3. Speculative Philosophy of Science vs. Logical Positivism: Preliminary Round

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Abstract: I outline the theoretical framework of, and three research programs within American speculative philosophy of science during the period 1900-1931. One program applies verificationism to research in psychology, one investigates the methodology of research programs, and one analyses scientific explanation and other scientific concepts. The primary sources for my outline are works by Morris Raphael Cohen, Grace Andrus de Laguna, Theodore de Laguna, Edgar Arthur Singer Jr., Harold Robert Smart, and Marie Collins Swabey. I also use my outline to provide a partial comparison of American speculative philosophy of science and 1930s logical positivism. My comparison suggests that logical positivism was a proposal for substantially narrowing down and winding back American philosophy of science and was based on positions that were already problematized in the American context.

1. Introduction

(North-)American philosophy of science developed substantially during the early decades of the twentieth century. Key figures, starting with James Edwin Creighton and continuing with Edgar Arthur Singer Jr., Morris Raphael Cohen, and others, articulated conceptions of philosophy that equated it with, or at least intimately tied it to, speculative philosophy of science. They also used these conceptions in training substantial numbers of philosophers of science.[1] Creighton's location at Cornell University was particularly significant, since it played a central role in populating American philosophy departments (Cohen 1910; Schneider 1946, p. 471; Katzav and Vaesen 2022). In line with the ongoing professionalization of philosophy, the work of philosophers of science was prominent in the first two professional American philosophy journals, *The Philosophical Review* and *The Journal of Philosophy, Psychology and Scientific Methods* (Katzav and Vaesen 2022). More broadly, a substantial body of work in the philosophy of science was produced by dozens of American philosophers of science.

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[1] There were about 500 people registered as members of the American Philosophical Association in 1931, with about 6% of them identifying the philosophy of science as their primary research focus (PROC 1932).

This work included work on many of the topics subsequently important within analytic philosophy of science. By far the largest group of philosophers of science working in America were speculative philosophers of science, with only a small minority of these identifying as pragmatists (Katzav and Vaesen 2022).

There is, however, still no detailed examination of American speculative philosophy of science's research or of the bearing of this research on the development of philosophy of science after the arrival of logical positivism in America in the 1930s. Standard histories of the field start with the arrival of logical positivism and continue with what is assumed to be its internally driven development into logical empiricism and, eventually, the broader analytic philosophy of science of the late twentieth century. Insofar as pre-logical positivist American philosophy of science is recognized, it is the work of a few key pragmatists, most notably Charles Saunders Peirce and John Dewey.

I aim to enrich our understanding of American philosophy of science by taking a closer look at American speculative philosophy of science. I will present this tradition's conception of philosophy of science, especially of the logic of science, in more detail. Some input from the work of Cohen and of Filmer Stuart Cuckow Northrop aside, I do this using the work of some of Creighton's students, especially Harold Robert Smart but also Grace Andrus de Laguna, Theodore de Laguna (Grace's husband), and Marie Collins Swabey. So too, I will describe the development of three of speculative philosophy of science's research programs in the logic of science during the period 1900-1931, starting in each case with the work of Singer and then examining developments in the work of either the de Lagunas or Smart. One program applies verificationism to research in psychology, one investigates the methodology of research programs, and one provides logical analyses of scientific explanation and other scientific concepts. I will, finally, use my history to reconsider the impact of logical positivism in America.

My focus is on speculative work around Singer and Creighton not only because of their centrality to American philosophy of science and because of their own influence but because their work and that of their students is representative of a broader swath of American philosophy of science. The focus on work by speculative philosophers will also facilitate reconsidering the impact of logical positivism in America. I will conclude that logical positivism appears to have been a proposal for narrowing down the scope of and winding back American philosophy of science. Logical positivism also appears to have rested on positions that had already been problematized in the American context.

In section 2, I outline American speculative philosophy of science's view of philosophy of science. In section 3, I outline three of its research programs in the logic of science. I then, in section 4, compare American speculative philosophy of science with logical positivism. Section 5 is my conclusion.

2. Speculative philosophy of science and the logic of science

American speculative philosophy of science was part of the broader tradition of twentieth century Anglophone speculative philosophy. Roughly, what characterized speculative philosophers as speculative was their insistence on the epistemic independence of philosophy from established opinion, including from science, in developing perspectives on reality. Speculative philosophers are contrasted with critical ones, according to whom philosophy aims to answer its questions by appealing to established opinion and somehow analyzing or unpacking it rather than going beyond it. In the American context, the schools associated with critical philosophy were those of new and critical realism (Katzav and Vaesen 2017).[2] Logical positivism was a form of critical philosophy (Katzav and Vaesen 2022).

On the speculative conception of philosophy of science, philosophy of science comprises the logic of science and speculative metaphysics (Benjamin 1936). Speculative metaphysics aims to offer visions of reality that include a depiction of humans and of how they, and their distinctive characteristics, fit into the broader scheme of things. Moreover, these visions are to be developed in light of research in the logic of science while nevertheless going beyond science in what they envisage (see, e.g., Creighton 1902 and 1912; Cohen 1910 and 1930; Northrop 1925).

Within the speculative tradition, the logic of science had as its object the systematic organization of scientific knowledge. Thus, the logic of science was concerned with how judgement—conceived of as a positive epistemic attitude towards hypotheses and other representations—and inference are exemplified in the structures of scientific knowledge, including in classification, explanation, experimentation, and theory (Smart 1931, 25-26). Similarly, inquiry into the system of knowledge was taken to require, and often involved, an examination of whether there is only one kind of scientific judgement/inference or whether, for example, each special science came with its own kind of judgement/inference (Smart 1931, 31). The question whether there are multiple kinds of scientific judgement brought with it the question of how the judgements of the different sciences relate to each other. Asking this last question involved considering whether the concepts and laws of any science are reducible to those of any others (Smart 1931, ch. 2). Not surprisingly, speculative philosophers of science investigated the range of the special sciences, including formal logic and physics (de Laguna and de Laguna 1910; Northrop 1925; Smart 1931).

The concern with judgement and inference also brought with it a concern with the status of scientific judgements, specifically with whether such judgements are either epistemically or ontologically fundamental (Smart 193, 35-44; Swabey 1930, ch. 3). The epistemic question here is to what extent scientific judgements are evaluated against empirical evidence holistically, that is, as members of systems of inferentially related judgements, rather than individually, one at a time. The related ontological question is whether it is concepts, judgements or inferences that are ontologically fundamental.

One of the key theses shared by many speculative philosophers of science was that scientific judgment involves abstraction, including idealization. For this reason, it was often assumed that the logic of science should include an examination of the ways in which judgement is abstract as well as how this affects scientific inference. In other terms, the logic of science should involve a critique of scientific judgement, in the sense of an examination of

[2] Not all members of these schools, however, were opposed to speculative philosophy. William Pepperell Montague, for example, was a new realist and a speculative philosopher (Sheldon 1954).

the extent to which it provides us with less than the full truth (Cohen 1930; Smart 1926, 92; 1931, 217-225).

Let me bring out four more features of the speculative logic of science. First, its questions were approached descriptively, that is, as part of an investigation into how science actually is, and normatively, that is, as part of an investigation into how it ought to be. Thus, for example, the critique of science did not merely describe the limitations of scientific inference but also included suggestions about overcoming some of these (e.g., Cohen 1931). Second, how to understand judgement was often informed by evolutionary ideas, including Hegelian and Darwinian ones. This led, for example, to views according to which judgements have evolved to have characteristic functions and thus are to be understood in terms of these functions (de Laguna and de Laguna 1910, Part III). Third, not unrelated, the nature of judgement was often taken to be substantially illuminated by empirical investigation into its evolution. Answering philosophical questions in the logic of science was thus not limited to logical analysis, conceived of as the articulation of conceptual truths (de Laguna and de Laguna 1910, Part III; Swabey 1930, preface). Fourth, speculative philosophers working on the logic of science often saw no tension between empiricist, or even verificationist, views about the logic of science and speculative metaphysics (e.g., G. de Laguna 1942).[3]

3. Research programs in the speculative logic of science: 1900-1931

3.1. Methodology of psychology and verificationism in the work of Singer and de Laguna

Our first excursion into the speculative logic of science starts with Singer's 'Choice and Nature' (1902). Singer there considers whether there is legitimate room for psychological, ethical, and other non-epistemic considerations in scientific inference, more specifically in deciding which scientific hypotheses to accept. In his terms, the question is what role there is for free choice in scientists' decisions about how to interpret nature. William James argued that, where evidence is lacking, we can freely decide what to believe (1897). Singer's response to James and similar positions is that, in the absence of empirical evidence for or against a hypothesis, scientists ought to acknowledge their uncertainty about it. And if there is no possible empirical evidence that bears on the hypothesis, it is meaningless:

> Before those who really claim the right to believe in unsupported possibilities, science can only plead its inability to grasp their meaning. 'Either,' it says, 'your so-called beliefs are conceivably capable of confirmation or they are not. If they are, they await the event to be confirmed or refuted, as my doubts await it to be resolved. If they are not, but pose as faith in bare possibilities, they escape all chance of destruction by abandoning every vestige of content'. (1902, 74)

[3] The logic of science is broader than what Katzav and Vaesen call the critical component of speculative philosophy of science. The critique of science and empirically informed, speculative answers to philosophical questions are not part of the critical component of philosophy of science (2022).

Here Singer deploys a verificationist criterion of significance for scientific judgements, that is, one that ties their meaningfulness to the existence of conditions in which they can be individually tested. On his criterion, for a scientific judgment to be meaningful, it must "be capable of confirmation or refutation from an indefinite series of other points of view" (1902, 77). Further, Singer takes his criterion to be empirical; it is based on observing scientific practice (1902, 73-74; 77-78).

One of the main areas where Singer applies his verificationist criterion of significance is the philosophy of mind. His view is that a consistent empiricism cannot allow one to infer the existence of other people's mental states if these are conceived of as being essentially subjective. Conceiving of them in such a way would render claims about other minds unverifiable. As he puts it, some philosophers endorse

> that curious bit of reasoning commonly known as the 'analogy argument' which runs somehow thus: I am aware, and I alone am aware, that certain of my bodily acts are accompanied by mental states. When I observe similar acts in other bodies I infer that they too are accompanied by like states of mind. (1911, 180)

But, retorts Singer,

> An inference from a single case … has … no value at all … no series of observations, no probable error; no ground for inference; no meaning as a datum. (1911, 181)

The claim here is that, if we can only make a single observation of a kind of correlation to support a judgement that the correlation holds more broadly, and thus cannot test the judgement from multiple perspectives, we cannot estimate the judgement's probable error. In other terms, we cannot provide a probably correct estimate of how far our judgement might be from the truth. Such a judgement is supposedly meaningless.

Singer's view of scientific judgement is part of his broader view of the mental, and both views illustrate the widespread speculative goal of understanding the mental functionally. According to Singer, mental states, including judgements, are goal-oriented (teleological) dispositions to behavior that are fully public and social rather than private; sufficient observation will make each state fully visible to others (1911). Each type of mental state is thus supposedly differentiated by its function. As we have seen, scientific judgements are partly characterized by verifiability, which is part of their function.

We will see how Singer thinks verifiability fits with some of the broader goals of science in the next section. The remainder of this section considers how his research program in the logic of science was developed further by (Grace) de Laguna. She explicitly acknowledged (1927, xii) the influence of Singer's work on her philosophy of mind and, in particular, was drawn by the idea that knowledge of other minds is not knowledge of other essentially private mental states but of functional states. She pursues this idea in the context of a methodological investigation of Margaret Floy Washburn's psychology.

Washburn used ontological dualism to underpin her book *The Animal Mind* (1908). In this book, she assumes that mental states are essentially private and, in accord with this assumption, proceeds to inquire what it is like for various organisms, including single-celled organisms, to have such states. In a review of the second edition of Washburn's book, de Laguna argues that, contrary to Washburn, psychology should not have essentially private mental states as its objects of study, partly because claims about such states are not verifiable. In support of the unverifiability of essentially private mental states, de Laguna deploys what later, when Ludwig Wittgenstein offered a similar argument, came to be called a 'private language' argument. Could one, for example, fix the meaning of 'being angry' by taking it to refer to some essentially private mental process? Not according to de Laguna. She writes,

> No psychologist, I venture to assert, ever discriminated such a process and mentally labelled it 'anger' for purposes of scientific reference and comparison. Suppose he had done so, and tried to classify later experiences as 'anger' or 'not-anger' by comparison with this. He would find himself in serious perplexity, first, because it is very difficult to recall a past emotional state for purposes of comparison; and second, because he would probably find himself using the term in an arbitrary way, and making statements which could not be verified by others. (1918a, 621-2)

De Laguna's arguments lead her to conclude that psychology can only have as proper objects of study factors that are functions of the standardized conditions of the experimental setup. Mental states are, accordingly, to be conceived of in terms of their causal role in standardized conditions. As she puts it,

> It is an essential condition of scientific investigation of any phenomenon that observations made by one individual shall be verifiable by others. Otherwise indeed a phenomenon is not even identifiable. This was the point of my argument that psychological phenomena investigated experimentally 'become in effect functions of the factors constituting the standardized conditions of the experiment'. (1919, 297)

De Laguna moves beyond Singer in that her use of verificationism is methodological. She is arguing that scientific hypotheses need to have verifiable implications about their subject matter if that subject matter is to be identifiable and thus if scientific progress is to be possible at all. Thus, de Laguna is not using verificationism as a criterion of significance. Indeed, she makes clear that it is the task of metaphysics to take up questions that science must, because of its methodological commitments, leave aside, including about the ultimate nature of mental phenomena (1919; 1927, 127-128).

Plausibly, what lies behind this shift in the use of verificationism is the sophistication of de Laguna's holistic theory of meaning. She and her husband, Theodore de Laguna, argue that concepts, which they took to be judgements, never regulate behavior directly by connecting stimuli and responses but via logical interrelationships within systems of concepts. How we respond to any situation depends on relevant systems of concepts, including such systems' internal logical structure. For the scientist, the relevant system is primarily the system of concepts of their own science, along with these concepts' closely knit inferential interrelationships. Because of this, a concept's meaning cannot be specified in terms of correlations between stimuli and behavioral responses, such as observation claims, but also requires specifying its logical relations to other relevant concepts (de Laguna and de Laguna

1910, especially chapters 2 and 5 of part III; Katzav 2022). Thus, on the de Lagunas' view, it is only within systems that concepts have meanings and generate predictions. So, the meaningfulness of a concept depends on being part of a system of concepts rather than on being individually verifiable. They accordingly reject verificationist criteria of significance, including Singer's criterion, which requires that each judgement be individually verifiable. Further, (Grace) de Laguna argues that there are, in addition to concepts, other forms of representation, such as perception, that have content that cannot be fully captured using concepts. Part of her motivation is that she recognizes that we do have introspective (phenomenological) knowledge that appears to resist conceptualization (1927, 290). So, even if we cannot make conceptual sense of some phenomenon, it does not on her view follow that we cannot make sense of the phenomenon.

Not surprisingly, given de Laguna's theory of meaning, she can develop an account of mental phenomena that is less naïve than Singers', where they are equated with observable behavior. Her account treats them as theoretical entities (1918a, 626). More specifically, she thinks about mental phenomena in terms similar to what later came to be known as 'functionalism': an explanation of the nature of a mental state is, roughly, to be given in terms of its causes, effects and relations to other mental states, when fulfilling its proper function (De Laguna 1927; Katzav 2023).

Singer's verificationist research program was also developed by other figures, including quite a few of those who, unlike de Laguna, were his students. These students included, for example, the philosopher of science C. West Churchman (1948) and the logician Henry Bradford Smith (1928). De Laguna's own methodological approach had an impact on the development of psychology. In particular, a father of modern cognitive science, Edward Chase Tolman, is convinced by her argument that psychology ought to offer functionalist accounts of mental phenomena (1922, 45).

3.2. The methodology of scientific research programs

Let me turn to another early twentieth century research program in the logic of science, one that also starts with Singer's discussion of the role of non-epistemic considerations in hypothesis choice. This second program is what, in the second half of the twentieth century, came to be called 'the methodology of research programs'.

Despite Singer's appeal to a verificationist criterion of significance, he does, in the end, allow room for free choice in deciding which scientific claims to accept. Here too, he deploys verificationism to support his position. Roughly, his argument is as follows: for an observation claim to be potentially confirmable and falsifiable (testable), and thus meaningful, the claim must represent two kinds of error, namely probable error (which, for observation claims, amounts to what we would call 'measurement error') and constant error (the conditions under which the observation is expected to be correct). So, for example, for some report about the length of a rod to be testable, it must include a margin of error as well as a specification of the temperature, stress, and other standard conditions under which the claim about the rod is supposed to be correct. Singer accordingly takes it that the simplest kind of scientific judgement, the observation one, is hypothetical; it tells us roughly what would be observed in certain standard conditions. It is not categorical, stating that such and such is the case. Similarly, Singer holds that scientific laws are conditional in their application (1902, 78-80).

Still, science must make categorical claims. Singer calls the sets of assumptions that underpin, and so make possible, categorical applications of law and observation claims 'classification systems.' These specify research questions for the scientist and, in doing so, guide the acceptance of scientific claims. Classification systems thus set up what we would call 'research programs.' Further, since there are always multiple classification systems from which the scientist might choose and since they underpin accepting laws and observations, there is always a choice in how scientists respond to observed exceptions to their laws (1902, 81-82). Thus, while Singer's criterion of significance implies that each scientific claim must be empirical, it also leads to recognizing that choice has a role in determining every scientific fact. His resulting position is a form of idealism in which choice plays a role in constituting nature. Regarding science, he tells us,

> Whatever is required to account for the way in which one of its stages follows on another is essential to the nature of experience. And since at any stage of our growing knowledge at which we try to tell what Nature is, the describer is presented with a choice, and since no stage can be found which does not embody past choices, I take it that this series of choices is involved in anything we do or can mean by Nature. (1902, 82)

Singer uses a series of case studies drawn from the history of science to support his claims about the role of classification systems and to explain how they are selected. The replacement of a classification system, and of the laws that presuppose it, is guided by the goal of systematicity or unity. When empirical evidence suggests exceptions to our laws, we are not satisfied with accepting the exceptions. The goal of a global unified system of knowledge drives us to formulate new, maximally unified, and thus relatively simple, schemes of classification and corresponding sets of laws. In this way, we deepen our understanding of nature (1902, 82-90).

Among the speculative philosophers who further develop the discussion of research programs after Singer, we find the de Lagunas.[4] One of the main ways in which they go beyond positions such as Singer's is in their variant of holism. We have seen that they had a holistic view of conceptual meaning. They also had a holistic view of hypothesis evaluation. On the de Lagunas' view,

> The validity of a universal principle is not a matter of its own individual adequacy as a description of reality; nor, again, is its validity relative to the whole existing body of human knowledge (if, indeed, we can speak of such a thing). It may correctly enough be said that the validity of such a principle depends upon its place in the developing structure of our knowledge, if we remember that this place is not definitely determined, but is exceed'ngly variable. A law is not judged as true because it marks the limit of human knowledge and because we are not able to correct any given formulation of it. Its truth is always a matter of context. It is valid if we find a certain harmony between

[4] Another important starting point for the de Lagunas' work on research programs is that of Creighton (Katzav 2022).

> the character and degree of its abstractness and the character and definiteness of the conclusions in view of which it is asserted. (de Laguna and de Laguna 1910, 153)

The de Lagunas, then, agree with Singer that laws are not abandoned merely because they confront counterexamples but rather partly due to relevant background assumptions including the goal of theoretical cohesion. To this extent, they all agree that hypothesis evaluation is holistic. However, the evaluation of laws in science is to some extent a local affair, one tied to the specific theoretical and empirical state of knowledge within given disciplines. Counterexamples to laws will be tolerated by scientists within a given context as long as the laws are sufficiently well articulated in order to manage the complexity of available empirical data. It is only when available theoretical systems of laws are too crude to do this that alternatives will be sought. Here the de Lagunas disagree with Singer or, at least, note a serious lacuna in his treatment of hypothesis evaluation. He fails to recognize that the goal of a unified system of all science is too distant to play a central role in explaining hypothesis choice. In addition, the de Lagunas present their development of the theory of judgement as one that takes further the evolutionary account of judgement. A proper evolutionary account, on their view, needs to recognize that judgement itself is not just functional but also that its function evolves and thus that, for example, the standards of judgement will change in different contexts (1910, 135-148). Singer does not recognize such evolution.

Like Singer, the de Lagunas use case studies to support their claims about the treatment of laws in science (1910, 149-161). I, however, want to emphasize a further distinctive feature of their work on research programs. While Singer recognizes but says little about the psychological and sociological factors that play a role in the evolution of research programs (1902, 80), the de Lagunas say quite a bit on this topic. Grace says more in “Cultural Relativism and Science” (1942). Theodore says more in his *The Factors of Social Evolution* (1926). Here is one particularly striking summary of his views from that book:

> Often enough, when our principles are contradicted, we simply deny the accuracy of the new observation or the veracity of the report. More often, perhaps, we ascribe the apparent contradiction to the operation of unknown disturbing causes. Nothing is more familiar to us than that a rule should have exceptions. The proverb even has it that ‘the exception proves the rule.’
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> But if the exceptions become frequent, and especially if they begin to exhibit a certain regularity, the whole complexion of the matter changes, for the principle itself becomes charged with the fault. It may not be at once given up — in fact, it is extremely unlikely that it should; for the extensive correlation of detail that it formerly accomplished, it still accomplishes, and there is nothing as yet to take its place. But a condition of instability is produced. Attempts are continually being made to correct the principle in question so as to accommodate the troublesome exceptions; but too often the new formulae fail to cover much that was satisfactorily accounted for by the old. A division between conservative and radical parties occurs, just as in the case of a moral or political issue. And, despite all differences of detail, the final settlement is reached in fundamentally the same fashion. Comparative shortcomings must be appreciated, not

> counted; and the importance ascribed to each is, in the last resort, determined by tastes and prejudices. (1926, 94-95)

This quote includes within it much that became so familiar from Kuhnian philosophy of science, including, in its statement that the new formulae fail to cover everything that the old ones did, a recognition of what came to be called 'Kuhn-loss,' and, at the end of the quote, an account of the socio-political factors operative in scientific revolutions.

In closing this subsection, let me emphasize that the discussions of research programs by Singer and the de Lagunas are examples from a broader discussion during the early decades of the twentieth century. I could equally have used authors other than the three I have chosen to show that these decades included a rich discussion of this topic. I could have followed Cohen's 1931 discussion in his *Reason and Nature* (1931, 80-146), Smart's discussion from the same year (1931, 34-45), or other discussions.

3.3. Scientific explanation and other topics in logical analysis

The already presented examinations of methodology in psychology and of scientific research programs is substantially empirical. Thus, Singer's statement of a verificationist criterion of significance is an empirical claim about science. At least part of the time, he seems to be reporting on scientists' conception of meaningfulness. So too, his case studies provide empirical support for his claim about the role of classification systems in science. The de Lagunas' discussion of research programs in science is clearly partly empirical, concerning which standards of acceptance are actually used in science. In this section, I will present a speculative research program in which the focus is more on logical analysis conceived of as the provision of logical or conceptual truths. The program concerns the nature of scientific explanation and, once again, at least partly goes back to Singer's work.

Singer's paper 'On Mechanical Explanation' (1904) is concerned with whether there is ultimately one kind of scientific explanation. More specifically, Singer's question is whether the mechanical ideal can be realized, that is, whether ultimately all explanation in the natural sciences is mechanical explanation. His response is a partial one and is that, if all scientific knowledge in the natural sciences can be conceptually reduced to that of mechanics, all scientific explanation in the natural sciences will be mechanical explanation. He also argues against what he takes to be some of the strongest objections to the possibility of such a reduction.

Singer offers the following account of what it is for one science to be reduced to another:

> *any science x, dimensions* abcd*, is reducible to any science y, dimensions* abc*, when it may be shown in any manner that the term* d *is expressible as a function of* abc. (1904, 271).

By 'dimensions of a science,' Singer means the kinds of independent measurements that need to be made for its formulae to yield definite predictions. In mechanics, according to Singer, these dimensions are measurements of mass, length, and time. It follows, since he thinks that reduction to mechanics will vindicate the mechanical ideal, that he thinks that mechanical explanations are functional explanations in terms of mass, length and time. And the mechanical ideal of explanation will be achieved when the apparently extra dimensions of the natural sciences can be expressed as functions of mass, length and time, so that these last three are the only real, independently measured quantities, and thus the only dimensions of all the natural sciences (1904, 267).

Keeping in mind Singer's verificationist criterion of significance suggests further that he thinks that, when a dimension, *d*, is expressed as a function of others, our judgements about *d* are shown to be the same as our judgements about the others. So, Singer's verificationist criterion of significance goes along with a corresponding verificationist criterion of sameness of meaning. For, by hypothesis, no judgement about *d* can have any measurable implications that are not already captured by judgements that are about the other dimensions. Thus, any judgement putatively expressing something distinct about *d* from the judgements of the reducing science would have no empirical implications and thus be meaningless. It seems that reduction of a science involving *d* to one not involving *d* means that our judgements about *d* say nothing more than what is expressed by judgements about other quantities. This is why, for Singer, successful reduction of the natural sciences other than mechanics to mechanics is conceptual and will vindicate the mechanical ideal.

Singer goes on to argue that the facts of biology, including teleological ones, cannot serve to demonstrate the unrealizability of the mechanical ideal. He also expresses the (unargued) view that, if the facts of biology cannot thus be used, no facts from another science can (1904, 282-3). I want to focus, however, on the (partial) support he offers for the mechanical ideal. This ideal came under repeated attack by later philosophers of science within his tradition.

The de Lagunas' meaning holism challenges Singer's attempt to vindicate the mechanical ideal. Their holism permits, because it tells us that logical structure has a role in fixing concept meaning, multiple theoretical systems with the same predictions but without the same content. So, their view would be that reducing a science to mechanics, in Singer's sense of reduction, does not automatically imply that the two have the same content. If anything, doing so threatens to change the meanings of the involved scientific terms by changing their logical interrelationships. A reduced science may not be saying the same thing as it said before reduction and thus may not be able to explain what it used to explain.

Indeed, in directly critiquing the mechanical ideal, (Grace) de Laguna argues that neither the concepts of physics nor those of psychology can begin to express what all the sciences express. One of her arguments involves observing that the principles of classification of physical science do not even allow identifying social kinds, such as election victories or bank collapses. She notes that the different sets of physically described events–redistributions of mass and energy—embodying the class of electoral victories by the USA's democratic party—cannot be classified by physics as events of a single kind. So, one cannot identify, never mind describe or explain, democratic victories using physical terms (1917a). Similarly, psychological kinds, such as resembling classes of experiences, do not generally map onto physiological, never mind physical, kinds (1918b). Conversely, physical kinds do not generally map onto corresponding psychological kinds (1917b).

De Laguna, however, does not explicitly offer a theory of scientific explanation. Smart does and also, like de Laguna, rejects the idea that there is only one kind of explanation in science.[5] Smart's discussion of explanation in science starts with an argument for thinking that, contrary to positivism, scientific judgement in the mathematical sciences is explanatory (1931, ch. 2). This, in turn, leads him to ask what the nature of explanation in these sciences is and eventually to argue that, and consider how, explanation in the mathematical sciences differs from explanation in other sciences.

Smart articulates his view of scientific explanation in the mathematical sciences using a conceptual distinction between descriptive and explanatory laws:

> We must distinguish between two types of law, the empirical or descriptive, and the theoretical or explanatory. Empirical laws do merely describe the 'how' of things; but the real problem of science is to discover some theoretical basis for things. Thus why comes to mean … an explanation in terms of natural conditions, or … in terms of systematic organization. (1931, 60)

On this analysis, what makes an explanatory law explanatory (in the mathematical sciences) is not just its generality but also its distinctive inferential role. Such a law allows us to derive empirical laws or generalizations and assign them to natural classes. Explanatory laws thus show empirical laws to be necessary and, in doing so, explain them. For example, Newton's law of gravitation determines whether a planet's path will be elliptical, hyperbolic or parabolic just given a planet's initial velocity, without an initial direction. So, the law of gravitation delimits three natural classes of empirical laws and thus allows us to see the laws that belong to each of these classes as necessary in respect of the type of conic section they follow. This is why the derivation of these empirical laws is explanatory. (1931, 55-56)

So far, Smart has in effect pointed to a lacuna in Singer's discussion. Singer seems to assume that any general principle, or in his terms any function, is explanatory and thus neglects the important distinction between explanatory and empirical laws. Disagreement proper arises when Smart considers explanation in biology. Smart rejects the view that "ultimately the biological sciences must seek the same type of answers to their problems, as satisfy the physicists" (1931, 61). While mechanical explanation should be pursued as far as possible in biological science, we find that biological science's primary aim is still the classification of individuals. Moreover, a proper understanding of how this affects biological explanatory principles should convince us that biology should not, even in the long run, aim solely at explanatory laws of the kind found in physics. Physics views the individual merely as the exemplification of laws and thus abstracts from concrete reality in a way that diminishes the individuality of the phenomena with which it is concerned. Biological science is concerned with classificatory judgements that relate individuals to classes. And individuals are understood to belong to their classes as a function of the totality of their organization, so that biological sciences are still inevitably concerned with individuals as wholes (1931, 61-3;159-162).

[5] Smart, unlike de Laguna, never mentions Singer. This should be no surprise. Smart, following citation customs in his milieu, offers limited citations and does not tend to cite those in his own research community. The small size of the American philosophy of science community, however, means that Smart will have known Singer's work.

Regarding the individual as a whole, explanation "means the tracing of an evolutionary process of chance, and possible reference to purpose" (1931, 63).

I have used the work of Singer, the de Lagunas, and Smart to illustrate some of the positions being developed in the logical analysis of scientific explanation within American speculative philosophy of science. The work of other figures, such as that of Cohen (1931) or A. Cornelius Benjamin (1927; 1936), could equally illustrate views of scientific explanation. So too, while I have focused on the logical analysis of scientific explanation, I could equally have focused on logical analysis relating to measurement, probability, confirmation, the problem of induction, the observation-theory distinction, realism about theoretical entities, idealization, or causation. Each of Cohen (1931), Smart (1931) and Swabey (1930) offer us relevant discussions of all these topics, except for Smart who, as far as I am aware, has no discussion of measurement or of probability.

4. Speculative philosophy of science and logical positivism: initial comparison

4.1. A comparison of the scope and development of speculative philosophy of science with that of logical positivism

Katzav and Vaesen observe (2022) that early twentieth-century speculative philosophy of science provided logical analyses of general methodological scientific concepts. Such analyses only start to appear in logical positivism in the 1930s and, in key cases, only appear with its demise and the development logical empiricism in the 1940s and 1950s. The project which logical positivism brought with it to America, and which was first clearly articulated in 1928 by Rudolf Carnap (1967), was centered on establishing the unity of science via various verificationist, reductivist projects and on the verificationist elimination of metaphysics (e.g., Blumberg and Feigl 1931; Carnap 1934; Reichenbach 1938). Discussions of induction and probability only begin to emerge within logical positivism in the 1930s. And analyses of confirmation and of scientific explanation that are influenced by logical positivism only emerge in the 1940s, with the work of Carl Gustav Hempel, the associated demise of logical positivism, and the development of logical empiricism (Giere 1996).[6,7] What the previous sections, especially 3.3, suggest that we add to this story is that the speculative discussions of the logic of explanation not only preceded logical positivism but were part of a long-standing research program, one that started as early as the turn of the twentieth century and that exhibited substantial theoretical development. This program in the analysis of explanation was, further, part of a broader collection of programs of analysis, one that arguably encompassed all the main projects of analysis of later philosophy of science.

Similarly, while logical positivists were aware of the difficulty of deciding when to accept falsifying evidence (e.g., Neurath 1931; Carnap 1937, 317), they do not themselves develop a substantive research program investigating these difficulties. It is only in the wake of Karl Popper's 1959 publication of the English translation of his 1934 book *Logik der Forschung* and the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962 that research into the methodology of research programs enters what became analytic philosophy of science alongside logical empiricism.[8] Yet, as Katzav and Vaesen observe (2022), and as we saw in section 3.2, work on the methodology of research programs already had a place within the earlier speculative tradition. What section 3.2 allows us to add here is that speculative work on research programs went back at least to the start of the twentieth century and exhibits substantial theoretical and methodological development. My discussion of speculative work on research programs also indicates that speculative logicians of science

[6] I here adopt a use of 'logical positivism' and 'logical empiricism', one that roughly follows the periods of their introduction into widespread use (Feuer 1941; Salmon 1999). On my use, 'logical positivism' refers to a body of principles that was brought to America by immigrant philosophers of science and played a central role in the subsequent development of philosophy of science in that country; the principles are those attributed to the logical positivists in this and the next section. 'Logical empiricism' refers to the dominant philosophy of science that developed in the 1940s and 1950s when the principles of logical positivism fell out of favor. While it is sometimes assumed that 'logical positivism' and 'logical empiricism' are coextensive and substantially broader in their extension (e.g., Creath 2023), it is important for my comparative purposes to distinguish between what influential immigrant philosophers of science brought with them to America in the 1930s, what was proposed by some immigrants but never had an impact there (see footnote 9), and what was found there in later decades.

[7] Carnap does discuss the problem of confirmation in the 1930s. However, he then thinks this problem is the same as that of determining the verification conditions of propositions and is not concerned with analyzing the concept of confirmation (Carnap 1936, 420; Giere 1996, 340).

[8] In 1934, Popper recognizes the fallibility of falsification and incorporates it into his view of diachronic scientific development (1959). He thus makes a start at developing a methodology of research programs. Despite appearing to mistakenly complain that Popper does not recognize the fallibility of falsification, Neurath not only does not go beyond pointing out that when to revise falsification claims is not straightforward but appears to suggest that there is no relevant philosophical issue that needs to be addressed (1931; 1935).

tended to gain philosophical insights from case studies drawn from the history of science, unlike 1930s logical positivists.[9]

Section 3.1 indicates further that even verificationism and its implications, supposedly key, distinctive foci of logical positivism, were integral parts of speculative philosophy of science and were developed within one of its long-standing research programs. To be sure, many have noted the affinity between classical pragmatism's criteria of significance and verificationist ones (e.g., Misak 2005; Uebel 2015). But the pragmatist criteria tie the meaningfulness of ideas to their practical consequences, that is, to how accepting an idea would or should in general affect behavior or, even more broadly, affect us in any way (Legg and Hookway 2021). Singer's principle, and the de Lagunas' dissent from it, focus specifically on empirical testability just as do logical positivist criteria of significance.

We thus find that the most prominent aspects of analytic philosophy of science in America during the second half of the twentieth century—logical positivist verificationism and worries about it, logical empiricist analysis of general concepts in science, and discussions of research programs—were developed research programs within an earlier, largely forgotten speculative tradition. But we ought to remind ourselves, there was much more to this tradition. It included extensive discussion of idealization in science, of how idealization affected hypothesis selection, of speculative visions of reality, and of work on the logic of individual special sciences. American logical positivists were apparently proposing a substantial narrowing down and winding back of the philosophy of science.[10]

4.2. Logical positivism and some problem situations in American philosophy of science

How did the arrival of logical positivism in America affect problem situations in American philosophy of science, that is, affect its range of questions, as well as the range and viability of available answers to these questions? To begin answering this question, we need more detail about logical positivism in its American context. My further presentation of it will center on five theses which were shared by key logical positivists—Rudolf Carnap, Herbert Feigl, and Hans Reichenbach—during at least the 1930s, when their work came to be known in America. To begin with, Carnap (1934; 1937), Feigl (Blumberg and Feigl, 1931; Feigl 1943) and Reichenbach (1938) embraced verificationist criteria of significance and of sameness of meaning. In *The Unity of Science*, which originally appeared in 1932 and was translated into

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[9] My claim is not that the logical positivists were uninterested in the history of science but only that they did not tend to use it to evaluate philosophical claims. Edgar Zilsel, to be sure, has been taken to be a logical positivist and arguably did endorse the idea of a case-study based argument for the unity of science. However, whether for practical reasons or because he came to reject this idea, Zilsel never actually used his work in the history of science to support the thesis of the unity of science (Raven and Krohn 2003, xlix and li-liii). Moreover, he neither accepted the logical positivist view that philosophy is logical analysis nor had an impact in America (Raven and Krohn 2000, xix and xliii). While some of Neurath's work from the 1910s can also be taken to include case-study based philosophy of science, it is unclear whether it does (Zemplén 2019, p. 219). In his logical positivist phase, he at most permits that philosophy has a role in conceptual analysis (1931).

[10] Whether logical positivists' views of formal logic might have been more novel is not something I take a stand on here.

English in 1934, Carnap divided meaningful statements into protocol statements, i.e., observation statements, and non-protocol statements. He was undecided about whether protocol statements were best understood as requiring no verification or as directly verifiable by experience. However, he thought that a meaningful non-protocol statement must be (to some degree) verifiable by the protocol statements it entails (1934, 42-50). Further, non-protocol statements which implied the same protocol statements were supposed to have the same meaning (1934, 51). Carnap presents similar commitments in *The Logical Syntax of Language*, published in 1934, though there his focus is on distinguishing meaningful and meaningless sentences and thus on syntactical categories (1937, 319-20). Feigl's views were, during this period, akin to those of Carnap (Feigl 1943, 392-393). Reichenbach, on the other hand, endorses a probability theory of meaning. On this theory, "a proposition has meaning if it is possible to determine a weight, i.e., a degree of probability, for the proposition", where weights are determined by observation. Further, two statements have the same meaning "if they obtain the same weight, or degree of probability, by every possible observation" (1938, 54).

Alongside criteria of significance and of sameness of meaning, Carnap, Feigl and Reichenbach shared reductivism about science, the view that the meanings of all scientific statements could be reduced to those of some privileged set of statements. At the outset of his logical positivism, Carnap aimed to use explicit definitions to fully translate all the statements of all the sciences into protocol statements about similarity classes of sense data, though he also proposed the possibility of an alternative translation scheme into protocol statements about observable physical objects. He quickly came to prefer a reduction base of physical statements and to propose a less ambitious form of reductivism. On this less ambitious proposal, non-protocol statements have their meaning partially specified by entailing, with the help of stated correspondences between what their non-observational terms describe and what observational ones describe, protocol sentences (1967, preface). The development of Feigl's views of reduction (1943) are similar to Carnap's. Reichenbach, however, thought that a statement, *p*, is reduced to a set of statements, *S*, if *p* is coordinated with *S*, either by definitions or empirically, so that *p* and *S* have the same meaning according to the probability theory of meaning (1938, 94-5, 216-17). Further, according to Reichenbach, all statements, including, e.g., those of sociology, are reducible to observation statements about physical objects (1938, 211-17).

The introduction of logical positivist verificationism into the USA could not itself have amounted to philosophical progress. As we have seen, verificationist criteria of significance and of sameness of meaning already had an, at least, decades long history there. Moreover, verificationism was still on the scene in the period 1920-1940. The papers by Singer that I have been discussing were all republished in his 1924 book *Mind as Behavior and Studies in Empirical Idealism*, and his work is extensively discussed by his students and other prominent American philosophers (Clarke and Nahm 1942). These discussions could hardly have gone unnoticed in the small community of American philosophers of science.

Indeed, the reductivist forms of verificationism positivists brought with them would very plausibly seem, from the American perspective, at best to require substantial work. There were, to begin with, the challenges that the de Lagunas' meaning holism suggest for verificationist criteria of significance and sameness of meaning. Their holism also challenges positivist reductivism. It is not just that, as pointed out in discussing Singer's reductivism, the idea of reducing various sciences to some fundamental one is threatened but also the idea of reducing all the statements of a science to its observation statements. If the de Lagunas are correct, the concepts of a science have their meaning partly by virtue of their inferential role in the system of the science's concepts. This applies equally to the concepts used in observation statements, so that the positivist goal of using them as primitives in terms of which all others are to be analyzed appears to be blocked.

(Grace) de Laguna, further, explicitly challenges reductivism about science, partly by noting the absence of straightforward mappings of sociological kinds onto physical kinds. If, as she suggests, this implies that these sciences must talk about different things, even a partial translation of sociology into physics must fail. For similar reasons, even a partial translation of physics into psychology must fail. Such challenges came later to be recognized as key challenges to reductivism about science (Katzav 2023). Smart supports de Laguna's perspective with his detailed examination of the types of explanation provided in mechanics and biology, arguing that the latter rightly offers a kind of explanation that rests on concepts that cannot be captured in physical terms.

Two final logical positivist theses which I will discuss are the restriction of philosophy to logical analysis and the rejection of metaphysics (Blumberg and Feigl 1931; Carnap 1934, 1937; Feigl 1943; Reichenbach 1938). According to the first of these, philosophy is epistemology and is solely concerned with the logical analysis of the structure of scientific knowledge. Again, Carnap's views are illustrative. He writes that "the activity of philosophy consists ... in clarifying the notions and statements of science" (1934, 33). He adds that everything other than logical analysis that has been a traditional part of philosophy is a "confusion of non-scientific pseudo-problems" (1934, 23). To be sure, Carnap and other logical positivists permit the empirical investigation of knowledge, but they are clear that such investigation cannot answer the questions of philosophy. Carnap tells us that the empirical investigation of knowledge is merely an investigation of the origin of knowledge by psychology; it is not an investigation of the nature of knowledge (1934, 22-4). Finally, there is, as part of the rejection of traditional philosophy, the rejection of all metaphysics. Not even logical analysis is allowed to contribute to metaphysics. More specifically, metaphysics is meaningless or, at least, almost entirely meaningless; exceptions to the meaninglessness of metaphysics allowed by Feigl (1943, 385) comprised metaphysical statements that are 'disreputable' inductions from available observations.[11]

Speculative philosophers of science recognized logical analysis as part of epistemology but also included empirical research within it. As we have seen, they informed their work on the methodology of research programs by empirical considerations and offered explicitly empirical hypotheses about such programs. Now, while logical positivists thought that empirical considerations could only contribute to the causal understanding of knowledge and not to epistemology, speculative philosophers of science were well motivated in thinking that empirical considerations were relevant to epistemology. For example, according to the de

[11] One can, despite the logical positivists' claims, view their goal of unifying science via reductivist projects as a metaphysical one. In what follows, however, I will follow their lead and use 'metaphysics' to refer to work that aims to make substantive claims about reality rather than merely to provide logical analyses.

Lagunas, types of judgements are types of evolved, functional states. As a result, such types tend to have been selected for specific functions and can thus be understood by an examination of how they were selected and of the purposes for which they were selected. An understanding of their functions should bring with it an understanding of their success conditions and thus of how they are to be evaluated (de Laguna and de Laguna 1910; Katzav 2022). As far as I can tell, logical positivists did not criticize these positions.

The verificationist criterion of significance is the explicit motivation logical positivists offered for their rejection of metaphysics (e.g., Carnap 1937, p. 278). Those who had already examined and rejected verificationist criteria of significance would rightly not have been impressed by appeals to them. (Grace) de Laguna, for one, persisted in promoting speculative metaphysics (Katzav 2023). But even someone like Singer would have seen no reason to conclude that metaphysics is meaningless. Singer's deployment of verificationism is, we have seen, part of an investigation that aims to determine to what extent scientific evidence leaves room for choice in how scientists represent nature. It is as a result of this investigation that he concludes that choice of classification system reflects scientists' freedom and thus points to the hypothesis that scientists mold nature through their choice of classification systems. Thus, for Singer, idealist metaphysics is the result of a verificationist exploration of science. To be sure, some variants of the verificationist criterion of significance could be deployed against Singerian metaphysics. His idealism cannot plausibly be said to have been strongly confirmed by empirical evidence, so that it would be rejected as meaningless by a criterion according to which only fully or strongly verified hypotheses are meaningful. But these variants of verificationism were quickly recognized to be unacceptable, even by logical positivists.

5. Conclusion

When logical positivism arrived in America, it was in effect proposing a substantial narrowing down of philosophy of science, one encompassing not only the rejection of metaphysics-related philosophy of science and empirical philosophy of science but also of the logical analysis of science. What was proposed for elimination, further, was part of an established tradition of work with decades of development behind it. This proposal for philosophy of science went along with the proposal of the adoption of a logical positivist approach to philosophy. While the standard historiography of philosophy of science sees this approach as largely unopposed by local philosophy of science, if only because it supposedly did not exist, my work suggests that the local tradition had its own established, evolving speculative approach. My work also suggests that it is not obvious how the speculative approach was challenged by what the logical positivists brought with them. If anything, it seems that the logical positivists' proposals were, when viewed from the American perspective, poorly argued, and based on an already problematized set of assumptions, including verificationism, reductionism, and methodological misgivings about empirically informed philosophy.

I have, to be sure, only provided the very beginnings of a comparative examination of the local and immigrant approaches to philosophy of science. I have not looked at much detail or substantially evaluated arguments. Nor have I looked at all the challenges posed by speculative philosophy to logical positivism, or at all the ways in which these schools might be contrasted. For example, there were philosophers of science, such as Swabey, who aimed to provide synthetic a priori justification of key inferential and ontological scientific principles (1931). Their arguments were developed in response to critiques of synthetic a priori knowledge and thus provided a ready-made challenge to the empiricist immigrants. Similarly, one can compare work by speculative philosophers on the special sciences with imported work. For example, one can contrast the metaphysics-driven philosophy of physics of Filmer S. C. Northrop and Andrew Ushenko with the philosophy of physics of Reichenbach. There remains much work to be done before we understand what happened to the philosophy of science with the arrival of logical positivism in America, never mind how it affected the subsequent development of the field.

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