

CHAPTER 9

“God Liberate Us from His Symbols”

In 1895, Volterra entered into a long polemical exchange with his Turin colleague Giuseppe Peano, a mathematician renowned for, among other things, the construction of the first space-filling curve, the modern definition of an abstract vector space, the axioms for the natural numbers (now universally known as Peano’s axioms), and the geometrical calculus (which would turn out to be of considerably less significance).

Peano taught calculus both at the university and at Turin’s Royal Military Academy. Despite his acknowledged contributions, he had, by the time Volterra arrived in Turin, been somewhat marginalized by the mainstream Italian mathematical community. He had become obsessed with using his own peculiar notations of mathematical logic in the classroom, much to the dismay of students and faculty alike. In 1891 Peano embarked on an ambitious program to recast mathematics in its entirety, using his symbolic notation. Between 1894 and 1908, he would publish five editions of his *Formulario Mathematico*, and when the calculus volume of the *Formulario* appeared in 1898, he used it almost to the exclusion of all other texts in his classes. One of his students recalled that Peano taught from it

with the greatest love and much patience, the first pages [being] dedicated to the symbols of logic and then several lines of several other pages dedicated to accurate definitions of the concepts, to the various operations, and several passages of various parts of mathematics. Only in the last months of the academic year did Peano arrive at briefly treating, and always with his symbols, the calculus by the system of vectors, and of explaining some applications of curves, with calculations of lengths, areas. . . . We disliked having to give time and effort to the “symbols” that in later years we might never use.¹

The dispute between Volterra and Peano arose over a classical problem in rational mechanics: the wobbling of the earth’s axis of rotation. In studying the rotational motion of solid bodies, the eighteenth-century Swiss mathematician Leonhard Euler had developed equations suggesting that the earth shimmies slightly as it spins on its axis—enough to cause the location of the poles to wander with a period of ten months. The confirmation of Euler’s equations came more than a hundred years later, when astronomers succeeded in measuring the periodic variation in the latitude of the poles.

Volterra's interest in the rotational problem dates from this time; and in 1894, in his rational mechanics class, he had used it "to illustrate [Heinrich] Hertz's concept of substituting hidden movements for the consideration of forces in the investigation of natural phenomena."² At the suggestion of the astronomer Giovanni Schiaparelli, director of the Brera Observatory in Milan, Volterra submitted a nineteen-page paper, "On the theory of the movements of the earth's pole," to *Astronomische Nachrichten* in February 1895, showing that hidden motion of matter within the earth can cause the earth's poles to shift slightly. Two days later, having just been elected a resident member of the Royal Academy of Sciences of Turin, he presented a short note on the same argument, adding that other types of constant motion on the surface of the earth—ocean currents, rivers and streams, evaporation of water, and rain—might also explain the shifting of the earth's axis. In March, he presented three more notes on this subject.

Although it ostensibly focused on what today would be considered a problem in geophysics, Volterra's quarrel with Peano was really rooted in their divergent approaches to mathematics. A pure mathematician by trade, Peano had an uncanny gift for choosing topics that would turn out to be important in the development of mathematics in the twentieth century. Volterra, too, was a pure mathematician (some would argue that his most significant contributions lie in pure mathematics), but unlike Peano he did not shy away from the notion that his work might have real-world applications, and indeed actively embraced a range of mathematical problems pertinent to physics, celestial mechanics, astronomy, geology, and geophysics—and ultimately even biology and economics. Peano was fundamentally interested in the logic—and beauty—of mathematics' underlying structure. He also specialized in finding counterexamples to apparent theorems, and delighted in holding other mathematicians to his standards of rigor. Peano had taken his degree in mathematics, Volterra in physics, an important difference between them. Volterra's interest in the problem of the earth's wobble led him to formulate a mathematical theory that could be either verified or falsified by hard data—the gold standard of scientific investigation. Peano was less committed to finding a detailed solution of the problem than in illuminating its intricate mathematical architecture. Single-mindedly bent on promoting his geometrical calculus, Peano used the wobble problem to demonstrate his system's superiority over integration, which was Volterra's forte. The use of the geometrical calculus in Peano's very first paper on the wobble topic set the stage for their dispute. From that moment on, the battle between two giants of Italian mathematics was joined.

One Sunday in early May of 1895, seated in the elegant high-ceilinged meeting hall of Turin's Royal Academy of Sciences, Volterra listened in amazement as Peano launched into a talk about the displacement of the North Pole. "[A]lthough I saw him every day, without saying a word to me he set out to repeat my calculations," Volterra told a colleague afterward.³ Peano also implied that an article he had published in the January 1895

issue of the *Rivista di Matematica* (a journal founded and edited by himself), about the rotation of a falling cat filmed by the French physiologist and pioneer cinematographer Étienne-Jules Marey, had piqued Volterra's interest in the problem. Drawing an analogy to the descending cat, Peano asked, "Can our Earth change its orientation in space by means of internal motion, just as every other living being? From the viewpoint of mechanics, the question is identical. But to Professor Volterra belongs the credit for having proposed it first. He made it the object of several notes presented to this Academy."⁴

The next step, Peano said, would be to consider how the geometrical calculus he had developed could be brought to bear on the problem, for which, he implied, no satisfactory treatment currently existed. Volterra jumped to his feet at the end of Peano's talk, his words laced with irritation. "[T]he numerical calculations of academy member Peano start from ideas already expounded in [my] various notes but are founded on rather unreliable numerical data," he told the assembled group, according to the minutes of the May 5 meeting.⁵ Warming to his subject, Volterra insisted that calculations based on rigorous data did exist and that he had made such calculations himself a while ago, starting with important results obtained by the American astronomer Seth Carlo Chandler, who had found a period of about fourteen months in the polar motion. And while Volterra had delayed presenting his own work until he had developed a general theory, he had come to the May 5 meeting intending to present "A theorem on the rotation of bodies and its application to the motion of a system with internal stationary motions." After listening to Peano, however, he wished to present a second note, which would test his mathematical model against Chandler's observational data. He asked, and the Academy granted, permission to go home and get it.

Volterra once said of Enrico Betti that "when he talked mathematics he more often than not thought physics."⁶ As his second note, "On the periodic motions of the earth's pole," presented later that same day, made plain, Volterra, too, excelled in using mathematics to solve physics problems.⁷ Starting with the hypothesis that polar shift could be decomposed into a series of harmonic motions, he analyzed in detail the relationship between the rotation of the earth and its stationary internal motions. (By "stationary," Volterra meant the cyclical motions of mass inside the earth.) A comparison with observational data followed. Was it possible to reconcile the data with Euler's classic equations? Carlo Somigliana, who watched his friend's argument with Peano unfold, described how Volterra managed it:

Volterra set about calculating the value of the angular momentum corresponding to the internal motions necessary to produce a variation in the Eulerian period that would make it equal to Chandler's period. He found that the component along the terrestrial axis of this angular momentum had to

be $1/1,053$ of the angular momentum of motion of the Earth, assumed rigid. This result is not verifiable, but it is plausible.⁸

On May 19 at the Turin Academy, Peano presented another paper on the topic, but he pulled it before publication, citing an error. Then, on May 30, he sent Volterra a letter that opened on a conciliatory note ("After your comments, I recognize the influence of the earth's deflection on the polar motion") and included a promise to read Volterra's articles on the subject. The closing lines, however, struck a dissonant chord. "But you'll agree that your way to deal with the problem introduces three integrations, *with as many superfluous arbitrary constants* [emphasis added], which come from the differentiation of the first three equations."⁹ Volterra sent back a terse reply: "The method of integration that I used doesn't present three superfluous arbitrary constants, as you say. The differentiation of the equations expresses the principle of the area, which includes three arbitrary constants, for the special choice of the x,y,z axis, [and] is necessary to establish the equations of rotation—that is to say, to remove the nine cosines from the equations." After rather unkindly mocking Peano for his ignorance of classical mechanics ("After all, the process of integration is the same followed for every problem of rotation from Euler to the present day; for this, therefore, you can consult any mechanics book"), Volterra asked him if he intended to withdraw his May 5 note as well or else modify his remarks at the next session of the Academy—in which case, Volterra would cordially refrain from any public remarks about the matter.¹⁰

Peano apparently declined to do either, reportedly telling Andrea Naccari, who served as the Academy's secretary for the class of physical, mathematical, and natural sciences, that he didn't repent of anything he had written. In a letter to Corrado Segre, Volterra passed on a remark made by Enrico D'Ovidio to the effect that all Peano was willing to concede "is that the earth is not a sphere," prompting Volterra to declare that Peano was "ignorant of mechanics from Euler up to the present."¹¹

Ever since Peano's note on May 5, Volterra had been fuming about what he perceived as his lack of collegiality and his cursory treatment of Volterra's work. In the privacy of his own home, he took to pen and paper to express his rage and resentment. One can almost hear him lecturing the walls (or more likely, Angelica) about Peano's various affronts. Among Volterra's unnumbered and undated notes housed in the Accademia dei Lincei in Rome are several that clearly relate to this period, such as this.

It was his duty to inform me [of his interest in the problem of the earth's wobble]: it is something that any educated person would do; [and] What is absolutely false is that, as Peano affirms, once the idea is set, the problem is solved. In fact: 1) it is not so easy to put the problem into an equation, since several calculations are needed, which he has to include

by copying my results; 2) from the point of view of developing any analytical solution, the path is very difficult. . . . Professor Peano dares to recount a story of how I came to develop and apply the idea of cyclical motions [the story of the poor falling cat proffered at the May 5 meeting]. His affirmations on this matter are perfectly gratuitous; I will say more: they are completely false, as is always the case with those who want to read minds, with incredible irresponsibility and no basis in reality.

And there was this, which may have reached to the heart of the matter: "That the note of Professor Peano does not have any scientific purpose is shown clearly by this: That he uselessly repeats works and researches that I have already published."¹²

While overseeing final examinations as a visiting proctor at the University of Pisa at the beginning of June, Volterra asked Segre to find out, if possible, what Peano was planning to do about the offending May 5 note. Segre replied that he didn't know, but did say "I believe it will appear in the Academy's proceedings. I don't know if Peano intends to publish some words of correction or not. . . . In any case, I believe it is better for you to wait and learn about Peano's intentions: the more so since if he does nothing at the June 9 session, there is still another session remaining before the holidays. I am very pleased to see things advance so as to be able to hope that you will not be annoyed and distracted by a polemic later on."¹³

Segre knew whereof he spoke, having exchanged acrimonious words in print with Peano just a few years before—and worse, discovering that Peano enjoyed playing the role of *provocatore*. "I hold the scientific polemic to be one of the forms under which ideas may sometimes usefully be expressed,"¹⁴ he reportedly told Segre.

On June 9, at the Turin Academy of Sciences Volterra presented a theory that modeled the pole's motions, supposing the earth to be plastic. There was no recantation by Peano. On June 23, the last session before the summer recess, Volterra delivered yet another note, which he described as "observations" on his second paper of May 5; it brought the number of his papers on the subject to seven. No sooner had he finished his presentation than Peano rose to make his own on the motion of the North Pole, taking pains first to acknowledge recent work in the field by others—save one, Vito Volterra, whose name he conspicuously left off the list. To make matters worse, he insinuated that Volterra was a sloppy mathematician: "To decide such a question, one must make complete calculations, omitting nothing," Peano told his audience.¹⁵ Once again his paper featured the notations and symbols that he had developed for use in his geometrical calculus, which most contemporary mathematicians, Volterra included, found less than edifying.

Determined to correct the record, Volterra now sought a larger arena. On July 2, 1895, he submitted his eighth paper on the subject to the *Rendiconti dell'Accademia dei Lincei*, the monthly published proceedings of the venerable national academy, and let fly at the outset:

Prof. Peano, in a note presented to the Academy of Turin in the session of 23 June of this year, and which has just now been printed, shows that a system which is symmetric about an axis, and which constantly maintains its form and density distribution, may have variable internal movements that follow a law such that the rotational pole moves continually farther from the inertial pole. Seeing that this result can be obtained as an evident and immediate consequence of formulas considered by me and explained by me in several preceding memoirs, which Prof. Peano forgot to cite, although they were published this year in the same *Acts* of the Academy of Turin, I may be allowed to show this here, avoiding the employment made by said author of methods and notations not generally accepted and proceedings hardly suited to making clear the path taken and the result reached.¹⁶

If Volterra thought that scolding Peano in the Academy's proceedings would put paid to their quarrel, he was mistaken. In fact, Peano was delighted. Here was an opportunity not only to goad Volterra further, and on the latter's home ground, but also to showcase the superiority of his geometrical calculus in solving a classic problem in rational mechanics. Since he had not yet been elected a member of the Lincei (he became a corresponding member in 1905), he asked Eugenio Beltrami to submit the paper on his behalf. Volterra's brief note, "On the motion of a system in which there are variable internal movements," had appeared on September 15th; Beltrami presented Peano's note (to add insult to injury Peano had appropriated Volterra's title as his own) on December 1st. He began by revisiting the falling-cat experiment—"Discussing the question of the displacement of the earth's pole, produced by movements of parts of the earth, such as the ocean currents, I pointed out to several people the identity of the two questions, seeing that instead of a cat and its tail, one could talk about the earth and its ocean"). He derided Volterra's technique for solving a very difficult mathematical problem ("Now, if the question can be easily and completely solved by my way, the reason others run into great difficulties, I believe, depends on their habitual use of long formulas to indicate simple ideas"), and, not surprisingly, decided the question of who deserved credit for solving the problem of the earth's polar motion in his own favor ("I believe it useless to add anything else, seeing that finally Prof. Volterra agrees with my result that 'relative movements, however small, acting for a sufficient time, can displace the earth's pole, even supposing the continents rigid' "). For good measure, Peano "translated" the opening calculations of Volterra's maiden

paper at Turin's Academy on February 3 into vectorial language, explaining that Volterra had begun "by writing three equations that, understood geometrically, say that a certain vector is constant. He differentiates them, transforms them, and in the general case that interests us here, arrives at a single integral (on the last page of the memoir), which signifies 'The length of this vector is constant.'"¹⁷

On January 1, 1896, Volterra in effect walked away from the dispute. As Segre had warned, engaging in a polemic with Peano could end up costing the other disputant far more than it was worth in time and energy. In a three-page letter to Francesco Brioschi, president of the Lincei and senior statesman of Italian science, Volterra made his case for the final time, spelling out his side of the story:

Relative to what is said in the beginning of [Peano's] note, it seems to me that it is not worth the effort of spending any words, seeing that no one can doubt my priority, whether with respect to treating the question or with respect to the fundamental idea which forms its point of departure; nor can any doubt arise about the originality of my idea, as I explained in my lectures of last year. . . and it is not necessary for me to justify myself with the cat question, as Peano hints—a question, for that matter, about which he limited himself to writing in his journal a simple and brief review of the work of others. . . Having thus shown to be empty and unfounded any of the points of criticism made of me by Peano, and that his assertions are neither original nor exact, he himself having recognized them as such, for my part I hold this polemic definitively closed.¹⁸

Brioschi transmitted Volterra's letter to the Lincei several days later, for publication as "Replica ad una nota del Prof. Peano."¹⁹ A comment that Volterra later made in a letter to Tullio Levi-Civita, an exceptionally talented young mathematician at Padua, shows how hard he struggled to extricate himself from the controversy: "At the end of last year I was very busy preparing several translations I needed before the start of the New Year, and the holidays, which I was to hoping to have free, I was forced to spend on work excessively serious and in many ways unproductive, which I could not avoid."²⁰

Two years later, in 1898, Volterra published his definitive work on the polar shift problem, some 156 pages of closely reasoned arguments embedded in a forest of calculations and diagrams, in Gösta Mittag-Leffler's journal, *Acta Mathematica*. True to his word, he made no further attacks on (and did not even mention) Giuseppe Peano.²¹

Meditating on Peano's behavior in a letter to Carlo Somigliana, Volterra concluded, "It is very strange to recognize one's error and then publish [it] again; but there is a lack of balance to such a degree in Peano's mind that

we should not be terribly surprised." To which Somigliana replied, "God liberate us from his symbols, if these are the results to which they can lead."²²

In time, Peano's refusal to teach a traditional calculus course cost him his teaching post at the Royal Military Academy, whose engineering faculty succeeded in finding an alternative calculus instructor. Peano remained at the University of Turin, teaching calculus until 1925. No action was taken there; he was an important mathematician, and if the students suffered, so be it. The mathematician Francesco Tricomi, who came to Turin in 1925, long after Volterra's departure, suggests in his autobiography a darker strain of discord in the university's mathematics department:

Among the colleagues I found... were, besides Peano—and [A.] Terracini, who arrived almost together with me—C. Somigliana (1860-1955), G. Fano (1871-1952) and T. Boggio (1877-1963) as well as G. Fubini, who was appointed at the Polytechnic but taught higher analysis at the University, and C. Burali-Forti, who taught only at the Military Academy. But I must say that, as unfortunately often happens, these professors did not get on very well among themselves, and on one side [was] the "Jewish" group (headed until his death by C. Segre) with conservative tendencies, to which Fano and Fubini adhered, and on the other side the "vectorialists" group of Peano, Boggio, and Burali-Forti, who breathed the spirit of rebellion instead. As for the noble C. Somigliana, descending directly from A. Volta, who was then dean of the faculty, he oscillated between the one and the other but leaned toward the "Jewish" group, notwithstanding that he was not completely immune from a little anti-Semitism.²³

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Eighteen ninety-six turned out to be a banner year for Volterra. Having divested himself of the Peano matter at its start, he proceeded to publish six papers between January 12 and April 26, dealing with the inversion of definite integrals—the solution of the type of integral equations that are now called Volterra integral equations of the first kind. He had been sitting on his results for some time. The impetus for what became in Volterra's hands a systematic treatment of integral equations owed something to his young colleague Levi-Civita, who had reported on the solution of several "interesting" cases of integral equations of this type at the November 17, 1895, session of the Turin Academy, taking the opportunity to bemoan the absence of a systematic study of the problem. Levi-Civita's lament may have inspired Volterra's first inversion paper, which he delivered at the Academy in the early winter of 1896 and in which he offered "a small contribution"

to the problem, limiting himself, as he put it, "to the simplest case."²⁴ In a letter to Levi-Civita at the end of February, Volterra confessed that he had long despaired of publishing this work, because his method lacked elegance ("*poco elegante*"), but he now had found "a more general and direct expression" that he intended to communicate to the Lincei at its March 1st meeting; other results needed to be cast in an appropriate form before publishing, and after that he planned to move on to the case of constant limits. In closing, Volterra begged Levi-Civita's indulgence: "Excuse me if I have made you lose time with my concerns, but it is very pleasing to carry on a correspondence with you about an argument that has always interested me."²⁵

Quick to appreciate the subtlety and penetrating intellect of his junior colleague, Volterra undertook to mentor Levi-Civita in much the way that Ulisse Dini and Enrico Betti had looked after him in his early years at Pisa. Like Volterra, Levi-Civita had studied under a noted mathematician (in his case, Gregorio Ricci Curbastro) and distinguished himself in mathematics at an early age, advancing rapidly up the academic ladder. He took the hand of friendship that Volterra extended. The two mathematicians would exchange many letters over the years. A man of enormous scientific versatility, Levi-Civita was appointed to the chair of mechanics at Padua in 1898 (again like Volterra, he was only twenty-three at the time), with Volterra's strong backing. Their personal friendship dated from that autumn, when Levi-Civita visited Turin; it would be cemented when Levi-Civita joined the faculty of the University of Rome in 1918.

Volterra's circle of friends in Turin included Giovanni Vailati, a mathematician turned logician and historian of mathematics, a champion of pragmatism and a habitu  of cafes. A polymath with an irrepressible laugh and a passion for music and books, he was above all, a scholar with a vast breadth of interests ranging from Archimedes to school reform (his classmates at Turin had dubbed him "the Philosopher"). Several years after graduating from Turin in 1884 with degrees in mathematics and engineering, he had become a teaching assistant in Peano's calculus course. He published a number of papers on mathematical logic in Peano's *Rivista di Matematica* and later collaborated with Peano in the writing of the early chapters of the *Formulario*. Following a stint as assistant in Luigi Berzolari's projective geometry course, Vailati became Volterra's assistant in the fall of 1896, a position Volterra had personally arranged on a visit to the Ministry of Public Instruction. Vailati wanted to offer a course in the history of mechanics, and to do that he needed official standing. Volterra secured for him the title of "voluntary" (i.e., unpaid) assistant and arranged to have Vailati's name listed in the university calendar; as a result, Vailati was allowed to teach his own course for several years—though without a paycheck.

Early in 1897, Volterra, along with 2,000 other mathematicians, was invited to an upcoming mathematical congress in Zurich. According to its organizers, Switzerland had the advantage of being centrally located

and well versed in hosting international conferences. In selecting a neutral country, the organizing committee may also have felt the need to avoid stirring up bitter memories of the Franco-Prussian war, still a sore point with many French. The three-day meeting that August, billed as the first International Congress of Mathematicians, attracted 242 participants from 16 countries, including 38 women. Although Vailati begged off, Volterra decided at the last minute to attend the congress, accompanied by Angelica. He was promptly elected secretary for the meeting's Italian-language papers—not an onerous assignment, as only two of his countrymen, Peano and Francesco Gerbaldi, professor of analytic and projective geometry at the University of Palermo, presented their papers in Italian. All the sessions took place at the Swiss Federal Polytechnic school, whose lavishly decorated lecture-hall on the second floor provided ample seating for talks by plenary speakers Henri Poincaré, Felix Klein, Adolf Hurwitz, and Peano. Living up to their reputation as good hosts, the local arrangements committee organized a pre-Congress reception on Sunday evening, August 8, at the Tonhalle, a stunning complex by the lake, and a banquet in the Tonhalle's main hall at one o'clock on Monday, followed by a steamboat excursion on the Lake of Zurich. The steamer returned to Zurich around nine o'clock in the evening, where a parade of boats decorated with wreaths, flowers, and flickering lanterns greeted the guests as they disembarked. Mathematics held sway on Tuesday, with sessions devoted to arithmetic and algebra, analysis and theory of functions, geometry, and history and bibliography. On the last day of the meeting, it was announced that Paris had been selected as the site of the second congress, to be held in 1900, with Germany poised to host the third congress—perhaps in five years. Since that first meeting, the International Congress of Mathematicians has become the most important mathematical meeting in the world.

After Peano's address ("Mathematical logic") and Klein's ("On the question of instruction in higher mathematics,") the congress adjourned, and the participants boarded special trains for one last social event, a banquet on the summit of the Uetliberg, a ridge of hills overlooking the city. "The afternoon was warm, but not uncomfortable," the American mathematician William Fogg Osgood, later recalled. "The panorama of the Alps was unusually distinct and many remained till a late hour in the evening, enjoying the moonlight landscape that lay before and beneath them."²⁶ Writing to Vailati, from Zurich, Volterra had only one complaint: "As I had predicted, given the limited time, it was difficult to get to meet everyone who was present."²⁷ He did speak with two who sang Vailati's praises—the Danish historian of mathematics Hieronymus Georg Zeuthen and the Polish mathematician Samuel Dickstein, who would later commission the translation of Vailati's inaugural lecture at Turin, "The deductive method as an instrument of research," into Polish.

The rapport that existed between Volterra and Vailati shines through their letters, which exhibit far more camaraderie than most of Volterra's

other correspondence with colleagues. Volterra writes to his junior colleague almost as if he were affectionately addressing a younger brother—he encourages and advises Vailati, praises his research, asks his opinion. In the Vailati correspondence, Volterra reveals another side of his personality: humorous, self-deprecating, gifted with a reporter's eye for detail. Flashes of a witty and unique cast of mind gleam from a letter he wrote to Vailati in 1898 from Morges, in the Swiss Alps:

Morges is a microcosm. . . a beautiful panorama, a good hotel, clean streets with stores, many bicycles, intelligent people and. . . even a philosopher: a poor soul struggling between materialism and mysticism, with a strong tendency toward the latter, according to fashion of the last quarter of hour, which suggests long sleeves for the ladies, tight pants, and spiritualism in philosophy, with the related recollection of religious ideas. Indeed, it is for this poor soul that I have a request for you: to send me—if you have a copy available—your review of the history of spiritualism by [Count Cesar Baudide] Vesme and the reference—if you are aware of it—of the journal that published (in French) the articles by [William] Crookes and [Oliver] Lodge about spiritualism. Every other article or review of yours would be very much welcome, but I won't say so, since I can't allow myself to ask for them.

I think you would enjoy yourself very much if you were here.

Unfortunately I am too weak, too far away, and too miserable an echo of your philosophical ideas, and I have more of a habit of contradicting than of supporting them, although I am persuaded that ideas in philosophy are. . . so vast and abused that there is the same tiny difference between supporting and contradicting an idea as there is between $1/\infty$ and $-1/\infty$.²⁸

In 1899, when Vailati decamped for Sicily to become a schoolteacher in Syracuse, Volterra lost not only an assistant but a good companion. In those earlier years, as they strolled the streets of Turin, Vailati regaled Volterra with insights into books he was reading and together they discussed what contemporary philosophers were up to. "What you tell me about the most recent philosophical novelty fills me with wonder," Volterra wrote to him on one occasion. "To understand everything I need another animated discussion between Piazza Castello and Via della Cernaia."²⁹ After Vailati left Turin, he and Volterra would continue their "animated discussions" in the mails.

Like Vailati, Volterra was a bachelor in his Turin years. Many Italian mathematicians of Volterra's era remained, so to speak, single integers, all their days: Cesare Arzelà, Betti, and Somigliana were among those who stayed lifelong bachelors. As the nineteenth century drew to a close, Volterra

found himself facing that most deceptively simple of mathematical questions:
How do you move from one to two?