INTRODUCTION

IN A TEXT FROM the mid-1970s, a mathematician estimated that approximately one million theorems were produced in the world every five years.[1] If this affirmation still applies today, a whole set of questions arises at once. If we are indeed dealing with the authentic production of a massive number of statements, what "recognition" can a given theorem acquire, and to what reality does such recognition correspond? Through what pathways can a mathematical result—and its author, or even its challengers—obtain some form of accreditation? In other words, how does the collective validation of a theorem come about in practice? In particular, in fields that involve highly specialized skills, can the decisive role of "experts" be observed, and, if so, in what ways do the latter intervene in the process of certifying (or rejecting) a given theorem?

These questions will be addressed here on the basis of an actual case study documenting the way a theorem in mathematical logic was developed and accredited in the first half of the 1990s. On the basis of my own observations, textual analyses, and interviews, an empirical study carried out simultaneously with the logical research in question, I shall focus on the various tests to which this statement has been subjected and try to specify the factors on which the recognition it has achieved depends.

In so doing, I shall be exploring an avenue of research that has been largely neglected up to now by the social sciences. Indeed, while Émile Durkheim took "science" and especially "logic" as objects of sociological investigation par excellence,[2] the observations he set forth in 1912 concerning the unfortunate lack of pertinent empirical studies devoted to "logic"—studies destined to establish sociological analysis properly speaking[3]—still apply today. Without celebrating Durkheim's representations of the objects of logic and sociology, the present study aims to help make up for the lack of studies and analyses by seeking to account for certain observable modes of production of certified knowledge in logic in the early 1990s. My aim here is thus as much to take a step forward by offering for sociological reflection a "new" object of empirical investigation—logic being generally understood in the social sciences as an object essentially having to do with method—as to develop tools to describe this object.

[1] See in particular Stanislaw M. Ulam, *Adventures of a Mathematician* (New York: Scribner, 1976), p. 288.

[2] See Émile Durkheim, *The Elementary Forms of Religious Life*, trans. Joseph Ward Swain, 2nd ed. (London: George Allen and Unwin, 1976), pp. 437–38.

[3] Ibid., pp. 431–32.

But what is meant by *certified knowledge in logic*? One of the goals of this study is precisely to identify as specifically as possible the reality to which such a proposition may correspond. Thus I shall be able to attribute a precise meaning to this notion only at the conclusion of my work. However, I shall use the term provisionally here to refer to any statement that is relatively stabilized, more or less broadly accepted, advanced and validated by groups of actors in logic, whether they present themselves as philosophers, mathematicians, researchers in artificial intelligence, or in some other guise. I shall relate anything that these researchers may advance as "theorems" in mathematical logic, as philosophical statements in logic, or as statements attributing the working of a given system (especially in computer science) to logical formalisms.[4]

The problem raised in this book has parallels, needless to say, in various projects undertaken in the humanities and in philosophy, and especially in a set of analyses produced by sociologists of the so-called experimental sciences, analyses based on the study of the research process, the elaboration of facts, and the administration of proofs.[5] In the first chapter, I embark on a discussion with the authors of a certain number of these texts, in a dialogue that will continue throughout the book. For now, it seems important to clarify certain aspects of my approach to the project and of the empirical investigations I undertook in order to "observe" modes of production of certified knowledge in logic.

A Sociologist among Logicians

I carried out my first observations during the academic year 1991–92 at the University of California, San Diego (UCSD). I first sought to identify as exhaustively as possible the various forms of logical activity that were being undertaken on that campus. Because I had been unable to find any sociological studies devoted to work in logic or to the profession of logi-

[4] The term "(logical) formalism" is used here to characterize a (logical) theory expressed with the help of a formal language. More detailed explanations of the various meanings given by me and other authors to this expression will be provided in the course of my analysis.

[5] On this last point, see especially Lorraine Daston, "Marvelous Facts and Miraculous Evidence in Early Modern Europe," *Critical Inquiry* 19 (1991): 93–124; Daston, "Objectivity and the Escape from Perspective," *Social Studies of Science* 2 (1992): 597–618; Simon Schaffer, "Universities, Instrument Shops and Demonstration Devices in 1776," Paris, Séminaire CRHST, "Spaces of Experiment," no. 2, May 10, 1994; Christian Licoppe, *La formation de la pratique scientifique: Le discours de l'expérience en France et en Angleterre (1630–1820)* (Paris: La Découverte, 1996); Alberto Cambrosio, Peter Keating, Thomas Schlich, and George Weisz, "Regulatory Objectivity and the Generation and Management of Evidence in Medicine." *Social Science and Medicine* 63 (2006): 189–99.

cian, the establishment of a cartography of logical activity seemed to be a necessary first step in order to arrive at an empirical understanding of the various exercises to which this term was applied, and in order to make initial contact with the actors involved, with an eye to later, more focused studies. Since there was no consensus among the faculty members I encountered as to how to define the field of logic—indeed, the matter often remained an object of debate—I studied the distribution of often mutually exclusive definitions, without making any a priori exclusions of my own. A phase of preliminary investigation, carried out through a series of interviews, analyses of written accounts of activities, and observations made during seminars, led me to a number of departments specializing in mathematics, philosophy, cognitive science, and computer science.

Prior to taking on this project, I had acquired a general background in science while I was taking more specialized courses in sociology, logic, history, and the philosophy of science. This itinerary turned out to be an asset that made it easy for me to engage researchers in dialogue and read their texts rapidly, without needing to begin by identifying and acquiring a mass of highly sedimented skills and knowledge. But training in mathematics and logic alone would have been insufficient for this project. It would have been difficult for me to construct a problematics from a sociological perspective, to formulate an empirical research project on such an object and not deviate from it, had I not had the opportunity to become familiar with an already vast literature in the sociology of the experimental sciences, and had I not already been interested in the notion of self-evidence in logic and in the possibility of developing a sociological approach to the issue. Since this specific form of "openness" compensated for the "difficulties" deriving from my own form of competence in logic and in science, my background was ultimately beneficial in that it enabled me to *circulate* freely within the framework of these preliminary investigations.

After I had grasped certain phases of the formation of statements in mathematical logic during research seminars by observing the participants' discussions, their interventions at the blackboard, and the written traces they produced, I was finally led by the relatively limited scope of logical activity practiced at UCSD to look for more appropriate sites in which to pursue my problematic. On the basis of bibliographical research and the testimony of various informants, I decided to continue my investigations at the Massachusetts Institute of Technology and Harvard University in a first phase, and at Stanford University and the University of California, Berkeley, in a second phase. Stanford had been represented to me on numerous occasions as one of the foremost centers of logical activity in the world.

Despite difficulties in setting up meetings at these sites with the faculty members involved and in obtaining authorization to observe certain of

their activities, I was able to carry out a large number of interviews, take notes on interactions as they occurred, and observe and analyze a substantial production of texts (on paper, blackboards, or computer screens), especially during research seminars and lectures given by philosophers, researchers in artificial intelligence, and specialists in mathematical logic. In all these spaces, the production of logical knowledge was the work of scholars associated with a variety of disciplines: mathematics, philosophy, computer science, cognitive science, or linguistics.

During my study, I followed the day-by-day progress of several projects involving the simultaneous development of software and logical formalisms, projects carried out by teams in laboratories where I stationed myself as an observer. I used methods that had already been developed and tested in the sociology of the experimental sciences, bringing to bear ethnographic or ethnomethodological approaches.[6] Among the various forms of logic developed by the actors in question, I chose to follow systematically, in the various sites, the steps involved in developing and setting forth a specific logical formalism known as fuzzy logic. This decision was based primarily on a convergence of oral and written accounts that stressed the "tremendous flowering" of fuzzy logic at the time.

What were the material results of this course of action? All in all, between 1991 and 1996 I devoted about a year and a half to on-site observations. Supplemented by retranscriptions of interviews, these observations filled several notebooks. Some of the interviews, seminars, and project meetings were recorded on audio cassettes. I was also given permission to copy video recordings of certain project presentations and seminars, whether I had been in attendance or not. In addition, my research led me to collect a very large set of documents both on paper and in electronic form.

The process of reviewing and partially analyzing these data was spread over some three years. But since I have been talking about "observations," it is perhaps time to specify what sorts of things I was able to *see*.

Observing Demonstrations

In the course of my investigations, the actors I met characterized the objects of logic in contrary ways, sometimes arguing about them among themselves. Their own efforts were in fact invested in quite a wide range

[6] See especially Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills: Sage, 1979), and Michael Lynch, *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* (London: Routledge and Kegan Paul, 1985).

of activities. For my part, not wanting to prejudge what might be characteristic of their practices, I continually asked myself what it was important to observe. I sought to remain attentive to aspects that might have escaped me earlier, and I had many doubts about the object and the appropriate sites for my inquiry. Nevertheless, I was particularly struck early on by the considerable resources in time and energy deployed by the researchers in the activity of *demonstrating*. This work corresponded to the implementation of variable practices, as the following examples will make clear.

Let us consider first of all a project I followed in a laboratory at MIT involving the simultaneous development of a logical formalism and of software. Both were designed to be used to archive and annotate multimedia documents. This project brought together five researchers who spent most of their time preparing or carrying out, on the one hand, presentations at the blackboard among themselves or in front of others to explain the "principles" behind the software and the formalism to be implemented and, on the other hand, computer-based "demos" in front of small groups of invited guests, sometimes including the project's sponsors, to show them how the software worked. These activities were *constitutive* of the emergence of the formalism and the software, as much from the standpoint of pursuing and obtaining financing as from that of its content: indeed, the critiques formulated during these meetings largely determined the project's evolution, down to its smallest details. The moments of demonstration were *essential* to the advancement of this process, and in fact they structured the overall activity of the group.

In the same way, the time devoted to demonstration (including all the requisite preparatory steps) by academics affiliated with the departments of philosophy or mathematics at Stanford was also quite considerable. The preparation and presentation of articles or exposés at the blackboard, implying a large number of interactions and a material work of *writing*, played a central role in the formation and reformulation of statements or proofs. Moreover, the written or oral demonstrations gave rise to stagings that were as theatrical, as routine and stabilized, relatively speaking, as were the "demos."

The activity of logical production thus bore little resemblance to the image of actors working in near-total isolation, and it was not exclusively circumscribed by the minds of individuals. As a sociologist, then, I was not condemned to abandon my investigation for lack of competence to account for the activity in question. The work of logic unfolded through a great number of interactions, and these played an essential role in the production and transformation of statements. The activity involved also mobilized a huge gamut of material resources (written documents and arrangements for writing: blackboards, computers, laboratories, and so on), and its stakes varied according to the actors. In other words, logical

activity looked like an authentic object for sociology, for it was eminently "social," if only because it involved groups of actors whose ties were in part forged or dissolved in the course of frequent and intense interactions.

Moreover, this activity did not appear "abstract" *in itself*: the process of abstraction could be grasped in its materiality.[7] While the activity had to be characterized as "theoretical," it seemed to me that that term should not be used in opposition to the term "practical," insofar as it implied a material work of writing that was carried out at various work sites and that called on a set of particular manual and visual skills. The expression "theoretical practice" thus seemed well suited to account for the specificity of the practices at work.

In other words, I found myself confronted with a large number of *mediations* of logic. I borrow the term *mediation* from studies in the sociology of music dealing with the way music is presented through the media.[8] The history of music seems in fact to have encountered the same difficulties with so-called immaterial objects as the history of logic and mathematics. I use the term "mediation," as opposed to "intermediary," in order to characterize all the resources that can be considered as "go-betweens" or conveyors (texts conveying ideas to readers or instruments conveying music to listeners, for example), and in order to make them visible as beings in their own right and as beings constituting the objects they are said to convey. During my investigation, texts and devices such as computers were often presented by researchers as putting the "principles" of a certain logic to work, while for me, as an analyst, they constituted mediations of that very logic.

Finally, I must stress the fact that the exercise of demonstration always struck me as the activity that best characterized the actors' work, structuring rather than punctuating it. The word "demonstration" implied variable practices—"demos," written proofs, and so on—that could be distinguished through observation, even if these practices were conflated under

[7] See Bruno Latour, "Sur la pratique des théoriciens," in *Savoirs théoriques et savoirs d'action*, ed. J.-M. Barbier (Paris: Presses Universitaires de France, 1996), pp. 131–46; for convergences with the program proposed by Éric Brian for a material history of abstraction, see Éric Brian, "Le livre des sciences est-il écrit dans la langue des historiens?" in *Les formes de l'expérience—une autre histoire sociale*, ed. Bernard Lepetit (Paris: Albin Michel, 1995), pp. 85–98. For a discussion of the place to be attributed to a materialist approach in the social and cognitive sciences, see Dan Sperber, "Les sciences cognitives, les sciences sociales et le matérialisme," in *Introduction aux sciences cognitives*, ed. Daniel Andler (Paris: Gallimard, 1992), pp. 397–420. See also the research in ethnomethodology on the modes of scientific interaction and production with the help of graphic constructions, especially on the basis of "following" a group of researchers in physics in Elinor Ochs, Sally Jacoby, and Patrick Gonzales, "Interpretive Journeys: How Physicists Talk and Travel through Graphic Space," *Configurations* 2, no. 1 (1994): 151–71.

[8] See Antoine Hennion, *La passion musicale* (Paris: Métailié, 1993).

a single umbrella term. The examination of modes of demonstration focused primarily on the intersection between the analysis of technologies of proof and the analysis of forms of ostentation. The observation of a wide range of demonstrative practices, with significant corresponding divergences of principle, allowed me in any event to grasp, without having to pass judgment as to the legitimacy of any particular point of view, a remarkably "chaotic" situation: statements that certain specialists took to be "rock solid" were contested just as vigorously by others, so that there was, on the whole, a pronounced splintering of the logical certainties advanced.

A Necessarily Detailed Analytic Account

In the face of such phenomena, the existing models of the nature and methods of logic that had been developed by the philosophy of logic struck me in numerous instances as of quite limited usefulness. Once the meticulous work of observing had been completed, the task of describing and analyzing proved very complex. Indeed, I found no way to avoid fine-grained descriptions if I wanted to account for the dynamics I had observed in terms understandable to nonspecialists. I could not simply refer to specific documents and developments; I had to present them in their "raw" state. The constraints had to do in particular with the large number of specialized practices and skills brought into play and their deep sedimentation: up to that point, their circulation had been limited to highly restricted circles, which meant that they would be virtually inaccessible to almost all readers unless special efforts were made on my part—all the more so in that by and large philosophy had not taken up these practices and skills as objects of study and thereby done the work of mediation.

In the face of this last difficulty, highlighting a particular dimension of logical activity and the specificity of the objects of the philosophy of logic, I finally decided to focus my analysis initially on a specific type of statement, namely, *theorems* in mathematical logic. Given that theorems are the objects generally perceived (rightly or wrongly) as being at the heart of what logicians do, I chose to study the way they emerge and the way they achieve the status of certified knowledge.

It soon became clear that if I wanted to account in a satisfactory way for the emergence of even one theorem, an entire book would barely suffice. This observation as such represents an important result of my research. This book thus constitutes the product of a necessary, drastic selection from among a very large set of data bearing on the work of actors in logic.

How was I able to settle on one particular theorem, given an annual production that appears quite colossal on a worldwide scale? My choice corresponded first of all to an exceptional opportunity to follow the various phases of the production and presentation of a logical theorem on a "relevant" time scale (the meaning of "relevance" here will become fully clear at the end of this study). The choice also corresponded to a certain "convenience" factor: It would give my readers access to an excellent observation point for a certain number of demonstrative practices and modes of production of certified knowledge in logic, which I was able to find at other sites of investigation as well. In this sense, my choice can be compared to the geneticists' choice of the fruit fly. The metaphor has its limits, however, given that my own inquiry was not based on performing experiments, and of course I do not presuppose the "universality" of certain results.

To be sure, I went beyond the use of methods that some might describe as microsociological and sought insofar as possible to circumscribe the scope of the practices described, to grasp their historical emergence, and to develop a cartography of the relations and groups involved. However, since there was not an already-constituted body of sociological or sociohistorical knowledge of logic on which I could rely, I was unable to go as far in this undertaking as I would have liked; I have only been able to sketch its broad outlines.

In this context, if my research allows glimpses of other pertinent avenues for research, if it offers useful tools for investigation and shows the value of in-depth study of practices and strategies that have been overlooked in sociology and social history, although they correspond to a dimension of scientific activity that is often presented as crucial, it will already have achieved a nonnegligible goal. I should like to think that it might go even further and contribute to a renewal of the questions raised or even of the models presented in the philosophy of knowledge that have to do with the methods and nature of logic.

Grasping the Emergence of a Logical Theorem

The purpose of the analysis that follows is thus, as promised, to attempt to account for the emergence of a particular logical theorem at the beginning of the 1990s and to study to what extent and by what means it managed to achieve the status of certified knowledge. The first question to which we must return is methodological in nature: how was I able to have access to such a process? The description of my itinerary will give a first glimpse of certain logical practices that I shall analyze later in more detail.

As I mentioned earlier, I found myself on the University of California, San Diego campus during the 1991–92 academic year, and I tried to sort out as exhaustively as possible the various forms of logical activity that were carried out in that setting. This led me to a number of different academic departments; a member of the cognitive science department suggested that I should talk to Charles Elkan, an assistant professor of computer science.

During our conversations, Elkan stressed the importance of research on artificial intelligence in computer science departments in the United States. He pointed out the existence of "trends" and remarked on the great diversity of pro-logician and anti-logicist approaches in artificial intelligence. In his descriptions, intellectual and professional investments were closely intertwined. Elkan displayed a pronounced preference for one particular school of thought, indicating his particular interest in the "classical" logical theory known as "first-order logic" and in an "important" logical theory in artificial intelligence in the United States known as "nonmonotonic logic." He cited a certain number of pioneers in this field who were working primarily in the San Francisco Bay area, at Stanford and Berkeley.

According to him, the relatively recent developments of fuzzy logic and of a theory in artificial intelligence called "neural networks"[9] used much poorer and less effective tools than those of "classical" artificial intelligence. When I asked Elkan on what this judgment was based, he said that he had formed his first opinions about fuzzy logic by reading some articles in the mass media about its basic principles and applications to household appliances. To become better informed, he had read an article published in a scholarly journal by an author cited as one of the principal specialists in fuzzy logic.[10] He had also participated in a conference in Australia that brought together researchers working on the "applications" of fuzzy logic.

The definitions of fuzzy logic and the demonstrations of its properties to which Elkan had been exposed led him to formulate a rather negative judgment about the theory. He asserted that the "applications" functioned well, but that this functioning should not be attributed to fuzzy

[9] The name refers to a method that seeks to exploit and implement artificially a biological process attributed to the brain and presumed to account for intelligence: interconnection of neurons in networks. See P. M. Churchland, *A Neurocomputational Perspective: The Nature of Mind and the Structure of Science* (Cambridge: MIT Press, 1989). See also J. D. Cowan and H. D. Sharp, "Neural Nets and Artificial Intelligence," in *The Artificial Intelligence Debate: False Starts, Real Foundations*, ed. S. R. Graubard (Cambridge: MIT Press, 1989), pp. 85–121.

[10] Bart Kosko, "Fuzziness vs. Probability," *International Journal of General Systems* 17 (1990): 211–40.

logic as such. He further declared that he planned to write an article in which he would offer proof that fuzzy logic was contradictory, and he sent me the paper in draft form.

Several months later, early in the summer of 1993, Elkan sent me another version of his article, which was to be presented in July and published in the proceedings of the annual conference on artificial intelligence in the United States, AAAI'93. In this text, "The Paradoxical Success of Fuzzy Logic,"[11] the author undertook a critical analysis of both the "practical" and the "theoretical" developments of fuzzy logic. He began with an attempt to offer proof that a version of fuzzy logic characterized as "standard" and described as a formal system endowed with a specific axiomatics was "in fact" only ordinary logic with two truth values (true and false), even though it was generally presented—still according to the author—as a logical system allowing an indefinite number of truth values. The axioms that constituted the point of departure of this demonstration were four in number (here, "$\wedge$" represents the logical connector "and," "$\vee$" the connector "or," "$\neg$" the connector "not," and "t" represents the truth value of the assertion in parentheses:

$$t(A \wedge B) = \min \{t(A), t(B)\}$$
$$t(A \vee B) = \max \{t(A), t(B)\}$$
$$t(\neg A) = 1 - t(A)$$
$$t(A) = t(B) \text{ if } A \text{ and } B \text{ are logically equivalent.}$$

In support of the path his demonstration had followed, Elkan stated the following theorem: "For any two assertions A and B, either $t(B) = t(A)$, or $t(B) = 1 - t(A)$." Elkan "immediately" deduced from this that fuzzy logic, as a formal system characterized by the four preceding axioms, was "in fact" a logical system with two truth values (zero and one).[12]

Elkan's formulation and his line of reasoning already raise a large number of questions on several levels. At this stage, however, my purpose is not to explore these questions. The access that I seek to construct for my readers, corresponding in particular to the explicitation of the resources that a theorem like Elkan's mobilizes, is a principal concern in the developments to follow. The same thing is true of the proof, which I shall analyze in detail later on.

[11] Charles Elkan, "The Paradoxical Success of Fuzzy Logic," *Proceedings of the Eleventh National Conference on Artificial Intelligence* (Menlo Park, Calif.: AAAI Press; Cambridge: MIT Press, Aug. 1993), pp. 698–703.

[12] Ibid., p. 698.

Following this demonstration, Elkan set forth some critical developments regarding the "applications" of fuzzy logic. The following theses were announced in the summary that preceded his article:

> Fuzzy logic is not adequate for reasoning about uncertain evidence in expert systems.[13] Nevertheless, applications of fuzzy logic in heuristic control[14] have been highly successful. We argue that the inconsistencies of fuzzy logic have not been harmful in practice because current fuzzy controllers are far simpler than other knowledge-based systems. In the future, the technical limitations of fuzzy logic can be expected to become important in practice, and work on fuzzy controllers will also encounter several problems of scale already known for other knowledge-based systems.[15]

Elkan's logical proof lay at the heart of the arguments he developed in his article. This was also the case for the structure of the major controversy, involving numerous researchers in artificial intelligence, that his text provoked as soon as it appeared. The controversy led the author to produce new written and oral formulations of his theorem and its proof, and to take many accompanying actions (the exact nature of what I am calling "accompanying actions" will become clear in due course). This, then, is how I finally found myself confronting phenomena related to the development of a logical theorem, phenomena for which I now propose to account.

What is the best way to grasp these debates and the various undertakings of the participants? While the emergence of a logical theorem, the characterization and modalities of its access to the status of certified knowledge, had never before been the object of a sociological investigation, to my knowledge,[16] certain methodological operations used in soci-

[13] The expression "expert system" has largely become synonymous with "application of artificial intelligence," inasmuch as it is a system of rules making it possible to carry out a "reasoning" process through a computer (for example, to diagnose a machine breakdown). See P. S. Sell, *Expert Systems: A Practical Introduction* (New York: John Wiley, 1985), and Frank Puppe, *Systematic Introduction to Expert Systems: Knowledge Representations and Problem Solving Methods* (New York: Springer-Verlag, 1993).

[14] The author is referring here to all the little "automatic" control devices that are included, for example, in household appliances. On this topic, see for example L. A. Zadeh, "Fuzzy Logic," Stanford University, CSLI Report no. CSLK-88–116 (1988): 20–25. See also R. N. Clark, *Introduction to Automatic Control Systems* (New York: John Wiley, 1962), and Greg Knowles, *An Introduction to Applied Optimal Control* (New York: Academic Press, 1981).

[15] Elkan, "Paradoxical Success," August 1993, p. 698.

[16] As we shall see, Donald MacKenzie's study of the debates over the formal verification of a microprocessor nevertheless offered some interesting points of comparison for my research. See MacKenzie, "Negotiating Arithmetic, Constructing Proof: The Sociology of Mathematics and Information Technology," *Social Studies of Science* 23 (1993): 37–65.

ology to study the experimental sciences could nevertheless be drawn upon profitably here to apprehend such a dynamic. To "follow" the controversy, I looked closely for written traces in various media, and I completed the study through a series of interviews. Oral testimony and written references to other texts were the key elements that allowed this step-by-step approach. However, if it is the case, as I shall attempt to show, that phenomena associated with "referring practices" were integral to the demonstrative strategies that I shall have to make explicit, we can then consider that the principle of circulation was supplied by the various demonstrations of the actors themselves. Thus I developed my own methods of investigation by taking into account the specificity of the set of practices observed—for these practices were quite different from the ones encountered in studies focusing on the experimental sciences.

Moreover, from the standpoint of the skills characteristic of a sociologist, this empirical undertaking could have been relatively classical if, among the media constituting the framework in which the controversy unfolded, there had not been an electronic forum on the Internet. As we shall see, this forum played a very important role in the observation of certain phenomena of which it is in part the origin.

I had discovered the existence of this forum in early 1994 during a conversation with a researcher in fuzzy logic. Numerous messages related to the controversy had been exchanged previously and were no longer directly accessible, but, fortunately, electronic archives had been created. With the necessary computer resources at hand, I was able to access the archives, carry out the corresponding work of connection, collection, and analysis (a new task for a historian dealing with recent events), and attempt to reconstitute the evolution of the debates.

Accessing the Specialized Skills of Workers in Logic

In the hope of enabling readers to appreciate the intricacies of this process, to assess the progressive and relative recognition of the theorem (or rather of the successive theorems), and to grasp the operators of stabilization and the demonstrative practices at work in the debates, I have adopted a particular narrative structure. While I referred earlier to my intention to try to provide readers with a means of *access* to Elkan's theorem, my project in fact extends to the dynamics of production of certified knowledge in its entirety.

This approach entails determination on my part to surmount one of the major difficulties typically faced by a sociological approach to such an object: the risk of producing a text that the bulk of the potential audience, including students and researchers in the social sciences, will find unread-

able. To avoid ending up with a work that is accessible solely to those who practice the activity described, it was important at the outset not to resort to a descriptive vocabulary that would simply echo that of the actors (for want of interrogating it adequately). In reality, I had to take at least two types of readers into account.

First of all, I wanted to provide readers who might have little or no knowledge of logic, and who might assume that such a study was inaccessible, with all the elements necessary for grasping the phenomena under consideration. To this end I traced back certain modalities for learning the practices involved, and I gradually constructed a vocabulary suitable for describing them.

However, I did not want to overlook those who already had some background in logic, and who might be tempted to go "straight to the point," that is, to "form an opinion" about Elkan's demonstrations and those of other researchers, according to a well-internalized academic or professional custom, on the basis of their own practice of logic, without even noticing that the problematics of this research might lie elsewhere. The goal, after all, is to provide the means for analyzing the modalities of a dynamics of production of certified knowledge; thus "judging" the researchers encountered and their production, deciding that so-and-so is "right" or "wrong," is not in any sense the aim of this study.

A reader who formed an opinion at the outset and who deemed, for example, that a particular actor "was right" on the basis of that actor's interventions would still not have the means to know whether the latter had succeeded in rallying other participants to his or her point of view and had actually won the debates. At this stage, readers are actually in the same situation as the researcher was some time earlier: if they want to know what factors played a role in the evolution of the debate and its eventual outcome, they must not neglect any path a priori. That is why I shall spend time analyzing the exercise of logic in detail, by examining the modes of intervention of the various protagonists in the debates (and in particular the modalities through which they take the floor or, more often, intervene in writing).

In this sense, the itinerary proposed to the first type of reader is perfectly adapted to the second type as well. In particular, presenting and analyzing Elkan's proof only at the *end* of the process makes it possible, on the one hand, to supply the "naïve" reader with all the elements needed to grasp it, and, on the other hand, to keep the knowledgeable reader from jumping the gun and mistaking the nature of the exercise.

But let me be more precise about the steps in this process. The Internet discussion group to which I have referred was an essential forum, as we shall see: the one in which the first "public" reactions to Elkan's article appeared, owing to the lag in publication of scholarly journals; I shall

thus begin by examining the noteworthy exchanges that appeared there. Before doing so, I shall take care both to offer the necessary details about the way the forum works and also to use the context of a classroom to analyze the development of the logical vocabulary and the techniques that a number of the actors share (chapter 2). I shall then plunge directly into the heart of very diverse logical practices and into literally cacophonic interventions that will warrant in-depth analysis (chapters 3 and 4).

We shall then look at other mediations of the debates, in particular the various actions that accompany the proofs, publications in other media, and assorted reformulations of the demonstrations, from the very first drafts of Elkan's article to the publication of diverse versions of the text. In this way I shall try to account in a detailed way for the process of producing certified knowledge, for the dynamics of the controversy and the mode in which it reached a stable state. In particular, we shall attempt to understand how, beginning from so many different viewpoints and divergent approaches to the practice of logic, certain researchers succeeded in occupying center stage and certain overtly federating positions managed to impose themselves (chapters 5, 6, and 7).

However, before embarking on such an analysis, it is essential to note the contributions of particular works to the design of this study, along with the questions that these works raise. As a first step (chapter 1), an examination of specific approaches to logic and mathematics in the human sciences and in philosophy will enable us to identify perspectives from which one can gain *access* to the highly closed world of actors in logic.

PART ONE

ACCESSING THE WORLD OF PRODUCERS OF LOGICAL STATEMENTS

What has been able to make social life so important a source for the logical life? It seems as though nothing could have predestined it to this role, for it certainly was not to satisfy their speculative needs that men associated themselves together. Perhaps we shall be found over bold in attempting so complex a question here. To treat it as it should be treated, the sociological conditions of knowledge should be known much better than they actually are; we are only beginning to catch glimpses of some of them.

—Émile Durkheim, *The Elementary Forms of Religious Life*, trans. Joseph Ward Swain, 2nd ed. (London: George Allen & Unwin, 1976), p. 432