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Abstract: In this article, I reply to the preceding articles by Naomi Oreskes, Chrysostomos Mantzavinos, Brad Wray, Sarah Green, Alexander Bird, and Timothy Lyons. These articles contain a number of objections and suggestions concerning systematicity theory, as developed in my book *Systematicity: The Nature of Science* (Oxford University Press 2013).

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Let me first express my sincere thanks to the six authors who spent considerable time and energy with *Systematicity*. Their contributions are extremely useful, because they give me the opportunity to spell out some things more explicitly, and address misunderstandings, for some of which I am responsible. This is the most intense dispute I have ever had since finishing the book. Furthermore, I strongly welcome their suggestions on how to develop systematicity theory further, and to apply it to and connect it with areas not yet addressed. However, as the papers are very rich in content, in this limited space I will not be able to do full justice to them, for which I can only apologize.

I shall first address those contributions that criticize systematicity theory (Oreskes 2017 and Mantzavinos 2016). Some of their points rest on misunderstandings that are caused by damaging obscurities in my book. Then I will turn to applications and possible extensions of systematicity theory and comment on them (Wray 2016, Green 2016, Bird 2017, and Lyons 2017). Of course, I welcome these contributions very much because they may lead to developments of systematicity theory that I had not anticipated. I shall begin with Professor Oreskes' paper. In the text below, simple page references are to the electronic version of the contribution of the pertinent author in this special issue.

On Oreskes' cases of facsimile science

I like Professor Oreskes' paper very much because of her detailed and informative case studies that serve as a concrete testing ground for systematicity theory.¹ However, her paper first left me somewhat confused because it appears to consist of two parts that do not seem to cohere well. However, discovering the source of that confusion was highly productive because it gave me the opportunity to identify a fundamental obscurity in my book of which I was not aware, and which I can now clarify.

Let me start with Oreskes' title, or more precisely, with the first part of her title: "Systematicity is necessary but not sufficient" (p. 1). What she means is that systematicity is not sufficient *for something to be a science*. In this generality, I could not agree more. As I have

¹ From my professional point of view, it is ideal when historians of science take a philosopher's theory seriously enough to confront it with historical examples. At least the philosopher can learn from that encounter.

had already many discussions about the status of systematicity regarding its necessity and sufficiency for science, I can now clarify the issue once and for all.² In order to do so, I shall investigate the application of the systematicity concept in four different situations, coming closer and closer to real science. I shall try to ascertain whether a correct diagnosis of the difference of science and non-science results.

I begin with an application of the systematicity concept that is not Oreskes' topic (rightly so), but that has generated confusion on other occasions. There are activities that are not directed at the production of knowledge but can nevertheless be conducted systematically. Take the activities of stamp collecting³ or loading a dishwasher: they can be performed quite erratically or rather systematically. No one would seriously claim that the latter mode would make loading the dishwasher a science, although jokingly, this may be a fitting comment in obsessive cases. The same holds for countless other cases of activities not intended to lead to knowledge, which can be performed more or less systematically. Note that increased systematicity need not always have a positive connotation. For instance, avoiding encounters with spiders may be fine, but doing this in an excessively systematic way may lead your psychiatrist to diagnose the phobic anxiety disorder arachnophobia.⁴ Clearly, in cases where systematicity is applied to situations that are not aimed at knowledge production, the result certainly cannot count as science. Expressed differently, systematicity is an instrument that can be applied in various areas, but if applied in areas far from knowledge and knowledge production, it will certainly not produce science, because it does not even produce knowledge. Thus, in order to approach our intended subject, we have to turn to cases in which systematicity is applied to knowledge and knowledge production, and investigate whether in these cases the result is science.

In a second step, we may note that in many of our everyday practices of producing knowledge we use systematic procedures (see Hoyningen-Huene 2013, p. 22). Think of a teacher who, returning from a field trip, counts the number of pupils who have reentered the school bus. She needs reliable knowledge that no one is missing and therefore counts the pupils—a systematic procedure—, instead of only guessing their number. Examples of a similar kind abound. Thus, the systematicity of everyday knowledge does not make it scientific knowledge. Again, this is a case where systematicity, in this case of knowledge, is not sufficient for science.

In a third step, we can focus on areas of knowledge production by professionals (thus we are not dealing with everyday knowledge), which have a high demand for reliability. Still, according to our general understanding, the product is unambiguously not science. I gave several examples in *Systematicity*, among them the Violent Crime Linkage Analysis System ViCLAS, market research, product development, and chess theory (Hoyningen-Huene 2013,

² In his review of *Systematicity*, Markus Seidel also criticized the lack of clarity in my main thesis regarding necessity and sufficiency for science (Seidel 2014, p. 36). – Please note that the last four words of the referenced sentence in the main text are a joke.

³ I encountered this as a putative counter-example to systematicity theory at the “Hoyningen Symposium – Systematicity: The Nature of Science” at Tilburg University on 22 Feb 2012 (https://www.tilburguniversity.edu/upload/94651d56-b78c-4eff-b8f1-f4a828105932_Hoyningen%20Symposium%20%282012%29.pdf); I may note that I have shortly dealt with stamp collecting in *Systematicity* (Hoyningen-Huene 2013, p. 210). Also Mantzavinos uses it, restricted to one dimension of systematicity, in his contribution—at least half-jokingly (p. 7).

⁴ 2017 ICD-10-CM Diagnosis Code F40.210: <http://www.icd10data.com/ICD10CM/Codes/F01-F99/F40-F48/F40-/F40.210> (accessed 8 Aug 2017).

pp. 23-24, 114-115, 121-123). Another impressive example is air crash investigation, now popular through a television series, in which specialists try to identify the cause of an air crash in an extremely systematic fashion, sometimes also including series of experiments. Still, the result is not science, although one may say that the specialists do proceed scientifically. The reason for this knowledge not to belong to science is that despite the very systematic procedures employed in it, especially with respect to the defense of knowledge claims, it lacks a sufficient degree of epistemic connectedness. Epistemic connectedness is an indispensable dimension of systematicity (dimension 6) for knowledge to count as scientific—or so *Systematicity* argues (Hoyningen-Huene 2013, pp. 118-121).⁵

Finally, in the fourth step we reach real science. In Chapters 2 and 3 of *Systematicity*, everyday knowledge is compared with knowledge uncontroversially established as scientific knowledge. *Systematicity* articulates an empirical generalization about these situations, which is its “main thesis”:

Scientific knowledge differs from other kinds of knowledge, in particular from everyday knowledge, primarily by being more systematic (Hoyningen-Huene 2013, p. 14).

However, this thesis is remarkably opaque with respect to the question whether the alleged higher degree of systematicity is necessary, or sufficient, or both, for knowledge to be scientific (in comparison with other forms of knowledge). Unfortunately, I did not remove this opaqueness in the ensuing discussion of the thesis in Chapter 2, although I expressed the intention to clarify and explicate the thesis (Hoyningen-Huene 2013, p. 21). The only way to assess the epistemic status of the thesis with respect to necessity or sufficiency of systematicity for science is to investigate the arguments that I provided in *Systematicity* for the thesis.⁶ These arguments were presented in Chapter 3 by comparing a variety of cases of *established science* with corresponding everyday practices, diagnosing a lower degree of systematicity of the latter in every case. This procedure establishes at best (short of overgeneralization) the *necessity* of systematicity for science, because all investigated cases exhibit higher systematicity of science. Oreskes has clearly seen this (pp. 1-2), but it would have been much better if I had removed the opaqueness of the thesis by explicitly stating that the arguments in Chapter 3 at best established the *necessity* of systematicity for science, and not its sufficiency. In *Systematicity*, one can infer the latter fact only by a sympathetic reading of Section 5.4, in which I introduce systematicity theory’s demarcation criterion. After the parade of the arguments supporting systematicity theory’s main thesis, the introduction of a (new) demarcation criterion only makes sense if systematicity is not by itself sufficient for science. However, it would have been overwhelmingly better if I had said explicitly that systematicity by itself is not sufficient for science, especially in the cases of pseudoscience, and this is why I introduced systematicity theory’s demarcation criterion (impressively abbreviated “STDC”).

Quite obviously, Oreskes has been misled by the missing clarifications and assumed that my thesis was that systematicity of knowledge is sufficient for it being scientific. Therefore, she presents her three beautiful case studies of “facsimile science” (p. 4),⁷ which convincingly demonstrate that the systematicity of some body of presumed knowledge does not

⁵ I shall come back to epistemic connectedness in my comments to Mantzavinos’ paper; see p. xx.

⁶ This is what I teach my students. If the epistemic status of a thesis by some author is opaque in some respect, investigate the arguments that the author adduces for the thesis. Often, from these arguments it can be inferred what the author exactly means by the thesis (or, at least, what the author *should* mean).

⁷ “Facsimile science” is a very fitting expression for what I had described as “enterprises that may resemble science, or even pose as science, but are not science” (see Hoyningen-Huene 2013, p. 199).

exclude the possibility of it being utterly pseudoscientific (pp. 6-17). In fact, I have used the case of homeopathy myself as an example of pseudoscience, and have claimed that systematicity theory is indeed capable of identifying it as such (Hoyningen-Huene 2016), contrary to what Oreskes claims: “Thus, I suggest that systematicity does not provide a demarcation criterion that permits us to reject homeopathy; other criteria must be invoked” (p. 9). The source of the disagreement is that Oreskes *does not use* STDC in order to judge homeopathy, contrary to what she announces at the beginning of her discussion of the case studies (p. 5). What does Oreskes do in her case study instead? First, she states that homeopathy is systematic (p. 8), but then clarifies her own analysis by stating that “the relevant question is not whether the latter [the candidate for pseudoscientific status] is systematic but whether the former [the reference science] is more systematic” (p. 9). Second, as this can apparently not be demonstrated, she concludes “*that according to the criteria of systematicity, homeopathy appears to be a science*” (p. 9, italics in the original). The arguments that systematicity theory cannot identify her other cases as pseudoscientific, creation science and climate change denial, proceed similarly.

As I said, the problem with Oreskes’ procedure is that before and during the investigation of her case studies, Oreskes *does not apply* STDC. Instead, she only asks whether in her cases the pertinent pseudoscience is less systematic than the pertinent reference science (and the answer may be in the negative). This, however, is not what STDC claims. STDC claims that the overall systematicity *increase* of a pseudoscience in a certain time interval is substantially smaller than the overall systematicity *increase* of the pertinent real science in the same time interval (compare p. 19).⁸ My claim here is that the application of the *real* STDC, and not just the comparison of systematicity degrees, does classify Oreskes’ extremely useful case studies as pseudosciences, as they should be. However, Oreskes also has an objection to this when, toward the end of her paper, she engages in a discussion of the real STDC, almost taking back all of her previous discussion by introducing the new paragraph by: “Admittedly, Professor Hoyningen-Huene has a response to this.” (p. 19) And indeed, my response is that it is not the comparison of *degrees* of systematicity that is relevant for the identification of pseudosciences, but the comparison of overall systematicity *increases*. However, Oreskes has also three objections to the real STDC, which I shall discuss now.

First, Oreskes claims that the identification of the appropriate reference science for homeopathy may be problematic, and she considers as possible reference sciences, apart from pharmacologically-based medicine, also the possibility of “ayurvedic or traditional Chinese medicine” (p. 19). However, this appears to be very far-fetched to me. In *Systematicity*, I claim “that the pseudosciences typically compete with *established sciences*” (p. 203, my italics), and it had therefore always seemed obvious to me that the reference science of homeopathy is Western allopathic scientific medicine. Note also, when discussing the case study of homeopathy earlier in her paper, Oreskes herself compares homeopathy with “conventional allopathic medicine,” apparently as a matter of course without mentioning any possible alternatives, which seems just right (p. 9).

Second, Oreskes claims that it “seems unlikely that we could agree on a quantitative scale for systematicity”, and even if we could, “it is not clear that the ‘true’ sciences would always win over the facsimile sciences” (p. 19). I myself have expressed skepticism about a possible quantification of degrees of systematicity (Hoyningen-Huene 2013, p. 21). However,

⁸ See Hoyningen-Huene 2013, pp. 203-207; please note that this description of STDC is extremely compressed.

non-quantitative comparative terms suffice for such comparisons, like in the familiar cases of temperature comparisons such as “today it is much warmer than yesterday.” The second part of Oreskes’ statement raises a more serious point. Her argument is that if in the time interval considered the pseudoscience starts in a very unsystematic state, its increase may be very steep, larger than the systematicity increase of the established science that may start at a state of already high systematicity. Accordingly, STDC would not classify the facsimile science as such.

I doubt that Oreskes’ argument is correct. How could a pseudoscience that begins at a point of low systematicity increase its overall systematicity? It must do so by increasing its systematicity in a least one of the dimensions. However, if it is a pseudoscience in Popper’s sense (i.e., not falsifiable), it has no empirical content, and thus cannot improve its explanatory power, nor its predictive power, nor its defense of knowledge claims, etc. If it is a pseudoscience in the sense that all of its claims are false (this is “the grounds upon which most mainstream scientists have rejected them”, p. 20), it is also impossible to improve its systematicity in any of the dimensions (I shall come back to this point below on p. xx). At least these special cases of pseudosciences cannot exhibit a steep increase of overall systematicity, and I doubt that any kind of pseudoscience can exhibit it. If in an area a competitor to an established science emerges, which starts with low systematicity but shows indeed a systematicity increase that is stronger than the systematicity increase of the established science, it is the emergence of a serious, yet underdeveloped competitor to the established science, rather than a pseudoscience. Why do we believe that astrology, homeopathy, or creation science are pseudosciences in the first place? Because they have produced very few cognitively worthwhile results at best, perhaps none at all, over the years. Instead, most of their epistemic activity consists of defensive moves, often directly or indirectly triggered by pressure from the pertinent reference sciences.⁹ Yet, the systematicity increase of the pertinent reference science indicates that cognitive progress is possible, thus blaming the specific approach of the pseudoscience for the failure, and not the intractability of the pertinent subject matter. Thus, I fail to see that the situation to which Oreskes refers and that would indeed be problematic for STDC, can arise.

There is another problematic aspect of STDC that Oreskes mentions but dismisses (p. 9 fn. 38), which also concerns the appropriate gauging of the systematicity increases of the putative pseudoscience and the established science. The direct comparison of factual systematicity increases may be unfair because the putative pseudoscience has received much less (financial) support than the established science. This argument has troubled me. However, Oreskes’ counter-argument seems to be that the given argument is weak in cases in which the putative pseudoscience cannot produce any convincing results. If the systematicity increase is zero, no gauge factor taking into account lesser support can increase it. This persuasive rebuttal, however, also invalidates Oreskes’ argument just discussed.

Oreskes’ third problem starts with the false presupposition that I assert “that the systematicity of ‘real’ science necessarily increases over time” (p. 20). Of course, I am aware of phases of stagnation in scientific development.¹⁰ My contention is that in situations in which

⁹ This was my impression of the pseudosciences that I had considered before I read Oreskes’ article. Her detailed case studies deepen this impression.

¹⁰ See, e.g., Hoyningen-Huene 2013, p. 183: “there may be episodes of stagnation of some scientific discipline or field caused by whatever factors, and clearly our hypothesis [of overall systematicity increase, P.H.] is not supposed to apply to such cases.”

we—scientists as well as philosophers—are convinced that certain fields are pseudosciences, the increase of systematicity of the reference science plays a major role. In light of such progress, we get the impression that any alternative enterprise that does not achieve what is in principle achievable and that poses as serious science, is a pseudoscience. Oreskes' own example of the phase of extraordinary science (Kuhn's term) in geology between the 1930s and 1950s may indirectly illustrate this point:

Theoretical accounts of mountain-building proliferated; one geologist asked if geology could even be considered a science when so many competing ideas, including the seemingly bizarre idea of moving continents, could “run wild” (p. 20).

In situations like this, a clear standard of what *is* scientific is missing: an account of mountain-building that exhibits clear systematicity increase over some time interval. Because of this missing standard, it is very hard to dismiss any new “wild” approach as clearly pseudoscientific (unless it involves angels etc.). This produces the impression of the dubitable overall status of geology as a science that the geologist's question quoted above testifies.

Concluding her discussion, Oreskes hints at two elements that should be included in an adequate demarcation criterion: “the domain of generativeness” and the proven falsity of claims. She thinks that these “additional factors” take “us out of the domain of systematicity” (p. 20). However, generativeness is a vital part of systematicity, as already indicated by the name of dimension 8 of systematicity: “the generation of new knowledge” (Hoyningen-Huene 2013, pp. 132-141). According to systematicity theory, the generation of new knowledge is fueled by an underlying “ideal of completeness”, dimension 7 of systematicity (Hoyningen-Huene 2013, pp. 124-132). Thus, “generativeness” is, if I understand it in the intended sense, an essential part of systematicity. Furthermore, I am somewhat surprised by Oreskes' contention that the falsity of (pseudoscientific) claims would be an “additional factor” that has not been taken care of by systematicity theory. On the contrary, dimension 4, “the defense of knowledge claims” (Hoyningen-Huene 2013, pp. 88-108), is entirely devoted to this question. There I state that “science appears to be the human enterprise that is most systematic in its attempt to eliminate error in the search for knowledge. It is thus evident that the defense of knowledge claims is an absolutely indispensable dimension in science's systematicity” (Hoyningen-Huene 2013, p. 89).¹¹ Clearly, a facsimile science producing only falsehoods will not increase its overall systematicity, because it not only fails in dimension 4, but as a consequence also in the other dimensions (predictions, explanations, etc.), and thus no dimension contributes to an overall systematicity increase. Thus, if there is a systematicity-increasing reference science, the verdict by STDC about a facsimile science producing only falsehoods is unambiguous: it is pseudoscience.

To summarize: Oreskes is right that in general systematicity is not sufficient for science, and I should have said so explicitly. Furthermore, a pseudoscience may look (and even

¹¹ There is a similar statement in fn. 41 on p. 10 of Oreskes' paper: “Hoyningen-Huene's nine criteria of systematic science (p. 27) say nothing about testability. Therefore, I believe it is appropriate to consider testability, along with falsifiability, to be distinct from systematicity.” It is distinct from but implicit in dimension 4 of systematicity, the defense of knowledge claims. The error elimination I refer to in the sentence from *Systematicity* (quoted above) presupposes testability and hence falsifiability. In the beginning of Section 3.4 on the defense of knowledge claims, I state explicitly that I do not want to take sides in the philosophical dispute about, roughly speaking, confirmation versus falsification (Hoyningen-Huene 2013, pp. 89-90). I therefore chose a vocabulary that was meant to be neutral with respect to this controversy, and spoke about “the defense of knowledge claims”. I regret that this choice seems to have been somewhat misleading.

be) more systematic than the corresponding everyday knowledge. However, the pseudoscience's striking lagging behind the reference science with respect to systematicity *increases* identifies it as a pseudoscience. It is only in uncontested situations that a higher degree of systematicity, encompassing all dimensions except perhaps predictions, of some body of knowledge is also a sufficient indicator for it being scientific.

On Mantzavinos' alternative to systematicity theory

Mantzavinos' dialogue that takes up so charmingly the very last sentence of *Systematicity* (p. 1), is much less a literary fiction than readers may assume. It is partly based on a real conversation between Chrys and me about systematicity. This conversation took place in May 2015 during a walk around the Acropolis, to which Chrys had invited me in the context of a talk I had given in the building of the old University of Athens right at the *hodós theorías*.

After a mostly accurate¹² dialogical presentation of his understanding of systematicity theory, Mantzavinos contrasts it with a sketch of an enterprise that he deems superior in its power to elucidate the nature of science, namely an investigation of scientific rationality (p. 11). More to the point, the goal is “a synthetic theory of scientific rationality, [...] encompassing both the individual and the collective level” (p. 13). What is meant by “a theory of scientific rationality” is, in my opinion, much clearer stated by the expression “scientific methodology” (p. 16). It concerns all the procedures and their elements that enable science to produce the kind of high quality knowledge for which science is valued. What is special about Mantzavinos' proposal is its explicit consideration of both the individual and the collective level. In the traditional understanding of methodology, science becomes a “one person game”, i.e., the traditional methodological directives address the individual scientist. Alternatively, some forms of group rationality have been discussed. Mantzavinos wants to integrate these two perspectives into a single coherent one, because in his view “[s]cientific rationality emerges spontaneously as a result of the different forms of interaction, both cooperative and competitive, among individual scientists organized in diverse institutional structures which make up what we call ‘science’” (p. 13). In fact, his project is heavily multidisciplinary, and he delivers only “a framework to be filled-in and advanced further by epistemologists [...]; by historians [...]; by sociologists, political scientists and economists [...]; and by constitutional lawyers” (p. 13). Of course, given the complexity of science, such a multidisciplinary “procedural conception of science” (p. 13) integrating the philosophical normative perspective with the empirical disciplines looks like an extremely promising project. However, in the given context I have two reservations.

¹² For instance, one of those inaccuracies is the characterization of the nine dimensions of systematicity as “classes of scientific activities” (p. 10, also pp. 4-6, 11-12); Mantzavinos apparently dislikes their denomination as “dimensions”. Some of the dimensions may indeed be described as activities, but this certainly does not hold for the ideal of completeness (dimension 7), and it is not a good description for epistemic connectedness (dimension 6). Another reason for their denomination as dimensions is something not even hinted at in *Systematicity*. One can define a nine-dimensional space, spanned by the nine dimensions of systematicity, and each axis represents all the “values” that the respective specific systematicity can take on. Then any sub-discipline of science at a particular time is represented by a point in this space. This space represents the relationships between disciplines, mainly by their distance, and the historical change of individual sub-disciplines, by the motion of the respective point in time. I have developed this space in Hoyningen-Huene 2018, in press-b. Another inaccuracy is that I certainly do not claim that “to stop asking the ‘What is X?’-question altogether is equivalent with stopping philosophy” (p. 10). However, both of these points are of no consequence for the present debate.

First, it is difficult to evaluate the sketch of a theory laid down on a few pages as a possible alternative to systematicity theory that has been worked out at book length.¹³ Philosophy is full of wonderful programs and ideas, but the litmus test of any program or idea is the concrete elaboration, considering its detailed parts and consequences, historical presuppositions, etc., etc. When I started working on systematicity theory in the mid-1990s, its idea looked grandiose and comparatively easy to me, and only in the course of working it out I realized how problematic it was and how large the number of unforeseen problems was that I had to confront. Therefore, a fair comparative evaluation of competing conceptions of the nature of science (or of any subject in philosophy) should involve candidates that have been worked out to a similar degree of detail. Before this elaboration, it is virtually impossible to decide in which respects an alternative to systematicity theory is superior to it, or whether this alternative could be used to amend systematicity theory, or whether its content is in the end virtually contained in systematicity theory, etc.

Second, I do have serious doubts that Mantzavinos' project will fully reach its goal of elucidating the nature of science, at least with the ingredients so far presented. The reason is that I fail to see that the project really focusses on *scientific* rationality (in the sense of a scientific methodology). Instead, the project so far sketched will explicate the methodology of *any cognitive enterprise that seeks to achieve reliable knowledge*. For instance, when Mantzavinos claims that "acceptance or rejection of beliefs on the basis of evidence" are "the cornerstones of scientific rationality" (p. 12), then this characterization is certainly not exclusively appropriate for science. The same holds for other characteristics of science that Mantzavinos adduces:

- the synthetic character of the enterprise, "encompassing both the individual and the collective level" (p. 13);
- the "enterprise is embedded in certain practices employed by the participants and unfolds according to normative standards that have emerged in a long evolutionary process of trial and error" (p. 13);
- "the fallibilistic attitude" (p. 15) – Mantzavinos himself mentions that it "can be applied to all areas of human activity" (p. 15);
- The "*appropriate institutionalization of the possibility of criticism*" (p. 17, italics in the original);

Other professional knowledge-seeking enterprises have the same characteristics, without resulting in *scientific* knowledge, although the knowledge gained may be extremely reliable. I have mentioned some examples of such non-scientific knowledge seeking enterprises above (p. xx). To take up Oreskes' line of criticism: Mantzavinos' project will delineate something that is necessary for science, but not sufficient.

From the standpoint of systematicity theory, Mantzavinos deals with two dimensions of science that are indeed central: the defense of knowledge claims (dimension 4) and critical discourse (dimension 5); I have emphasized the centrality of dimension 4 in *Systematicity* several times (e.g., Hoyningen-Huene 2013, pp. 89, 210). Mantzavinos proposes to integrate these two dimensions in one multidisciplinary package. When he will have finished his project, it may well be that I prefer his results to what I have written on these two dimensions in

¹³ I had the same problem with the theory sketches of Carrier 2015 and Scholz 2015 that were put forward as potential alternatives to systematicity theory (see Hoyningen-Huene 2015).

Systematicity. However, I would insist that a more encompassing characterization of science should also include discussion about the other seven dimensions, i.e., descriptions, explanations, predictions, completeness, representations, etc. In particular, I would insist that a proper characterization of science should be capable of telling the difference between reliable *scientific* knowledge and equally reliable *non-scientific* knowledge. I fail to see that Mantzavinos has resources in his approach to tackle this question.

On Wray's Continuity Thesis

Brad Wray discusses the question of whether or not scientific knowledge is continuous with common sense (or "everyday") knowledge. In the literature, there are highly diverging views on this question, ranging from Einstein's view of science as "nothing more than a refinement of everyday thinking" to the idea of a fundamentally different character of science due to its use of the scientific method. Two facts about science seem to be undeniable. On the one hand, there are areas of science that are extremely remote from everyday knowledge, both with respect to their content and with respect to their generation. For example, think of the discovery of gravitational waves radiated from the inspiraling of two black holes (Abbott 2016). On the other hand, whenever a science emerges, it never emerges out of nothing, thus there are always continuities with what went on before. One can therefore expect that a discussion of the Continuity Thesis will unavoidably exhibit some ambiguities, and this is the case in Wray's paper. More specifically, there are four distinct sources of ambiguity and related vagueness that I found troublesome.

First, what is it that the Continuity Thesis claims to be continuous, or its opposite thesis declares to be discontinuous? In other words, what is the subject matter of the Continuity Thesis? The candidate most often mentioned is

- "knowledge", more precisely "scientific knowledge" versus "common sense knowledge" or "the layperson's knowledge" (pp. 1-5, 7, 9-10, 13).

However, other candidates include

- "scientific reasoning" versus everyday "thinking" or "reasoning" (pp. 2, 3, 8, 10);
- "modes of investigation" (p. 2), which is perhaps the same as "the way scientists approach inquiry" versus "the approaches of the layperson" (p. 9), different "ways of knowing" (p. 9), and "methods employed by scientists and laypeople" (p. 13);
- "scientists' understanding" versus "the laypersons' understanding" (pp. 8-9);
- the "sense of evidence" (p. 3);
- the valuation of "simple hypotheses" (p. 3);
- perhaps also "world views" (pp. 7-9) or "conceptions of the way the world is" (p. 8).¹⁴

The second source of vagueness of the Continuity Thesis concerns the nature of the continuity, or of the discontinuity, respectively. The continuity is expressed in various ways:

¹⁴ I am not sure whether it is legitimate to include "world views" into the list of candidates for the subject matter of the Continuity Thesis (or its opposite); therefore I say "perhaps". The reason is that Wray uses the fundamental difference between the scientific and the layperson's world views as *evidence against* the Continuity Thesis (p. 7), so the Continuity Thesis should have a subject matter different from what world views are. However, aren't world views consisting of beliefs (or knowledge) about the world, so of something that is the subject matter of the Continuity Thesis?

- “scientific knowledge is *just an extension* of common sense” (W21, my italics);
- “scientific knowledge is *not fundamentally different* from the layperson’s knowledge” (p. 2);
- “the difference is *merely a matter of degrees*” (p. 4, emphasis in the original).

However, Wray also affirms a discontinuity between common sense and science in various ways:

- “the *gap* between science and common sense could become *quite large*” (p. 6, my italics);
- “scientific knowledge is *fundamentally different* from the layperson’s knowledge” (p. 13, emphasis in the original; similarly pp. 3-4, 7);
- “*qualities* of scientific knowledge that are *seldom or never associated* with layperson’s knowledge” (p. 4, my italics);
- “the mode of investigation [...] is *different in kind*” (p. 2, my italics; similarly p. 4);
- there are “cases where scientific knowledge “directly contradicts common sense knowledge”” (p. 5);
- “science and everyday knowledge as *distinct classes*” (p. 5, my italics).

The coexistence of these statements both for continuity and discontinuity points to a relationship between continuity and discontinuity that is more complicated than simple opposition. Scientific and everyday knowledge seem to exhibit, in the very same situation, continuity *and* discontinuity. This is indeed what Wray wants to express.

Wray states that here are cases “in which a difference in degree constitutes a difference in kind” (p. 4).¹⁵ This implies that the *discontinuity* (between whatever the candidate is) is generated by a *continuous* change of something.¹⁶ This in itself is not troubling because there are clear cases where the continuous change of one parameter of a system indeed leads to a discontinuous change of another one (without or with sharp borders of the second parameter). Think of the change of color due to a change of wavelength, or of a phase transition due to a change of temperature. However, this induces a third kind of vagueness: why should this situation, if one can persuasively argue for its existence in science, be supportive of the *Continuity Thesis*? Wouldn’t it be equally justified to state that the admission of discontinuity is evidence for a *Discontinuity Thesis*? If one denies the latter, what kind of discontinuity would justify a *Discontinuity Thesis*?

The fourth kind of vagueness concerns the intended scope of the *Continuity Thesis* (or its opposite). Does it unequivocally hold, or not hold, for all the sciences, or should one distinguish among the sciences? For example, even if one thinks that highly mathematized knowledge about gravitational waves, i.e. about ripples of spacetime, is different in kind from everyday knowledge, could one not hold the view that some parts of nursing science are at least similar in kind to knowledge of nurses before the advent of nursing science? Aren’t the

¹⁵ Hopefully to the amusement of the reader, this idea was a centerpiece of Friedrich Engels’ *Dialectics of Nature*, called “the law of the transformation of quantity into quality and vice versa”: Engels 1962 [1925], pp. 348ff., English translation https://www.marxists.org/archive/marx/works/download/EngelsDialectics_of_Nature_part.pdf, Chpt. II Dialectics. This “law” even made it as a “causal mechanism” into the *Proceedings of the National Academy of Sciences*: see Carneiro 2000.

¹⁶ I am not even entering mathematics: many discontinuous functions (in the mathematical sense) can be approximated to any degree of accuracy by continuous functions.

sciences too diverse such that the relation between everyday and scientific knowledge cannot be captured by one general thesis regarding continuity or discontinuity?

Therefore, I cannot say where I stand with respect to the Continuity Thesis. It depends crucially on how the thesis is spelled out more precisely. However, all of this seems to be of lesser concern when it comes to Wray's central question regarding the epistemic authority of science. Here, I fully agree with him "that it is the greater *systematicity* of science that grounds the epistemic authority of scientists" (p. 13, emphasis in the original). This insight, it seems to me, can be established without entering the morass of the Continuity Thesis.

On Green's considerations of science and common sense

Green's contribution is especially welcome because she establishes more explicit connections between systematicity theory and debates in science education. Clearly, there is an overlap area between systematicity theory and science education. Whereas the dynamical version of systematicity theory (Sect. 5.1 of Hoyningen-Huene 2013) claims something about the transition from pre-science to science in disciplinary terms, science education tries to understand and support the transition of individuals from pre-science to science. It is highly plausible that the two areas may learn from each other. However, for increased usefulness of systematicity theory for other areas, including science education, Green pleads for "a more contextualized account of systematicity" (p. 17), which I can support without the slightest reservation. Of course, the discussion in *Systematicity* often proceeds at a rather abstract level, and whether this discussion is really worthwhile can only be probed at a concrete level, applying and therefore contextualizing systematicity to the situation at hand. I thus fully agree with Green's statement "that the notion of systematicity would be more useful for science (and arguably also for philosophy of science) if made more concrete and more contextualized" (p. 17). It is also correct to state that *Systematicity* often considers the relationships between certain results without giving much attention to the processes that produce these results (e.g., p. 3). However, only the detailed investigation of those processes will disclose "whether increased systematicity really explains the most salient features" of the respective development (p. 20). This holds especially for the relation between science and common sense, which is an important subject matter of science education. It is very gratifying that systematicity theory is of some help in elucidating the cognitive processes that lead from common sense to science, as Green states on several occasions (e.g., pp. 16, 18, 21).¹⁷ Given my general appreciation of Green's project, two remarks will suffice as a comment. These remarks are intended to remove potential misunderstandings.

First, I am not particularly happy with the summary of *Systematicity* as "Hoyningen-Huene's thesis is that scientific knowledge can be characterized as an *extension* of everyday knowledge" (p. 2, my italics). For Green, this is apparently a version of the continuity thesis because she writes afterwards: "Hoyningen-Huene emphasizes continuity between everyday knowledge and scientific knowledge" (p. 2). However, this is as misleading as writing "being an adult is an extension of being an adolescent" because there is continuity between these two developmental stages. The term "extension" does not appear to be helpful in these contexts. Furthermore, the distance between common sense knowledge and scientific knowledge varies

¹⁷ On p. 18, Green mentions the potential of systematicity theory "as a tool for the comparison of standards in specific situations and in different traditions." This potential has been used in *International and Comparative Education* by Pietraß 2017.

tremendously with subject matter. Some parts of science are immediately intelligible for the educated layperson whereas other parts are utterly incomprehensible (compare Hoyningen-Huene 2013, p. 194). Thus, my criticism of the continuity thesis as expressed above (in my contribution to Wray's article) also applies to Green's treatment. Basically, all I claim is that the genesis and further development of science is piecemeal, and not by giant steps (like the current understanding of biological evolution).

Second, I am not particularly happy with the statement, ascribed to me, that "common sense is sometimes a 'victim of science', resulting from the incumbent increase in systematicity" (p. 2-3, similarly pp. 6, 8, 14; similarly "giving up common sense" on p. 20). This sounds as if the whole of common sense is replaced by something scientific, which can be legitimately doubted (as Green does: e.g., pp. 6, 12). However, I only claimed that in some situations "common sense *notions*" become a victim of science (Hoyningen-Huene 2013, p. 194, new italics), thinking, for instance, of the common sense notion of simultaneity being abandoned by the special theory of relativity (Hoyningen-Huene 2013, p. 192). I therefore think that my claim is much more modest than Green assumes, and more plausible.

Clearly, if one wants to learn something about common sense, one should not turn to *Systematicity*. The concept of common sense is hardly developed there, and the book is silent about processes of common sense cognition. At the time of writing the book, I thought it sufficient to present a few simple examples of common sense in order to direct the reader's attention to the contrast to science, on which I was focused. However, developing this line of thought further and fruitfully applying it to science education, as Green does, will require going far beyond the primitive sketch of common sense knowledge that *Systematicity* offers.

On Bird's case study and his hypothesis (H)

Naturally, I like Alexander Bird's contribution very much because it provides a wonderful case study that shows how systematicity made clinical medicine a science. It seems plausible that there are many other cases like this to be found in the history of medicine. I had myself envisaged a study of the emergence of nursing science, which happened in the 1990s. However, since I could not find any comprehensive study of the emergence of nursing science, I could not realize the task. Therefore, I am very happy that Bird produced such a convincing case study from the history of medicine, which so nicely illustrates a core claim of systematicity theory. In addition to the case study, Bird proposes a hypothesis (H) that aims at broadening systematicity theory (pp. 1-2), and he criticizes my use of the concept of knowledge. Let me consider these subjects in turn.

Bird proposes a hypothesis (H) that claims the *necessity* of systematicity for the characteristically reliable hypotheses of science (pp. 1, 4, 13-15). The reason is that ordinary cognitive capacities exhibit far-reaching limitations such that we are liable to various sorts of bias when straightforwardly applying these capacities. Applying systematicity is the only way for us to overcome such biases and to produce knowledge that is indeed reliable (pp. 2, 12-13). Perhaps one could add that systematicity is especially important when non-observable entities are involved in the respective knowledge claims. However, Bird is aware that a comprehensive argument for his hypothesis (H) is beyond the reach of his paper (p. 4). I have no reason to object to Bird's hypothesis. It centers on dimension 4 of systematicity theory, the defense of knowledge claims. This is all right, because dimension 4 is essential to systematicity theory. However, opponents of systematicity theory might object that a systematic probing of

knowledge claims in order to overcome bias is also part of other positions in the philosophy of science. Therefore, the objection continues, Bird's case study and his hypothesis (H) support indiscriminately various philosophical positions, and systematicity theory is only one of them. A counter-argument to this objection should probably show that, in what Bird describes under the rubric of systematicity, more than just dimension 4 is involved. This is also what Bird suggest for a comprehensive argument for his hypothesis (H): it "would require careful consideration of all nine dimensions of systematicity [...] in order to show how each, in an appropriate scientific context, is necessary for the reliability of the means by which we come to believe propositions in that context" (p. 4; see also p. 15). It would be interesting to see detailed case studies in which the interplay of different dimensions of systematicity becomes visible, especially at critical junctions of scientific development.

Now I come to Bird's criticism of the concept of knowledge that I am using in *Systematicity*. Bird objects that it lacks the crucial component of truth (p. 3), and he is right. I state explicitly in *Systematicity* that "it would be quite dangerous to presuppose that what counts as scientific knowledge today is literally true. [...] Rather I use [the word knowledge] in the sense of a body of belief ... that is well-established, widely held in the relevant community, not regarded as tentative or falsified" (Hoyningen-Huene 2013, p. 21). I further claim that this is also the way scientists use the word 'knowledge', with which Bird disagrees (p. 3). However, it is important to note that Bird's disagreement with me is not a sterile argument about the correct meaning of the word 'knowledge'. For Bird, the point is important because without the component of truth, "one cannot use knowledge to link science and systematicity in the way that I do" (p. 3). Thus, the truth component of knowledge plays an essential role in Bird's argument.¹⁸

Let me begin the discussion by asking why Bird and I disagree about the way scientists use the word 'knowledge'. Bird thinks that "scientists will call 'knowledge' the body of belief that they accept as being non-tentative, but that's because they think that those beliefs are true" (p. 3, fn. 4; similarly p. 4). I think that Bird is right in a great many cases. For instance, why is it that "the scientific consensus is that birds are a group of theropod dinosaurs that evolved during the Mesozoic Era"?¹⁹ Or why is it that the "geoscientific community accepted plate-tectonic theory after seafloor spreading was validated in the late 1950s and early 1960s"?²⁰ Clearly, this is so because the members of the pertinent communities hold the respective beliefs to be true. However, contrast that with the general acceptance of the Schrödinger equation as the foundation of computational chemistry.²¹ Is it because all quantum chemists believe that the Schrödinger equation is true? Some may do, but others certainly

¹⁸ I may note that in the background of this dispute, there is a longer controversy between Bird and me about scientific realism and scientific antirealism (see, e.g., Bird 2008 and Hoyningen-Huene 2008). However, I will not refer to it. In *Systematicity* as well as in this paper, I try to remain neutral with respect to this controversy. Still, there is no denying that Bird's willingness as well as my reluctance to embrace truth in the given context may be influenced by the pertinent philosophical leanings.

¹⁹ https://en.wikipedia.org/wiki/Origin_of_birds (accessed May 4, 2017) with reference to *The New York Times*, March 28, 2016: <https://www.nytimes.com/2016/03/29/science/dinosaurs-birds-evolution-american-museum-of-natural-history.html>. – Note to students: reference to a Wikipedia article is legitimate in this case because we are dealing with a well-known and uncontroversial fact; the same holds for the next footnote.

²⁰ https://en.wikipedia.org/wiki/Plate_tectonics (accessed May 4, 2017).

²¹ See, e.g., Lewars 2016, pp. 2-3.

don't.²² Those who don't typically refer to the Schrödinger equation as a "model", implying by this choice of words that the equation does not represent anything real in nature. In this view, the Schrödinger equation may one day be replaced by another equation that is empirically more successful, but equally a model not representing anything in nature.

The upshot is that both Bird and I have overgeneralized, or: we are both partially right. There are cases in which scientists accept "a body of belief ... that is well-established, widely held in the relevant community, not regarded as tentative or falsified" as knowledge, *because* they indeed believe it to be true. There are other cases (like the Schrödinger equation) in which at least a considerable fraction of scientists accepts a well-established theory, although they think of it as *merely* being a model. Why do they still accept it, although they don't believe it to be even approximately true? Systematicity theory's answer is, because this belief ranks higher in several dimensions of systematicity than its competitors do, especially regarding descriptions, explanations, predictions, defense of knowledge claims, and epistemic connectedness. What does it mean to say that these scientists "accept" a theory that they would qualify as merely a model? It means that they are using it, without reiterated justification, as a legitimate scientific means, for predictions, explanations, technical applications, etc. In fact, they use it *in their scientific practice* in the same way as their colleagues do, who think that that belief embodies truth about nature.²³ The important question is now whether or not 'knowledge' of the latter sort, i.e., "with no implication of truth", can be used "to link science and systematicity" in the way Bird does; Bird denies this (p. 3).

First, according to Bird the "link between science and knowledge" requires more than science's aiming "to produce an accepted body of belief—after all, that aim could be achieved by mass hypnosis" (p. 4). Instead, one should "say that science aims at knowledge in the sense of (something like) reliably generated true belief" (p. 4). However, there is more than the alternative between knowledge as an accepted body of belief (*whatever* the means of acceptance are) and knowledge as (roughly) reliably generated *true* belief. In between is the possibility of belief that ranks high (higher than alternatives) on the relevant dimensions of systematicity including, of course, the defense of knowledge claims, and that is *therefore* accepted in the community. At least for some fraction of scientists it is not the case "that being systematically produced will make a claim more likely to be true (which is *why* it is more likely to be accepted)" (p. 4). Those scientists who, for instance, believe that Schrödinger's equation (and related equations) are merely models and who work in the respective area, will try to increase the systematicity of their models without hoping that the next model will "likely be true".

Second, we may ask whether a concept of knowledge not implying truth would endanger Bird's hypothesis (H). I think that this is not the case. H states:

The reasoning processes by which we come to accept the propositions characteristic of science are reliable, and so knowledge-producing, only if they are systematic (p. 4).

According to Bird, the necessity of systematicity for science is rooted in the fact that

²² I shall come back to this point in my reply to Lyons (p. xx). Anecdotal evidence suggests that more theoretical chemists with a background in chemistry prefer the realist interpretation than those with a background in physics.

²³ I shall come back to this presumed fact that behaviorally it does not make a difference whether a scientist interprets the theory she works with realistically or not, in my comment to Lyons (p. xx).

it is systematicity that enables inquiry to overcome the limitations of our ordinary cognitive capacities (p. 2).

However, even if systematicity does not lead to beliefs for which it is likely that they are true, it will still enable us to overcome our cognitive limitations, for instance in terms of all sorts of biases. Therefore, I fail to see that the validity of Bird's hypothesis (H) depends on a realist view of knowledge, i.e., one in which knowledge implies truth. The less ambitious concept of scientific knowledge that I proposed in *Systematicity* suffices.

Finally, Bird states that "it makes a lot of sense to say that science aims at knowledge in the sense of (something like) reliably generated true belief" (p. 4). Taken by itself, which means especially: isolated from the thesis of *epistemological* scientific realism, this is also the central claim of Timothy Lyons' "Socratic scientific realism". I shall therefore treat this statement in the context of my discussion of Lyons' paper, to which I am turning now.

On Lyons' Socratic scientific realism

Briefly, in my understanding Lyons' thesis is this: systematicity theory is OK, but it needs to be supplemented by Socratic scientific realism (pp. 6-7). Socratic scientific realism is "a non-epistemic, purely axiological scientific realism" (p. 1).²⁴ It states that science *seeks* truth, more precisely, "a particular subclass of true claims" (pp. 2, 7),²⁵ without implying that it in fact *reaches* truth, fully or approximately (p. 6), hence the emphasis on the non-epistemic character of this kind of scientific realism. Lyons claims that systematicity theory needs this supplement of Socratic scientific realism "in order to both explain and justify key dimensions of systematicity in science" (p. 2, similarly pp. 6-7, 9, 24, 26), otherwise, these crucial dimensions of systematicity hang in the air.²⁶ Lyons also considers the possibility that it may not even be necessary to *supplement* systematicity theory with Socratic scientific realism, because there are several statements in *Systematicity* that one can plausibly read as implicit endorsements of Socratic scientific realism (p. 27). In that case, Lyons would only make explicit what is already implicitly contained in systematicity theory.

I have much sympathy for Lyons' enterprise. In my work on Thomas Kuhn's philosophy of science, I had criticized Kuhn regarding his cognitive values in a very similar way as Lyons criticizes me regarding key dimensions of systematicity. I claimed that Kuhn had left two important problems unsolved, namely the *explanation* of the historical change of cognitive values held in one particular scientific community, and the *explanation* of the differences between the cognitive values held in different communities (Hoyningen-Huene 1992, pp. 497-499). Furthermore, I claimed that the sought explanations had to refer to some ultimate goal of science, which was an adaptation of Hempel's idea that a *justification* of Kuhn's cognitive

²⁴ In its general tendency, Lyons' Socratic scientific realism appears to be quite similar to Feyerabend's "normative realism". Feyerabend's position does not concern "a factual issue", but "is an issue between ideals of knowledge" (Feyerabend 1958, reprint pp. 33-34). For a detailed discussion of Feyerabend's normative realism and its background, see Oberheim 2007, pp. 188-192.

²⁵ Lyons characterizes this particular class of true claims as "experientially concretized truth" (pp. 9ff.). At this point, I do not have to get into the details of this characterization; I shall come back to it on p. xx.

²⁶ At least in spirit, Lyons' suggestion agrees with the line of criticism put forward by Mariam Thalos in her excellent critical review of *Systematicity* (Thalos 2015, esp. pp. 354-357). Thalos stresses that "Aristotle's notion [of science] is meant to draw a divide between reasoning that is oriented towards truth and reasoning that is oriented towards something else" (p. 354). She contends that systematicity theory needs this distinction, too, in order to succeed in distinguishing science from activities with practical purposes (p. 356).

values had to refer to an ultimate goal of science. Kuhn's cognitive values, properly concretized, would be instruments for the realization of the ultimate goal of science. As the suitability of these instruments depends on the particular disciplinary and historical scientific situation, the values could differ with respect to disciplines and times. With respect to the ultimate goal of science, I had accepted in my paper Hempel's proclaimed goal of science "to formulate an increasingly comprehensive, systematically organized, world view that is explanatory and predictive" (Hempel 1983, p. 91). However, I have always thought of Kuhn's cognitive values as specific regulatory statutes for the idea of truth, when concretized for a particular historical situation.²⁷ As I ascribed to Kuhn an epistemological antirealism, my position was very close to Socratic scientific realism in Lyon's sense, which is strictly non-epistemic.

Therefore, given my reaction to Kuhn's cognitive values, Lyon's proposal looks very attractive. Its promise is to justify and explain the different dimensions of systematicity, and to bestow some unity on them (exactly what I had tried to do with Kuhn's cognitive values). In *Systematicity*, I have declared that

I have no systematic theoretical argument for choosing precisely these nine dimensions and whether this list is complete. Such a theoretical argument would probably consist of some principle that could be developed such that it yields just these nine dimensions. Lacking such a principle, my procedure to identify these dimensions is, broadly speaking, empirical (Hoyningen-Huene 2013, p. 36).

A little later, I added somewhat resignedly:

This procedure may appear to be philosophically unsatisfactory, and perhaps it is. However, this is quite common in the natural sciences, for example. When studying new sorts of systems, there is no a priori way to find out what the relevant state variables are, or in another terminology, what the relevant dimensions are in order to describe and explain the systems in question. [...] Lacking a deeper philosophical principle that could do the desired job, namely to single out the relevant dimensions in which science and other knowledge seeking enterprises differ, I do not see any alternative to this empirical procedure (Hoyningen-Huene 2013, p. 37).

Lyon's Socratic scientific realism promises to provide just the desired philosophical principle. If the kind of truths that science aims at is properly articulated, the different dimensions of systematicity could possibly be more or less straightforward consequences of it. However, I have three reservations about what Lyon claims to be the goal of science in terms of truth. I fear that Lyon's characterization does not do justice to the diversity of goals existing within the sciences.

First, there are active scientists who are explicitly instrumentalists. A prominent example is Stephen Hawking:

I take the positivist viewpoint that a physical theory is just a mathematical model and that it is meaningless to ask whether it corresponds to reality. All that one can ask is

²⁷ I did not *write* that in my paper because I was not aware that one could consistently defend a purely *axiological* scientific realism without implying the acceptance of *epistemological* scientific realism. Therefore, Lyon's statement that "contemporary anti-realists who challenge the epistemological tenet take their challenges, at least implicitly, as sufficient condemnation of the axiological tenet" (p. 6) applied to me. In a recent paper, I defended the consistency of realistic interpretations of physical theories in scientific practice with an antirealism on the metalevel (Hoyningen-Huene 2018, in press-a), which seems akin to the consistency of axiological realism with epistemological antirealism.

that its predictions should be in agreement with observation (Hawking and Penrose 1996, pp. 3-4).

The reason Hawking that gives for his stance is clearly opposing Socratic scientific realism:

I don't *demand* that a theory correspond to reality because I don't know what it is. Reality is not a quality you can test with a litmus paper. All I'm concerned with is that the theory should predict the results of measurements.²⁸ (ibid., p. 121, my italics)

Many physicists hold similar views.²⁹ Imposing truth as the goal of science in spite of such explicit denials would probably require some sort of as-if construction. The claim would be that in their scientific practice, such physicists behave *as if* their goal was scientific truth; their opposition to Socratic scientific realism has no behavioral consequences. To that extent, their opposition to Socratic scientific realism would be just a private ideology. However, I would much prefer that an empirical meta-hypothesis about science could do without such contrafactual constructions that give the impression of immunizing ad hoc hypotheses.³⁰

Second, there is a large group of sciences investigating possible designs of products and processes with certain desired properties. I shall call this group of sciences the "technology producing sciences". A large subgroup thereof are, of course, the engineering sciences. Another important subgroup are the medical sciences, insofar as they develop technologies in the sense of "a set of techniques used to act on [...] the physical condition of the patient's body."³¹ The engineering sciences investigate possible designs of products and processes of manufacturing, control or measurement.³² Novel inventions of such products or processes can be patented. A crucial condition for patenting is that the invention "works", or more technically: "The invention must be industrially applicable and practicable, and it must be possible to replicate its implementation."³³ By contrast, (European) "patents are not to be granted in respect of 'methods for treatment of the human or animal body by surgery or therapy and diagnostic methods practiced on the human or animal body'."³⁴ This explicit exclusion of medical technologies from patentability is due to their special moral status. I wonder whether the goals of the technology producing sciences can really appropriately be described in terms of truth. Of course, the "working" of novel products and processes can be redescribed in terms of truth, such as: "it is true that product or process X works". However, it seems quite odd to describe the goal of the technology producing sciences as "to discover the truth about certain yet inexistent products and processes" instead of (roughly) "to invent products and processes with certain desired properties". Again, this redescription strongly

²⁸ See also Hawking and Mlodinow 2010, esp. Chapter 3.

²⁹ This is very difficult to document. In various workshops and summer schools for physicists in which I gave philosophy of science talks, I asked the participants, typically PhD students and post-docs, about their stances regarding instrumentalism versus realism. Very roughly, the result was half-half. It would be nice to have a rigorous empirical inquiry into that question.

³⁰ Another way to deal with this case might be the distinction between institutional goals of science and individual motives of scientists. Perhaps, one could claim that the institutional goal of science is truth, independently of an individual scientist's motives. However, instrumentalists like Hawking would probably not accept this move. They would assert that it does not make sense to claim truth as the institutional goal of science, because this goal is unintelligible – see Hawking's quote above.

³¹ Wootton 2006, p. 8. – I wish to thank Simon Lohse for suggesting the inclusion of medicine at this point.

³² Note the difference between engineering *science* and concrete engineering *work*, although there is no sharp boundary; see, e.g., Hoyningen-Huene 2013, pp. 12, 114, 121-122.

³³ <https://www.ige.ch/en/protecting-your-ip/patents/patent-basics/what-is-an-invention.html>, accessed 8 Oct 2017

³⁴ https://www.epo.org/law-practice/legal-texts/html/guidelines/e/g_ii_4_2.htm, accessed 20 Nov 2017.

smacks of ad hocery, because it neglects the fundamental difference between discovery (of something existing) and invention (of something yet inexistent).

Third, I do not think that mathematics, the humanities, certain parts of the social sciences, and some parts of the biomedical sciences can really be captured by Lyons' more concrete description of the goal of science. He describes the goal of science as "an increase in a system's degree of implication" (p. 14). The "systems" Lyons refers to are more or less large-scale theories, having potential applications in many, possibly very diverse areas if developed properly. This means that science seeks truth in the form of deep theories, and that further scientific work attempts to connect these theories in various ways to experience, generating "experientially concretized truths" (p. 11, similarly p. 13). In this process, the core components of these deep theories are *treated* as true. Tellingly, Lyons' examples for the latter are Newton's and Einstein's theories (pp. 11-13). It seems very plausible that large areas in the basic physical sciences can be characterized in such a way, and in these areas Socratic scientific realism (in its present form) and systematicity theory may be close allies. However, it is not evident that Lyons' characterization of the goal of science is adequate for all the natural sciences, for instance for all parts of biology.³⁵ In much of experimental biology, laws (or even theories) seem not to play a dominant role as goals; the same seems to hold for the non-technological parts of medicine, in which interest is focused on the functioning of the human body. The situation is similar in the humanities. It is very doubtful that their primary goal are theories with the features and applications characteristic of large-scale physical theories. This is because the kinds of explanations the humanities seek do not essentially depend of such theories, in contrast to large parts of the physical sciences.³⁶ Finally, Lyons' description of the goal of science certainly does not fit pure mathematics, which is mostly, if not completely independent of any "experientially concretized truth".

Thus, my problem with the present form of Socratic scientific realism is that it is, put concisely, too physics-centered. The goals of the sciences (in the wide sense) are too diverse for one concretization of truth to fit all. Even an abstract idea of truth as some sort of normative ideal, to be spelled out in more concrete terms differently for different areas, does not seem to fit perfectly those areas in which the ultimate goal is the production of new technologies.

For physics and related sciences, I think that Lyons' proposal for the unification of the two grand meta-empirical hypotheses about science, systematicity theory and Socratic scientific realism, is indeed productive. For the universe of all science, however, it seems that so far, the articulation of the goal of science in Socratic scientific realism is too narrow. Perhaps it could be widened in a dialectical process in which the presumed goal of science is adjusted to the dimensions of systematicity (and possibly vice versa) such that the goal implies (in some loose sense) the dimensions. This would not be a fraudulent fitting in order to get the desired result. Rather, one could view it as an explication of the goal of science on a very abstract and a less abstract level. Clearly, these levels are interdependent, but – *pace* Lyons – no logical ordering seems to be necessary in their construction in order to make them legitimate.

Conclusion

³⁵ See, e.g., Weber 2005.

³⁶ See, e.g., Hoyningen-Huene 2013, pp. 63-63, 68-78.

It is very gratifying that so much serious thought has been expended on systematicity theory. First, I sincerely hope that I did not do any injustice to the authors in my comments by seriously misunderstanding their papers. Second, I had to realize that in *Systematicity* I have not articulated the theory as clearly as I had hoped. Third, there are several areas to which further connections with systematicity theory may prove fruitful. This confirms the image of philosophy as an ongoing, open-ended dialogue.

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I have followed all of the reviewers' suggestions.

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