Introduction

ALTHOUGH THERE HAVE been several excellent studies of aspects of Poincaré's work this book is the first full-length study covering all the main areas of his contributions to mathematics, physics, and philosophy. It presents an introduction to his work, an overview of its many fields and interconnections, and an indication of how Poincaré was able to tackle so many different problems with such success. What emerges is a picture of Poincaré as a man with a coherent view about the nature of knowledge, one that he expressed in many of his popular philosophical essays and applied in the conduct of his own research. What he emphasized above all was the act of human understanding. His preferred means of attaining the understanding of a problem was to find the right generalization of its core concepts, often in the form of an analogy, but one measured by its ability to generate new results. He endorsed Ernst Mach's idea of the economy of thought, and he spoke of looking for the "soul of the fact"—the right relationship between the facts that constitutes a productive principle.

He did not disdain rigor, he regarded it as essential, but he observed that rigorous proofs could be too long to be comprehensible, and in mathematical physics they could also fail to capture the way nature seemed to work and be incapable of producing answers useful to the physicist. Formal arguments, as in geometry, could distort the subject by emptying it. His use of the idea of a group underpinned his epistemology and frequently inspired his search for fruitful analogies. He trusted his intuition to make productive contact with calculations in every domain from differential equations to algebraic topology. But he did not put his trust in pure insight—he admitted his own intuitions were frequently wrong—or in finding the "true" meaning of things. On the contrary, he was very aware that what some might call the progress of science, more critical observers would call the accumulating ruins of failed theories. He knew very well that the best theories in physics were inconsistent, he wrote about these problems in several essays and addressed them in

his papers and books on electromagnetism and optics, and what he felt was a typical mistake was to infer the existence of objects responsible for the phenomena. For him, theory change was usually a matter of abandoning the objects while refining the relationship between the facts (mathematical and experimental) that had been carefully built up.

The picture of Poincaré that emerges would perhaps have pleased the later Wittgenstein. We have no certainty beyond what shared use and discourse can guarantee, no unmediated access to reality. We do what we can according to our best understanding of the rules of the game (axioms, principles, the best experimental data). Mathematics and physics together offer us a rule-governed way of living in the world, although we may, from time to time, have to change the rules, and our ability to frame these rules is, in some ways, built into how our minds work.

In this book I argue that understanding, thus conceived, was Poincaré's aim in everything he worked on. I trace how he came to the core ideas that animated his theories in many different domains. In addition to the numerous links he found between them, much of his work exhibits standard features that display his sense of the understanding one can have. In the tension between rigor and understanding, acute in both mathematics and physics, there is no doubt that Poincaré frequently failed to provide rigor. But he usually aimed at sharing understanding, expressed in ways in which new knowledge can most efficaciously be acquired. Assessing the extent to which he succeeded is one of the aims of this book.

This is a scientific biography of Henri Poincaré. It is confined entirely to his public life: his contributions to mathematics, to many branches of physics and technology, to philosophy, and to public life. It presents him as a public figure in his intellectual and social world; it leaves the private man alone apart from a deliberately brief account of his childhood and education. A full biography is underway with a team of scholars at the Archive Henri Poincaré at the University of Lorraine, and their book, due out in 2015, will be very valuable.

The book (2006) edited by Charpentier, Ghys, and Lesne can be recommended as an introduction to the implications of Poincaré's work for mathematics today, and I am sure that readers of this book will want to consult Verhulst's *Henri Poincaré: Impatient Genius*, which I have not yet seen.

VIEWS OF POINCARÉ

Pictures are like mirrors: what we see in them reflects aspects of ourselves. The frontispiece shows the famous picture of Poincaré at the age of 57, standing alone on the seashore. He has his back to us, he is slightly stooped, we cannot know what he is thinking, but most likely the picture suggests to the viewers that Poincaré is lost in his own, remarkable thoughts. Some may connect it with Newton's famous remark about picking up a few pebbles on the seashore:

> I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me. (Brewster 1855, vol. 2, 407)

It may invite us to enter our own speculations faced with the immensity of the oceans. Those who know of Poincaré's profound interest in physics may recognize the connection between the man and his research into the shape of the earth. Others, knowing how visionary and innovative was his mathematics, may see isolation and loneliness. Or just a middle-aged man enjoying a quiet holiday.

How Poincaré was regarded in his lifetime is inseparable from who he was. It is in the community of professional mathematicians that he first emerged, where he made his most profound and lasting contributions, and where his reputation remains most secure. He amazed his contemporaries with his unending stream of intellectual achievements. These began in 1881, when he brought together the mathematical subjects of complex function theory and complex differential equations with an entirely unexpected use of non-Euclidean geometry, to create the theory of automorphic functions. It continued in the middle years of the 1880s with his work on real differential equations and his radically new way of handling celestial mechanics. Despite his ever-deepening involvement with physics, his work in mathematics continued all this time. His most lasting achievement is his creation of the subject of algebraic topology, but he was one of the few to advance the subject of complex function theory in several variables in the 1900s, and he made important contributions to algebraic geometry and Sophus Lie's theory of transformation groups, and even to number theory.

Nonetheless, by the 1890s he had become a professor of astronomy and celestial mechanics, occupied with the fast-moving field of electricity, magnetism, and optics after Maxwell. This brought him into contact with Hertz, Lorentz, and, if only obliquely, Einstein. He was now professionally a physicist and a mathematical astronomer, a man nominated, albeit unsuccessfully, more than anyone else until recently for the Nobel Prize in physics. The physics community greatly appreciated his lectures on a dozen different topics in contemporary physics, and saw him as the leading French authority on electricity, magnetism, and optics, and the author of one of the founding papers in the theory of special relativity—although Einstein's theory was not widely accepted until after the First World War.

The public at large heard of him first in 1890, when he won a prestigious prize for a study of the motion of the planets and the stability of the solar system, and he went on to write two major works on celestial mechanics. He was a loyal French citizen, active in the Bureau des longitudes for many years. He was and remains almost unique among mathematicians and scientists in presenting his ideas to a general audience as well as his peers; he wrote widely on many topics and engaged forcefully in important issues about the nature of mathematics and contemporary physics, and the relationship between science and ethics. As this book shows, he was also highly regarded in his lifetime for a number of interventions in technological discussions.

In many different ways Poincaré exerted a deep and lasting influence, and yet he had few if any pupils working on his ideas, while his contemporaries often kept themselves at a safe distance from what he did and preferred other topics. He was a naturally elegant writer, but his style was often to describe what he had seen in a problem after prolonged contemplation, and he left many details for his readers to fill in. A professor of his time, he was not expected to create a school and his habit of having little to add to his sometimes impressionistic writings did not help. Barely two years after his death Europe plunged into a war in which many young French mathematicians and scientists were killed. When it was over there were few who were eager to develop Poincaré's legacy, and its personal character, with its deep commitment to mathematical physics, was uncongenial to the next generation in France—the young Bourbaki with their orientation toward the abstract pure mathematics that was coming out of Germany. Only after another war, and the full development

of the Bourbaki style, did mathematicians begin to rediscover the riches of what Poincaré had set out so many years before.

The modern science, mathematics, and philosophy communities differ in interesting ways, and one is their attitude to the past. A physicist today finds it hard to look back beyond the two great changes in 20th century physics: quantum mechanics and the general theory of relativity. Poincaré's contribution to the special theory of relativity is secure, if inextricably entangled with that of Einstein, but much else in what he did is shrouded in the obscurity of a lost way of thought. Many mathematicians, however, see him today not only as the exemplary creator of new branches of mathematics, but as the source of ongoing topics of research in several areas. As the recent excitement over the solution of the so-called Poincaré conjecture showed, Poincaré's work in topology lives among mathematicians to this day as, to an extent, he does among philosophers of science, who still discuss his philosophy of conventionalism. It fed into the ideas of the Vienna circle, and it has kept his popular essays in print for a century.

Which partial view of Poincaré to present first? The mathematician may always have been the most significant, and Poincaré was an extraordinary mathematician. Most mathematicians would be pleased to produce the work in any one of the first ten volumes of his *Oeuvres*; each one has some papers of remarkable depth and originality: the first two volumes carry the theory of automorphic functions, volume 4 his work on Abelian functions and complex functions of several variables, volume 6 the invention of algebraic topology, volume 7 his work on celestial mechanics and the discovery of chaotic dynamical systems, and volume 10 his work on the partial differential equations of mathematical physics. But by being largely topically organized they give a misleading impression of the man, who switched from one topic to another with great rapidity: to give just one example, in 1905 he published on number theory, geodesics on convex surfaces, the dynamics of the electron, a report on the French geodetic survey in Peru, and a popular philosophical paper on mathematics and logic. However, the last century has not made his mathematics any easier to explain to a broad audience.

Few mathematicians in any period would aspire to preeminence in a branch of physics as well, but Poincaré was one of the dominant figures in the theory of electrodynamics after Lorentz, eminent in celestial mechanics, and capable of publishing in every branch of the subject.

His contributions to physics and astronomy were markedly theoretical, although when space exploration began in earnest NASA brought out an English translation of Poincaré's major work on celestial mechanics, *New Methods of Celestial Mechanics* (1967), but it too belongs chiefly to the experts for whom it was intended. So Poincaré the physicist is more accessible than the mathematician, but his claim on a modern audience has been attenuated by the great changes in physics since his death.

Some mathematicians and physicists might hope to reflect philosophically on their subject, or to assist in its popularization, but starting in 1891 Poincaré did both, often in the same article and certainly in each of his four books of essays. As the public philosopher is the easiest to appreciate, I shall start with him, asking the reader to take on trust that Poincaré's words carried particular weight because he was a highly respected, internationally recognized expert in many fields. In December 1885 he had won the Prix Poncelet of the Institut de France for his mathematical work. In 1886 he became the professor of mathematical physics and probability in the Faculté des sciences in Paris, and was elected president of the Société Mathématique de France (SMF). In January 1887 he was elected a member of the geometry section of the Académie des sciences. This then will be the introduction to this remarkable figure. After some general reflections on Poincaré's ways of working, we move to look at the public intellectual, and then turn to look at his work in mathematics and mathematical physics.

POINCARÉ'S WAY OF THINKING

It also makes sense to start with the philosopher for the good reason that to a remarkable degree, Poincaré was guided in his mathematical and scientific work by his philosophical reflections. There are times in mathematics and physics when the practical mathematician or physicist becomes a philosopher of their subject, whether they, or the later professional community admits it explicitly. As Hilbert said of Dedekind's work on one occasion, "The mathematician was thus compelled to become a philosopher, for otherwise he ceased to be a mathematician."[1]

[1] Quoted in Corry (2004, 379).

Whenever the nature of the mathematics changes profoundly, as it did in Poincaré's time with the arrival of characteristically modern mathematics, or physics, as it was to do with the general theory of relativity and with quantum mechanics, some mathematicians and physicists are decisive in the conceptual—indeed, philosophical—reformulation of their subject. The most prominent proponent of the new, highly abstract, modern mathematics was David Hilbert in Göttingen, and it is easy to argue that there was a sea change, even before he articulated it, that many German mathematicians went with and many French opposed: what Cantor advocated, with Hilbert's subsequent blessing, Charles Hermite, the leading French mathematician of his day and an important influence on the young Poincaré, found pointless and disturbing. But Poincaré, for all he spoke against the new set-theoretical foundations of mathematics that were coming out of Germany was not a spokesman for French values, rather, only for his own. He could appreciate the axiomatic approach often favored by Hilbert, he favored the general over the particular in his own work (unlike Hermite). However, what holds his life's work together to a remarkable degree is the tight hold his epistemology had on his ideas of ontology, on what constitutes an answer to a mathematical or physical problem (and what does not), and on what the practice of mathematics and physics consists of.

Poincaré argued that we had every reason to believe the universe was intelligible, but could never know what it was "truly" like. We could only hope to live in it in an effective manner. To do so as a mathematician and a physicist meant, he argued, recognizing the two subjects as parts of the same subject because mathematics was the only language the scientist can speak, and because physics had given mathematics the concept of the continuum, without which little mathematics could be done. As concerns language, this is a theme that runs right through Poincaré's work. In his view, we share the usage of key terms, and we strive for objectivity through discourse, not for truth, a word he seldom used. As for the continuum, this, he said, was chosen by us as a matter of convenience without which science as he knew it could not proceed, but it was not forced upon us—he knew that other continua were mathematically consistent.

Poincaré entertained three levels of working assumptions in our intellectual life. The first was that of hypotheses: these could turn out to be true or false, and scientists make them all the time. At a more fundamental level, certain hypotheses, such as the laws of mechanics,

had, over time, been elevated to the level of principles. They could not be verified, but they were extremely plausible and led to a very elegant theory with great range and predictive power. They were conventions, adopted for their efficacy not for their truth, and might one day have to be abandoned in favor of better ones, but until that day they were beyond discussion and controversial evidence should first be assessed on the assumptions that these basic principles hold.

At the third level, underpinning all these conventions, were two epistemological ones. Geometrical conventionalism was his explanation of how knowledge is possible at all. It is a theory of how the individual constructs his or her notion of space and, like cognitive science today, it is a mixture of evolutionary ideas and ideas about early mental development.[2] But it, too, leaves open the idea that other intelligent beings might make a different construction of space, and find it to be, say, non-Euclidean where we find it to be Euclidean. There would be no fact of the matter, only a choice based on convenience, albeit one long since built into the workings of our (and their) minds. In particular, our concept of distance between objects rests on our idea of how to measure, and this is typical of Poincaré: no concept is admitted without a way of evaluating it and deciding upon its correctness. Confronted with an apparent truth, he would always ask: How do you know?

Poincaré's second epistemological convention was his explicitly argued belief that we have a built-in understanding of reasoning by recurrence, from which flows our knowledge of what the natural numbers are, and indeed all the mathematics that does not depend on the continuum. This is not the view of most mathematicians today, but we know now that all rich mathematical theories rest on some assumptions. His loneliest position was his rejection of Zermelo's set theory, on the grounds that the transfinite sets it required were too big to be understood, and if they could not be understood, they could not usefully be talked about. In this sense, Poincaré's preference for our knowledge of the natural numbers over axiomatic set theory is not so strange.

He believed strongly that a knowledge claim had to come with an account of how we can know it. In a long controversy with Bertrand

[2] The ultimately flexible, purely utilitarian character of the conventionalism distinguishes it from Kantian intuition, which is, at least in Kant's presentation, an unanalyzed and perhaps even unanalyzable mental grasping.

Russell over what it is to know what distance is, he emphasized that it was imposed on us by our understanding of measurement. Inspired by Maxwell he had confidence in experimental results and rigorous mathematics but no compelling need or logical force in filling the gap between experimental results (measurements, laws) and mathematical theorems with stuff (such as electric fluids, electrons, and the like, that people—Lorentz included—populated their papers with).[3] In part this was a somewhat Kantian recognition that the ultimate nature of things is hidden from us, but it was also a belief that such talk often turned out to be wrong, whereas the theoretical relations between the objects usually prospered. It was the relations we could work with, not the things. He sometimes said he dealt with the form but not the matter of a subject, often capturing the form as a particular mathematical object, the group. But he seldom studied any individual group in detail; for him the crucial fact was the existence of a group. It allowed him to explain how we arrive at a particular convention for understanding space; equally it allowed him to explain why certain functions in number theory exist, and why certain topological spaces are different.

A striking late expression of this idealist strain in his thinking illuminates his ideas about space. In the preface he wrote to the first three-volume collection of his essays he emphasized that he was interested in the language we use to talk about space and how we might have confidence in it without ever having access to some phantom of a fundamental reality. "Space," he remarked, "is only a word that we have believed a thing" (1913, 5). A decade earlier, in a controversy with Le Roy over the nature of scientific knowledge, Poincaré discussed how the brute facts (simple observation statements such as "it is dark") are translated into the language of science, and where the fundamental principles of physics enter the analysis. He was firmly of the opinion that these principles are human creations, and as such capable of revision—which is a good view to hold when theories are likely to change.

Talk, discourse, was crucial to Poincaré. He recognized that we cannot compare our individual sensations, but we nonetheless agree on what is red. Likewise, he argued that in accepting as conventions the

[3] Perrin's experiments of 1908 convinced him of the existence of atoms because his analysis showed that it was possible to make definite statements about them that could be consistent with other theories in physics.

fundamentals of Newtonian mechanics, and so moving them beyond the reach of experiment, we are agreeing on what we shall say is true. He made no distinction between saying "the earth rotates" and "it is convenient to say that the earth rotates" because the only way to make the first remark is to subscribe in advance to the second: to unpack its meaning was, for him, to discover that it is a statement in Newtonian dynamics. Truths by convention were part of a shared discourse.

For Poincaré, understanding was central, and it was captured in the ways people were enabled to say what they could not say before.[4] So Poincaré in his own way preferred to speak of use rather than of meaning, even in mathematics. The axiomatic approach promoted by Hilbert does indeed tell us how to use the elements of an abstract structure but never what they are (although we may recognize specific exemplars). But when Poincaré speculated on how to do mathematics he came back to the importance of analogy in guiding one's thoughts, formulating new ideas, and generally doing something new. The rigorous side of mathematics was valuable, for without it there was nothing, but what mattered more was invention, the productive use of old and new ideas. And this was expressed in the steady expansion of our mathematical language. And when Poincaré talked about mathematics he valued new facts only if they united seemingly disparate elements in an unexpected way that, as he put it, "enables us to see at a glance each of these elements in the place it occupies in the whole" (*L'avenir* 375). Elsewhere, in words that not every lesser mathematician might feel comfortable with, he disparaged the mere production of new combinations of mathematical entities as work that can be done by anyone and that is highly likely to be absolutely devoid of interest; the task is to discern the few useful new combinations, which will usually be of ideas drawn from widely separated domains. He said that mathematicians set great store by the elegance of a solution or a proof because of the economy of thought that Mach had identified as a measure of intellectual economy.

Whenever he found it possible, he was rigorous. As he put it in *L'avenir*: "In mathematics rigour is not everything, but without it there is nothing." His education at the École polytechnique raised him to fluency in the theory of differential equations as it stood in the early 1870s, in complex function theory, in the use of Fourier series, and in coordinate geometry.

[4] See the insightful paper, Epple (1996).

This gave him a battery of standard methods to apply, of checks to run before certain theorems can be used, and general tricks of the trade that he rapidly fitted into his way of thinking mathematically. That said, when he himself felt an argument of his was imprecise, as was often the case in his work in applied mathematics, where there is a need to make approximations to get results and rigorous means are not available, he recognized that the proper test is the experimenter's, whose results can guide a theory when rigorous mathematics cannot. And on occasion he offered a vision of how things should be that, in the absence of a proper terminology and body of new results, could only be imperfect, as he did when creating algebraic topology. This is true quite broadly: Poincaré gave his readers good reasons to believe what he said, and where he could be rigorous he usually was, but he was happy to publish when he had convinced himself, and on those occasions his contemporaries had to be content with little more than a shrug.

Poincaré was explicit that understanding was not chiefly a psychological state. When he spoke to the Psychological Society in 1907 he noted that the feeling of having made an insight could be delusive, and the error would only be revealed "when we attempt to establish the demonstration" (1908d, 395). But he did appreciate the power of analogy, and indeed he often sought out analogies and demonstrated their use in various aspects of his work. These were never speculative, groundless analogies, but the result of detailed examinations of stories drawn from different domains that could be used to make productive comparisons. Indeed, his way of working further dismantles the idea that Poincaré pursued a path of pure intuition. He worked regularly from 10 till 12 in the morning and from 5 till 7 in the late afternoon. He found that working longer seldom achieved anything, but that it was not always possible to switch off, which was why he never worked in the evenings. He took a complete rest when on holiday. When reflecting on a topic he liked to walk about, but when preparing he took few notes and very often began to tackle a problem without any clear idea about the solution. He typically felt drawn on by a topic almost like an automaton and did not have the sensation of making an effort of will, but "with Poincaré the feeling of certainty must be rather weak, for every truth appeared to him to be debatable in some respect" (Toulouse 1909, 192). Organizing his ideas generally came easily to him, but if the effort became painful he would give up and abandon the work, which usually

happened when he lost interest. On the other hand, he sometimes found that things went well by dropping a subject and coming back to it at intervals.[5]

He set out his views on what it is to understand mathematics in several places, most programmatically in his (1904c) on definitions in mathematics. This was an address at the Musée pédagogique, and chiefly concerned itself with the teaching of mathematics in schools, but Poincaré was concerned with the marked difference between formal and intuitive mathematics. Just as a precise definition might not be understood in a classroom, understanding a proof is not the same thing as checking its logical validity. Today, he said, we know that our predecessors worked with many imprecise definitions that we have made precise, that of continuity, for example, but in the last fifty years logic has sometimes produced monsters: functions that are not continuous, or, if continuous, have no derivatives. Indeed, strictly speaking, these are the typical functions and the ones one finds without going looking for them are a small corner of the whole. But if logic was our only guide, the student would have to begin in a teratological museum, and this would be as futile as studying an elephant only with a microscope. Everything would be correct, but it would not show the true reality. So it is necessary to proceed in the reverse direction, and build up the student's intuition before gradually introducing rigor. "It is not enough to doubt everything," he said in paragraph 7, "one must know why one doubts." Even the future professional mathematician (*géomètre*) needs intuition, "For if it is by logic that one proves, it is by intuition that one invents" (para. 9). The real skill, said Poincaré, is in choosing between the different correct things one can write down so as to see the goal from a long way away, and without intuition the mathematician is like a writer who knows grammar but has no ideas.

The question of *why* something is true was much more important for Poincaré than the question of *what* is true. Even a strict proof would not always answer the question of why a result holds: there had to be what he called the "soul of the fact" (*L'avenir* 376), the central idea that explained why it was true and also how it could be proved. This did not necessarily make the proof entirely short or obvious, but it told him how to tackle the problem and which standard techniques would resolve it.

[5] See Toulouse (1909, 145–146).

This accounts for the longevity of his popular essays. It is quite remarkable that so many of them are worth reading well over a hundred years after they were written, and they remain fresh because they address issues that will always come round in the education of mathematicians, scientists, and the general public. Perhaps non-Euclidean geometry has become a surrogate for some other belief about space, but the issue of how a rigorous mathematical theory can be interpreted in physical terms remains pertinent. Certainly Poincaré is not the best person to read about special relativity, but his reflections on how the best science of his day seemed to be crumbling in the face of almost inexplicable laboratory results speaks to anyone concerned about how theories change (as they do). His hostility to logicism and his deep distrust of the rising axiomatic set theory leave Poincaré's strictures about how to define mathematical objects with limited appeal—except that to this day a number of eminent voices raise concerns about the cavalier acceptance of the infinite. Very likely his predictions for the future of mathematics look no better than most gamblers' best estimates, and no better than Hilbert's selection—and Hilbert was in a much better position to make his predictions come true—but read in their entirety they say valuable things about how mathematics does, or might, develop. And no one has written better about how to be an inventive mathematician. Other volumes in the series Poincaré wrote for tried for topicality, and now they pay the price of being yesterday's news. Poincaré tried to speak about human understanding, and that does not date. He might even be presented, with his theory of knowledge, as a precursor of cognitive science.

Poincaré's Achievements

Poincaré first emerged on the mathematical scene in 1879 and 1880 with a number of small papers on number theory after the manner of Hermite, who was pleased with them, and a couple on differential equations. He turned 26 in 1880, so he was no prodigy. But in 1881 he began to publish the work that had led him to write the essay and its supplements which placed him second in a prize competition of the Académie des sciences, and a stream of new ideas began that transformed the study of three areas of mathematics: complex function theory, differential equations in the complex domain, and non-Euclidean geometry. By 1884, when Poincaré's interests began to embrace yet

Figure 0.1. Charles Hermite (1822–1901). Source: Arild Stubhaug, *The Mathematician Sophus Lie* (Springer, 2002).

more fields he had rewritten the theory of Riemann surfaces, created new classes of functions that solve a large class of hitherto intractable differential equations, and placed at the center of it all the topic of non-Euclidean geometry that had previously been merely exotic. The new functions he defined, variously called Fuchsian or Kleinian functions after other investigators or more generally, automorphic functions, were a generalization of elliptic functions, a subject of considerable importance in its own right although one that did not detain Poincaré.

It is possible to trace Poincaré's progress quite closely in these years (as chap. 3 below describes) and we do not see a sudden flash that illuminates the whole. Rather, we see the gradual emergence of a governing family of ideas, built around the deepening appreciation of the group idea. Once Poincaré saw how non-Euclidean geometry entered the story he had a program that he could pursue that raised questions that, mostly, he could solve and that, in what was both a cooperation and a competition with the German mathematician Felix Klein, led eventually to a brilliant insight that had to remain an unproved conjecture for twenty-five years.

Thereafter, his work displayed no particular pattern. Unlike most of his contemporaries he did not stay in one field and deepen his understanding of it. Nor, like some more restless souls, did he simply switch fields from time to time. He took up new interests, but seldom dropped any. His earliest interests remained his last—his very last paper, as fate was to determine it, was on Fuchsian functions and number theory. But a significant shift came when he began to develop theories that applied to planetary astronomy: the shape of planets (described in chap. 5 below), their orbits, and the long-term stability of the solar system.

These were traditional questions going back at least as far as Newton, and they were central to an establishment that revered Laplace, but Poincaré invigorated them. Once again he soon reached a governing idea, in this case that for such problems the long-term behavior of the solution curves was what had to be understood. This marked a complete contrast with the astronomers' incremental tradition in which prodigious amounts of calculation were deployed to calculate the ephemerides for only a few years ahead. Poincaré succeeded to a remarkable degree with a preliminary study of differential equations and their solution curves on surfaces—which was a further way for him to appreciate their topology—and then embarked on what became a lifelong involvement with planetary motion. What remains one of his most celebrated discoveries is his demonstration that there is a deep reason for the failure of traditional methods to resolve even the simplest nontrivial problem, the three body problem: even three bodies moving under their mutual gravitational attraction can display chaotic motion and have orbits extremely sensitive to the initial conditions, thus making long-term predictions almost impossible.

When Poincaré became a professor of mathematical physics and probability in 1886 his interest in physics deepened, and no topic was more important and exciting than the theory of electricity, magnetism, and optics. To the British this meant the theory presented by James Clerk Maxwell, who had died in 1879, but this theory was distasteful to French scientists who found it lacking the elegant mathematical sophistication they were used to in their own tradition. Poincaré even found it inconsistent, but he also admired it for its depth, its mathematics, and its appreciation of the fact that there will not be a unique explanation of nature if there is any explanation at all. In the 1890s Poincaré became

the French expert on the theory, the man who could indeed provide an elegant exposition of the ideas of Maxwell, Helmholtz, and Hertz, point out their strengths and weaknesses, and in due course do the same for Lorentz's contributions. He also became the adjudicator of a number of disputes in the subject, contributed to the technological exploitation of the new ideas, and, in 1905, the author of one of the lasting ideas in what is now the subject of special relativity: what he modestly called the Lorentz group. Finally, in 1911 his grasp of Max Planck's new theory of quanta was influential in the acceptance of the new ideas with a speed that Planck had feared impossible.

From 1890 until his death Poincaré retained an interest in the theory of real and complex functions in one and several variables and worked successfully on a number of outstanding problems. He made lasting contributions to Sophus Lie's theory of transformation groups and to algebraic geometry. But his major contribution to mathematics in those years was undoubtedly that of topology. It was one of his abiding beliefs that a qualitative analysis of a problem ought to precede a quantitative one, and the pioneer of qualitative methods in mathematical analysis was Bernard Riemann, who had died in 1866 leaving behind such a profound reorganization of the subject that it was take at least a generation to assimilate. Poincaré's involvement with Riemann surfaces early in his career educated him in the power of Riemann's ideas—in many ways Riemann and Poincaré were kindred spirits—and his formulation of the three body problem had led Poincaré to contemplate problems in extending Riemann's ideas to three dimensions. What he accomplished here essentially created a new branch of modern mathematics: algebraic topology. It may have done so in part because his methods were so visionary that they had more or less to be done again and differently in order to be rigorous, but also set out an attractive topic and ways of approaching its problems. He outlined several ways of defining three-dimensional manifolds, sketched what later would be called a Morse-theoretic decomposition of them, and described the two natural algebraic objects that are associated to a manifold, their first homotopy and homology groups, with enough precision to establish a profound problem, one that grew in successive interpretations to become the Poincaré conjecture.

This great range invites the question: Was there one Poincaré or many? If, trivially, there were many—the Poincaré of (sometimes) rigorous

pure mathematics, the applied mathematician and lecturer happy with heuristic arguments, the scientist immersed in the details of geodesy or celestial mechanics—there was also only one. Not just because he took a firm view of what his task was, which was to develop the understanding of everything he looked at, but because of the many analogies and links he found between the subjects he worked on. Among the links he mentioned explicitly were ones between celestial mechanics and problems in the theory of complex functions of several variables; between physicists' intuitions and the often contrived methods for solving the partial differential equations of mathematical physics; and between geometry, physics, and philosophy. The right approach for a book such as this one is surely to follow his own, and to seek to explain what he discovered by conveying why it has to be described in particular ways, to aim to reilluminate the radiating centers of his own systems of ideas.

His way of working explains why Poincaré had rather distant relations with his contemporaries and no real students. As his nephew Pierre Boutroux explained to Mittag-Leffler, Poincaré was willing to be very patient with students, but when it came to expressing an opinion his standards were very high: either they had really grasped the idea, or they had not. Add to that the fact that the French system was much more closely tied to the old model of young independent inventors making their way in the world than the German graduate school approach, and the fact that most mathematicians in the 19th century worked on their own anyway, and his isolation is less surprising. But it did not spring from any reluctance to express himself, or from an "ivory tower" mentality: he served energetically on numerous committees and editorial boards.

Another measure of the man is afforded by the work of others that excited and impressed him. The first of these seems to have been Georg Cantor's work on point-set topology, which he applied to his own work in the 1880s. He was impressed by Lie's theory of transformation groups when he met Lie in Paris, but he did not work on the subject until 1900, after Lie was dead. Hill's new approach to the study of the motion of the moon he regarded as an insight into dynamical systems that was likely to be very useful in numerous ways. Among the physicists, the ideas first of Hertz and then Lorentz impressed him and drew him to the frontier of electromagnetic theory. Hilbert's *Foundations of Geometry* he recognized as presenting a profound and radical challenge to his own ideas, and this seems to have impressed him more than Hilbert's work on integral

equations, where Poincaré always gave the palm to Ivar Fredholm's contributions. He appreciated Hermann Minkowski's *Geometry of Numbers* as a breakthrough in number theory, a topic Poincaré regarded as particularly difficult, and he seems to have appreciated the work of Italian geometers on the theory of algebraic surfaces sufficiently to produce his own, complex analytic, version of one of their most incisive results. His last enthusiasm was for Planck's insight into the quantum nature of radiation. On the other hand he never learned much from Einstein's theory of special relativity and seems not to have fully grasped it, despite coming up with the Lorentz group at the same time. He did little with the work of his contemporaries, whether he got on with them personally (as he did with Paul Appell) or not (Émile Picard), and he seems to have not cared about the younger generations of French analysts—Émile Borel, Maurice Fréchet, Henri Lebesgue, and Paul Montel—and not to have taken up even the idea of measure theory, despite his interest in probability theory. He may even have thought their interest in the strange behavior of functions somewhat misplaced. Even those who strayed into his territory, like Jacques Hadamard and Paul Painlevé, do not seem to have become mathematical confidantes. He disliked what he saw of the attempt to reduce mathematics to logic, and while he remained polite he was doubtful that any attempt to reduce mathematics to axiomatic set theory would succeed.

In Context

It would be impossible, and therefore absurd, to summarize the state of France in the second half of the 19th century, but a few perspectives are most relevant here. The defeat by the Prussians in the Franco-Prussian War and the loss of parts of Alsace and Lorraine in 1870–71 charged national feelings with potent emotions of shame, rivalry, and renewed patriotism. These burst out in the Dreyfus affair, which began in 1894, but they permeated scientific life in the form of repeated comparisons with whatever was happening in Germany.

Education prospered in the aftermath of the defeat. As Gispert (1991) has described, the higher education budget more than doubled between 1877 and 1883, new university professorships were created, especially in the provinces, research became professionalized, student numbers increased. The French Mathematical Society, founded in 1872, was

initially dominated by graduates of the École polytechnique, but as the status of teaching rose so too did that of the École normale supérieure at the expense of the École polytechnique until by 1900 Poincaré was a marked exception in having attended the École polytechnique: Appell, Borel, Cartan, Darboux, Goursat, Hadamard, Painlevé, and Picard were all normaliens. New journals were founded—as they were in all academic subjects and in all types of journalism. Even so, Germany with its twenty-two universities and its productive emphasis on research, was the object of many a nervous glance. Poincaré, as we shall see, made numerous contributions to several of these journals, and his popular reputation was lucratively sustained as a result.

The Belle Epoque, as the period in French history from the 1890s to 1914 has become known, was a period when foreign travel took off among the middle classes, and champagne, high fashion, and operettas were the height of fashion. As a child Poincaré had traveled widely with his parents, and he continued this habit in later life. He also liked music, Wagner especially, but did not play an instrument although he had been taught piano briefly as a child.[6] But it was also the period of Zola's novels, when the gap between the rich and the poor widened considerably, and fashionable pleasures were contrasted with the miseries of the growing slums. On occasion Poincaré spoke out against what he saw as the mindless accumulation of wealth, although his involvement in explicitly political activity was sporadic, and he was a moderate on the side of Dreyfus (see chap. 2, sec. "The Dreyfus Affair" below).

French scientists were among those who hoped to improve the quality of life, Pasteur most famously, but the half century also saw the arrival of the telegraph, wireless, street lighting, and a number of other technological breakthroughs, and Poincaré was to contribute to the discussion of many of these issues. The ideology that accompanied these contributions to the creation of modern life was usually positivism in the form that Auguste Comte had given it in the 1850s but without his oddly religious overtones. This was a belief that the evidence of the senses, perhaps refined through simple scientific theories, was the only reliable form of knowledge, superior to any form of metaphysics or organized religion.

[6] When Poincaré died, *Le monde artiste* (20 July 1912, p. 458), recorded that Poincaré liked to go alone to matinees at the Opéra-Comique to listen to Pélleas et Mélisande and with his young daughters to La trompette, where he was the most attentive and assiduous listener.

Positivism was the default position of most scientists, and sometimes spilled over into a view that scientists were therefore particulary suited to advise government, perhaps best from a position of studied neutrality that had the unintended side effect of keeping the state funding of science at low levels, with detrimental effects on the growth of physics as a discipline in France. It often went with a naive progressivism, a belief that science dispassionately and sensibly applied would lead to steady, and ultimately remarkable improvements in the quality of life. Its greatest weakness was a failure to understand politics, where, of course, it was opposed by the powerful Catholic Right, and played into the lasting divisions created in the traumatic conflict of the French Revolution. Poincaré was far from being a positivist, but when he was drawn into defending his views of science against the arguments of Le Roy who sought to present science as little more than the inventions of scientists, he was arguing against opinions intended to bolster the theology of the Church, as is discussed below (see chap. 1, sec. "Science, Hypothesis, Value").

Émile Boutroux, the most prominent philosopher of his generation, and who was only nine years older than him, was his brother-in-law and it is often speculated that some of the Kantian aspects of Poincaré's thought derive from Boutroux. One of his central concerns in Boutroux's major book (1874/1895) was to bring a respect for contemporary science into philosophy without, as a result, introducing a determinism that would shut out ethics and free will. To do this he invoked a layered nature of science that put logic at the lowest level, with mathematics upon it, then mechanics, and so on until physiology and finally sociology was reached, and argued that no layer could be reduced to the one below because an element of contingency always intervenes. Mathematics could not then have an a priori claim on our knowledge of the world, whatever its rigorous character, and moreover, as he put it "the law of causality is but the most general expression of the relations arising from the observable nature of things."[7] The word "relations" here alerts us to the later ideas of Poincaré, and in fact Boutroux went on to distinguish two kinds of laws in science. The first kind are mathematical, abstract, and almost necessary although capable of revision, but they are remote from reality. The second kind are intuitive, observational, and completely empirical, but are not deterministic at all. The former operate when science explains,

[7] See É. Boutroux (1874, 25–27), quoted in Heidelberger (2009, 123).

the latter when it describes. But the mathematical laws are rooted neither in reality nor in the fundamental nature of the intellect: "mathematics is necessary only with respect to postulates whose necessity cannot be demonstrated, and so is hypothetical after all."[8] As Heidelberger points out, following Pierre Boutroux at this point, the element of contingency gives a pragmatic edge to Émile Boutroux's analysis that brings it close to Poincaré's conventionalism. On Boutroux's formulation, as on Poincaré's, the mathematical laws of nature cannot be made to apply except by treating them as free choices of the mind, taken with as much pragmatism as is worthwhile.

But it is also the case that Poincaré thought through anything that interested him for himself. He was never in the grip of a philosophical orthodoxy that drove him to explain everything in somebody else's language as if it had greater epistemic depth. He rather resembled Helmholtz in his struggles with the Kantians: sometimes sympathetic, but never a party member.[9] This was because his expertise was in mathematics and physics, topics the philosophers (Boutroux excepted) had largely ignored, but which he knew raised genuine problems in epistemology and ontology and on which he knew he had something original and important to say. The reflections of mathematicians and physicists on their subjects in the years 1880–1914 in fact created philosophies of their subjects well in advance of anything the professional philosophers deigned to contribute.

Habits and Customs

We have some evidence of how Poincaré actually worked on a daily basis. Like all really good mathematicians, Poincaré, kept a structured account or story of mathematics in his mind, one that placed the key concepts, methods, and theorems in a coherent way. He read in the fashion of some of the best mathematicians, as Pierre Boutroux observed,

> He did not force himself to follow long chains of deductions, the closely-woven net of definitions and theorems that one usually finds in mathematical memoirs. But going straight away to the result that

[8] See É. Boutroux (1895, 215), quoted in Heidelberger (2009, 135).

[9] I do not find Poincaré's conventionalist epistemology as close to Kant's synthetic a priori as does Folina (1992).

> lay at the centre of the memoir, he interpreted it and reconstructed it in his own way; he took control of it in his own way and then, taking the book up in his hands once again he looked rapidly through the propositions, lemmas, and corollaries, that furnished the memoir. . . .Instead of following a linear route his mind radiated from the centre of the question he was studying to the periphery. As a result, in his teaching and even in ordinary conversation he was often difficult to follow and could even seem obscure. When he expounded a scientific theory, or even told a story, he almost never began at the beginning but, ex abrupto, he set forth at once the salient fact, the characteristic event or the central person, someone he had absolutely not taken to time to introduce and whose name his interlocutor did not even know. (P. Boutroux 1914/1921)

He added, "All his discoveries my uncle made in his head, most often without the need to check his calculations in writing or setting his proofs down on paper. He waited for the truth to strike him like thunder, and counted on his excellent memory to remember it."

We may note a number of approaches to topics that Poincaré frequently tried, among them the following.

1. He looked whenever possible for transformations of a problem, not just to simplify it but because groups of transformations were at the heart of every place where they arose. As we shall see (for example in chap. 3) whenever he studied geometry, the corresponding transformation group was the key to all the important issues.

2. In problems on mathematical physics he looked for the basic principles: conservation of energy and of momentum, least action, and so forth.

3. When problems depended on some parameters, as for example many problems in dynamics do, he looked at the effect of varying the parameters on the quantities he was trying to find. If some important function vanished for a certain value of the parameters, would it vanish for a range of parameter values?

4. He frequently made a distinction between the qualitative and the quantitative aspects of a problem, arguing that the broad

features had to be understood first before the details could be fitted in, and that understanding the broad features aided the more detailed quantitative explorations.

5. He appreciated the close connection between real harmonic functions and complex analytic functions that Riemann had pioneered in the 1850s, and extended the intuitive properties of harmonic functions to the more complicated setting of functions of several variables.

6. He had a liking for naive geometrical analyses involving curves and surfaces, and in that setting for finding a way of reducing the dimension of the problem, for example looking at a flow on a surface by seeing how the flow repeatedly crossed a curve, and by reducing questions about three-dimensional manifolds to their two-dimensional boundaries. But he was not a visualizer who relied on what he could draw or see with his mind's eye.

7. He was comfortable with the fundamental features of finite-dimensional vector spaces, such as spaces of solutions of certain equations, or objects specified by parameters, and willing to consider infinite-dimensional analogues.

8. He analyzed problems in mathematical physics by looking phenomenologically at very small regions, where the physical process could be expected to be linear, and from this deducing an infinite system of linear equations that might then produce a linear partial differential equation. But he abandoned this final step with vigor when he saw that the new quantum theory was necessarily discontinuous.

Of course, the precise problem area would then determine how much detail Poincaré would think fit to give. The discovery of a new, and solvable, class of differential equations in pure mathematics, or of the way to generalize an important function in number theory, did not call for much detail, but when he was occupied with problems in new technology, as he was with radio waves, he was much more willing to show how the fundamental theory delivered useful information.

In 1909 Poincaré had allowed himself to be subjected to a battery of medical and psychological examinations conducted under the direction

of Dr. Étienne Toulouse, the director of the Laboratoire for experimental psychology at the École des hautes études in Paris. Toulouse was interested in the psychology of exceptional people, and had already published a similar investigation of Zola in 1896 and some notes on Berthelot in 1901. Among other matters, Poincaré also answered questions about his personal views. He had believed in religion when he took his first communion, but found that by the age of 18 he had ceased to believe. He believed in freedom of thought, the right to research and to tell the truth, and for that reason he opposed clerical intolerance. He was a republican in politics, and thought that the state should not intervene very much, except in certain matters of health. He favored political equality for all, and had no theoretical objection to judicial or political rights for women, although he feared the influence of the church upon them. He was indeed a man of his time in his progressive beliefs in the merits of science for the public good, the role of the scientific intellectual, his republican dislike of the anti-intellectual positions of the Catholic Church and its insistent attempts to shape opinions and extend their influence. This was the milieu he grew up in, and most likely because of the damage caused by the Franco-Prussian War he was quite patriotic and very willing to serve his country in the best ways he could. He was never bellicose, but he was not averse to military rhetoric (he had, after all, studied at the École polytechnique) and there is a hint from time to time that he did not cite German authors when perhaps he should have done (he read and spoke German fluently, but he seldom cited widely). He had principled reasons for staying away from the world of politics, and was only drawn into the Dreyfus affair when he saw he had a contribution to make, but had strong views on education and on ethics that he expressed quite frequently as his popular esteem grew.

We also learn from Toulouse's account, which was published in 1909, and is in part a record of facts that anyone who knew Poincaré in person would probably have known. He was 1.65 m tall (5′ 5″) and weighed about 70 kg (154 pounds), which was regarded as moderately corpulent.[10] He had begun to put on weight when he married. His face was colored and his nose large and red. His hair was chestnut, his mustache fair. He had, it seemed, a share of minor ailments: digestion took some two

[10] It gives him a body mass index of 25.7.

to three hours during which time he could not think productively and indigestion frequently interfered with his sleep. For that reason he kept regular habits, with breakfast at eight, lunch at noon, and dinner at seven (he ate meat quite a lot), and never had coffee after dinner. He went to bed at ten and rose at seven. He did not smoke and never had, and disapproved of the habit. He had never exercised systematically, but he liked walking and would willingly go fifteen kilometers. He seldom had headaches. He made no mention to Toulouse of the illnesses that had affected him in public on several occasions, and only mentioned a disabling bout of rheumatism when he was 32.

Toulouse reported that Poincaré either made his mind up quickly or found it increasingly difficult to do so; and he noted, as many did who knew Poincaré, that he seemed almost permanently distracted. He relayed the story (p. 27) of Poincaré discovering to his surprise one day on a walk that he had a birdcage in his hand and having to retrace his steps to find the place where he had inadvertently picked it up. He also reported that Poincaré would answer questions even when it seemed that he had not been listening. This compares well with Boutroux's memory that Poincaré

> thought in the street, when he went to the Sorbonne, when he went to take part in a scientific meeting, or when he went on one of the long walks he was accustomed to take after his dinner. He thought in his room at home or in the lecture theatre at the Institut, when he wandered about, pulling a face and playing with his key ring. He thought at the table, at family reunions, even in the salons, often interrupting brusquely in the middle of a conversation to force his interlocutor to follow a chain of thought he had come up with. (P. Boutroux 1914/1921, 197–200)

Toulouse also noted that when animated by a topic Poincaré liked to walk about with his hands behind his back, his brow furrowed and his eyes blinking. He was good with languages, spoke clearly and correctly but rather timidly, so outside of the lecture theater he spoke on public occasions only after careful preparation, and often by reading from notes. But he had never thought to learn a speech by heart and give it from memory. Writing quickly and clearly came easily to him, although he did not have particularly fine handwriting and never had. But although right handed he could write quickly with his left hand, and recalled being

ambidextrous up to the age of eight; he also had difficulty distinguishing his left from his right.

The most interesting finding Toulouse made concerned Poincaré's visual abilities. Poincaré had taken to wearing spectacles in his early thirties, but had good vision in both eyes, and he did reasonably well on the tests of short term visual memory. He was not good at recognizing faces, and was helped by hearing people's voices. Strikingly, he did not think visually, and claimed to have no long-term visual memory and to rely on his motor memory; when asked to copy simple figures from memory, which he did quite well, he did so by recalling the motion of his eyes. Toulouse put it this way: "Poincaré analyses the objects that he sees and looks at; it is by analysis that he reproduces them. The lucidity of his observations is remarkably clear." But he noted that Poincaré did draw occasionally. Altogether a curious set of abilities for someone who is rightly regarded as one of the great geometers.