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The Philosophy of the Future of Science

Abstract

In this paper, I argue that the philosophy of science has not paid enough attention to the future of science. Even though the philosophy of science has deepened our understanding of science, explicit conceptual tools to understand the estimating of possible futures of science are missing from its repertoire. I argue that the philosophy of science can achieve two main objectives of the futures research: enhancing understanding and challenging conventional thinking. While there are legitimate concerns about the epistemic and ethical impossibility of predicting scientific innovations and discoveries, it is nevertheless possible to investigate a wide range of questions concerning the future of science. I sketch structural taxonomies as a tool for the estimating of possible futures of science. A structural taxonomy is a map of scenarios that are possible according to some philosophical theory of science. I show how the merits of such taxonomies can be assessed and how the assessment sheds new light on the existing philosophy of science. I conclude by noting that future-oriented thinking is highly valuable for our current understanding of science.

Future of Science; Philosophy of Science; Futures Research; Development of Science

1. Introduction

Science has changed considerably during its history. Not only have the contents, methods, goals, and assumptions changed but so have its technological, social, and cultural settings. Moreover, many, if not most, aspects of science are dependent on these settings. The technological, social and cultural settings are in constant flux and it seems reasonable to conjecture that the rate of technological, social, and cultural change will increase in the 21st century. The conclusion, that (at least some aspects of) science will therefore also change in the future, follows immediately.

The philosophy of science is currently working in close connection with the sciences. In addition, the philosophy of science has drawn on resources from the research on the history of science. Despite many complications in such collaboration, it seems that we understand more and more about science and its development. Surprisingly, very little has been said about the estimating of possible futures of science (or sciences, to be exact) on the basis of our improved understanding of science and its development. Only fragmented lines of thoughts concerning the estimating of the future of science[1] are present in the literature.[2] While there are many reports (e.g. EU; NATO) that summarize possible future topics and methods in science, there has been very little reflection on how the future of science can be estimated. As the philosophy of science has shown, science is opaque and difficult to understand. Given the opaqueness, the reports concerning the future of science appear hopelessly simplistic without reflection on the conceptions of science that they embrace. The problem is that the philosophy of science has not been much of a help here. Even though history and the philosophy of science have deepened our understanding of science, explicit conceptual tools to understand the estimating of possible futures of science are missing from its repertoire.

In this paper, I analyze how the philosophy of science can improve our ability to estimate the future of science. I proceed as follows. In §2, I motivate the estimating of the future of science by characterizing the basic tenets and goals of futures research and by arguing that the philosophy of

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[1] When we estimate the future of science, we are always estimating possible futures of science (see sections 2 and 3). I often use "the future" for convenience.

[2] e.g. IFTF. (2006); Rescher (1999); Martin (1995); McIntyre (2007); (Popper 1957); Small et al. (2014); Tromp (2018).

science has good prospects to be coordinated with the tenets and goals. I argue that prediction is not the only goal of future-oriented thinking and therefore the impossibility of predicting the future of science does not cut off the relevance of the philosophy of science in our future-oriented thinking. In §3, I focus on how the theories developed in the philosophy of science can provide theoretical insights on possible futures of science. I show that we can formulate taxonomies of possible futures of science on the basis of philosophical theories. In §4, I analyze how the merits of such taxonomies can be assessed. I argue that the assessment must be based on historical, normative, and practical considerations. I also argue that the assessment of the taxonomies enables us to shed new light on the philosophical theories that are the bases of the taxonomies. By unpacking the possible future commitments of the theories, we gain deeper knowledge concerning their nature. In §5, I conclude the discussion by noting how the disagreements and deep theoretical battles in the philosophy of science can be turned into a resource when we attempt to estimate the future of science.

2. The Prospects: Enhancing Understanding and Challenging Conventional Thinking

To motivate the philosophical study of the future of science, it is useful to begin from a tension that concerns the future of science. On the one hand, it is often taken for granted that the developments in science (or, as in pessimistic visions, the degeneration of science) will be a central driving force of technological, economic, social, and cultural phenomena. The future of science is too important a topic to be left without attention. Not only funding decisions and science policy depend on some estimates of how science can develop but also – and more importantly – our ability to understand the future of society in general. There are countless ways in which the future of science and the future of conceptions of science affect society: What technologies we have (e.g. EU 2019), who we consider as epistemic authorities (e.g. Mede & Schäfer 2020), how we perceive the human-nature relations (e.g. Allen 2018), and so on. Our ability to anticipate and prepare for changes in such areas depends on our ability to estimate the future of science.

On the other hand, science is often considered to be essentially unpredictable. In order to tell what the future of science looks like we should know what theories are accepted in the future. How could we, even in principle, know the future theories? If anything, we know that many past theories have been rejected. Even when science has appeared to be complete, completely new horizons have arisen (Rescher 1999, 23-26). Probably the same fate awaits our theories, the argument continues. Moreover, the development of science should not be planned according to some presumed future, as there are some seriously bad experiences about cases, such as Lysenkoism, where science failed because its presumed future was foretold. Polanyi argued that “Any attempt at guiding scientific research towards a purpose other than its own is an attempt to deflect it from the advancement of science” (1962, 62) and Merton famously alarmed us about planning the future of science: “Science must not suffer itself to become the handmaiden of theology or economy or state. The function of this sentiment is likewise to preserve the autonomy of science. [--] In other words, as the pure science sentiment is eliminated, science becomes subject to the direct control of other institutional agencies and its place in society becomes increasingly uncertain.”. (Merton 1968, 597).

Sir Karl Popper has provided an elegant expression of this enormous tension between the centrality of science in society and the difficulties and dangers in the estimating of its future:

“The course of human history is strongly influenced by the growth of human knowledge. [However, we] cannot predict, by rational or scientific methods, the future growth of our scientific knowledge. We cannot, therefore, predict the future course of human history.” (1957, ix-x.)

It is important to understand the context of Popper’s statement. He is arguing against “the belief in historical destiny” (ibid, vii) and, more specifically, against “the possibility of a *theoretical history*;

that is to say, of a historical social science that would correspond to *theoretical physics*. There can be no scientific theory of historical development serving as a basis for historical prediction" (ibid. x). The historical context is revealed by Popper's dedication of the book to "victims to the fascist and communist belief in Inexorable Laws of Historical Destiny".

Given the Popperian tension and given the importance of understanding the future of science, I suggest that we abandon the all-or-nothing approach of Popper and study *to what extent* and *with what reservations* the future of science can be estimated even if we cannot predict it. It is a bit ironic that, given the basic tenets of futures research, the ethical dangers and theoretical problems associated with the estimating of the future of science should not be seen as an external barrier for the study of the future of science. On the contrary, they are exactly the issues that the study of the future of science should focus on. Given the current understanding of the basic tenets and goals of futures research, Popper was, in fact, an early advocate of the study of the future of science.

To see this, we can note that the two main objectives of future estimation are the following: (Wright et al. 2013, 631):

A) "enhancing understanding: of the causal processes, connections and logical sequences underlying events — thus uncovering how a future state of the world may unfold".

B) "challenging conventional thinking in order to reframe perceptions and change the mindsets of those within organizations".

Given A and B, we "can provide information, ideas and stimuli to support a third objective; better decision making and strategic planning" (ibid.).

Moreover, we should also note that the futures research includes an ethical component: Among the possible future states of the world, we should identify and steer towards those that are desirable (or "preferable") (Bell 1997; Marien 2002). This topic cannot be discussed in detail in the limits of this paper. However, I will briefly mention some of its implications in §4.

Given these basic tenets of the futures research, we can understand Popper and the estimating of the future of science in a new light. First, we should study the causal connections and processes that affect science and its relation to society, culture, and technology. More generally, we can study patterns of development of science. Popper (1957) can be understood as providing one such study of the patterns of development of science, as he argues that the patterns of development of science and society cannot be extrapolated from history and abandons the idea of straightforward lines of development that are grounded on nomological necessities.

It is important to note that the futures research is not committed to studying patterns that are independent of human decisions. On the contrary, it is hardly possible to understand the patterns if one does not study the causal role of human decisions within the possible patterns of development. Moreover, ethical decision-making and work towards desirable futures are possible only if we understand the causal role of decisions within the patterns of development. The study of patterns of development of the future of science does not preclude decision making but makes it possible. Only if we understand the causal network where decisions are made, we can understand the consequences of those decisions and whether they enable us to achieve desirable goal. Finally, understanding decision-making against the background of patterns of development makes it possible to avoid wishful thinking and hubris. For example, sometimes we "try to 'force' nature into 'boxes;' but nature resists" (Godfrey-Smith 2003, 177) and the consequences of the resistance can be harmful when the decisions do not receive an adequate reality check, like in the case of Lysenkoism. It is, of course, a major question how the resistance and other related issues in the development of science should be understood and conceptualized, as we will discuss in §3. Nevertheless, it is obvious that there are limitations to our ability to simply decide the future of science.

Secondly, we should challenge conventional thinking and reframe perceptions. Popper can be understood as challenging what he considered as a dangerous convention among certain thinkers, that of attempting to predict the future of society in simplistic terms, including visions of historical destiny. Moreover, he reframed the possibility of predicting the future by using current knowledge by arguing that it is impossible to predict the future *because* human knowledge is growing (1957, x). In general, philosophers of science have questioned conventional ways of thinking about science or critically engaged with the basic assumption in the conventional thinking. Whether or not particular philosophical analyses have been correct, they have posed important questions concerning the workings of science. For example, it has become obvious that there are theory changes that cannot possibly be understood in terms of accumulation of breakthroughs and unproblematic continuity of knowledge. Even the most optimistic philosophers, scientific realists, accept this, and the question has become – for example, in the debates concerning the so-called *divide et impera* strategy – what kind of continuity, if any, there exists through theory change and what that continuity, or the lack of continuity thereof, tells us about the epistemic merits of scientific theories (see Psillos 1999, 103-109).

These two aspects, the study of patterns and the challenging of conventional understanding, are often connected in the historical philosophy of science. In these studies, a philosophical theory of science is assessed against historical evidence. A canonical example of such a study is Laudan's pessimistic metainduction which argues that there have been successful theories in the past that were false, and therefore we cannot infer the truth of a theory from its success. According to Laudan's study, there has been a pattern of development that challenges a way of thinking: Successful theories have turned out to be false and we are not allowed to infer "that science works because it has got a grip on how things really are" (1981, 48). The historical philosophy of science is relevant to the estimating of futures of science since it allows us to compare, contrast and debate possible future changes against the causal framework of the past (Bradfield et al. 2016, 61) and to "to tease out conflicting viewpoints, misunderstandings and biases" (ibid. 64). even though we cannot predict the future of science unambiguously (see §4 below). This is exactly what Laudan does: "Nothing I have said here refutes the possibility in principle of a realistic epistemology of science. To conclude as much would be to fall prey to the same inferential prematurity with which many realists have rejected in principle the possibility of explaining science in a non-realist way. [--] Given the present state of the art, it can only be wish fulfilment that gives rise to the claim that realism, and realism alone, explains why science works." (1981, 48). Laudan does not claim that he has cut off the link between success and truth once and for all. Rather, he argues that, given what has happened in the past, we should be rather careful when understanding the development of successful science in terms of its convergence towards truth.

In §4 we will discuss the role of historical considerations in the estimating of the future of science in more detail. Here we can notice that the achievement of the main objectives of the estimating of the future of science (i.e., enhancing understanding and challenging conventional thinking) is not confined to the descriptive historical philosophy of science. There are at least two approaches that are relevant to the objectives.

The first one provides rational reconstructions of episodes in history of science. The question is not what in fact happened but what would have happened, had the development of science followed a particular philosophical framework. We do not compare, contrast, and debate the possible futures of science against the actual history but possible histories. The actual history is not ignored as it provides a reality check for the possible histories, but it has no direct role in teasing out conflicting viewpoints, misunderstandings and biases. While rational reconstructions have a reputation as parts of an outdated philosophy of science, in §4 we see why they are still highly relevant.

The second one focuses on conceptual analysis. It is obvious that the analysis of conceptual problems at the heart of scientific results, methods, and practices is central to our ability to enhance understanding and challenge conventional ways of thinking. Conceptual analyses can range from "a description of ordinary and scientific usage and judgment" to "recommendations about what one ought to mean by various [--] claims", as Woodward (2003, 7) notes. Different points in this continuum have different relevance with respect to our two main objectives: The more descriptive an analysis is, the more understanding it provides about the actual workings of (and problems within) science; The more normative an analysis is, the more it challenges conventional ways of thinking. In best cases, the normative and descriptive aspects are balanced (Woodward 2003, 7-8) and therefore conceptual analysis enables us to enhance understanding and challenge conventional ways of thinking.

We have now seen how different types of philosophical studies can satisfy the two main objectives of the futures research, enhancing understanding and challenging conventional ways of thinking. We have also seen how these studies can tease out conflicting viewpoints, misunderstandings, and biases. By now it should be clear that the attempt to estimate the future of science does not carry with it the idea that we can simply decide or plan how the future of science will be like. On the contrary, we have seen that the philosophy of science can provide understanding about the possible *consequences of* and *restrictions to* decisions and challenge the conventional frameworks where the science-related decisions are made. In this way, by achieving the two main objectives of the estimating of the future of science, the philosophy of science could "provide information, ideas and stimuli to support a third objective; better decision making and strategic planning" which is the third objective of futures research (Wright et al. 2013, 631).

In this section, I have argued that there are good chances that the philosophy of science can improve our understanding of the future of science. However, we still need to ask how the estimating of the future of science is possible and what are the main obstacles in the estimating. I now turn to these issues.

3. Structural Taxonomies of Possible Futures of Science

In order to understand how and to what extent we can estimate the future of science, we need to notice that there exists a unique[3] source of difficulties in the estimating of the future of science. The problem is that there are strong and compelling arguments that show that if we were able to predict a scientific discovery or innovation, then we would have already achieved the discovery or innovation, which is a conradiction. There are two slightly different versions of this problem. First, if we are able to describe a radically new conceptual innovation of the future, we have already made the innovation. "Any invention, any discovery, which consists essentially in the elaboration of a radically new concept cannot be predicted, for a necessary part of the prediction is the present elaboration of the very concept whose discovery or invention was to take place only in the future. (Macintyre 2007, 93). Given that we will have radical conceptual innovations in the future, it follows that our conceptual schemas are insufficient for predicting radical conceptual innovations of the future. Secondly, even if we had a sufficient conceptual schema and made a prediction concerning a novel discovery, we would not have sufficient justification for our belief that the discovery will be made. If a theory T implies that some D is the case, and if we do not already believe that D, then we do not have enough justification for T. Once D is discovered, we might believe in T because D justifies it; but at this point, we can no longer predict D. (See [Finocchiaro 1973, 37] for a similar argument.)

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[3] I.e., a source that is not merely based our general epistemic limitations and general problems in the estimating of the future.

Both arguments above rely essentially on the view that science will change in the future. They also assume that the most important thing to know about the future of science is what exactly will be known, i.e. the exact results of science. The problem is that the first claim is ambiguous and the second one questionable. Of course, the whole point of estimating the future of science depends on the view that science will change in the future. However, it is unclear how much it can change and why. An interesting aspect of the estimating of possible futures of science is to map how much science can change and for what reasons. I return to this soon. Moreover, even though it would be great if we knew what discoveries will be made in the future of science, it does not follow that other questions are futile. For example, the motivation for expensive experiments with fusion power is not that we are able to predict their outcome (whether or not fusion power will be commercially useful) but that we can estimate that there are good chances that we get an answer to our question (i.e., it can be expected that the experiments are good enough to inform us about the possible commercial use of fusion power) (see Claessens 2020, Ch. 12). Even if we cannot predict the future results of science, there still remain many interesting questions we can ask with respect to the future of science. The arguments against the possibility of estimating the future of science are therefore seriously limited.

Moreover, one crucial element in the arguments against the possibility of estimating the future of science is the assumption that the goal of the process is to predict particular events, discoveries and innovations. This appears to be a way too restricted stance towards future-oriented thinking. It is questionable in general, and not just with respect to scientific discoveries and innovations, whether the accurate prediction of particular events in human society is the golden standard of successful futures research. Given that the main objectives of futures research are enhancing understanding and challenging conventional thinking, there is much more to the futures research than predicting. The arguments above do not prove that nothing interesting can be said about the possible futures of science. In fact, given the main objectives of futures research, we have reasons to think that focus on the predicting of particular events puts the cart before the horse.

First, notice that the occurrence of a particular event usually depends on the surrounding context which makes them difficult to predict. Staley argues that “[E]vents are so dependent on individual actions, accident, contingency, context, and any one of countless other variables, [that] venturing a prediction about future events is doomed from the start” (2002, 75). Secondly, decisions affect the future. In order to make meaningful decisions, we have to understand the consequences of those decisions. This is possible only if we understand the possible contexts where the consequences of the decisions unfold, and these means that knowledge about the general context of the future is logically prior to knowledge of particular events. Given these two observations, it seems that we should study possible contexts (or “structures” as I will call them below) where events and decisions might take place in the future.

Understanding possible future contexts can be achieved through the formulation of scenarios: “The goal of scenario writing is not to predict the one path the future will follow but to discern the possible states toward which the future might be ‘attracted.’ [--] If a prediction is a definitive statement of what the future will be, then scenarios are heuristic statements that explore the plausibilities of what might be.” (Staley 2002, 78). While there are different definitions of a scenario and subtle differences between the definitions, in this paper, we can consider scenario simply as a “description of a future situation and the course of events which allows one to move forward from the actual to the future situation” (Amer et al. 2013, 23).

What kind of scenarios can the philosophy of science produce? I suggest that philosophical theories can be used to formulate possible structures of the future of science. I have adopted the term ‘structures’ from Staley (2002, 88) who underlines the boundaries within which events occur and the contexts that produce events, but a natural inspiration for the use of the term in the philosophy of futures of science comes from Kuhn's *The Structure of Scientific Revolutions* which describes the basic epistemic, social, and institutional factors that shape the overall development of science. So, by the notion of *the structure of the future of science*, I refer to a (possible) configuration of factors that produce and set boundaries for scientific development. Given that the philosophical theories of science formulate principles concerning the factors that produce and bound scientific developments, philosophical theories can be used to describe *possible futures of science*: Different theories incorporate different principles, and each theory provides one possible structure of the future of science. It is important to stress that the attempt is not to predict the future. Rather, the attempt is to formulate theoretically possible structures of the future science. These structures serve as the bases for more detailed pictures concerning the future that we create by adding contents to the structures. By the notion of *content* I mean the possible knowledge, methods, and institutional arrangements that might be adopted in the future. Once we have added some possible content to a structure, we have formulated a *scenario* of the future of science. Within a scenario, it is possible to describe particular events.

By using theoretically based structures and by adding several possible contents to these structures, we can build *structural taxonomies of possible futures of science*. Let's take some examples to clarify the approach. I begin with a somewhat detailed example and then move on to examples that are less detailed but complementary to the first one.

(E1) In the Kuhnian philosophy of science, there are (mainly) two kinds of periods in the development of science: *normal science* and *revolutionary science*. A normal science period is a one in which a paradigm defines the research in a scientific field. A paradigm is a "universally recognized scientific achievement that for a time provides model problems and solutions to a community of practitioners" (Kuhn 1970, viii). A paradigm, then, is the condition under which science can develop in a steady fashion. Revolutionary science, on the other hand, is a period in which an existing paradigm is challenged due to its inability to solve important problems and a new paradigm is established. Different paradigms are mutually incommensurable, as there are no shared standards that enable scientists to choose between competing paradigms in the period of revolutionary science. Kuhn makes the point dramatically: “the proponents of competing paradigms practice their trades in different worlds” (1970, 150). It is understandable, then, why a change of paradigm constitutes a scientific revolution.

Kuhn’s theory defines *a possible structure of the future of science*. If Kuhn is right,[4] whatever the details, science will be dominated by a paradigm and this domination will end during a revolution. Given this structure, we need to fill in the contents in order to create scenarios. Which paradigms will continue their dominance? Which paradigms are under serious doubt? What are the possible courses of action, given that a field of research is under doubt? For example, consider the debates concerning the Future Circular Collider (FCC). The debate concerns the possible discoveries and new knowledge created by the FCC. Sabine Hossenfelder (2020) has argued that the cost of the FCC is too great given the chances of possible discoveries. Michela Massimi (2020) has argued that the FCC can be defended once we understand scientific progress not in terms of “great” discoveries but in terms of excluding possibilities. This debate provides us with two sets of scenarios. The first set concerns whether the FCC is built or not. The second set concerns the possible futures within the FCC-centered research.

Take the first set. Given the Kuhnian picture, either the paradigm set by the research with the Large Hadron Collider will continue as the FCC is built (content_1), or the inability to solve important problems leads to a decision to attempt some other approaches in physics ($\text{contents}_{2,1–n}$). Hossenfelder gives two examples of such alternatives “high precision measurements at low energies

[4] And this is a big IF. Let’s assume it for the sake of illustration.

or increasing the masses of objects in quantum states”. Notice that in the Kuhnian structure, there is an ambiguity whether $contents_2$ count as revolutionary. On the one hand, $contents_2$ would be the outcome of the inability to solve central issues within high-energy physics. On the other hand, the Hossenfelder’s suggestions stem from the current background of physics. As Toulmin (1970) pointed out, the absolute revolution vs. normal science distinction is too restrictive interpretive tool. As a consequence, the Kuhnian taxonomy involves ambiguities that need to be removed. We come back to this issue in the next section.

Next, take the second set of scenarios. Hossenfelder argues that it is possible that no significant discoveries will be made with the FCC ($content_{1,1}$). In the Kuhnian structure, the inability to solve problems leads to a revolution. Massimi argues that in addition to clear discoveries that are to be expected on the basis of current theories and methodology ($content_{1,2}$) it is possible that “vain” experimental attempts create the ground for “a revolution similar to the one behind relativity theory in rethinking the theoretical foundations for a new physics” (2020) and such revolution in the foundations of the Standard Model is one possible content ($content_{1,3}$).

Figure 1. presents a taxonomy that is created by adapting a Kuhnian structure and the examples of contents taken from Hossenfelder and Massimi. The taxonomy shows how the Kuhnian structure classifies different futures with respect to their place in the paradigm-revolution scheme. The structure adds a level of interpretation of the future possibilities as it enables us “to discern the possible states toward which the future might be ‘attracted’” (Staley 2002, 78). Such taxonomies are useful because their theoretical bases are transparent and because they allow us to see at a glance what kind of futures are possible. Once we have side-by-side several taxonomies that differ in their theoretical basis, the understanding of different possible futures is enhanced even further (see below). Moreover, they provide a rather direct feedback loop. Given that the interpretations of certain contents within the structure are dubious, we are able to reflect the merits of the theory that

serves as the basis for the structure. For example, the interpretation of contents (2,1)-(2,2) as revolutionary scenarios seems somewhat problematic. Moreover, contents (1,1) and (1,3) both constitute a scenario of revolution, but they disagree on when a revolution is possible. (1,1) says that mere lack of results leads to a revolution, whereas (1,3) says that revolution requires that there are further experimental results that do not fit the existing theories. In §4 and §5, I return to this issue.

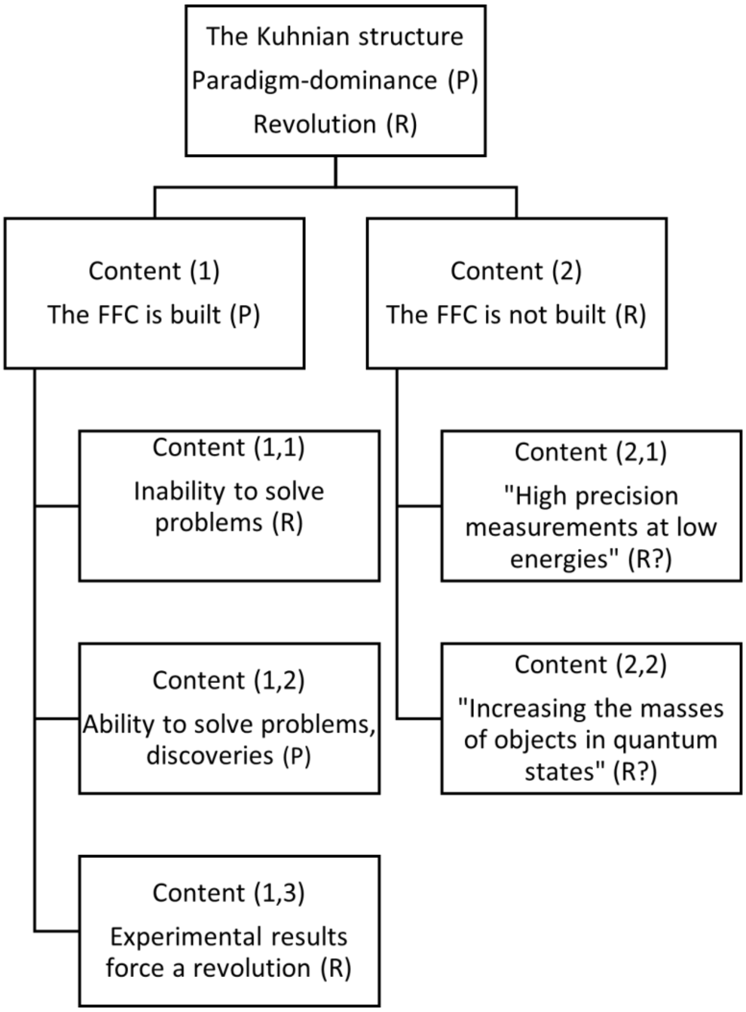

Figure 1

Without taking a stance with respect to the credibility of the Kuhnian structure or the specific suggestions on the contents, we can still see how we can formulate taxonomies of different scenarios about possible future of science. All contents from (1) to (2,n) provide a "description of a future situation and the course of events which allows one to move forward from the actual to the future situation". Moreover, and more importantly, the scenarios enhance understanding and challenge conventional ways of thinking. For example, Massimi (2020) explicitly argues that "particle physics community has long stopped (if ever did) following any Popperian method of hypotheses-testable predictions-falsification" and the possible future of the FCC should not be understood in those terms. Massimi also makes an important note on the scientific revolutions: The direction of a revolution is not arbitrary. Rather, revolution can only change a field whose foundations have been examined by a long tradition of detailed research. This implies that, while we cannot predict the future of science because there (arguably) are fundamental changes, it does not follow that the possible changes cannot be narrowed down. We cannot expect a revolution in the foundations of the Standard Model without "the ongoing, unfailing, and indefatigable efforts of experimentalists at places like Cern". On the other hand, Hossenfelder (2020) challenges the centrality of the FCC for the future of physics and science in general by arguing that the money has better uses, given climate change and pandemics. Hossenfelder also discusses a crucial but unknown causal factor that needs to be taken into account when scenarios are examined, the properties of the targets of research: "But there is no reason why the particles that make up dark matter or dark energy should show up in the new device's energy range. And that is assuming they are particles to begin with, for which there no evidence. Even if they are particles, moreover, highly energetic collisions may not be the best way to look for them. Weakly interacting particles with tiny masses, for example, are not something one looks for with large colliders."

The discussion in the example of the Kuhnian structure also shows how the worries in §2 that nothing can be said about the future of science or that the estimating leads to wishful thinking and external control of science are exaggerated. The example illustrates that there can be reasonable discussions about possible futures of science, based on what we have learned about the development of science, and that the reasonable discussion does not threaten the openness of the paths in scientific development.

(E2) To balance the revolution-centered Kuhnian structure, it is possible to choose theories with different tenets as the theoretical basis of a structural taxonomy. For example, consider structural realism which says that the structural or mathematical contents of successful theories are preserved through theory change (Worrall 1989; see discussion in Frigg & Votsis 2011). There may not be ontological continuities, as Laudan (1981) argued (see also §2), but there are structural continuities. Given the structural realism, we are able to build scenarios where disconnection-inducing revolutions do not dominate the landscape of theory change. A structural realist's taxonomy would be constituted by expansions of the following scheme:

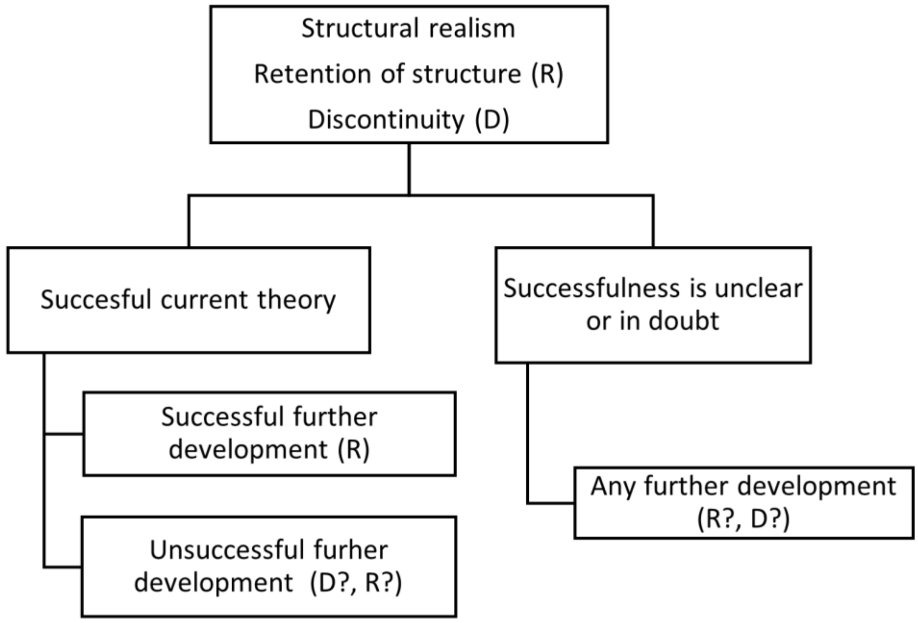

Figure 2

A notable implication of the structural realism taxonomy is that it can provide clear scenarios only when we track paths of successful science.[5] Although this might appear as a serious limitation, there are considerations that support the focus on successful science. First, it is easy to create an unsuccessful science. There are too many scenarios where a science without success exists. One can take current successful theories and add nonsense to them, or one can create a whole new theory consisting of nothing but nonsense. However, such scenarios are of interest only in special cases. Usually, we are interested in understanding the future of science on the assumption that science remains at least as successful as it is now. Secondly, the focus on successful science enables to describe the boundaries of change in the future of science: at least in the case of successful theories,

[5] Structural realism is also restricted to rather formal sciences and seems to leave out less formal ones (Frigg & Votsis 2011, 269). I return to the consequences of such limitations in the next section.

the structural features may not be completely abandoned in the future. As said, the structural-realism taxonomy balances the overall revolution-centeredness of the Kuhnian taxonomy.

Indeed, different taxonomies are at their best in providing information about possible futures of science when they are used side-by-side. For example, some scenarios in the Kuhnian and structural-realism taxonomies differ with respect to the continuity in the scientific development. Such differences enable us to be aware of the consequences of our theoretical commitments. However, some scenarios in the Kuhnian and structural-realism taxonomies are rather similar. For example, in both taxonomies. there are scenarios where theoretical change without theoretical continuity happens. Such similarities enable us to understand repeatable patterns across different theoretical commitments.

(E3) A structural taxonomy does not have to be based on the kinds of macro-principles of scientific development that the Kuhnian taxonomy exemplifies. For example, in an interesting study, Bedessem and Ruphy propose "three epistemological conditions that influence the occurrence of the unexpected in the course of a scientific inquiry" (2019, 1). The study enhances understanding and especially challenges conventional thinking according to which "a research whose agenda is set according to external considerations is less hospitable to the full flourishing of the unexpected than a research whose agenda is freely set internally by scientists" (ibid.) Bedessems and Ruphy's study is interesting from the perspective of the estimating of the future of science also because it confirms that, while we are unable to predict future inventions or discoveries, we can still say interesting things about the processes or structures surrounding the inventions and discoveries.

The three epistemological conditions that influence the occurrence of the unexpected are the following:

(i) Leeway for the manifestation of uncontrolled factors: “Unknown causal pathways existing in the real world are thus inoperative (or less operative) in highly controlled laboratory conditions, thereby limiting the occurrence of unexpected results. Inversely, a low degree of isolation and control favors the manifestation of unknown causal pathways, hence the occurrence of unexpected results”. (Ibid, 2.)

(ii) Diversity of objects under study and of experimental approaches: “[M]ultiplying the types of objects and the types of experimental approaches used to study them increases the probability that some uncontrolled factors intervene and that some unknown causal pathways become manifest.” (Ibid, 2.)

(iii) Hegemony and plasticity of the theoretical background: “[W]ell-established theoretical framework may hinder the occurrence of the unexpected when it is in a hegemonic, monopolistic position, that is, when it constitutes the dominant theoretical framework of inquiry in a given field (ibid.).

Bedessem and Ruphy argue (2019, §5) that importation of exogenous problems that “incorporate interests and needs external [--] to scientific communities” may actually favor the occurrence of the unexpected. We can formulate the following taxonomy on the basis of these insights.

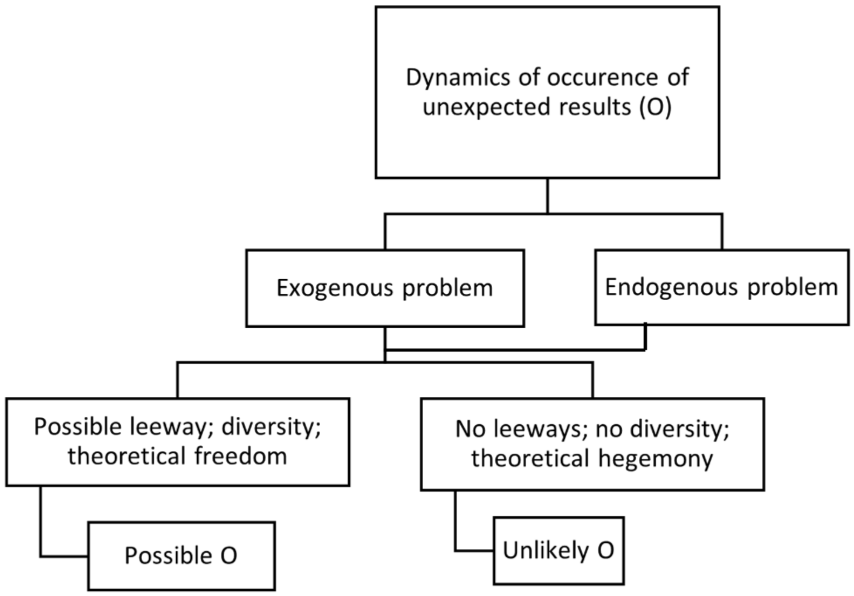

Figure 3

This simplified taxonomy provides information about many scenarios concerning the occurrence of unexpected results. It tells us in what kinds of situations unexpected results can occur and what makes them unlikely. Most importantly, the taxonomy encodes the main insight of Bedessem and Ruphy's study by making explicit that the occurrence of unexpected results can occur both in endogenous and exogenous problem situations. In this way, we can recognize repeatable patterns in seemingly different phenomena within a single taxonomy (whereas (E2) showed how such patterns can be found across taxonomies).

I conclude this section by noting that the impossibility of predicting the future results of science does not mean that we cannot say anything interesting about the possible futures of science. On the contrary, by focusing on the dynamics and boundaries of scientific development, analyzed by the philosophy of science and related fields, we can draw extremely nuanced pictures of possible futures of science in different scales. We can enhance understanding and challenge conventional thinking by building taxonomies of possible futures of science. However, we have to be careful here and take some critical distance from the taxonomies. We need to ask how the merits of a taxonomy are to be judged.

4. Assessing the Merits of a Structural Taxonomy

In this section, I argue that there are at least four sets of considerations that are relevant in assessing the credibility and usefulness of a structural taxonomy. In addition to explicating how the merits of a taxonomy can be assessed, I also point out that there are interesting connections between the different sets of considerations which sheds new light on the nature, prospects, and limits of philosophical theories of science.

(I) *Historical considerations*. A straightforward way to assess the merits of a taxonomy is to ask how warranted the base-theory is. A good theory must be supported by the history and current state of science. There is, however, the serious problem that it is difficult to tell how philosophical theories could be tested against the history of science.[6] Bolinska and Martin (2020) have summarized the discussion in terms of methodological and metaphysical objections to the use of case studies in support of philosophical theories. "Methodological objections claim that historical accounts and their uses by philosophers are subject to various biases. [--] Metaphysical objections, on the other hand, claim that historical case studies are intrinsically unsuited to serve as evidence for philosophical claims, even when carefully constructed and used." (Ibid, 37).

There are four methodological objections. According to *construction bias*, historical accounts are theory-laden, and "it is always possible to construct alternative narratives for a given historical episode" (ibid, 38). According to *selection bias*, philosophers can cherry-pick those historical episodes that support their case (ibid, 38-39). According to *interpretation bias*, "not only might our

[6] See Donovan et al. (1988); Pitt (2001), Schickore (2011), Kinzel (2015); *JPH* 12 (2) (2018).

theoretical commitments bias the construction and selection of case studies; they can also affect how case studies are *interpreted* (ibid, 39). According to *application bias*, "[e]ven if we agree on the facts about historical cases and how to interpret them, we might still disagree about what we ought to conclude on that basis" (ibid, 39).

Bolinska and Martin argue, convincingly, that the possible biases do not make it impossible to adjudicate between different philosophical theories on the basis of historical considerations. Of course, it is impossible to find *the* philosophical theory that is confirmed by *the* historical evidence, but this does not mean that the relative strengths of different philosophical theories and historical interpretations cannot be assessed. Moreover, Bolinska and Martin argue that "[h]istory, philosophy, and indeed most academic disciplines rely on careful, critical analysis to answer difficult questions, even if a firm answer is not immediately forthcoming" and that "whatever stand we take, we should admit its fallibility" (ibid, 40)

We should also notice that the seriousness of methodological limitations depends on the goal of our research. If we want to know how science *really* works, the methodological problems seriously injure the prospects of successful research. However, if our goal is to enhance understanding and challenge conventional thinking (see §2), then we can settle for something less. Given that different structural taxonomies are intended to be used together to map the possible futures of science, we do not have to make the dubious choice of only one base-theory on the basis of historical and factual considerations. Rather, we can rank the credibility of the base-theories on the basis of what we currently know about the history of science and prefer some theory over others. However, the alternatives that are ranked lower are not lost from sight in the process. Rather, they show – literally – what future options the preferred taxonomy might miss.

The metaphysical problems are *Heraclitianism* and *contingency*. According to *Heraclitianism*, "[s]cientific concepts, experimental methods and standards, and even the notion of science itself shift from one historical moment to another (ibid, 40). The historical particularities resist philosophical generalization. According to *contingency*, "[b]ecause history is not governed by strict, deterministic rules [--], it might in some meaningful sense have gone differently. Because history might have gone otherwise, we have ample reason to doubt whether historical examples can constitute firm evidence for philosophical claims that seek to generalize about scientific practice and process". (Ibid, 41.)

Bolinska and Martin suggest, correctly it seems, that the metaphysical objections can be addressed by finding a suitable type of contingency in history. The basic idea is that if a historical outcome is contingent *upon* a set of factors, then those factors explain the outcome (Ibid, 42-43). This in line with the counterfactual approach to causal explanation (e.g. Woodward 2003); if Y would not have been the case, had X not been the case, then X explains Y.[7] Given that a philosophical theory incorporates the set of factors that the historical outcome depended on, the philosophical theory is supported by its ability to explain the outcome. The variability and contingency of history do not pose any problems to the philosophical theories as long as the theories can account for the variability and contingency.

In general, it is not despite but because of the invariability and contingency of history that philosophical theories that make sense of historical processes are powerful tools to build scenarios of the future of science. We need to understand different types of changes that might occur in the future and we need to be able to tell upon what factors those changes will depend. Philosophical theories that make sense of historical variability and contingency are well suited as the bases of

[7] Of course, we have to specify how X was supposed to change in order to avoid technical problems, but that is not our business in this paper.

structural taxonomies precisely because of the conceptual similarities between historical contingencies and future possibilities.

Given the observations above concerning the methodological and metaphysical problems in using historical evidence in the philosophy of science, there is no serious obstacle in using historical considerations in the assessment of philosophical theories that serve as the bases of structural taxonomies, given that we follow good methodological practice and explanatory reasoning. As long as we have philosophy of science that makes sense of the history of science, we can assess the merits of a structural taxonomy by assessing how warranted its base-theory is in the light of up-to-date knowledge in the field. The estimating of possible futures of science is under epistemic control as long as the philosophy of science is.

(II) *Normative considerations*. When we estimate the future, we are not only interested in what could happen but what *should* happen. As noted in §2, the futures research is also interested in preferable and desirable futures. Given this, we are often eager to make suggestions about epistemic and institutional principles that could improve science (let's call these "normative principles" for the sake of clarity). In fact, such suggestions seem essential for the fruitful development of science. However, such principles often describe, explicitly, activities and institutional arrangements that cannot be found in history. If we suggest a structural taxonomy that has as its base-theory a normative principle, there often are not direct historical considerations that bear on the credibility of the principle. How to evaluate the merits of such taxonomy?

I suggest that the merits of a normative principle can be tested by rationally reconstructing historical episodes in accordance with the principle. The idea of rational reconstructions goes back to Lakatos (1971) who suggested that we can evaluate philosophical theories of science by asking how well they enable one to reconstruct the history of science as a rational development. Basically, we have to provide an account of how the history of science would have been, had it developed in accordance with a philosophical theory, and then measure the distance between the would-have-been history and the actual history. In essence, we have to study counterfactual developments. This connects rational reconstructions with more direct historical considerations. As we saw above, a philosophical theory that suggests that the factors F explain some development of science is supported by the history of science if it is the case that, had F not existed, the outcome O would have been different. Analogously, in a rational reconstruction, we study what would have happened, had some other set of factors F* existed in the past. If F* would also have produced O or some *better outcome* than O, the philosophical theory incorporating F* is supported.

The basic question we need to ask when assessing a normative principle is whether we should have adopted it in the past. If so, then it could provide us with an insightful structural taxonomy. In this way, we can use historical considerations to assess normative principles. Notice that normative principles do not have to be novel suggestions. Theories that have been abandoned because they do not account for the factors that have actually shaped the development of science can sometimes be revitalized as normative theories[8] – as long as they suggest that some course of action or organization would have led to a preferable outcome.

The first two ways of assessing the merits of a structural taxonomy told how historical support is relevant for the merits. The next two will focus on the features of taxonomies themselves but they also provide a feedback loop back to our understanding of the historical virtues of the base-theories.

(III) *Usefulness of a taxonomy*. The more scenarios a taxonomy incorporates and the more definite the scenarios are, the better the taxonomy. An insightful taxonomy provides unambiguous scenarios

[8] This sometimes happen when scientist themselves adopt a philosophical position, such as falsificationism.

when more contents are added to its structure. A *fault spot* of a taxonomy is a scenario that is dubious, incoherent, or ambiguous, and taxonomies should avoid fault spots.

For example, in the Kuhnian taxonomy, there was the ambiguity whether scenarios$_{2,}$ where the FFC is not built, are scenarios of revolution or not. Moreover, the taxonomy incorporates two scenarios of revolution with rather different causal structures. In the scenario (1,1), a revolution occurs because certain problems cannot be solved. In the scenario (1,3), a revolution occurs because the attempts to solve the problems provide knowledge that can be used to formulate a new theoretical frame. Given these fault spot ambiguities, the Kuhnian taxonomy has its weak sides.

The fault spots in taxonomies suggest topics that require further historical (or conceptual) investigation. For example, in order to make the Kuhnian taxonomy more insightful, we have to ask detailed questions about the dynamics of the (supposed) revolutions in the history of science.[9] In this way, future-oriented thinking can open new perspectives and lines of research concerning the development of science. Our fault spots in the estimating of the future of science reveal fault spots in our understanding of the development and workings of science.

(IV) *The comparison between taxonomies*. We can compare different taxonomies with each other in order to find disagreements, agreements, fault spots, and blind spots in the taxonomies. A *blind spot* is a scenario that is altogether missing from a taxonomy. We already saw, in the previous section, that comparison between taxonomies can provide information about repeatable patterns across theoretical backgrounds. Here, it is useful to distinguish between two kinds of taxonomies: (i) dominant taxonomies that are well-supported by criteria (I-III) above, and (ii) challenger taxonomies that tease out the shortcomings of the dominant taxonomies. Dominant taxonomies

[9] I am not suggesting that there have not been refinements to the Kuhnian theory before. Obviously, there have been, for example in Toulmin (1970). My suggestion is that the future-oriented analysis can improve this existing practice. I also discuss the refinements in the next section as a part of estimating of the future of science.

support each other when they agree on scenarios and they question each other when they disagree on scenarios. A good and insightful dominant taxonomy agrees with many other dominant taxonomies. Challenger taxonomies, on the other hand, are good and insightful when they present scenarios that are missing from dominant taxonomies. Challenger taxonomies are important in their ability to challenge the understanding – conventional or not – embedded in the dominant taxonomies. They enable us to better understand the implausible but possible futures that might challenge our courses of action concerning the future.

The comparison between taxonomies also provides feedback for the philosophical theories. Given that we can find similarities and differences between the scenarios of different taxonomies, we are able to see how similar or different the commitments of two different philosophical theories are or how many ambiguities they share. For example, if two seemingly conflicting theories agree in great detail on the scenarios they entail, we have to ask what the fundamental difference between the theories is supposed to be or how come it does not have many practical consequences. Moreover, structural taxonomies can also indicate that some topic is not analyzed enough in the philosophy of science in general. A surprising future-related question that we cannot answer should indicate that something is missing from the philosophy of science.

In this section, we have seen how the merits of a structural taxonomy can be assessed. In essence, we have no other evidence for the philosophical theories than historical evidence since there obviously is no direct evidence of the future. All we can do is to study how science has and could have (plausibly) developed. Given that the future might not resemble the past, historical evidence is *prima facie* dubious with respect to the future. Given this, we had to make some modifications to the use of historical evidence: First, we do not choose but prefer one philosophical theory over the others and therefore keep the wide future possibilities is sight. Secondly, we allow normative theories to be used as the base-theory of a structural taxonomy in order to see futures that do not follow historical but merely possible (and desirable) principles. Thirdly, once we have found dominant taxonomies, we stay aware of unnoticed future possibilities by formulating challenger taxonomies. Given these qualifications, we can find historical support for our scenarios of the future and, at the same time, avoid drawing straightforward analogies between the future and the past.

Moreover, we have seen how future-oriented study of science can reveal dubious commitment, ambiguities, incoherence, and ignored topics in the philosophy of science. In this way, the future-oriented philosophy of science can enhance our current understanding of the present science and its development. Future-oriented thinking is a great tool for our research *now*, not merely in the future.

5. **Conclusion**

In this paper, I have argued that deep theoretical analyses of possible futures of science are missing. Despite the important questions regarding the future of science, the impossibility of predicting and dangers in controlling the future of science have made the philosophy of science skeptical towards the prospects of estimating of possible futures of science. Due to this, conceptual tools to estimate the future of science are missing from the repertoire of the futures research. In this paper, I challenged this state of affairs. In §2, I argued that the philosophy of science has, due to its nature, good prospects of achieving the central objectives of the research, enhancing understanding and challenging conventional thinking. In Section3, I went through arguments that establish the impossibility of predicting future innovations and discoveries and argued that such predictions do not exhaust interesting future concerns. I argued that we can formulate scenarios of the future of science by using philosophical theories of science. By adding contents (describing the possible adoption of some knowledge, methods, or institutional arrangements) to the general structures outlined by philosophical theories, we can achieve rich taxonomy of scenarios of the future of science. By creating scenarios of the future of science, the taxonomies enhance understanding and challenge conventional thinking. In §4, I argued that the merits of the taxonomies can be rationally assessed and, therefore, we can critically estimate the possible futures of science.

The theoretical plurality and disagreements in the philosophy of science can be turned into a resource in the estimating of futures of science. The plurality enables us to (i) formulate many taxonomies and thereby exhaust a wider space of possibilities, (ii) compare different taxonomies to find repeatable patterns across theoretical commitments, (iii) provide different interpretations of a node in a taxonomy (e.g. different views on the dynamics of revolutionary change in science interpret different nodes as revolution-constituting), and (iv) formulate challenger taxonomies that challenge the received views. While the philosophers of science do not agree on many things, it is clear that the philosophy of science, as a whole, has deeply enhanced our understanding and shown many weaknesses in the conventional ways of thinking about science. The prospects of estimating the future of science serve as a measure of the often hidden or questioned progress in the philosophy of science.

Moreover, we saw in §4 that future-oriented thinking can open new perspectives on the existing philosophical issues and even point out ignored topics in the philosophy of science. While the differences between theories cannot be reduced to their future consequences, asking future-oriented questions helps to improve the current understanding of science. By approaching the possible futures of science, we open a whole new perspective on the philosophy of science.

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