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Review of Peter Spiegler's *Behind the model: a constructive critique of economic modelling*. Cambridge: Cambridge University Press, 2015, 201pp.

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Peter Spiegler examines the current state of theoretical and empirical modelling in economics. According to him, this involves answering two questions: First, how can one determine what causal factors are essential for a phenomenon and, second, how does one ensure that the methodological tools employed represent these features faithfully (p. 9)? Spiegler puts forward new answers to these questions. He argues that a look *behind* mathematical models in economics is necessary: these methods are themselves not capable of showing that the economic phenomena and their representations within formal models are compatible, in the sense that the formal models conceptualise their domain of applicability accurately for a given epistemic purpose. He suggests that this compatibility between formal methods and economic phenomena needs to be checked with the help of an interpretative-hermeneutic method, akin to techniques used in anthropology and sociology, and that this should give rise to a new subfield of economics: interpretative economics.

Economists and philosophers of economics alike might be puzzled by this suggestion of giving qualitative methods such a key role within economics. I think both should be stimulated by Spiegler's proposal. Throughout the book, it becomes evident that he is a philosophically highly informed economist who identifies relevant issues in a precise manner and skilfully navigates through the nitty gritty details of particular episodes of economic modelling. Even if one leaves aside his call for a substantial reform of economics, this book contains a lot of food for thought. For example, his discussion of the New, New Institutional Economics and Dynamic Stochastic General Equilibrium (DSGE) modelling provides rich case studies that put on the radar of philosophers of economics subfields of the discipline that have so far not received enough attention.

Spiegler's two specific claims are the following: Blind spots are a problem for *all* formal methods in economics and there *needs* to be an interpretative-hermeneutic method to assess the aptness of these formal techniques. I am intrigued by these two claims but not convinced by them. My worry is that Spiegler does not do enough to support them. Before spelling this out, let me briefly summarise the three parts of the book.

In part I, Spiegler introduces a pragmatic account of formal modelling and puts forward a criticism of theoretical and empirical formal modelling that sets the stage for the rest of the book. He suggests the following framework that is inspired by Mäki (2009): An epistemic agent S uses a model M to represent X for purpose P. The success of S in accomplishing P is judged against disciplinary norms N (p. 25). He further differentiates between four stages of formal theoretical modelling: In the delimitation phase, a social phenomenon is delimited and a research question is formulated in ordinary language (e.g., why is there involuntary unemployment?). In the denotation phase, the delimited social phenomenon is connected to a mathematical model in two steps: first, the formal structure of the model is described informally—with the help of ordinary language names for the phenomenon (yielding what he calls a proto-model); second, the model is presented in purely formal terms. In the solution phase, purely mathematical operations are performed to arrive at a result. In the interpretation phase, the solution stated in mathematical terms gets re-translated into ordinary language using the correspondence established in the denotation phase (pp. 46-52). According to Spiegler, the same four phases can be used to describe econometric modelling. In this case, however, two additional relations need to be accounted for: 1) econometric models are (sometimes) models of an economic theory and 2) econometric data is data about economic phenomena (p. 73).

Crucial for understanding Spiegler's criticism of formal economic modelling is his view of models as metaphors. Metaphors invite us "to project the attributes of mathematical objects onto […] social entities: to 'see' social phenomena through the overlay of mathematical relations" (p. 53). Following Hesse (1963), he suggests that the illumination provided by a metaphor requires that there is enough relevant similarity (positive analogy) and sufficiently little relevant dissimilarity (negative analogy), such that one can see the neutral analogies (i.e., the ways in which the two entities related in a metaphor may possibly be similar) as

relevant similarities (p. 54). He helpfully clarifies this requirement further by introducing the "no essential negative analogies" (NENA) condition (p. 58). What the NENA condition requires is that the dissimilarities between a formal model and its target do not pertain to the essential properties of the target. Essential properties are defined with respect to the purpose of the modelling exercise (p. 56). Put differently, the model should not "distort or obscure […] the target subject matter" but rather should be apt for the target system in light of an epistemic purpose (p. 58). This prompts the crucial question for Spiegler: What properties does a given social phenomenon need to have to be compatible with a formal construct introduced in the denotation phase of formal modelling? According to Spiegler, formal modelling presupposes that "(1) the objects under study are plausibly stable, modular and quantitative, with no qualitative differences among instantiations of each type; and that (2) the relations between these objects are plausibly fixed and law-like throughout the context of the study in the modelling exercise" (p. 63). He illustrates these two conditions with the help of Shapiro and Stiglitz's (1984) efficiency wage theory. According to this theory, it can be beneficial for managers to pay more than the market clearing wage since it undermines the incentive for workers to shirk because there is a credible threat of being unemployed. For Spiegler, the concept of effort as represented in this theory (i.e., a continuous variable that does not allow for qualitative distinctions among its instantiations) violates condition 1) as it does not, for example, account for relevant qualitative distinctions between effort types such as effort in an assembly line vs. effort in an advertising firm (p. 64). The theory also violates condition 2), since the proposed stable relation between wage level and effort of employees is depending on the connotation that a wage regime has in a company and this effect cannot be a priori known and, crucially, not be assumed to establish a fixed relation between wage and effort level (p. 65). Hence, Shapiro and Stiglitz's efficiency wage theory lacks essential compatibility with its intended target domain and, hence, should be viewed as a problematic formal modelling exercise. Spiegler closes part I by arguing that an exactly parallel condition to the NENA condition holds for econometric modelling: successful econometric modelling presupposes that there is a homorphism between data and social phenomena (p. 77).

In part II, Spiegler applies the essential compatibility requirement between models and their target domain to two case studies: New, New Institutional Economics and DSGE modelling. In the interest of brevity, I focus on the latter. He argues DSGE models had failed in the run-up to the recent financial crisis since their formal structures precluded seeing the crisis' relevant dynamics. In other words, DSGE models have significant blind spots since they do not meet the essential compatibility requirement. In particular, the assumptions that aggregate macroeconomic behaviour can be represented as optimizing behaviour of a representative agent, that financial markets are efficient, and that the macroeconomy is a log linear system represent essential features of macroeconomic phenomena incoherently as they rule out chaotic dynamics (pp. 120-125).

In part III, Spiegler puts forward his constructive proposal against the background of the criticisms in the first two parts of the book. He suggests founding a new discipline within economics: interpretative economics. The role of this discipline is to provide the relevant information for checking whether the methodological tools of economists are essentially compatible with their intended domain of application. This new field should establish the meaning of economic concepts (such as effort or wage) and assess whether these concepts display the stability requirements set out in the NENA condition, and, hence are susceptible to formal modelling (p. 166). He suggests an interpretative-hermeneutic method to accomplish these tasks. This method involves three related steps: 1) choosing a fore-understanding of the phenomenon to be analysed, 2) refining this fore-understanding through a contact with the phenomenon that is open-ended enough to "allow the phenomena to speak for themselves" (p. 172), and 3) interpreting the phenomena of interest utilising the information in 1) and 2) (p. 172).

Spiegler discusses Bewley's (1999) work on wage rigidity during recessions as an exemplar of this new field of economics. Bewley, frustrated by empirical and theoretical limitations of available accounts of wage rigidity (e.g., Shapiro-Stiglitz's efficiency wage theory), engaged in a set of unstructured interviews, inspired by interpretative survey techniques in sociology and anthropology, with businesspeople responsible for hiring and compensation decisions. He found, among other things, that these businesspeople are focussing on the morale of the work force and that the mechanism of the efficiency wage theory

qua punishing mechanism undercuts this morale significantly. This is evidence for Spiegler's claim that Shapiro and Stiglitz' theory violates the NENA condition (pp. 176-181). Importantly, Spiegler believes that a hermeneutic-interpretive method, and hence a non-formal method, is necessary to retrieve the information about the essential compatibility between models and target systems (pp. 146-148).

I think Spiegler's emphasis of potential blind spots of formal modelling techniques and his attempt to come up with a constructive proposal to determine the extent of the respective blind spots of various formal techniques should be highly welcomed. However, in my view, there is a central ambiguity running through this book that makes it hard to pin down exactly what Spiegler's main line of criticism against formal economic modelling is. Because of this ambiguity and the dialectical space it opens up, I do not see why the interpretative-hermeneutic method should be the way forward to address the shortcomings of formal modelling techniques.

Spiegler's comments about the essential compatibility of formal models with economic phenomena can be read in two ways. One can read it in the *strong sense* that formal models in economics *are* bound to fail since economic phenomena *do not exhibit* the required modularity and stability described in the NENA condition in virtue of some fact about economic reality. This reading is supported by Spiegler's claim that the "potential hazards of model-target mismatch are endemic to all mathematical economic modelling" (p. 191), his characterisation of the meaning of social phenomena as "fluid, evolving, and imprecise" which are "formed dialectically—i.e., by agents acting within norms and conventions which are in turn shaped by these individual actions" (p. 146), and his remarks about how formal models of New, New Institutional Economics "render institutions susceptible to economic analysis by converting them into something else" (p. 96). Alternatively, one can read Spiegler's claim in the *weak sense* that formal models in economics *might* fail since economic phenomena *might not exhibit* the required modularity and stability described in the NENA condition. The clearest indication that Spiegler might have this interpretation in mind is contained in a footnote on Lawson's realism (see, e.g., Lawson 1997). Spiegler claims in this passage that there is a central difference between the NENA condition and Lawson's conditions for the aptness of formal modelling of economic phenomena: "A central difference between [Lawson's] conditions and [my conditions] is that Lawson's conditions

are pitched at the level of ontology—whereas [my conditions] deal with the plausibility of claims and therefore are essentially pragmatic conditions" (p. 63, fn. 17).

I think that both readings of the claim face some argumentative challenges. Consider the strong reading to start. To make a case for a *general* inaptness of formal methods to model the economic realm requires taking a stance regarding the ontology of the economic realm. In fact, one needs to defend a substantial (social) ontology which rules out characterisations of economic entities and relations that could be—in principle or in relation to the epistemic purposes of modelling—formally modelled. Coming up with such an ontology and some sort of epistemic access conditions to establish that the ontology is in fact an accurate description of the economic realm is a daunting task. Taking Spiegler's remarks about Lawson's realism seriously, a charitable interpretation suggests that he never actually had the strong, more radical claim in mind, while discussing the limits of formal modelling. However, I do think there are some gaps in the defence of the weak claim as well.

Spiegler (p. 121) states explicitly that the NENA condition should be read as not only involving the dyadic relationship between models and targets but also the purposes and norms of the modelling exercise. So, showing that a formal economic technique is inapt for modelling a domain requires showing that the technique is inapt in relation to a *particular* epistemic purpose. The case studies that Spiegler provides reveal that some modelling techniques are inapt in relation to some epistemic purposes such as the explanation of the phenomenon of wage rigidity. However, I do think that his discussion overlooks two important aspects of the model-purpose link.

First, a formal modelling technique might be apt given one changes the epistemic purpose. To put it differently, a domain of investigation might be satisfying the NENA condition given one changes the epistemic purpose. Let me illustrate this with one of Spiegler's own papers. Together with Stephen Marglin, he analysed the effectiveness of the fiscal stimulus in the 2009 *American Recovery and Reinvestment Act* (ARRA) (Marglin and Spiegler 2014). With the help of surveys—an interpretative technique—they established that the counterfactual claim that states would have been able to spend at the observed levels in the absence of ARRA was false. Spiegler (pp. 187-188) rightly concludes that this piece of information invalidates the use of an econometric model by

Cogan and Taylor (2012) aiming at *measuring the stimulus impact* which is built on this false assumption. However, what it fails to show is that for a different epistemic aim, Cogan and Taylor's (2012) formal model would be inapt. It might be apt, for example, for revealing *one relevant causal factor* in the causal-nexus of the stimulus impact, i.e., consumption smoothing considerations by states. Importantly, as Spiegler (p. 187) himself notes, the surveys of Marglin and Spiegler (2014) do support assumptions regarding consumption smoothing on state level. Moving away from Marglin and Spiegler (2014), some theoretical models might not be apt for the analysis of policy-intervention, however, they can be fruitfully used to study the working of isolated causal mechanisms in the economy (see, for example, the Caballero et al. 2015 model that introduces a mechanism for re-balancing asset markets in an economy that is at the zero-lower bound of the interest rate). I do think that shifts of epistemic aims can be pursued across different theoretical and empirical modelling exercises. The underlying reason for this is that for different epistemic aims, distinct aspects of a model could be representationally relevant. Now, if for a given epistemic purpose some aspects of a model are not representationally relevant, then these aspects cannot ground a violation of the essential compatibility requirement between a model and a target. For example, if one claims that a formal model provides a causal-mechanistic explanation of an economic phenomena, more elements of the model must be viewed as representationally relevant than in the situation where a model is used for short-term forecasting with the option for daily feedbacks of prediction errors into the model (for example a vector-auto-regression model). If one disagrees with this possibility of formal models being apt (or inapt) depending on the epistemic purpose pursued, I think, one is committed to the strong reading of Spiegler's claim and, hence, faces the challenge that was mentioned above.

Second, some of the methods that Spiegler discusses might be apt for the epistemic purposes that he evaluates them on, despite his claim to the contrary. His discussion of state of the art DSGE models is a case in point. Spiegler claims that this modelling framework is inapt for assessing real world macroeconomic phenomena since it cannot display non-linear dynamics (pp. 120-125). However, it should be noted that the standard DSGE framework with rational expectations does not preclude markets from collapsing and allows for multiple equilibria outcomes

(see, e.g., Den Haan 2003; 2007). Again, the key question seems to be what epistemic aim should be realised with a particular model.

If these aspects are relevant for state of the art economic modelling, then I do not see the urge to have a special discipline within economics assessing the aptness of formal modelling techniques. An alternative upshot of Spiegler's analysis—following from the weak claim—could be to demand from economists to state more precisely what the epistemic purpose of their formal modelling endeavours are. Note that this point is independent of additional reservations one might have against the reliability of interpretative-hermeneutic methods (and in particular against their empirical tools of interpretative interviews and participant studies).

Spiegler's book is an economically extremely well-informed engagement with the foundations of formal modelling. Even if one shares my reservations regarding his reform proposal for the discipline, this book provides plenty of fruitful case studies and frameworks that certainly advance our understanding of the economic practice.

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