From Positivism to Conventionalism: Comte, Renouvier, and Poincaré

Abstract

Considered in its historical context, conventionalism is quite different from the way in which it has been caricatured in more recent philosophy of science, that is, as a conservative philosophy that allows the preservation of theories through arbitrary ad hoc stratagems. It is instead a liberal outgrowth of Comtean positivism, which broke with the Reidian interpretation of the Newtonian tradition in France and defended a role for hypotheses in the sciences. It also has roots in the social contract political philosophy of Renouvier, who explicitly drew the analogy between conventions in political life and the conventional acceptance of hypotheses in the sciences, and conceived a philosophy that permits scientists to set aside foundational worries and explore new ideas. Although Poincaré and Renouvier may have hesitated to accept certain then recent developments in mathematics and the sciences such as non-Euclidean geometries, this conservatism cannot necessarily be attributed to their conventionalism. It may instead reflect the engineering background they shared with Comte, which emphasizes practical applications. Although Renouvier and Poincaré may have seen no practical use for these new ideas, unlike Comte they did not prohibit others from pursuing them, reflecting conventionalism's more liberal attitude toward recent developments in the sciences.

Keywords: Comte, Conventionalism, Hypothesis, Poincaré, Renouvier

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1. Introduction

Poincaré's conventionalism has often been read as a conservative philosophy of science. Philosophers such as Popper, Lakatos, and Reichenbach have dismissed conventionalism as a philosophy that allows scientists to arbitrarily adopt ad hoc maneuvers to preserve pet theories, holding back scientific progress. Hence, for instance, Poincaré has been criticized for his resistance to non-Euclidean geometry, relativity theory, and Cantor's contributions to number theory.

However, to regard conventionalism as scientifically conservative and licensing arbitrary choices is to fail to consider it in its full historical and philosophical context. Previous scholars such as Mary Jo Nye (1979), Jerzy Giedymin (1982), Helmut Pulte (2000), Roberto Torretti (1984), and María de Paz (2014) have considered Poincaré's conventionalism in the context of other contemporary developments in the sciences, mathematics, and epistemology. But to the best of my knowledge nobody has taken into account developments in political philosophy, where the concept does not necessarily entail that of an arbitrary choice or decision.

French philosophy of science developed largely outside of academic philosophy among thinkers educated in mathematics and the sciences. The key figures in this paper – Auguste Comte, Charles Renouvier, and Henri Poincaré – were all educated at the École Polytechnique. These thinkers broke with the foundationalist epistemology that dominated academic philosophy in France. Academic philosophers received a largely humanistic and literary education with little exposure to the sciences. Towards the beginning of the century, Pierre Paul Royer-Collard had introduced into France Thomas Reid's commonsense empiricism, which Victor Cousin subsequently blended in an eclectic mixture with Cartesian foundationalism and the method of introspection, along with ingredients from the philosophies of Leibniz, Condillac, Maine de Biran, and others. Following Reid's interpretation of Newtonian methodology, Cousin and his followers proscribed the use of hypotheses in science. Cousin even went so far as to praise Descartes' work for being "free of hypotheses", thus displaying little understanding of Descartes' – or anyone else's – actual work in science (Cousin 1860, 3). Yet this eclectic spiritualist philosophy dominated the university and was even a required course in the lycées.

Comte was one of the earliest thinkers in France to reject Cousin's philosophy and to defend of the use of hypotheses in science (Naville 1895, ix – xi). Other early French proponents of the method of hypothesis were also connected with the École Polytechnique, including André-Marie Ampère as a faculty member, and Sadi Carnot, who, like Comte was a student while Ampère taught there (Naville 1895, 249-53). This school prepared students for advanced study in engineering by providing them with a thorough grounding in mathematics, physics, and chemistry, taught by the leading mathematician and scientists of France. Although the relative emphasis on theory versus application at the École Polytechnique itself varied over the decades, it was nevertheless a feeder school for the schools of application, and students' education there may have produced in them a tendency to consider the practical applications of science. Poincaré, for example, went on to study at the École des Mines.[1]

Renouvier had already begun reading the first volume of Comte's *Cours de philosophie positive* as a teenager, before matriculating at the École Polytechnique in 1834 (Hamelin 1927, 7), where Comte was teaching mathematics as a *répétiteur* (Belhoste 1994, 26).[2] Renouvier initially adopted Comte's positivist views, but then subsequently defended an even broader role for hypotheses in science, and ultimately argued that all of the sciences rest on hypotheses adopted as conventions. As I have argued elsewhere, Renouvier, drawing on the social contract tradition in political philosophy, made an explicit analogy between the role of conventions in political and social life on the one hand and in the sciences on the other hand (Schmaus 2017; 2018).

Interpreted in this historical context, conventionalism turns out to be a liberal rather than a conservative philosophy of science. For Renouvier, science works best when scientists are free to adopt whatever conventions they find most suitable for advancing their inquiries, and to modify or even adopt new conventions when the old ones no longer prove to be fruitful guides to research. His conventionalism thus incorporates a voluntary but not arbitrary element in our knowledge. It was not Renouvier's or Poincaré's conventionalism itself so much as the conventions that they chose to adopt that were conservative. Their conventionalism was liberal insofar as they maintained that scientists and mathematicians should be free to adopt even those conventions with which these philosophers may have disagreed. Conventionalism granted greater intellectual liberty to scientists to try new things than did foundationalist philosophies of science, whether empiricist or rationalist, such as those that had been defended by Cousin's followers. Whatever conservatism about new developments in science we may find in Poincaré or Renouvier may reflect their preference for theories and methods that had more obvious and immediate practical applications rather than anything in the conventionalist philosophy of science.

2. Comte

Of the three thinkers under consideration here, Comte is the one whose scientific conservatism may most justifiably be attributed to his philosophy of scientific method. He notoriously restricted astronomy to the study of the geometrical and mechanical phenomena presented by celestial bodies and ruled out the investigation of their chemical composition, their mineralogical structure, and the physiology and sociology of any organized beings who may live there, maintaining that such phenomena were inaccessible to observation (*Cours* I, 301-3).[3] He was nevertheless very forward-thinking in defending the use of hypotheses, contrary to the French followers of Reid, finding hypotheses indispensable to scientific inquiry, as long as they were testable.

Comte argued for the need for some theory to guide observation in the very first lesson of the *Cours de philosophie positive:*

> In order to apply itself to the making of observations, our mind has need of some sort of theory. If, in contemplating the phenomena, we do not immediately attach them to some principles, not only would it be impossible for us to combine these isolated observations, and, consequently, to draw any fruit from them, but we would even be entirely incapable of retaining them; and, most often, the facts would remain unperceived before our eyes. (*Cours* I, 23 ; 1988, 5)[4]

Comte made similar remarks about the need to attach observations to theory in his discussion of the methods of observation appropriate to the study of social phenomena (*Cours* II, 139). He then added that any attempt at pure empiricism risks resulting in our observations supporting unspoken metaphysical assumptions. It is better that our observations be guided by explicit positive hypotheses (*Cours* II, 141). Comte had questioned naïve empiricism and recognized the need for some theory to guide observations as early as his “Considérations philosophiques sur la science et les savants” of 1825:

> Absolute empiricism is impossible, no matter what has been said about it. Man is incapable by his nature not only of combining facts and of deducing any consequences from them, but even simply of observing them with attention, and of retaining them with certainty, if he does not immediately attach them to some explanation. (Comte 1825, in *Système* IV, appendix, 141, trans. 592–93; 1998, 149)[5]

The role of hypotheses in the empirical sciences comes out most clearly in Comte’s account of the methods of astronomy. In the history of this science, empirical observations consisted simply of measuring the coordinates of points of light in the sky and recording the times of these measurements. Astronomical phenomena such as the paths of the planets, the daily motions of the celestial bodies, and even the shape of the earth “are for the most part essentially constructed by our intelligence” (*Cours* I, 305). Comte’s understanding of the concept of astronomical phenomena thus compares with that of Newton, for whom it included the facts that the orbits of the planets and their satellites obey Kepler’s laws. In astronomy, Comte said, “the mind must construct the form or the motion which the eye could not embrace. . . . The simple geometric sketch of the diurnal motion would remain impossible without an abstract hypothesis that one compares to the concrete spectacle, in order to connect the celestial positions” (*Système*, I, 500, trans. 405).

For Comte, hypotheses in science need not be strictly true in order to guide research and contribute to scientific progress. In his opening lesson in the *Cours* on biology, he wrote about the use of hypotheses in the investigation of the unknown functions of known organs, or the hidden organs responsible for known functions: "If the hypothesis is not at all exactly true, as it ought to happen the most often, it will for that reason no less always necessarily contribute to the actual progress of the science, in directing the whole of the active researches towards a clearly defined goal" (*Cours* I, 720). The only condition he put on such hypotheses is that they are susceptible of "positive verification" (*Cours* I, 720). This condition is linked to the communal character of scientific research. For Comte, science depends on a community of researchers who will check each other's work: "Either to observe or to meditate, every mind always depends on others, who prepare his materials and verify his results. . . . The boldest innovator rarely acquires a full confidence in his own discoveries, as long as he has not obtained some freely given approval." (*Système*, II, 386, trans. 314.)

According to Comte, a hypothesis is an "anticipation of what reason and experiment would show immediately if conditions were more favorable" (*Cours* I, 457). He distinguished two kinds of hypotheses: those "relative to the laws of phenomena", which he found admissible, and those that "concern the determination of the general agents which are to account for the various kinds of natural effects", which he found anti-scientific, hindering progress in science (*Cours* I, 458). Comte criticized physicists for not following the model of astronomy more closely and limiting themselves to investigating the laws governing phenomena, instead of speculating about their underlying causes. He proscribed hypotheses that postulated ethers and fluids to explain the phenomena of heat, light, electricity, and magnetism, finding them untestable. In optics, he was equally opposed to the emission and the wave theories of light (*Cours* I, 458). He credited Ampère with having made "electrology" a positive science by replacing metaphysical hypotheses concerning fluids with mathematical laws (*Cours* I, 548). He claimed that Ampère had adopted the hypothesis that the action of an indefinitely long rectilinear conductor on a magnetic needle varies inversely with the shortest distance between them, which was then confirmed as a law through the experiments of Savart and Biot (*Cours* I, 555).[6]

Comte was not opposed to the molecular and atomic hypotheses of matter. The difference is that fluids and ethers at that time were defined as invisible in principle, whereas atoms and molecules are just too small to see. Comte permitted the postulation of unobservable entities in the sciences as long as they could be represented spatially, thus allowing for the application of mathematics for the purpose of deriving test implications. He thought that the luminiferous ether postulated by the wave theory of light did not meet this condition. Comte's objections to the emission theory were more empirical. He protested that the particles of light would have to travel at incomprehensible, inexplicable speeds, and also referred to Euler's objections (*Cours* I, 531), which had to do with the emission theory's difficulties with explaining diffraction, and the fact that light rays can penetrate each other without disturbing each other.

Although Comte allowed for the postulation of atoms and molecules, he maintained an instrumentalist attitude towards them, warning his readers against assigning a "faulty reality" (*une réalité vicieuse*) to them (*Cours* II, 736). He compared the atomic theory to the concept of inertia in mechanics, and cautioned us not to think that either concept "exactly represents external reality" (*Système* I, 520, trans. 421). Although Comte believed that there is what he called a "universal order" in nature, he maintained that science should aim to represent this structure only to the extent we need to know it for practical purposes (*Système* IV, 175, trans. 155).

While Comte recognized the role of hypotheses in the construction of astronomical phenomena, he failed to recognize the role of mathematical conceptual work in the construction of the basic concepts of mechanics, according to Michel Blay. He says, for instance, that Comte overlooked the conceptual problems relative to the transition from Newton's mechanics to the analytical mechanics of Lagrange. Comte simply took the continuity of time, space, and speed for granted, and took no notice of the fact that Leibniz had to argue for what he called the "law of continuity" against the advocates of discontinuism. According to Blay, it was only after Leibniz had defended the principle of continuity that the kind of rational mechanics Comte favored became possible (Blay 2018, 57-59).

Comte maintained that the basic principles of mechanics are drawn directly from observation, criticizing those who thought otherwise. He said that the improvement of rational mechanics through the use of mathematical analysis has led to the habit of seeing nothing but simple questions of analysis in this science, and even to the attempt to "establish a priori through purely analytical considerations ... the fundamental principles of this science, which Newton wisely presented as resulting from pure observation." Comte then argued that mathematical analysis can be used only to deduce one thing from another, not to establish the basis of a science: "That which establishes the reality of rational mechanics, is precisely, on the contrary, to be founded upon some general facts, immediately furnished by observation . . ." (*Cours* I, 227-28).

However, the basic principles of mechanics for Comte were not the same as they were for Newton. He reformulated them in such a way as to eliminate any reference to what were for him unacceptable metaphysical notions, in particular to forces acting as causes, specifically to continuously acting forces resulting in accelerations. For Comte, the three basic principles of mechanics include:

1. Newton's first law, the law of inertia (*Cours* I, 233)

2. Newton’s third law, that for every action there is an equal and opposite reaction (*Cours* I, 235)
3. “The principle of independence or the coexistence of motions, which immediately leads to what is commonly called the composition of forces” (*Cours* I, 236)

The last one, according to Blay (2018, 65), has its source in Huygens. Newton’s second law, that the change of motion is proportional to the motive force impressed, and takes place following the straight line in which that force is impressed, is not included.

It is astonishing that Comte could have thought that the principles of mechanics were simply derived from observation, even given his formulation of them. Indeed, it is not entirely clear that he did think that, as we have seen that he said subsequently in the *Système* that we should not think the concept of inertia represents external reality (*Système* I, 520, trans. 421). Nevertheless, it would appear that for Comte, what he maintained about the role of hypotheses in constructing the concepts used to describe the phenomena in astronomy may have been less true for concepts in the other sciences. Indeed, in his account of the methods of astronomy, Comte said that reasoning plays a more important role in this science than it does in all the others, and that “Nothing truly interesting is ever decided there by simple inspection, contrary to what happens in physics, chemistry, physiology, etc.” (*Cours* I, 305).

Criticisms of Comte’s notion of positive science, similar to Blay’s, were in fact raised even in the nineteenth century. William Whewell argued that, contrary to Comte, metaphysical discussion has been inextricably bound up with the discovery of the laws governing the phenomena in celestial mechanics, optics, and chemistry (1860, 227-28). Antoine-Augustin Cournot questioned whether scientific progress in fact does consist in the gradual elimination of metaphysics. For Cournot, philosophy and metaphysics are inseparable from the theorizing that distinguishes a true science from a mere aggregate of empirical facts (1872, vol. 2, 223-26).

There is thus a tension in Comte between what he said about the role of hypotheses in astronomical observation and what he did not recognize about the basic concepts of rational mechanics and other sciences. Comte could not accept that the sciences rested on a priori theorizing, and professed that their basic concepts could be easily drawn from observations. The problem was somewhat compounded when he said that what counts as a contribution in science depends on the freely given approval of the members of the scientific community, which suggests that discussion and consensus formation must play some role in validating scientific knowledge. One wishes that Comte had said more about this process. Ultimately, Renouvier and Poincaré would resolve these difficulties by developing a third alternative to both a priori and empirical principles, in which, at least in mechanics, the basic principles are accepted as conventions.

3. Renouvier

Although Renouvier was never attracted to Comte's authoritarian political philosophy, he was still defending a positivist philosophy of science when he published the first of his *Essais de critique générale* in 1854.[7] He maintained that science should be limited to investigating the laws governing phenomena and that metaphysical notions such as substance, cause, and force should be banished from science. For instance, he found hypotheses that treated heat, electricity, and light as inherent qualities or powers in matter difficult to verify, and he said: "Physics will attain its positive state when it is permitted to substitute for the qualities that it studies motions with known laws, produced in defined environments, and all without hypotheses" (1854, 229; 1912a, 2:67).[8] Nevertheless, he allowed for the use of provisional hypotheses for purposes of investigation, exploration, and discovery (1854, 91-93; 1912a, 1:112-13).

Like Comte, Renouvier allowed for hypotheses concerning unobservable entities and processes as long as these hypotheses were testable and interpreted instrumentally. He even permitted provisional hypotheses in the construction of systems of classification in natural history (1859, 507-11; 1912b, 2:153-56). The main difference from Comte is that Renouvier gave wider scope to the instrumental use of hypotheses. In addition to their roles in anticipating laws and guiding experiments, they could also be employed to link or coordinate the facts when the laws governing the phenomena are unknown. Such hypotheses can serve as aids to memory and reason by reducing the phenomena to more general facts. Renouvier also found them necessary for teaching physics. He cited the luminiferous ether as a good example of this use of hypotheses (1859, 512–13; 1912b, 2:157–58).

Unlike Comte, Renouvier regarded the luminiferous ether as at least an indirectly verifiable hypothesis, requiring additional assumptions in order to test it (1859, 514; 1912b, 2:158-59). He gave a somewhat different account after Maxwell published his mechanical model of the ether in 1861-62 (Maxwell 1861-62), which was after Comte had passed away. In 1873, Renouvier explained that hypotheses that postulate unobservable entities or processes in order to explain the mode of production of some phenomenon can be testable as long as the mode of production is physical or mechanical (1873b, 344). The following year, he also defended the hypothesis of a gravitational ether, as suggested by Newton in queries 21 and 22 in the *Opticks* (1874a, 5-8). Renouvier's reasoning here is like Comte's in the case of the molecular hypothesis, in that Renouvier saw no reason to rule out hypotheses concerning things that are too small to see with the naked eye, as long as they can be represented spatially, and he considered the luminiferous and gravitational ethers to consist of particles that could be represented in this way.

Throughout the 1870s, Renouvier continued to defend an expanded role for hypotheses in science, even arguing that they were indispensable for constituting the very objects of study that gave unity to each of the sciences (1873b, 340-41; 1876, 402). Any statement that went beyond the facts or the principles of the understanding was a hypothesis for Renouvier (1912a, 2:14). There was another at least equally important difference between Renouvier and Comte as well. Beginning with his second *Essai*, in 1859, Renouvier characterized the sciences as each resting on a social contract, much like societies in general. For Comte, on the other hand, the very idea of a social contract belonged to what he regarded as the outdated metaphysical stage of social thought. However, the social contract concept gave Renouvier an advantage over Comte, in that it allowed him to explain the source of the normative force of the methodological rules and basic principles of the sciences. Just as in society at large, whatever authority these contracts carry is contingent upon the freely given consent of the parties to the contract.

According to Renouvier, political history and the history of science reveal a series of social contracts succeeding one another. When the limitations of these contracts are brought to light, revolutions ensue. There is a pattern of alternation between periods emphasizing authority and periods emphasizing individual liberty. Schools of thought may be treated as authoritative for a while, but as liberty increases, what people once accepted on authority is subsequently questioned and challenged with observation, experiment, and reasoning (1859, 551-56, 559-61; 1912b, vol. 2, 197-201, 203-5; 1864, li-lii; 1912c, xli). In the third *Essai* of 1864, he explained that revolutions bring greater clarity to the social contracts:

> Revolutions are produced in methods, in the sciences, in society, for the reason that the personal contract and the social contract must become explicit and voluntary, for the truth just as for the good. Old, obscure clauses are clarified and take the form of laws; judged unjust or false, corrected or even annulled. (1864, lii–liii; 1912c, xli–xlii.)

Renouvier’s concept of the personal contract is analogous to Kant’s notion of duties to oneself, which make the social contract logically possible. For Renouvier, the search for greater clarity is a communal effort, involving deliberation and consensus formation. The social contract is a living contract, constantly open to reexamination, reappraisal, and reformulation. According to Renouvier, people will continue to see themselves as united in a common enterprise across time, regardless of these revolutionary interruptions (1864, liii; 1912c, xlii). Although Comte also had a concept of scientific revolution (e.g., 1820, 32; *Cours* I, 112n, 119, 170, 199), he had little to say about the process through which scientific revolutions occur or how they are like or unlike political and social revolutions.

In the 1870s and 1880s, Renouvier’s notions that the sciences rest on both hypotheses and social contracts appear to have come together in his idea that the sciences ultimately rest on *conventions*. He had used the term convention decades earlier to characterize numbers other than positive integers, but in this context it meant a useful fiction (1854, 143-49, 466; 1912a 1:225-30, 248). His earliest reference to conventions in the sciences occurred in an 1873 article titled “Proposition d’un nouveau critère de certitude”, which dealt with the possibility of philosophy becoming a science. In this article, Renouvier said that before philosophers can accept a criterion of certainty, or presumably any other such convention, they must first form a social contract that establishes them as a scholarly community in which they recognize each other as “moral agents in the realm of thought" (1873a, 198), thus implying that scientists must do the same. As moral agents within this community, they agree to engage in discourse about rules and standards and to be bound by them until such time as they agree as a community to change them. This basic social contract makes it possible to discuss and then accept particular assumptions, concepts, and methods as conventions and gives whatever conventions are adopted their normative force.

A decade later, Renouvier gave a somewhat different account of what he considered conventions in sciences ranging from mathematics and mechanics to physiology and natural history. He argued that each of the sciences is constructed upon postulates or hypotheses that rest on the "consent and agreement" of the scientists who pursue the field, "who maintain among themselves the convention of not submitting them to examination" (1885, 7). Many of the conventions in the sciences take the form of agreeing not to raise certain foundational issues, especially of a metaphysical or epistemological sort. For instance, biologists are precluded from inquiring into whether animals have souls and freedom of the will. The sciences, he said, "cannot actually be constituted except on the condition that they exclude from their researches certain questions that would be the first to resolve if they fell within the province of scientific methods. Without this renunciation, they [the sciences] would not obtain the assent that is necessary to them" (1885, 8). In mechanics, for instance, there is an agreement not to inquire into the nature of mass or force, "and on what authority one affirms the principle of inertia or such other principle indispensable to the foundation of this science" (1885, 7). Renouvier thus appears to have recognized that Comte could only pretend that the basic principles of mechanics were simply drawn from experience. But instead of maintaining that they rested on a priori foundations, Renouvier avoided both horns of Comte's dilemma and proposed that we should regard these principles as conventional, much as Poincaré was soon to do as well.

In his philosophical essays, Renouvier characterized scientific revolutions as having to do with clarifying the conventions and social contracts that guide research. A somewhat different picture of scientific revolutions emerges from his accounts of episodes in the history of science. In an article titled “La Physique de Descartes”, Renouvier characterized Newton’s work as revolutionary in comparison with Descartes’. It was revolutionary in method, including greater empirical rigor, quantitative precision, and the use of mathematics for drawing out test implications:

> The more considerable and profound revolution that he [Newton] brought to the method of the sciences, and to which one largely owes the positive and decisive progress accomplished since the moment of his discoveries, consists in granting a preponderant part to systematic experience in the preparation of inductions, in the establishment of their premises, in the verification of hypotheses; to recognize its exclusive right to motivate judgments without appeal, and finally to give explanations a sufficiently precise character to lend themselves to calculation and lead to deductions for which experience is called either to confirm or deny the results (1874a, 5).

Renouvier thought that Descartes, on the other hand, had made only a superficial and summary use of experience (1874a, 5).

Renouvier’s reference to positive progress since Newton’s discoveries perhaps reflects a continuing Comtean influence. However, his comparison of Newton with Descartes also reflects the influence of Fontenelle, whose *Éloge de Newton* Renouvier quoted on the very same page. In the passage he quoted, Fontenelle contrasted Descartes, who he claimed began from “clear and fundamental ideas” and descended to the phenomena of nature as necessary consequences, with

Newton, who he said took a more timid and modest approach, starting from the phenomena and ascending to “unknown principles”, admitting them regardless of their consequences. Descartes began with what he clearly understood in order to find the cause of what he saw, while Newton began with what he saw to find the cause, either clear or obscure (1874a 5, n. 1). That is, Fontenelle held that there is greater clarity in Descartes’ works because that’s what Descartes sought, while Newton followed the empirical phenomena wherever they led, whether or not the results were clear. Renouvier echoed Fontenelle again in a second article that year, in which he found that the subject matter of Newtonian physics was less clear than that of Cartesian physics (1874b, 129).

There was an ambiguity in Newton’s celestial mechanics that Renouvier recognized. He saw that Newton had been read as admitting attractive forces and action at a distance, but could also be read as having sought purely mechanical explanations of gravitational phenomena. Renouvier maintained that the second was the correct interpretation, and over time he became increasingly critical of those who attributed forces and non-local action to Newton. In an 1882 article on Descartes and Newton, he said that Newton was perfectly clear in the General Scholium to the *Principia*, and in the Queries to the *Opticks*, that he intended a mechanical account of the law of gravitation. Renouvier came down very hard on Voltaire and other self-styled “Newtonians” for holding that Newton believed that there were inherent forces in matter (1882, 89).

To put it simply, Renouvier could see that revolutions in science are open to alternative interpretations. So then what should we make of Renouvier’s earlier philosophical account according to which greater clarity is sought through revolutions?

In his defense, one could say that Renouvier did not think that there was anything deterministic about scientific progress. There is no necessary march to greater clarity. It is rather a contingent matter, depending on the voluntary choices made by scientists. For Renouvier, clarity in basic explanatory principles may be just one epistemic value that must be weighed against others such as empirical rigor and quantitative precision, where he believed Newton's physics won hands down over Descartes'. Greater clarity may be something that is achieved only over time, rather than all at once. Renouvier explained how in the century and a half subsequent to Newton, concepts such as quantity of motion, living force, work, etc. were all clarified. The discovery of the mechanical equivalent of heat also elucidated the object of study of physics. All the phenomena of physics could now be explained mechanically and there was no more need for subtle fluids, forces, and the like (1874b). Renouvier maintained that heat, light, electricity, chemical affinities, and even internal physiological processes could all be shown to "correspond" to motions on the molecular level (1874c, 161).

Renouvier appears to have recognized that it is precisely because science depends on the voluntary choices of scientists, weighing the clarity, rigor, and precision of competing theories against each other, that liberty is essential to science. He thus presented a liberal alternative to Comte's view of scientific progress. Consistent with this liberal outlook, he asserted in his *Science de la morale* that there is more progress in science during republics than during empires (Renouvier 1869, vol. 1, 235).

Also consistent with this liberal outlook, Renouvier granted scientists full liberty to question the fundamental principles and methods of a science even when he did not agree with their doing so. In an 1878 article on what he called the scientific spirit, he even named some revolutions in science that he was not quite happy with. Here he complained that "The progressive usurpation of imagination over reason extends to subjects where it seemed that correct thought was safe from revolutions” (1878, 200). He specifically referred to “new logics”, non-Euclidean geometry, and what appear to be psychophysics, evolutionary theory, and parapsychology. By “new logics”, I assume Renouvier meant the work of mathematicians such as Boole, De Morgan, and Cantor, as he did not provide any specific details.[9] Also, he did not actually use the term psychophysics, but complained about physiologists pretending to measure the intensity of sensations. But it was only the last of these sciences, that is, parapsychology, that he refused to take seriously at all. Here he spoke of the “pretention that less rigorous minds have of establishing as objective facts spiritual phenomena, which actually belong to pathological psychology”, finding fault in particular with A. R. Wallace and W. Crookes (Renouvier 1878, 200).

Renouvier also found fault with mathematicians who claimed that non-Euclidean geometry is an empirical science. He personally found non-Euclidean geometry absurd, since he thought space (and time) did not exist outside our forms of intuition, which are Euclidean. Nevertheless, in a paper later cited by Poincaré,[10] Renouvier granted mathematicians the liberty to pursue non-Euclidean geometry and allowed for the possibility that it may one day have useful applications. “It is, it is true, something; it is even a lot, if one may add to it the hope that these analyses of pure curiosity concerning sophistical hypotheses will one day produce useful results for true science” (1889, 346). Indeed, he found that it was not enough to say that mathematicians may doubt Euclidean geometry if they are interested. Rather, “they may doubt it through curiosity of mind and through simple liberty of supposition” (1889, 346).

At least with regard to geometry, then, it is difficult to attribute Renouvier’s conservative attitudes to his conventionalist philosophy of science. On the contrary, it was due to this philosophy that he appears to have grudgingly accorded others the freedom to pursue that of which he was skeptical. If the postulates of Euclidean geometry are simply conventions, he could see no reason not to grant others the right to pursue the consequences of a different set of conventions.

One might be tempted to put the blame for his resistance to alternative geometries on his Kantianism instead. It is true that he regarded Euclid's parallel postulate as a synthetic a priori principle. However, Renouvier's conception of the synthetic a priori is different from Kant's. Renouvier allowed for the possibility that synthetic a priori principles could be doubted, since, after all, they had been doubted:

> Synthetic a priori judgments, the foundation of geometry, are not necessary judgments in the sense in which the word *necessary* has been understood for such a long time, which is to say absolutely irresistible, and that there is actually no limit, even in geometry, to the power of doubt inherent in free reflection (1891, 62).[11]

The key question for Renouvier regarding non-Euclidean geometry appears to have been whether it would ever yield useful results, perhaps reflecting his engineering background. Unfortunately, he did not live long enough to see the application of non-Euclidean geometries to relativity mechanics. His preference for the practical may also explain his attitude to what he called the "new logics" (1878, 200). These mathematical works were something he could not see any use for at the time.

4. Poincaré

The connection between Renouvier and Poincaré is less direct than that between Comte and Renouvier. Unlike Comte, Renouvier never held a teaching position. Poincaré certainly knew of Renouvier's publications, as he had cited Renouvier's 1889 article on geometry.[12] Nye (1979) has documented Émile Boutroux's philosophical influence on Poincaré, and I would argue that Boutroux was in turn influenced by Renouvier.[13]

Due at least in part to Renouvier, the concept that the sciences rest on conventions was already part of the intellectual climate when Poincaré turned from mathematics to philosophy. Like Renouvier, Poincaré questioned the epistemological basis of rational mechanics. He argued that Newton's law of inertia is not testable, asked how we are to measure force and mass, and said, "We do not even know what they are" (Poincaré 1902, 77; 2018, 76), much as Renouvier had previously maintained that scientists adopted the convention not to inquire into their meaning.[14] For Poincaré, these concepts are understood only in relation to each other and to other concepts like acceleration and density (1902, 77–78; 2018, 76).

However, Poincaré made clear that the principles of mechanics were not arbitrary conventions, but rather conventions whose adoption was justified by experimental results:

> Are then the law of acceleration and the rule of composition of forces only arbitrary conventions? Conventions, yes, arbitrary, no. They would be arbitrary if we were to lose sight of the experiments that led the founders of mechanics to adopt the laws and which, as imperfect as these experiments may be, suffice to justify them. From time to time, it is fitting to bring our attention back to the experimental origins of these conventions. (1902, 85–86; 2018, 82)

Since Poincaré clearly said that conventions in mechanics are not arbitrary, it is puzzling that some Poincaré scholars have sought the origin of his notion of mechanical convention in the

Commented [W1]: I put it back the way I originally had it. You had substituted a semi-colon for the comma after testable, and changed "said" to "answered." This made it read like he was answering someone else's question. The way I put it, he simply is saying three things, which I separated with commas.

use of arbitrary signs and symbols in mathematics.[15] What appears to have been arbitrary for Poincaré are not the conventions at the basis of mechanics, but what he called “indifferent hypotheses”, such as whether matter is continuous or composed of atoms. These were indifferent to Poincaré because he thought the results turn out to be the same regardless of which assumption one chooses. (Poincaré 1902, 115; 2018, 209-10)

For Poincaré, not even the choice of geometrical postulates is arbitrary. The postulates of geometry are neither experimental facts nor synthetic a priori judgments,[16] but conventions chosen in light of experiments: “Among all possible conventions, our choice is *guided* by experimental facts, but it remains *free* and is limited only by the need to avoid all contradiction” (1902, 45; 2018, 43; Poincaré’s emphases). For Poincaré, Euclidean geometry is neither true nor false, and asking whether it is true makes as little sense as asking whether the metric system is true and the old units false, or whether Cartesian coordinates are true and polar coordinates are false. Nevertheless, Poincaré found Euclidean geometry to be the most useful, for two reasons. First, it is the simplest, but not as a consequence of our “habits of mind” or because we have a “direct intuition” of Euclidean space. He did not actually define simplicity, but merely said that Euclidean geometry is simpler in the way that a first degree polynomial is simpler than a second degree one, or that the formulas of plane trigonometry are simpler than those of spherical trigonometry. His second reason for holding that Euclidean geometry is the most useful is that it agrees with the properties of natural solids, “those bodies akin to our limbs and our eyes and with which we fashion our measuring instruments” (1902, 45; 2018, 43). This argument raises the question why we perceive natural solids to have Euclidean properties, if it is not because our forms of intuition are Euclidean, and thus appears to be in tension with his first argument.

Like Renouvier, then, Poincaré preferred Euclidean over non-Euclidean geometry. But he also said that the choice of conventions is free: neither philosopher would prohibit mathematicians from pursuing non-Euclidean geometry. Yet Poincaré also held that if we were to discover negative stellar parallax, we could either abandon Euclidean geometry or conclude that light does not necessarily travel in straight lines, and that "everyone" would find the latter alternative more advantageous (1902, 61; 1908, 107; 2001, 484-85; 2018, 59-60). But this often-cited "parallax argument" should not be interpreted as implying that Poincaré was in favor of preserving favored conventions through ad hoc maneuvers.[17]

Poincaré made it quite clear in his discussion of the principle of the conservation of energy that he was opposed to what more recent philosophers of science have called the "conventionalist stratagem" of defending principles by adopting ad hoc hypotheses. In *La Valeur de la Science*, he gave an account of the Curies' calorimetric experiments with radium and the principle of the conservation of energy, and said that their results could be explained away through an ad hoc, untestable hypothesis of an "unknown energy." But Poincaré argued that nothing was to be gained by rescuing a principle in this manner (1905, 114-15; 2001, 312-13). He maintained that if a principle ceases to be fruitful, that is, if it no longer leads to new predictions, it should be rejected even if experimental results do not contradict it. Sometimes science must start afresh (1902, 124; 2018, 118). With hindsight, one could argue that the conventionalist stratagem of postulating a new type of energy was the right move. But this would be the right move only if the hypothesis were testable.

Commented [W2]: I left it as *the* Curies since there were after all two of them, Pierre Curie and Marie Curie, working together

5. Conventions

As we move from Comte to Renouvier and Poincaré, we find an increasing emphasis on the voluntary aspect of science and knowledge generally. Renouvier is the key figure here, since he was a political philosopher as well as a philosopher of science, and introduced the voluntary element through the idea of the sciences resting on social contracts. In the intellectual historical context provided by Renouvier's philosophy, Poincaré's conventionalism turns out to be a liberal philosophy of science, allowing scientists to pursue alternatives to the reigning theories in their disciplines. There is nothing inherently conservative about adopting as conventions those principles that are best able to account for known experimental results or lead to new predictions that are worth testing. It is the philosophy that would insist on grounding knowledge on secure foundations that is inherently conservative, as it prevents science from taking a step forward if it is not absolutely certain. Conventionalism allows risk taking. Conventions are not something that stand in opposition to empirical science, but rather are the agreed-upon starting points among scientists that make empirical research possible in the first place, and which scientists retain the liberty to modify or even replace when these basic assumptions no longer prove fruitful. As Janet Folina (2019) argues, conventions for Poincaré are like hypotheses that one holds fixed in order to be able to test other hypotheses.

The analogy between social contracts in political life and conventions in the sciences may also help us with another interpretive question: Must conventions admit of alternatives? Some interpreters would limit Poincaré's conventions to geometrical postulates, the principles of mechanics, and linguistic conventions. David Stump, for instance, argues that for something to be a convention in science for Poincaré, there must be alternatives that are not merely logically possible but can also serve as useful guides for theory construction in science (e.g., Stump 1989, 2015). For instance, there are alternatives to Euclid's parallel postulate that would lead to non-

Euclidean geometries. Other Poincaré scholars would argue that to regard this postulate as a convention is simply to place it in a third epistemological category that is neither a priori nor empirical (e.g, de Paz 2014). The existence of reasonable alternatives is not necessary to consider something a convention.

Renouvier applied the concept of a convention very broadly, to include all sorts of questions to be excluded from science, such as the agreement among biologists not to inquire into a possible role for souls in living organisms. As such metaphysical entities are placed outside science, this convention is like a rule for constructing explanations in the biological sciences, and can thus be compared to Poincaré's so-called "natural hypotheses", or rules for the construction of theories in the physical sciences, which according to Poincaré cannot be avoided. As examples, he cited the assumption that an effect is a continuous function of its cause and the assumption that the influence of very distant bodies is negligible and can be safely ignored. He did not say that alternatives were not logically possible. Rather, natural hypotheses "are the last ones that we should abandon" (Poincaré 1902, 114-15; 2018, 109), suggesting that alternatives were not reasonable to pursue.

But this is only to raise the question what counts as a reasonable alternative. Consider the role of conventions in social life. Some cases are unproblematic, like conventions concerning on which side of the street we should drive our cars. It truly does not matter, as long as everyone agrees. Both alternatives are reasonable. Other conventions may be more like the legal age of consent for marriage, which also varies by country and even by state in the US. Here it does seem that some conventions may be better than others and that there may even be some logically possible conventions that would be totally out of the question. A person should be mature enough to understand that to which they are giving their consent. What is a reasonable alternative is a matter of degree.

Now, some conventions in mathematics and the sciences, such as how positive and negative values are assigned to the quadrants in a Cartesian coordinate plane, are clearly like the driving convention. But Poincaré's natural hypotheses may be regarded as conventions in the sciences that are more like marriage laws. We could, say, decide to include discontinuous functions or effects of distant bodies. At least, we will need a convention to tell us how distant a body must be before we can exclude it, which could be a bit like agreeing how young is too young to give consent to marriage. Also, there could be applications where it may prove to be a good idea to include discontinuous functions, and we could then adopt the appropriate conventions. These conventions may vary by discipline, much as conventions like marriage laws vary by country.

Perhaps we can agree on this much: we need to distinguish at least two kinds of conventions in mathematics and the sciences. There is one type, like geometrical postulates, where the various alternatives may all be reasonable to pursue. But then there is another type, like Poincaré's natural hypotheses and Renouvier's excluded metaphysical and epistemological questions, where the alternatives may be less reasonable or not reasonable at all. Nevertheless, because they rest on consensuses among working scientists, they are conventions, much like the conventions that govern life in the wider society.

6. Conclusion

Renouvier and Poincaré may have held conservative views about recent developments in mathematics and the sciences in spite of, rather than because of, their conventionalism in the philosophy of science. Their distrust of things such as Euclidean geometry or Cantor's contributions to number theory may have been due to their preference for useful science, reflecting their background in engineering, rather than to their conventionalism. They saw no practical use for these parts of mathematics. The natural hypotheses Poincaré favors, such as ignoring the influence of distant bodies, appear to be the sorts of assumptions one might make in practical applications of science, or conventions that might be adopted by engineers.

Comte also revealed the engineer's preference for theories in mathematics and the sciences with practical applications. But unlike Comte, neither Renouvier nor Poincaré would prohibit others from pursuing lines of research for which the practical applications were not immediately apparent. Their conventionalism was thus a liberal alternative to Comte's restrictive, authoritarian positivism.

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Endnotes

[1] Other polytechnicians went on to the École des Ponts et Chaussées (bridges and roads), the École d'Application de l'Artillerie et du Génie (military engineering) and other schools of application (Belhoste 1994; Shinn 1980).

[2] The *répétiteurs* led small discussion groups among students to clarify what they may not have understood in lecture, corrected students' written work, and suggested ways they could improve it (Shinn 1980, 46).

[3] *Cours* refers to Comte's *Cours de philosophie positive*, published in six volumes from 1830 to 1842. All references are to the two-volume edition published in 1975. There is no complete translation of this work. All translations are my own.

[4] As Laudan (1981, 147) argued, such passages in Comte should not be read as suggesting that he held anything like the twentieth-century notion of the theory-ladenness of observations. Comte saw no reason that observations gathered under the guidance of one hypothesis could not be used in support of another hypothesis.

[5] *Système* refers to Comte's *Système de politique positive*, published in four volumes from 1851 to 1854. Comte republished his earliest writings as an appendix to the fourth volume in order to demonstrate the continuity of his thought over the course of his career. "Trans." refers to the English translation of 1875-77, which is divided into four volumes in the same way as the original. However, as this translation is not always reliable, I offer my own translations instead, and provide the page numbers to the nineteenth century English translation only as a convenience to the reader.

[6] Comte's history of this episode is not quite accurate. See Hofmann 1996, chapter 8.

7 The topics of the first four paragraphs of this section, except for the discussion of Maxwell's ether, are treated at much greater length in chapter 6 of Schmaus, 2018. Renouvier's ideas about social contracts are treated in chapter 5. However, I have previously given only scant attention to Renouvier's views on scientific revolutions.

8 Renouvier published the first three of his *Essais de critique générale* in 1854, 1859, and 1864. He then published much expanded versions of the first two in 1875 and of the third in 1892. These second editions were then re-issued in posthumous editions in 1912. I provide page references to both the original and the more readily available posthumous editions.

9 Poincaré has a chapter in *Science et méthode* (1908) titled "The New Logics", which discusses the work of Russell, Couturat, and Hilbert. However, these works were published after Renouvier had passed away.

10 In an 1891 article, Poincaré referred to Renouvier's 1889 paper on non-Euclidean geometry in a footnote about Renouvier's debate with Auguste Calinon and Georges Lechalas on this topic. However, he dropped the reference when he republished this paper in 1902 as the third chapter of *Science et l'hypothèse* (Poincaré 1891, 772n1; 2018, 40n6).

11 For Renouvier, a judgment is synthetic a priori if it includes concepts that fall under more than one category. For instance, Archimedes' principle that a straight line is the shortest distance between two points is synthetic a priori because it includes both a qualitative and a quantitative concept, and hence cannot be analytic a priori. (1889, 344; 1891, 10-11).

12 However, to date I have found no evidence that Poincaré had read any of Renouvier's publications in which he discussed the roles of social contracts and conventions in the

sciences. Gould (1961, 116) was unable to find any written material either in the archives or held by family members in which Poincaré revealed his intellectual relationships with the philosophers of his time.

13 Boutroux and Renouvier shared a mutual friend in William James.

14 Pulte (2000) finds an even earlier conventionalist interpretation of mechanics in Carl Gustav Jacob Jacobi, in 1847-8. Renouvier did not read German and appears to have been unaware of Jacobi.

15 Giedymin, for instance, argues that the source was the geometry and associated philosophy of Joseph Diaz Gergonne, Julius Plücker, and Sophus Lie (Giedymin 1977, 290). However, Gergonne (1818, 13) used the term “convention” only in a single paragraph in which he said that to make a definition is to attach an arbitrarily chosen word to a collection of ideas. Giedymin quotes Plücker and Lie discussing the arbitrariness of Cartesian geometry, which he calls “conventionalist terminology” (1977, 287), but he does not quote either of them actually using the term “convention”. In seeking the source for Poincaré’s concept of convention, Giedymin thus appears to have assumed that conventions must be arbitrary.

16 Poincaré did not define synthetic a priori, but appears to have used it more in Kant’s than in Renouvier’s sense. See note 11.

17 See, for example, Giedymin 1977, 282-84, where he criticizes Reichenbach, Weyl, Nagel, and Grünbaum’s readings of this argument.