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Reclaiming Naturalized Critical Realism: Response to McWherter

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ABSTRACT

This paper responds to Dustin McWherter's (2015) detailed critique of my assessment of Roy Bhaskar's method of transcendental argumentation in chapter four of my *Naturalizing Critical Realist Social Ontology* (2013). I begin by describing some naturalist ontological and epistemological views defended in my book, thereby showing that my naturalist challenge to the original version of critical realism is not only methodological (or metaphilosophical) but also substantial. I also indicate that this point is effectively downplayed in McWherter's framing of the debate in terms of competing metaphilosophies. I then consider how the doctrine of transcendental idealism is presupposed in Kant's transcendental deduction and question the consistency of McWherter's various descriptions of Bhaskar's transcendental arguments. Finally, I provide detailed responses to McWherter's objectives to my views. My conclusion is that naturalized critical realism is more coherent and scientifically viable position than the neo-Kantian version of critical realism defended by McWherther. Nevertheless, I think that there is enough overlap between original and naturalized critical realism to regard the latter as a revised and elaborated version of the former.

KEYWORDS: Bhaskar, McWherter, Kant, critical realism, transcendental argument, naturalist philosophy of science

Introduction

Dustin McWhether (2015) provides a detailed critique of my interpretation and assessment of Roy Bhaskar's method of transcendental argumentation, presented in the fourth chapter of my *Naturalizing Critical Realist Social Ontology*. In this article, I respond to McWherter's objectives to my arguments and defend the core argument of my book; namely that critical realism should be naturalized.[1]

I begin by discussing McWherter's framing of his critique in terms of competing metaphilosophies. I argue that this framing is remarkably narrow considering the scope of the arguments advanced in my book, since it leaves out many important arguments and substantial ontological views that are relevant for the assessment of my naturalized version of critical realism. Then, after a few preparatory remarks on Immanuel Kant's transcendental philosophy, I consider McWherter's descriptions of Bhaskar's early transcendental arguments, which I show are mutually inconsistent. I use the rest of the article to respond to McWherter's objections to my critique of Bhaskar's method of transcendental argumentation. I do this by, among other things: developing further my earlier assessment of Bhaskar's a posteriori premises regarding experimentation in physics and chemistry by drawing on studies in naturalist philosophy of experimentation; clarifying my conceptual distinction between explanatory and transcendental necessity; and showing how epistemic success of scientific practices can be assessed empirically. I also indicate a number of weaknesses in Bhaskar's transcendental argument

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[1] The term 'naturalism' has many meanings. I primarily use it to refer to the relation between philosophy of science and its objects (i.e. empirical sciences). Hence, the term 'naturalist philosophy of science' refers to a philosophy of science that is continuous with the empirical sciences and rejects all projects that aim to provide a philosophical foundation for the empirical sciences (e.g. by using a priori arguments). Accordingly, the term 'naturalist social ontology' refers to an ontology of the social world that is develop by means of studying ontological assumptions and presuppositions of the epistemically successful (social) scientific research practices. It is, therefore, continuous with the empirical sciences. 'Naturalized critical realist social ontology' is then a species of naturalist social ontology which draws on original critical realism but is not justified by means of transcendental arguments and does not include entities that cannot be studied by the methods of empirical sciences (for details, see Kaidesoja 2013). Note that this usage of 'naturalism' is different from the notion of 'methodological naturalism' (i.e. a view that the methods of natural and social sciences are essentially similar) which is used in Bhaskar's (1998) second book.

from experimentation that I mentioned in my book but that were not convincingly resolved in McWherter's paper; despite his philosophically sophisticated attempt to reconstruct and defend Bhaskar's argument. I believe that these arguments clarify and support my earlier argument that critical realism should be naturalized.

The framing of the debate

McWherter (2015) frames his paper in terms of the debate between "metaphilosophical naturalism" and "naturalized transcendentalism". Accordingly, he focuses on the philosophical method of transcendental argumentation and uses Bhaskar's transcendental argument from experimentation as his core example. There is nothing wrong with the metaphilosophical focus of this kind, since one aspect of the arguments formulated in my book concerns the proper relation between philosophy of science and the empirical sciences. Nevertheless, in this section, I want to indicate a number of issues that are not considered in McWherter's paper, which are relevant to the assessment of my naturalized critical realism.

Though Bhaskar's (1978, 30-36) transcendental argument from experimentation is surely his most important and elaborated transcendental argument, it is only one among many transcendental arguments that he presents in his philosophical works. In addition, it is clear that this particular argument is not intended to justify all aspects of Bhaskar's early transcendental realist ontology, let alone his critical realist social ontology. For these reasons, it is important to keep in mind that there are many transcendental arguments in Bhaskar's early (and later) philosophy that are significantly different from his transcendental argument from experimentation and that are not discussed in McWherter's (2015) paper. In my book, I develop a critique of Bhaskar's transcendental arguments for his social ontology in addition to my general critique of his transcendental method (see Kaidesoja 2013, 95-98). McWherter focuses only on the latter critique while ignoring the former.

McWherter's paper might also give rise to a misleading impression that my challenge to the original critical realist ontology is exclusively methodological (rather than substantial) and that I have not presented any plausible alternatives to Bhaskar's ontological views. For this reason, it is important to note that my book contains discussions on many naturalized alternatives to some of Bhaskar's key ontological doctrines that, in my assessment, contain problematic anti-naturalist elements. Here is a list of the most important alternatives:

- I defend a naturalist interpretation of the concept of causal power according to which causal powers can be attributed only to concrete entities (or powerful particulars). I argue that this view is not only different from 'the transcendental realist account of the concept of causal power' that can be reconstructed from Bhaskar's early works; but that it is also more compatible with the epistemically successful research practices in the empirical sciences.

- I argue that Richard Boyd's *Homeostatic Property Cluster* theory of biological kinds, such as biological species, offers a promising alternative to Bhaskar's concept of essence-based natural kind that is inconsistent with the research practices of modern evolutionary biology. Boyd's theory is naturalistic in the sense that it is based on careful analysis of the research practices and theories in evolutionary biology.

- I argue that a compositional and gradual account of the concept of ontological emergence that draws on Mario Bunge's and William Wimsatt's ideas (which were developed in close contact with the empirical sciences) not only avoids the ambiguities that I found from Bhaskar's views on ontological emergence but also provides some conceptual tools to empirically study mechanisms of social emergence.

- I develop a broadly Bungean version of the systemic social ontology that includes the concept of concrete social system (with causal powers) as well as some other interrelated concepts (e.g.

social mechanism, social structure, social action, organization and social class). Though it shares many assumptions with critical realism, this social ontology forms an alternative to Bhaskar's doctrine of internal relations and his view on (causally efficacious) social structures as abstract entities that, in my assessment, contain problematic anti-naturalist ontological elements.

- I outline an embodied, situated and distributed approach to action-related human cognition that is rooted in the current cognitive sciences and show that it is incompatible with some aspects of Bhaskar's ontology of mind and action (for details and references, see Kaidesoja 2013; for some further elaborations of these views, see Kaidesoja 2015).

These ontological views should not be regarded as descriptions of 'transcendental conditions' of anything because I do not think that the notion of 'transcendental condition' makes sense in the context of naturalist philosophy of science (cf. McWherter 2015, 72). Despite this, I explicitly present them as naturalist alternatives to the anti-naturalist elements in the original critical realist ontology, as identified and criticized in my book. Due to the significant overlap between the above ontological views and Bhaskar's original ontological views (e.g. commitment to a realist theory of causation in terms of causal powers and mechanisms and to a version of emergent materialist social ontology), I think that these naturalist ontological views together provide a revised and elaborated version of the original critical realist social ontology. For this reason, I use the term 'naturalized critical realist social ontology'.

It is also worth noting here that my book raises naturalist critiques against some aspects of the critical realist epistemology (see Kaidesoja 2013, chapters 2 and 3).[2] For example, I argue that neither Bhaskar nor other prominent critical realists (e.g. Tony Lawson) have developed a plausible epistemological account of how scientific models—that include simplifying and idealizing

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[2] As I acknowledge in the book, many (though not all) of these critiques have been presented before in the commentary literature on critical realism.

assumptions—can be successfully used to represent some aspects of their target systems (e.g. causal mechanisms). None of these critiques are discussed in McWherter's paper. Furthermore, I do not merely formulate these critiques but also argue that a naturalized approach to epistemology of science would help to elaborate critical realist epistemology. I exemplify this view by showing in detail how a realist account of the practice of representing reality with idealized and simplified scientific models can be developed by drawing on the important contributions by naturalist philosophers of science, such as Ronald Giere, Uskali Mäki and William Wimsatt (Kaidesoja 2013, 25-47; 62-63).

In these respects, McWherter's critique leaves out vital aspects of my position. Of course, it is not possible to address everything in one paper. However, I want to draw attention to the absences in McWherter's paper, because I think that they are highly relevant to the understanding and evaluation of my arguments for naturalized critical realism.

Remarks on Kant's transcendental philosophy

This section briefly revisits Kant's transcendental philosophy because some of my disagreements with McWherter concern the relation between Kant's transcendental idealism and his transcendental deduction of the categories of pure understanding (shortly: transcendental deduction). McWherter defends a view that these two can in principle be separated; while I believe that there is a tight connection between them. In order to avoid misunderstandings, my intention here is not to defend Kantian views but rather to indicate how deeply Kant's transcendental deduction is embedded in his larger philosophical system, that he sometimes calls transcendental idealism. These remarks are relevant for the assessment of Bhaskar's transcendental arguments, because Bhaskar formulates his arguments by using Kantian terminology while defending transcendental (or categorial) realism that is rejected by Kant.

To consider the role of 'transcendental arguments' (i.e. transcendental deductions and proofs) in Kant's philosophy, we should begin by asking: What are the aims and distinctive features of Kant's transcendental philosophy? Kant scholar Patricia Kitcher (1996, xxxi-xxxii) describes them as follows:

> The goal of transcendental philosophy is to investigate the necessary conditions for knowledge with a view to showing that some of those necessary conditions are a priori, universal and necessary features of our knowledge, that derive from the mind's own ways of dealing with the data of the senses. The term "transcendental" has often been a source of confusion, because it includes three not obviously related ideas: (1) the idea that some conditions are necessary for knowledge and (2) the idea that some claims are *a priori*, in stating universal and necessary features of the world, and (3) the idea that some features of our knowledge are *a priori*, in the sense that they do not derive from sensory experience, but from our minds' ways of dealing with sensory evidence. What is distinctive about Kant's philosophy is his belief that some of the necessary conditions for knowledge are also *a priori*, in all four sense of the term: they are universal, necessary, cannot be established by sensory experience, and reflect the mind's ways of dealing with sensory experience; the term "transcendental" constantly draws attention to that complex doctrine.

This description of Kant's transcendental philosophy is consistent with my reading of Kant's transcendental arguments and comparison of these arguments to Bhaskar's transcendental arguments (see Kaidesoja 2013, chap. 4).

As Kitcher indicates in the above quotation, an important feature of Kant's transcendental philosophy is that it is focuses on the necessary conditions of possibility for our sensory experience that are assumed to coincide with the necessary conditions of possibility for scientific knowledge. This enables Kant to hold that the premises of his transcendental deduction are uncontroversial since nobody

can consistently deny the existence of her own sensory experience. Nevertheless, insofar as I can see, he does not deny that there are other necessary conditions for our cognitive experience and scientific knowledge than those that are derived from our sensibility and understanding. Kant only argues that we cannot have any knowledge of those other necessary conditions as they are independently of our cognitive experience and knowledge (cf. McWherter 2015, 57-58).

According to the standard interpretation, Kant's transcendental deduction is tightly connected to his transcendental idealist view regarding the objects of perceptual and scientific knowledge. Kicther (1996, xliii) again provides a clear account of the aims and the nature of transcendental deduction in Kant's *Critique of Pure Reason*:

> The purpose of transcendental deduction is to establish the legitimate use of these concepts [i.e. pure categories of understanding –T.K.] in science and metaphysics, by tracing their origins back to the necessary operations of the understanding combining sensory information in a way that makes it usable in cognition. The deduction reveals them to be legitimate by showing that they are indispensable for any cognition at all […].

It is precisely because Kant thinks that these concepts have their origins in our understanding, rather than in sensory data, that he can plausibly regard them as a priori in all four senses of this term. Furthermore, it is important to note that the term 'deduction' is not used here in the sense of logical entailment of the conclusion from the set of premises. Kant rather uses this term in the older legal sense, meaning that the point of his transcendental deduction is to establish *the legitimate uses of certain a priori concepts without recourse to experience* (e.g. Kant 1996, A84).[3]

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[3] The letter A refers to the first edition of the book first published in 1781 while the letter B refers to the second edition published in 1787.

As was already emphasized in my book, for Kant, the transcendental perspective is totally separate from the empirical perspective (e.g. Kaidesoja 2013, 86). This is because he considered transcendental inquiry to consist of infallible claims about the universal and necessary features of "the mind's ways of dealing with experience" that "cannot be established by the sensory experience" (Kitcher 1996, xxxi). It is easy to find textual evidence for this view in Kant's works. In the preface of his *Critique of Pure Reason*, Kant (1996, Axv) for example writes that:

> As regards *certainty*, I have bound myself by my own verdict: that holding *opinions* is in no way permissible in this kind of study; and that whatever in it so much as resembles a hypothesis is contraband, which is not to be offered for sale at even the lowest price but must be confiscated as soon as it is discovered. For, any cognition that is to hold […] a priori proclaims on its own that it wants to be regarded as absolutely necessary. So does, but much more so still, a determination of all pure a priori cognitions, for is it to be the standard and hence is itself to be the [prime] example of all apodeictic (philosophical) certainty.

Hence, in Kant's view, there is no room for fallible claims or hypothesis in transcendental philosophy. His transcendental proofs and deductions and proofs are meant to ground the "absolute necessity" and "apodeictic certainty" of his synthetic a priori propositions. His transcendental deduction is not an exception. For Kant, it would be illegitimate to incorporate a posteriori premises to this philosophical argument because this would compromise the (alleged) apodeictic certainty of its conclusions.[4]

Now, due to the fact that Kant's transcendental deduction concerns our ways cognizing the objects of our experience rather than things-in-themselves considered independently of our cognition,

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[4] In contrast to McWherter's (2015, 63-65) view, I think that Kant understood his transcendental arguments for the synthetic a priori status of Euclidean geometry to be completely a priori. This is because Kant's contention is that our mind (or rather the transcendental subject) somehow imposes this geometrical form to our spatial cognition. Of course, Kant's views on this issue have become highly implausible in the light of subsequent developments in the empirical sciences (see Kaidesoja 2006).

this argument is tightly connected to transcendental idealism. The term 'transcendental idealism' can, in turn, be understood as referring to Kant's philosophical system whose core thesis states that "what we know of objects depends in part of our ways of knowing them" (Kitcher 1996, xxxvi). Accordingly, I think that Kant believes that his transcendental philosophy forms a whole in the sense that its different parts comprise a complete and unified system in which all elements support each other (see, e.g., Kant 1996, Axii-Axiii). Kant's view seems to be, then, that you have to accept this system as a whole if you intend to pursue transcendental inquiries regarding the necessary conditions of possibility for our cognitive experience and scientific knowledge. So, in this interpretation, Kant holds that transcendental arguments that aim to establish particular synthetic a priori proposition are fully intelligible and justified only when considered together with the other doctrines that together comprise his system of transcendental idealism. This is the sense in which I think that Kant's transcendental deduction can be said to presuppose the doctrinal system of transcendental idealism.

McWherther on transcendental arguments

With these views in mind, it is time to move on to consider McWherter's accounts of Bhaskar's transcendental arguments. At the beginning of his paper, McWherter (2015, 55, footnote 2) gives a very broad characterization of transcendental arguments to those unfamiliar with them:

> the purpose of [transcendental] arguments is to begin with some (usually, but not necessarily always, epistemically significant) phenomenon and then contend that something else (e.g., an epistemic principle or ontological structure) makes that phenomenon possible or intelligible and therefore serves as the latter's 'transcendental condition' or 'condition of possibility'.

This very loose characterization of transcendental arguments does not say much about the nature of their premises nor their logical form.

In the same context, McWherter (2015, 55, footnote 2) notes that "Bhaskar gives a conventional account of a transcendental argument's logical form", but he does not explicate this account in his paper. Here we should take a closer look at Bhaskar's (1978, 257) description of the conventional account of a transcendental argument, that McWherter (e.g. 2012, 224) has utilized elsewhere. It can be written as follows:

Major premise: Only if Q, then P

Minor Premise: P

Conclusion: Q[5]

In the context of Bhaskar's transcendental realism, P is a proposition that describes some aspect of epistemically successful scientific practice (e.g. experimentation in physics and chemistry) that actually exists and is intelligible to us. Q in turn is a proposition about the categorial structure of reality (e.g. a claim that empirical regularities observed in experimental settings and causal powers that produce them are ontologically/categorially distinct). On this reading, the aim of Bhaskar's transcendental argument is to show that certain ontological features of the world are among the necessary conditions of possibility (or intelligibility) of the epistemic successfulness of the scientific practice of interest.

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[5] In my book, I reconstructed Bhaskar's transcendental arguments in a way that does not follow this account (see Kaidesoja 2013, 87) because I thought that my interpretation would be more in line with those ontological arguments that Bhaskar actually develops in his early works when compared to the conventional view that he discusses in his reply to his critics. Nevertheless, since McWherter (2015) mentions the conventional account in his paper and has utilized it elsewhere (e.g. McWherter 2012, 224), I decided to address it directly in this paper. This is McWherter's (2012, 225) reconstruction of Bhaskar's transcendental argument from experimentation that uses the conventional account:
  Major premise(s): Only if extra-experimental reality is an open system (Q1), causal laws are not constant conjunctions of events (Q2), and causal laws are the transcendentally real tendencies of generative mechanisms (Q3), then experimental activity is intelligible (P)
  Minor premise: Experimental activity is intelligible (P)
  Conclusion(s): Extra-experimental reality is an open system (Q1), causal laws are not constant conjunctions of events (Q2), and causal laws are the transcendentally real tendencies of generative mechanisms (Q3)

I agree with Bhaskar that, according to the conventional account, transcendental arguments exhibit a quite trivial form of inference. Bhaskar (1978, 257) nevertheless acknowledges that the philosophically interesting part of these arguments does not consist in their "deductive form" but "in the production of the knowledge of the major premise." This process in which the major premise is established can be called *transcendental analysis*. The purpose of transcendental analysis is to provide us with reasons to hold that Q truthfully describes the (transcendentally) necessary condition(s) of P.

Now, it is crucially important to note that if we accept the conventional account of transcendental arguments, then we have to commit ourselves to the view that *there can be only one valid Q (or only one valid set of Q's) for a given P*, since, according to the major premise, P follows "*only if* Q". In other words: if Q is not the case, then P does not follow. If we assume that our description of P is true, then this view comes quite close to the Kantian notion of transcendental necessity which suggests that transcendental arguments can be used to justify synthetic a priori propositions that are infallible and absolutely certain (see the above discussion on Kant's transcendental deduction).

Nevertheless, Bhaskar (1978, 260) clearly rejects the latter view when he writes that "there is no way of demonstrating the uniqueness of the conclusion of such an [transcendental – TK] argument in advance of every possible philosophical theory" and that "I have not demonstrated that transcendental realism is the only possible theory of science consistent with these activities [i.e. those activities analysed in his RTS – TK]; only that it is the only theory *at present* known to us that is consistent with them." This in turn suggests that it is always possible that there is some ontological view Z that describes a necessary condition of possibility of a given P (while being inconsistent with the Q), even though Z may not be known at the moment when the transcendental argument is presented. Now, given this, it follows that Bhaskar cannot consistently accept the conventional account of transcendental arguments insofar as he rejects the formulation of its major premise (i.e. "Only if Q, then P").

To put the same point in other way: It is surely one thing to claim that (i) P is possible (or intelligible) "Only if Q" and that (ii) currently the best account of the possibility (or intelligibility) of P that we know is Q, and we cannot a priori rule out the possibility of the alternative accounts (e.g. Z) that equally well (or better) explain the possibility (or intelligibility) of P while being inconsistent with the Q. Furthermore, I think that Bhaskar's transcendental arguments cannot be plausibly interpreted as deductive arguments (in the modern sense of the term) if we accept the claim (ii) instead of the claim (i). Note that, according to modern deductive logic, the inference from the slightly revised major premise "If Q, then P" and minor premise "P" to the conclusion "Q" is a well-known fallacy called *affirming the consequent*. It is regarded as a fallacy in deductive logic precisely because we cannot deductively infer that "Q" is the case even if we know that "If Q, then P" and "P" are both true, since we cannot rule out the possibility that "If Z, then P" is also true.[6] Now, despite these ambiguities in Bhaskar's views regarding the nature of transcendental arguments, the conventional account is the only characterization of the logical form of transcendental arguments in his first two books.

A similar ambiguity can be found in McWherter's paper. He seems to reject the conventional account of the logical form of transcendental arguments, since he states, for example, that "no transcendental argument can guarantee that there cannot be some unconsidered alternative transcendental condition [i.e. Z – TK] that equally or better explains the possibility of the explanandum [i.e. P – TK]" (McWherter 2015, 60; also 73). Insofar as I can see, this view is inconsistent with the major premise in the conventional account. Nevertheless, in the footnote 13 of his paper McWherter (2015, 57) states that "I think the sense in which Bhaskar's transcendental arguments are 'apodictic' should probably be understood in terms of the logical connection between the premises and conclusion", suggesting that this connection is one of logical entailment (i.e. deductive inference). In the same footnote, he also refers to

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[6] I presented essentially the same point in other terms in my book (see Kaidesoja 2013, 93-94).

Mervyn Hartwig's (2015, xv) recent text in which we find again the conventional account of transcendental arguments described above that is said to be "[t]he final logical form of a transcendental argument" that was accepted by Bhaskar. As already mentioned above, McWherter (2012, 224-225) also uses the conventional account in his earlier reconstruction of Bhaskar's transcendental argument from experimentation. So does this mean that McWherter after all thinks that Bhaskar's transcendental arguments are consistent with the conventional account of transcendental arguments? If this is the case, then his views regarding Bhaskar's transcendental arguments are inconsistent. According to my diagnosis, inconsistencies such as these can be expected for commentators of Bhaskar's works since his transcendental arguments are formulated ambiguously (see Kaidesoja 2013, chap. 4).

Furthermore, McWherter (2015, 71) states that Bhaskar has "separate transcendental arguments for the impossibility of causal laws being regularities on the one hand and for causal laws instead being the tendencies of generative mechanisms on the other." So McWherter seems to contend that Bhaskar's critique of the Humean regularity theory of causation consists of a separate transcendental argument from his positive argument for his transcendental realist account of tendencies. Now, this critique is surely quite different from the conventional account of transcendental arguments that, insofar as I can see, does not explicitly include any critiques of alternative views (cf. McWherther 2012, 225). Though it surely makes sense to claim that the conclusions of Bhaskar's transcendental arguments for his transcendental realist ontology become more credible once he has shown that (some of) the alternative ontological views are problematic, I nevertheless fail to see how critiques of this kind would comprise transcendental arguments in their own right. As I have argued elsewhere, I think that they are better characterized either as *reductio ad absurdum*-type of arguments or *immanent critiques* than transcendental arguments (Kaidesoja 2015, 361-362). This in turn suggests that one can accept Bhaskar's critique of Humean

regularity theory while rejecting his positive transcendental arguments for his transcendental realist ontology.

Elsewhere McWherther considers Bhaskar's (e.g. 1986, 7) later view that transcendental arguments are subspecies of *retroductive arguments*. In this context, McWherter (2015, 73) claims that Bhaskar thinks that "transcendental analysis is neither more certain nor logically different than scientific explanation." Now, one might ask: what exactly are these retroductive arguments and how are they related to the conventional account of transcendental arguments? Here is Bhaskar's (1986, 7) answer to the first question: "A retroductive argument moves from a description of some phenomenon to a description of something which produces it or is a condition for it." This broad characterization surely fits with the mechanism-based explanations developed in the empirical sciences and, therefore, seems to support McWherther's claim that there is no logical difference between Bhaskar's transcendental arguments and scientific explanations.

But if transcendental arguments really are retroductive arguments of this kind, then there is no way in which one may consider them as deductive arguments in terms of modern logic because retroductive arguments include *ampliative inferences* while the premises of deductive arguments already contain all of the information that is needed to infer the conclusion.[7] This means that even if the description of the phenomenon in a retroductive argument is true, it does not guarantee the truth of the description of "something which produces it or is a condition for it". For this reason, retroductive arguments are different from the conventional account of transcendental argument insofar as the latter is considered as a deductive argument.

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[7] In addition, retroductive arguments in Bhaskar's sense are clearly different from Kant's transcendental deductions, since the latter proceed a priori while the former may well involve recourse to experience or empirical observations.

McWherther does not clearly say whether or not he thinks that transcendental arguments are retroductive arguments. This nevertheless seems to be the sense in which he talks about "naturalized transcendental arguments" (McWherter 2015, 76-78). If this is the case, then these "naturalized transcendental arguments" are not compatible with McWherter's (2012) own reconstruction of Bhaskar's transcendental argument from experimentation since this reconstruction presupposes the conventional account that, as was indicated above, is different from the retroductive account of transcendental arguments.

Now, given these inconsistent accounts of transcendental arguments, it is hard to see in which sense McWherter defends transcendental arguments in his 'naturalized transcendentalism'. Next I will move on the latter part of this paper which addresses in detail McWherter's objections to my arguments.

Transcendental arguments and transcendental idealism

According to McWhether (2015, 56-62), my critique of Bhaskar's transcendental arguments is question-begging because it takes for granted that there is a necessary connection between Kantian transcendental arguments and transcendental idealism (hereafter: "TA-TI –connection", as it is abbreviated by McWherter). Furthermore, he claims that it is not enough for me to just point out that Kant advocates the TA-TI connection but I must also show that this view is (or can be) justified if I want to use it in my argument against Bhaskar's transcendental arguments which reject this connection. In McWherter's view, Kant is wrong to endorse the TA-TI connection in the first place (or at least his justification for this connection can be undermined by referring to some revisionary interpretations of Kant) and, therefore, Kantian transcendental arguments can in principle be used to justify Bhaskar's transcendental realist ontology that is incompatible with transcendental idealism. This in turn shows, according to

McWherter's assessment, that my critique of transcendental arguments that presupposes the TA-TI connection cannot be rectified.

Now, if I wanted to provide a conclusive argument against Bhaskar's philosophical method by presupposing the validity of Kantian transcendental idealism and the necessity of TA-TI connection, then McWherter would be right in arguing that my critique is question-begging since Bhaskar surely rejects both. Though I admit that some of my earlier formulations place too much weight on the TA-TI connection (e.g. Kaidesoja 2013, 83-84), I will argue here that, contra McWherter's views, my comparison between Kant's transcendental deduction and Bhaskar's transcendental arguments can be used to indicate certain weaknesses in Bhaskar's transcendental method, even though it does not presuppose the validity of Kantian transcendental arguments and transcendental idealism. Since this kind of criticism of Bhaskar's transcendental arguments does not aim to be a conclusive proof of impossibility, it does not presuppose that the TA-TI-connection is justified. It only presupposes that the TA-TI interpretation of Kant is the standard one that can be backed up with textual evidence.

One of the key aspects of my critique of Bhaskar's transcendental method concerns the ambiguities that result from his adoption of Kantian terminology (e.g. the terms such as 'synthetic a priori truth', 'pure reason', 'apodeictic demonstration', 'transcendental deduction' and 'transcendental necessity') in his descriptions of the nature and uses of this method. Some of those ambiguities were already mentioned above. In this context, it is important to note that because the TA-TI-interpretation of Kant is the orthodox way of reading Kant's transcendental philosophy, as is acknowledged by McWherter (2015, 56), and because, according to this interpretation, the abovementioned Kantian terms are tightly connected to Kant's system of transcendental idealism (as was shown above as well as in my book), it is a remarkable fact that Bhaskar does not develop in detail alternative accounts to those Kantian terms that he uses in his early philosophy.

I think that McWherter's paper similarly fails in this respect. This is because, as was argued above, his characterization of the nature of transcendental arguments is ambiguous in the same way as Bhaskar's. I would also say that a mere plea for the dialectical continuity between Kant's and Bhaskar's philosophy and reference to (more recent) revisionary interpretations of Kant's philosophy[8] are not enough to justify the uses of Kantian terminology in ontological argumentation unless one carefully specifies the non-Kantian meanings in which these Kantian terms are used *in the context of the (original) critical realist philosophy of science*. Hence, it is not sufficient to assert that these terms may be semantically separated from Kant's views unless one does not develop a detailed philosophical system in which they are clearly defined such that they are semantically separate from Kant's views (cf. McWherter 2015, 58-62). In this respect, the burden of proof is still on the defenders of Bhaskar's transcendental method of ontological argumentation.

Furthermore, even if one rejects the TA-TI interpretation of Kant, the lack of detailed specification of the transcendental method still forms a problem in the original critical realist ontology. For example, despite the fact that Bhaskar (e.g. 1998, 170) states that the conclusions of his transcendental arguments should be considered as fallible, his (and his followers') confidence in these arguments to decisively resolve ontological debates is remarkably strong. This in turn suggests that they consider transcendental arguments as less fallible than many of the other types of arguments, including those developed in the empirical sciences. It is not at all clear what justification Bhaskar and his followers have for this belief given that many aspects of the conclusions of Bhaskar's ontological arguments have been seriously challenged from the viewpoint of current philosophy of science and developments in the empirical sciences (see Kaidesoja 2013). Accordingly, in his discussion on Bhaskar's transcendental

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[8] In one of my earlier papers, I commented on the relevance of Kenneth Westphal's revisionary interpretation of Kant to the debate on critical realist ontology (see Kaidesoja 2006). Here I have nothing to add to these comments.

arguments, Daniel Little (2015, 351) indicates that "the general statement 'all assertions are fallible' is too abstract to help very much. We want to know what the conditions of knowledge are for different kinds of assertions, and how confident we can be, given available reasons and evidence, that the given assertion is true." Since Bhaskar does not clearly answer queries of this kind in his writings and places much weight on his transcendental arguments to reach conclusive ontological conclusions, Little (2015, 353) contends, rightly in my view, that Bhaskar can be said to rely "too heavily and confidently on philosophical methods [e.g. the method of transcendental argumentation – TK] to arrive at ontological conclusions." In my view, this problem of overconfidence is not resolved by merely detaching the transcendental method of argumentation from the TA-TI -connection. A better way to overcome it is to replace Bhaskar's transcendental method of ontological argumentation with the naturalist method that saves the reasonable core of the former.

A posteriori premises of Bhaskar's transcendental arguments

The second set of McWherter's objections revolves around my assessment of Bhaskar's a posteriori premises in his transcendental argument from experimentation. In particular, he claims that I have not provided any good reasons to disagree with these premises (McWherter 2015, 66-68). Whilst I grant that I did not provide a detailed discussion of the premises of this particular transcendental argument, my argument was not that Bhaskar's a posteriori premises are completely false and should all be rejected (cf. McWherter 2015, 67-68). I rather indicated that Bhaskar's "highly abstract description of scientific experiments is oversimplified and one-sided" as well as "in need of certain specifications and revisions" (Kaidesoja 2013, 89) in the light of empirical analysis of scientific experimentation in different sciences. Accordingly, I argued that Bhaskar focuses only on a specific type of experiments in physics and chemistry and does not discuss the plurality of different kind of experiments in the current sciences (e.g. randomized controlled experiments, natural experimets and field experiments). I think, however, that

McWherter is right to argue that these abstract claims alone are not enough to repudiate Bhaskar's a posteriori premises regarding those experiments that he analyses.

In this section, I first discuss the role and content of a posteriori premises in Bhaskar's transcendental argument from experimentation. I then question some aspects of these premises by drawing on the two studies in the naturalist philosophy of experimentation. These studies focus the type of experiments in physics and chemistry to which Bhaskar refers in the premises of his argument.

Bhaskar (1978, 261) contends that the premises of his transcendental arguments include reference to "some characteristic activity as conceptualized in experience". Nevertheless, this statement is pretty vague unless it is specified whose experience counts in the case of scientific practices. This is because we cannot rule out a priori the possibility that the same scientific practice is experienced and conceptualized quite differently by different people (e.g. scientists, non-scientists, sociologists of science and philosophers of science). In order to answer queries of this kind, Bhaskar emphasizes the critical context of his transcendental arguments. He indicates that it is his opponents' experience that counts, since the force of his arguments "depend upon the acceptability and acceptance […] of the minor premise concerned" (Bhaskar 1978, 260). Hence, given the context in which his transcendental arguments for the transcendental realist ontology were originally developed, it was a consensus among philosophers of science in the late 1970's about their experience and conceptualization of the core features of natural scientific practices (or activities) that was regarded as the relevant experience in Bhaskar's early transcendental arguments.

In my book, I indicate, among other things, that one cannot assume a priori that philosophers of science would end up in a consensus about how to conceptualize a scientific practice, such as experimentation (Kaidesoja 2013, 89). Here, I am not interested whether or not there actually was a

consensus among philosophers of science about the core features of experiments in physics and chemistry in the late 1970's. Rather, I will assess Bhaskar's premises from the viewpoint of the current naturalist philosophy of science. Before identifying those features in Bhaskar's premises that should be specified and revised, it should be stipulated what exactly these premises are.

I rely here on McWherter's (2015, 67; cf. 2012, 203-304) useful account of Bhaskar's four initial premises:

(1) natural scientific experimentation is an intelligible activity

(2) scientists initiate the sequences of events that are experienced in experiments

(3) regular sequences of events are available in experiments

(4) these experimental regularities somehow allow epistemic access to causal laws (however such laws may be conceived)

So let us assume that these four a posteriori premises should be accepted for Bhaskar's transcendental argument from experimentation to get off the ground. In what follows I address the question: Are these premises acceptable in the light of more recent naturalist philosophy of science?

I believe that the first premise can easily be accepted by naturalist philosophers of science. They do not question the intelligibility of experimentation but they rather aim to deepen our understanding of this important scientific practice. By contrast, I will argue that the premises (2), (3) and (4) are not at all uncontroversial among the naturalist philosophers of science. In particular, I try to show why their empirical analyses of experimental practices in physics and chemistry give us good reasons to reconsider some aspects of Bhaskar's premises. As we shall see, the assumptions that "scientists initiate and experience regular sequences of events in experiments" and that "these experimental regularities

somehow allow epistemic access to causal laws", in particular, turn out to be problematic. I begin with the first assumption and, then, assess the second.

Although the importance of intentional actions of experimenting scientists are emphasised by the naturalist philosophers, many of them would dispute the claim that reference to "regular sequences of events" experienced by scientists is the most adequate way to describe the nature of experiments in physics and chemistry. In order to discuss reasons for this view, I will examine James Bogen's and James Woodward's (1988) influential paper on epistemology of experimentation in which they contend that the conceptual distinction between *data* and *phenomenon* is crucially important for understanding scientific experimentation. They argue for this view by indicating that data produced in experiments and phenomena explained by theories serve quite different functions in experimental research: data provides evidence for some scientifically interesting phenomenon while facts about this phenomenon provide evidence for a general theory that aims to explain the phenomenon (or some of its aspects). For example, data produced in experiments may include "bubble chamber photographs, patterns of discharge in electronic particle detectors and records of reaction times and error rates in various psychological experiments" that provide evidence for the existence of phenomena such as "weak neutral currents, the decay of the proton, and chunking and recency effects in human memory" (Bogen and Woodward 1988, 306). Their other reason for this distinction is that only data (e.g. the results of measurements achieved by using various research instruments in experimental settings) can typically be observed by experimenters while scientifically interesting phenomena are mostly unobservable since they have to be inferred on the basis of data analysis.

In order to support these views, Bogen and Woodward (1988) argue that data generated in experiments is not directly about the phenomenon of scientific interest because the results of measurements that experimenters observe typically contain influences of various other causal factors

than those related to the phenomenon. This point can be exemplified by citing their description of experiments that are conducted to estimate the melting temperature of lead:

> While the melting of lead occurs whenever samples of lead are present at the appropriate temperature and pressure, and results from a characteristic change in the crystalline structure of this metal, the observed value of each thermometer reading depends not just on the temperature at which the lead began to melt, but on various perceptual and cognitive factors at work in the observer, on the various factors which determine the workings of the thermometer, on the mechanisms by which heat is transmitted from the sample to the thermometer, and no doubt on various other sources of random and systematic error as well. (Bogen and Woodward 1988, 319)

So the phenomenon of interest in this example is the melting point of lead while the relevant data consists of the observed thermometer readings that are generated by complex causal processes involving various and changing causal factors that may be partially unknown to experimenters. Therefore, even when they use a thermometer that is known to be reliable, experimenters have to estimate the exact value of the melting point by taking into account many potential sources of error that are partly unknown to them. They do this by calculating the mean of a distribution of thermometer readings and assuming that sources of error operate randomly and independently of each other. For these reasons, Bogen and Woodward contend that, strictly speaking, the melting point of lead cannot be perceptually experienced (or observed) by experimenters. It is rather inferred on the basis of the data generated by experimenters through many measurements in suitable experimental settings. The estimated value of melting point of lead can, in turn, be used as evidence when scientists assess, for example, competing explanatory theories about the molecular structure of lead.

According to Bogen and Woodward (1988), these points apply to more complex experiments in physics and chemistry as well, since various causal factors always interact in complex and idiosyncratic ways in different instances of experiments that are conducted in these (and other) sciences. Hence, the effects of confounding causal factors can be partially detected and controlled only by means of repeated measurements and uses of statistical techniques to estimate the values of variables that aim to describe the phenomenon of interest. In addition, experimenters often detect the same phenomenon by using independent data sets that were produced in different experimental settings since this enables them to show that the phenomenon of interest is robust rather than an artefact of a certain type of instrumentation and experimental design. For these and some others reasons, Bogen and Woodward argue that experimental data generated by repeated measurements in the specific experimental setting have to be processed by means of various statistical techniques of data analysis before it can be used as evidence for the specific phenomenon of scientific interest.

Insofar as we accept that the conceptual distinction between data and phenomenon is crucial for the adequate description of experimentation in physics and chemistry, it can be argued that Bhaskar's premises (2), (3) and (4) about experimentation should be revised in the light of this distinction. This is because these premises blur the distinction between data and phenomenon: They do not clearly state whether experimenters are supposed to experience data or phenomena, nor do they specify whether data or phenomena (inferred on the basis of data analysis) are considered as providing epistemic access to causal laws in experiments. Perhaps it might be suggested, by utilizing Bogen and Woodward's terms, that Bhaskar's view is that experimenters in physics and chemistry perceptually experience (by using the suitable observational instruments) phenomena and that these experiences, in turn, provide empirical

evidence for their general theories about causal laws.[9] But if Bogen and Woodward are right, then this view is mistaken because phenomena cannot typically be perceptually experienced in any plausible sense.

To conclude: insofar as we accept Bogen and Woodward's (1988) arguments about the importance of the distinction between data and phenomena, then Bhaskar's premises about experiments are in need of certain revisions. In my view, critical realists should at least seriously consider this distinction, because Bogen and Woodward seem to be able to provide not only more detailed but also empirically more adequate descriptions of experiments in physics and chemistry when compared to Bhaskar's descriptions.

It is worth adding that the distinction between data and phenomena may also suggest revisions to the original critical realist ontology. Even though they are realist about particular phenomena investigated in scientific experiments, Bogen and Woodward (1988, 321-322) argue that different types of phenomena form an ontologically heterogeneous collection that does not neatly fall into any of the traditional ontological categories (e.g. object, object with features, event, process, or state). For this reason, their analysis questions the view that experimental phenomena should always be understood in terms of "sequences of events" (cf. Bhaskar 1978, 33-34). Furthermore, on the basis of their analysis of experimentation, it can be argued that Bhaskar's ontological distinction between open and closed systems should be understood in gradual terms since, unlike what is sometimes claimed by Bhaskar (1978, 46), experimenters typically fall short of constructing completely "closed systems" that would isolate a

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[9] This view resembles Bhaskar's (1978) distinction between empirical experiences, actual events and transcendentally real causal laws.

particular causal mechanism from all other causal mechanisms and factors.[10] So in these respects Bogen and Woodward's paper also challenges some aspects of Bhaskar's early transcendental realist ontology.

Another issue I want to raise here concerns the notion of causal law in the premises of Bhaskar's transcendental argument and, more generally, his account of the relation between causal laws and experimentation. Bhaskar (e.g. 1978, 12) clearly assumes in his premises that statements about universal causal laws should be considered as essential parts of physical theories, even though he is not yet committed to any specific interpretation of the semantics of law statements. In addition, premise (4) in the McWherther (2015, 67) reconstruction explicitly says that "experimental regularities somehow allow epistemic access to causal laws". Bhaskar (1978, 33) also suggests more straightforwardly that an experiment we are conducting "enables us to identify" causal laws. In a sense, the distinction between data and phenomena can be used to question this conception as well. It can be argued that, strictly speaking, an experiment in physics or chemistry typically provides an opportunity to *identify* phenomena rather than general causal laws; though experimental descriptions of a phenomenon may be used as evidence for the general theories about causal laws. Nevertheless I discuss here a slightly different (though parallel) line of argument that problematizes Bhaskar's description of how experiments in physics are used to empirically evaluate theories about causal laws.

In his book *Representing and Intervening*, Ian Hacking argues that practices of experimentation in physics are relatively autonomous from discussions on competing theories in theoretical physics. Accordingly, the point of Hacking's (e.g. 1983, 26-31) "entity realism" is to defend realism about some "unobservable entities" studied in experimental physics without committing himself to a realistic interpretation of any general theory about these entities or (non-phenomenological) "universal causal

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[10] I also indicated in my book that the world outside laboratories may not be as open as is sometimes suggested by Bhaskar, because context-dependent statistical regularities can surely be detected in "open systems" by using suitable statistical methods (Kaidesoja 2013, 60; for a similar view, see Benton 1981).

laws" that allegedly govern their behaviour. His position is motivated, among other things, by the observations that (i) practical details related to designing and running experiments in physics often have relatively little to do with general theories in physics, (ii) experiments in physics not only function as empirical tests of general theories but may also reveal new phenomena that are not predicted by any existing theory and (iii) experimenters in physics often use idealized and mutually incompatible theoretical models for different epistemic purposes.

According to Hacking's (1983, chapter 16) realist argument from the practice of experimentation, the unobservable entities that experimenters use in their experiments can be considered as real because of those practical operations that experimenters can do with them in order to *intervene on some other entities*. For example, Hacking (1983, 24) notes that experimenters in physics routinely use standard emitters "with which we can spray positrons and electrons". He further contends that experimenters can be said to understand the causes and effects of spraying to the extent that they can spray positrons and electrons "to find out something else" (Hacking 1983, 24). According to Hacking's view, it is precisely this practical feature of experimentation that provides us with a good reason to regard electrons and positrons as real entities. The same point is expressed in his slogan: "*if you can spray them then they are real*" (Hacking 1983, 23).

Though it is not necessary to address the details of his analysis of experiments in physics, it is important to understand that both Hacking's descriptions of experiments in physics and his philosophical conclusions that are based on these descriptions are significantly different from those of Bhaskar. For one thing, Hacking considers the relation between experiments and (non-phenomenological) causal laws to be much more complex than Bhaskar, even though he does not comment on Bhaskar's work.[11] It is

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[11] In addition to Hacking, there are also other philosophers of science who have explicitly questioned the traditional philosophical accounts of 'universal scientific laws' (e.g. Cartwright 1983; Giere 1999; 2006; Wimsatt 2007).

also clear that Hacking's position of entity realism is different from Bhaskar's transcendental realism, since Hacking (e.g. 1983, 37) explicitly separates his "*realism about entities*" from "*realism about theories*" whereas Bhaskar does not make a distinction between these two views. Nevertheless, there are also interesting similarities between their ontological views, since Hacking (1983, chap. 2), for example, uses the term 'causal power' in his book and, accordingly, advocates a non-Humean view on causation. In these contexts, he nevertheless refers to Nancy Cartwright's (1983) work rather than Bhaskar's.

Here I do not defend Hacking's entity realism against Bhaskar's transcendental realism. My point is rather to indicate that Hacking's empirical analysis of experimentation questions Bhaskar's conception that experiments in physics can be assumed to provide experimenters with an epistemic access to general causal laws. This is precisely the view that Hacking rejects on the basis of his analysis, meaning that he would surely dispute the premise (4) in Bhaskar's argument. Moreover, Hacking's work provides an additional challenge to Bhaskar's transcendental argument. Hacking's ontological views can be described as neither empirical realist nor transcendental realist while Bhaskar seems to assume that all competing ontological views relevant to his argument belong to the category of empirical realism. I am not aware that Bhaskar or other critical realists have answered these challenges. I also fail to see how this can be plausibly done without revising some of Bhaskar's a posteriori premises regarding experimentation in physics and chemistry.

In sum I believe that the previous remarks contain good reasons to reconsider Bhaskar's a posteriori premises of his transcendental argument from experimentation. I also suggest that naturalization of critical realism would provide a better strategy to engage in recent discussions on experimentation in philosophy of science than Bhaskar's method of transcendental argumentation, since naturalist ontological arguments include empirical analysis of scientific practices as their core component (Kaidesoja 2013, chapter 4).

Naturalist ontological arguments and transcendental arguments

The third and final set of McWherter's (2015, 75-78) objections concerns the relation between Bhaskar's transcendental arguments and my naturalist alternative to them. In this context, he argues that the contrast between the explanatory necessity and the transcendental necessity is not sufficiently clarified in my book and that there is no significant difference between these concepts insofar as we consider Bhaskar's transcendental arguments from experimentation. Accordingly, he also claims that my sketch of naturalist ontological arguments includes the same kind of a priori reasoning as Bhaskar's transcendental arguments. For these reasons, McWherter (2015, 78) concludes that the naturalist ontological arguments cannot be considered to be genuine alternatives to (his reconstruction of) Bhaskar's transcendental arguments from experimentation.

I agree with McWherter that the concept of explanatory necessity could have been further clarified in my book, but I disagree with his claim that there is no significant difference between the notion of explanatory necessity in my naturalist ontological argument and Bhaskar's notion of transcendental necessity. In this section, I clarify the concept of explanatory necessity and show how it differs from the (conditional version of) transcendental necessity. I also argue that a priori reasoning from a set of a posteriori premises to their (allegedly) transcendentally necessary conditions is one of the features that separate transcendental ontological arguments from naturalist ontological arguments (assuming that the former can be consistently specified). Though I already presented this argument in my book (see Kaidesoja 2013, 93-95), the clarification of the concept of explanatory necessity enables me to specify some details of this argument.

When I introduced the term 'explanatory necessity' in my book, I had in mind the method of causal analysis in terms of necessary and sufficient conditions. Here I take a closer look at this method

and use it to clarify the meaning of explanatory necessity in my naturalist ontological argument. My example of the uses of this method of causal analysis in empirical sciences concerns historical explanations of events and processes.

Historians often ask what had to be in place before certain historical event (or process) could have happened at all (i.e. what were its necessary conditions) and what actually were the actions of concrete historical actors (or types of actors) embedded in specific circumstances which generated the event or process of interest (i.e. what were its sufficient conditions). To take a concrete example, a historian interested in explaining the historical emergence of the first newspapers in Finland in the late eighteenth and early nineteenth century may argue that the availability of printing presses, paper manufacturing and enough literate people were among the necessary conditions for the formation of the first newspapers. She may continue her explanatory argument by suggesting that these necessary conditions were not by themselves sufficient for the emergence of newspapers in Finland. In addition, the establishment of the first newspapers required that certain people (e.g. H.G. Porthan, J.V Snelmann and J.L.Runeberg) and organized groups (e.g. The Fennomans) with suitable financial resources did found and run newspapers for various purposes in specific social contexts. These purposes included, for example, the promotion of political interests of specific interest groups and social classes as well as the creation of a sense of national identity under the rule of Russia. These kinds of descriptions of the actions of various individuals and collective actors in specific social contexts can then be regarded as accounts of the sufficient conditions for the emergence of newspapers in Finland. These conditions were not necessary because those people and organized groups (with their reasons for their actions) as well as the specific contexts of their actions could have been very different, whereas it is hard to imagine newspapers without printing presses, paper manufacturing and literate readers. In this way, a historian may construct her explanatory narrative by

using the distinction between necessary and sufficient conditions in order to differentiate the two types of causes in the causal field of her interest.

Though the distinction between necessary and sufficient conditions is too crude and/or misleading for many purposes in explanatory historical research and it may also turn out to be somewhat vague in many cases, I think that there are also cases where this distinction can be made relatively clearly. The historical emergence of Finnish newspapers appears to be a case in point. So my claim is not that this is the only or the best method of causal analysis in historical research, let alone in other empirical sciences. Rather, I want to indicate that it is one of the methods of causal analysis used in empirical sciences (encompassing not only natural but also social sciences and some humanities) and that it includes a relatively specific account of the concept of explanatorily necessary condition (see e.g. Braumoeller & Goertz 2000).

Moreover, this concept of explanatory necessity is different from the Kantian notion of transcendental necessity, because it is entirely an empirical question whether historians' accounts of the necessary and sufficient conditions of a certain historical event (or process) are true. This means that historians have to justify their fallible analyses of the necessary and sufficient conditions for the historical event of interest by means of providing relevant empirical evidence for their accounts of these conditions. The evidence typically includes descriptions (or reconstructions) of the particular instances of explanandum on the basis of the historical traces of various kinds. For example, old printing presses, samples of the old Finnish newspapers and historical documents that pertain to the establishment and governance of newspapers can be used as evidence in the above argument. In this sense, there is nothing a priori in this type of explanatory reasoning: its inferences and conclusions should always be evaluated

in the light of available historical evidence and historians may also try to find new historical evidence to empirically assess explanations of this kind.[12]

I argue that the concept of explanatory necessity works in an analogical way in the context of naturalist ontological inquiry, even though the task of ontological inquiry is different from that of explanatory historical research. For example, we can say that our hypothetical accounts of the necessary ontological conditions of the epistemic successfulness of scientific experiments in physics aim to refer to those ontological features of the aspects of the world that have to be in place in successful experiments of this kind. To my mind, the most reliable way to produce and evaluate knowledge of these conditions is to empirically analyse scientists' written and verbal descriptions (e.g. publications written on the basis of experiments or descriptions that scientists give about their experiments in interviews) of their scientific experiments and their actual experiments (e.g. by doing participatory observation in a laboratory). These types of empirical evidence can then be used in the development and assessment of the premises, inferences and conclusions of the naturalist ontological arguments. Due to their limited resources and abilities, naturalist philosophers may also have to rely on the results of empirical studies done in science studies but the connection between their ontological arguments and empirical analysis is still crucial.

Furthermore, there are typically many competing ontological explanations for a scientific practice. It is the task of the naturalist philosopher to find out which explanation is the best on the basis of available evidence. This is the reason why naturalist ontological arguments can be described as inferences to the best explanation. Nevertheless, it may also turn out that there are two or more competing accounts of those ontological conditions that equally well explain the epistemic successfulness of the practice and that there is no way to decide between them on the basis of available empirical evidence (cf.

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[12] I also think that this kind of explanatory reasoning in terms of necessary and sufficient conditions can be combined with mechanism-based explanations but do not justify this view here.

Ylikoski 2015, 225-226). I think that in cases like these, we just have to admit that, at the current situation, there is no one best ontological explanation for the scientific practice under analysis. This is not to deny, however, that further empirical analysis of the practice of interest may well provide new evidence that helps to decide between the competing ontological explanations. I think that the situation is pretty much the same in the (other) empirical sciences.

The previous sketch of the naturalist ontological argument presupposes that we are able to identify epistemically successful scientific practices. In his paper, McWherter (2015, 69) notes I have not specified "what constitutes empirically assessable epistemic success" and that I have not provided "reliable principles for judging the epistemic success of scientific practices". I admit that these worries are legitimate in the sense that naturalists surely reject the possibility of providing any extra-scientific (or a priori) justification for the epistemic norms used in science and, therefore, rely on the empirically assessable epistemic success.

Part of my answer to these worries is simply to grant, on the basis of my own observations and empirical sciences studies, that the normative criteria that scientists actually use (and have used) to evaluate epistemic success are relatively context-specific and, therefore, it is not feasible to postulate any universal criteria for epistemic success (e.g. Kaidesoja 2013, 101). It is important to bear in mind that this does not amount to saying that anything goes in science, because communities of scientists working in a specific field often use the specific normative criteria of epistemic success that are more or less collectively accepted by the relevant researches. Hence, naturalists may well empirically identify and apply the same criteria of epistemic success that are used by the relevant scientific community. Nevertheless, sometimes there may be good reasons to ask whether the epistemic criteria used by a community of scientists are reliable at all. For example, the research results produced by researchers belonging to a community of parapsychologists may appear seriously biased or misleading to

psychologists working outside this particular community even though insiders may consider their practices as epistemically successful. Do we then have to introduce some extra-scientific (or a priori) norms of epistemic success in order to assess whether the operative epistemic norms in cases of this kind are well justified?

I think not. In order to argue for this view, I will utilize Ronald Giere's (e.g. Giere 2008; cf. Kaidesoja 2013, 27-28) naturalist account of epistemic norms in science. Giere (2008, 219) assumes that the operative epistemic norms in science can always be written in the form of conditional norm: "if the goal is G, use method M." Insofar as this assumption holds, then, according to Giere (2008), it is possible to empirically assess how reliable a certain method M (or the scientific practice that is built upon the uses of the method M) actually is (or has been) in obtaining the goal G. This procedure of assessing the justification of specific epistemic norms related to the method M is empirical in the sense that a naturalist philosopher of science has to utilize some other empirical methods of the sciences in order to evaluate the justification of epistemic norms pertaining to the uses of the method M. It follows from this that it is not possible to assess the justification of all scientific methods simultaneously even though the justification of them all in principle can be empirically evaluated one by one. Hence, this procedure enables us to assess empirically, for example, the criteria of epistemic success used by parapsychologists. Now, I think that these remarks are sufficient to give some idea how epistemic norms may be empirically evaluated in the naturalist framework.[13]

Given the sketch of naturalist ontological arguments presented in my book and the above elaboration of the concept of explanatory necessity and empirical assessment of epistemic success, I think that both the premises and explanatory reasoning in the naturalist ontological arguments of this kind have

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[13] For further discussion of the naturalist account of epistemic normativity in science, see Kaidesoja 2013, 25-30.

to be grounded in empirical analysis of concrete instances of those practices. In other words, there are no a priori guarantees that our explanatory reasoning would produce true claims even if our initial descriptions of the scientific practice of interest were empirically well-grounded. This is not to deny that we may well be interested in general features shared by many experiments and that, therefore, we have to abstract from the idiosyncratic details of specific experiments that are irrelevant to our naturalist ontological inquiry. I also think that we should not require that reasoning in this type of arguments should proceed a priori even if we would accept that a priori reasoning can be understood in fallible terms (I will soon return to this point).

Though naturalist ontological explanations of this kind should be regarded as a posteriori arguments, there are two differences between them and the mechanism-based causal explanations developed in the empirical sciences. First, the aims of these two types of explanations are different: ontological explanations trace the ontological assumptions and presuppositions of epistemically successful scientific practices[14], while causal explanations typically aim to refer to one or more causal mechanism(s) that generated some phenomenon of interest as well as to specify the conditions in which those mechanisms typically operate (or generate their typical effects). Second, the level of conceptual abstraction in naturalist ontological explanations is typically higher than in scientific explanations. Despite these differences, I think that it would be misleading to call arguments of this kind as "naturalized transcendental arguments", because the term 'transcendental' is so tightly associated to Kant's transcendental philosophy whose purposes and assumptions are inconsistent with the naturalist philosophy of science (cf. McWherter 2015, 75-78).

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[14] Unlike what McWherter (2015, 76) suggests, this aim is not exactly the same as the aim of Bhaskar's transcendental arguments. It is not a defining feature of naturalist ontological arguments that they should concern the most general categorial structure of the world. In addition to the ontological conditions of scientific practices, naturalist philosophers may also study the ontological assumptions and presuppositions of specific scientific theories, models and explanations.

Moreover, in contrast to naturalist arguments of this kind, the concept of transcendental necessity (even when it is relativized to certain a posteriori premises) in Bhaskar's transcendental arguments still contains a priori elements that should be rejected by naturalists. As McWherter (2015, 76) insists, in transcendental arguments "the ampliative inferences, the mode of justification, and (consequently) the conclusions of transcendental analysis *must be* a priori relative to the analysis's explanandum, because a posteriori knowledge drawn from *particular instances* of the explanandum cannot significantly inform transcendental analysis." McWherter (2015, 76) exemplifies this point by referring to Bhaskar's transcendental arguments from experimentation in which "the conditions of possibility of experimentation *in general* cannot be internal to any particular experiment since they pertain to and are presupposed by all experiments." So the idea seems to be that the level of generality of Bhaskar's transcendental arguments necessitates that the transcendental analysis of the practice of experimentation should proceed a priori without recourse to empirical experiences (or evidence) about particular experiments. In this view, then, transcendental analysis begins with some a posteriori descriptions of the generic features of experimentation but then proceeds a priori by using the Kantian pure reason only. Unlike Kant, McWherter holds that this kind of a priori reasoning provides only fallible results that may be contested, even though it takes place without recourse to empirical evidence from specific instances of the explanandum at issue.

In my view, naturalists should reject these a priori elements of the allegedly naturalized transcendental arguments. One reason for this is that we have to assume that the (explanatorily necessary) ontological conditions (i.e. the ontological features or aspects of the world) that make certain types of experimentation epistemically successful should be in place in every instance of successful experiment of this type insofar as we think that our hypotheses about these conditions are relevant to the ontological explanations of the epistemic success of the practices of this kind. For this reason, it is perfectly

acceptable to proceed by: conducting empirical case studies on the instances of certain types of experiments; and then making cautious and fallible generalizations on the basis of these studies. Otherwise, there is a significant risk that we overgeneralize from some salient instances of experiments that may not turn out to be representative.

The same point can be made by utilizing the previous analogy between causal analysis in terms of necessary and sufficient conditions and naturalist ontological explanations. It is reasonable to expect that in every instance that a newspaper was formed in eighteenth and nineteenth century Finland, there were, at least, a printing press that was used in printing the newspaper, supply of paper that was suitable for printing, and enough literate people who were able to read the newspaper. In a similar manner, it can be argued that some instances of certain ontological categories (e.g. structured entities with causal powers) were there when a successful scientific experiment of certain kind was conducted. Therefore, I do not see how there can be ontological explanations of the epistemic success of a certain scientific practice that would hold only at the generic level but not in the particular instances of the practice.

For these reasons, I think that naturalists should argue for their fallible descriptions of those (explanatorily necessary) ontological conditions by means of empirically analysing particular instances of experimentation and fallibly generalizing from them. This would make ontological arguments not only more local but also more relevant to the actual scientific practices of interests when compared with Bhaskar's transcendental arguments that proceed from highly abstract and partially misleading descriptions of generic scientific practices.

Conclusion

I think that I have been able to answer the most important objections that McWherter (2015) raises against my interpretations of Bhaskar's transcendental arguments and my naturalist alternative to them. In

addition, I have questioned the coherence of McWherter's views on Bhaskar's transcendental arguments and argued that McWherter's allegedly naturalized transcendental arguments still contain problematic a priori elements that should be rejected by naturalists. I believe that the above arguments also indicate that McWherter has not shown how Bhaskar's transcendental arguments can be naturalized. In these respects, the above arguments support my earlier argument that critical realism should be naturalized.

Finally, I would like to note that McWherter's philosophical approach is rather restricted and backward looking in that it emphasizes exegesis of Bhaskar's and Kant's works while largely ignoring recent developments in philosophy of science and empirical sciences. By contrast, one of the aims of my book is to consider the relations between critical realism and more recent (scientifically realist and naturalist) philosophy of science as well as to indicate how critical realist philosophy of science can be further developed so that it will become more relevant to (social) scientists engaged in empirical research. This kind of bridging work is also important in order to more adequately integrate critical realism into the current philosophy of science that emphasizes the importance of empirical analysis of scientific practices of various kinds. For these reasons, I believe that—if we want critical realism to perform its role as underlaborer for the twenty-first century empirical sciences—my naturalist approach is more promising than that of McWherther's neo-Kantianism.

Despite our disagreements, I would like to thank Dustin McWherter for providing a thoughtful and detailed critique of my work. I look forward to reading his response to my arguments.[15]

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