FRENCH NEOPOSITIVISM AND THE LOGIC, PSYCHOLOGY, AND SOCIOLOGY OF SCIENTIFIC DISCOVERY

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This article is concerned with one of the notable but forgotten research strands that developed out of French nineteenth-century positivism, a strand that turned attention to the study of scientific discovery and was actively pursued by French epistemologists around the turn of the nineteenth century. I first sketch the context in which this research program emerged. I show that the program was a natural offshoot of French neopositivism; the latter was a current of twentieth-century thought that, even if implicitly, challenged the positivism of first-generation positivists such as Comte. I then survey what French epistemologists—including Ernest Naville, Élie Rabier, Pierre Duhem, Édouard Le Roy, Abel Rey, André Lalande, Théodule-Armand Ribot, Edmond Goblot, and Jacques Picard, among others—had to say about the logic, psychology, and sociology of discovery. My story demonstrates the inaccuracy of existing historical accounts of the philosophical study of scientific discovery.

1. Introduction

The current article deals with a forgotten research program that developed out of late nineteenth to early twentieth-century French neopositivism, a program that took as its key object of study the logic, psychology, and sociology of scientific discovery. In section 2, I start by describing French neopositivism. This current of thought, associated most notably with Henri Poincaré, Pierre Duhem, Édouard

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Le Roy, and Gaston Milhaud, aimed to provide a corrective to first-generation positivism (à la Comte), which it—implicitly or explicitly—considered to be excessively empiricist (Brenner 2003, 2015). Although French neopositivists did not converge on a single doctrine (2015), they all assigned to the human mind a much more active role than first-generation positivists had done (2003). They were especially impressed by the mind's contribution to the discovery of new hypotheses and theories. Accordingly, I argue, it was only natural for them and other French epistemologists to closely examine what is currently often referred to as the context of discovery, including the psychology and reasoning (or *logique*) involved in discovery, and the sociological conditions that favor invention.

Section 3 offers a summary of the work that was produced within this "discovery program." I show that it was a truly thriving program: an impressive number of renowned and by now forgotten French epistemologists were part of it, and these epistemologists studied an impressive number of topics associated with scientific discovery, including the logical and nonlogical principles of discovery, the psychology of scientific genius, and the collective nature and sociological determinants of discovery.

Section 4 then assesses existing histories of the philosophical study of scientific discovery. These historical accounts (Laudan 1980; Feist 2006; Schickore 2018; Longino 2019) suggest that after the 1840s, scientific discovery was virtually ignored by philosophers until it became, during the late 1950s, one of the prominent fields of inquiry in the Anglo-American world. My story calls for a substantive revision of these reconstructions.

Finally, section 5 highlights some of the questions that my study raises regarding the history of the philosophy of science in general.

2. French Neopositivism: Fertile Ground for the Study of Scientific Discovery

My entry point into the neopositivist current of thought that was emerging within late nineteenth-century France will be a paper by Édouard Le Roy, a former student of Henri Poincaré. The paper (Le Roy 1901) was the first explicit attempt at characterizing the new intellectual movement.

According to Le Roy (1901, 140), the neopositivists—he cites, without aiming to be exhaustive, Gaston Milhaud (1896), Joseph Wilbois (1899, 1900), Henri Poincaré (1891), and himself—respond to Comte's positivism, which they would find (1) too simplistic and utilitarian, and in light of this, too little concerned with the pursuit of truth (realistically conceived), and (2) insufficiently trusting of the creative powers of the mind.[1] Regarding 1, the new positivism would object to Comte's uncritical endorsement of the "instinctive doctrines of common sense" (Le Roy 1901, 141)—to the fact that the Comtean scientist naively submits to the supposedly immediately given. But, Le Roy continues, common sense tends towards utility rather than truth. Accordingly, for the sake of truth—which Comte would have too easily given up on—scientists ought to detach themselves from common sense, the concerns of practical life, and the illusions of "évidence vulgaire" (142).[2] Theories of motion that rely on Euclidean geometry, for instance, are useful and conform to our daily-life intuitions. Yet, for the sake of truth, we might need to overcome our "instinctive preference" for the Euclidean perspective (142). This, in turn, requires what is mentioned in 2, namely, the active operation of the mind. The mind needs to free itself from the immediately given and create an alternative (i.e., non-Euclidean) framework or hypothesis that guides the subsequent empirical study of worldly phenomena. Thus, writes Le Roy, "the free activity of the mind intervenes as an essential principle in the genesis of the most positive forms of knowledge."[3]

The work of the authors that Le Roy cites suggests, however, that his portrayal of neopositivism is only partly accurate. It is doubtful that Milhaud, Wilbois, and Poincaré shared Le Roy's concerns about utility, truth, and realism (cf. 1): Milhaud (1896) appears to endorse a coherentist account of truth (in terms of "accord harmonieux d'un ensemble de conceptions"; 301) rather than a realist account; Wilbois (1899, 40) argues that the predicates *true* and *false* do not apply to hypotheses and theories; Poincaré is known to have had a more than complicated relationship with realism (for an overview, see Ivanova 2013) and, throughout his work, emphasized the convenient (rather than true) character of foundational hypotheses in physics. Still, these authors do seem to agree on 2: as Brenner (2003) suggests, they were indeed more trusting of the free and creative operation of the human mind than Comte had been (see 2). Let me briefly substantiate this point.

1. Note that French neo-positivist thought may be characterized in terms other than those mentioned in Le Roy's double thesis, e.g., in terms of its engagement with the history of science, or its engagement with Duhem's confirmation holism and Poincaré's conventionalism. I here rely on Le Roy's characterization, however, because it better helps us to understand the movement's interest in scientific discovery.

2. French epistemologists often use the term *induction vulgaire* when referring to induction by simple enumeration. Implicitly, Le Roy might here thus be criticizing Comte's inductivist approach. Alternatively, *vulgaire* might simply refer to common sense.

3. Here and elsewhere, my translation: "l'activité libre de l'esprit intervient comme principe essentiel dans la génèse du savoir le plus positif" (Le Roy 1901, 146).

Gaston Milhaud (1896, 296) challenges the positivist view that science proper restricts itself to claims that can be verified by sense experience, and that it thus excludes what Comte called nonverifiable *chimères*. Milhaud shows that the concepts that allow a scientist to formulate a natural law at all are in fact nonverifiable. For example, the concept of temperature in "phosphor melts at a temperature of 44 degrees" is nothing but a stipulation, telling us what it means for an object to have a temperature of such-and-such degrees. Force, in Newton's second law of motion, is contingent on a myriad of interdependent concepts (e.g., momentum, body) and hypotheses (e.g., Newton's law of inertia) that cannot be substantiated, neither by a priori nor empirical evidence (292). Such examples, Milhaud contends, demonstrate that in physics there is a substantive "intervention créatrice de l'esprit" and "liberté de conception" (292).

Joseph Wilbois (1899, 1900) addresses the arbitrary nature of physical theories. Each experience, according to Wilbois, is preceded by the choice of an arbitrary symbol. Light might be symbolized by a ray of propagating particles (Newton), the elongation of a molecule in an elastic medium (Fresnel), or an electromagnetic perturbation (Maxwell). The scientist is free to select such symbols (or what we would now call models), at least to the extent that their implications do not conflict with experience. Once chosen, what subsequent experiences impose on us are in fact properties of the symbol (Wilbois 1899, 604); Newton's, Maxwell's and Fresnel's frameworks are each able to account for the properties of light (e.g., polarization), and do so according to the specificities of the symbols they start from. Wilbois also hints at some of the factors that may influence the adoption of a particular symbol. The predominance of mechanistic symbols, for instance, might betray a psychological preference for convenience (613) or, because it is more pronounced in the English than among the French (614), it may be a matter of culture.[4]

Le Roy, Milhaud, and Wilbois each acknowledge their debt to Henri Poincaré (the third name on Le Roy's list). And, indeed, Poincaré earlier had characterized physical hypotheses in a way that resonates closely with the characterizations by Le Roy, Milhaud, and Wilbois. Euclidean or non-Euclidean frameworks, Poincaré writes, "are neither . . . synthetic judgments a priori nor experimental facts. Rather, they are conventions."[5] Conventional too, according to Poincaré, are many nonmathematical hypotheses, such as Fresnel's luminiferous aether (Brenner 2003, 49). Plausibly, such conventionalism is an expression of what Le Roy called the creative powers of the mind (see also Brenner 2003, 51).

4. For a similar point about the English and the French, see Duhem (1906a, 94–129).

5. "Ne sont . . . ni des jugements synthétiques a priori ni des faits expérimentaux. Ce sont des conventions" (Poincaré 1891, 773, cited by Brenner 2003, 39).

A notable omission in Le Roy's list (but not in his early work [1900] or in the bibliographies of Milhaud and Wilbois) is Pierre Duhem. But what we have just said about Poincaré, by and large, also holds for Duhem. Indeed, Brenner (2003, 51) shows that Duhem too (as early as 1892) characterized certain hypotheses in physics as arbitrary conventions.[6]

To say that neopositivists endorsed the view that scientific theories develop, at least in part, according to the free and creative operation of the human mind doesn't imply that they converged on a single doctrine. As Brenner (2013, 20–27) points out, the margin of freedom that the aforementioned authors allow for varies. Poincaré, for instance, was much more restrictive than Le Roy and Duhem in the types of hypotheses that scientists could freely adopt. Still, even Poincaré would have to concede that scientific facts do not simply impose themselves on us; for them to emerge, scientists have to posit at least some definitions and hypotheses that cannot be substantiated, and that prove useful "en fixant notre pensée" (Poincaré 1902, 24).

Let me now clarify why the new positivism was fertile ground for the philosophical study of scientific discovery. In another paper, Le Roy (1905) makes the link explicit. The old positivism restricts itself to the logical study of the results of science, to inductive inferences from specific observations to generalized theories. The new positivism, by contrast, insists that any such inference is preceded by the formulation of hypotheses, many of which are not arrived at inductively. And it is in the invention of such novel hypotheses that the free activity of the mind (cf. Le Roy's thesis 2) is most apparent: "The mind, when in a state of invention, has abandoned the realm of discourse [i.e., the logical discourse pertaining to what we would now call the context of justification], and has moved to a realm of continuity, in which it finds itself free to create" (Le Roy 1905, 206).[7] Accordingly, a complete picture of scientific development would have to add to the study of scientific results a study of the activities that allow scientific results to be obtained in the first place, more particularly, the activities associated with imagining fruitful hypotheses.

Hypothesis formation is also, as we have seen, an important theme in the philosophies of Milhaud, Wilbois, Poincaré, and Duhem. It is thus no surprise that in them, too, we see an interest in what many would now call the context of discovery (see sec. 3.1), including the psychological (for Wilbois, see above; for Poincaré, see Hadamard 1945) and social (for Wilbois and Duhem, see

6. Further, Duhem emphasized scientists' concern for convenience and freedom in adopting such conventions and, accordingly, was unlikely to have endorsed Le Roy's thesis 1.

7. "L'esprit, en état d'invention, s'est éloigné du discours, dans une région de continuité mouvante où il se sent libre de créer."

above) conditions of discovery and the conditions or principles that, in spite of scientists' creative freedom, make hypothesis formation a rational endeavor, as I discuss later. Similarly, it is no surprise that in such an intellectual climate, other French epistemologists turned to the study of discovery. Given that hypotheses appeared to be key for understanding scientific development, it was only natural to carefully study their origins.

The next section sketches the views of some of the most prominent players within this French discovery research program.

3. Turn-of-the-Century French Work on Scientific Discovery

3.1. The Logic of Discovery

Picard (1928a) makes a useful distinction concerning the art of discovery (one that has been implicit in the above). The art of discovery comprises, first, meth- **q2** ods that pertain to the *discovery* of scientific facts, in the sense of *empirically establishing or verifying hypotheses* (16) and, second, methods that pertain to the *inventionof hypotheses*. Note that Picard's distinction coincides with the Anglo-American distinction between discovery and justification. But, what French epistemologists call discovery, Anglo-Americans call justification; and what French epistemologists call invention, Anglo-Americans call discovery. Further, Picard acknowledges that processes of invention cannot always be neatly separated from processes of discovery. Still, he insists that, for the purpose of studying the methods of science, the distinction remains useful (195).[8]

Methods of discovery (or what Anglo-Americans would call methods of justification) had been a concern to philosophers at least since Bacon (Picard 1928a, chap. 1, 15–118); the originality of turn-of-the-century French thinkers primarily lies in their work on methods of the second type (methods of invention, or what Anglo-Americans would call methods of discovery)—work that, precisely because of its engagement with hypothesis formation, conformed to the tenets of the new positivism, as I defined earlier. This body of work addressed the nonlogical and logical principles of invention.

Research on *nonlogical principles* was largely initiated by Ernest Naville, a Swiss associate of the Institut de France in Paris.[9] In a monograph, Naville (1880, 123–96) introduces what he calls "les principes directeurs des hypothèses."

8. Picard's distinction has elements of two of the versions of the discovery/justification distinction that Hoyningen-Huene (2006) discusses. It recognizes distinct processes (version 1) and distinct methods (version 2) in discovery (or Picard's invention) and justification (or Picard's discovery).

9. Naville did not initiate theorizing about the role of hypotheses in science, though. In this regard, he cites many earlier writers, including Renouvier, von Liebig, Bernard, and others (see Schmaus 2007).

These are principles that guide a scientist when he needs to select, among the indefinite number of hypotheses that may account for a fact, the one that is likely most fruitful to pursue further (123). Generally, such selection is made according to the principle of unity, which manifests itself in three forms: generalization, harmony, and simplicity (137). The principle of generalization tells us to adopt the hypothesis that captures the general in the particular; the principle of harmony, the hypothesis that best accommodates the universal interdependence of things; and the principle of simplicity, the hypothesis that postulates as few elements and laws as possible to account for the phenomenon in question. Such principles were commonly discussed by subsequent authors. Duhem (1905b, 339–46), for instance, contests the rationality of principles of *simplicity* (but not the existence of guiding principles as such). Indeed, Duhem argues, recent advances in physics depended critically on scientists' willingness to renounce simplicity and to replace the simple hypotheses of the old mechanics with far more complex ones. Poincaré and Picard are not insensitive to such debunking arguments, but insist that simplicity remains important as a regulatory ideal (see Poincaré 1902, 173–74; Picard 1928a, 175–76). According to Le Roy (1901, 146), judgments of simplicity are too context-dependent to be of guidance in hypothesis selection; they betray our mind's instinctive preference for utility and, accordingly, frustrate science's realist aims. Scholars also take an interest in *harmony*. So we see Picard (1928a) reformulating Naville's principle of harmony, or rather transposing it into the principle of analogy that Lalande (1893) had introduced. Lalande's principle tells us to look for similarity in difference—in other words, for analogy; for example, despite the differences between the two species, hypothesis formation concerning human behavior might be informed by facts concerning the behavior of birds and vice versa.

Whereas nonlogical principles specify the qualities that hypotheses ideally exhibit (e.g., they must be fruitful, simple, general, harmonious), logical principles specify how scientists (ought to) reason to invent hypotheses that exhibit the qualities specified by the nonlogical principles. Accordingly, nonlogical principles pertain to hypothesis assessment, whereas logical principles pertain to hypothesis generation.

Work on such *logical principles* had analogy as a key object of study.[10] Élie Rabier (1888, 255) distinguishes between the simple perception of an analogy and the analogical inferences that are drawn from it. From a perception of a

10. That analogy sometimes also appeared in discussions about nonlogical principles of discovery (e.g., in the work of Picard) indicates that the distinction between nonlogical and logical principles is not always clear-cut. Later, I discuss Duhem's comparison between simplicity (a nonlogical principle) and analogy (a logical principle).

resemblance on certain points, the scientists infers, as a hypothesis, resemblances on other points. Subsequently, Rabier carefully argues that such analogical reasoning is the dominant mode of reasoning in the formation of each of (what he regards as) the most salient types of hypotheses in science: hypotheses concerning the existence of a law of nature, the precise form of a law, and the existence and nature of one of the terms in a law (chaps. 13, 14).

Picard (1927, 1928a) elaborates on this, arguing that to each of Rabier's types of hypotheses there corresponds a distinct type of analogical reasoning: *induction analogique, identification analogique*, and *détermination analogique*. *Induction analogique* goes from particular to general: from the observation of a property in a particular object, one infers the hypothesis that, generally, other similar objects possess the property. Or, based on the observation that, in a particular case, two phenomena co-occur, one hypothesizes that the phenomena, in a law-like fashion, co-occur in similar cases. *Identification analogique*, in contrast, goes from general to particular. From similarities observed between a particular object and a certain class of objects, one identifies the object as belonging to the class. In light of such class membership, one formulates hypotheses concerning the particular object—more specifically, one hypothesizes that what is true of the class of objects also is true of the particular object. Finally, *détermination analogique* goes from particular to particular: what is known about a particular object is, hypothetically, extended to another particular object similar to the first one.[11]

Another marked effort at understanding the role of analogy in hypothesis formation came from Picard's mentor, Edmond Goblot. Goblot (1918, 300–11) had argued that all hypotheses are suggested by *induction analogique*. By identifying two other types of analogical reasoning (*identification analogique* and *détermination analogique*), Picard thus substantively revised the views of his mentor.

Rabier, Goblot, and Picard, it appears, were united in the same project, a project that aimed to establish the principles of reasoning implied in the invention of hypotheses (i.e., analogical reasoning). Less systematic, but still significant, treatments of analogical reasoning include Ribot (1900, 22–25), Le Roy (1905, 27–31), Duhem (1906a, esp. chap. 3), and Cresson (1922, 89–110). Interestingly, Duhem (1906a) offers instructive examples of the power of analogy: the analogies between light and sound allowed Huygens to formulate his wave theory of light; the analogies between the propagation of heat and of electricity prompted Ohm fruitfully to introduce Fourier's equations into the study

11. Although Picard contends that his three types of analogical reasoning correspond to Rabier's three types of scientific hypotheses, it is unclear whether they indeed do so, especially because Picard's argument here is sketchy at best. This concern, though, is orthogonal to the purposes of my article.

of electricity; and the analogies between magnets and electric insulators enabled substantive advances in theories of magnetism and dielectric polarization. Such examples lead Duhem to claim that, in the construction of hypotheses, the use of analogy is “la méthode la plus sûre et la plus féconde” (153) and thus is to be preferred over principles of simplicity (see n. 5 and related discussion).

Importantly, the aim of many of the scholars above was normative. The work of Naville and associates on the nonlogical principles of invention was supposed to help in selecting hypotheses that are most worthy of pursuit. And the inquiries of Rabier, Goblot, and Picard into logical principles aimed to evaluate reasoning strategies in terms of their effectiveness in guiding the generation of hypotheses worthy of pursuit. That such strategies were called logical doesn’t imply they were considered to be elements of demonstrative logic. Rather, the term *logical* was interpreted in a broader sense, as pertaining to any of the methods that promote the attainment of truth (Picard 1928, 7–9). **q3**

3.2. The Psychology and Sociology of Discovery

Apart from addressing normative questions, French epistemologists also concerned themselves with projects that were descriptive in nature, in particular with the study of the psychology and sociology of invention.

Their psychological work was nonexperimental and informed by the (auto)biographies of some of science’s most creative minds, including Newton, Lavoisier, Ampère, Pasteur, Bernard, Darwin, and Helmholtz. French epistemologists used these stories to decompose the elusive notion of *le génie*. Accordingly, they closely examined the roles played in invention by memory (Ribot 1900; Le Roy 1905; Rey 1924; Picard 1928b); perseverance (Mentré 1919; Picard 1928b); imagination and intuition (Naville 1880; Ribot 1900; Dugas 1903; Le Roy 1905; Segond 1919; Rey 1924; Picard 1928b); unconscious processes (Cresson 1922; Montmasson 1928); perspicacity and curiosity (Paulhan 1901; Goblot 1918; Picard 1928b); associative and abstractive thought (Le Roy 1905; Poincaré 1922; Rey 1924; Picard 1928b); and aesthetic, egoistic, altruistic, and other effects (Ribot 1900; Paulhan 1901; Poincaré 1922; Picard 1928b).[12]

Despite their concern with the psychology of scientific genius, turn-of-the-century French epistemologists rejected the traditional view that scientific progress results merely from the work of (particularly gifted) individuals. From a study of the numerous authors who influenced da Vinci, Duhem (1906b, 1, cited in Picard 1928b) infers the collective nature of discovery: “Big discoveries

12. It is beyond the scope of the current article to review the different positions that French epistemologists took on all of these issues. For such a review, see Picard (1928b, 101–216).

are almost invariably the result of a slow and complicated process of preparation, a process that is pursued over centuries. The doctrines of the most powerful thinkers result from a multitude of efforts, accumulated by a pool of obscure workers."[13] The history of statistics (Duhem 1905a) and the history of physics (Duhem 1906a) teach him the same lesson. Boutroux (1914) and Brunschvicg (1912) establish the collective nature of discovery by means of examples from the history of mathematics.

Additionally, French scholars at the time investigated the social determinants of discovery. De Candolle (1873) provides a detailed statistical study of the social conditions that favor scientific development, including religion, institutions, governments, language, funding, education, tradition, the size of the pool of scientists, and the proportion of mathematicians to natural scientists. Various other authors examined the effects of culture on scientific productivity and the distinct types of science that cultural groups (e.g., the ancient Greeks, Hindus and Arabs, the French, the English, the Germans) pursue (see Wilbois 1899; Duhem 1906a; see also Duhem 1915; Goblot 1922; Rey 1924). Picard, in particular, deals with the organization of scientific research. He observes that science has increasingly come to serve industry and governments at war and laments the corresponding "crise de la science pure" (1928b, 90–91). In order to address this crisis of pure science, and to stimulate research as such, he proposes various reforms: in the recruitment of scientific personnel (to select personnel based on research rather than teaching qualities [294–95]); science funding (to decentralize research and multiply the number of laboratories [296] and to increase the overall governmental budget for science [300]); education (to stimulate, following Dewey, experiential learning [297–98, 310–14] and to cultivate curiosity [309]); rewards (to provide researchers with incentives to excel [299]); international cooperation among scientists (to promote cooperation by protecting intellectual property rights [301–2]); and scientific labor (to Taylorize scientific production processes [303–4]). So, even when dealing with the sociology of science, Picard did not eschew taking up normative questions.

4. Revisiting the History of the Philosophical Study of Scientific Discovery

As I noted in the introduction, several authors have attempted to write a history of the philosophical study of scientific discovery during the nineteenth and twentieth centuries. One such account is by Larry Laudan (1980), who,

13. "Les grandes découvertes sont presque toujours le fruit d'une préparation lente et compliquée, poursuivie au cours des siècles. Les doctrines professées par les plus puissants penseurs résultent d'une multitude d'efforts, accumulés par une foule de travailleurs obscurs."

interestingly, was himself part of the Anglo-American postpositivist movement that put scientific discovery on the agenda (again). According to Laudan, the logic of discovery (or what French epistemologists would call the logic of invention) was abandoned during the 1830s and 1840s because the divorce of justification from discovery had by then become orthodoxy. Before that era, the enterprise of articulating a logic of discovery served a justificatory purpose. Scholars hoped to develop an infallible way of discovering true or useful theories; the infallibility of the process would guarantee that theories were epistemically well grounded. With the decline of this infallibilist program (which Laudan attributes to the work of Herschel, Whewell, and Comte), there occurred a natural shift from the analysis of the genesis of theories towards the post hoc justification of theories. Because one now acknowledged that theories couldn't be proven to be true but merely shown to be probable or likely, justifying them by the fallible procedure of reasoning from consequences to antecedents became viable. Theories now could be assessed independently of a knowledge of their generation, and the logic of discovery was abandoned.

Laudan notes two exceptions to this trend: Peirce's (1908) and Hanson's (1958, 1960) work on retroduction (also known as abduction). According to Peirce and Hanson, it is through the mode of retroduction that scientists formulate plausible hypotheses. In the words of Peirce: "The complexity of experience may be seen to be embraced under a unitary concept, or idea; and [in] this search he [the scientist] is at length thus led to some more or less reasonable *conjecture*, which would subsume if positively asserted the facts of experience under such a concept. This is the first and logical stage of inquiry. I call it retroduction because it starts at consequents and recedes to a conjectural antecedent from which these consequents would, or might very likely logically follow."[14]

Laudan argues, however, that retroduction does not tell us how to invent hypotheses. Rather, it assesses the plausibility of hypotheses and, accordingly, whether a given hypothesis is worthy of pursuit. Since Laudan construes discovery as concerned only with the eureka moment of invention (i.e., the time when a new idea first dawns), retroduction doesn't qualify as a logic of discovery.[15] Neither Peirce nor Hanson, Laudan concludes, invalidates his claim that the project of finding logics of discovery has been dead since the 1840s.

14. Peirce (1908, cited by Pietarinen and Bellucci 2014). See also Hanson's (1960, 104) schematic representation of the argument.

15. Laudan, like many other authors, seems to construe Peircean retroduction as a form of inference to the best explanation, for he writes: "[Retroduction] tells us . . . when an idea is worthy of pursuit (namely, *when it explains something we are curious about*)" (1980, 182). Pietarinen and Bellucci (2014), though, demonstrate the inadequacy of such an interpretation. According to Peirce, a retroductive hypothesis may very well explain any actual fact, but it does not need to. What matters is not the explanatory

Further, Laudan claims that any attempt to revive such a project would lack a philosophical rationale. According to Laudan, studying the genesis of hypotheses is a descriptive endeavor and, accordingly, should be left to the empirical sciences (sociology, psychology, and history of science); as a normative discipline, philosophy ought to restrict itself to science's justificatory practices.

Schickore (2018) agrees with Laudan that interest in the logic of discovery waned concomitantly with the rise of the discovery and justification distinction, and she largely accepts Laudan's assessment of Peirce and Hanson. Yet, Schickore adds that the early twentieth century witnessed a few scattered attempts at demonstrating the existence of heuristic reasoning strategies in the eureka moment of invention; she cites Schiller (1917), Carmichael (1922), and Benjamin (1934). These scholars recognized that such strategies did not conform to principles of demonstrative logic but insisted that they were systematic enough to be called logical. Interestingly, Schiller (1917) and Benjamin (1934) hint in passing at the power of analogical reasoning in the generation of new ideas. But, Schickore suggests, serious philosophical work on analogy and discovery started only in the 1960s, following the publication of a monograph by Mary Hesse (1966).

Regarding the history of the psychology of science, Feist's (2006) historical reconstruction starts with Galton (1874). The latter book reports on a survey Galton conducted among Fellows of the Royal Society, a survey that aimed to assess the respective contribution of nature and nurture to scientific genius. Systematic work on the psychology of science, according to Feist, did not begin to appear until the 1950s. Plausibly, Feist intentionally restricted himself to what happened after the psychology of science had detached itself from the history and philosophy of science—although, in this regard, his inclusion of Galton is an exception. Histories of philosophical work on the psychology of science, it seems, still need to be written.

Also lacking are histories that specifically deal with the philosophical study of the sociology of discovery. Still, Longino (2019) sketches the history of philosophical approaches to the general sociology of science. She notes a couple of precursors to the body of work produced by Anglo-American postpositivists in the wake of Kuhn (1962), namely, Mill's (1859) argument for the collective nature of scientific achievements, Peirce's (1878) consensualist theory of truth, and Popper's (1963) account of the importance of criticism in the development of scientific achievements.

power of the hypothesis but whether knowledge that the hypothesis is actually true would produce knowledge of the facts observed. Accordingly, Laudan might have been too quick in characterizing Peircian abduction as a form of assessing hypotheses, particularly in terms of explanatory power.

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In light of my story, the pictures painted by Laudan, Schickore, Feist, and Longino appear significantly inaccurate. Laudan and Schickore are mistaken that the logics of discovery project was largely abandoned in the 1840s in response to the widespread acceptance of the justification/discovery distinction: turn-of-the-century French scholars within the discovery program accepted that distinction but did not discontinue the project. More specifically, they thought, indeed, that discovery (or, in French terminology, invention) and justification (or, in French terminology, discovery) were different processes, which involved different methods (see n. 3). But they added that a proper understanding of scientific development requires a detailed account of both discovery and justification. Further, French epistemologists' logics of discovery was a normative project. Laudan, thus, appears mistaken in thinking that discovery can be meaningfully studied only by nonphilosophical (i.e., empirical) means. To be sure, some of the French epistemologists' work, more specifically their work on what I have called the nonlogical principles of discovery, deals with the assessment of hypotheses rather than with the eureka moment of invention, and thus, at least if we follow Laudan's demarcation criterion, it falls outside the logics of discovery. Yet, we have seen that French epistemologists also concerned themselves with the logical principles of the eureka moment. And we have seen that many of their inquiries into that phenomenon pertained to what Schickore writes was not systematically investigated until the 1960s, namely, analogical reasoning.

My story also undermines Feist's picture, according to which the psychology of science, before it became an independent field of inquiry, had been a concern only to Galton. In fact, Galton (1874, v–vi) was responding to de Candolle (1873; see also Hilts 1975). Further, because Galton's primary interest was sociological, he discussed only a couple of the many psychological traits that French epistemologists associated with scientific genius.

Finally, Longino's reconstruction is incomplete in two senses. First, it ignores a substantive body of work that preceded work that was done after the collapse of the logical empiricist consensus on the social dimension of science, a collapse that, according to Longino, was principally brought about by Kuhn (1962). Second, Longino's historical account primarily pertains to approaches that deal with the social dimensions of hypothesis assessment. My story indicates that her account needs to be complemented with an account that covers the philosophical study of the sociology of hypothesis generation (or invention).[16]

16. Longino's account is incomplete in yet another sense. The psychology and sociology of science seemed to have been an important field of inquiry for Anglo-American philosophers of science also during the supposed logical empiricist consensus (see Howard 2003).

5. Discussion

This article has uncovered a forgotten research program within French philosophy of science and described how that program has been overlooked by scholars working on the history of the philosophical study of discovery. There are a number of important issues that space constraints prevent me from properly addressing here. To begin with, my principal aim has been to substantiate the existence of a thriving discovery program within French philosophy of science at the turn of the century. Accordingly, I gave the reader a sense of the impressive number of authors who were part of the program, and of the impressive number of topics that they dealt with, rather than giving a detailed picture of what they had to say about those topics. Now that the program has been rediscovered, the natural next step is to properly analyze its contents.

My study raises questions about the relationship between the French discovery program and a similar program that took off in the Anglo-American world around the 1960s, according to Laudan, Schickore, Feist, and Longino. A cursory comparison of the two programs suggests that they might have converged on a number of points (e.g., the role of cognitive values in theory selection; the role of analogical reasoning; the psychology of scientific genius; the integration of the history, philosophy, psychology, and sociology of science). Such convergences, if properly established, would raise questions of origins. One possibility is that Anglo-American scholars were familiar with the French program and borrowed and further developed some of its ideas. Kuhn, for one, seems to have been acquainted with the work of at least some of the players in my story (Duhem and Brunschvicg; see Simons 2017; also Metzger, Koyré, and Piaget). Another possibility is that the convergences result from independent evolution. On this account, the two programs were similar responses to similar (i.e., positivist) environments.

Another question that my story raises concerns the relationship between the French discovery program and subsequent French epistemologies. On the face of it, many of the topics that the program was concerned with—such as the logical and nonlogical principles of discovery, the psychology of scientific genius, the organization of scientific research—were abandoned after two monographs by Picard (1928a, 1928b). Other topics, however, continued to be investigated in the French tradition. For instance, Hélène Metzger (1930, 1937; see Freudenthal 2015) studies the imaginative and creative activities of the mind in the invention of novel hypotheses. Bachelard develops a psychology of science that, in more than one respect, echoes French philosophies at the turn of the century. For one, Bachelard (1934) insists, as Le Roy (1905; see sec. 2) had done, that in order to attain objective knowledge, the mind needs to overcome instinctive ideas (and this, according to Bachelard, can be achieved by a form of psychoanalysis; see Chimisso 2008). Also, like Duhem (1905a, 1906b), Boutroux (1914), and Brunschvicg (1912), Bachelard (1949) recognizes the collective nature of scientific inquiry (his psychology, then, is one that intends to promote interactions that are conducive to objective knowledge among the actors in the collective, viz., teachers and pupils; see again Chimisso 2008). Another prominent figure that integrated psychological research into his philosophy of science is Piaget, who studied with Brunschvicg and Lalande. In a series of lectures, Piaget (1970) aimed to show that the psychological development of individuals, more specifically their development of knowledge, is isomorphic to how scientific knowledge develops. Finally, Koyré (1963, cited by Chimisso 2008) critically engages with but ultimately rejects explanations of scientific knowledge that invoke cultural factors (explanations of the kind endorsed by, e.g., Wilbois [1899, 1900] and Duhem [1906a]).[17] **q4**

What is striking, though, is that Metzger, Bachelard, Piaget, and Koyré hardly refer to the scholarship discussed in section 3. Possibly, this means that their projects were part of a phylum that developed out of French neopositivism largely independently from the discovery phylum. In order to assess this possibility, one would need carefully to study the interactions among the main players across the phyla, the similarities and differences between the phyla, and the ancestry of observed similarities. Plausibly, such a study will also help us explaining why one phylum, in terms of lasting impact, turned out to be so much more successful than the other. In any case, the French discovery program provides ample room for further study.

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17. Kuhn, through reading Merton, was familiar with Piaget's work (see Kuhn 2000, 279). Furthermore, in the preface of the 1970 edition of his *Structure of Scientific Revolutions*, Kuhn approvingly refers to Metzger and Koyré (Simons 2017).

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