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Value judgements and economic models – a Weberian perspective

Abstract. The paper argues for the need to introduce analysis of value judgements into literature on economic modelling, which does not currently deal with this topic. It starts with a prescription formulated by Max Weber, that because social science is so permeated with value judgements (such as acceptance of certain ethical values and policy ends, or some methodological convictions), social scientists should openly state values and policy ends they accept while doing research. From this, a meta-theoretical prescription is formulated: whenever analysing a piece of research as its user or methodologist, value judgements expressed or assumed by the author need to be taken into account. If this is so, then a meta-theory of how to identify these components will be useful. As economics is a model-based science, it is desirable that this meta-theory be about models, or be part of a broader theory of models or modelling. Uskali Mäki's "model of a model" is an example of such theory of models that is easy to amend and refocus to account for this requirement.

Keywords: Max Weber, economic modelling, value judgements, model of a model.

1. Introduction

A question of objectivity is arguably more pressing in social than in physical sciences. Numerous factors contribute to this, from the fact that social scientists are themselves part of society and not only observe but also participate in social life, to the observation that political entanglement of scientists, most notably economists, creates incentives to substitute value judgements

for actual analysis. But this characterisation is already problematic as it reveals that there are a few meanings of "objectivity" one can focus on.

One of these meanings relates to ontology – there is an objective social world, which means that scientific theories are capable of uncovering reality. A related statement would be that social facts are at least partially independent of the minds and thoughts of scientists and other agents acting in society. Another meaning relates to epistemology – theories produced by scientists can be empirically and theoretically adequate, i.e. well-grounded on the basis of some reasons deemed to be "right reasons" (so scientific method rather than, say, political bias). Yet another could be seen as merging objectivity and intersubjectivity – conclusions of scientific inquiry tend to converge towards a consensus among scientists. In this paper, however, I am interested in still different sense of "objectivity", namely the one relating to the fact-value distinction. On this understanding of the term social science would be objective if scientific inquiry were value-free and unconcerned with particular interests.

In fact, even here there are two separate issues at play. First: if it is at all possible for research in social sciences, particularly economics, to be independent of researcher's convictions. Second: even if it is impossible to achieve absolute separation of research and convictions, does that preclude objectivity in economics? This latter question is the core of Max Weber's discussion of objectivity of social science, most notably in his essay "'Objectivity' in Social Science and Social Policy" [1904] (published in English as part of "The Methodology of Social Sciences" [1949]). Since Weber, there was much debate about value-neutrality of economic research, from Robbins [1932] to Sen [1970], but after that the issue seems to have died down among economists. Indeed, it seems that mainstream economists today readily accept that their inquiries, at least as long as explicitly classified as *positive* – as opposed to *normative* – economics, are value-free (even in popular press, as in Mankiw [2011]). As a corollary, it can be observed that while welfare economics tends to be seen as involving value judgements, many economists hold that it is separate from other branches of economics, thereby indirectly upholding the fact-value distinction [cf. Putnam & Walsh (eds.) 2011].

But it is far from obvious that economists are right to claim this, and the issue continues in philosophy of economics (as evidenced, for example, by

Putnam & Walsh (eds.) [2011][1]). What is curious, though, is that this strand of inquiry does not really connect to what is arguably the most prominent area in today's philosophy of economics – research regarding economic models and modelling. Models are rightly considered the most important tool in the arsenal of modern economists – economics is a model-based science [cf. Morgan 2012]. But this means that, as far as it is productive to discuss the role of values in scientific practice in economics, it is also productive to connect this topic to the topic of economic modelling. In other words, when providing an account of what a model is, or how the modelling practice looks like in research, a philosopher of economics might want to consider the role of values in modelling.

The understanding of "values" and "value judgements" in this paper is consistent with Weber's, but it also expands on Weber's thought in a way consistent with more modern accounts. In what follows these terms will be taken to refer to ethical convictions of researchers (such as: "it is desirable to reduce income inequality") or to policy ends assumed in a piece of research (as in: "suppose the government wants to reduce income inequality; this model examines various ways to go about this issue to determine which one is the most effective under our assumptions"), as well as to so called methodological value judgements. The latter understanding of value judgements results in claims that methodological decisions of scientists can constitute value judgements as well.

Yet another possible explication leads to the conclusion that to prefer one research area over another is itself a value judgement, as such preference reveals that the researcher in question thinks some matters more important than others. These various meanings of this paper's central terms will be expounded upon in section 2.

Therefore, this paper proceeds as follows. It begins, in section 2, with a classic take on the role of values in social science, i.e. Weber's understanding of what it means for social science to be objective. In short, the conclusion here is that even though economics in never truly value-free, this does not mean

[1] For discussions of this topic outside of economics see e.g. Kincaid et al. [2007], Forge [2009]. For Polish-language readers, Czarny [2004] is a comprehensive discussion of value judgements in economics.

it is not objective as long as aims and values of a given researcher are clearly articulated. This description can then be taken further and applied to economic science as it is actually practised. In practice, is the Weberian prescription followed? This is an important question, but a different issue is more central to the aim of this paper – if there is a need to be able to identify values and political ends in research in general, and in modelling in particular, a theory of this will be useful. Therefore, section 3 focuses on one of well-known accounts of economic models, by Uskali Mäki [2009, 2011, 2013]. Mäki's "model of a model" goes beyond standard realist descriptions of models as tools for representing some parts of the world. It includes a modeller using the model for a particular purpose, and an audience the model is addressed to. The claim here is that there might be a need to add more detail to the description of the modelling agent – so as to include her value judgements inherent in modelling practice. It will be shown that Weber's account is well suited to become the basis of such action. As will be explained, to proceed in this manner is different than to examine the purpose of the model or its intended audience (which Mäki does). In conclusion, the view advocated here provides a fuller account of economic models.

2. Weberian view on the objectivity of social sciences

As succinctly stated by Dahrendorf [1987, p. 577], Max Weber claimed that "statements of fact are one thing, statements of value another, and any confusing of the two is impermissible." It would seem, then, that Weber advocated that social science be completely value-free. It is rather clear, however, that research in economics (and other disciplines, for that matter) is riddled with value judgements, even if a researcher in question is not aware of that.

On the most general level, to choose your study area over some other is a value judgement in itself, as it demonstrates judgement on what is more, and what is less, important. Also, all scientific reasoning presupposes normative commitments such as primacy of logic and evidence over authority. Furthermore, there are *methodological* value judgements, which concern weighing simplicity, ability to express the model in precise mathematical terms, internal

and external consistency, predictive power, etc., against each other. They enter the scene when one is interpreting inferences, data, models, and so on. For example [cf. Shrader-Frechette 1993, ch. 3; 1994, ch. 3]: a researcher committed to the methodological value of predictive power can claim that a model that was not tested against data does not provide sufficient basis for drawing conclusions. On the other hand, someone holding external consistency in high regard might claim that untested models are sufficient basis for drawing conclusions provided that they are at least consistent with data. As we will see, this kind of value judgements is one of the two most important ones from the perspective of this paper.

On a more specific level, to accept cardinal utility measures for the purpose of engaging in welfare economics is a value judgement as it assumes all individuals measure utility on the same scale. Yet another one would be to then engage in interpersonal comparisons of utility (against Robbins [1932], among others). To accept – as is customary in economics – that it is desirable to increase efficiency is a value judgement as well. This last example could be described, in line with the tradition in analytic philosophy, as an *evaluative* judgement – a judgement which corresponds to a statement of value; most commonly, but not exclusively, they correspond to some ethical claim that something is good or bad, just or unjust, desirable or undesirable, etc. [Baujard 2013] Using, and reformulating, an example about income inequality given in the introduction, we could state a judgement such as "current levels of income inequality in country A are unjust". Building on this example we could formulate a *prescriptive* judgement, one that corresponds to a statement of recommendation [*ibidem*] - "we should aim to reduce income inequality in country A."[2] The other example given in the introduction ("suppose the government wants to reduce income inequality; this model examines various

[2] The third traditionally recognised class of judgements are *descriptive* judgements, which correspond to statements of fact. For example: "income inequality in country A took value X in January 2018." There are, of course, well-recognised problems in demarcating descriptive, evaluative, and prescriptive judgements (for a short general discussion see Baujard 2013; for a succinct analysis of evaluative components of descriptive judgements see Sen 1980; for various distinctions within the class of prescriptive judgements see Sen 1967). But these problems should not concern us here as this distinction is presented only to organise thinking and nothing of substance hinges on it.

ways to go about this issue to determine which one is the most effective under our assumptions") can be seen as paving the way for a kind of conditional prescriptive judgement. Suppose the model in question allows a conclusion that method M of reducing income inequality is the most effective under the model's assumptions. Then it can be claimed that "under our assumptions, we should apply method M if we want to reduce income inequality." This is a conditional prescriptive judgement as it prescribes the course of action conditional on: 1) the policy goal/end being reduction of income inequality; 2) assumptions of the model holding[3].

It is this kind of evaluative and prescriptive judgements, concerning ethical choices and policy ends, that forms the second type of value judgements that is important for the purpose of this paper – they are the ones Weber primarily addressed in his writings. Of course, evaluative or prescriptive judgements are not necessarily concerned with ethics or policy – one can have a variety of methodological prescriptions, to name just one example. (In fact, Weber's own prescription that scientists should be clear about their values is a methodological prescription.) However, what Weber was particularly focusing on were values entering consideration when one is analysing things from the perspective of a specific end (see below) – which is why evaluative and prescriptive judgements are presented in such context in current paper. It is also why in some contexts below terms "value" and "end" can be used interchangeably.

Weber, being of course aware that it is impossible for social sciences to be utterly devoid of values, avoided troubling claims that they should be entirely value-free. He did this by applying a two-step approach [Hoenisch 2003]. First, he believed that ultimate values could not in any way be justified through value-free analysis. So, from the point of view of science, it is impossible to choose any, say, ethical or political theory over any other without taking into consideration a value or an end that is to be realised – and this value or end is dictated by convictions of a particular scientist [cf. Lassman & Speirs 1994][4].

[3] The question of what it means for the model's assumptions to "hold" (do they need to be true?; approximately true, whatever that might mean?; is it enough for them to give the model sufficient predictive power?; etc.) is an important one in philosophy of economics, but it falls outside the scope of this paper.

[4] This does not mean, of course, that *no* reasonable discussion about values is possible.

But then comes the second step – once the value or end has been chosen, a value-free analysis is entirely possible of what the most effective means of attaining that end are. Such approach is visible in Weber's writings, where he often declares openly what his standpoint will be in a given essay. At one point in his "The Nation State and Economic Policy" [1895/1980], he clearly states that the situation at Germany's eastern border will be assessed from the standpoint of the German people. Thus, he provides the basis (the end to be achieved) against which the assessment of facts needs to proceed. By doing this, he also paves the way for conditional prescriptive judgements in the form of "if the good of the German people is the value we want to uphold in our policy, then we should engage in action A on Germany's eastern border."

In the same work he underscores that "…the ideals we introduce into the subject matter of our science are not peculiar to it, nor are they produced by this science itself."[5] This means that even though economics necessarily involves value judgements, these value judgements sit above the subject matter of economics and can – and in fact *should* – be disentangled from economic facts. Practical prescription which follows is that a scientist should be open and clear about her values when engaging in an inquiry. Using the terms just introduced, we could say that a scientist should be open and clear about evaluative (and methodological) judgements involved in her inquiry because these evaluative judgements then serve as basis for formulating (conditional) prescriptive judgements. This clarity, then, allows for objective analysis of social phenomena from the perspective of a given end.

A caveat is needed before we go further. For the purposes of this paper the Weberian perspective on value judgements is interesting inasmuch as it generates the practical prescription that social scientists should be open about their values. For Weber, these values are understood as acts of *valuation* (*Werthen*), which mean personal judgements, like taking a stand on some issue [cf. Bruun 2001]. But, strictly speaking, the Weberian approach to value judgements is much broader, and has two facets. Practical valuations of a given scientist, her ethical convictions or political views, are one of them. The other, and logically

[5] This is also underscored by Weber in his famous lecture "Science as a vocation" [1917/1989], as well as in "The meaning of "ethical neutrality" in sociology and economics" [1917/1949].

prior, is the concept of *value relation* (*Wertbeziehung*), taken by Weber from the works of German philosopher Heinrich Rickert and then amended. For Rickert, and then for Weber, value relation was a theoretical concept related, essentially, to choosing the object of scientific research which is *valuable*, where *value* (*Werth*) enters in the sense of being interesting, worth knowing about (*wissenwerth*). For Weber, this value relation was culturally determined and an indispensable part of social science *qua* science. What is interesting is decided not by some universal principles that scientists should know (whereas such universality was argued for by Rickert [cf. Bruun 2001, p. 147-8]), or some metaphysical weight of a given problem, but by the interests of the scientist's public. So, in a way, the very act of selecting the material for research has a kind of a value-related component, and this component is tied to the pressing issues of the times that are in the interest of the public (otherwise, the resulting work of the scientist will itself not be "worth knowing about"). But this component is related to values in the *theoretical* sense of "interest". So it is very different from *practical* value judgements of the scientists, which are their valuations. This means Weber can see a kind of value judgements even in the act of choosing the subject matter for research[6], and yet still say that scientific analysis should be free of values understood as valuations [cf. Weber 1917/1949] (at least in the sense explained above, where Weber's two-step procedure was discussed)[7]. Having said that, in what follows I will be focused on the practical prescription that scientists should be open about their values (valuations).

The above description notwithstanding – and as noted above – the mainstream conviction is that economics is by and large a value-free science. In light of foregoing observations on normative content of economic research and on the Weberian stance, this claim could be understood as correct only if what is being claimed is that economists, when engaging in what is usually called positive economics, are merely trying to establish the most effective

[6] Which goes back to the most general type of value judgements involved in selecting a study area and noted at the beginning of this section.

[7] For an extensive discussion of these matters, see e.g. Bruun [2007]. Bruun [2001, 2007] also demonstrates how the concept of value relation led Weber to the well-known concept of ideal types.

way of achieving a given end. But even then this would be a rather weird way of using the phrase "value-free" - if the end is dictated by values, values are always present in economic analysis. So at best there would be a value-free *component* to an economic theory or model, while the whole piece of research would not be considered devoid of normative statements or presuppositions. In other words, the best we can do is to separate values from facts and be clear about this separation. Such view also has a long tradition in economics, having been radicalised by Robbins [1932] and endorsed by Musgrave [1999] in his discussions of public policy. A methodological prescription of this sort was also accepted in welfare economics when social welfare function was the main topic [e.g. Bergson 1954]. But the point here is, economists are rarely that clear about the value-fact separation.

One has to remember that this paper is written from the Weberian perspective, and on this account some such value-fact separation is possible. I do not think, though, that this perspective, as it is being used in this paper, assumes we need to be able to *fully* separate these two levels *in each and every case*. To uncritically assume something like this would be to disregard many advances in philosophy of science in the last fifty years [cf. Gonzalez 2013], and it would be just wrong. But if one were to follow writings of Putnam [2004], or authors in Putnam & Walsh (eds.) [2011], and claim that there is no strong ontological difference between values and facts, it would not deem any attempt at separating them futile or unnecessary (similar position is defended in Niiniluoto [2009]). On the contrary, it would rather reinforce the need of such separation inasmuch as we are able to do so, if only to avoid as much confusion as possible. And for the purpose of this paper, such lack of clear distinction would also not nullify, but rather reinforce the need to incorporate value judgements in our view of models and modelling practice in economics, as discussed in section 3. In any case, the Weberian perspective is useful for the purpose at hand, especially given that some version of it seems to underpin thinking of many economists, among them those inclined to view economics as largely value-free[8].

[8] In other words, it is not required that we accept Weber's strong claim that statements of fact are one thing and statements of value another. The Weberian perspective employed in this paper is Weberian because Weber's methodological prescription – that we should strive to be as

This raises possibilities for philosophical and methodological scrutiny. One recent example of such endeavour can be found in Desmarais-Tremblay [2014]. The author juxtaposes two theories of public finance. One was proposed by Richard Musgrave, who openly admitted his theory was normative. The other was by James Buchanan, who formulated it in response to Musgrave and stated that his theory was positive. Desmarais-Tremblay provides an explication of the Weberian approach to value judgements, and concludes that Buchanan was closer to Musgrave on the normative-positive spectrum than he admitted. This is evidenced by implicit value judgements that were harder to spot than in Musgrave's theory. The Weberian approach, as explicated by the author, provides two distinct tests of whether a theory is normative, one of which we can invoke here. It goes: a theory is normative when its author does not explicitly formulate value judgements that are implicitly present; or when she does, but at the same time explicitly endorses values expressed in these judgements. It is in this latter sense that Musgrave's theory is classified as normative because Musgrave states that he agrees with the position that some so-called "merit goods" need to be provided publicly. On the other hand, Buchanan failed to clearly articulate some judgements in his theory, so his account is normative in the former sense[9].

In any case, the Weberian approach to the value-fact divide provides at least one prescription when it comes to evaluating a piece of research in social sciences: whatever is being said, see what ends are being served and what values are being endorsed[10].

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clear about our values as possible – is deemed an important piece of methodological guidance, as well as useful in formulating the meta-theoretical prescription presented in section 3. This meta-theoretical prescription follows fairly straightforwardly from Weber's methodological prescription.

[9] The second test of normativity is that a theory will not be normative even when value judgements are present, provided that valuations are made solely by the agents in the model/theory. Thus, any external intervention by the theorist will mean it is normative. It is the case with Musgrave's postulate that collective action problems related to public goods will not be solved by intra-theory agents and economist must declare that state intervention is required. Contrastingly, Buchanan sought to show that such problems will be solved by the agents, therefore his theory is not normative in this sense.

[10] As Weber's account of value judgements in scientific practice is used here as a starting point for further ideas, I do not purport to present any novel interpretation of his writings on this

3. Model of a model

As for now, three things can safely be asserted on the basis of the foregoing discussion:

1) economics is a science in which value judgements are commonplace;

2) given that it is so, an evaluation of a piece of research needs to involve some mention of these judgements, especially if the author of the evaluated piece did not explicitly explain her stance (this is where the influence of the Weberian approach is most visible);

3) economics is a model-based science.

Given this, it seems rather curious that discussion of how value judgements appear in scientific practice in economics is not really present in the literature on economic modelling. Let us take one of well-established – although, of course, far from uncontroversial – accounts of what is a theoretical economic model, that by Uskali Mäki [2009, 2011, 2013].

Mäki represents one of the most prominent traditions in thinking about economic modelling. It can be termed "isolationism." Traditionally, isolationists are scientific realists (i.e., in the most basic formulation, they postulate that there are real causal relations occurring in the world that is at least partly independent of the agent's thoughts and states of consciousness). The key terms in their accounts are "isolation" and "representation", meaning that a model is a model by virtue of it being a sort of a "stand-in" for the world (target system). The model takes into account only salient characteristics of the target (so it isolates them from all the unimportant features), at the same time somehow representing these salient features, e.g. by resembling the target in some way. Given this basic setup[11], it feels natural to confine our talk about models to simple dyadic model-target relations. But in his recent research Mäki admits

topic. Additionally, the description provided in the text is necessarily brief. Readers interested in a detailed analysis of Weber's stance on values and value-free science should consult Bruun [2007]. For general Weberian methodology, presented in context of important debates of his day, see Ringer [1997] and Eliaeson [2002]. For a recent argument for the continued relevance of Weber's approach to science, see Ghosh [2014].

[11] There are, of course, other ways of explicating what an economic model is. One of the most prominent alternatives to isolationism is what can be termed "constructivism", as represented, e.g., in influential papers by Sugden [2009] and Grüne-Yanoff [2009].

this is not enough and other components enter the relation between the model and the target, making it more nuanced. Therefore, such richer idea of a model looks as follows [Mäki 2013, p. 91]:

"**[ModRep]**
Agent A
uses (imagined) object M as
a **representative** of (actual or possible) target R
for **purpose** P,
addressing **audience** E,
at least potentially prompting genuine **issues of resemblance** between M and R to arise,
describing M and drawing inferences about M and R in terms of one or more **model descriptions** D,
and applies **commentary** C to identify and coordinate the other components."

Issues of representation and resemblance were briefly mentioned above. Introduction of an agent is meant to demonstrate that nothing is a model without being used as such by some modeller. There are also various purposes one can have in using a model, such as isolating some causal relation, exploring the range of possible causal relations, general explanation of a phenomenon, prediction, influencing policy-making, improving a mathematical structure, etc. A model can also vary depending on an audience – it can be directed at specialists in cutting-edge research in the field, or first-year students, or policy makers, etc. According to Mäki, model description will be influenced by the intended audience – mathematical symbols for specialists, metaphors and real-life examples for laymen.[12] Finally, model commentary identifies and coordinates the other components – it should specify purpose and audience, it should also help in understanding e.g. the role of unrealistic assumptions in the model, etc.

[12] It is far from clear to me that model description can always be so easily decoupled from the model itself. In other words, it is not clear that the same core idea (abstract object) expressed with use of mathematics or with use of natural-language metaphors will always be the same model (or even – if it will really be the exact same idea). Certainly, Mäki's account – as any other – is not without its problems, but it should not detract from the main topic of this study.

Such extended "model of a model" can be used here as it will be instructive to show that even on such broad account an important aspect related to value judgements is missing, and it should not be if economics is indeed so permeated with value considerations.

Consider two very different classes of models. One class consists of models in the Austrian tradition, i.e. representing versions of the Austrian Business Cycle Theory (ABCT). The other class consists of models in the Real Business Cycle (RBC) tradition. Objects in both classes (or at least some of them) have arguably the same general target – cyclical fluctuations in economic performance – and purpose – to explain these cyclical fluctuations by identifying their causes. As done by practising economists, they are also intended to represent, or at least it is not implausible that they are. Their intended audience is on some level the same – this being professional economists – but there are obvious differences in that RBC models are aimed mostly at mainstream mathematical economists, and the Austrian research is largely confined to the narrow Austrian audience. Model description is where the difference is most visible, as RBC models are expressed via mathematics with some added interpretation in English, while most Austrian models are formulated only in English, without the use of mathematics. And of course the explanation itself of why the economy is going through boom-and-bust cycles is completely different.

One could stop here, just noting the differences. But then an important *why* question would go unaddressed, namely: *why* are the content of the model, its intended audience, and, maybe most importantly, its description so different if the target and purpose are by and large similar? In fact this is largely due to normative convictions of both schools. Austrians underscore the importance of capital theory and its complexities (the role of time in production processes, interplays between time preference and the structure of production, etc.) [cf. Garrison 2001]. Modern mainstream macroeconomics, together with the New Classical school in which the RBC theory originated, abstracts from capital theory, thus implicitly treating it as unimportant in explanations of business cycles. In other words, there are differences in the content of the *explanans* between these two classes of models. At least some of them trace back to the most "high-level" type of value judgements mentioned in section 2 – the ones involved in attaching importance to one research area over some other.

Further, Austrians explicitly [Hülsmann 2001, p. 36] differentiate their brand of economics from neoclassical and other mainstream schools by insisting that they are focused on analysis and representations of human action, while mainstream economic thought is confined to analysis of quantities of things that are subject to human action. Yet further, Austrians claim that individual human action, which is the proper subject of economic inquiry, cannot be mathematised, or at least not in the style of modern economics. This is an example of a methodological value judgement, which links back to my statement at the beginning of section 2 that this kind of judgements is of importance in current paper.

All these discrepancies (and the list above is far from exhaustive) do not stem from differences of purpose – both schools are trying to explain how business cycles come about. Differing intended audience cannot be a cause as well, for such conclusion would assume that if the audiences were switched and mainstream economists for some reason started speaking to the Austrian audience, they could just adopt models of the Austrian variety and use them like any other model. But simple observation confirms that this could not be the case. A mainstream economist, whether of New Classical or any other variety, just would not accept the way Austrian economics is being done – and *vice-versa*. There is a gulf separating both ways of thinking about the economy.

Part of this gap is explained by methodological value judgements mentioned above. But it is not implausible to suppose that differences not only in methodological value judgements, but also in what I referred to as evaluative and (conditional) prescriptive judgements concerning ethical choices or policy ends, may be able to explain some of it as well. It is not to say that the pre-existing political stance of some group of economists will completely determine the outcomes of their models. It is to say that the overall political stance (as well as some specific view on a given policy matter) of a given economist does have a chance of influencing their thinking while devising a model[13]. This seems to occur in the case analysed by Desmarais-Tremblay, who ascribes some importance to e.g. Buchanan's libertarian sympathies or

[13] This will apply, of course, only to models whose purpose has actually something to do with policy matters. If the purpose of a model is to work out the complexities of some mathematical structure, then such value judgements will not appear.

Musgrave's conviction that there *just are* some goods that should be treated as merit goods. The free-market orientation of both RBC and Austrian theorists follows from their respective theories as a conclusion, so it might seem hard to view it as an implicitly assumed value judgement. Austrians go even further in their claims that central banks should be abolished [cf. Hayek 1990, Salerno 2010, ch. 19]. Again, this follows from their Misesian theory of business cycles [cf. Mises 1949, ch. XX]. But it is, again, plausible to suppose that these kinds of factors will enter the background assumptions of a modeller whenever a new model is designed e.g., to aid policy-makers. As it is not the aim of this paper to work out the complexities of this case study, I leave this as an intuitive supposition at this stage, and will analyse case studies in more detail in an upcoming paper[14].

The above is a simple, and obviously broadly sketched, example showing how one can work to distinguish models not using the components of Mäki's account of models, but using a separate, additional component – modeller's values and value judgements assumed in the model[15]. It would perhaps be more proper, and more interesting, to compare models from the same intellectual tradition that differ neither in purpose nor in intended audience, nor in the type of model description, and yet are different in important respects that can be traced back to modeller's values. Therefore, a fitting case study will be undertaken as the next research step on the topic. Mainstream monetary economics seems to be a promising avenue, especially models attempting to give theoretical underpinnings to decisions of central banks. They are directly policy-relevant, which means they are formulated with specific policy ends and perspectives in mind. They are situated within one general-equilibrium paradigm and use similar mathematical techniques. Yet they are often different with respect to decision rules upon which a central bank in the model is acting [cf. Clarida et al. 2000, Belke & Klose 2013]. It means, of course, that they are

[14] More intuition for suppositions in this vein is supplied by a recent paper by Salter & Luther [2016], who try to show that it is possible to express the ABCT in the mainstream framework, with equilibrium and rational expectations at its centre. If this is really the case (some Austrians would surely object), then the policy conclusions might result not from methodological judgements, but from *bona fide* ethical value judgements.

[15] Mäki's account is thus refocused – his original aim was only to describe and explain what a model is, while here the aim is to be able to classify models according to their characteristics.

different in their assumptions. In our present context the question is: *why* are they different in their assumptions? Are the differences resulting from normative considerations (such as: "we think the central bank should be operating in this and this way"), or do they sit in the positive realm (as in: "given that the aim of the central bank is this and this, its decision rule should be that and that")? It should be visible that these kinds of questions stem directly from the Weberian approach to understanding value judgements in social sciences.

One potential criticism that could be levied against this approach is that to identify all implicit value judgements in an evaluated model, one might have to guess what the author meant in some statements, and maybe even look at her biography, history of political engagement, opinion pieces in popular press, etc., to understand her broader normative commitments. This would render the whole enterprise daunting to the point of impossibility. Such criticism would be misguided, though. First of all, this being a practical prescription, it does not assume that all value judgements need to be uncovered in the process of understanding and interpreting a model. What is important is the attitude and awareness this prescription elucidates. Second of all, as evidenced by Desmarais-Tremblay's paper [2014], such interpretations of economic research are possible and valuable. The author does use some biographical facts about Musgrave and Buchanan to strengthen his argument, but the biggest weight is lifted by a close reading of books and papers. In general, exercises in interpretation is what any practising scientist is doing whenever reading any piece of research. Whatthe current paper does is it adds an additional dimension to these readings. In this context it is worth remembering that the meaning of any sentence, theory, or model is not an abstract, objective entity, but it is produced at the intersection of the author's intent and reader's own baggage of experience ("what the reader brings to the reading of the text" - Samuels [1990, p. 9]). So there is no one way of going about this issue.

In any case, Mäki's account of models is useful here as it readily admits inclusion of modeller's values into its scheme, giving something like:

[ModRep2]
Agent A,
expressing value judgements contained in set V,

uses (imagined) object M as
a **representative** of (actual or possible) target R
for **purpose** P,
addressing **audience** E,
at least potentially prompting genuine **issues of resemblance** between M and R to arise,
describing M and drawing inferences about M and R in terms of one or more **model descriptions** D,
and applies **commentary** C to identify and coordinate the other components.

Depending on one's view on the possibility of separation of values and facts, it will not always, or maybe not even often (or maybe even never), be possible to specify *all* elements of the set V. But, as mentioned above, this is not a crucial problem for this approach as long as some elements can be specified. Also, the word "expressing" is used here deliberately (instead of, for instance, "making") to allow for the possibility that in some model value judgements are not being made actively or consciously.

4. Conclusion

What has been said here can be summed up thusly:

1) if Weber is right, then there is a need to distinguish and be explicit about normative and positive components of a given piece of research because normative components will impact policy conclusions;

2) if this is so, then a meta-theory of how to identify these components will be useful;

3) economics is a model-based science, so it is desirable that this meta-theory be about models, or be part of a broader theory of models or modelling;

4) Mäki's "model of a model" is an example of such theory of models that is easy to amend and refocus to account for the requirement stated in 2).

Finally, it is worth noting that the Weberian approach is especially suited for such use. It admits of possibility that economists *qua* economists (as opposed to economists *qua* private persons) will be making value judgements

or working with some particular ends in mind (only Robbins's version of this approach would claim such activities have no place in economics). Yet it also states that some separation of values and facts is possible in practice – as opposed to what might be termed "strong non-neutrality of economics" thesis [Mongin 2006, Niiniluoto 2009]. (E.g. Myrdal [1958] claimed that value judgements and factual statements cannot be distinguished logically. This also relates to Putnam's position mentioned in footnote 2.) These two Weberian claims give rise to point 1) above and point 2) at the beginning of section 3.

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