

## Book Symposium

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**Elaborating Naturalized Critical Realism:  
Response to Ruth Groff, Dave Elder-Vass,  
Daniel Little and Petri Ylikoski**

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**Abstract:** This paper is a reply to the discussions of Ruth Groff, Dave Elder-Vass, Daniel Little, and Petri Ylikoski of Tuukka Kaidesoja (2013): *Naturalizing Critical Realist Social Ontology* (London: Routledge).

**Keywords:** Critical realism; Naturalist philosophy of social science; Transcendental arguments; Causal powers; Social structure; Social system; Social mechanism; Roy Bhaskar; Robert Merton; Mario Bunge.

I will begin my reply by further clarifying the naturalist position outlined in my book and my motivation to naturalize critical realist social ontology. This section responds to Ruth Groff's doubts concerning the viability of the naturalist approach of my book. After that, I will address Groff's and Petri Ylikoski's points regarding transcendental ontological arguments and my naturalist alternative to them. The rest of this paper seeks to clarify my interpretations and critiques of the accounts of causal powers and social structures offered by Bhaskar and his followers, and to defend the usefulness of the notions of "naturalized" causal power and the concrete social system in the context of social ontology. I will try to substantiate these points by relating them to some sociological theories and explanatory practices. Due to space limitations, I have decided to omit a separate discussion on ontological emergence, since this topic was already clearly and interestingly addressed by Daniel Little and Ylikoski.

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## Naturalism in the Philosophy of Social Science

In her review, Groff claims that the naturalist position defended in my book includes a view that “[all? – T.K.] philosophical questions are properly settled via the deliverances and/or methodology of sciences” and that “naturalism is the right norm for philosophy.” She also seems to believe that my critiques of the views of Bhaskar and other critical realists were intended to be totally destructive rather than constructive. Since I already discussed naturalism in the introduction to this symposium, I think that it is sufficient to restate that my intention was to defend naturalism *in the context of the philosophy of social science (including social ontology)* and to suggest how the original critical realist social ontology can be naturalized by replacing its problematic anti-naturalist elements with naturalist ones (see Kaidesoja 2013, chapter 1). It is important to note that these tasks are quite restricted compared to those of defending naturalism in all fields of philosophy or destroying critical realism completely. In any case, Groff is not convinced that critical realism needs to be naturalized.

She claims that “Kaidesoja never actually makes a case why we should undertake to naturalize philosophy.” Evidently, she expected me to present some kind of philosophical justification for my naturalist approach. If this is the case, then the irony is that in the naturalist philosophy of science, purely philosophical arguments are rejected in favor of empirically based and practically oriented arguments. In the book, I explicitly indicate that this makes all justifications for the naturalist philosophy of science circular in the sense that naturalists have to take for granted that some scientific practices and methods are successful (or instrumentally rational), since they utilize some scientific methods and theories in the naturalist philosophy of science (Kaidesoja 2013, p. 53; chapter 4). I also admit that this means that naturalists cannot provide any independent philosophical (i.e., non-scientific) justification for scientific research or the naturalist approach in the philosophy of science, because such a justification would contradict the basic assumptions of their position. It does not mean, however, that naturalists cannot examine the reliability of any specific method used in the sciences for certain purpose(s) by means of empirically analyzing the method’s reliability. For this, and some other reasons discussed in chapter 2, I argued that normative critiques of *particular* scientific methods and practices are possible from the naturalist viewpoint even though the circularity of justifications for this approach surely implies that a general critique of science should be rejected as unfeasible.

It is important to add that many naturalist philosophers of science have argued that the methods used in the sciences are variable and that the border between science and non-science is sometimes vague (which is not to deny that

there are practices that are generally deemed scientific and reliable, such as classical scientific experiments and randomized controlled trials). For this reason, I explicitly admit that some of the methods traditionally used in philosophy, such as some forms of conceptual analysis and thought experiments, can be included in the naturalist philosophy of social science on two conditions: (i) They are *not* assumed to provide a source of a priori ontological knowledge, nor an a priori foundation for epistemic norms of science, and (ii) they are used to serve the core purpose of naturalist inquiry, which is to increase our understanding of actual scientific practices (see, Kaidesoja 2013, chapter 1). Indeed, it would be odd to reject the use of methods of this kind given that they are used in many empirical sciences to address conceptual and methodological issues that arise in scientific practices.

Nevertheless, I admit that the naturalist philosophy of social science outlined in the book remains mostly at the programmatic level in the sense that I did not engage in detailed and systematic empirical analysis of social scientific practices. This is something that I intend to do in my current and future work. However, I defended the fruitfulness of the naturalist approach in the philosophy of social science by supporting my methodological and ontological arguments with examples from the social and cognitive sciences as well as discussing important works in the naturalized (or practically oriented) philosophy of science, including those of Mario Bunge, Ronald Giere, Ian Hacking, Uskali Mäki, and William Wimsatt.

## Transcendental Arguments and Inferences to the Best Explanation

In contrast to naturalist philosophers of science, Bhaskar and other critical realists propose transcendental arguments for their ontological views. In the book, I argued that ontological arguments of this kind should be replaced by arguments based on inferences to the best explanation that begin with the empirical analysis of scientific practices and theories.

In Groff's review, she claims I argue that "Bhaskar's so-called 'transcendental argument' *against Hume's account of causation* fails" (italics added). There are two interrelated problems in this interpretation: the first one is that Bhaskar's (1978) critique of the Humean account of causation is not a transcendental argument at all, but rather a *reduction ad absurdum* type of argument, which may also be called an *immanent critique*. In this argument, Bhaskar skillfully demonstrates the variety of absurd implications that follow if we describe (non-instrumentally understood) scientific experiments in terms of the Humean regularity theory of

causation. However, it is crucially important to note that this critique alone *does not provide a positive argument for Bhaskar's specific transcendental realist ontology*. Secondly, in contrast to Groff's claim, I think Bhaskar's immanent critique of the Humean account of causation is convincing. Accordingly, I admit that it can be claimed to provide indirect support to *any realist account of causation*, as it challenges the alternative Humean view on causality, which is anti-realist. However, the problem is that the transcendental realist ontology defended by Bhaskar (1978) – let alone his social ontology – includes a number of other ontological doctrines in addition to the concepts of causal power and the causal mechanism that constitute his realism about causation. Furthermore, unlike in the 1970s when *A Realist Theory of Science* (RTS) was published, today the causal powers theory is not the only realist alternative to the Humean regularity theory of causation. Other alternatives include, for example, Wesley Salmon's process theory and James Woodward's interventionist theory of causality.

Hence, the targets of my critique are Bhaskar's neo-Kantian *transcendental arguments for his transcendental realist ontology* that, in my view, should be separated from his immanent critique of the Humean account of causation, even though both Bhaskar and Groff appear to hold that these two arguments necessarily go together. Perhaps this point was not made clear enough in the book, but I think that it is implicitly included in my discussion on Bhaskar's transcendental arguments (e.g., Kaidesoja 2013, p. 90). Accordingly, one of the core points in my critique is that one cannot plausibly argue for ontological views by means of Kantian transcendental arguments unless one commits him- or herself to a Kantian doctrine of transcendental idealism (or some historically and culturally relativized version of it) according which the objects of cognitive experience and scientific knowledge are partly constituted by the forms of our sensibility and the categories of our understanding (or by our scientific theories and practices). Since Bhaskar (1978) advocates realism with respect to ontological categories and objects of scientific inquiry, transcendental idealism of this kind is explicitly rejected by him. In my view, the unresolved tension between transcendental arguments and ontological realism in Bhaskar's work is one of the reasons why his transcendental arguments for the transcendental realist ontology are problematic and hard to reconcile with a naturalist philosophy of science.<sup>1</sup>

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<sup>1</sup> I really liked Daniel Little's discussion on the degrees of fallibility in the context of critical realism. I would like to add one citation from Bhaskar's postscript to *The Possibility of Naturalism* that supports Little's views. A few pages after having claimed that his transcendental arguments are fallible, Bhaskar (1998, p. 176) writes that "[w]e can be certain that society exists and has certain general features" that, according to his view, are "transcendentally necessary conditions for any knowledge."

Though I reject Bhaskar's a priori reasoning to establish "the (conditional) transcendental necessity" of the ontological conclusions of his transcendental arguments and believe his meta-philosophical position to be incoherent, I nevertheless admit that there is something interesting in the ontological arguments presented in RTS. I agree with Dave Elder-Vass' and Daniel Little's views that my naturalization of Bhaskar's ontological arguments in RTS can be seen as an attempt to save the reasonable core of these arguments by means of removing the anti-naturalist (or aprioristic) elements from them. Nevertheless, the points raised by Ylikoski cast serious doubt about the usefulness of the naturalized arguments of this kind in social ontology. His main worry is that the ontological argument that was outlined in my book can never be implemented in practice because it requires there to be only one ontological view compatible with the scientific practice of interest. Ylikoski criticizes this view by claiming that it is always the case, at least in principle, that there is more than just one ontological theory that is compatible with a scientific practice.

Now, let me restate that the conclusions of the naturalistic ontological arguments sketched in the book were meant to be fallible claims about "the relative explanatory power of the proposed ontology" (Kaidesoja 2013, p. 99). In this sense, the naturalist argument form was intended to include the idea of "inference to the best ontological explanation" of the epistemic successfulness of the scientific practice *from the currently available ontological theories (that are relevant to the practice)*. I also believe that further empirical analysis of the practice of interest may suggest that we should change our initial ontological explanation or that our judgment with respect to its epistemic successfulness was premature.

The previous points do not, however, resolve Ylikoski's concern that there may be situations where many different ontological theories are each compatible with scientific practice, and there is no way to decide which one is the best on the basis of the available evidence. Even worse, he suggests that there may be no shared understanding of the criteria of evaluation to be used in the assessment of competing ontological theories.

I agree that situations of this kind are in principle possible, but I think that it is ultimately an empirical question how frequently they occur in the context of naturalist (or practically oriented) social ontology. It seems to me that there are reasons to expect that the number of plausible *naturalist* theories in social ontology may typically be relatively limited. For one thing, it is not easy to develop an internally and externally coherent ontological theory that would not only fit with our a posteriori description of the scientific practice under study but also increase our understanding of the practice (e.g., provide some idea why the scientific practice is epistemically successful). Note that external coherence here means that our social ontological theory should be compatible with the other accepted naturalist

ontological theories regarding the social sciences as well as the relevant parts of the behavioral and natural sciences. I think that this criterion, too, restricts the number of plausible candidates for naturalist social ontologies in each case quite remarkably. For example, it can be argued that the extreme forms of social constructionist and holistic social ontologies as well as strictly individualist social ontologies all face problems regarding external coherence, even though they may be internally coherent and compatible with *some* social scientific practices built upon these ontologies – naturalists may of course have sound reasons (including ontological ones) to criticize these scientific practices as well, but this is not my concern here. For these reasons, I tend to think that ontological arguments of this kind are a bit more promising than Ylikoski suggests, but I admit that there may also be other kinds of naturalist ontological arguments worth pursuing.

## Causal Powers and Kinds of Things

Let us move on to more substantial ontological issues discussed in the book. I argued that Bhaskar uses the concept of causal power in at least two different senses without clearly separating them from each other (Kaidesoja 2013, p. 117ff). One of these senses is identical to the account of causal powers in Harré and Madden's (1975) book, as well as in some of Harré's (e.g., 1970) earlier works. In this view, causal powers studied in the special sciences (precluding fundamental physics) are considered dispositional properties of particular things (e.g., instances of chemical substances, biological organisms, and artefacts) that they have by virtue of their intrinsic structures (i.e., the specific organizations of their parts). The other sense is what I call "the transcendental realist account of causal powers." This account ascribes causal powers to *abstract* universals and structures. Though I sometimes attribute this view to Bhaskar, my intention was not to claim that he (or his followers) explicitly advocates this view. Rather, I tried to show in chapter 5 that there are passages in Bhaskar's early works that do not make sense unless one assumes the view that causal powers are located in some kind of transcendental realm of being that is ontologically distinct from concrete powerful particulars. Textual and other evidence for this view can be found not only on pages 117–123 but also in other parts of my book (e.g., discussions on Bhaskar's philosophy of mind and his notion of social structure in chapter 5). Both Groff and Ylikoski seem to miss these points.

For example, Groff claims that my view is that Bhaskar is a "Platonist about properties" who thinks that "universals exist independently of their

instantiations” and that properties of concrete particulars are, therefore, “fundamentally otherworldly.” This is somewhat odd reading of my work for three reasons. Firstly, as indicated above, I explicitly point out that in some passages of RTS, Bhaskar relies on a concept of causal power that is essentially similar to the one developed in the early works of Harré and his collaborators (see, e.g., Kaidesoja 2013, p. 118). Secondly, my focus in the book is on *causal powers of entities studied in the empirical sciences* rather than universals in general. The key word here is “causal” since, unlike what Groff suggests, the transcendental realist account of the concept of causal power appears to be at odds with the most successful scientific practices (e.g., classical experimentation) that track the causal powers of things by means of causal interventions. In my view, there is no need to introduce transcendently real causal powers in order to make sense of these practices, since Harréan (non-transcendently real) causal powers of powerful particulars already do the job. Thirdly, the label of Platonism is used only in one short passage in the book where I metaphorically suggest that in certain contexts Bhaskar’s way of using the concept of causal power commits him to the view that “resembles Plato’s world of ideas of which the world of empirically observable material things imperfectly mirrors” (Kaidesoja 2013, p. 121, italics added) rather than Aristotle’s views on universals. In contrast to Groff’s reading, this metaphorical analogy was not meant to be the core point of my critique of Bhaskar’s account of causal powers.

Nevertheless, I still believe that there is also something similar to Platonism in some of Bhaskar’s and the other critical realists’ views on causal powers. Let me focus here on Dave Elder-Vass’ claim that “there are types of things of which it is true, irrespective of whether tokens of the type actually do exist, that tokens of the type would have certain causal powers if they did exist.” According to Elder-Vass, such causal powers that are not properties of any actually existing powerful particular (presumably also including actual entities that existed in the past) may still be considered “non-actually real.” He also suggests that this view can be found in Bhaskar’s early works and argues that non-actually real powers of this kind should be included in the “naturalized” critical realist ontology.

It is important to note that Elder-Vass’ point does not concern the *causal potentials* that are rooted in the currently existing concrete entities, nor does it concern exercised causal powers that are *counteracted by the other causal powers* and, therefore, remain unmanifested in some observable sequences of events. As both Groff and Ylikoski rightly emphasize, situations of a “multiple determination” of patterns of empirically observable events were among the reasons why Bhaskar (1978) introduced the term *tendency* (i.e., exercised but not empirically manifest causal power) and the distinction between the actual and the real. In my view, both non-exercised causal powers of things (i.e., causal potentials) and exercised

powers of things that in certain circumstances (e.g., when counteracted by the other powers) remain empirically unmanifested are both included in Harré and Madden's (1975) discussion on causal powers. Nevertheless, I admit that Bhaskar (1978) should be credited for making the latter point more explicit in RTS by introducing the notion of *tendency*. One of the core points of my book nevertheless is that such activated (or exercised) causal powers whose effects are not directly manifested in specific observable events are best understood as dispositional properties of the concrete (in contrast to abstract) entities studied in the sciences.

Nevertheless, the question raised by Elder-Vass does not concern the views related to multiple determination of events. I take his question to be rather the following: Should a naturalist include in his or her ontology such types of things (or natural/scientific kinds), including their non-actual causal powers, that (i) instantiate in the future, and (ii) will never be instantiated in the actually existing world? It seems to me that naturalists should incorporate the kinds of things with causal powers that are currently non-actual but will instantiate in the future into their ontology. This is because the purpose of explanatory practices in the sciences is to produce new knowledge about the world, and this sometimes requires the postulation of entirely new kinds of entities with novel causal powers. Nevertheless, nothing specific can be said about the nature of these kinds of entities and their powers before they have been instantiated (or their instantiations detected) and their powers studied by means of empirical methods.

By contrast, I think that naturalists should not include the ontological category of the never-instantiated natural/scientific kind in their ontology, since it is hard to see how never-instantiated kinds can be studied by means of empirical methods. As suggested in Elder-Vass' review, scientists and engineers may surely speculate – on the basis of the fallible knowledge embedded in their current theories and models – on what kinds of entities and powers might emerge if certain actually existing entities were combined in novel ways while having no interest or opportunity to evaluate their claims by means of experimentation or using other empirical methods. Nevertheless, I tend to think that ontological naturalists should not include the possible causal powers of the speculative entities of this kind in their ontology if they will never be instantiated in the actual world. One reason for this is that there are no clear plausibility criteria for the postulation of this type of causal powers, which is problematic since human imagination is unlimited. Furthermore, it appears to me that metaphysical speculations about never-instantiated kinds of entities and their possible powers tend to distract us from more important questions regarding the ontological assumptions of actual scientific theories and practices.

Let us return to more mundane issues. Though the other reviewers are favorable to a realist account of causation in terms of causal powers and regard it



relevant to the practices of explanatory social research, Ylikoski is skeptical about its value to the methodology of the social sciences. I agree with him that the causal powers theory, even in its “naturalized” version, does not provide us with all the tools that are needed to analyze causal attributions, inferences and explanations in the social sciences. I nevertheless think, in contrast to Ylikoski, that this theory has already contributed to the methodology of the social sciences. For example, it has helped sociologists to question the viability of correlation-based statistical analysis as a means of developing proper causal explanations to social phenomena. The analytical sociologist Peter Hedström (2005, p. 105) argues that in the correlation-based “causal modeling” tradition “[t]heoretical statements have become synonymous with hypotheses about relationships between variables, and variables have replaced actors as the active subjects with *causal powers* [italics added].” The core assumption in Hedström’s critique is that only concrete actors and entities, not statistical aggregates (e.g., income levels or education levels), can have causal powers and, therefore, function as parts of explanatory mechanisms in causal explanations. Thus, he holds that in the context of sociology, “[a] statistical analysis is a test of an explanation, not the explanation itself” (Hedström 2005, p. 113). To my mind, these are pretty important methodological points.

## Social Structures, Social Systems, and Social Mechanisms

Considering the title of this journal, I think that it is a high time to discuss social ontological issues. In the book, I argued that if social structures are understood as relatively enduring ensembles of internally and necessarily related social positions (e.g., capitalist and wage-laborer) or roles (e.g., teacher and pupil) that are occupied by individuals, as is done in many passages in Bhaskar’s (e.g., 1998, p. 28–29; 40–44) and his followers’ works (e.g., Archer 1995, chapter 6), then social structures cannot have causal powers. This is because they are conceptual abstractions rather than concrete entities. I also explicitly indicate that this is not the only way in which the concept of social structure (or society) is used in Bhaskar’s and his followers’ work, though it appears to be the dominant one (e.g., Kaidesoja 2013, p. 72–73; 133–136). Thus, Groff’s citations of Bhaskar’s work are not sufficient to challenge this interpretation, given that I explicitly point out (Kaidesoja 2013, p. 78–89; 129–136) – agreeing with some other commentators (e.g., King 1999) – that Bhaskar uses the concepts of social structure and society in many different senses without clearly separating them from each other, and that there are tensions in his different views on the ontological status of social structures.

Nevertheless, Groff also suggests, if I understood her views correctly, that there is nothing in Bhaskar's early works that would justify the view that he regards social structures as conceptual abstractions rather than concrete entities. Accordingly, she claims that internally related social positions and roles "are no less concretely instantiated than are 'social systems' [in Bunge's sense – T.K.]." Below, I argue that both of these views are mistaken. I do so by focusing on class-positions and role structures that are often used as examples of social structures in Bhaskar's (e.g., 1998) and his followers' works (e.g., Archer 1995).

Before discussing these examples, it should be noted that in the postscript of the third edition of *The Possibility of Naturalism*, Bhaskar (1998, p. 169–170) himself admits that he uses the concept of structure in the book ambiguously "to refer both to the abstract form or type and the particular concrete instantiation or token of it." Though he now acknowledges the problem that was mentioned above, it seems to me the solution he proposes is not entirely convincing. He basically claims that abstract structures exist only as concrete instantiations that he calls, following Andrew Collier's suggestion, *structurata* (Bhaskar 1998, p. 170). For the sake of simplicity, in the following I replace the term "structuratum" by "social system," which refers to concrete interaction groups, organizations and some larger social entities (e.g., national economies) that are ultimately composed of interrelated and interacting human individuals and the artefacts they use in their interactions.

One way to interpret the previous claim would be to say that the same abstractly defined social structure is instantiated in many social systems at the same time due to the fact these different systems have the common relational essence. For example, we might say that the capitalist mode of production understood in terms of Marxian theory is currently instantiated in particular societies or national economies that we call capitalist, since they all share the capitalist relations of production based on the private ownership of the means of production and including (at least) the internally related positions of capitalist and wage-laborer in which the occupants of the former position must exploit those in the latter in order to sustain their position.<sup>2</sup> It is important to note, as I already

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<sup>2</sup> This example may be problematic in the sense that recent empirical studies on varieties of capitalism suggest capitalisms in different countries include quite profound institutional and organizational differences (Hall and Soskice 2001). However, the point of the example is to show in what sense Bhaskar's concept of social structure is more abstract than Bunge's notion of social system, rather than to evaluate the Marxian theory of the capitalist mode of production. I am also aware that there are more culturally oriented class-theories compared to that of classical Marxism, but since Bhaskar (e.g., 1998) defends a realist interpretation of Marx's theory in his early works, I think that this example is justified.

suggested in my book (Kaidesoja 2013, p. 156–157), that the concept of social position in capitalist relations of production refers to the theoretically defined *class* positions rather than to positions in any concrete social system, even if it is assumed that the capitalist mode of production does not exist independently of its concrete instantiations in particular societies, economies and/or organizations. So, the first sense in which Bhaskar's concept of social structure refers to more abstract entities than Mario Bunge's concept of social system is that, for Bhaskar, social structures include internally related class positions.

Bunge (1998, p. 69), by contrast, holds that theoretical descriptions of social classes, including those based on Marxian theory, may be objective insofar as they refer to the properties (e.g., interests or certain kind of resources) that are shared by collections of individuals in certain historically existing societies or economic systems. Nevertheless, he emphasizes that neither social classes nor their internally related ensembles as such are concrete social systems because “the members of a class [...] are not necessarily bonded together by any social ties” (Bunge 1998, p. 69) that involve concrete social interactions. It follows from this that social classes (or class structures) cannot have causal powers because they are merely descriptive concepts (or categorizations of people) rather than concrete entities (or powerful particulars). By contrast, Bunge (1998, p. 69) regards *class-based organizations* (e.g., employer organizations, trade unions and socialist parties) as social systems that are “just as concrete and real” as their members.

I do not claim that the distinction between abstract class-positions and concrete class-based organizations is totally unknown to critical realists, since a similar distinction between “classes-in-themselves” and “classes-for-themselves” was already made by Karl Marx. My view nevertheless is that the critical realist account of social structures in terms of internally and necessarily related social positions does not provide the conceptual tools needed in the separation of concrete social systems (including class-based collective actors) from conceptual abstractions (such as class positions), but it rather obscures the difference between them. Note also that I do not question *the descriptive uses of such conceptual abstractions that do not refer to concrete entities (or their kinds)*, since I only suggest that we should not ascribe causal powers to abstract structures that are distinct from interacting human beings and their concrete social systems (including the concrete social processes and mechanisms that bring about and sustain the unequal distributions of resources in a society).

The other type of social structure often mentioned by Bhaskar and his followers is the role structure in which each role is considered as being “necessarily and internally related to others (e.g., doctor/patient; landlord/tenant; teacher/pupil)” (Archer 1995, p. 186). Their view appears to be that only those

roles that (conceptually) presuppose each other (e.g., someone cannot be called a teacher unless there is one or more pupils that he or she teaches) are conceived of as being internally related. In order to be a bit more concrete, let me focus here on Margaret Archer's views on the supposedly internally and necessarily related roles of the teacher and pupil. The reason why I decided to discuss Archer's views here is that she not only advocates critical realism and draws on Bhaskar's early social ontology, but her ideas on role structures are also more developed than those of Bhaskar's. The following argument also extends to those presented in my book, since I do not offer a detailed discussion of role structures in it.

In what follows, I compare Archer's account of the necessarily interrelated roles of the teacher and pupil to Robert K. Merton's (1957, 1968) theory of role set applied to the role of the teacher in a concrete school. In particular, I suggest that Merton's theory of role set, which was developed in the context of empirical sociological research and has been highly influential in subsequent sociological role theories, presupposes the concept of a concrete social system whose referents include concrete schools. By contrast, I will indicate that the relation between Archer's (1995) account of the roles of the teacher and pupil and concrete schools remains vague, to say the least. In addition, I argue that it is fruitful to clearly distinguish between social structures and mechanisms of concrete social systems, as is done in the Bungean systemic social ontology defended in my book.

To begin with, Archer (1995, p. 186–187) holds that necessarily and internally related roles should be separated from their incumbents since the former are thought to predate and condition the actions of those people who occupy them at a certain instant of time. She also assumes that roles of this kind endure “a succession of incumbents possessing very different personal characteristics” (Archer 1995, p. 186). In her discussions on the nature of roles, Archer (e.g., 1995, p. 173–174; 276) also suggests that, when the normative contents of the interrelated roles – such as teacher and pupil – are specified or defined at a certain instant in time, the same role structure can be assumed to be instantiated in many concrete social systems – such as schools – located in the area of interest. At the same time, she admits that all role structures may undergo changes, which may be initiated by the role incumbents themselves. Hence, she argues that social scientists should examine “the interplay between a role and its occupants” (Archer 1995, p. 187) over time. In addition, she maintains that people tend to simultaneously occupy many social positions and roles that involve incompatible normative expectations and that their performances (or personifications) of the same role may be slightly different due to their variable personal powers and abilities (e.g., Archer 1995, p. 187; 275–276).

By contrast, in the exposition of his theory of role set, Merton makes a distinction between the concepts of social status and social role. In his view, which builds upon an existing sociological tradition, the concept of status refers to “a position in a social system, with its distinctive array of designated rights and obligations” (Merton 1968, p. 41).<sup>3</sup> The concept of social role in turn refers to “the behaviour of status-occupants that is oriented toward the patterned expectations of others (who accord the rights and exact the obligations)” (Merton 1968, p. 41). According to Merton’s (e.g., 1968, p. 432) theory of role set, each social status in a structured social system typically involves multiple role relationships rather than a single unified role – this is where his views differ from the more traditional views on social roles. Accordingly, he defines the term “role set” as referring to “that complement of role relationships which persons have by virtue of occupying a particular social status” (Merton 1968, p. 423, italics removed). For example, the social status of a school teacher in a public school in Merton’s day “has its distinctive role-set which relates the teacher not only to the correlative status, pupil, but also to colleagues, the school principle and superintendent, the Board of Education, professional associations and, in the United States, local patriotic organizations” (Merton 1968, p. 42).

Even though this may seem to be a minor point, it is nevertheless crucially important to note that Merton’s theory of role set is different from the idea that people occupy multiple different social statuses (or positions) in different social systems at the same time – a view that he also advocates. Rather, the concept of role set refers to situations where a person who occupies a status (e.g., the status of a school teacher) in a particular social system (e.g., a public school) with a specific (though complex and changing) social structure faces an array of different kinds of normative expectations that pertain to his or her recurrent actions and interactions as the incumbent of the status. In particular, Merton (1968, p. 44; 424–425) holds that (at least in typical cases) these expectations are not entirely compatible with each other meaning that conflicts among the role relationships in a role set are common. He further suggests that there tend to be systematic differences in the expectations, interests and values of the incumbents of different statuses (e.g., pupil, school principle and member of the school board) that belong to the role set of a specific status (e.g., school teacher) (Merton 1968, p. 44). In some cases, these differences can be analyzed in terms of different locations of the incumbents in the class structure of society, since, for example, “the members of a school board are often in social and economic

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<sup>3</sup> Note that in my book, I use the term “status” slightly differently to refer to status hierarchies in social systems that are based on peoples’ perceptions of one another’s rank or worth (Kaidesoja 2013, p. 158–159).

strata that differ significantly from the stratum of the school teacher” (Merton 1968, p. 44).<sup>4</sup>

I think that it is obvious that not all of the different role relationships in the role set of a teacher can be considered internal relations in the sense that they are somehow necessary (or constitutive) for someone occupying a status of a teacher in a public school. For example, it is possible to function as a teacher without actively participating in the professional organizations of school teachers, even though those teachers who participate in those organizations do so as school teachers. So, Merton’s analysis suggests that we should not assume that social statuses in concrete social systems are accompanied by singular and unified roles. In contrast to Archer’s account of roles, it is also likely that there are significant differences between the role sets of different teachers, not only at different schools (located in a specific area and analyzed at a certain instant of time) but also within the same school. For these reasons, it can be concluded, first, that the concept role relationship in Merton’s theory of role set is “less abstract” than the notion of internally and necessarily related role in Archer’s account, and, second, that the analysis of concrete social systems leads to a more realistic and complex understanding of the role structures of concrete social systems compared to Archer’s views on internally and necessarily related social roles.<sup>5</sup>

In addition, Merton (e.g., 1957, 1968) was one of the first sociologists who more or less consciously sought to shift the emphasis of explanatory social research from the analysis of social structures and functions of social systems to social mechanisms that reproduce and transform concrete social systems. For example, in his discussion on role sets, he maintains that it is crucially important to try to identify “the social mechanisms through which some reasonable degree of articulation among the roles in role-sets is secured or, correlatively, the social mechanisms which break down so that structurally established role-sets do not remain relatively stabilized” (Merton 1968, p. 425; also 1957). Without going into the details of the mechanisms he proposes, it should be noted that Merton’s (1968, Chapter II) well-known and highly influential methodological view was that the best way to theorize social mechanisms, social structures and social systems of various kinds is to develop relatively abstract and general but empirically testable “theories of the middle-range” (see Pawson 2000).

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<sup>4</sup> Archer (1995, p. 275–276) mentions the term “role set” in passing, but she does not define it or mention Merton’s theory of role sets. Archer also fails to notice that the theory of role set is incompatible with the critical realist doctrine of necessarily and internally related roles.

<sup>5</sup> Kemp and Holmwood (2012) have also recently criticized critical realists for assuming a too unified view of roles. However, they do not base their critique on Merton’s theory of role sets or the notion of a concrete social system.

The upshot is that insofar as critical realists understand social structures in terms of internally related social positions and roles, this view on the nature of social structures can be misleading in the context of empirical research because it (i) ascribes causal powers to abstract entities (or taxonomic categories), such as social classes and abstractly understood roles; (ii) does not provide conceptual tools to study the complexity of the social structures of concrete social systems; and (iii) fails to systematically separate social structures from social mechanisms. By contrast, the Bungean systemic social ontology that I defend in my book states that only concrete entities – including structured social systems – can have causal powers and mechanisms. It is also perfectly compatible with the kind of complexity of the role structures analyzed in Merton's theory of role sets and can be used to support a methodology based on the ideas of mechanism-based explanation and middle-range theory.

## The CESM Model of System

Finally, let me try to specify the status of Bunge's *CESM* (*Components, Environment, Structure, Mechanisms*) model of a concrete system, since one of Ylikoski's critiques concerned its (lack of) relation to substantial social scientific theories. Firstly, I would say that this model was developed in close contact with the empirical sciences, even though it should not be regarded as systems theory in any substantial sense. It is rather a highly abstract and general model of ontological categories that can be used in the analysis of any concrete system studied in the sciences. So, although the CESM model is not directly empirically testable due its high level of conceptual abstraction and generality, this is not a problem since, unlike some social systems theories, it is not meant to explain any empirically observable phenomena but rather to function as an abstract template that social scientists can use for developing explanatory theories that (if true) refer to specific mechanisms in concrete social systems. Moreover, when applied to social entities, it is possible to argue for the CESM model by showing that it provides a better ontological explanation for certain successful social scientific practices and theories than some competing social ontologies (e.g., ontological individualism and holism that both reject the notion of a concrete social system) (see Bunge 1998, 2003).

Secondly, Bunge (2003, p. 37) holds that in the substantial research conducted in the special sciences, scientists typically utilize a *reduced CESM model* that focuses only on two (or perhaps three) levels of the organization of the concrete system under study (e.g., individual members of the school class and

their artefacts and the school class as a social system with emergent properties) due to the fact that it would be impossible to study all of the interacting components of any complex system. When used in the context of the social sciences, the reduced CESM model then suggests that one should choose the proper level of description first and then specify the system components (e.g., individual members and their artefacts or social subsystems), environment (e.g., the other social systems with whom the members interact), structure (e.g., relations between the components, especially social relations) and mechanisms (e.g., concrete social processes capable of producing or preventing changes in the system or in some of its components) of the concrete social system under study insofar as one wants to build a sociological theory about it (or about some of its aspects, such as its social structure or social mechanisms). In this respect, the status of the reduced CESM model of social systems is similar to Peter Hedström's (2005) DBO (*Desire, Belief, Opportunity*) theory of action, since they both aim to provide useful theoretical concepts and heuristic ideas for the formulation of empirically testable theories and models about social mechanisms in explanatory sociology. Nevertheless, it is entirely an empirical issue how useful the reduced CESM model turns out to be in the context of theoretically informed empirical research.

## Conclusion

I hope to have been able to reply to the core critiques raised by my reviewers and further elaborate on some of the views outlined in my book. I would like to thank the *Journal of Social Ontology* for organizing this review symposium. In addition, I am grateful to all four reviewers for their thoughtful comments and critiques.<sup>6</sup>

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