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Pursuitworthiness in the scheme of futures

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Abstract
In this paper, I argue that analyzing pursuitworthiness in science requires that we study possible futures of science. The merits of different criteria of pursuitworthiness need to be assessed against scenarios of the future of science. Different criteria recognize and ignore different scenarios. As a consequence, different criteria enable us to manage different future possibilities. While it might be impossible to predict the future of science, there are still many interesting things we can say about the possible futures of science. We can construct scenarios of the future of science on the basis of philosophical accounts of science. I point out that the topic of pursuitworthiness is not the only topic that can be approached by connecting philosophy of science with the construction of scenarios.

Keywords Pursuitworthiness · Futures research · Scientific change · Scenarios · Future of science

1 Introduction

Science requires resources. This limits the number of research programs[1] that can be initiated and kept alive. Due to this, we have to make decisions concerning which research programs to pursue. This generates the problem of *pursuitworthiness*. We have to identify those research programs that are worthy of pursuit. On what criteria can the identification be based?

The problem is difficult because we (most likely) do not want to define pursuitworthiness in terms of the actual success of a research program. A novel research program cannot have much actual success before it is actually developed. Still, we

[1] In this paper, I use the term "research program" in a non-technical sense to refer to any systematic research that attempts to achieve something: a new theory, model, experimental result, paradigm, technology, and so on. I make more detailed distinctions when it matters to our views on the future.

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most likely wish to develop novel lines of research. This means that the accounts of theory-choice and scientific rationality that focus on the actual features of research programs are not straightforwardly applicable to pursuitworthiness (Whitt, 1990, 467). The question, then, is how to assess the potential of a research program.

To see the depth of the problem, we may notice that even if we focus solely on the actual and mature research programs, the problem of pursuitworthiness is not automatically solved. The actual success of a program thus far does not guarantee that it will be successful in solving new problems in the future. Even less the success guarantees that it is better than some other candidate research program that we could reasonably develop using the available resources. As Nickles puts it, "What justifies a scientist's decision about what to work on now, or next, is not its past—its confirmation track record—so much as what it promises for future innovation" (2006, 161). Moreover, it has been pointed out that scientific discoveries might require more and more resources as a research program develops (Rescher, 1978). If so, the further we develop one program, the less discoveries it provides, given the limited resources. At some point, alternative programs would need to be considered in order to solve the situation.

Many suggestions have been given concerning what criteria can be used to identify pursuitworthiness, and these have been helpfully summarized by Shaw (2022, 104). The criteria include the potential to interesting extensions, unification, ability to handle outstanding problems, empirical fertility, conceptual clarity, explanatory power, programmatic character, exemplary practices, and ability to achieve goals (Achinstein, 1993; McKaughan, 2008; McMullin, 1976; Šešelja & Straßer, 2014; Šešelja et al., 2012; Shan, 2020; Whitt, 1992). In other words, the criteria suggest that a research program is worthy of pursuit if it is potentially well-organized and can, therefore, achieve the virtues that are associated with good science.

Analysis of pursuitworthiness is a future-oriented topic. It is concerned with the question of *what could happen in the future* on two levels. The first level is internal to a particular research program. On that level, we wish to know whether the research program could be fruitful in the future. The second level concerns the overall development of science and the context in which science is conducted. This second level motivates the analysis and also the usefulness of any given criterion of pursuitworthiness. The second level is partially built into some of the criteria of pursuitworthiness. For example, the ability to handle outstanding problems and achieve goals is the ability to react in situations where we demand solutions to novel problems from science. Here the possibility of changes in the context of science, i.e., to what questions we demand science to answer in the future, motivates the criteria. The two levels come together in that criteria of pursuitworthiness are criteria that attempt to capture which research programs could be fruitful, given a set of possible future conditions in science. For example, the ability to handle outstanding problems tells that the research program has the potential to be fruitful in conditions where we demand answers to novel and urgent questions.

However, despite the fact that pursuitworthiness is concerned with the question of what could happen in the future and despite the fact that some criteria refer to possible futures of science, the criteria have not been assessed against a systematic mapping of possible futures of science. Many of the criteria commit to and are motivated by certain possibilities for the future of science but remain silent about other, perhaps contradictory, possibilities. For example, Šešelja and Straßer rely in their approach to pursuitworthiness on the notion of *scientific crisis*. The need for plurality in pursuits stems from the fact that "these times of crisis we do not want to face empty-handed" (Šešelja & Straßer, 2014, 3112). But what if there are no crises in the future? Then it would be a waste of resources to develop research programs to overcome crises. I want to emphasize that I am not suggesting that the reasoning of Šešelja and Straßer is misguided. However, the future with crises is only one possible future of science. It might very well be serious enough to guide our decisions, but we would still like to understand what other possibilities there are to put the criterion based on crises into perspective. Seen from another angle, Šešelja and Straßer suggest that we should have a criterion of pursuitworthiness that enables us to prepare for and react to a significant future possibility, a crisis in science. No research program is pursuitworthy *simpliciter* but only with respect to a goal or possible future state of affairs. This means that there is no criterion of pursuitworthiness *simpliciter* but only criteria that enable us to prepare for and act toward kinds of possible futures, such as crises. The question of possible futures is at the core of the discussion of pursuitworthiness.

It is important to notice from the beginning that the discussion about what could happen (i.e., possible futures) is not a discussion about what is likely to happen (i.e., likely futures). Criteria of pursuitworthiness are not necessarily criteria for which research programs are likely to succeed in the future (even given the failure of actual and most mature programs). They are criteria for which research programs we should pursue, given some possible future changes. For example, a scenario of a possible crisis in the future might force plurality in our pursuits, even if we assign low probability to a crisis. Moreover, as we will see in §2, considerations of pursuitworthiness are motivated by uncertainty and the ability and desire to affect the future. Pursuitworthiness is, therefore, best understood as a discussion about what kinds of research programs enable us to prepare for and work toward those futures that we find significant. This significance does not depend on the likeliness in any straightforward way. In this paper, I focus on how to find such significant possible futures against which criteria of pursuitworthiness matter, not on the question of how to assess their likeliness (see §5).

The main argument of this paper is that, since there are consequential questions about the future of science that we are uncertain about, we have to be prepared for different possibilities, such as crises, and the preparedness requires mapping of possibilities. The existing discussion of pursuitworthiness is motivated by the same idea of preparedness for future changes and thereby requires a mapping of future possibilities. I argue that there is a need and a way to map possible futures of science, and the criteria of pursuitworthiness can be assessed in a novel way in the light of such mappings. I am not suggesting a criterion of pursuitworthiness. Rather, I wish to make explicit the connections between uncertainty about the future of science, criteria of pursuitworthiness, and preparedness for different future possibilities. I am not suggesting that the mapping of possible futures is necessary for making correct decisions about pursuit in all contexts. Clearly, it is not, as decision-makers seem somewhat successful in identifying successful science in specific contexts even without such mapping (Shaw, 2022). Rather, I argue that the mapping of possible futures is necessary for the assessment of philosophical criteria of pursuitworthiness. The criteria are motivated by, and directed to, specific views on what could (or should) happen in the future, i.e., possible futures of science.

To be more exact, in this paper, I discuss how analyses of pursuitworthiness are committed to and motivated by scenarios of possible futures of science. Criteria of pursuitworthiness are built to prepare us for and react to specific types of futures and not others. I argue that the merits of criteria can be assessed against rich sets of scenarios of the future that show (i) what kinds of futures a particular criterion enables us to prepare for and react to, and (ii) what kinds of futures the criterion is silent about. I show how we can systematize our answers to the question of *what could happen in the future of science* and thereby strengthen the analysis of pursuitworthiness that is also concerned with the same question. I draw resources from futures research which is a field that concerns possible and desirable futures and our cognitions about those futures. There are specific needs and motivations that shape futures research, and the discussion on pursuitworthiness could benefit from closer contact with these goals and motivations. I argue that how we should think about criteria of pursuitworthiness depends on how we should conceive and reason about the future in general.

I proceed as follows. First, I explain why and how futures research focuses on many possible futures. I show that similar motivations are present in discussions concerning pursuitworthiness, albeit implicitly. This motivates the rest of the discussion. Secondly, I will discuss the problem of predicting the future of science (Shaw, 2022) and show how this problem can be mitigated in the discussion on pursuitworthiness by replacing prediction with the mapping of possible futures. Thirdly, I will discuss how implicit conceptions of science guide our reasoning concerning the future of science. I argue that making these conceptions explicit would improve our assessment of the criteria of pursuitworthiness. Finally, I discuss how the topic of pursuitworthiness is only one indication of the value of the philosophical study of the future of science. I argue that the themes raised by pursuitworthiness reveal that, in general, the estimating of possible futures of science demands serious attention in the philosophy of science.

2 Pursuitworthiness and possible futures of science

In futures research, possible, probable, and desirable futures are studied (Amara, 1974; Bell, 2009). An essential component in the mapping of futures is the critical study of our own conceptions that ground different scenarios[2] of the future (Bell, 2009; Inayatullah & Milojevic, 2015; Inayatullah, 1998). As Bell points out, "[t]

[2] While there are different definitions of *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). I do not wish to use *scenario* in any of its stricter technical senses (see e.g., Spaniol & Rowland, 2019).

he exploration of possible futures includes trying to look at the present in new and different ways, often deliberately breaking out of the straitjacket of conventional, orthodox, or traditional thinking and taking unusual, even unpopular perspectives" (2009, p. 76–77). The interplay between understanding possible futures and the critical study of our conceptions about the future is captured best by the notion of *alternative futures* that has been a guiding concept in futures research (see Slaughter, 2020). The critical study of the conceptions that guide our views on the future means that the future scenarios should be varied and that we should create scenarios that challenge the ones that we take to be most natural or plausible (see §4 and §5). This idea is captured by the notion of *alternative futures*. The notion also suggests why futures research is not essentially concerned with prediction or likely futures. Rather than approximating a likely future outcome, the attempt is to increase awareness of possible futures and thus allow us to prepare for a variety of outcomes. Interestingly, this is also a motivation for discussion of pursuitworthiness, as we will see.

The study of the future is grounded on the assumptions that

(i) the future is difficult to estimate reliably (*uncertainty*),
(ii) decisions affect the future (*accountability*),
(iii) the most desirable futures may not be causally accessible (*humility*).

Futures research, therefore, requires that

(a) many possible futures are mapped (to account for i and ii),
(b) the desirability of the possible futures is evaluated (to account for ii), and
(c) our preferences have a reality-check (to account for iii).

The basic assumptions of the study of the future may seem rather trivial. However, their consequences are far from trivial and their value stems from their ability to guide systematic analysis of possible futures, as we will see. Many future-oriented discourses, such as the one on pursuitworthiness, can be framed and developed further in terms of these assumptions.

In addition to basic assumptions, the two main objectives of futures research 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".

To be sure, futures research is not a unified field (see Bell, 2009), and the assumptions and the goals above do not, in themselves, provide a systematic approach to issues concerning the future. Rather, this paper proceeds by building, step-by-step, an approach to the mapping of possible futures of science that is in accordance with the assumptions and goals. This type of mapping allows us to analyze the issue of pursuitworthiness in a novel way.

To see this, we may begin by noting that criteria of pursuitworthiness rely on views on the possible dynamics of science and, thereby, views on the possible future dynamics of science. An overreaching theme in the literature on pursuitworthiness is the view that, in science, new lines of research and the prospects of existing lines need to be constantly assessed. As Shaw notes, "most (if not all) proponents of a logic of pursuit want to allow space for some pluralism" (2022, 104). In the relatively early discussions on pursuitworthiness, the value and need for pluralism were framed as facts of the history of science that need to be appreciated in the spirit of the historically oriented philosophy of science. For example, Laudan, following Feyerabend,[3] notes that pursuitworthiness is a worthy issue because every new research tradition occurs in circumstances where they are less acceptable than their rivals. Another side of the coin, according to Laudan, is the historical fact that a scientist can work in two different traditions. (Laudan, 1977, 110). Also, Achinstein simply notes that "Sometimes a scientist presents a new theory without attempting to argue that it is true or even probable [–]" and associates this phenomenon with the need to allocate resources (1993, 90).

More recently, the discussion on pursuitworthiness has focused on more nuanced motivations and reasons behind the pluralistic aspirations that frame the analyses on pursuitworthiness.

In their approach to pursuitworthiness, Šešelja and Straßer rely on the notion of *scientific crisis*. "[T]he history of science reveals that scientific knowledge is highly dynamic and we shouldn't be all too assured with the theories we have accepted. Not just is it the case that theories often have to be altered and adjusted, but sometimes they have to be entirely replaced." (Šešelja & Straßer, 2014, 3112). The need for plurality in pursuits stems from the fact that "these times of crisis we do not want to face empty-handed" (Šešelja & Straßer, 2014, 3112). Šešelja and Straßer argue that we need to have robust scientific knowledge that is able to maintain performance in the face of perturbations and uncertainty. Because any theory may face crises, we need more than robust theories. We need robust scientific knowledge as a whole. From this perspective, "scientific knowledge is composed and structured by layers of more and more entrenched theories" (Šešelja & Straßer, 2014, 3113). In addition to accepted theories, we need a layer of "alternative theories that may in times of crisis offer good backups for the accepted theories, or that may under further development eventually surpass the currently accepted theories" (Šešelja & Straßer, 2014, 3113). Pursuitworthy theories do not have to be justified, but they have to be *potentially epistemically justified* and contribute to the robustness of scientific knowledge.

It is interesting to note that Šešelja and Straßer rely on three (implicit) assumptions about the future that are also at the core of futures research. First, there are

[3] As we will see, Feyerabend can be also viewed as a limiting case of motivating criteria of pursuitworthiness because he thought that no substantial criteria for pursuitworthiness can be found (see Shaw, 2022).

uncertainties about the future. History indicates that even our best theories may face serious crises (*uncertainty*). The claim is not that crises are likely but that they have occurred in history (indicating possibility) and that the possibility of a crisis is serious enough to suggest that we have to have alternative research programs at hand. Secondly, our actions should enable us to prepare for and react to future changes. Pluralism in pursuits is a condition that enables us to respond to multiple possible futures, even the ones that generate scientific crises (*accountability*). Thirdly, we need to prepare for futures that would show that our best efforts today are fruitless. Even if we wished that we should do nothing more than to develop the current best theories further, it might turn out that we have to do more (*humility*).

The analysis of Šešelja and Straßer also matches the goals of futures research. It relies on (presumed) patterns in scientific development. There have been crises and there might be crises in the future. The analysis also challenges a conventional view of scientific knowledge as consisting of accepted theories. What also matters for our future aspirations are alternative backup theories.

The themes that arise in Šešelja and Straßer (2014) repeatably occur in the framing of analyses of pursuitworthiness. For example, Nickles argues that "[g]ood research is highly adaptive to changing situations, including local contingencies" and that epistemic appraisal (EA) is not highly adaptive in this way (2006, 161—162). Therefore, we need heuristic appraisal that evaluates the future potential of a claim, technique, proposal, etc. Again, we see the assumptions of *uncertainty*, *accountability*, and *humility*. Moreover, Nickles relies on historical considerations in his argument for the need for heuristic appraisal. He points out that "[s]ome of our most fertile theories, models, and programs have been known in advance to be false and even inconsistent (or otherwise incoherent) at some point" and "there are also plenty of examples in which a presumably true theory or correct result is unfruitful as a future research site" (Nickles, 2006, 162). Here, patterns of historical development inform the construction of tools for heuristic appraisal that enable us to react to future changes and contingencies. Nickels also challenges conventional thinking by arguing that "standard EA leaves us with a severely limited account of scientific decision-making, even when the key materials are already assembled and on the table for appraisal" (2006, 159).

Accountability is a topic that has been emphasized in the analyses of pursuitworthiness. This is the case especially when the discussions widen the scope of criteria of pursuitworthiness from epistemic promise to a wider set of values in a society. DiMarco and Khalifa (2022) argue that pursuitworthiness can be approached in terms of criticism of scientific pursuits. "First, one may be criticized for failing to fulfill one's obligations. Second, one may be criticized for doing something prohibited." (DiMarco & Khalifa, 2022, 87). Both epistemic and moral considerations are relevant for criticism. Interestingly, DiMarco and Khalifa argue that "criticizing pursuits is central to the ways that non-scientists — politicians, entrepreneurs, activists, and the broader society — can hold scientists accountable" (2022, 86). Pursuitworthiness should not be framed merely in terms of pluralism that serves as a mechanism toward "the goal of adequacy and accuracy of scientific knowledge", as Šešelja and Straßer (2014, 3113) frame the issue. Instead, the possible futures of science that our actions (including decisions) affect and that should determine our judgment of pursuitworthiness need to be constructed more widely. The wider construction incorporates the perspective of non-scientists as well.[4]

A similar point has also been highlighted by Kitcher. In Kitcher's *well-ordered science*, scientific projects are analyzed through moral constraints and preferences of citizens (Kitcher, 2001). Kitcher argues that "Even when informed scientists and policymakers try to think broadly about research options and how they might promote the collective good [–], the visions are still partial and limited" (2004, 221). Kitcher suggests that "we need a place for a more synthetic view of the possible developments of our current sciences. Instead of jumping from one partial perspective to the next, we should create a space in which the entire range of our inquiries can be soberly appraised." (2004, 221.) Again, the possible futures of science need to be constructed in terms of scenarios that take into account more than the epistemic promise that scientists judge a research program to have. Scientific actions affect the future beyond the epistemic qualities of science and the actions should be evaluated accordingly.

This relationship between pursuitworthiness and the required richness of scenarios of the future can be clarified and systematized by relying on the definition of pursuitworthiness by Šešelja et al. (2012). They argue that

> "It is rational for Y to pursue X if and only if pursuing X is (sufficiently/most/etc.) conducive of the set of goals Z" (Šešelja et al., 2012, 53).

Different future scenarios are relevant for the pursuitworthiness depending on the nature of Y, X, and Z. Šešelja et al. argue that "[b]y interpreting each of the variables *X*,*Y*,*Z* in a different way, we can obtain different notions of pursuitworthiness" (2012, 53). Of course, not all interpretations are equally interesting.[5] However, we should not limit Z to epistemic goals. Šešelja et al. argue that "scientific inquiry as a part of the scientific practice may concern a broader spectrum of non-epistemic (or non-cognitive) goals as well, such as ethical, social or political goals" (2012, 64). Assessments of pursuitworthiness require that we take into account many aspects of possible futures on which the pursuit of a scientific research program may have an impact.

Thus far, we have seen how analyses of pursuitworthiness require a rich mapping of possible futures of science. The relevant considerations in the mapping range from epistemic to social values to moral constraints. We have noted that the richness of future scenarios would benefit our assessments of criteria of pursuitworthiness. However, there is a problem. It appears that there are fundamental obstacles in the estimating of possible futures of science when it comes to epistemic aspects of science. It seems that the difficult question is not how to get beyond the epistemic assessment of pursuitworthiness but how to get the epistemic assessment off the ground in the first place. In the next section, I introduce the problem following Shaw

[4] See, however, Douglas (2003) discussing the difficulties in balancing epistemic and moral considerations.

[5] For example, it might be that a person (Y) would fabricate data (X) to become famous (Z) but this is hardly an interesting analysis of pursuitworthiness.

(2022). The problem is that it seems that we should be able to predict the future of science in order to assess pursuitworthiness, but there are fundamental obstacles to predicting the future of science. This threatens the whole notion of pursuitworthiness. I sketch a solution to the problem by noting that (a) analyses of pursuitworthiness make assumptions about the dynamics of the development of science, and (b) scenarios of the future can be constructed by systematizing these assumptions. Given the scenarios, we can assess the merits of the criteria of pursuitworthiness even if we cannot predict the future.

3 Unpredictability of science

We have seen that analyses of pursuitworthiness are motivated by the need for an ability to respond to possible future changes and demands. These changes and demands may range from epistemic to those generated by changes in values and preferences. However, Shaw (2022, 104) points out that the contents of the criteria of pursuitworthiness are often explicated in terms of accepted theories. It appears that, in one way or another, the assessment of the pursuitworthiness of a research program depends on its relation to accepted theories and the current knowledge-base, i.e. the existing cognitive horizon (see Šešelja & Straßer, 2014 for the notion). In a sense, this is a natural way to assess pursuitworthiness, as we cannot rise above our current knowledge. However, this creates problems for the analysis of pursuitworthiness.

First, following Feyerabend (1993), Shaw notes that, in hindsight, some of the most pursuitworthy research programs, such as Galileo's, were not consistent with the existing cognitive horizon in any meaningful sense (2022, 105). It seems that every criterion of pursuitworthiness is too restrictive and could potentially cut off valuable research because the criteria require consistency with the existing cognitive horizon. This already questions the notion of pursuitworthiness, but there is an even more serious problem.

Secondly, and more fundamentally, the assessment of pursuitworthiness in terms of the existing cognitive horizon may be self-defeating. As Shaw summarizes,

> "[According to Feyerabend,] the very structure of pursuitworthiness judgments is problematic. Since any benchmark within a cognitive horizon used to assess a pursuit can become undermined by the pursuit itself or exogenous changes in a cognitive horizon, then any pursuitworthiness criteria may be self-defeating. The proper cognitive horizon to utilize would be the result of the pursuit, rather than a precondition for it. This entails that we can only determine that research is pursuitworthy in retrospect." (2022, 105.)

It appears that we can assess the pursuitworthiness of a research project only if we can predict the future of science. Predicting the future of science is impossible because we need to use the existing cognitive horizon to make those predictions while the future can change that very horizon. Because predicting the future of science is impossible, we cannot assess the pursuitworthiness of research programs, the reasoning concludes. In what follows, I will tackle this issue. I will argue that there is much more to the study of possible futures of science than attempting to predict the future of science. I will also argue that the assessment of pursuitworthiness does not depend on our ability to predict the future as long as we can systematically map scenarios of the future. While Shaw (2022) argues that there are contexts where aspects of the future of science can be predicted with some success, the arguments against the possibility of predicting the future of science are strong enough to suggest that we need a general approach to the estimating of the future of science. Such an approach enables us to estimate the future of different aspects of science in many different contexts (see §5). Even though it might be an empirical question where and to what extent the future of science can be reliably predicted, the general approach guarantees that we have a way of understanding the future of science in contexts where predictions are neither possible nor our goal (for example, when we wish to prepare for a crisis, see §2). The general approach also underlies the assessment of criteria of pursuitworthiness, as we will see in the next sections.

In addition to the argument concerning the potentially self-defeating use of the existing cognitive horizon, there are surprisingly few explicit arguments against the possibility of predicting the future of science.[6] The arguments seem to stem from the same insight: If we were able to predict a scientific discovery or innovation, then we would have already achieved the discovery or innovation This is a contradiction. Therefore, we cannot predict the future of science. There are two slightly different versions of this problem. First, if we were able to describe a radically new conceptual innovation of the future, we would 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 may have radical conceptual innovations in the future, it follows that our conceptual schemas are insufficient for predicting the future of science. 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.)

I will build my response to the issue on three observations. First, the arguments above assume that there will be somewhat radical crises and changes in our scientific knowledge in the future. To see this, notice that, for example, if we assume that our current theories and knowledge-base are rock-solid, then there is no risk of a self-defeating use of them in the assessment of the pursuitworthiness. I will not assess the strengths and weaknesses of the assumption concerning radical changes.

[6] A famous use of the unpredictability of science in wider contexts is Popper’s argument that “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.).

However, I wish to point out that it is still an assumption. We can have many other assumptions about science as well. For the very reason that we cannot predict the future, we cannot know which of the assumptions is ultimately true. However, we can systematize different assumptions and provide a map of future scenarios that these different assumptions generate (see the next section). This enables us to become aware of and prepare for different future possibilities. While the whole point of estimating the future of science depends on the view that science may change in the future, it is unclear how much it can change and why. A central aspect of the estimating of possible futures of science is to map how much science can change and for what reasons.

Secondly, the arguments 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. Even though it would undoubtedly be great if we knew what discoveries and conceptual innovations will be made in the future of science, it does not follow that there are no other important things that we can assert about the future of science. 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 to know what we wish to know (i.e., it can be expected that the experiments are good enough to inform us about the possible commercial use of fusion power) (e.g. Claessens, 2020, Ch. 12). Even if we cannot predict the future results of science, we can still assert important things about the future such as that some experiment is good enough to provide a valuable outcome (even if we are not predicting the outcome). The arguments against the possibility of estimating the future of science are therefore seriously limited in their scope when they focus merely on discoveries and innovations.

Thirdly, one crucial element in the arguments is the assumption that the goal of the study of the future is to predict particular events, such as discoveries and innovations. This appears to be a way too restricted stance towards future-oriented thinking. It is questionable, to say the least, in general (i.e., not just with respect to scientific discoveries and innovations), whether the accurate prediction of particular events in human society is the gold 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 futures research than predicting. As we will see in the next sections, enhancing understanding and challenging conventional thinking can be achieved by formulating many different scenarios of the future and the formulation of the scenarios has little to do with prediction. The arguments above do not prove that nothing interesting can be said about possible futures of science. In fact, given the main objectives of futures research, we have reasons to think that the focus on predicting of particular occurrences puts the cart before the horse.

First, notice that the occurrence of a particular event usually depends on the surrounding context. This is the very feature that makes them difficult to predict. As Staley points out, "events 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. It follows that knowledge about the possible context of the future is epistemically 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 might take place in the future.

In the next section, I argue that we can study the possible future contexts of science and we can do this by systematizing our views and theories of science. Philosophy of science is a rich source of different views and theories of science and, therefore, enables us to build rich taxonomies of possible future contexts of science. Understanding different possible futures of science will enable us to critically assess criteria for pursuitworthiness in the wider scheme of possible futures.

4 Scenarios of the future of science

Understanding possible future structures 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 a 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).

In this section, I suggest a method for formulating scenarios of the future of science that is in accordance with the basic tenets of futures research that were discussed in §2. I do not claim that it is the only or even the best method to formulate the scenarios – for example, one might adopt a more pragmatic approach such as the Delphi Method (see e.g., Bell, 2009, 261–272). However, the method is tailored for the purpose of using theoretical accounts of the dynamics of science as the basis of scenarios. In this way, the method is well suited to systematize and critically assess the assumptions that are intertwined with discussions of pursuitworthiness in the philosophy of science. Moreover, my claim in this paper is rather modest: I suggest that there is at least one way to formulate scenarios of the future of science that makes the scenarios relevant to criteria of pursuitworthiness. Perhaps in the future, there will be other – better – methods (or application of existing methods) but this does not affect the main argument of this paper.

I also wish to emphasize that the scenarios that the method in this section constructs are not scenarios of how the pursuit of some particular research program might proceed in the future, at least not directly. Rather, they are scenarios about possible future situations in science that affect what kind of research programs we might have or wish to have. The scenarios are situations for which a criterion can enable us to prepare for or react to. For example, a scenario of a crisis in an existing research program is a scenario where we will need to have a backup research program (as suggested by Šešelja & Straßer, 2014, see above). This means that the scenarios are also situations that a criterion of pursuitworthiness does or does not prepare us for. In this way, the scenarios are relevant for the assessment of pursuitworthiness (as I argue in detail in the next section) while not being in themselves scenarios of how the pursuit will proceed.

What kind of scenarios we can construct concerning the future of science? Imagine we did not have any ideas or assumptions about how science works and develops. In such a situation, the best strategy to begin the estimating of possible futures of science would be to use systematic, clear, and warranted accounts concerning the development of science. One place (among others) where one should look for such accounts is the philosophy of science. However, there are controversies and incompatible accounts in the field. Which one should we use as the basis for our estimations? I claim that we should not focus on choosing one account as the best but use as many as we reasonably can to build scenarios of the future.

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. However, 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 a 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 of science. These structures serve as the bases for more detailed pictures (scenarios) 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. In a taxonomy, we begin with a structure that describes a configuration of factors that produce and set boundaries for scientific development. We then add possible contents to the structure. Contents create the nodes (1, 1.1 … n.1, … n.n) in the taxonomy. These nodes are *scenarios* of the future.

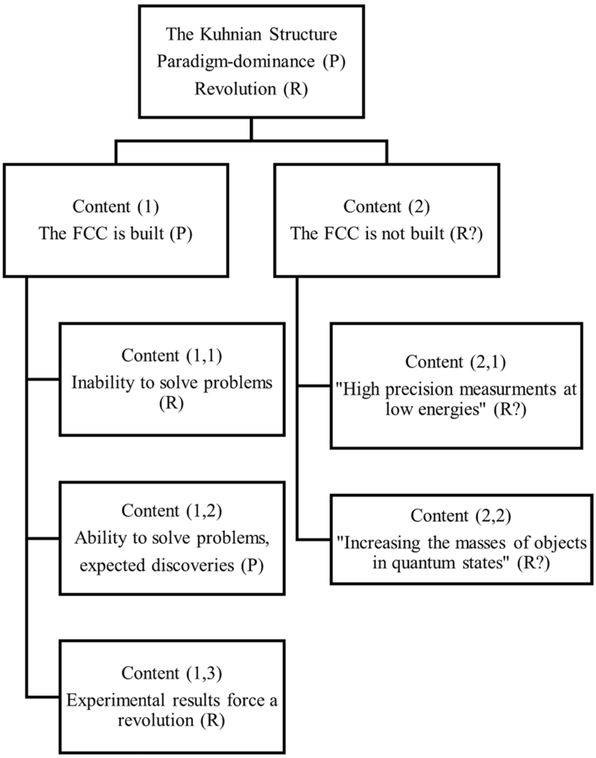
Let’s take a well-known theory of science as an illustration to clarify the approach.

In Kuhn’s 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 account defines a possible structure of the future of science. *If* Kuhn is right, 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 facing a crisis?

As an illustration (see Fig. 1), 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

Fig. 1 A Kuhnian taxonomy of possible futures of the FCC



"great" discoveries but in terms of excluding possibilities. This debate provides us with two main branches of scenarios. The first branch concerns whether the FCC is built. The second branch concerns the possible futures in the situation where FCC is built.

Take the first branch. Given the Kuhnian picture, either the paradigm set by the research with the Large Hadron Collider will continue as the FCC is built (contents 1), or the uncertainty about its ability to solve important problems leads to a decision to attempt some other approaches in physics (contents 2). Hossenfelder gives two examples of such alternatives "high precision measurements at low energies or increasing the masses of objects in quantum states" (2020, paragraph 11). Notice that in the Kuhnian structure, there is an ambiguity about whether the 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, Hossenfelder's suggestions stem from the current background of physics. As Toulmin (1970) pointed out, the absolute revolution vs. normal science distinction is a too restrictive interpretive tool. As a consequence, the Kuhnian taxonomy involves ambiguities that need to be removed. We come back to this issue below.

Next, take the second branch. 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, paragraph 13) 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. Each node (1)—(2.2) presents a scenario of a possible future of science. 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).

The Kuhnian structure of scenarios suggests that there might be a need for a criterion of pursuitworthiness that takes into account possible scientific crises (see also §5). As Shaw puts it "Revolutions, in Kuhn's sense, are allowable or even desirable events in scientific development on the anti-realist account" (2018, 87).[7] However, it does not imply that such crises are necessary. Moreover, we saw that the taxonomy has certain ambiguities about how a revolution might occur. These ambiguities should be resolved to analyze what we wish from backup theories that are expected to work in the case of crisis. For example, if a revolution requires serious contenders for the current paradigm, the backup theories have to be found with a criterion that is able to allocate extensive resources in a reliable matter.

[7] I thank an anonymous referee for pointing out Shaw's analysis which I take to support (indirectly) the argument in this paper.

Structural taxonomies are useful because (i) their theoretical bases are transparent, and (ii) they allow us to see at a glance what kind of futures are possible, given the theoretical bases. 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, (iii) they provide a rather direct feedback loop. Given that the interpretations of certain contents within a structure are dubious, we are able to assess the merits of the theory that serves as the basis for the structure and thereby assess the scenario. 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. Content (1,1) suggests that a mere lack of results leads to a revolution, whereas (1,3) suggests that revolution requires that there are further experimental results that do not fit the existing theories.

Without taking a stance with respect to the credibility of the Kuhnian structure or the specific suggestions concerning the contents, we can still see how we can formulate taxonomies of different scenarios about possible futures of science. All contents from (1) to (2,2) provide a description of a future situation and the course of events that allows one to move forward from the actual to the future situation. In other words, the contents create scenarios.

Moreover, and more importantly, the scenarios *enhance understanding* and *challenge conventional ways of thinking* which are the objectives of futures research (see §2). For example, Massimi explicitly argues that "particle physics community has long stopped (if ever did) following any Popperian method of hypotheses-testable predictions-falsification" (2020, paragraph 8) 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 (see above). This means that, while we perhaps cannot predict the future of science as there might be 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" (Massimi, 2020, paragraph 13).

Let's take another illustration. 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, but there are structural continuities. Given structural realism, we are able to build scenarios where disconnection-inducing revolutions do not dominate the landscape of theory change. The scenarios that structural realism provides suggest, contra to Kuhnian scenarios, that there might not be fundamental changes to be expected in our successful and mature theoretical science. Given the scenarios of continuity, criteria of pursuitworthiness that are motivated by possible fundamental changes are seen in a critical light. As Shaw puts it "Our conditional acceptance of realism or anti-realism, then, has important implications for theory choice and pursuit" (2018, 87).

A structural realist's taxonomy would be constituted by expansions of the following scheme (Fig. 2).

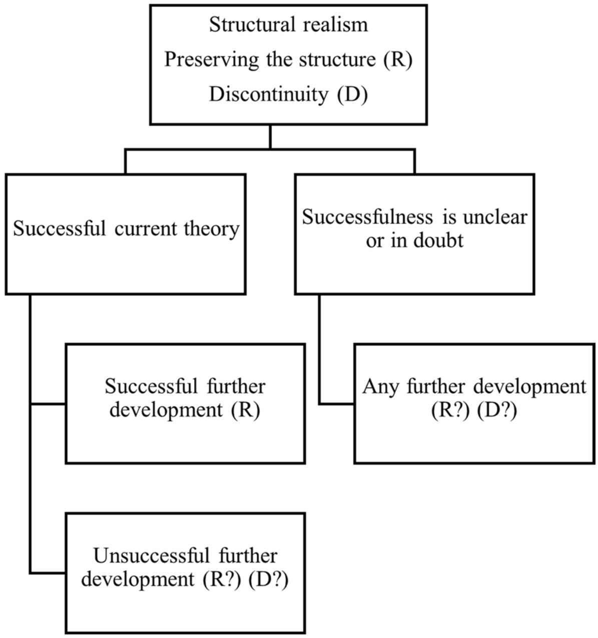


Fig. 2 A taxonomy of continuities in science on the basis of structural realism

A notable feature of the structural realism taxonomy is that it can provide clear scenarios only when we track paths of successful science.[8] 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, the structural features of a theory 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. We do not need to commit to the truth of theories behind taxonomies in order to make them serve their function of (i) describing the space of possibilities, and (ii) comparing the spaces of possibilities of different theoretical views. By comparing different taxonomies, we can see which scenarios are possible across taxonomies and which are unique to certain

[8] Structural realism is also restricted to rather formal sciences and seems to leave out less formal ones (Frigg & Votsis, 2011, 269).

theoretical commitments. For example, both taxonomies above allow that the FCC might provide further evidence for the current cognitive horizon. However, only the Kuhnian suggests that there might be vain experiments that force a revolution. The optimism toward current successful theories in structural realism leaves this scenario obscure. There are also meta-level agreements and disagreements between the taxonomies. For example, both taxonomies above suggest that it is unclear what will happen if we simply abandon the paradigm set by the research with the Large Hadron Collider.

Of course, not all taxonomies are not equally credible, plausible, or useful. In this paper, we cannot discuss in detail how to assess the merits of the taxonomies, as we focus on their use in assessing criteria for pursuitworthiness. However, we can note that there are ways to assess the merits. First, we can note that the credibility of an individual structure depends on the credibility of its theoretical basis. While these bases are difficult to assess, considerations of evidence from the history of science,[9] coherence, simplicity, and explanatory power are relevant. Moreover, we can evaluate the usefulness of individual structures in scenario-work on the basis of (i) how easy it is to collect evidence for individual contents within the structure, (ii) the number of possible contents (too few leave us blind but too many are difficult to manage), (iii) clarity of the contents (for example, we noted ambiguities in the Kuhnian taxonomy), and (iv) the consequences of different contents on our ability to act. While these practical criteria may leave out real future possibilities, they still enable us to direct our limited resources to the study of futures that we might achieve (or avoid). There are reasons to be optimistic about the assessment of the merits of taxonomies. As we wish to use many taxonomies side-by-side, the assessment of the merits does not even have to be able to provide a clear ordering of taxonomies.

5 Pursuitworthiness and scenarios of the future

As we saw, in the previous section, scenario-based futures research opens up many possible futures of science. This makes it natural to assess the criteria of pursuitworthiness from a novel perspective.

First, we can assess the scope of a criterion by explicating its assumptions concerning the dynamics of science and analyzing how many scenarios are compatible with these assumptions. For example, a criterion that relies heavily on the continuity of a current research program would be limited in its scope, as the criterion would not allow us to understand pursuitworthiness in the context of a scientific crisis. Limited scope is not necessarily a bad thing. For example, some scenarios might be such that, no matter what we do, we cannot prepare for them (for example, a scenario of a sudden and fundamental crisis). Some scenarios might be rather completely far-fetched or incredible. However, wide scope is, in principle, a valuable feature, as it enables us to act and react toward many possible outcomes. This

[9] See e.g. Donovan et al. (1988), Pitt (2001), Schickore (2011), Kinzel (2015), Kuukkanen (2018), Schickore (2018), Scholl (2018), McAllister (2018), Martínez-Ordaz and Estrada-González (2018), Tambolo (2018), Virmajoki (2018), Bolinska and Martin (2020).

matches the assumptions of *uncertainty*, *accountability*, and *humility*, and enables us to see multiple possible causal patterns and thus challenge conventional thinking (see §2). This brings us to the next point.

Secondly, we may ask how well a criterion enables us to act towards desirable futures. By a *desirable future*, we do not mean here a future that contains a scientific result we wish to achieve. Rather, we mean a future where science works, whatever this means. For example, Šešelja and Straßer's idea of robust science suggests that a desirable science is one with which a possible crisis can be faced in a resourceful way. Taxonomies of scenarios are maps that enable us to see possible future conditions and see how those conditions might be achieved. An overview of possible futures improves our ability to compare the desirability of different future outcomes and to see how different assumptions about science shape how we understand the notion of *well-working science*. For example, given the Kuhnian taxonomy (see the previous section), we may think that a science that generates a crisis in an orderly manner is a well-working science. Once we have identified desirable futures by constructing scenarios, we can assess to what extent a criterion of pursuitworthiness enables us to act towards these futures. For example, a criterion that relies heavily on the continuity of a current research program would not enable us to generate a crisis in an orderly manner as it would deny or remain silent about possible future crises.

Consider a more detailed illustration. 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 challenges the idea that "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" (Bedessem & Ruphy, 2019, 1). Bedessem and Ruphy's study is interesting from the perspective of the estimating of possible futures of science. It confirms that, while we are unable to predict future inventions or discoveries, we can still say interesting things about the processes and 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". (Bedessem & Ruphy, 2019, 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." (Bedessem & Ruphy, 2019, 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 (Bedessem & Ruphy, 2019, 2.).

Bedessem and Ruphy argue (2019, 4) that the 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 (Fig. 3).

This simplified taxonomy provides information about many scenarios concerning the occurrence of unexpected results. It tells us in what types 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 unexpected results can occur both in endogenous and exogenous problem situations.

Thirdly, the construction of scenarios of the future does not merely use philosophical accounts (or mere assumptions) as theoretical bases but also enables us to critically evaluate the philosophical accounts. There are ambiguities and blind spots which become visible when we construct scenarios of the future. For example, we noted, in the previous section, that the Kuhnian taxonomy contains ambiguities. In the Kuhnian taxonomy, there was the ambiguity of whether scenarios (2), where the FCC is not built, are scenarios of a revolution or not. Moreover, the taxonomy incorporates two scenarios of revolution with rather different causal structures. In scenario (1,1), a revolution occurs because certain problems cannot be solved. In 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. The ambiguities and blind spots in taxonomies suggest topics that require further historical and 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

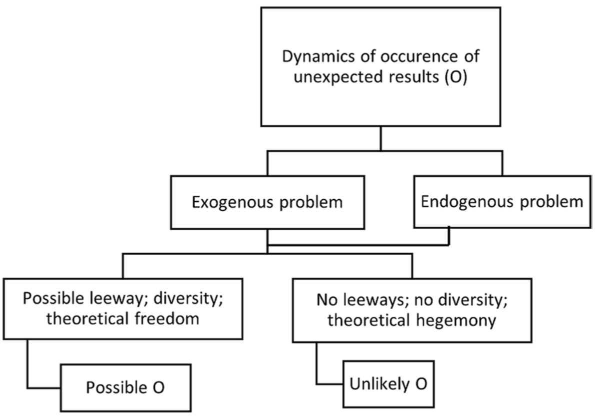


Fig. 3 A taxonomy of futures with unexpected results

in the history of science.[10] In this way, future-oriented thinking can open new perspectives and lines of research concerning the development of science. The difficulties in the estimating of the future of science reveal difficulties in our understanding of the development and workings of science.

To sum up, assessing the criteria of pursuitworthiness against the wider scheme of futures has many advantages. First, it enables us to manage and work with *uncertainty* by comparing the scope of a criterion to the scope of possible futures. Secondly, it enables *accountability* by revealing the scope of a chosen criterion. Whether we like it or not, by choosing a criterion, we steer toward particular futures. Thirdly, it enables us to maintain *humility* by (i) mapping scenarios where our choices of what to pursue, no matter how they are made, do not achieve what we wish for, and (ii) by revealing the intertwined nature of a desirable science and its possible futures. Fourth, the wider scheme of possible futures enables us to (i) identify multiple possible causal patterns, and (ii) to challenge and critically assess different accounts and assumptions concerning science.

Finally, there is one thing that needs to be clarified. In this paper, I have discussed how the merits of criteria of pursuitworthiness can be assessed in terms of how well they enable us to prepare for and react to different kinds of possible futures of science. As we saw in §2, criteria of pursuitworthiness are criteria for what to do in science in order to prepare for changes and achieve desirable goals. The prospects and limits of such criteria can be assessed only against a systematic mapping of possible futures that enable us to see what futures a criterion captures or ignores. This does not mean that we cannot assess the pursuitworthiness of a research program without an explicit criterion of pursuitworthiness in some contexts. For example, Shaw shows how predictive validity studies of "retrospective correlations between review scores of manuscripts submitted to journals or grants submitted to funding bodies with their down-the-road success" (2022, 106) indicate that decision-makers are able to assess the short-term potential of research programs. This means that people seemingly can predict the future of science in specific contexts and the mapping of possible futures is not needed in such contexts to approach the future. However, this ability to predict seems limited to short-term success and relevant only in the context of urgent science (Shaw, 2022). More fundamentally, the denial of the possibility of prediction is not at the core of the topic of pursuitworthiness. The need for a criterion of pursuitworthiness stems from two sources, as we have seen in §1 and §2: First, there might be research programs that could be successful but do not receive resources due to the lack of actual success. Secondly, some of these research programs might be useful to us in the future, given a possible future state or goal of science. That we can, in specific contexts, predict the success of a research program does little to address the worry that, in the future, we might face a situation where our current scientific path no longer delivers what we need (whether this

[10] 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 a 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.

is due to an internal crisis of a program or changes in social demands for science). To avoid these situations, we need an explicit criterion of pursuitworthiness that is built to take into account such possible future situations. Given that criteria of pursuitworthiness are formulated on the basis of what could happen, their merits can be assessed thoroughly only by comparing them against different futures scenarios. While we may be able to predict the future of science in some context, there is still a need for the mapping of a rich set of possible futures because these possibilities might include ones that we would like to prepare for deliberately. The two tasks are not competitors but serve different functions, one being more practical and the other more reflective.

6 The question of possible futures of science

In the previous sections, we have seen that analyzing pursuitworthiness should be shaped by explicit acknowledgment of *uncertainty* (many possible futures are in accordance with our understanding of the possible dynamics of science,) *accountability* (decisions affect many aspects of the future and should be made accordingly), and *humility* (the future may resist our hopes, needs, and demands) (see §2). The construction of scenarios of the future allows us to think about the future in terms of these assumptions. Moreover, the construction of scenarios on the basis of philosophical accounts of science allows us to enhance understanding and challenge conventional thinking and thus achieve the goals of futures research. Pursuitworthiness is best approached by locating the criteria of pursuitworthiness within a wider scheme of futures.

While the problem of pursuitworthiness, i.e., how to decide what to do in science, is at the core of our thinking about the possible futures of science, it is far from the only topic that motivates the study of the possible futures of science. There are countless ways in which the future of science and the future of conceptions of science affect society: What technologies are available (Joanny et al., 2020; Reding & Eaton, 2020), who are considered as epistemic authorities (e.g. Mede & Schäfer, 2020), how the human-nature relations are perceived (e.g. Allen, 2018), how to generate novel innovations (e.g. Kuhlmann & Rip, 2018), and so on. The ability to anticipate and prepare for changes in many areas of life depends on the ability to estimate the future of science. In order to understand possible futures of science, we need deep understanding about the nature and development of science and we have to critically analyze our conceptions of science. Philosophy of science engages with these issues and is, therefore, valuable in the estimating of possible futures of science.

Given this, it is surprising how little has been said about the topic of the future of science in the philosophy of science. Even though history and the philosophy of science have deepened our understanding of science, the topic of the future of science has not received widespread philosophical attention. Only scattered *philosophical* research on the topic can be found outside the discourse on pursuitworthiness (e.g., Popper, 1957; Rescher, 1978, 1999; MacIntyre, 2007; Shaw, 2018; Tromp, 2018; Brown, 2020; Shaw, 2022; Virmajoki, 2022). It seems that the theoretical and ethical problems associated with the topic of the future of science have prevented philosophy of science from making progress on this front. The theoretical problem concerns the unpredictability of science, and we have already tackled the issue. Instead of predicting discoveries and innovations, we can fruitfully study possible configurations of factors that produce and set boundaries for scientific development in the future. The ethical problem concerns the planning of the future of science in accordance with some presumed future. There, indeed, are 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).

It is a bit ironic that given the basic tenets of futures research (see §2), the ethical dangers and theoretical problems associated with the estimating of possible futures of science should not be seen as an external barrier to 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 problems, we should (i) map many possible futures, (ii) reflect the desirability of the possible futures, and (iii) provide a reality-check for our visions. Again, the construction of scenarios enables us to do exactly this by enhancing understanding of patterns of development and by challenging conventional thinking.

Given (i) the need to understand possible futures of science in many contexts, (ii) the basic tenets of futures research, (iii) the ability to avoid theoretical and ethical problems by following the tenets, (iv) the construction of scenarios as a tool to understand the possible futures of science, and (v) the rich resources that the philosophy of science provides for such scenario-work, the topic of the possible future of science is something that the philosophy of science should focus on more and thus make its relevance visible in a wide range of contexts. As we have seen, pursuitworthiness is one theme that carries such promise but is far from the only one.

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