BOOK REVIEWS

Jeremy Gray, ***Henri Poincaré: A Scientific Biography***. Princeton, NJ: Princeton University Press (2012), 608 pp., $35.00 (cloth).

Like a *Dictionary of Scientific Biography* entry, this "scientific biography" opens with a brief chronological account of Poincaré's career and then proceeds topically. The chronological chapter already shows the richness of Poincaré's thought: even as he worked on an impressive variety of topics in pure and applied mathematics and physics, Poincaré was called on to adjudicate disputes over the validity of experimental results and help administer surveying expeditions. The topical chapters bring out connections between Poincaré's endeavors, as when we see how Poincaré's use of Cantor's ideas in his work on trajectories (see 260ff.) informs his rejection of impredicative set theory. Against this background, Poincaré's views look less dogmatic than they otherwise might. In particular, more weight accrues to his view that experimental and observational data must always (on account of their crudity) be interpreted in light of some convention. Jeremy Gray seems most impressed, however, by the other direction of influence. On his telling, Poincaré's thought is unified "to a remarkable degree" by the "tight hold his epistemology had on his ideas" of (inter alia) "what the practices of mathematics and physics consist of" and the standards for their success (7).

Gray identifies Poincaré's "geometrical conventionalism" as "his explanation of how knowledge is possible at all" and as "underpinning" his other epistemological views (excepting his account of arithmetical knowledge; 8). Gray shows that Poincaré's first argument for the conventionality of geometry was the translatability of non-Euclidean into Euclidean geometry and that Poincaré introduced the "cooled sphere" model in response to a critic (George Mouret) who dismissed non-Euclidean geometry as a useless game (44). This illustrates a major strength of the book: it ranges far beyond Poincaré's easily available collected papers to reconstruct his tactics and approaches. Gray understands Poincaré's conventionalism as "a theory of how the individual constructs his or her notion of space," according to which the choice between constructions is "based on convenience," although also "long since built into the workings of our minds" (8). As Gray observes, the most systematic presentation of this view is a *Monist* article of 1898. There Poincaré claims that the "group of Euclid . . . is simpler because certain of its displacements are interchangeable with one another" (quoted on 54–55). But Gray sets little store in this argument for intrinsic simplicity. He takes Poincaré's considered view to be that "a preference for the Euclidean group of transformations" is as "hardwired in us" (531) as the ability to organize sensations.

Gray claims Poincaré's epistemology was also "underpinned" by "his use of the idea of a group" (1). Because Poincaré "seldom studied any individual group in detail" (9), Gray speaks of "the group idea" rather than group theory. The obvious connection with geometrical conventionalism is that Poincaré supposes us to have an innate, "latent" idea of certain groups (*Foundations of Science*, trans. G. B. Halsted [New York: Science, 1929], 91), of which one was chosen to be the form of experience. But on Gray's account, the prominence of groups also reflects the "idealist" view that knowledge reaches only to "relations we [can] work with" rather than to "things" whose "ultimate nature . . . is hidden from us" (9). (In the case of geometry, the role of the group idea in hiding space's "true nature" is that for geometrical properties to be cognized through experience, relations between sensations must form a group. But this assumption cannot be verified by experience. For it involves the convention that particular experiences which satisfy the group properties "only approximately" are to be considered "resultants" of patterns that satisfy them exactly and offsetting "qualitative" distortions—just as, Poincaré argues, the "natural solids" used as measuring instruments are thought to expand and contract ["On the Foundations of Geometry," *Monist* 9 (1898): 11].) By showing this to be the context of Poincaré's affirmations that systems of relations survive theory change, Gray makes it plausible to understand the relations' objectivity as merely "a profound coherence [among] sensations" (73) and thereby issues a sharp (if implicit) challenge to structural realist readings.

Gray's explicit argument for attributing idealism to Poincaré is less satisfying. I find his interpretation very plausible, but proponents of realist readings will probably not be swayed. Gray associates Poincaré's emphasis on groups with his refusal to "admit" a concept "without a way of evaluating it and deciding upon its correctness" (8). He understands this as a disturbing "willing[ness] to collapse" metaphysics into epistemology. Against Poincaré's view that spatial distance is understood in terms of a convention for comparing lengths, for instance, Gray endorses Russell's contention that if "distances are there to be measured, they must be there before measurement" (82; also 532).

This seems to overlook important nuances of Poincaré's view. Gray appears to conflate conditions on the admissibility of concepts with requirements on the meaningfulness of statements (as when he takes Poincaré's view of the concept of distance to imply that Plato could not meaningfully "say that dinosaurs [once] roamed the earth" because he "could have had no means of verifying it"; 57). When Poincaré says that "every mathematical theorem must be capable of verification," he speaks of verification as involving the efforts of "many generations, one hundred if need be" (*Mathematics* *and Science: Last Essays*, trans. J. W. Bolduc [New York: Dover, 1963], 62). To insist that every statement's method of verification be specified "in advance" would not fit with Poincaré's clear recognition that some problems are made solvable by expanding the canon of solution procedures, long after they are posed (*Foundations of Science*, 369). Such insistence would also have barred Poincaré from proposing the conjecture named after him (which Gray claims he did not pose as a conjecture; 451).

Nor is Poincaré blind to verificationism's consequences in the way Gray supposes. On Gray's reading, Poincaré did not see how his view "leads to problems with the concept of truth" (57) because he avoided the concept and "seldom used" the word (7). But Poincaré introduces a volume of his essays by naming truth as our ultimate goal and "the sole end worthy of" our activities (*Foundations of Science*, 205). His problem regarding truth is rather to explain why "the harmony expressed by mathematical laws" exhausts the "truth we can attain" (209).

The material so far discussed comes from the chapter on Poincaré's popular essays. (Incidentally, the book's organization reflects Gray's contention that what Poincaré did [and even "who he was"] is "inseparable" from "how he was regarded in his lifetime" [3]. Thus, the first topical chapter concerns the essays, which won Poincaré his largest audience.) Chapter 11 on Poincaré's philosophy of science will also hold special interest for this journal's readers. I regret to say that the discussion of Poincaré's views in physics (on which this chapter focuses) strikes me as a weak spot in Gray's otherwise very useful account.

Gray maintains that in philosophy of science, "the crucial test" for any view is theory change and that those "best suited to grapple with" it are structural realism and "Wittgensteinian skepticism about any kind of knowledge" (534). However well this maps the contemporary landscape, Gray's two options do represent the dominant interpretations of Poincaré. Gray very sensibly points out that Poincaré's epistemology of arithmetic excludes the meaning skepticism about "+" associated with Wittgenstein (540). But he stresses the similarity between Wittgenstein's position and the view of "meaning as use" (10; see also 93–95) he attributes to Poincaré (2). His account of how this view leads to idealism is, however, problematic (as I argued). Meanwhile, Gray supposes that realist interpreters (such as Elie Zahar) understand the "convenience" of successful conventions as a mark of verisimilitude. He objects that "Poincaré's resistance to Minkowskian space-time" had "nothing to do with verisimilitude" but reflected either his epistemology or simple "personal convenience" (536). Thus, Gray's case against realism comes to rest on his explanation of Poincaré's "Galileanism." But this is flawed.

Gray initially bases the claim that Poincaré remained "Galilean to the end" on his 1912 remarks that, while some physicists find the (inhomogeneous) Lorentz group of transformations more convenient, nothing "constrains" them to choose it, so those who disagree "can legitimately retain the old [group]" (quoted on 112). But Poincaré can be placed in one of these camps only by analyzing the implications he draws from (what he calls) Lorentz's version of the relativity principle. Gray endorses Louis de Broglie's claim (which appears as an epigraph; 367) that Poincaré failed to grasp its "true physical" consequences. Among these, Gray evidently includes the way "Lorentz's mathematical contrivance" is turned "into a plausible definition of local time" ("by making it the time on a moving observer's clock"; 373). For Gray's summary account of how Poincaré "regarded Lorentz's local time" is that Poincaré "always compared" apparent time with "true" (etherial) time, rather than compare apparent times with one another (376). (Gray argues, moreover, that even if Poincaré's work on longitude determination equipped him to understand Lorentz's local time in terms of the time required for exchanged signals to travel [when synchronizing clocks], this "does not involve the idea that observers might be in a state of relative motion" [369].) This is, however, a significant departure—for which Gray provides no direct textual evidence—from other recent scholarship.

Gray claims Poincaré failed to grasp the physical consequences because he was "committed to the epistemological aspects of physics." By this Gray seems to mean that Poincaré could not consider formulas and coordinates in abstraction from Euclidean and Newtonian "ideas" (377–78), in particular, the privileging of a reference frame "at rest" (for Poincaré, the ether frame; 374). Since I do not have space to try to adduce counterexamples to this generalization, I merely note that the imputation of Newtonianism seems poorly grounded. To show that no "large gap" separates Poincaré's denial of absolute time and motion from Newton's views, Gray argues that even for Newton, some "assumption" is required to relate absolute quantities to measurable ones (371). But this neglects the theoretical warrant supplied by Newton's Laws of Motion for privileging, as measures of time, actual motions that approximate pure inertial motion.

Gray's treatment of Poincaré's mathematical work shares the (considerable) merits and (minor) defects of his account of Poincaré's philosophy. Drawing on a vast range of sources, it vividly reconstructs how Poincaré came to his results. Gray's organizational scheme leads him to begin with the work, on automorphic functions and the three-body problem, that brought Poincaré to prominence (through prize competitions sponsored by, respectively, the Paris Academy of Sciences in 1880 and the king of Sweden in 1890). This reflects Gray's insistence (elsewhere, e.g., in "Anxiety and Abstraction in Nineteenth-Century Mathematics," *Science in Context* 17 [2004]: 23–47) on situating results within the networks and discourses through which they win recognition. The shift of focus away from theorems, onto the events surrounding them, brings their motivation and development into view. Poincaré's "uniformization theorem" is an example. The uniformization theorem for arbitrary many-valued analytic functions is widely regarded as the nineteenth century's most important contribution to complex analytic function theory. Gray's account of it is an afterword to the chapter "The Prize Competition of 1880," which is not an obvious place to look since (on Gray's own account) Poincaré did not formulate the theorem until 1883. (This is somewhat obscured by Gray's speaking of "the general uniformization theorem" in the context of Poincaré's uniformization result for algebraic functions [232].) Showcasing the result as the outcome of a certain investigation helps us see it as a contemporary might have, rather than in hindsight.

Gray's discussions of technical material are prefaced by expositions of the key notions (and the circumstances of their emergence). Some of these are marred by typographical errors (such as transposed variables, "x" for "z" on 213–14 and "b" for "c" on 404) or overly casual wording. Most glaringly, in the antecedent of Desargues's theorem, the "if" is missing (92). Such problems will not faze cognoscenti, but it is sad that Gray's overture to a broader readership is undermined by his editors' slackness.

Gray's accounts of Poincaré's mathematical papers occasionally seem hasty. I cite two examples. Gray claims that when Poincaré came to study curves defined by certain differential equations, he "transferred the flow on the plane to one of the sphere by stereographic projection" (255; also 254). But Poincaré used a gnomonic projection, with good reason; it deals more satisfactorily with the line of infinity of the plane (see Jacques Hadamard, "The Early Scientific Work of Henri Poincaré," *Rice Institute Pamphlet* 3 [1922]: 172). In his account of the paper that introduces the method of "sweeping out," Gray has Poincaré discussing "rigorous solutions" (403; also 410), where Poincaré is in fact concerned with "rigorously prov[ing] the possibility of a solution" ("Sur les Equations aux Dérivées Partielles de la Physique Mathématique," *American Journal of Mathematics* 12 [1890]: 215). This keeps Poincaré's remark that such a proof is a solution from being a tautology, and it makes clear why such proofs may nevertheless be useless to the physicists who pose the problems: the series that represents the function of interest may converge too slowly to be "suitable for numerical calculation" (ibid.; cf. *Foundations of Science*, 377), a point made by Gray earlier on the page. Most importantly, the distinction between a solution and proof of its possibility lets Poincaré find rigor "from a physical point of view" ("Sur les Equations," 285 and 294) in arguments that do not meet analysis's standards. Indeed Poincaré's explicit aim is to establish a distinction between physical and analytical rigor ("Sur les Equations," 215), although Gray takes him to deny its possibility (403).

I note in passing that Gray makes nothing of an analogous distinction concerning Maxwell's theory. According to Poincaré, Maxwell proved the possibility of a mechanical explanation, but Gray considers only his claim that Maxwell did not provide the explanation.

Even if Gray misses some details, it is deeply rewarding to take in his "big picture." This book's regard for historical antecedents and breadth of scope are unmatched in the Poincaré literature, and will make it invaluable for every subsequent study of Poincaré's thought as a whole.

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Alcino J. Silva, Anthony Landreth, and John Bickle, ***Engineering the Next Revolution in Neuroscience***. New York: Oxford University Press (2014), 204 pp., $39.95.

Newly minted scientists will never read everything published in their subfield; there is just too much science done these days. Yet they must still design useful new experiments, despite their limited view of what has already been done. That is a tall order. Computers are good at mapping and visualizing large data sets. By harnessing their power, one might see where the ground has already been well trodden and where gaps remain.

Such is the suggestion in *Engineering the Next Revolution in Neuroscience* by Alcino J. Silva, Anthony Landreth, and John Bickle. As the authors note, in molecular neuroscience "the experimental record now grows at a rate that outstrips the human capacity to integrate it" (124). Silva (a prominent molecular neuroscientist), Landreth (an entrepreneur and former member of Silva's lab), and Bickle (well known to philosophers of science) outline a hypothetical method for computerized mining of research results. They focus on one subfield, studies of long-term potentiation (LTP), but the method is meant to be generally useful for neuroscientists. After introducing the project (chaps. 1 and 2), the authors taxonomize both experiments and analyses in molecular neuroscience (chaps. 3 and 4). They use this taxonomy to develop a theory about experimental reliability (chaps. 5 and 6). This is then integrated into a hypothetical system for visualizing the experimental landscape in molecular neuroscience (chaps. 7–9). Interspersed throughout, ostensibly by way of illustration, is a historical narrative about the challenges of studying LTP.

I will start with the big vision and work backward. In a representative passage, the authors envision "a glowing map of every phenomenon studied in neuroscience, laid out in constellations of hypothesized causal interactions. Surveying its landscape, we find each tentative causal connection appended with a dependability score, based on the outcomes of the different forms of Integration analyses. . . . We type into the search bar: 'highlight all causal paths involving CA1 and CaMKIII with dependability scores higher than n' and a legion of phenomena and their connections fade into the back-