria of its recognition. Among the well-known abstract expressions of this idea in philosophical literature, one finds the ideas that truth is warranted assertability and that truth is ideal acceptability. It then takes further specification to get to a more detailed idea about which properties of a model precisely enhance its warranted assertability or ideal acceptability. The important core idea of these conceptions is that those properties of a model – such as satisfying the criteria of experimental support and policy efficacy etc. – contribute to the truth of the model rather than just to its acceptability or justifiability (as in non-epistemic conceptions of truth). No mere correlation prevails, but rather it is claimed that truth is *constituted* by the successful application of certain criteria of its recognition. It is easy to see that this conception may help transform apparent falsehoods into truths: a model that appeared false on some non-epistemic conception of truth (just because the world does not seem to be the way it is represented in a model) may be true after all (in virtue of meeting some criteria of acceptability).

In the remainder of this section, I will briefly discuss a special notion of truth-recognition from the two perspectives of [a] and [b]. This notion has been presented and discussed recently in economics and its methodology. The general idea is that the truth of a model is recognised by checking whether the model is found persuasive by some relevant audience.

The key idea along the lines of [b] is that truth is constituted by persuasiveness, or it is the persuasiveness of a model that makes it true. Not only is persuasiveness one of the criteria of truth, it is all there is to truth. As I read her, this idea has been suggested by Deirdre McCloskey in various writings since the mid-1980s (McCloskey 1985, 1994). I have attempted to clarify and organise her remarks about the issue in some of my earlier work, and have

argued, in the spirit of [a], for a conception of non-epistemic truth that accommodates the notion of rhetorical persuasion but does not conflate truth and the criteria of its recognition (Mäki 1995, 2000). In resisting [b] and defending [a], I am implying that I do not endorse strategy [2] of turning apparent falsehoods into truths just by conceiving of truth as epistemic or recognition-dependent.

Regardless of whether persuasiveness is equated with truth or whether it is taken to serve as its criterion, it cannot be left unconstrained. On my interpretation of McCloskey's views, she invokes two kinds of constraints: persuasion must concern the beliefs of the properly educated majority of economists (the social constraint) and it must be an outcome of an "honest" conversation that conforms to the canons of the *Sprachethik* that proscribe against sneering, closed-mindedness, not listening when others talk, and so on (the moral constraint). While McCloskey takes persuasiveness meeting these constraints to be all there is to truth, I view it as just one fallible criterion for recognising what is true and what is false. There is a correlation between truth and persuasiveness, but it is not stable, it may vary from situation to situation.

In general, it may be that the closer to the truth (in a recognition-transcendent sense) a model's intended truth claim is, the easier it is to persuade an audience about it – but this has to be qualified with a number of provisos. There is no stable and tight correlation between truth and persuasiveness. Successful persuasion is relative to a number of things, such as various properties of the audience, such as its capacities for critical reflection, its background beliefs and convictions, various non-epistemic (political, financial, reputational) interests involved, rhetorical conventions, distribution of power resources. I suggested above that on view [a] one needs an additional account of further conditions on which the envisaged correlation is weaker or stronger. In the case of persuasiveness, those further conditions have to do with the social and institutional structure that constrains, enables and shapes, and often prevents, the relevant conversational encounters. In the case of economics, they are related to the academic conventions, cultures and institutional structures of the discipline, including its educational system, the incentive structures shaping research and publishing, and its dominant political geography that allocates academic authority. The more "democratic" and otherwise morally and intellectually virtuous such conditions, the better the chances of discovering significant truths.

While one can improve the correlation between truth and persuasiveness by seeing to it that such ideal conditions are met as closely as possible, the correlation can never adopt the value of one. Meeting even the maximally ideal conditions in full does not guarantee that the truth will be discovered and recognised; this is an implication of truth being non-epistemic. On the other hand, the good news are that it is in no way impossible, often not even difficult, to succeed in recognizing the truth even though the ideal conditions are far from being met.

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