Certifying Knowledge

Sociology of a Logical Theorem in the Field of Artificial Intelligence

Claude Rosental

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The Occasional Papers of the School of Social Science are versions of talks given at the School's weekly Thursday Seminar. At these seminars, Members present work-in-progress and then take questions. There is often lively conversation and debate, some of which will be included with the papers. We have chosen papers we thought would be of interest to a broad audience. Our aim is to capture some part of the cross-disciplinary conversations that are the mark of the School's programs. While members are drawn from specific disciplines of the social sciences—anthropology, economics, sociology and political science—as well as history, philosophy, literature and law, the School encourages new approaches that arise from exposure to different forms of interpretation. The papers in this series differ widely in their topics, methods, and disciplines. Yet they concur in a broadly humanistic attempt to understand how, and under what conditions, the concepts that order experience in different cultures and societies are produced, and how they change.

Claude Rosental is a Research Fellow at the Centre National de la Recherche Scientifique (CNRS) in Paris where his fields of study are the Sociology of Logic and the Sociology of the Forms of Demonstration. Author of numerous articles published in France, England, and the U.S., his book *La trame de l'évidence: Sociologie de la démonstration en logique* (PUF 2003) is under review for translation by Princeton University Press. The paper he gives here was part of the book in progress written during his year at the Institute in 2002-03: tentatively entitled *An Historical Sociology of Forms of Demonstration: The Case of Fuzzy Logic,* was an attempt to bring the force of empirical study to a field rarely considered in this context. By analyzing the social uses of demonstrations in their historical evoloution, he investigates whether or not such observation can contribute to our understanding of logical forms as they articulate different spheres of science, technology, and society. A somewhat modified version of this essay appeared in the *American Sociological Review.*

Certifying Knowledge: Sociology of a Logical Theorem in the Field of Artificial Intelligence

While Durkheim ([1912] 1990: 616-17, 625-26) held science, and especially logic, as an object of sociological investigation par excellence, his observation in 1912 concerning the rarity of relevant empirical research on logic,[1] designed to lay the foundations of sociological analysis as such, is still relevant almost one century later. My purpose here is to contribute to the exploration of a world of practices widely ignored by social scientists: I will analyze the concrete modalities of the production and the certification of a particular logical theorem in the field of artificial intelligence in the 1990s.[2] I investigate in particular how this theorem has been collectively accredited[3] in practice and what "recognition" it and its author have received.[4] Drawing on the results of empirical work combining ethnographic observations, textual analyses and interviews, I successively consider the different phases of the emergence of the theorem, from the earliest drafts, to its first publication, and then the several new versions written in response to the various reactions it triggered.

I begin by examining representations of "logic" in the social sciences in order to show that logic should not be considered a static methodological tool. I argue that logic itself is also a proper object of sociological inquiry and ethnographic observation, and a privileged source of exploration of the material and social forms of intellectual work such as the establishment of credibility.[5]

The theorem studied here was first presented in a context of strong competition between proponents of various approaches to artificial intelligence, and was the starting point of a critical article concerning the "paradoxical success" of one of the branches of artificial intelligence known as fuzzy logic. As such, it generated a major controversy in this field: from the first reactions to the theorem, and the strong critiques that followed, a few dominant points of view emerged. How the debates stabilized, and how the theorem was eventually accredited are the key aspects of my analysis.

Let me begin by demonstrating just how much logic—the object of this research—has been disregarded by the social sciences.

Logic as seen by social scientists

Apart from some circles of sociologists who specialize in the study of science, logic is often understood by the social sciences as a method likely to guarantee the coherence of analyses (Passeron 1991: 154-160), to reveal their structure (Berthelot 1996), or to make possible a judgment about the rationality of observed behaviors. Apart from some noteworthy exceptions (e.g., Naville 1982), the representations of logic that one encounters in social scientific literature commonly date either to the nineteenth century or back to Ancient Greece. These are comparable to approaches first denounced then largely abandoned by logicians in the early twentieth century. Logic is presented in such writing as a stable rather than a dynamic instrument and as being structured by the existence of a small set of unchanging and ideal "principles" or "rules," easily invoked and seldom questioned. Reference is most often made to syllogisms (Rosental 2002a). This static view is one of the reasons why logic is rarely perceived as a possible object of sociological inquiry and empirical investigation. But other reasons exist.

First, like intellectual work in general, logic is often perceived as an activity in itself essentially immaterial. As several authors have pointed out, social scientists find it difficult to analyze processes of abstraction in their material dimensions (e.g., Brian 1995; Hutchins 1995; Latour 1996). The production of logical statements is commonly apprehended as an essentially solitary process, the result of purely individual cerebral activity and therefore unobservable by the sociologist.

Second, various forms of idealization and stylization of logical activity have been developed in philosophy. These forms have seemingly managed to seal the genre such that the question of logic is conceived only in the form of a philosophical dissertation, of the study of texts by authors as mediums for ideas, or of analysis of the relationship between a typical knowing subject (not a multiplicity of social actors) and the objects of logic (e.g., Husserl [1901] 2001).

Third, as Lynch and Bogen (1997) have pointed out, "core" sociology textbooks devote no attention to the methodological implications of recent sociology of science. Little or no space is devoted to the sociology of scientific knowledge in general texts on theory and method. This is unfortunate as there is work conducted by sociologists of science and mathematicians that offers evidence for understanding mathematical work specifically (and, logical work more generally) as a collective and "social" activity.

Since the end of the nineteenth century (e.g., Sigwart 1889; Erdmann 1923; Bloor 1976), some authors have been devoted to theoretical discussions about an anthropology or sociology of mathematics and logic.[6] Several historical case studies on mathematics have also been conducted from a sociological prospective, although they have been based on textual analysis. These works are certainly not cumulative: in them, the term "social" takes on differing meanings, and the corresponding programs of research go in different or opposite directions, without generating real debate (Rosental 2003: 33-79). Nonetheless, I will briefly evoke several of these research lines here so as to position this study amongst a set of disparate works.

Some authors, like Bloor, have been particularly interested in exhibiting the lack of consensus over mathematical statements and conceptions, pointing to their variations over time and between societies or other institutional settings. Pushing Lakatos' (1976) critique of "formalist philosophy" further, Bloor (1976, 1981, 1982) argues that formal reasoning must fit with the particular institutional configurations in which it develops. Mackenzie (1993, 1999) has also studied historical cases where mathematical notions were locally negotiated or debated; his case studies relating to the relatively recent rise of computer formal methods have led him to conduct both textual analysis and interviews.[7]

Other authors have not focused their attention on controversies. Some, like Pickering and Stephanides (1992: 139-167) in their discussion of the development of Hamilton's theory of quaternions, have argued that symbolic manipulations are in part objectively constrained and in part determined by the social game in which the mathematician participates.[8] In his study of the crossing point in eighteenth-century France between mathematical calculus and administrative counting that would, one century later become the science of statistics, Brian (1994) argues that mathematicians at the Royal Academy of Science were able simultaneously to configure their objects and institutions through the use of various decomposition practices.

Another form of research that does not look at institutional causality focuses on the textual resources of mathematical practices. In his careful analysis of Gödel's proof, Livingston

(1985) adopts an ethnomethodological stance that takes the actors' competent practice as the ideal metalanguage in which to describe their activity.[9] Semiotic approaches offer a still different view of mathematical practice: for exemple, Brian Rotman (1993) argues that mathematical texts define a triple body of the mathematician in a transhistorical and ideal way.[10]

If most of the previous studies essentially rely on textual analysis (rather than ethnographic observations), and apply to mathematics rather than logic (see also Collins and Restivo 1983; Gingras 2001; Restivo 1992; Restivo, Bendegem, and Fischer 1993; Warwick 1992), I argue that ethnographic observations of logic in action are indeed possible.[11] Through the very performance of such a study, I challenge the view that logical activity is necessarily unobservable by the sociologist because of its immateriality or cerebral embededness: workers in the field of logic should not be treated as atypical subjects defined only in relation to their texts, but as fully-fledged social actors. Their views should not be seen as simply determined by stable institutional configurations, but rather as contributing to the (re)shaping of the social, as in the case studied by Brian (1994). Nor is logical activity solely a textual practice as it has often appeared in the view of mathematics provided by previously cited semiotic and ethnomethodological approaches.

Finally, logical debates should not be treated as simple exchanges of argument. This point is generally not made clear in studies of mathematical controversies based on textual analysis where textual points of view are opposed to one another by the analysts in a relatively homogenous way. Lakatos' (1976) presentation of the controversy around Euler's theorem, in the form of imaginary dialogues, provides an extreme example of such a treatment. I will challenge this possible representation by exploring in detail the material economy of scholarly exchanges and the very mediations of such debates.[12] I will also take up a number of related issues: What does the expression of a point of view in the public and private spheres mean and require for the actors? Who has the resources to express him or herself and in which cases? What individual and collective representations emerge from such interventions in the debate and how? Who reads what, how and why? How are symbolic languages appropriated and do they generate univocal readings of proofs? As a result, do the actors necessarily reach a consensus by means of a simple victory by one side over the other (e.g., Latour 1987:1-100)? Or are misunderstandings possible and can they contribute to the building of specific forms of agreement?

Let us now turn to the heart of the matter: an analysis of the dynamic of the certification of a logical theorem.

Ethnography of a theorem

The theorem analyzed here was formulated in the early 1990s by an Assistant Professor at the University of California San Diego, Charles Elkan. It was published for the first time in July 1993 in the proceedings of a large annual conference in the United States on artificial intelligence, the AAAI 1993 (Elkan 1993). The article in question denounced the "paradoxical success" of electronic and computer applications of a logical theory called "fuzzy logic." His denunciation was based on the proof of a theorem stating that fuzzy logic, characterized by a system of four specific axioms, is in fact nothing but classical binary logic.[13] Elkan presented this result as a direct challenge to one of the founding ideas of fuzzy logic, which is supposed to allow for the expression of an infinite number of degrees of truth along a continuum defined by the poles of "true" and "false" (like "half true," for example).

During the 1991-92 academic year, I too was working at the University of California—

San Diego. While looking for relevant empirical material to contribute to a sociology of logic, I observed that the activity of logical production on campus was neither immaterial[14] nor individual. I confirmed this observation by further ethnographic investigations conducted between 1992 and 1994 at sites such Harvard University, MIT, Stanford University, UC—Berkeley, in the departments of mathematics, philosophy, computer science and cognitive science, as well as at various conference sites. I found logical activity carried out in conference rooms, in laboratories, and in front of computers; it involved the creation of working groups, the organization of seminars, engagement in multiple interactions and an intense practice of writing.

It was during the first phases of this research that I met Elkan. He was then a member of the computer science department—a discipline that brings together a large number, if not the largest number of producers and manipulators of logical formalisms of his generation. He told me about his intention to publish a theorem showing the contradictory nature of fuzzy logic, a project already supported by an initial draft proof. In conjunction with field work on other dynamics of production of certified knowledge,[15] I started to study this author's work, the development of his oral theses and his writings, at a time when his article was still in the draft stage. This position enabled me to relate the writing of his text and amendments to it, to his accounts and speeches (given either in private or in research seminars, for example), and to the study of his daily work, especially the interactions in which he was involved.

For the next phase I investigated the modalities and consequences of the rejection and acceptance of his paper at major conferences on artificial intelligence. I studied the methods for selecting articles for such conferences. I relied again on ethnographic observations and interviews conducted on and outside the sites of the conferences, as well as on texts' analysis, especially those texts produced for the conferences (including brochures, programs, etc).

As soon as it was published, Elkan's article attracted a great deal of attention in the research community. Initially, points of view were exchanged in an Internet forum called comp.ai.fuzzy, devoted to fuzzy logic. Its electronic archives allowed me to reconstruct the discussions. I also had access to the content of private mail, and held interviews with the actors involved. The debate subsequently shifted on to specialized journals. One year after the theorem was first published, the "discussions" stopped. My investigations over this "last" period rely mostly on analysis of texts and interviews.

Between autumn 1994 and 2001, a systematic bibliographical search led me to find traces of some comments on Elkan's article, and citations in comp.ai.fuzzy and in specialized journals. A few articles were also devoted to continuing the debate (Trillas and Alsina 2001a, 2001b; Elkan 2001), but, on the whole, the controversy was clearly running out of steam. The arguments put forward were not really new and triggered no new "outburst" of reactions. An analysis of the context of citations of Elkan's article provides some evidence that the representations of Elkan's theorem have hardly evolved since summer 1994.[16] To date, the outcome of the process, as described here, appears to have "lasted".

How did Elkan's theorem emerge and gain access to a possible status of certified knowledge? Before addressing this issue, I should make clear that the process I am about to study is not universal, in the sense of being identical to that of any other emerging logical theorem. Yet a study of its characteristics is an excellent laboratory for a whole range of probative mechanisms I have encountered in other fields. Moreover, the study of this particular case has the virtue of highlighting the limits of a number of general representations of the modalities of knowledge production in the domain of logic—representations that, as we will now see, are highly reductionist.[17]

The role of interactions and common attitudes

Elkan's theorem was not only the fruit of solitary work by an isolated author. Its formulation stemmed partly from a researcher's circulation in an academic community that denounced—most often orally—the very limited value of fuzzy logic. The author's interactions with academics in that community appear to have played a major part in the progressive elaboration of his article and in the consolidation of his conviction about the accuracy of the first version of his theorem even before he considered himself ready to submit it for publication.

Elkan's research was, as I said, in the field of artificial intelligence. In the context of a wide diversity of pro-logician and anti-logician schools constituting this domain (Graubard 1988), he "adhered" to a pro-logician culture whose favorite research objects and tools were so-called first order logic, often called classical logic, and non-monotonic logic (Elkan 1991, 1992a, 1992b). Elkan could thus be approximately described as an advocate of what was commonly called classical AI, as opposed to the upsurge of rival theories such as fuzzy logic, neural networks or genetic algorithms which offered an alternative to the methods of elaborating electronic and computer products previously developed by "classical" AI (Dubois and Prade 1980; Bouchon-Meunier 1993; Cowan and Sharp 1988; Mitchell 1996).

Elkan worked in a milieu whose most famous representatives were on the faculties of Stanford University and MIT. These academics denounced the very limited value of alternative theories and methods that were also an increasing source of competition for available research resources (industrial and military grants, university posts, etc.). Univocal denunciations by members of this milieu were usually expressed orally and directed at proponents of such theories and methods during chance meetings at the university or in seminars; they frequently took the form of jibes or witty remarks intended to attract attention in public. Their denunciations generally mobilized similar types of argument: they might, for example, assert that texts on fuzzy logic were "incomprehensible" and that it was therefore a "waste of time" to study them or to talk to their authors. The following passage from an interview with a famous researcher in classical AI illustrates this attitude :

> Now with regard to fuzzy logic, I have to admit a reaction to it which is similar to the composer Rossini's reaction to Wagner's operas. Somebody asked him what it was, and he said this is a work one can't make a decision about for the first year, and I have no intention to listen to it for more than a year. So I worked a little bit in fuzzy logic and this discouraged me for looking at it anymore.

Moreover, if the opportunity arose, the scholars of such opinions would refuse, on a variety of other grounds, to finance projects on fuzzy logic, to publish its writings, to sit on a Ph.D. jury, or to recruit a researcher in this area. Due to such practices, interactions with researchers on fuzzy logic were reduced to a minimum and their writings appear to have been largely ignored, although they constitute an abundant literature (on production in the 1970s, see, for example, Gaines and Kohout 1977).

Elkan, deeply involved both "intellectually" and "professionally" in the so-called classical AI community, was imbued with these attitudes towards fuzzy logic when he considered writing a first version of his article. As a result of having read several articles in journals devoted to fuzzy logic, one of which was an article by a researcher in fuzzy logic (Kosko 1990) and one which praised a critique of fuzzy logic (Elkan said he "already knew" at that stage which of the two points of view was "the right one"), and having participated in a conference attended by several researchers in this field (Elkan qualified some of the papers delivered as "incomprehensible"), he was convinced of the intrinsic limits of the theory of fuzzy logic. He noted in an interview that fuzzy logic "applications" might function well, but that this should not be ascribed to fuzzy logic as such.

Elkan gathered his reflections on these interactions and readings in a written draft of an article, which he amended as the months went by. This paper consisted primarily of proof of a theorem which, as it was written during the last quarter of 1991, stated that fuzzy logic, characterized by a system of four specific axioms, was a logic with only one truth value (and not two as in the first published version of this text). According to him, this showed the contradictory nature of fuzzy logic.

Elkan's "negative" representation of fuzzy logic and the absence of elements enabling him, at least initially, to identify a flaw in its proof when rereading it (what Davis and Hersh [1987], notably, have called "the lack of counter-evidence"), were some of the assumptions that allowed him to conclude that his theorem was accurate. Elkan then submitted his paper to an "eminent" colleague working in the field of probability theory. The latter also found no errors in Elkan's proof. According to Elkan, this colleague even added that he had suspected that such a result could be demonstrated and that Elkan's proof had confirmed his doubts about fuzzy logic. This reaction could only reinforce Elkan's conviction that his theorem was correct.

It was only later, in the late stages of the publication process, that Elkan submitted his thesis to other interlocutors (see the acknowledgements in [Elkan 1993: 702]). In this process, he became convinced that the very last step of his proof was wrong. This prompted him, in the process of amending the proof, to formulate a substantially different result—namely that fuzzy logic, characterized by a system of four specific axioms, has a total of *two* truth values rather than only one.

His peers' attitude of patent and avowed disregard for the abundant literature on fuzzy logic visibly contributed towards the stabilization of this version of his theorem. The open self-confidence of their opinions hardly encouraged Elkan to question his assumptions or the representativeness of his readings as regards the characterization of fuzzy logic that he had adopted or to seek other, possibly contradictory, opinions by submitting a draft of his proof to researchers in the field of fuzzy logic itself.

Thus, the actual experience of the "correctness" of Elkan's theorem, for both its author and his interlocutors, appears to have been a matter of a degree of mutual convictions upheld by attitudes amply shared by the group of actors within which Elkan was interacting. Demilo, Lipton and Perlis' (1979) thesis appears to apply in Elkan's case. According to the three mathematicians, the interactions in which the author of a proof is engaged play an essential role in the conviction that she or he can obtain of its validity. The author's conviction would be strengthened by third parties' conviction. Moreover, this example illustrates and extends (to the case of a logical theorem) Lakatos's (1976) claim that the formal presentation of a result, despite the impression of everlasting stability it may provide, often hides a long process whereby the proof, the final and intermediate results, and the concepts involved are negotiated.[18] In Lakatos' view, informal thought always circumvents formal presentations, which in fact only represent the "minutes" of these negotiations.

What, then, was the significance of the submission of Elkan's article for publication for the collective accreditation of his theorem?

Heterogeneous evaluations

Elkan's (1993) article was the object of contradictory decisions (refusal then acceptance for publication) by two separate selection committees: first the organizing committee of a major conference on fuzzy logic in the United States; second, that of the AAAI'93 conference. This result stems primarily from the fact that selection processes were not based exclusively on the direct application of straightforward universal criteria for the validity of proofs;[19] had this been the case, Elkan's theorem would have been evaluated in only one way. The modalities and significance of the outcome of these processes were far more complex, and were indeed completely opaque to anyone who had not followed them in detail.

The selection of contributions to conferences, involved a large number of evaluators—with a ratio of as many as 170 reviewers for 270 texts submitted, as it was the case of the organization of a preceding major conference on fuzzy logic. Selection could be described as the product of a specific *sequence* of decisions by groups of actors. For a given article, the outcome of the selection process depended essentially, as in other areas of scientific research, on reviewers' choices and know-how, the topics chosen for the different sessions (which may not have corresponded clearly to the text), quotas chosen by program committees for each session, representations of the type of publication to select (which were often heterogeneous), and elements of assessment that were constantly the object of creative work, trade-offs, hesitations and uncertainties among the reviewers.

Even this list of several factors can not account for the complex, in part uncertain and creative character of this evaluative work. For reviewers, it consisted of *seeking*, often on a case-by-case basis, several *decisive* criteria on which to base and to justify decisions. Most often the idea was to escape inexpressibility, the vocabulary of taste, or an observation of irrationality or relative arbitrariness of choices, and to replace them by the registers of justification likely to win the acceptance of their potential interlocutors (members of the program committee and sometimes authors, in cases of objections to decisions). Some members of the program committees responsible for assigning articles to the various evaluators said that they chose reviewers for their "capacity to say something" even if they were not always considered to be perfectly capable of understanding and evaluating papers in detail, considering the extreme specialization of the knowledge concerned.

But apart from the diversity characterizing the evaluation of texts in general, a number of other specific elements at least partly explain why Elkan's article was refused for the first conference and accepted for the second.

These two conferences, which brought together academics, researchers working for universities or high-tech firms, engineers, industrialists and scientific journalists, were organized for various purposes. One of the principle aims was to allow actors working throughout the world in the same sub-specialty—whether related to essentially "theoretical" work (e.g. mathematical theory of fuzzy sets) or to the development of a particular type of device via different methods (e.g. the construction of pattern-recognition software)—to meet one another and to interact. But these gatherings simultaneously afforded the possibility for the founders of a research domain to show all participants the fertility and "universal" nature of one or more theories considered to be representative of the approach they had helped to forge (such as fuzzy logic or non-monotonic logic in "classical" AI) through the entire range of its "applications."

Thus, they were at once places for the *gathering* of actors sharing an interest in the same area, tools to facilitate interactions and exchanges, and resources for the organization of *demonstrations of strength* (Mukerji 1997) celebrating the virtues of particular theoretical approaches. For this reason, as far as the organizers were concerned, some of these conferences had to be both relatively "unselective" (that is, with a refusal quota close to one out of two papers) and "representative" of the extreme diversity of research and achievements in the field(s) in question.

Elkan's paper, which was neither a theoretical nor a practical *contribution* to fuzzy logic, was unlikely to be selected for a major conference on that topic. Moreover, the chairman of the program committee understood Elkan's theorem to convey a basic error: it mobilized an axiom equivalent to the principle of the excluded middle in order to describe fuzzy logic. For the chairman, this error was comparable to attempting to show that non-Euclidean geometry is impossible by inadvertently introducing into the proof an axiom of Euclidean geometry. As a result, Elkan's paper was categorically rejected.

In contrast, AAAI's organizers were essentially proponents of "classical" AI methods. This factor is important for our understanding of why Elkan's paper was accepted at AAAI'93. Indeed, this conference took place in a context of very stiff competition on various fronts between proponents of a number of so-called classical AI approaches and those of fuzzy logic. There are few explicit written accounts of the forms of this competition. Some messages posted in an electronic forum devoted to fuzzy logic, comp.ai.fuzzy, have addressed this issue. The following extract of message offers a good illustration of such accounts (page numbering corresponds to that adopted on the forum):

> Of course there is anti-fuzzy, anti-Neuralnet, anti-GeneticAlg, anti-HotNewMethod feeling in the AI community, but it is neither a bigoted nor a hidden bias. Underanalyzed, it may look like a "Not Invented Here" syndrome. (J. Pollack, article 856).

As a result of the competition between sub-fields of AI, very few papers on fuzzy logic were delivered at the AAAI. Indeed, it is easy to imagine that the proponents of "classical" AI methods in charge of the organization of the AAAI were not inclined to promote the approaches of their competitors by publishing their articles. And as a general rule, fuzzy logicians did not even try to send their articles to the AAAI; their behavior was based on previous bad experiences-their infrequent attempts to submit paper rarely led to publication. This situation was partly pre-orchestrated by a powerful device: when submitting a paper at the AAAI, authors had to indicate in which session their text might be presented; when so doing, they had to choose a category of session within a list which did not contain "fuzzy logic."

In the context of heated competition between sub-fields, conference organizers of the AAAI failed to perceive any flaw in Elkan's theorem. They then not only decided to publish it, but to promote it by awarding it a prize. It was listed as one of the four best articles at this conference. Indeed, it appeared that Elkan's article could be seen by proponents of "classical" AI approaches as a precious tool in the fight against their opponents.

The acceptance by AAAI of Elkan's theorem, and its refusal by the fuzzy logic conference, brought into play a set of mechanisms, competencies, considerations and (partly contextual) decisions, which differed from one forum to the next. The selection of articles did not amount to a mechanism through which to identify theorems that any reader could apprehend as being true (or, if not selected, false).

It seems that these phenomena were not specific to the conferences in question. Indeed, in the field of mathematics, according to Davis and Hersh (1987), any article submitted to a journal can be refused or accepted, depending on the reviewers concerned; decisions on the satisfactory nature of a proof, on that which is problematic and on that which is interesting, are based on "corporatist" expertise that is often inaccessible for individuals outside a sub-discipline; an author's reputation, her/his reliability and originality, and the reviewing committee's representation of the research field covered by the journal, are all key elements in the acceptance or rejection of an article, especially since reviewers generally do only partial checks and trust the author in regard to the rest of the proof.

In the case of Elkan's theorem, the actors working in the research fields concerned (and especially those who had participated in the selection of papers) were fully aware of the complicated meanings of a paper's rejection and acceptance. Depending on how much they knew about the dynamics of evaluation of papers for the conferences on fuzzy logic or for the AAAI'93, and depending on their methodological options (which could prompt them to state that they could "guess that such a theorem could be proved" or, by contrast, lead them to think that such a theorem "could only be false"), they could interpret publication of Elkan's theorem and the prize awarded to him very differently. Some took it as a sign, considered more or less certain, of the correctness and relevance of the theorem; for others it was seen as overt antagonism towards fuzzy logic. The following message from comp.ai.fuzzy illustrates the later point:

> It is clear from papers such as Elkan that not enough is being done in certain forums to assure that papers dealing with fuzzy logic, either pro or con, are subjected to a fair and competent review process. Many technical conferences and meetings are, in the views of many in the fuzzy-logic community, unfairly hostile to fuzzy logic, while being, on the other hand, ready to accept the work of skeptics with nary an effort to determine its value. (E. Ruspini, article 816)

What is indisputable is that the prize awarded to Elkan's article triggered strong reactions by researchers in fuzzy logic and generated debate in several quarters. As soon as the article was published, comp.ai.fuzzy became the main locus of an exchange of opinions on Elkan's theorem for a period of six months, at which time the first reactions were consolidated in the form of articles published by specialized journals. What were the first representations of Elkan's theorem that emerged from debates on comp.ai.fuzzy during that period?

From the original proof to its substitutes

Authors and readers of messages posted on comp.ai.fuzzy were mostly academics, researchers and engineers working for universities and industrial organizations in the fields of fuzzy logic and "classical" AI. Due to its absence of selective or editorial constraints, the forum helped to open relatively "broad" debate within this specific gathering, and allowed the public expression of many points of view. The breadth of opinions expressed there was due not least to the possibility of publishing essentially rough texts written, for example, during a "break" of a few minutes.[20]

In this framework, what was the nature of the interaction around Elkan's theorem? At first, the author was accused of errors: the exact significance and scope of his theorem, as well as the nature of the hypotheses mobilized in his proof, were criticized. As time went by, the messages revealed that Elkan's article was hardly accessible and/or seldom read, as the following examples indicate:

> Elkan's argument is a straw horse, from what you have said of it. [. . .] I

would like to see what he has to say. [. . .]where can I get this document? (J. Wiegand, article 767)

"Is Elkan's paper on the reducibility of Fuzzy Logic available on the net? Thank you for your help." (Allen, article 1778)

There was a proliferation of "abstracts" and "presentations" of the theorem and its proof designed to support the critiques. Although they were presented as faithful substitutes for Elkan's productions, in reality they were all reformulations of the theorem, constructions of *new* demonstrative mechanisms, often elaborated from messages displayed previously in the forum—in other words, from secondary sources. Saying what the theorem and its proof consisted of, or judging them by writing a review on the subject, were therefore basically the same exercise, amounting to the production of new proof.

Points of view were often drafted by using passages from previous messages and by inserting comments in the course of a step, considered critical, in a proof or counter-proof. This mode of writing spawned collective texts consisting of several levels of citation and proof sequences. It also led to explicit disagreements, for some participants wrote that they failed to see at such-and-such a step what others had shown or claimed to see.[21] The extract of the following message will clearly illustrate this point (note that use of the symbol " >," generated by email software in the formulation of answers to former messages, corresponds to quotes; a sequence of several symbols of this type corresponds to the same number of levels of citation):

>>Zadeh's logic is distributive and, therefore, the roots of Elkan's mistake do not lie on distributivity but on failure of the law of the excluded middle (as correctly pointed out by Kroger).

Well *let's look at this again*. We can simplify Elkan's proof to the following: Starting from Gaines axioms t(true) = 1, t(not A) = 1 - t(A), t(A or B) = max(t(A),t(B)), t(A) = t(B) if A and B are logically equivalent. We may consider t(A or not A) = t(true) by the last axiom => max(t(A), 1-t(A)) = 1 => t(A) = 1 or t(A) = 0. I think the problematic step is the first one, the use of the last axiom. *I don't see the excluded middle coming in here*. (H. Lucke, article 827, emphasis added)

What representations of Elkan's theorem resulted from this set of proofs?[22] As mentioned above, considering the material difficulties of obtaining access to Elkan's original article, but also the small number of individuals who actually read it attentively—due primarily to the fact that many participants spent very little time on these debates—it appeared that representations of the theorem were shaped partly on the basis of "substitutes" proposed in the forum. Some participants thus either strengthened or completely reversed their point of view on this theorem after reading earlier messages. The following extract of a message constitutes a good example:

>>t(not(A and not(B))) = t(B or (not(A) and not(B)))

This statement was worrying me too. I was wondering if it really should hold in fuzzy logic. There are other statements in classical logic that don't work in fuzzy logic. It would seem the answer is no, it wouldn't. I wasn't sure if I was missing something though, but I think Loren's post illustrates

> the mistake in it. (S. Kroger, article 1823)

On the other hand, most of the participants, after reading those same messages, maintained positions based on antagonistic normative approaches as to what logic, fuzzy logic, a logical proof or ways of discussing it were *supposed* to be.[23] Therefore, certain participants expressed contradictory opinions as to the usefulness—or the need to maintain—the law of excluded middle:

> This doesn't make sense to me. I cannot imagine a scenario in which I was uncertain about the truth of (B or not(B)). (H. Lucke, article 813)
>
> You're still thinking Boolean. [. . .] With fuzzy logic (if B or not B) has much more meaning than in Boolean (it's meaningless in Boolean). (Y. Tanaka, article 814)

Others highlighted norms for holding a debate, as in the following passage:

> I did not reply to his [Elkan's] post since it was directed at an emotional level. (J. Wiegand, article 832)

In particular, sometimes highly stabilized approaches regarding priorities in the writing of proofs such as Elkan's—that is, regarding the most essential elements of proof—were then at play.[24] Although formal proof provides some instructions for its own reading, the heterogeneity of conceptions at work in the practice of logic, which corresponded to the heterogeneity of know-how with which the participants in the debate were endowed (apart from a minimum of shared competencies), worked against the replication of readings desired by the authors of these articles.

This type of interpretation might serve to provide a resource for understanding why logic (like mathematics) is even debated at all, a state of affairs hard to explain if one considers symbolic writings as such to allow for immediate consensus. It might also give meaning to Demilo, Lipton and Perlis' (1979) claims, by extending to the field of logic their observation that the vast majority of mathematical theorems are contradicted or disqualified—when they are not rejected or simply ignored by mathematicians.

In any case, highly fragmented and contradictory points of view about Elkan's theorem and its proof resulted, especially in regard to questions of their validity, scope and exact meaning. While certain authors affirmed that Elkan's theorem was correct, others stated that it was not. Various reasons were invoked, including his introduction of axioms inadequately characterizing fuzzy logic, the improper introduction of a principle of distributivity (of and/or) in the proof, the implicit and erroneous introduction of the law of excluded middle, the inappropriate nature of the notion of logical equivalence mobilized, the introduction of erroneous numerical equalities, or the incorrect "injection" of binary values into statements about fuzzy logic.

After a few months, some few points of view started to acquire more visibility than others: those of Elkan and those of several fuzzy logic personalities working in computer science, such as Didier Dubois and Henri Prade from the CNRS (France) and Enrique Ruspini from SRI (USA). Before they were able to express themselves in journals, Dubois, Prade and Ruspini had put in token appearances in the on-line forum to counter-balance the proliferation of representations of fuzzy logic, and of the theorem, that were too distant from what they considered to be reasonable or desirable. Several mechanisms were adopted by some researchers to make certain messages more visible than others: citations and frequent reappearance, with placement of some texts in a data base accessible via Internet, the electronic address of which was often displayed. The following extracts will illustrate this point:

> Please read especially the enlightening and very well written evaluation by Enrique Ruspini (sent in the next mail). [. . .] Again, please read especially the enlightening and very well written evaluation by Enrique Ruspini (first article in this mail)." (W. Slany, article 1823)

> Two responses to Elkan's paper, one by Enrique Ruspini and the other by Didier Dubois and Henri Prade, may be found as ftp.cs.cmu.edu:/user/ai/areas/fuzzy/doc/elkan/response.txt. (FAQ, article 2072)

Mechanisms such as these promoted differentiated visibility of messages; the texts displayed in the forum did not have the same impact on the shaping of further representations of Elkan's theorem. The capacity for conviction that could be granted to these new proofs and counter-proofs of the theorem was therefore not only *limited*; in the very frame of this material economy of access to texts, it was also *variable*.

Hence, the shaping of representations of Elkan's theorem in the context of the forum did not derive from a sum of individual homogeneous examinations of its proof obtained from attentive readings of an easily accessible text. Nor was this process reducible to an exchange of *arguments*, or even *discussions*, given the scriptural dimension of these interactions, and the establishment of the aforementioned material economy of access to texts. Finally, the process of dialectical evaluation to which Elkan's theorem had been subjected by no means resulted in a clear and uniform collective view of its correctness. Even the apparently simple question of the exact scope and significance of the theorem was the object of a multiplicity of representations.

Polysemy, differentiation or coordination of points of view?

In the eyes of many leaders in the field of fuzzy logic, the publication of Elkan's theorem could have resulted in a substantial loss of credibility for their research domain. The following passage provides a good example of such a concern:

> The impact of the paper should absolutely not be underestimated. The AAAI proceedings are very well read in the AI community, and I fear that this article will destroy some of the trust that has so far been given to fuzzy logic in the AI community. This is a very dangerous situation as it might entail academic hostilities between the AI and FL communities. (W. Slany, article 1823)

Several of these leaders rallied to organize an "effective" counter-attack on several fronts. In conjunction with their intervention in the electronic forum, a protest letter was addressed to the organizers of the AAAI'93, and plans were made for the publication of several answers to Elkan's article.

Thus, several months after the conference, the center of debate shifted from the electronic forum to journals that specialized in artificial intelligence. This shift of *exhibited interaction* to other arenas was accompanied by a radical transformation in the time-scale of debates, and a substantial rise in the threshold of investment that had to be surmounted in order to "stay in the game". Henceforth, to make a point of view public, authors were now required to produce sufficiently polished texts and to subject them to editorial constraints. It also required full investment in a milieu in which inter-individual relations and "reputations" were essential in the processes of selecting (and often commissioning) articles, as Davis and Hersh (1987) have also noted in the field of mathematics.

Some researchers in fuzzy logic already had extensive experience of this type. For many years they had endured a wide range of criticism leveled at their research domain; they had already, prior to this controversy, compiled what they called a "big collection of foolish quotations." They had thus accustomed themselves to defending fuzzy logic and to act as its spokespersons, gradually building up real repertoires of counter-proofs for both written and oral use. In the latter case, the repertoires were often supplemented with a store of witticisms and amusing anecdotes, thus echoing the jibes of fuzzy logic's critics. The reactions of this group to Elkan's article were based on already well-established know-how and wide experience with methods of counter-attack.

Endowed with these specific competencies, several of the defenders of fuzzy logic united to formulate a response that would stand in deliberate contrast to the cacophony of messages in the electronic forum. The repetition on the net, the constant attack upon the same elements of counter-proof, was a source of *discredit* to Elkan's theses, but was at the same time likely to give the impression that all researchers in fuzzy logic stood against the theses in a knee-jerk fashion. No fewer than eight major researchers in fuzzy logic, including Didier Dubois and Henri Prade, co-signed an article whose content was close to that of several of the messages in the electronic forum, thus subjecting Elkan's point of view to the test of numbers in case their approach was not enough to marginalize it (Berenji et al. 1994).

Yet this unity among the stars of fuzzy logic was only apparent. The computer scientist Bart Kosko from the University of Southern California, author of the article that had served as a starting point for the formulation of Elkan's theorem, had not participated in the debates (at least not to my knowledge). He defended a definition of fuzzy logic that, although increasingly visible and supported by a growing number of proponents, was nevertheless challenged by other researchers in fuzzy logic (Kosko 1993; Ruspini 1993a, 1993b; McNeill and Freiberger 1993a, 1993b). At the time of publication of Elkan's theorem, two coalitions of actors were thus emerging, each defending rival views of fuzzy logic; this conflict was increasingly open (Rosental 1998b). Publication of Elkan's theorem had helped to mobilize certain "big names" in fuzzy logic against the Koskoian view. Faced with spokespersons of fuzzy logic who had countless reasons not to encourage his publication in journals, and with the risk of being caught in a crossfire if he publicly expressed his point of view, Kosko notably, if not paradoxically, kept silent. In so doing he helped to enhance the image of unity between fuzzy logic researchers in regard to Elkan's theorem.

The multiplicity of counter-proofs formulated in the electronic forum were thus succeeded by a small number of interventions by recognized spokespersons for fuzzy logic organized around united points of view. "The" theorem of Elkan was presented, in particular, as a result that was "in fact" very simple (when reformulated "appropriately"), "known for a long time" and "without any effect" on the foundations of fuzzy logic. The image of two-sided confrontation between Elkan's point of view and "that" of researchers in fuzzy logic was progressively built up through additional approaches such as a co-signed article, for example, or publication in journals (and shortly afterwards in the electronic forum) of "abstracts" of published papers, in which the standpoints of participants in the debate were simplified and frozen in a conflict reduced to its simplest expression.

In particular, once the debate had shifted to journals, a FAQ (Frequently Asked Questions) section was created in comp.ai.fuzzy that presented interaction around Elkan's theorem by contrasting two, "single" or "distinct" points of view.

> The presentation of Elkan's AAAI-93 paper [. . .] has generated much controversy. The fuzzy logic community claims that the paper is based on some common misunderstandings about fuzzy logic, but Elkan still maintains the correctness of his proof.

AI Magazine highlighted the following opposition in oversized letters :

> The theorem in my paper is correct [. . .] (Elkan 1994b)

> The author fails to acknowledge that by definition, classical equivalence does not apply to fuzzy assertion. (Berenji et al. 1994)

As mentioned above, considering the editorial constraints that required a heavier investment in contributions to the debate, the possibilities for publicly expressing a point of view were substantially reduced for some. The journal *IEEE Expert*, by eventually devoting a special issue to the debate on Elkan's article, in August 1994, had nevertheless opened an exceptional forum for the question. But even this apparent opening remained relatively limited: the authors it included were more or less the same "stars" whose opinions had already appeared elsewhere, such as Didier Dubois, Henri Prade, Enrique Ruspini, and the computer scientist Lotfi Zadeh from UC Berkeley, who was generally reputed to have "invented" fuzzy logic.

Although the number of participants in the debate grew smaller, and although most of those who had participated in the electronic forum disappeared, this did not mean that there was any consensus over the limited number of emerging points of view. What appeared to be a sudden unity of opinion owed its coherence from the fact that some had managed to remain in the debate and to enhance its resonance while others had kept silent because they were unable or unwilling to overcome the obstacles to asserting their opinions. This singular dynamic worked powerfully towards stabilization of the debate. What, now, about Elkan's approach and that of his partisans? What were their reactions to this counter-attack?

The series of viewpoints expressed in the forum and in private letters sent to Elkan by researchers in fuzzy logic, as well as in personal conversations with them, had allowed him an opportunity to clearly perceive the divisions that existed between researchers, especially as to the definition of their own research objects and their representations of his theorem and its proof. The existence of these evolving rifts gave Elkan the opportunity to multiply reformulations and personalized approaches. This, in turn, afforded the chance to quiet antagonistic reactions to him, and to "solidify" his results—or, more precisely, what he could re-present in each case as "the nature" of his results. This was a considerable resource for the author of the theorem, for it emerged in circumstances wherein little support for Elkan's theses had been publicly expressed during the months immediately following publication of his article.

Elkan thus had the opportunity to elaborate and test several different reformulations on diverse interlocutors. He adapted his talks to suit the circumstances in which he delivered them, adjusting his presentations in differentiated, evolving and sometimes-personalized ways. To borrow from Goffman and Habermas, Elkan was more involved in dramatic than in communicative acting (Goffman 1959; Habermas 1984). His talks were given in a way that allowed him to curb the virulence of the counter-attack by researchers of fuzzy logic without, for all that, deceiving researchers in "classical" AI.

In one instance, for example, Elkan formulated a private answer to a criticism of the "implicit" introduction into his proof of an axiom ("equivalent" to the law of excluded middle) rejected by fuzzy logic in order to obtain the theorem: "the average user" of fuzzy logic could fail to be aware of the impossibility of mobilizing this type of axiom, despite it being "so usual". And while, according to one of Elkan's interlocutors, on the day Elkan delivered his paper at the AAAI'93 he affirmed that he did not see why he could not introduce the law of excluded middle, Elkan subsequently claimed, in another context, that he had used an "equivalent" axiom in his proof by attributing an essentially educational value to his theorem.

Such nuances underline the primacy of public expression for grasping acts of enunciation (Quéré 1990). Because his article, a singular material device launched in the world, eventually proved somewhat ineffective in countering criticism, Elkan *added to it* by producing new texts and new speeches. He thus provided new instructions for its interpretation and general comprehension thereby forging new tools for changing readers' relationship with his original text.[25] Such adjustments also helped to stabilize debate since they limited disagreements by making them appear retrospectively as misunderstandings (which differed, of course, depending on the interlocutors and publics).

Elkan thus undertook a fundamentally different exercise than did his opponents. While their idea was to organize their debate as a stable and coordinated counter-attack, Elkan proved to be highly mobile and a talented mediator capable of producing different and evolving answers. He had to be more skilled at improvisation than orchestration. For him this appeared to be no routine exercise but rather a new, ad hoc approach.

Yet after a few months, Elkan forged tools that enabled him to limit his production of differentiated answers that had become overly demanding of both his time and energy. In a new version of his article published in the journal *IEEE Expert* (Elkan 1994a), a draft of which was available on the Internet at the end of 1993, he had perfected reformulations that he could present to widely diverse publics. These reformulations did not concern the core of the proof itself, but the formulation of the theorem, the comments on the result, and the footnotes. Considering their polysemic nature, their effect was twofold. After the different readers or listeners had read or heard these reformulations, they could adopt radically divergent points of view on the nature and meaning of his theorem. But at the same time they could agree to grant it a form of correctness that was obviously variable and, to a certain extent, case dependent.

For example, in the new version of his text (the November 1993 version noted above), Elkan stated that the four axioms he had used to prove his theorem offered an "apparently reasonable" description of fuzzy logic. This expression could be interpreted in different ways by its readers. For some researchers in fuzzy logic who were relatively well informed as to current debate on the subject, its use might have signified that Elkan had taken into account criticism on the "limited" scope of his theorem, and that the "misunderstanding" was therefore over. However, a reader with little knowledge of the debates or the literature of fuzzy logic could continue to attribute a far more general scope to his theorem.

This diversity of possible readings was also maintained, throughout Elkan's text, by expressions alternately supporting either one interpretation or the other. The following extract offers a good illustration of this phenomenon

> The equivalence used in [my] Theorem [. . .] is rather complicated , but it is *plausible intuitively*, and it is *natural* to apply in reasoning about a set of fuzzy rules, since ~(A ^ ~B) and Bv(~A ^ ~B) are both re-expressions of the classical implication A -> B. It was chosen for this reason, but *the same*

> *result can also be proved using many other ostensibly reasonable logical equivalences*. (Elkan, November 1993 draft, emphasis added)

In other words, the new version of Elkan's article mobilized many ambiguous expressions to describe the ins and outs of the theorem ("plausible intuitively," "natural," "ostensibly reasonable"). As a result, readers could perceive the scope of the theorem in various ways that suited them.

Elkan could easily use this type of text in discussions with proponents of "classical" AI to assert that he had stood his ground and proved a theorem showing "the limits" of fuzzy logic. But he could also affirm "without any contradiction," to fuzzy logic researchers, that he had essentially formulated constructive criticism and had delivered an educational message by proving his theorem, to the point of being able to consider collaboration with them in the future. Indeed, after a long discussion with one of the leading figures in fuzzy logic, Elkan and his interlocutor had considered co-authoring a paper.

The polysemic nature of Elkan's text thus enabled the author to assert "the" correctness of his theorem in various modes in his private interaction. He could also rely on a single reformulation of his article for all his answers, thus avoiding the need to multiply adjustments to suit the forums in which he had to express himself.

The existence and evident success of this type of polysemic text may be seen to illustrate the limits of semiotic approaches in mathematics that seek to establish the univocity of signs while overlooking the possible failure of the effects apparently programmed in them. Another example of this kind of analysis can be found in by studying the work of Marcel Proust (see especially [1919]1989: 258-259). Proust demonstrates, through the subjectivies of his characters, how individuals understand (or intend) messages differently from (or for) people around; these differences depend upon the slightest of material mediations of their interventions. Proust's work might be said to illustrate the regular failure of communicative aims by insisting on the divergent understanding of even those signs that may seem to convey the most unambiguous meaning.

Yet it is the case that the stabilization of the formulation of Elkan's theorem caused the debate to run out of steam once different forms of agreement became possible. After *IEEE Expert* had devoted a special issue in August 1994 to reactions to the theorem, in which a few papers by important representatives of both fuzzy logic and "classical" AI were published side-by-side, there seemed to be no fundamentally new elements left to advance. However, as these debates finally appeared to be drawing to a close, no real consensus had been reached. Agreement that seemed to have formed around the statement "the correctness of Elkan's theorem" was simply apparent and had to be understood in relation to the *distribution* of distinct and often antagonistic points of view to which its expression corresponded. It was a matter not of a univocal statement representing a sublimation of temporary divergences but of a *collective statement*.

The latter term, coined by the medievalist Alain Boureau (Boureau 1989, 1992), appears indeed appropriate to describe the situation under study. "Collective statement" could be described as a cousin notion of "boundary object," with which sociologists of science are more familiar.[26] "Collective statement" refers to a "verbal or iconic fragment that creates around itself a certain convergence of languages and thoughts, through the play of a structural fuzziness allowing to capture a still implicit thematic and to welcome the most diverse projections and appropriations" (Boureau 1992: 1072). Such was for example the case of the statement "Vox Dei, vox populi" ("Voice of the people is voice of God"), which played an important role in the building of the English nation between the eigth and the twelfth centuries.

Similarly, the statement "the correctness of Elkan's theorem" was appropriated by each participant into their own specific mode of agreement while at the same time managing to serve as a point of coordination for various points of views regarding fuzzy logic. Thus, while it became necessary to talk about the "recognition" of Elkan's theorem and its certification by specialists, such "recognition" was possible only a distribution of partly united representations in a void produced by the increasing scarcity of publicly expressed points of view.

The changing shape and general evolution of representations of Elkan's theorem—like the reduction of a set of points of view expressed publicly in a small number of contributions to journals—did not occur as a simple formulation of more or less convincing univocal *arguments.* Other elements also played a decisive role in this dynamic, particularly the polysemy of Elkan's reformulations, his flexibiliy in producing evolving and differnentiated discourses, and certain other actors' capacities to coordinate with, consult and co-opt one another, and thus to manage the public visibility of their texts. Such competencies were all instrumental in stabilizing debate, and indispensable to the constitution of shared "evidence" of a statement lastingly endowed (according to my investigations, the situation has hardly evolved since summer 1994) with multiple significations: in other words, as "the correctness of a theorem" as a collective statement.

Conclusion

In the course of this analysis I have shown how "recognition" of a logical theorem depended on the combination of a multitude of conditions, and on the author's ability to overcome a set of distinctly different trials.[27] While the fact of *effectively* having empirically studied the steps in the emergence of a statement of this kind (in particular through observation) is in itself the first fundamental result of this research, the fact of that irreducibility is the second result. In a case such as the evaluation of Elkan's theorem, only an examination of the entire sequence of diverse mediations at play allows us to grasp the reality to which notions of "recognition", "approval" and "certification" of a statement correspond.

Note that although such mediations are often disregarded in stylized descriptions and in certain epistemological analyses, one should not conclude that this approach is an exercise in denunciation. The researchers involved did not try in any way to hide these mediations. In fact they often even saw them as subjects of debate and objects of more or less elaborate or allusive reflexive formulations.

In keeping with the results of this study, I therefore have no more intention now than previously to *stylize* the case under consideration. We end up therefore with a description of the shaping of representations of a theorem that defies a whole range of reductionist approaches.

First, this dynamic does not simply derive from a production of abstract reasoning, exchange of ideas, or verbal arguments. Representations of Elkan's theorem were primarily constituted during interactions between readers and texts. They were partly determined by the existence of a general economy of access to original texts and by the management, on the part of certain actors, of the visibility of their written production. Moreover, the expression of a particular point of view was, as such, a problematic act implying as it did various degrees of investment, as well as conditions that were not always met for a particular actor at a particular point in time.

Elkan's theorem was not evaluated collectively through the aggregation of attentive readings of the proof proposed in his article. The readings it did receive were often rapid and partial—when they existed at all. In the context of what could roughly be called the imperfect reading market (considering the limited resources of the actors for practicing reading, especially in terms of time, know-how and possibilities of access to the texts), points of view on Elkan's theorem were largely shaped on the basis of texts presented as faithful "substitutes" of the author's approach, and in the framework of a series of representations. If only for this reason, certification of the theorem could not be reduced to the systematic application of universally shared criteria of evaluation of a proof. This type of view would, moreover, have been particularly simplistic in so far as both Elkan's proof and its substitutes were confronted with a wide diversity of modes of reading and ways of practicing logic, especially as regards the definition of priorities in the drafting of proof.

In fact, simple agreement on the question of the correctness of Elkan's proof was particularly difficult to obtain because his texts were not read in the same way, despite the use of shared symbolic languages. Even before the problem of validity of the theorem, the protagonists faced the question of its exact signification and scope, closely related to that of the nature of the hypotheses mobilized. Answers to these questions were by no means unanimous. Admittedly, the proliferation of proofs and counter-proofs, versions of articles, and different and polysemic messages by Elkan, also contributed to a large extent to that state of affairs.

Moreover, the formation of representations of Elkan's theorem was not a sum of strictly individual approaches. It was very much a question of collective actions involving a substantial amount of coordination and set in the struggles between coalitions of actors, the configuration of which evolved partly in line with viewpoints expressed during the debate. This coordination of viewpoints and the management of their visibility by means of various mechanisms was one of the elements clearly showing that the practice of logic did not simply amount to the production of reasoning, whether oral or written.

Participants in the debates could not finally be compared to homogeneous or typical subjects, revealed or formatted solely in a relationship to the texts or logical ideas. They were social actors, participants in long-term and large-scale conflicts, engaged in sometimes rival activities and projects, endowed with variable resources and competencies, and therefore neither "typical" nor substituable to one another. Although entry into the debate was marked by the adoption of a process approach to phenomena, it appeared clearly that a better grasp of the actors' positions required a far more comprehensive and in-depth inquiry, which led in turn to the latter result.

Another point characterizing this study is certainly the fact that it reveals an absence of "signs," especially institutional ones, that could clearly be interpreted by the actors or the social scientist as guarantees of an unquestionable certification of the theorem. In particular, various significations could be associated with its publication; understanding of this significance varied with the interlocutor's familiarity with the modalities of selection that the paper had overcome.

Even apparent agreement on the statement "the correctness of the theorem" was not easy to interpret. In the case studied, we saw that it could correspond to very different representations of the signification and scope of the theorem. What agreement there was stemmed mainly from the emergence of a collective statement, appropriated in a wide variety of means. This type of result shows the extent to which it would have been simplistic to pose a priori the problem of the certification of the theorem in terms of univocal rejection or acceptance, and to believe that the use of symbolic languages was a panacea in the struggle against all ambiguity.

Furthermore, in so far as this apparent agreement was also in part due to the fact that not all participants managed to remain in the debate, we can see that the evolution of the public representations of Elkan's theorem did not operate only in the mode of conviction. Remember that those who disappeared from the debate had not always been convinced by other emergent points of view nor had they been prompted to renounce antagonistic normative approaches of logic to which they had possibly adhered previously.

Studies carried out in the sociology of science, given their tendency to present the emergence of scientific facts as victories of one side against another, have hardly accustomed us to agreements on statements based instead on divergent points of view wherein broad and straightforward consensus is not eventually reached.[28] Yet my research on other dynamics of the production of certified knowledge in logic suggests that the case studied here is a good laboratory for everyday situations. At the cost of a highly detailed analysis, I saw that apparent agreements on a variety of statements stemmed from relative misunderstandings, from points of view that were more or less visible, even if the number of specialists and the differences of representation at play were often much fewer.

The fact that some of the debate took place in an electronic forum certainly contributed to this interplay, which is more difficult to grasp in other contexts of logical production. The increase in use of electronic fora in the 1990's does not only constitute a tool for the social scientist to objectify certain scientific practices: it also changes the ways arguments are framed and scientific debates are conducted (Lewenstein 1995; Rosental 2000).

Finally, consider the representation of the process of certification of a theorem that defies various forms of reductionism. In particular, note that, even if a detailed analysis of the authors' texts would have been useful to enhance observation, the formation of points of view about Elkan's theorem was not simply determined by stable institutional configurations (e. g. the fractures in the field of AI), but also individually by the details of the proofs themselves. To this extent, I escaped from a form of social reductionism whereby debates would have consisted in a series of arguments of authority and institutional considerations would have entirely determined the reception of statements. On the contrary, the production of new statements was able to somewhat transform coalitions. This result will not surprise readers of Jack Goody, who clearly showed how the setting up of scriptural devices could have consequences for the transformation of the social relations of those who write or are put into writing (Goody 1977).

We have not, for all that, been forced to adopt an idealistic position consisting in highlighting the ideal truth (or falseness) of "the" theorem of Elkan, and to posit that it is enough in practice to explain "its recognition" (or "its rejection"). In fact, not only was it possible not to use such an approach, but it was the approach I used that enabled me to reveal, in this case, the erroneous nature of this hypothesis and the complex reality to which the above terms ("the" theorem, "its recognition", "its rejection") could correspond.

A vast field of empirical investigation into the concrete modalities of the collective accreditation of logical statements is open. Although the circles in which the workers of logic move are soundly entrenched behind their abstruseness, complex probative mechanisms, and other such tricks of the trade, it is nevertheless possible to provide elements of an answer to questions about the practices of the certification of logical theorems, and to construct a number of tools (especially cognitive) to further the inquiry. Since the page of excessively long discussions on principles has been turned, one can only hope that the social sciences will be quick to further the exploration and analysis such that we may come to have a better grasp, if not radically change, our representations of models for rationality and intellectual work in general.

ENDNOTES

1 If "sociology" should not be equated with "philosophy," neither should "logic" be equated with "mathematics." The overlap between these sciences is limited: they have in particular different research objects, histories and institutional settings. See Durkheim [1912] 1990: 616-17, 625-26. See also Mackenzie 1993: 49-50.

2 Even though the notion of certified knowledge has been long employed by sociologists of science (e. g. Merton 1942), the various and complex realities to which this term might refer deserve more study. Shapin and Schaffer (1985) have shown for example how certification had a priviledged link with witnessing in a specific socio-historical context. One of the purposes of this article is to contribute to the investigation of the possible meanings of this expression rather than starting from a closed definition.

3 Latour and Woolgar (1986: 208)'s notion of accreditation, which originally applied to persons and offered an account of how some individuals are accepted in the economic cycle of fact production, extends here to statements: an accredited statement is a statement that may be accepted in this economic cycle.

4 The term "recognition" is taken here as a black box (Latour 1987:131). Its meaning has to be provided a posteriori, instead of limiting it a priori to issues of rewards for example (e. g. Hagstrom 1965).

5 As Latour and Woolgar (1986: 194-198) have pointed out, some sociologists of science have used the term "credit" to denote reward, instead of linking it to an economy of fact production that implies various forms of investment. In order to insist on the fact that the receipt of personal rewards is not the ultimate objective of scientific activity, Latour and Woolgar have introduced the term "credibility" as a substitute to "credit". In this paper, I will adhere to this distinction.

6 Such is also the case of Bruno Latour (1987)'s program of anthropology of "formalism." "Formalism" does not simply refer to logic for the author but to material inscriptions in general, including logical inscriptions. Hence, empirical studies based on observations have been conducted by the author on "formalism," if not on logic itself.

7 From a cognitive point of view, the evolution of the material conditions of (a limited part of) mathematical production linked to the development of computers and related techniques of theorem-proving has indeed introduced considerable opportunities for social scientists in the past years to conceive inquiries which would not be exclusively based on text analysis. See in particular Mackenzie (1993, 1995, 1996, 1999), Rosental (1993, 2002b).

8 According to Gingras (1999), Pickering's posture resembles a Piagetian approach. See also Pickering 1995, 1999.

9 See also Livingston 1995.

10 For another semiotic approach of mathematics, see also Coleman 1988.

11 Some writings of sociological inspiration by mathematicians might be considered as already offering promising results regarding this issue, even if they addressed mathematical (and not logical) production. Indeed, these texts offered sources of non-stylized descriptive elements on concrete modalities of mathematical activity, based on personal experience or systematic observations of insiders (Fisher 1966, 1973; Demilo, Lipton and Perlis 1979; Davis and Hersh 1987).

12 I take the term "mediation" from the sociology and social history of art and, more particularly, from the sociology of music confronted with medias (Hennion 1993). The history of music seems to encounter 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 give an account of all those resources which might be considered as in-betweens (texts/instruments for example) in the production of knowledge/art, and to show them as proper beings.

13 The expression "classical binary logic" refers to the fact that all propositions are considered whether true or false. In other words, the propositions may be assigned one out of only two truth values (true or false). The four axioms mobilized in Elkan's proof are the following (here, "^" represents the logical connector "and," "v" the connector "or," "~" the connector "no," and "t( )" the truth value of the assertion in brackets): 1) t(A^B)=min {t(A),t(B)}, 2) t(AvB)= max {t(A),t(B)}, 3) t(~A) = 1-t(A), 4) t(A) = t(B) if A and B are logically equivalent. The theorem states that within the formal system defined by these four axioms: "For any two assertions A and B, either t(B)=t(A) or t(B)=1-t(A)." The author immediately inferred that fuzzy logic, characterized by these four axioms, is "in fact" a logic with two truth values (true and false). For an introduction to the "technical" notions introduced here, see Rosental (2003: 86-105).

14 For ethnomethodological arguments converging with this view, see Livingston 1985.

15 The documentation of this process relies hence on relatively large-scale investigations. Although not all collected data were relevant to grasp this specific process, many of them were directly or indirectly useful, in particular in order to know the "cultures" and fractures of the research communities involved. The investigations included ethnographic observations and interviews, especially within the communities of fuzzy logic researchers, probabilists, logic and artificial intelligence researchers at large (on the west coast and in Cambridge (MA), as previously mentioned). Around 200 interviews were conducted (face to face or by email); seminars, meetings, and some interviews were recorded, totaling 150 hours of audio and video tapes; around 1000 pages of fieldnotes have been taken; I also relied on the analysis of numerous books, articles, reports, drafts, brochures and (private and public) electronic archives of correspondences, whose volume is hard to evaluate all together.

16 The analysis could be conducted thanks to Nec Research Institute's Citeseer Search Engine (http://citeseer.nj.nec.com), and the Science Citation Index.

17 The concrete modalities of emergence of logical theorems have not been documented enough to allow to say more about the issue of the "representativeness" of the case. Although debates around a few major mathematical theorems have been studied (for instance Lakatos 1976; Goldstein 1995; Mackenzie 1999), it appears that comparing their similarities and differences would not be much more useful for that matter, especially if one relies on a recent estimate that evaluates the world production of mathematical theorems at around one million units every five years (Ulam 1976).

18 For a comparison with the field of biology, see for example Myers (1985).

19 Though the term " validity " has several meanings in logic, as well as in its history and philosophy, depending on the authors, it commonly refers to the issue of knowing whether a result " follows " by "correct rules of inference from the premises."

20 On this point, but also for further reading on the social uses of the Internet in research, see Rosental (1998 a).

21 On the sometimes major role of "those who have not seen" in science, see Ashmore 1993, Schaffer 1992. On the fecundity of a sociological approach of apparitions for insight into other human practices, see Claverie 1991.

22 "Magma" would be in fact more appropriate than "set", taken into account its importance in Greek rhetoric, but developing this notion here would go beyond the scope of this article. On this subject, see Cassin (1995).

23 The fact that the notion of proof itself might be debated here is certainly not a singular fact: (Mackenzie 1995, 1999) offer studies that document the evolution, variations and debates around the conceptions of mathematical proofs. For a contribution to the sociology of proof in logic and in mathematics, see also Rosental 1993, 2003.

24 These debates are evidently not unrelated to the radical dissention that has marked the history of logic up to the present (e. g. Largeault 1993: 21-23, 110-119; Carnap 1937: 51-52).

25 For an illustration over a longer period of the fully historical nature of readings in the field of mathematics, around the case of Fermat's theorem, see Goldstein (1995).

26 A boundary object is defined in Star and Griesemer (1989, 387) as an object that is "both adaptable to different viewpoints and robust enough to maintain identity across them." The authors have developed this notion to give an account of how the work of amateurs, professionals, administrators and others were connected to the Museum of Vertebrate Zoology at the University of California, Berkeley, during its early years.

27 The term "trial" refers here to the various types of tests which lead to perceive a statement as "objective" and which constitute the ontology of the latter (Latour, 1987, 74-79).

28 This situation is different from another interesting case where views perceived by the actors as opposite coexist (Bloor 1982).

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