

# Logic, Methodology and Philosophy of Science

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# 16 Philosophy of science in practice: A proposal for epistemological constructivism

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**Abstract.** *Philosophy of science in practice* (PoSiP), besides other things, aims at an epistemology of scientific practices that addresses questions such as: How is the construction of knowledge for epistemic uses possible? Epistemology that goes beyond *formal epistemology* has been banned from mainstream philosophy of science since the Wiener Kreis (Vienna Circle) in order to avoid drifting into the realm of metaphysics. However, formal epistemology cannot account for the possibility of *epistemic uses* of scientific knowledge (e.g. Cartwright, 1983). For, this account would involve as an epistemological presupposition that knowledge about new, previously unexamined systems can be derived from logical or mathematical structures that are ultimately grounded on experiential and experimental data. The plausibility of this presupposition draws on the belief that there *exist* such structures. Hence, metaphysics re-enters through the backdoor. When acknowledging that any claim to the *certainty* of knowledge involves metaphysics, the entire idea of *certain* knowledge may be abandoned (as in Van Fraassen's anti-realism). At this point, it becomes apparent that there is a need for an epistemology that suits scientific practices, especially those which aim at knowledge for practical uses. Indeed, a reconsideration of some of the presup-

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positions that vindicated formal epistemology seems in order, in particular: on the aim of science, on how to avoid metaphysics, and on the task of the philosophy of science. In this article Kant's epistemology is taken as a starting point for such reconsideration, since his work aimed to explain the *possibility* of knowledge. Based on contemporary interpretations of Kant's epistemology (e.g. Neiman, 1994), *epistemological constructivism* is proposed as a view in which the aim of science is to construct knowledge for epistemic uses. It involves the idea that scientific knowledge (patterns in data, and scientific laws, models and concepts) is constructed to enable and guide epistemic uses, which also entails that scientific practices develop *epistemic strategies* for the production of knowledge that meets this purpose. Accordingly, one of the tasks of PoSiP is to reconstruct, investigate, and evaluate epistemic strategies by means of which knowledge is constructed.

**Keywords:** philosophy of science in practice, Wiener Kreis, Vienna Circle, Kantian epistemology, synthetic a priori, constructive empiricism, representation, pragmatism, model construction, non-representationalism, epistemic tool, epistemic use, engineering sciences.

## 1 Philosophy of science in practice

The conference *Philosophical Perspectives on Scientific Understanding*, in August 2005 (De Regt, Leonelli, & Eigner, 2009), at the Free University in Amsterdam, was the prelude to a movement called *Philosophy of Science in Practice*.<sup>1</sup> In the invitation to the introductory meeting held at PSA 2006<sup>2</sup>, the founders (Boon, Chang, Ankeny, Boumans and De Regt) wrote: "We have begun organizing a network of scholars under the working name of "Society for Philosophy of Science in Practice" (SPSP). We aim to promote a philosophy of science that engages more closely with scientific practice, and with the practical uses of scientific knowledge." Different from other significant movements that originated from the philosophy of science, such as the sociology of scientific knowledge (SSK), and science, technology and society (STS), philosophy of science in practice (PoSiP) aims to maintain close ties to mainstream philosophy of science (also see Ankeny, Chang, Boumans, & Boon, 2011; Soler, Zwart, Lynch, & Israel-Jost, 2014).

One of the reasons why PoSiP crystalized at this conference on scientific understanding may have been that focus was turned to the scientists – the user and producer of knowledge – whereas traditional philosophy of science has frenetically aimed at an account of scientific knowledge in terms of a mere two-way relationship between world and knowledge only. Conversely, philosophers within this new movement aim at an understanding of science that avoids the belief that the objectivity of knowledge

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<sup>1</sup><http://philosophy-science-practice.org/en/mission-statement/>

<sup>2</sup><http://philosophy-science-practice.org/en/events/introductory-spsp-meeting-psa-2006/>

can be warranted by an account of knowledge-justification that eliminates the role of scientists, but that also avoids a mere psychological and sociological interpretation of scientists' subjectivity. Instead, the philosophy of science in practice aims to include the scientist by focusing on an *epistemology of scientific practices*.

The movement of philosophy of science in practice shows similarities to founding movements of traditional philosophy of science, such as the Wiener Kreis and in its wake Logical Empiricism. It is well known that those movements had pertinent societal concerns,<sup>3</sup> which motivated their members to aim at a philosophical account of knowledge that warrants objectivity (Hahn, Neurath, and Carnap, 1929/1973, and also see Uebel, 1996, 2008, 2016). Almost one century later, we live in a society that is thoroughly dependent on science in diagnosing problems, forecasting complex natural and societal processes, and in the development of (socio-)technology for welfare, well-being, and societal justice. The *philosophy of science in practice* is similar to its historical predecessors in the sense of being motivated by societal concerns. In this case, our concern is science's ability to play this role well. This implies a philosophy of science that, on the one hand, supports the production of knowledge that meets society's expectations of science, and on the other hand, explains the character of knowledge in such manner that possible misuses of the authority of science can at least be made transparent.

Concerning the societal role of science, our grandfathers in the philosophy of science wagered on the objectivity of science. In a rigorous, dedicated, and insistent manner, they strived for a philosophy of science that explains how the objectivity of science can be justified. Such objectivity essentially requires that the role of the scientist can be eliminated. However, critics within the analytical tradition of the philosophy of science have convincingly shown that objectivity cannot possibly be achieved by reducing knowledge to atomic sentences that are then used to describe observed (atomic) facts independent of whatever context, nor does reducing knowledge to formal logical structures void of meaning result in the desired objectivity (e.g. Quine, 1951; Feyerabend, 1962). A problem of warranting the objectivity of knowledge is that atomic facts cannot be generated by reading out (atomic) facts, implying that knowledge always involves one or another kind of *construction*, be it the ways in which facts are "pointed out" (i.e., discerned and given meaning), or the ways in which facts are connected to each other, and also, how they are related to theories. Moreover, the

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<sup>3</sup>In the 1930s, Hitler, in his speeches on his political ideology, explicitly expressed that, since science is not objective, it must be supervised by the state: "Der Gedanke einer freien voraussetzungslosen Wissenschaft konnte nur im Zeitalter des Liberalismus auftauchen. Er ist absurd. Die Wissenschaft ist ein Soziales Phänomen, und wie ein jedes solches begrenzt durch den Nutzen oder Schaden, den es für die Allgemeinheit stiftet. Mit dem Schlagwort von der objectiven Wissenschaft hat sich die Professorenschaft nur von der sehr nötigen Beaufsichtigung durch die staatlichen Macht befreien wollen" (quote from Klages, Mohr, and Sontheimer, 1970, and Feix, 1978). "The idea of a free unconditional science could emerge only in the age of liberalism. It is absurd. Science is a social phenomenon, and as such limited by the benefits or harm that it bestows upon the community. With the slogan of objective science the caste of professors just wanted to get rid of the very necessary supervision by the state power" (my translation).

objectivity of knowledge is no warrant for its *predictive* certainty – it requires skillful scientists to figure out how to apply scientific knowledge such as to produce reliable predictions about real-world problems (Cartwright, 1983; Boon, 2006). Therefore, in PoSiP we argue for the abandonment of the belief that societally desired qualities of science are warranted by objectivity of knowledge in terms of a two-way relationship between world and knowledge, and instead, adopt the idea that these qualities depend on a three-way relationship between world, knowledge, and scientist. Unavoidably, meeting societal goals through science must also be safe-guarded by moral and intellectual abilities of scientist, and these *abilities* are partially covered by notions such as objectivity and rationality. Philosophy of science in practice, therefore, cannot ignore these notions but rather than hunting down the objectivity and rationality of knowledge, it focuses on how scientific practices achieve these goals. Therefore, PoSiP aims at an *epistemology* of these practices that elucidates how knowledge is generated, which may also involve re-framing traditional notions such as truth, certainty, observation, explanation, experiment, and justification.

In this article, I take epistemological issues encountered in scientific research practices of the engineering sciences as paradigm example, and raise the question: How is construction of knowledge that allows for epistemic uses (in practical applications such as problem-solving) possible? Compared with traditional philosophy of science, focus is not on scientific knowledge as such (considering issues such as its truth and certainty), but on real-world problems in need of scientific knowledge that aids resolution. In addressing this question, my approach will be to investigate (in Section 2) assumptions and beliefs in mainstream philosophy of science that may hamper the development of an appropriate epistemology, including: the assumption that the construction of knowledge belongs to the *context of discovery* and therefore is a no-go-area; the idea that knowledge *represents* reality; and the belief that Kant's idea of *synthetic a priori judgements* has proven false. The connection between these assumptions is that they corroborate each other and entail an empiricism that is still close to Humean empiricism in the sense of assuming that objective knowledge consists of "basic empirical facts" (and plain data) that are to be acquired through *passive* observation (or measurement), and that any other knowledge should be built on (e.g., be reducible to, or verified by) these facts. In Section 3, I aim to present an outline of a revised interpretation of Kant's epistemology by contemporary Kant scholars (amongst others, Neiman, 1994, and Allison, 2004), which in my view is a fertile starting point for developing the desired epistemology of scientific practices. In Section 4, I will pick up on the claim that a representational view of knowledge is not well suited to an epistemology that accounts for the possibility of constructing knowledge for epistemic uses, and I will summarize why the alternative of *knowledge as epistemic tool* is a better fit for this purpose. Finally, in Section 5, I propose *epistemological constructivism* as a position that takes "the production of knowledge for epistemic uses" as the aim of science, and that adopts (the proposed version of) Kantian epistemology to explain how the construction of knowledge for epistemic uses is possible. Epistemological constructivism steers away from the realism/anti-realism debate – and from refined positions in this debate such as Van Fraassen's (1980) constructive empiricism and Giere's (1999) con-

structive realism – as this debate focuses on arguments for the justification or acceptance of scientific knowledge in accordance with one or another representational view of knowledge. Conversely, epistemological constructivism takes “the construction of knowledge for epistemic uses” as its guiding issue, including epistemic strategies for producing and using knowledge. One of the tasks of the philosophy of science is to investigate and evaluate these epistemic strategies.

## 2 How is construction of knowledge for epistemic uses possible?

What is scientific knowledge? And what purposes does it serve? In the philosophy of science it is commonly assumed that scientific knowledge, such as theories and scientific models, laws and concepts *represent* reality. The question that has led to the idea of scientific knowledge as *epistemic tool* as an alternative to this representational view (Boon & Knuuttila, 2009; Knuuttila & Boon, 2011; Boon, 2012), and to the idea of *epistemological constructivism* as a philosophical position that circumvents the realism/anti-realism debate (Boon, 2015), is: How is the construction of knowledge for epistemic uses possible? This philosophical question and proposed solutions emerged from epistemological and methodological problems experienced in concrete scientific research practices of the *engineering sciences*. Since most current scientific research is performed in one or another problem- or application context, I take it that this question and the proposed solutions are relevant to the philosophy of science in general.

Clearly, there is some friction between raising this question and several principles (norms, beliefs and presuppositions) in main-stream philosophy of science that have been accepted since, say, the establishment of the Wiener Kreis (Hahn et al., 1929/1973; Uebel, 2008, 2016). Most prominently, it is still assumed – as once clearly phrased by Carnap (1934) in the first issue of *Philosophy of Science* – that philosophy of science should only deal with formal aspects of science:

Philosophy deals with science only from the logical viewpoint. Philosophy is the logic of science, i.e., the logical analysis of the concepts, propositions, proofs, theories of science, as well as of those which we select in available science as common to the possible methods of constructing concepts, proofs, hypotheses, theories. [What one used to call epistemology or theory of knowledge is a mixture of applied logic and psychology (and at times even metaphysics); insofar as this theory is logic it is included in what we call logic of science; insofar, however, as it is psychology, it does not belong to philosophy, but to empirical science.] (Carnap, 1934, 6, his brackets.)

Hence, in traditional philosophy of science, epistemology as a theory of knowledge should focus on the logic of knowledge and avoid psychology, which underpins the

dogma that the *construction* of knowledge belongs to the *context of discovery* – a prohibited area for mainstream philosophy of science.<sup>4</sup> Furthermore, the philosophical question, summarized as “how . . . possible,” touches on basic assumptions in the philosophy of science, concerning what is: “science,” “scientific knowledge,” “the aim of science,” “the task and territory of the philosophy of science,” and also “the justification of *philosophical* claims.” In the cited quote, Carnap (1934) is very clear on what philosophy of science is. Therefore, we may first ask whether the very question of “how the construction of knowledge for epistemic uses is possible” *should* be addressed in the philosophy of science. If so, productively addressing it brings with it questions about assumptions of the philosophy of science itself.

An almost implicit assumption in the philosophy of science, but also in much science education, is that scientific knowledge can be applied in solving concrete problems by means of deductive reasoning upon laws and theories. This idea draws on the assumption that there is symmetry between prediction and explanation. Indeed, Hempel (1965) believed that laws discovered in the laboratory can be applied in practical applications:

The formulation of laws and theories that permit the prediction of future occurrences are among the proudest achievements of empirical science; and the extent to which they answer man’s quest for foresight and control is indicated by the vast scope of their practical applications. (Hempel, 1965, p. 333.)

Cartwright (1974, 1983) is one of the first authors criticizing this belief (see also Boon, 2006). Her fundamental critique is that scientific laws are only true – i.e., give true predictions – for circumstances close enough to the “nomological machine” that produced the regular behavior represented in the law:

[Usually] it takes what I call a *nomological machine* to get a law of nature. . . . A nomological machine is a fixed arrangement of components, or factors, with stable capacities that in the right sort of stable environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws. (Cartwright, 1999, pp. 49-50.)

The *semantic view* suggests a similar assumption about how scientific knowledge is applied for solving concrete problems. In this view, mathematical models are derived from abstract theories (called “models of the theory”)<sup>5</sup>, and thus involve the idea that

<sup>4</sup>The terms “context of discovery” and “context of justification” are often associated with Hans Reichenbach’s work, but see Schickore (2014).

<sup>5</sup>One of the philosophical problems inherited from logical empiricism and the so-called *syntactic view* that Suppes (1960a) and succeeding defenders of the *semantic view* of theories aimed to solve, is that the relationship between the theory and the object “referred to” by the theory cannot be understood in terms of a *correspondence* relationship. Therefore, Suppes (1960a) argued that the concept of model used by mathematical logicians should be the basis for a fundamental concept of model in any branch of, what he called the empirical sciences, which includes both the natural and the social sciences. Suppes adopted the

the application of theory is through “models of the theory” that are expected to correctly represent the real-world target system. However, this idea can be criticized for similar reasons as Cartwright (1983, 1999) has put forward concerning laws: usually these models are not correct about real-world systems – they only produce correct predictions for carefully designed experimental systems, and even then, before we can compare models derived from theory with experimentally derived data, the latter need to be corrected by means of “theories of data” and “theories of experiment” (Suppes, 1960a, 1960b)<sup>5</sup> A theorist who takes a semantic view may admit that the primary function of “models of the theory” is testing theories, but still assume symmetry between the role of models in testing theories and in predicting the behavior of real-world target-systems. Yet, this idea is criticized by Cartwright (1983) and also easily rebutted by concrete scientific practices. The outlined view does, however, reveal another assumption of the philosophy of science: that the aim of science is theories (not practical application), and that the task of philosophy is to account for the justification of theories (but not the justification of how knowledge is produced and epistemically used in solving practical problems).

In accordance with Cartwright’s critique, concrete scientific research practices show that philosophical assumptions on the applicability of scientific knowledge by mere deductive reasoning are inadequate, which is why the question “how ... possible” should be addressed in the philosophy of science. The need for an epistemology that addresses this question is therefore evident. But some of the assumptions and beliefs of mainstream philosophy of science seem to discourage these kinds of philosophical investigation. The examples discussed so far include the idea that the construction of knowledge, if it cannot be formalized, belongs to the *context of discovery*, which is beyond the domain of the philosophy of science; the idea of *symmetry* between explanation and prediction, which justifies the assumption that scientific knowledge

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concept of *satisfaction* to specify the semantic relationship between theories and models. Hence, *models* are the objects that *satisfy* the axioms of the theory (i.e., the model provides a realization in which the theory is satisfied). He calls these models “models of the theory.” Additionally, he introduced the notion “models of data,” which are the data-structures that, according to the theories of data and of the experiment, would be generated by the experiment but restricted to those aspects of the experiment which have a parametric analogy in the theory (Suppes, 1960b). Suppes’ formal approach in set-theoretical terms makes his approach to models somewhat tedious from the perspective of scientific practices. Van Fraassen (1980, p. 64) puts the relationship between models of the theory and experiments in more accessible terms: “To present a theory is to specify a family of structures, its models; and secondly, to specify certain parts of those models (the empirical substructures) as candidates for the direct representation of observable phenomena. The structures which can be described in experimental and measurements reports we can call appearances.” Van Fraassen uses the concept of *isomorphism* to specify the semantic relationship between “models of the theory” and what he calls “appearances.” It can then be specified how these “appearances” or “measurement reports” play a role in testing the theory: “The theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model.” In the language of a scientist, this means for instance that abstract theories are tested by comparison of models that are derived from the theory for relevant conditions of the experiment (i.e., mathematical structures), and data-models produced in experiments (i.e., “empirical substructures”) – where the theoretically generated structure must be isomorphic to the experimentally produced structure (also see Suppe, 1989). Still closer to the language of scientists is Giere’s (1988) account of the semantic view, which he summarizes in Giere (2010).



allows for correct predictions in new situations; the idea that knowledge such as models *represents* aspects of the real-world, and that its epistemic use is by virtue of this representational relationship; and, the idea that the *aim of science* is theories, while the *task of the philosophy of science* is their justification by formal methods.

Here, I will focus on the *representational* view of knowledge as one of the deeply rooted ideas in current philosophy of science that obscures an epistemology of “how the construction of knowledge for epistemic uses is possible.” More precisely, I aim to provide an alternative to the interconnected beliefs that scientific knowledge (theories, scientific models, laws and concepts) represents aspects of the world, and that its epistemic usefulness is due to this representational capacity. First, I will expand on earlier work, which proposes the idea of “knowledge as epistemic tool” as a viable alternative to “knowledge as representation.” Considering knowledge as epistemic tool stresses the fact that humans construct knowledge for epistemic uses of all kinds. Therefore, this notion is one element of the epistemology aimed at. The further development of this epistemology requires an account of how epistemic tools are constructed, justified, and actually used. Important in my argument is that the *construction* of knowledge should not be conflated with discovery, nor should the *application* of knowledge be conflated with representation or algorithmic reasoning. Rather, in scientific research practices, construction and use are less far apart than we tend to believe. *Constructing* an epistemic tool involves its justification, not only in view of established knowledge and data, but also in view of intended applications. *Using* it (in new, real-world situations) involves the assessment and justification of whether it suits this particular application, but also constructive activities by means of the epistemic tool (rather than merely using it as a basis for description or algorithmic reasoning). These two aspects of using knowledge require an understanding of its original construction, such as the idealizations and simplifications used in the construction of the epistemic tool.

Second, next to the claim that “knowledge as representation” hampers an epistemology of “how constructing knowledge for epistemic uses is possible,” I will argue that it is also hampered by the dogma that the construction of knowledge is beyond the scope of the philosophy of science. Although I do not aim to make a claim on the history of philosophy, I will start from the assumption that this dogma reflects the rejection of Kant’s notion of synthetic a priori judgments. This rejection happened already at the establishment of the philosophy of science by members of the Wiener Kreis, and was motivated by their urge to ensure that science did not become tainted by metaphysics and psychology. In the Manifesto they state:

In such a way logical analysis overcomes not only metaphysics in the proper, classical sense of the word . . . , but also the hidden metaphysics of Kantian and modern *apriorism*. The scientific world-conception knows no unconditionally valid knowledge derived from pure reason, no ‘synthetic judgments a priori’ of the kind that lie at the basis of Kantian epistemology and even more of all pre- and post-Kantian ontology and metaphysics. The judgments of arithmetic, geometry, and *certain fundamental principles of physics, that Kant took as examples of a priori*

*knowledge will be discussed later. It is precisely in the rejection of the possibility of synthetic knowledge a priori that the basic thesis of modern empiricism lies. The scientific world-conception knows only empirical statements about things of all kinds, and analytic statements of logic and mathematics.*" (Hahn et al., 1929/1973, 7, my emphasis.)

Epistemological analysis of the leading concepts, of natural science has freed them more and more from *metaphysical admixtures* which had clung to them from ancient time. In particular, Helmholtz, Mach, Einstein, and others have cleansed the concepts of *space, time, substance, causality, and probability*. The doctrines of absolute space and time have been overcome by the theory of relativity; space and time are no longer absolute containers but only ordering manifolds for elementary processes. Material substance has been dissolved by atomic theory and field theory. Causality was divested of the anthropomorphic character of 'influence' or 'necessary connection' and reduced to a relation among conditions, a functional coordination. (Hahn et al., 1929/1973, 10, their emphasis.)

However, current Kant scholars, such as Allison (2004) and Neiman (1994) suggest that the above reading of Kant is flawed.<sup>6</sup> Put very briefly, the *synthetic a priori* is not about fundamental principles of physics as suggested in these quotes – it is not about concepts in the sense of empirically revisable knowledge about reality. Instead, it concerns "the conditions for the possibility" of such knowledge, which Kant attributes to human cognition. Due to this flawed understanding of Kant's epistemology, philosophers of science were trapped in a position that is closer to Humean empiricism (although extended with modern logic). Instead of adopting Kant's basic idea that empirical knowledge of the world must already be understood as *actively constructed* by humans (which means accepting that knowledge-production results from a three-partite relationship between world, knowledge, and the human cognitive system), these philosophers retreated into the idea that empirical knowledge of the world *emerges passively* through observation (perception), and that (in order to warrant objectivity and avoid metaphysics) epistemology must ban the idea that some kind of synthetic a priori knowledge plays a role.<sup>7</sup>

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<sup>6</sup>Nevertheless, these scholars admit that Kant himself gave reasons for this flawed understanding, especially as a result of his *Metaphysische Anfangsgründe der Naturwissenschaft* (1786/2004) (Metaphysical Foundations of Natural Science) which he wrote as an application of his principles on human cognition in *Kritik der Reinen Vernunft* (1787/1998) (Critique of Pure Reason).

<sup>7</sup>Also see Massimi (2008, 2011), who argues that the empiricist idea that "literally true description of the way things are" is possible, is highly problematic even for the observable part of the world. She argues that a Kantian stance on (observable) phenomena studied in science can offer a genuinely new perspective on the issue of how we infer phenomena from data, by distancing both from a metaphysics of ready-made phenomena and from conventionalist ideas of how phenomena are established (e.g., as sometimes proposed in Logical Positivism and Logical Empiricism). Accordingly, Massimi (2011) aims at solving the controversy between Woodward's (1989, 1998) realist and McAllister's (1997) anti-realist account of how phenomena are inferred from data.

Although, mainstream philosophy of science has criticized, abandoned, and revised many of the initial ideas of members of the Wiener Kreis, their claim that Kant's notion of synthetic a priori judgments has proven false seems still widely accepted. As is probably already clear, I actually consider Kantian rather than Humean empiricism a better starting point for contemporary philosophy of science, and it is from this standpoint that I suggest the use of modern interpretations of Kantian epistemology in developing an epistemology that explains how the construction of knowledge for epistemic uses is possible.

### 3 Kantian epistemology<sup>8</sup>

In the last chapter of her monograph, which addresses "The task of philosophy," Neiman (1994) argues that Kant's work reflects a tension between two wholly diverse conceptions of philosophy and hence of his own procedure: a *regulative* conception and a *constitutive* conception. The latter "is reflected in Kant's determination to 'put metaphysics on the sure path of a science' and to complete a necessary edifice that will never need to be revised" (Neiman, 1994, p. 185), which agrees to how Kant is usually understood. In contrast, the regulative conception connects with an anthropological interpretation of the task of philosophy (see last part of note 8), which is the one I propose to endorse.

Kantian epistemology as it is proposed here, promotes the idea that scientific knowledge is not the product of algorithmic (deductive and inductive) reasoning upon experiences, but results from the capacity to demand explanations of experience, which in turn, "requires the capacity to go beyond experience, for we cannot investigate the given until we refuse to take it as given. To ask a question about some aspect of experience, we must be able to think the thought that it could have been otherwise. Without this thought, we cannot even formulate the vaguest why" (Neiman, 1994,

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<sup>8</sup>Although I do not consider myself a Kant scholar, I have been studying Kant's work repeatedly (both in German and in English translations) over the past 30 years. It provided me with clues for better understanding of scientific reasoning in research practices, which I did not find in mainstream philosophy of science. When still being a practicing scientist, in 1986, I read Putnam's (1981) *Reason, Truth and History*, which has significantly affected my appreciation of Kant's ideas, and my conviction that his ideas are crucial to an understanding of how we produce knowledge about the world (also see note 12). Even more so, Kant and Putnam's work made me understand what constitutes a Copernican revolution in our own naïve presuppositions: it cured me of basic intuitions (scientism) that in many respects were close to ideas that motivated members of the Wiener Kreis. Allison's (2004) detailed study of the *Critique of Pure Reason* was a feast of recognition, as, except for Putnam, I had not read anything so near to my own interpretation of Kant's work. But most of all was I touched by Neiman's (1994) work on Kant. She shows that instead of the common caricature of Kant as a shriveled scholar who is suffering of systematism, his work circles around the question of what it means to be human – i.e., Kant's fourth question: "What is the human being?" on which Kant concludes provocatively, that the whole field of philosophy could be reckoned to anthropology, whose business it is to tell us what is human (CPR IX, 25; Neiman, 1994, p. 185). Kant offers an extremely rich, deep and quite coherent philosophy on that very issue.

p. 59). Hence, the demand for explanation of experience is crucial to the *construction* of scientific knowledge. Importantly, in a Kantian epistemology an explanation is not regarded as a *representation* of the mind-independent world behind the phenomena (i.e., the picture or animation as seen by a God's eye; also see Putnam, 1981). Instead, an explanation of experiences is constructed, enabled, and guided by our ways of structuring and conceptualizing even at the level of "observable" phenomena (also see Massimi, 2008, and Boon, 2012).

According to Kant, in seeking the explanation of experience, reason seeks its systematization, which involves the employment of *regulative principles* of reason (such as, the principle of induction; the principle that every event has a cause; the principle that nature forms a system according to laws; and the principle of the unity of nature). Importantly, regulative principles are crucial to the possibility of science – they are not, as in a constitutive conception of Kant's epistemology metaphysical (or even scientific) claims about the world.<sup>9</sup> Explanation also involves surpassing the confines of experience, which allows theory to be extended to the realm of the unobservable. In this way, reason both constructs and anticipates entities that could not otherwise be incorporated into science (Neiman, 1994, p. 71). "[Reason] seeks for the unity of this knowledge in accordance with ideas which go far beyond all possible experience (CPR, A662/B690)."<sup>10</sup>

The following quote of Kant may illustrate the difference of view and appreciation of science in Kantian and Humean epistemology:

When Galileo rolled balls of a weight chosen by himself down an inclined plane, or when Torricelli made the air bear a weight that he had previously thought to be equal to that of a known column of water, or, when in more recent times Stahl changed metals into calx and then changed the latter back into metals by first removing something and then putting it back

<sup>9</sup>Very similar to Kant's notion of regulative principles as principles that make science possible, Hasok Chang (2009) introduces the notion of *ontological principles*, which, similar to Kant, is not knowledge empirically derived from the world, but concerns principles that allow for the intelligibility of epistemic activities – for example, the epistemic activity of *counting* involves the ontological principle of *discreteness*. Similarly, but closer to Kant's vocabulary, I have argued that the notion of "same conditions, same effects" is a regulative principle that cannot be proven or disproven, but that we must adopt, as without this principle, scientific experimentation for testing theories would not be possible (Boon, 2015).

<sup>10</sup>Kant's idea that "[Reason] seeks for the unity of this knowledge in accordance with ideas which go far beyond all possible experience" also concerns the formation of scientific concepts. On this aspect of the formation of scientific concepts Rouse (2011) has challenged the assumption of the Vienna Circle and Logical Empiricism that the aim of science is true (or empirically adequate) theories. He argues in favor of an image of science that would place *conceptual articulation* at the heart of the scientific enterprise: "Conceptual articulation enables us to entertain and express previously unthinkable thoughts, and to understand and talk about previously unarticulated aspects of the world" (quote in Rouse's paper at the San Francisco State Workshop, March 2009, on *The Role of Experiment in Modeling*, but not in Rouse, 2011). To this idea, I add that aiming at strict certainty – that is, avoiding any content that goes beyond what is empirically given – would drastically reduce our ability to develop concepts that enable us to think about interventions with the world; interventions that are 'unthinkable' without the added content of those concepts (Boon, 2012).

again, a light dawned on all those who study nature. *They comprehended that reason has insight only into what it itself produces according to its own design*; that it must take the lead with principles for its judgments according to constant laws and compel nature to answer its questions, rather than letting nature guide its movements by keeping reason, as it were, in leading-strings; for otherwise accidental observations, made according to no previously designed plan, can never connect up into necessary law, which is yet what reason seeks and requires. Reason, in order to be taught by nature, must approach nature with its principles in one hand, according to which alone the agreement among appearances can count as laws, and, in the other hand, the experiments thought out in accordance with these principles – yet in order to be instructed by nature not like a pupil, who has recited to him whatever the teacher wants to say, but like an appointed judge who compels witnesses to answer the questions he puts to them. Thus even physics owes the advantageous revolution in its way of thinking to the inspiration that what reason would not be able to know of itself and has to learn from nature, it has to seek in the latter (though not merely ascribe to it) in accordance with what reason itself puts into nature. This is how natural science was first brought to the secure course of a science after groping about for so many centuries. (CPR, Bxii-xiv.)

But if the business of science is that of constructing our own questions and answers, is there still a point in scientific knowledge being true or false? In Kantian epistemology, empirical truth can no longer be grounded in alleged insight into the nature of things-in-themselves. Kant proposes, instead, a version of what is known today as a *coherence theory of truth*. Reason's search for systematic unity is the search for this coherence. While isolated observation-sentences may be confirmed by sense experience, any statement of empirical law can only be regarded as such by virtue of its incorporation in a system of beliefs as a whole (Neiman, 1994, p. 75; and note that Quine, 1951 is close to Kant's view).

Kantian epistemology does not entail a *representational* image of scientific knowledge, that is, the idea that scientific knowledge represents the independently existing order, or structure, or "furniture" of the real world. Moreover, it aims to avoid the idea that pre-given objects or structures "out there" are somehow presented in our observations (or, in the data taken from measurements) and form the solid ground of scientific knowledge (as in Humean empiricism). Instead, in Kantian epistemology, synthetic a priori categories or concepts are crucial to the possibility of knowledge, and are therefore called *regulative principles*.

## 4 A non-representational view of knowledge

For understanding how "the construction of knowledge for epistemic uses is possible," I propose to adopt the presented interpretation of Kant's epistemology. Crucially,

Kant changed the problem of knowledge. He abandoned some of the ideals and assumptions maintained before and after his days: the ideal of truth as correspondence between world and knowledge; the belief that *certain* knowledge is possible; the basic idea of Humean empiricism that knowledge of basic facts can and should come about by *passive* observation; and, the philosophical question itself, of how truth is justified.<sup>11</sup> Central to Kantian epistemology is that the human mind plays an *active* role at different levels of structuring and conceptualizing the “sensible manifold.” Nature imposes the limits, but the mind produces conscious perceptions and judgments, and adds (morphological, logical, mathematical, causal, mechanistic, functional, etc.) structure by the way it asks questions and constructs answers. Without these functions of the mind there would be no conscious perception, nor empirical or scientific knowledge of any physical phenomenon. Furthermore, by these activities of the mind, empirical and scientific knowledge is constructed that goes far beyond experience. As a consequence, as Kant admits, knowledge about nature is never certain – it may prove incorrect when confronted with new experiences.

To the questions raised in Section 2 – What is scientific knowledge? And what purposes does it serve? – another one can be added: If knowledge is “only” a representation of (aspects of) the world, what would it be good for? It is commonly assumed that knowledge can be *used* (for instance in explanation and prediction) by virtue of this representational relationship. However, the philosophy of science has been unsuccessful in answering the question how scientific knowledge (e.g. scientific models) allows for epistemic uses. Knuuttila and Boon (2011) deny that models give us knowledge (i.e., allow for epistemic uses) *because* (i.e., *by virtue*) of this representational relationship, but instead, because they are representations in the sense of being conceptually meaningful constructs that allow for epistemic uses by the model-user.<sup>12</sup> We call these constructs *epistemic tools*, that is, conceptually meaningful tools that guide and

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<sup>11</sup>Putnam (1981) – who was a former student of Reichenbach – came to reject the anti-metaphysical project of the Wiener Kreis. Affected by Kant’s epistemology, he concluded that Logical Positivism & Empiricism is mistaken in holding on to the distinction between primary and secondary properties, which is fundamental to the idea that objective observational sentences are possible and that knowledge can be reconstructed in terms of elementary statements and logical or mathematical structures (also see Suppe’s 1974/1979 outline of the so-called Received View). According to Putnam (1981, pp. 60-61): “What Kant did say has precisely the effect of giving up the similitude theory of reference. ... I suggest that (as a first approximation) the way to read Kant is as saying that what Locke said about secondary qualities is true of all qualities ... *If all properties are secondary*, what follows? It follows that *everything* we say about an object is of the form: it is such as to affect us in such-and-such a way. *Nothing at all* we say about any object describes the object as it is ‘in itself’, independently of its effect on us, on beings with our rational natures and our biological constitutions. It also follows that we cannot assume any similarity (‘similitude’, in Locke’s English) between our idea of an object and whatever mind-independent reality may be ultimately responsible for our experience of that object. Our ideas of objects are not copies of mind-independent things.”

<sup>12</sup>Both Giere (2010) and Suárez (2003, 2004) adopt an account of the representational relationship between model and world in which the model-user plays a key role. However, they intentionally developed a *deflationary* notion of representation that only minimally characterizes it. As a consequence, their accounts are not very informative as to the epistemic functioning of models. They shift the problem of how it is possible that models are used to the competent and informed agent.

enable epistemic uses.<sup>13</sup> By epistemic uses we mean scientific reasoning in the broad sense, which not only involves explanation and prediction, but also, asking new questions, crafting new hypotheses, generating ideas on possible interventions with the target system, and even, reasoning towards models of non-existing physical phenomena. Furthermore, we argue that the construction of models involves prudently putting together *heterogeneous* aspects relevant to the problem at hand, in order to form a coherent whole that allows for specific kinds of reasoning. We show, for instance that the model of the *ideal heat-engine* (which is the precursor of thermodynamics) was constructed by Sadi Carnot, not to describe or explain an existing phenomenon, but to represent and investigate an imagined phenomenon (the cycle of conversion of heat into motive power and reverse). Carnot's modelling is a clear example of how the construction of a model involves putting together *heterogeneous* aspects, including empirical knowledge of his days on the behavior of gasses and steam-engines, experiential principles such as "the restoration of equilibrium at difference in temperature," theoretical principles, such as "heat (caloric) will always flow from a hot body to a cold body until the two bodies have the same temperature, by which equilibrium is restored," and the introduction of new theoretical concepts such as "reversibility of the process." This model allowed Carnot to theoretically predict the maximum efficiency ("as set by nature") of real heat-engines. Significantly, Carnot did not test the model by means of experiments. Knuuttila and Boon (2011) follow Boumans (1999) by showing that in scientific practices much of the *justification* of a model is in how it is constructed instead of how it is derived from theory (also see Hacking, 1992). This reconstruction of Carnot's modelling shows several things: firstly, that models are not always derived from theories,<sup>14</sup> but instead, constructed so as to meet *epistemic* criteria (e.g., empirical adequacy in view of accepted empirical knowledge, relevancy, and reliability of empirical and theoretical content, epistemic acceptability of simplifications, and coherency between all elements of the model) as well as *pragmatic* criteria (e.g., the intended epistemic uses, and simplicity and intelligibility so as to allow for specific types of scientific reasoning upon the model); secondly, that a sharp distinction between discovery and justification of scientific knowledge does not smoothly apply to knowledge constructed for epistemic uses; and thirdly, that the model "gives knowledge" (i.e., is epistemically useful), but *not* by virtue of a two-way representational relationship between the model and its real-world target.

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<sup>13</sup>The notion *epistemic tool* is different from Rheinberger's (1997) notion of *epistemic things*. An epistemic tool entails a *conception* of the target system (represented by means of representational means such as text, analogies, pictures, graphs, diagrams, mathematical formula, but also 3D material entities), and the epistemic tool is constructed such that we can use it in performing epistemic tasks (e.g., reasoning about possible interventions with the target system). Differently, an epistemic thing in Rheinberger's writing is the research object or the scientific object. Epistemic things are material entities or processes – physical structures, chemical reactions, biological functions – that constitute the objects of inquiry.

<sup>14</sup>Also see Morrison and Morgan (1999), who have criticized the semantic view, which assumes that models are derived from theories. They argue that, if attention is paid to the actual construction of models and their uses in science, it becomes evident that models are partially autonomous both of theory and of reality, playing different kinds of epistemic roles.

In a similar fashion, I propose that *scientific concepts* should be considered as *epistemic tools* (concepts may function as definitions, but this is just one way in which they function as epistemic tools; see Boon, 2012, also see Nersessian, 2009, and Andersen, 2012). My argument expands on Feest (2008, 2010), who harks back to Logical Empiricism by explaining scientific concepts in terms of operational definitions that are cast in terms of a paradigmatic experiment that generates the (purported) phenomenon indicated by the concept.<sup>15</sup> I agree with Feest that scientific concepts are tools for investigating the purported phenomenon. Yet, in accordance with Kantian epistemology, I propose that the formation of the scientific concept involves the scientists subsuming the initial operational definition under more abstract concepts (such as “object,” “property,” “process”) or theoretical concepts (e.g., force, field, etc.) or even analogies (also see Hesse, 1966; Hanson, 1969). In a Kantian epistemology, structuring and interpreting experimental data by means of concepts is not fundamentally different from bringing structure to them by means of using logic and mathematics. In other words, mathematical structuring (which involves employing “mathematical templates,” e.g., Humphreys, 2004) is similar to the use of “more abstract and theoretical concepts” in structuring and interpreting experimental data. Although formal epistemology only accepts the use of logic and mathematics in structuring data, there is no good reason why this should be preferred to the use of abstract and theoretical concepts. Again similar to structuring by means of logic and mathematics, the crux of subsuming experimental data under concepts is that abstract and theoretical concepts add hypothetical content to the preliminary concept (e.g., the operational definition). This additional content goes beyond the empirically given, and allows scientists to reason upon those concepts, and so derive new predictions and ask new (testable) questions about the purported object or property indicated by the concept.

In short, understanding the construction of scientific models and the formation of scientific concepts in terms of a Kantian epistemology allows for a non-representational understanding of scientific knowledge (knowledge as “epistemic tool” rather than as “representation” in the case of models and “definition” in the case of scientific concepts). In this manner, Kantian epistemology explains how the construction of knowledge for epistemic uses is possible.<sup>16</sup>

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<sup>15</sup>Important to the question “how the construction of knowledge for use is possible,” is: how we bring an observation under existing concepts, and how we generate new concepts for describing “apparently” similar phenomena. Kant uses the notions *determinative* judgment and *reflective* judgement as powers of the mind involved in these activities of applying and constructing knowledge. See Procee (2006) for a clarifying explanation of the two notions.

<sup>16</sup>Peirce’s pragmatism is deeply indebted to Kant’s epistemology. Peirce defended an account of scientific reasoning in terms of inductive, deductive, and retroductive reasoning (which are defined more broadly than their narrow logical meanings) that includes how knowledge is constructed. A concise account of these ideas can be found in Chapter III of *A neglected argument for the reality of God* (Peirce, 1908).



## 5 Conclusions – Epistemological constructivism

Since its foundation by movements such as the Wiener Kreis (Hahn et al., 1929/1973), mainstream philosophy of science has banned metaphysics, and therefore, rejects epistemology that entails metaphysical notions such as Kant's synthetic a priori concepts. With the exception of a few outspoken dissidents (e.g. Hesse, 1966; Hanson, 1969) most of philosophy of science has accepted that the construction of scientific knowledge belongs to the context of discovery, which is beyond the liable domain of the philosophy of science. This belief has had at least two societal disadvantages. Firstly, after Kuhn (1970) for example, this domain has been hijacked by other movements such as the sociology of scientific knowledge (SSK), which have emphasized the subjectivity and irrationality of science to the point of skepticism, leading to a loss of societal trust in science. Secondly, this narrow understanding of its own tasks has resulted in a philosophy of science that is of limited value to scientific practices.

Philosophy of science in practice, however, is a movement that encompasses epistemological issues of scientific practices in its remit, one example being an epistemology for the appropriate construction of knowledge. A focal point of this article, for instance, is an epistemology that facilitates an explanation of how the construction of knowledge for practical uses is possible. It has been argued that a revised, contemporary understanding of Kantian epistemology allows for avoiding well-known problems of (modern versions of) Humean empiricism. The core of the argument in favor of Kantian epistemology is, firstly, that the distinction between primary and secondary properties, needed to justify formal approaches in both the Received View and the Semantic View, cannot be maintained; and secondly, that according to new interpretations Kant's epistemology must be understood as *regulative*, rather than *constitutive*: regulative principles such as synthetic a priori concepts must be pre-supposed as "conditions for the possibility" of generating knowledge. Synthetic a priori in the sense of regulative principles, therefore, does not mean that these principles are *a priori* truths about how the world really is – but instead, that humans, in order to generate knowledge about the world, need to adopt these kinds of principles while knowing that proving them is not possible for fundamental reasons. Therefore, Kantian contrary to Humean empiricism, entails that even the most elementary bits of knowledge result from activities of the mind, not from passive observation.

Next, it has been argued that a Kantian epistemology means that it is not necessary to adhere to a representational view of scientific knowledge according to which knowledge "gives" us knowledge by virtue of the (two-way) representational relationship between knowledge and world.<sup>17</sup> Instead, it is proposed to view knowledge as epistemic tools, which are epistemic entities – such as descriptions of physical phenomena,

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<sup>17</sup>In several respects Giere's (2006) and Van Fraassen's (2008) notions of representation agree with the 'non-representational' view I aim to pursue, but there are also significant differences (see Boon, 2012, 2015).

and scientific laws, concepts, and models – that must be considered as representations of our conceptions, rather than representations in the sense of a two-way relationship. Indeed, in this manner, the strict separation between primary and secondary properties, that is, between knowledge in terms of formal (logical and mathematical) structures, and knowledge in terms of conceptual content that goes beyond direct experience, disappears.

All this results in a position that entails a view on the character of scientific knowledge and the aim of science, as well as on the tasks of the philosophy of science, which I propose to call *epistemological constructivism*. In this view, the aim of science is to construct knowledge for epistemic uses, and this can range from the highly abstract to the very concrete, as long as this knowledge can be comprehended by human cognition and fits with our experiences of the world. Epistemological constructivism adopts and stresses the idea that science cannot give certainty, as certainty would involve a metaphysical belief in the existence of a structure to the world that can be discovered by science, and from which relevant and true knowledge about concrete target systems can be deduced. Nevertheless, science (should) aim to meet relevant epistemic and pragmatic criteria. Although, epistemological constructivism takes Kantian epistemology as its starting point, it expands on it by assuming that in the history of science, humans have *constructed* more general scientific concepts (such as force, energy, field, evolution, cyclic, symmetry, reversible, operator, amplifier) and epistemic strategies (such as generalization, abstraction, mathematization, reduction, idealization, categorization, explanation, conceptualization, drawing analogies, and crafting graphs, diagrams and models) that are re-used and applied in the construction of knowledge in distinct scientific disciplines (also see Humphreys, 2004). In accordance with Kantian epistemology, these concepts and strategies suit human cognition and human experience of the world, rather than being determined by “how the world really is,” independent of humans. This is why their application and re-use in the construction of knowledge is possible. Epistemological constructivism, therefore, does not only put emphasis on the idea that knowledge is constructed, but also entails that scientific practices invent concepts and epistemic strategies for the construction of knowledge.<sup>18</sup> In conclusion, epistemological constructivism provides a preliminary answer to the main question of this paper: how the construction of knowledge for epistemic uses is possible, and also illuminates one of the aims of the philosophy of science in practice, which is to investigate and evaluate general epistemic strategies in science that enable the construction of knowledge for use.

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<sup>18</sup>Several authors have suggested that Kuhn defends a historicized (Neo-)Kantian position. Friedman (2008), for instance cites Kuhn, who in an Afterword (first published in 1984) appended to his book on Planck and black-body radiation, states: “The concept of historical reconstruction that underlies [this book] has from the start been fundamental to both my historical and my philosophical work. It is by no means original: I owe it primarily to Alexandre Koyré; its ultimate sources lie in Neo-Kantian philosophy.” Thus, according to Friedman, Kuhn, not only characterized his philosophical conception as a dynamical and historicized version of Kantianism, but also explicitly acknowledged the background to his own historiography in Neo-Kantian philosophy.

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