5 Mathematics as a Science

This chapter aims to consider mathematical knowledge not as the expression of some superior reality but as a huge collection of scientific facts whose shaping necessitated a fair amount of practical work. As we will see, by considering mathematical knowledge to be one specific product (among many others) of scientific activity, we may provide a reasonable explanation of its capacity to make important differences in other scientific domains (neurology, geography, gambling, computer science, etc.). Once this operationalization exercise is over, I will come back to the main goal of this part III: understanding when, how, and why mathematical knowledge takes active part in the constitution of algorithms (chapter 6).

Where Is the Math?

If we want to better understand how mathematical entities (formulas, theorems, conjectures, equations) are manipulated and related to ground truths and programming languages, we first need to better understand where they come from. Such entities surely do not exist by themselves; they need to be assembled by people in specific designated places. Where are these places? Who are these people, and what do they do?

Such trivial questions lead to many, many heterogeneous answers. This is one reason why dealing with mathematics can be dangerous: Where shall we start? From the mathematics of ancient Greece (Heath 1981a, 1981b; Netz 2003)? From mathematics of medieval Islam (Berggren 1986; Netz 2004)? From baroque mathematics of continuous change (Bardi 2007; Boyer 1959)? But if we use the adjective "baroque," we already define the seventeenth century in quite an orientated way (Deleuze 1992). Shall we then focus on more contemporary mathematics such as set theory (Ferreirós 2007; Tiles 2004), Weierstrass functions (Bottazzini 1986), and the subsequent "crisis of foundations" that shook up mathematics at the beginning of the twentieth century (Ewald 2007; Ferreirós 2008; Hesseling 2004; Mancosu 1997)? But what do we mean by "mathematics" anyway? Do we mean mathematical *texts* (Rotman 1995, 2006; Sha 2005)? Do we mean *famous mathematicians* such as Leibniz (Antognazza 2011), Gauss (Tent 2006), or Cantor (Dauben 1990)? Do we mean *philosophies of mathematics* that try to define what mathematics *is* (Aspray and Kitcher 1988; Corfield 2006; Hacking 2014)? Our head is spinning and we start to feel dizzy. But it is not over yet! Indeed, are we talking about *arithmetic* (Husserl 2012), *algebra* (Everest 2007), *geometry* (Netz 2003; Serres 1995, 2002), or *logic* (Fisher 2007; Rosental 2003)? Maybe are we talking about the *evolution* from numbers to logic (Kline 1990a), from logic to geometry (Kline 1990b; Netz 2003), from geometry to algebra (Kline 1990c; Netz 2004)? And even within arithmetic, geometry, algebra, or logic, are we talking about *theorems* (Villani 2016), *proofs* (Lakatos 1976; MacKenzie 1999, 2004, 2006) or *conjectures* (O'Shea 2008)? We do not know. We are lost in questions whose only enunciation makes us want to do something else. But we cannot; we must find a way to address mathematics as it seems important for the constitution of algorithms. How can we do so?

One way to avoid this spiral of confusion could be to start from some very basic hypotheses. We would, of course, have to develop these hypotheses and justify them by using concrete examples. To do this, we may need to mobilize a tiny part of the gigantic mathematics literature that scares us. One step after the other, one hypothesis after the other—coupled with some STS assumptions—we may end up with an operative definition of mathematical knowledge that could suffice to achieve our specific task: accounting for the way that computer scientists, when they try to assemble new algorithms, are sometimes able to mobilize certified propositions previously shaped by their mathematician colleagues. We surely do not need to revolutionize our understanding of these powerful statements we sometimes call "theorems," "conjectures," or "formulas." If we just manage to shape one simple version of what mathematicians *do* (instead of what mathematics *is*), our last duty—accounting for formulating practices—will be greatly facilitated.

Written Claims of Relative Conviction Strengths

To initiate our operationalization exercise and shape our first hypotheses, let us start with three scenes that all gravitate around mathematical notions:[1]

Scene 1

January 1994. Charles Elkan is in turmoil: his theorem demonstrating that only two truth values can be expressed by a system of fuzzy logic is highly contested.[2] What went wrong? The initial presentation of his theorem at the Eleventh National Conference on Artificial Intelligence went very well. The paper that further appeared in the conference proceedings was even selected for the "Best Written Paper Award" (Elkan 1993). The program committee saluted the elegance of the proof as well as its significance for further developments in expert systems. Everything was in place for his theorem to be accepted. But many logician colleagues—who did not attend the conference but did read some of its proceedings published by MIT Press—are quite upset. Elkan can even follow their dissatisfaction on the newly established internet forum "comp.ai.fuzzy" that is dedicated to advanced discussions in fuzzy logic theories and systems. The critiques are harsh. Some say—and try to demonstrate—that Elkan's basic hypotheses are flawed. Others accuse him of deliberately weakening fuzzy logic as it is a threat to old, "dusty" classical logic. Some colleagues even suspect him to be a thick-headed Aristotelian! As one of his friends advises him, Elkan should now "cool things down" and publish a "smoother" version of his theorem that could include some of its soundest critiques.

Scene 2

Summer of 1890. Alfred Kempe is puzzled;[3] although not really because Percy Heawood recently managed to find a flaw in the proof of the four colors conjecture Kempe previously published in the *American Journal of Mathematics* (Heawood 1890; Kempe 1879). Heawood did a great job, and being refuted is part of the game anyway. No, it is more that even though his proof was shown to be erroneous, Kempe does not think that Francis Guthrie's 1852 candid proposition—that says that four colors suffice to color any map drawn on a plane in such a way that no neighboring countries have the same color—is wrong. But how could such a basic intuition lead to such great difficulties? Do mathematicians not have the tools to prove this conjecture and make it a theorem once and for all? "Poor Heawood," thinks Kempe. "He is now hooked on it, as I was fifteen years ago. He'd better drop it; this four colors thing is old hat."

Scene 3

November 8, 2013, 3 p.m. I sit at the back of the lecture hall.[4] Around three hundred undergraduate students are also attending this Friday afternoon "Information, Computing and Communication" class that aims to inculcate (communicate?) the foundational concepts of computer science to future civil and mechanical engineers. I see my younger brother and his friends—good students—in the second row. They've just started their academic curriculum; I've almost finished mine. But here we are in the same classroom, waiting for the same information (orders?). The professor adjusts his microphone: "All right. Hi, everyone. So, last week we talked about the Nyquist-Shannon sampling theorem. Today, we'll start with another contribution of Claude Shannon to the mathematical understanding of digital signals, which is the Shannon-Hartley theorem. It is quite a powerful theorem that can be summarized with this formula here:

$$C = B \log_2(1 + \frac{S}{N}).$$

Of course, we'll go through it together."

At this point, we do not need to make any a priori distinction between "theorems" (scenes 1 and 3), "conjectures" (scene 2), "proofs" (scene 1 and 2), and "formulas" (scene 3). We just need to notice that all three scenes, while presumably concerning mathematics, deal with *claims that attract more or less adherence*. In scene 1, Elkan's claim about fuzzy logic first attracts the adherence of the Eleventh National Conference on Artificial Intelligence's program committee. But then, in January 1994, his claim repulses many logician colleagues who do not hesitate to publish "counterclaims" on the web forum "comp.ai.fuzzy." In scene 2, Kempe's claim about the veracity of Francis Guthrie's claim (the "four colors conjecture") also first attracts the adherence of the editorial board of the *American Journal of Mathematics*. But then, in the summer of 1890, Kempe dissociates himself from his own claim and adheres to that of Heawood. However, Guthrie's 1852 "candid" claim has not lost all of its conviction strength yet, which makes Kempe puzzled about the fate of Heawood. Scene 3 is quite straightforward: Shannon and Hartley's claim—and its correlated formula projected on the lecture hall's whiteboard—is about to be taught to a crowd of undergraduate students in engineering. There is little room for doubt here: in November 2013, Shannon and Hartley's claim attracts the adherence of quite a lot of people. In fact, their claim is so strong that a well-known pedagogical device—the exam—will soon verify that all students properly adhere to it.

These basic but fair observations are all we need to start our operationalization exercise. Mathematicians certainly do a lot of things, but among these things, they make claims that attract the adherence of more or fewer individuals. Let us assume then that the grand notions of "theorems," "conjectures," "formulas," or "proofs" can all be grasped in a down-to-earth manner; let us assume that, to a certain extent, they are claims that convince more or fewer individuals.

This way to consider mathematical knowledge—theorems, conjectures, proofs, formulas—as the product of some rhetoric may sound odd at first. Many grand narratives have indeed chanted the abstract power of mathematical truths that, by themselves, supposedly describe some superior reality.[5] But this is precisely the road we do not want to take, at least not yet. If we do not want to crash on the sharp rocks of epistemological accounts of mathematics, we need to plug our ears and, for the moment, ignore the sirens of necessity. Fortunately for us, our first operational hypothesis—mathematicians make claims that convince more or fewer individuals—echoes well the central thesis of Lakatos's (1976) important book on mathematics. As he showed, instead of an accumulation of self-evident discoveries, mathematics should be considered a creative process during which concurrent claims are subjected to criticism and improvement. But how are such claims criticized or improved? How do they gain or lose their relative conviction strength? Shannon and Hartley's claim in scene 3 seems much stronger than Elkan's claim in scene 1. Similarly, in 1890, the claim Kempe made in 1879 is now powerless in front of Heawood's claim (scene 2). How do such differences come about?

To better understand how (mathematical) claims gain or lose conviction strength, we need to make another basic observation about scenes 1, 2, and 3. If more or fewer individuals could adhere to the scenes' claims, it means that they could *access* these claims. What medium allowed such access? Some claims are oral, but we are obviously not dealing with them here. The claims in scenes 1, 2, and 3 are all *written*. This important characteristic allows individuals to read them and eventually—very rarely—adhere to them. In scene 1, it is Elkan's written claim as it appears in the conference's proceedings that makes the program committee adhere to it. But in January 1994, it is the multiplication of written counterclaims on the web forum "comp.ai.fuzzy" that begins tormenting Elkan. In scene 2, both Kempe and Heawood access their respective claims by reading mathematical journals. Finally, the engineering students in scene 3 are asked to adhere to Shannon and Hartley's claim projected on the classroom's whiteboard. Of course, Shannon and Hartley did not write their claim on the projected document; many individuals intervened to carry their claim further through time and space until reaching this specific lecture hall. But this translation process does not change the overall shape of the claim; it is still something that is written down on a flat surface. At this point, we can therefore slightly refresh our first hypothesis: mathematicians surely do a lot of things, but among these things, they *write* claims that attract the adherence of more or fewer individuals.

It is also fair to assume that the written claims in the above scenes did not appear ex nihilo. In order to be published in proceedings, specialized web forums, mathematical journals, or the slides of a computer science professor, they all had to overcome a series of tests, *trials* upon which their existence as written claims depended. I agree that this hypothesis flirts with the metaphysics of subsistence—close to "process thought" (cf. introduction)—as proposed by influential, yet contested, thinkers. Let us then consider it an assumption we need for our operationalization exercise. "Whatever resists trials is real" (Latour 1993a). The above (mathematical) written claims are real; they thus resisted trials. But what trials?

Resisting Trials, Becoming Facts

The first kind of trial we can consider regarding the conviction strengths of (mathematical) written claims such as those in scenes 1, 2, and 3 are the trials they must endure *before* their actual publication. Examining what we often call the "sources" of claims is indeed a common way to evaluate their seriousness.

For example, we can make the fair assumption that, all things being equal, a claim published in the journal *Nature* will generally have more conviction strength than a claim posted on some social media platform with very little monitoring. Without even considering their respective content, both claims will have different capabilities. Why is that? We must immediately put aside the question of prestige or symbolic power; these are shortcuts our sociological method of inquiry forbids us to manipulate. A more empirical grip on this topic would quickly point to the number of individuals who could prevent the publication of a claim. Very few people—or bots—can prevent me from publishing a claim on, say, Facebook. Conversely, many individuals can prevent me from publishing a claim in the journal *Nature*. Taking into account those who have to be convinced by claims in order for them to circulate and reach a broader audience is crucial as it somewhat calibrates the cost of disagreement. If someone disagrees with a claim I publish on Facebook, they can just shrug their shoulders and move on to something else.[6] But if the same person disagrees with a claim I publish in *Nature*, they will have to disagree with me, my institution, the funding agencies that supported my research, *Nature*'s editorial board, those responsible for the nomination of this board, and so on. Compared with a claim I publish on Facebook, a claim I publish in *Nature* is initially supported by a far bigger team of *external allies* (Latour 1987, 31–33).

But if we consider our three scenes, we quickly realize that surviving *publication trials*—and thus enrolling external allies—is not enough to assure any durable conviction strength of (mathematical) claims. Although this lecture, in terms of convinced *gatekeepers,* may be enough to quickly account for the conviction strength of Shannon and Hartley's claim within the lecture hall—the students being literally crushed by all its external allies (their professor, their manuals, all those responsible for the engineering curriculum of their university, the exam they will soon have to pass)—it does not help us understand the relative strengths of Kempe's, Heawood's, and Elkan's claims (scene 1 and scene 2). In scene 2, both Kempe's and Heawood's claims survived similar publication trials; both propositions were initially supported by roughly the same number of individuals.[7] Yet Kempe's claim became distrusted as Heawood's appeared certified. The situation is even more confusing in scene 1: even though Elkan's claim successively resisted the scrutiny of the sixty-eight individuals responsible for the publication of the proceedings and the selection of the "Best Written

Paper,"[8] his claim is seriously shaken up by posts on a web forum with almost no monitoring (Rosental 2003, 81–86). Again, these counterclaims must have survived other kinds of trials in order to gain such strength.

Another kind of trial that may provide strength to written claims is one that consists in successively enrolling *internal allies* by means of citations and references (Latour 1987, 33–45). Equipping one's claim with previously published claims is indeed an important conviction strategy that has even become a whole field of study.[9] In addition to allies outside of the written document, a claim with references and citations is now supported by allies inside of it. Or is it? While often necessary, augmenting the conviction strength of a claim by means of references and citations can be a risky endeavor. What if the references do not match the claim, or worse, what if some unmentioned references contradict the presented claim? In some cases, this *citation trial* is overcome. One example is Shannon's initial paper that presented the basic elements of what would later be called the "Shannon-Hartley theorem" (Shannon 1948). In this paper, Shannon enrolls previously "solidified" claims made by Ralph Hartley (hence his later inclusion in the theorem's name) and thirteen other important mathematicians. As far as I know, no serious disagreements about the use of these references emerged after Shannon's initial publication. But the same was not true of Elkan's publication. Although he mobilized thirty-nine internal allies to strengthen his claim about the limitations of fuzzy logic, his contradictors managed to find and publish many strong "counter references" on the specialized web forum. Elkan soon appeared as someone unaware of many recent uses of fuzzy logic in advanced expert systems (Rosental 2003, 157–168). Although they were at first certainly useful to convince the program committee of the Eleventh National Conference on Artificial Intelligence, the internal allies of Elkan's paper ended up working as stepping stones for his contradictors.

However, surviving or not surviving citation trials is, again, not enough to account for the relative conviction strengths of the claims in all of our scenes. Indeed, in scene 2, Kempe's 1879 paper makes only three references to former mathematical propositions, the first two being loose statements made by Augustus De Morgan and Arthur Cayley to the London Mathematical Society (Kempe 1879, 193–194) and the third one being a more important claim made by Augustin-Louis Cauchy about polyhedrons (Kempe 1879, 198). Yet this scarcity of references did not prevent his claim—the proof that Guthrie's 1852 proposition was correct—from convincing his mathematician colleagues for eleven years. The same is even truer of Heawood's claim, for his 1890 paper includes no references other than Kempe's 1879 paper. Again, this scarcity did not prevent his claim from attracting the adherence of the chief person concerned: Kempe himself (MacKenzie 1999, 22). There must be something else in published (mathematical) claims that makes them gain, sometimes, in persuasion strength.

Some potential objectors of published (mathematical) claims will not be impressed by lists of convinced gatekeepers nor by the references invoked by the author. To be convinced by a claim, these skeptical readers want to *see* the thing the author asks them to believe in. This strategy that consists of presenting the thing in question to the reader was precisely the one used by Heawood in his paper against Kempe. He did not only rely on external allies; he also showed a *figure* (see figure 5.1) that, according to Kempe's 1871 claim, was impossible to draw:

> Mr. Kempe says—the transmission of colours throughout E's red-green and B's red-yellow regions will each remove a red, and what is required is done. If this were so, it would at once lead to a proof of the proposition in question [the four-colours conjecture]. … But, unfortunately, it is conceivable that though either transposition would remove a red, both may not remove both reds. Fig [below] is an actual exemplification of this possibility. (Heawood 1890, 337–338)

We do not need to spend too much time on the specificities of Heawood's figure[10] nor on the role of drawings in published mathematical claims.[11] Here, the important thing to notice is the conviction strategy; just as scientists engaged in many other fields—biology (Rheinberger 1997), chemistry (Bensaude-Vincent 1995), climatology (Edwards 2013)—mathematicians try to gain in persuasion strength by adding the referent of what they write about. At this point, then, "this is not a question any more of belief: this is *seeing*" (Latour 1987, 48). If, until now, I put the adjective "mathematical" in parenthesis, it was not to grant too much specificity to mathematical claims; they too are part of the scientific genre that tries to silence potential objectors by gathering more and more supporters. Scientific as well as mathematical texts can indeed be compared with bobsled tracks allowing very little room for maneuver while implying high level of skills. In both cases, readers must start at point A, pass through checkpoints $B_{1,2,\ldots,n}$, and finally finish at point C, the claim that tries to be established as a *fact*.

If scientific literature can be described as texts gathering many external and internal allies in order to isolate their readers and force them to take

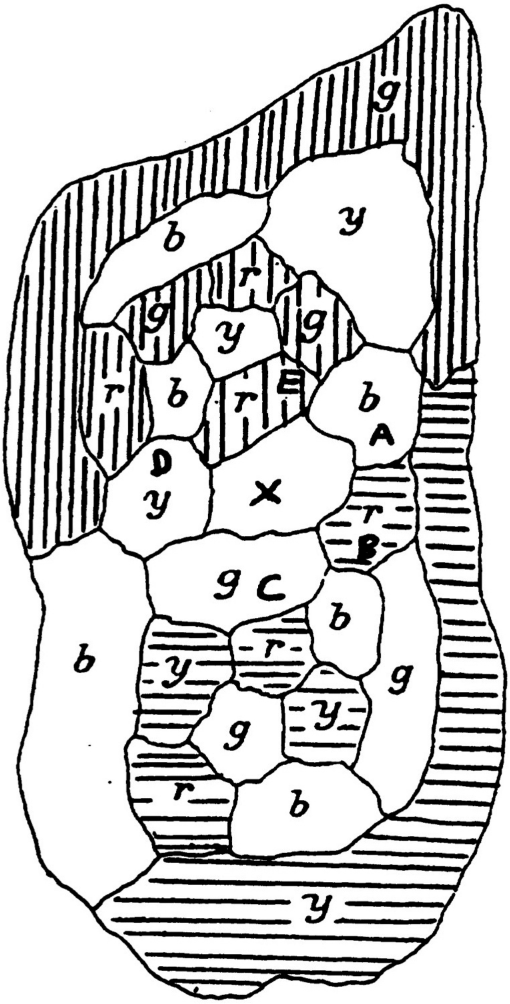

Figure 5.1

Reproduction of Heawood's figure showing that Kempe's proof does not hold. *Source:* MacKenzie (1999). Reproduced with permission from Sage Publications.

only one path, different scientific domains progressively shaped their own specific rhetorical habits.[12] In the case of mathematics, this whole *captation trial* (Latour 1987, 56–61) that consists in subtly controlling the movements of potential objectors has been finely analyzed by Rotman (1995, 2006). As he showed, mathematical publications are full of verbs in the imperative form, such as "construct," "define," "connect," or "compute." But a close analysis of these imperative forms reveals that they are in fact split into two distinctive types: *inclusive imperative* to establish premises—often equipped with references—and *exclusive imperative* to present lists of actions an imaginary reader should perform to reach the claimed result:

> Inclusive command marked by the verbs "consider," "define," "prove" and their synonyms—demand that speaker and hearer institute and inhabit a common world or that they share some specific argued conviction about an item in such a world; and exclusive commands—essentially the mathematical actions denoted by all other verbs—dictate that certain operations meaningful in an already shared world be executed. (Rotman 2006, 104)

These elements are crucial for our operationalization exercise as they indicate the felicity conditions of captation trials within mathematical texts. If skeptical readers, thanks to all the allies mobilized by the writer, have no other choice than to accept the premises and follow *one* specific path in order to reach one necessary conclusion, a mathematical text and its concomitant claim have, at least temporally, overcome their captation trial. In this respect, Kempe's 1879 paper on the four colors conjecture is quite illustrative. Remember that Kempe wanted to prove that four colors suffice to color any map drawn on a plane in such a way that no neighboring countries have the same color. How did he enjoin his readers to reach this conclusion? With a succession of inclusive commands, both Kempe and his imaginary skeptical reader start by defining a perfectly four-colored "singly connected surface" divided into many "districts" (Kempe 1879, 193). Once this basic common world has been instituted, they then consider two sets of "detached regions" either colored in red and green or in yellow and blue (Kempe 1879, 194). These premises allow Kempe and his reader to further define the properties of "points of concourse" (points where boundaries and districts meet) that themselves permit the definition of six classes of districts with different characteristics: "island districts," "island regions," "peninsula districts," "peninsula regions," "complex districts," and "simple districts" (Kempe 1879, 195–196). Once this quite complex common world has been instituted, Kempe then switches to exclusive commands and asks his reader to execute a series of operations:

> Now, *take* a piece of paper and *cut it out* to the same shape as any simple-island or peninsula-district, but larger, so as just to overlap the boundaries when laid on the district. *Fasten this patch* (as I shall term it) to the surface and *produce* all the boundaries which meet the patch … to meet at a point, (a point of concourse) within the patch. If only two boundaries meet the patch, which will happen if the district be a peninsula, *join them* across the patch, no point of concourse being necessary. The map will then have one district less, and the number of boundaries will also be reduced. (Kempe 1879, 196–197; italics added)

By asking the reader to reiterate this patching process, the whole imagined map is progressively reduced to one single district with no boundaries or points of concourse. Kempe then asks the reader to reverse the process; that is, to "strip off the patches in reverse order, taking off first that which was put on last. As each patch is stripped off it discloses a new district and the map is developed by degrees" (Kempe 1879, 197). At this precise point, Kempe switches to inclusive command again, thus instituting a *second common world* based on the first one that has just been modified. The author and the reader, together again, define the progressive reconstitution of all districts, boundaries, and points of concourse. Little by little, they soon realize that their recombination of districts, boundaries, and points of concourse is equivalent to, respectively, faces, edges, and points of polyhedrons as already defined by Augustin-Louis Cauchy in 1813 (Kempe 1879, 198). Once this polyhedron world has been instituted, Kempe switches one last time to exclusive command and makes the reader reach the claimed result: obviously—look, we have just done it together!—four colors suffice to color any map drawn on a plane in such a way that no neighboring countries have the same color.

We do not need to understand every little step of Kempe's paper. We just need to appreciate how Kempe manages to control the movements of his reader; from the initial premises to the conclusion, the reader is literally carried through Kempe's line of argument. His allies are quite numerous—"single connected surface," "districts," "detached regions," Cauchy's "polyhedrons"—and his transitions are smooth enough to transport the reader through the flow of necessity. But as we saw, Kempe's *captatio* was only temporary, for as eleven years later, Heawood managed to escape from Kempe's line of argument and propose a figure that dismantled the whole rhetorical edifice (see figure 5.1).

Publication, citation, and captation trials—just as any other claim trying to gain conviction strength and become a fact, mathematical claims must survive many jeopardies. Yet this is still not enough. A claim published in an important journal, with well-arrayed references and a smooth line of argument, may still vanish if it is not carried further by later claims. This is a sine qua non condition as there is no such thing as a solitary scientific fact: "Fact construction is so much a collective process that an isolated person builds only dreams, claims and feelings, not facts" (Latour 1987, 41). The fate of a claim, its progressive transformation into a solidified fact, depends ultimately on how it is used by later claims. We saw that Kempe's claim, despite its captation strength, ended up being refuted by Heawood. From the status of mathematical fact, it turned into mere fiction. What about Heawood's claim? It is difficult to call it a fact as it only concerned Kempe's fiction; it successively refuted Kempe's claim but did not provide any confirmable, or refutable, proposition. What about Elkan's claim, then? Despite Elkan's efforts to make it stronger—especially via the inclusion of many coauthors, better arrayed references, and smoother transitions (Elkan et al. 1994; Rosental 2003, 282–331)—it ended up being known for the doubtful reactions it gave rise to; that is, precisely, *for not being a fact*. Among our arbitrary mathematical examples, only Shannon's claim survived this important *posterity trial*, as scene 3 already suggested it. In fact, Shannon's claim survived the posterity trial so well that it progressively became part of a very small number of facts that are constantly used as resources in later claims. As it became more and more enrolled without any skeptical modalities, it became a black box with certified content presented in a clear-cut form. This *stylization process* (Latour 1987, 42) is typical of scientific facts that are much enrolled in later claims. Although Shannon went through several demonstrations in his initial paper, only the results of these demonstrations were progressively retained. These results were later concatenated, polished, and linked with former results established by Hartley until reaching a stylized form expressed by the formula presented in scene 3. Soon, perhaps, this strong mathematical fact may even become a "single sentence statement" (Latour 1987, 43): a scientific fact that is so accepted that it no longer needs any reference. If this happens, Shannon and Hartley's theorem will be part of tacit, undisputable, and necessary knowledge.

These last elements about blackboxed polished facts that may become part of tacit knowledge allow us to respond to an important objection:

Objection of a skeptical reader

> But is not mathematics different from all the other scientific disciplines in that it deals with fundamental truths? We could feel it when you presented Kempe's paper: in order to overcome the captation trial, he followed the timeless laws of deduction, did he not?

Not so long ago, it would have been very difficult to respond to this classical objection.[13] But thanks to the philological efforts made by Reviel Netz (2003, 2004), we now know that what we call "deduction" and "logical relations" are themselves blackboxed polished facts that were initially published around the middle of the fifth century BCE in Greece and southern Italy.[14] At that time, several self-educated amateurs who, presumably, tried to distance themselves from ancient Greece's highly polemical culture,[15] were surprised to discover that when they wrote *only* about the properties of lettered diagrams drawn on wax tablets, they could, step by step, express indisputable propositions. More precisely, by starting with some lettered parts of a diagram—say, two segments—they could, in turn, compare them with another lettered part of the same diagram. This very basic operation, made possible by the combination of drawings and letters on a flat surface, can be reconstituted as such: "This segment A here is equal to that segment B there. *And* that segment B there is equal to that segment C over there." In turn, thanks to the lettered diagram, Greek geometers could surreptitiously use conjunctive adverbs in a necessary way: "*Therefore* this segment A here is equal to that segment C over there." The shift seems trivial but is in fact crucial. Indeed, this first necessary result could be used to compare other parts of the diagram: "And that segment D over there is two times segment C. *Therefore*, segment A is half segment D." Progressively, by comparing more and more parts of the diagram, using more and more conjunctive adverbs and cumulating more and more intermediary results such as "A is half segment D," the Greek geometer could end up with a complicated yet necessary true proposition—the written list of indexical steps going from his first basic assertion to his last complicated one being the *proof* of the veracity of his claim.

For the sake of this section that only tries to present mathematical claims as part of the broader family of scientific claims, we do not need to dig further into the fascinating work done by Netz. Suffice it here to say that thanks to his efforts, we can now assert with some confidence that even deduction is the solidified product of past accepted claims. These constructed-yet-fully-logical laws of necessity must certainly have been surprising in ancient Greece.[16] But after centuries of enrollments in further claims, this style of reasoning—that obviously overcame its posterity trial—was progressively blackboxed, polished, and stylized until acquiring the status of indisputable knowledge.[17] Who would now quote Aristotle when using the inference rule of *modus ponens*? Yet even these principles of logic—dear to the formalist school of mathematics[18]—went through a process similar to that of Shannon and Hartley's theorem that very few mathematicians in signal processing would now try to contest. Just as the theorem they helped to shape, deductive laws were themselves shaped a long time ago by people equipped with specific instruments (in this case, lettered diagrams drawn on wax tablets and indexed to small Greek sentences).

Flat Laboratories

In the previous sections, we spent some time trying to stress the similarities between mathematical and scientific claims. It appeared that both need to survive similar trials to become, eventually, indisputable facts. No superior necessity helps mathematical claims to become certified facts; they too need to convince their readers in order to be enrolled in later claims and become, very rarely, polished black boxes.

However, so far, we have only considered one side of the coin. Although looking at mathematical published claims helps us realize that successful mathematical propositions could be considered genuine certified knowledge, we can legitimately assume that mathematicians do not prepare, write, and read papers all their working time. They must also spend time and energy on the *things* they write about. All the claims we considered in the last sections were indeed about things: limitations of fuzzy logic systems for Elkan, the four colors conjecture for Kempe, Kempe's claim about the four colors conjecture for Heawood, and maximum rate of information transmission over noisy channels for Shannon (and later, Hartley). But how are these things assembled? What practices lead to the presentation of these mathematical things—or objects—in published materials? Are these practices different from laboratory practices in other scientific communities?

As we prepare to look inside the locations in which mathematical objects are shaped, we immediately face a difficulty: there are very few empirical studies of such locations. Although there are robust studies about controversies within mathematical domains (Warwick 1992, 1993; MacKenzie 1999, 2000, 2004, 2006; Rosental 2003, 2004) and historical reconstructions of the shaping of mathematical objects from famous mathematicians' logbooks (Lakatos 1976; Pickering and Stephanides 1992), there are very few laboratory studies of mathematics.[19] It is thus with limited means that I will now try to stress the scientific aspect of mathematics a little bit more:

Scene 4

Salk Institute for Biological Studies at La Jolla (California), winter of 1972.[20] Paul Brazeau is on edge. His boss, Professor Roger Guillemin, is after him, casting doubts on his ability to handle the lab's brand new—and very expansive—radioimmunoassay. It is true that the graphs recently printed by the massive bioelectronic instrument are surprising; instead of showing that Guillemin's newly purified peptide triggers the growth hormone, it shows that it decreases it. This drives Guillemin crazy. But Brazeau and his technicians retro-inspected the whole experimental procedure a dozen times: there were no mistakes. The right amount of purified peptide was injected in the carefully assembled rat pituitary cell culture, and no mishandling occurred during the operationalization of the radioimmunoassay. "It's terribly simple," thinks Brazeau. "Either I am no conscientious professional or, for the last three years, we were all wrong about this peptide."

Scene 5

Dublin, fall of 1843. William Rowan Hamilton is in a challenging mood: even though he bumps into another impasse in his attempt to extend complex number theory to a three-dimensional space, he is obviously making important progress.[21] He is particularly proud of his new starting point; what a mistake it was to start his previous experiments from tiring algebraic models! As he now starts geometrically by moving from $x+iy$ to $x+iy+jz$, he possesses a three-dimensional line segment that is far easier to test (even though it adds a second imaginary number j right from the start). His first experiment was, in that sense, very conclusive. Thanks to the advice of his German colleague Gotthold Eisenstein, he could reach an equivalence between algebraic and geometrical definitions of the square of his three-dimensional segment by abandoning the assumption of commutation between *i* and *j*. He could then further test his model by multiplying two arbitrary coplanar triplets according to his new noncommutative rule for *ij*. Although he struggled at first to define the orientation of his new product, he realized—after several attempts—that Pythagoras's theorem could nicely do the trick. Here again, an encouraging achievement. Yet this last move led him to another problem: the algebraic and geometrical representations of this coplanar multiplication differ by a factor of $(bz—cy)^2$. "I must find a way to remove this superfluous term," he thinks. "I don't want to start the whole thing over again!"

Despite their cryptic aspects, what do these two scenes tell us about laboratory practices? Can we draw similarities between what takes place within Guillemin's laboratory of endocrinology (scene 4) and what takes place within Hamilton's laboratory of mathematics (scene 5)?

We can first notice that both scenes deal with *experiments*; they both put something to the test in order to evaluate its reactions. The peptide in scene 4 is, in 1973, still undefined. Guillemin—in line with recent claims about this class of amino acid polymer—is convinced that it should trigger the rat's growth hormone.[22] But how much is such growth hormone triggered? And under what circumstances? To have a clearer view on the capacities of this peptide, he puts Brazeau in charge of implementing an experiment he recently designed. In scene 5, a complex three-dimensional line segment $x+iy+jz$ is, in 1843, still undefined.[23] Hamilton hopes that this "triplet"—as he calls it—will allow him to extend the geometrical representation of complex number theory.[24] But at this point, nothing is certain. To better understand the capacities of his complex three-dimensional line segment, he puts it through two successive experiments: he first *squares* it and then *multiplies* it with another arbitrary coplanar triplet.

In both scenes then, experiments are run to test undefined entities. Yet experiments do not happen by themselves; in both scenes, *instruments* are used by scientists in order to help them probe their undefined entities. In scene 4, the delicately assembled rat pituitary cell culture and the very expansive radioimmunoassay are the two principal tools used to test the peptide. It is worth noting that both instruments are highly visible and take up a lot of space. The instruments in scene 5 are a priori less impressive but equally important. The first instrument is, obviously, the algebraic apparatus as progressively defined by medieval Islamic mathematicians; without any means to express relationships among variables in a condensed and succinct manner, Hamilton could not juggle his triplet.[25] But he also needs a coordinate space to express his triplet geometrically. In that sense, without the efforts of seventeenth-century mathematicians such as Descartes, de Fermat, Newton, and Leibniz, Hamilton would have no means to consider the transformations of his triplet. He further requires some insight from noncommutative algebra, as then recently proposed by Gotthold Eisenstein, to handle the complex product *ij* (Hankins 1980). Finally, he needs good old Pythagoras's theorem to multiply his initial triplet with another arbitrary coplanar triplet.[26]

At this point, we need to make another down-to-earth observation: although both laboratories have instruments to conduct experiments on undefined entities, the shapes of these instruments differ from each other. On the one hand, there is a bioelectronic assemblage that gathers peptides, Brazeau, rat cells, laboratory technicians, and an imposing metal box full of electronic parts; on the other hand, there are books, paper, Hamilton, and a pencil. There is little room for doubt here: the instruments do not take up the same amount of space. Hamilton's instruments appear *dryer* and *thinner* whereas Guillemin's instruments appear *wetter* and *thicker*. One could say—and that is the terminology I will use for the remainder of this section—that Hamilton's laboratory is *flat* whereas Guillemin's laboratory is *bulky*. Both laboratories are engaged in the same process—testing the reactions of an undefined entity—but they use instruments that are different in terms of occupied space.[27]

Can we in turn say that Guillemin's laboratory is more *expansive* than Hamilton's laboratory? If we only consider the relative price of their instruments, it seems indeed to be the case: paper is cheaper than laboratory technicians, most books (even in nineteenth-century Ireland) are cheaper than a radioimmunoassay from the 1970s, and pencils are cheaper than a rat pituitary cell culture. Yet if one considers the relative networks of both laboratory apparatuses, the question appears trickier. Indeed, how many efforts were needed to cultivate and sell standardized rat cells? Many, indubitably. But how many efforts were required to establish coordinate spaces? Many, indubitably. And what about algebra? As Netz (1998, 2004) showed, without centuries of commentaries on Greek geometrical writings, without

Byzantine libraries, and without the classification efforts of Bagdad mathematicians, no algebraic system of notation could have come into existence. The same is true of Pythagoras's theorem; many long-standing efforts were required to gather, compile, and preserve Pythagorean propositions from early antiquity to nineteenth-century Ireland. Let us then stick to the topological difference between our two laboratories: Hamilton's laboratory is *flatter* than Guillemin's.

If we continue to analyze both scenes, we can see that despite their topological differences, both bulky and flat instruments end up producing comparable *inscriptions*; that is, readable traces on documents. Indeed, the bulky bioelectronic experimental assemblage of scene 4 ends up producing graphs whose curves indicate that the rat's hormone decreases. The results of the experiment on the undefined peptide conducted by Brazeau are pieces of paper anxiously examined by Guillemin.[28] Similarly, the flat experimental assemblage of scene 5 ends up producing a series of coupled algebraic and geometrical equations; at first, both equations appeared equivalent (which was good news for Hamilton), but in the second step of the experiment, both appeared dissimilar (which was bad news for Hamilton). Yet, just as for Brazeau and Guillemin, the results of Hamilton's flat experiments are readable traces on documents he examines with his eyes.[29]

At this point then, we can tentatively say that both scenes deal with experiments, instruments (of different topologies), and series of inscriptions. But where does all this work lead to? At this stage, it certainly cannot lead to any published claim that may later become a scientific fact. Within these two laboratories, scientists impose tests on undefined entities, but how can these practices lead to the formation of *objects* capable of being described in academic papers?

Scene 6

Salk Institute for Biological Studies at La Jolla (California), January 1973.[30] There is nothing to do about it; even after two other meticulous experiments, the graphs printed by the radioimmunoassay still show that the rat's hormone decreases when put in contact with Guillemin's peptide. The rat pituitary cell culture is indisputable as are the composition of Guillemin's peptide, the radioimmunoassay, and Brazeau's professionalism

(Guillemin quickly admits it). The only way to escape from this impasse is to cast doubt on what the peptide does. Leading figures in endocrinology—including Guillemin—thought that this class of peptide triggered the growth hormone; obviously, it does the opposite. After being in contact with rat pituitary cell culture for a certain amount of time and after having gone through the radioimmunoassay with some consistent parameters, this *new thing* significantly decreases the rat's growth hormone. As it is certain that there have been no mistakes during the experimental procedures, a paper is now being prepared to convince skeptical readers about the existence of this new scientific object Guillemin starts to call *somatostatin* (literally, "that which blocks the body").

Scene 7

Dublin, fall of 1843.[31] There is nothing to do about it: the superfluous term $(bz—cy)^2$ within the geometrical expression of the length of a complex line segment cannot be removed without adding a new imaginary quantity. The rules of algebra—including noncommutativity—are indisputable, as are Pythagoras's theorem and Hamilton's scriptural operations (he ran the whole experiment several times). The only way to escape from this impasse is to cast doubt on the premises of the experiment: What if the extension of the geometrical representation of complex number theory required not three but four dimensions? Indeed, only the inclusion of a third imaginary quantity k as the product of i and j can make the superfluous term $(bz—cy)^2$ disappear. It is true that this new imaginary quantity needs in turn a fourth axis in order to be geometrically represented, but who cares? After the introduction of k as either an imaginary quantity (in the algebraic representation) or a fourth dimensional axis (in the geometrical representation), this *new thing* can be squared and multiplied while producing equivalent equations, hence effectively extending the geometrical representation of complex number theory. If Hamilton now manages to define the quantities k^2, ik, kj, and i^2—almost a formality at this stage—he will be able to completely define the behavior of this new mathematical object he starts to call *quaternion* (literally, "that which is made of four").

Again, beyond their cryptic aspects, what do these two scenes tell us about the formation of new objects within scientific laboratories? Can we draw some similarities between the progressive shaping of *somatostatin* (scene 6) and *quaternions* (scene 7)?

We can first see that in both scenes, inscriptions printed out by instruments begin by expressing singular phenomena. In scene 6, the graphs printed by the radioimmunoassay indicate confidently that after the peptide is injected in the rat pituitary cell culture over a specific period of time and after it goes through the radioimmunoassay with specific parameters, the growth hormone decreases significantly. This is what is inscribed within the graphs Guillemin can read; the whole experimental process ends up decreasing the rat's growth hormone. Trustful graphs become flatter; *therefore* the growth hormone decreases.

Similarly, in scene 7, the inscriptions produced by the hands of Hamilton indicate that after a fourth dimension is added to the triplet in order to geometrically express the new imaginary quantity *k*—itself required to make the superfluous term $(bz—cy)^2$ disappear—both algebraic and geometrical representations of complex number theory become equivalent. Again, this is the phenomenon described by the inscriptions Hamilton can read on a sheet of paper; the whole experimental process ends up expressing an extension of the equivalence between geometrical and algebraic representation of complex number theory. A trustful geometrical equation becomes equivalent to another algebraic equation; *therefore*, the geometrical representation of complex number theory is extended.

However, and this is the crucial point, by virtue of the experimental setting, the origins of these two phenomena—"quantifiable inhibition of the growth hormone" and "extension of the equivalence between geometry and complex number theory"—can be attributed to specific *things*. In scene 6, the only element whose actions were undefined at the beginning of the experimental process was the peptide. The actions of rat pituitary cell cultures, radioimmunoassay, Brazeau, and the technicians were all predictable; the unpredictable phenomenon—the graphs becoming flatter—must thus result from the action of this peptide-thing that "blocks the body." Similarly, in scene 7, the only element whose actions were undefined at this stage of the experimental setting was the third imaginary quantity *k* geometrically expressed by a fourth dimensional axis. The actions of noncommutative algebra, Pythagoras's theorem, and Hamilton's pencil and paper operations were all predictable; the unpredictable, yet anticipated, phenomenon—geometrical and algebraic equations becoming equivalent—can only be attributed to this four-dimensional thing that "groups together four numbers." In both scenes, new things emerge from the same attribution process; scriptural traces of a new phenomenon are imputed to the behavior of a previously undefined entity.

At the end of both scenes, this attribution process that imputes a behavior to a previously undefined entity by virtue of an experimental setting ends up being summarized by a term that encapsulates what the now defined thing does: "that which blocks the body" becomes *somatostatin* and "that which groups four numbers" becomes *quaternion*. New objects come into existence, but there has been no miracle; in both cases, the shape of the new object was progressively defined as scientists made it "grow" from a list of actions to the name of a thing. In scene 6, *somatostatin* was first "the graphs become flatter," *then* "under these experimental conditions, there is a diminution of the growth hormone," *then* "our new peptide decreases rat's growth hormone," and *finally* "somatostatin decreases rat's growth hormone." The same *reification process* (Latour 1987, 86–100) happened in scene 7: *quaternion* was first "two equations become equivalent," *then* "there is an extension of geometrical representation of complex number theory," *then* "four-dimensional representation allows the extension of geometrical representation of complex number theory," and *finally* "quaternions express geometrically complex number theory in a four-dimensional space." In both cases, experiments, instruments, and alignments of inscriptions—in short, *laboratory practices* (Latour and Woolgar 1986)—progressively led to the shaping of scientific objects whose properties and contours could, in turn, become the topics of papers claiming their existence.[32]

However, as we saw in the previous section, both somatostatin and quaternions as presented in papers that can be read by skeptical colleagues still need to overcome many trials to become certified scientific facts capable of being blackboxed, stylized, polished, and enrolled in further claims and experimental settings. Although both objects came into existence within their respective bulky and flat laboratories, they still need to attract the adherence of a wider community. But when the doubts of skeptical readers are removed, when the veracity of both claims are certified by the scientific institution, we can in turn confidently say that Guillemin *discovered* somatostatin and that Hamilton *discovered* quaternions. Or can we? We saw indeed that both objects were the results of laboratory practices that progressively shaped them. Can scientists discover objects they were previously constructing? Were somatostatin and quaternions already part of "nature" even though they had to be shaped in well-equipped (yet topologically different) laboratories? This is where the story starts to become tricky. If STS has long shown that scientific objects need to be manufactured in laboratories, the heavy apparatus of these locations as well as the practical work needed to make them operative tend to vanish as soon as written claims about scientific objects become certified facts. Once there are no more controversies or disagreements about a new scientific object, nature tends to be invoked as the realm that always already contained this constructed scientific object. Here, we encounter something we discussed in chapter 4 where we were dealing with computer programming practices: when facts are certified and enrolled in further studies, the experiments, instruments, communities, and practices that allowed their progressive formation are generally put aside (Latour and Woolgar 1986, 105–155). This is what makes the history and sociology of sciences (including mathematics) so difficult to conduct; as established facts are purified from the artificial setting that supported their formation, the temptation is great to start from these established facts and extrapolate backward (Collins 1975).[33]

However, if one is not interested in the history or sociology of sciences, if one "just" wants to speak about *objective* facts and eventually enroll them in further claims, the reference to nature appears completely justified. In that sense, one may of course say—as a kind of convenient shortcut—that Hamilton "discovered" quaternions or that Guillemin "discovered" somatostatin, but only because these objects ended up being accepted as certified facts, put in black boxes, translated, polished, and enrolled in later claims. As both initially manufactured objects presented in written claims successively resisted trials, the conditions of their production within dedicated laboratories can be, temporarily, neglected; nature can take over and support their raison d'être. In this respect, Latour's funny analogy is quite instructive:

> Nature, in scientists' hands, is a constitutional monarch, much like Queen Elizabeth the Second. From the throne she reads with the same tone, majesty and conviction, a speech written by Conservative or Labour prime ministers depending on the election outcome. Indeed she *adds* something to the dispute, but only after the dispute has ended; as long as the election is going on she does nothing but wait. (Latour 1987, 98)

The notion of "nature" is thus convenient to speak about noncontroversial scientific facts—why not?—but as soon as one speaks about scientific *controversies* or about scientific objects *in the making*, one needs to consider nature as the uncertain result of scientific practices.[34] This cautious position toward nature applies to “conventional” bulky scientific objects such as somatostatin as well as to “unconventional” flat scientific objects such as quaternions. Again, no superior reality makes mathematical objects appear to mathematicians. They too need to be shaped within (flat) laboratories equipped with instruments that print inscriptions.

Mathematic*able*

A good thing has been taken care of: it seems indeed that the construction process of scientific facts is quite similar to the construction process of mathematical facts. Theorems (cf. scenes 1 and 3), mathematical systems (cf. scenes 5 and 7), conjectures (cf. scene 2), and even formulas (cf. scene 3) may all be considered genuine scientific claims that try to convince colleagues of the existence of objects previously shaped within (flat) laboratories. If the vast majority of these claims do not overcome the trials that can make them become certified facts, some of them (e.g., Shannon-Hartley’s theorem, Hamilton’s theory of quaternions) may become stylized and polished black boxes that are used as instruments in further experimental settings. It is this huge—and changing—repository of certified mathematical facts that we may call “mathematical knowledge.” Moreover, several elements of this certified body of knowledge may, sometimes, become part of tacit, indisputable, and necessary knowledge (e.g., the logical laws of deduction).

However, despite the striking similarities between their respective construction processes, certified scientific and mathematical facts—and their correlated objects—still seem to differ significantly:

Objection of a skeptical reader

All right, let’s assume that both facts—and correlated objects—go through similar construction processes, as you obviously believe (while only relying on small, incomplete examples). An important difference subsists: mathematical objects never stop being used for the constitution of nonmathematical objects! We could even see it in the laboratory of endocrinology you used to illustrate your point. The graphs printed by the radioimmunoassay, which quantify how much the growth hormone is

> decreased by the peptide, are importations of solidified mathematical facts (in this case, basic analytical geometry). The same is certainly true of the inner mechanisms of the radioimmunoassay; complex mathematical theories must have been used to develop this costly instrument. Similar processes happen all the time in demography, climatology, political science, biology, and so on. Mathematical objects such as logarithms, Gaussian functions, or probabilities infiltrate all domains of "hard" science, helping scientists to shape new objects and facts. Yet the inverse is not true: how could peptides or radioimmunoassay help mathematicians shape new objects? Mathematicians have to do things by themselves, without the help of the other sciences. This is why mathematics is the queen of all sciences: without the work of mathematicians in their "flat laboratories"—we may keep that—there would simply be no exact sciences. Mathematical objects are so powerful; they must be of some superior nature. How could it be otherwise?

There are two glitches in this classical objection. First, it is not tenable to say that the practice of mathematics is self-sufficient, for many disciplines intervene in the construction process of mathematical objects and facts. Netz (1998, 2004) showed, for example, how *archiving* and *standardization* were central to overcome the stagnation of Greek geometry.[35] Thanks to the assembling of well-arrayed corpora of papyruses and parchments—especially in Byzantium—late antiquity commentators such as Eutocius became able to compare, annotate, and complete the entangled multiplicities of Greek geometrical writings. Progressively, these systematic standardization efforts made early antiquity's geometrical propositions commensurable; unlike Greek geometers,[36] medieval mathematicians—especially in Bagdad's *House of Wisdom* (Netz 2004, 131–186)—could *see* what Greek geometry was. Equipped with "intellectual technologies" (Goody 1977)—here, collections of standardized Greek geometrical treatises—mathematicians such as al-Khwarizmi and Khayyam could systematize and classify the geometrical problems solved by the Greeks. These systematic comparisons progressively led, according to Netz, to the formation of the algebraic language: "Al-Khwarizmi's algebra was, ultimately, a fairly unambitious ambition, translated into major transformations. Without himself doing anything beyond classifying the results of the past, Al-Khwarizmi, effectively, created the equation" (Netz 2004, 143).

Since archiving and standardization were, and are,[37] central to the formation of mathematical objects, do we have to say that these two respectable disciplines are the queens of the queen of all sciences? To me, a more reasonable position would be to accept that hierarchal classification of disciplines is misleading. When something allows something else to come into existence, it may not be a matter of vertical hierarchy but of *horizontal arrangement*.

This leads us to the second objection regarding the usability of mathematical objects for the assembling of nonmathematical objects. It is true that the combinational capabilities of mathematical facts are surprising. In every scientific discipline, recent or ancient mathematical discoveries are used to conduct experiments, organize inscriptions, express new phenomena, and eventually define new objects. I would go even further than our skeptical reader and expand this extreme combinability of mathematical objects to everyday life. For example, how many times a day do we use the basic precepts of arithmetic? Obviously, mathematics is everywhere, from laboratories of high energy physics to cashiers' desks. This capacity to infiltrate heterogeneous domains of activity is very impressive. But does it necessarily mean that mathematical objects come from a different *nature*? Does their plasticity necessarily manifest a supernatural *essence*?

Let us consider Guillemin's laboratory of endocrinology since it is the example used by our skeptical reader. It is true that the results printed by the computer of the radioimmunoassay required the application of elementary mathematical theories in order to indicate a diminution of the growth hormone. Was there some magic? Not if we consider more precisely the process by which the rat pituitary cell culture was "flattened" to become representable as a graph with numerical values varying through time. What happened indeed within the radioimmunoassay? Schematically, the very small radioactive waves emitted by the rat pituitary cell culture were captured and, after a series of translations, counted by the costly equipment. Radioactive waves became signals that, in turn, became discrete values varying through time. This transubstantiation process—or, more succinctly, *translation process*—that made a cell culture go from the state of complex liquid to the state of a writable list of (radioactive) values spread over time is precisely what allowed the enrollment of the elementary mathematical notion of "ratio" and the further calculation of the growth hormone's decreasing. How did the ancestral theory of ratios as developed by the Pythagoreans become applicable to the world of endocrinology? The concrete efforts to form differently (trans-form) the cell culture into quantifiable inscriptions, thus making it become a geometrical graph, allowed the *connection* between ratios and Guillemin's peptide. It was by flattening the cell culture and adapting it to the flat ecology of ratios that these mathematical objects became applicable to the cell culture. Nothing mysterious happened; by progressively translating a complex entity into a scriptural form, it became possible to link it with certified mathematical facts.

Another—better—example of such an empirical process that makes non-mathematical entities become mathematicable is provided by Michal Lynch (1985) in his book *Art and Artifact in Laboratory Science*. During the 1970s, an important topic in neurology was the plasticity of the brain; that is—briefly stated—its capacity to recover lost functions through the reorganization of some of its tissues. How this reorganization occurs was a controversial topic at the time of Lynch's laboratory study. Two major conjectures were in competition. The first one considered that the reorganization occurred through the densification of the synapses—the structures that allow interneuronal communication between axons and dendrites—*within* the damaged brain territory.[38] The second theory, labeled "axon sprouting," considered that the reorganization was due to the extension of axons *adjacent* to the damaged territory. For many reasons encompassing results of then recent laboratory experiments as well as promising industrial applications, the director of the laboratory studied by Lynch believed that axon sprouting was the main ingredient for the brain's reorganizational capacity (Lynch 1985, 32–33). But how could he demonstrate it? Many pitfalls got in his way. First, neurons are very small. Observing their (re)organization required powerful zooms. Fortunately, the advent of electron microscopy—a technology recently purchased by the laboratory—allowed him to make ultrastructural observations. But this led to another issue: at that time, these observations could only be made on tiny *slides* whose flat topology was different from the bulky topology of neurons. Fortunately, a "methodic series of renderings of laboratory rats" (Lynch 1985, 37) could be organized to properly slice brains and adapt them to ultrastructural visibility. But this extraction of brain slides led to another issue as a reorganizational brain process can only happen within a living brain. How could it then be possible to observe brain plasticity on dead sliced samples? Fortunately, the availability of *many* standardized laboratory rats with almost identical brains allowed the organization of a "chain of sacrifices" (Lynch 1985, 38). Although it was not possible to observe the reorganization of one living damaged brain, it progressively became possible to observe the reorganization of "same" damaged brains killed at different time intervals. A regular series of discrete—and meticulously referenced—dead slices permitted the reconstitution of the evolution of one living brain trying to palliate its damages. Yet the scientists followed by Lynch still needed to discern specific events within the mess of every single slide. They were indeed trying to account for axon fibers that were expanding their territories to damage zones. But how could they define territories of axons as well as their potential expansions? Fortunately—and this greatly contributed to designing the whole project—one interesting characteristic of the "dorsal hippocampus" helped them to establish points of reference common to all electron microscopic observable sections. It had indeed been demonstrated—and accepted—that the structure of the dorsal hippocampus looks like a grid, the dendrites of its cell bodies regularly intersecting axons indexed to different brain regions (Ramón y Cajal 1968). Therefore, if the brain researchers managed to produce electron microscopic observable slices of dorsal hippocampus extracted from similarly damaged rats' brains (killed at different time intervals), the "natural" grid structure produced by the intersections of the dendrites of dorsal hippocampus's body cells with axons indexed to different brain regions could constitute an initial empirical base for further measurements (Lynch 1985, 35–39). In other words, as it was certified that one specific part of the dorsal hippocampus contained cell bodies whose dendrites *always* intersected regularly with axons indexed to two different brain regions, which I call here α and β, it became possible to damage the β brain regions of all rats and then check if the axons indexed to α "sprouted" to infiltrate the territory of the axons previously indexed to β. But again, a new problem arose: how to go from specific electron microscopic views on slices to a *panorama* of many slices distributed over time? At the time of Lynch's study, the easiest way to operate this translation was first to take analogical photographs of electron microscopic dorsal hippocampus displays. Brain scientists then had to develop these photographs in high definition and equip them with a coordinate system scaled according to the ultrastructural levels of observation (between 2,160 and 24,000 times, depending on the photographs). How did Lynch's scientists concretely manage to equip these high-definition photographs? They pinned down the photographs on a cardboard sheet, hence creating a chronological *montage* of the microscopic displays. As Lynch put it, "these successions of photographs provided the visible configuration of brain ultrastructure that was addressed in the analytical phase of the study" (Lynch 1985, 38). But here again, it was not enough to measure an extension of axons indexed to α. Even though the dendrites of dorsal hippocampus's cell bodies regularly intersected axons indexed to α and β, it remained necessary to affix a *referential* common to all photographs. How did the brain scientists do this? It is difficult here not to quote Lynch's account:

> As each montage was constructed, it was analytically addressed in the following manner: a clear plastic sheet was laid over the surface of the photographs, and a linear scale was drawn over the surface of the sheet running in a vertical direction which paralleled the edge of the columnar montage of photographs. … A scale of "microns" (computed with reference to the magnificational power of the photographs) was plotted for the drawn-line, where the "zero" point was set at a horizontal line that approximated the alignment of the granule cell body layer. … *Measurement along this scale was used to estimate linear distance along the "vertical" alignment of granule cell dendrites as they arose from the cell bodies and coursed "upward."* (Lynch 1985, 38; italics added)

Flat linear distances are a priori far removed from neurons and the potential sprouting of their axons. Yet, once enlarged photographs of tiny little slices of standardized rats' dorsal hippocampus are mounted on cardboard and equipped with a linear scale drawn on clear plastic sheets whose "zero" point corresponds to the cell body of each slice, this venerable mathematical theory and its correlated objects become very, very close (Latour 1987, 244). The experimental setting of the laboratory and all of its instruments producing "alignable" inscriptions—standardized rats; tiny, carefully washed (and stained) slices of rats' dorsal hippocampus; montages of enlarged photographs; linear scales drawn on clear plastic sheets—end up conferring to rats' dorsal hippocampus the same *form* as graphs on which linear distances can be estimated. At the end of this measurement process, ratios of intact/dead terminals—junctions between axons and dendrites—plotted in terms of days *post* the lesion could even be computed by the scientists, thus demonstrating *statistically* the phenomenon of axon sprouting: "Measurement of this expansion showed a consistent reoccupancy of the lower 25 per cent of the region of the granule cell dendrites formerly occupied by the [damaged] layer of axons" (Lynch 1985, 35).

Again, as Lynch demonstrated, no magic intervened; laboratory practices made the relationships between axons and dendrites become *mathematicable*. Standardized rats became dorsal hippocampus, tiny slices became enlarged photographs, and a montage of cardboard became one regular geometrical space whose occupancy evolved through time. If some polished mathematical facts—computation of surfaces progressively occupied by intact terminals—did help demonstrate the existence of a nonmathematical phenomenon (axon sprouting), this *event* necessitated a succession of translations in order to connect the wet and bulky ecology of the brain with the dry and flat ecology of mathematics.

Formulating: A Definition

Mathematics does not apply to the world. A cascade of translations is required to connect nonmathematical entities with certified mathematical facts. But at this point of our operationalization exercise, one question remains: if the rats' dorsal hippocampus of the brain research laboratory we have just considered and the rat pituitary cell culture of Guillemin's laboratory both end up being trans-formed in order to fit with the networks sustaining solidified mathematical objects (themselves formerly described by claims that progressively became certified facts and even, sometimes, single sentence statements part of tacit undisputable knowledge), do they not *lose* many properties on the road? After all, from a rich and complex region of the brain, the dorsal hippocampus becomes a tinkered montage of gridded photographs; from a rich and complex soup of cells, the rat pituitary cell culture becomes a simple graph. To make both entities mathematicable, they must endure important *reductions*. But is it worth it? What justifies such flattening and drying?

In these specific situations, the gains of these reductions are important because the properties of the mathematical objects as formerly defined by mathematicians within their flat laboratories are progressively "lent" to the pituitary cell culture and the dorsal hippocampus. First, both entities become *easier to handle*. After the translation process from a cell soup to a graph, Guillemin does not need the cell soup anymore. He certainly conserves it for potential verifications, but whenever he needs to see or show the rat pituitary cell culture, he can now use the graph printed by the radioimmunoassay that expresses only the tiny important part of the soup's properties. The same is true of the brain research laboratory studied by Lynch: instead of handling tiny slices of hippocampus, brain scientists can now consider gridded photographs. One direct consequence of this ergonomic gain is that the reduced entities become also *more sharable*. Although it is impossible to e-mail—or, in these cases, fax—wet and bulky dorsal hippocampus, after their translation into a succession of photographs, trustful brain scientist colleagues based on the other side of the world are also able to scrutinize them. Transforming the hippocampus into gridded pieces of paper allows it to invest extended—yet expansive and fragile—communication networks. Such a reduced and flattened hippocampus therefore also becomes *more comparable*; if the brain scientists based on the other side of the world also manage to operate similar reductions on the dorsal hippocampus, they may be able to compare both successions of gridded photographs. The same is also true of Guillemin's graphs: instead of comparing cell soups, endocrinologists can compare graphs, a far easier endeavor.

Another gain of reducing entities and making them fit with the flat network of certified mathematical knowledge is that reduced entities become much more *malleable*; new takes appear that, in turn, suggest new instruments, tests, and inscriptions. For example, when active junctions between axons and dendrites become points within a uniform geometrical space, the instruments already defined by mathematicians for this geometrical space can be used to further probe the still undefined phenomenon of axon sprouting, thus producing new inscriptions that will precisely help to define it. Within this geometrical space, new tests can be made, such as *measuring* surfaces, *counting* terminals, and *calculating* ratios of occupancy. These tests and their correlated instruments will, in turn, produce readable inscriptions—here, lists of numbers—that will help further characterize the phenomenon under scrutiny. The same is true of Guillemin's rat pituitary cell culture: once complex biochemical reactions become discrete values varying through time, all the instruments that become available through this graphic form can be used to further probe the cell soup. What is the *slope* of the graph? What is the *speed* of the growth hormone's decreasing? Again, a flat reduced form enables the use of new instruments and the production of new readable inscriptions that help with the characterization of a new phenomenon.

This leads us to one last gain of these crucial reduction processes, perhaps the consequence of all the other gains:[39] when an entity is made compatible with mathematical facts, it also becomes *enrollable* within the written claim that will try to attest to its reified existence. This element is crucial if we want to understand the full additional strength these reduction processes may give to undefined entities. How indeed to include axons within a text claiming their ability to sprout? How to include Guillemin's new peptide within a paper attesting to its decreasing effect on the growth hormone? Reducing them until they reach the same *form* as certified "flat" mathematical facts allows them to become the *referents* of the prose that presents them to their respective scientific communities. In addition to making both axons and peptide easier to handle, more shareable, more comparable, and more malleable, reducing them to make them compatible with the flat ecology of mathematical facts allows them to be included *inside* the texts that talk about them. The reified object "axon sprouting," more than just being described in a paper, is also present within the paper in the flat and dry form that precisely allowed its mathematization (in this case, according to Lynch [1985, 40–49], as a succession of gridded photographs whose points move "upward"). Similarly, the reified object "somatostatin," more than just being described in a paper, is also within the paper in the form of a graph summarizing its behavior (Brazeau et al. 1973). The attentive reader may have noticed that we have now come full circle from the beginning of this operationalization exercise where we were talking about written claims of relative conviction strengths. The end results of laboratories, experiments, instruments, and inscriptions are indeed the formulation of claims that try to attract the adherence of individuals. In this respect, we should now be in a position to better understand the fascinating power of mathematical objects and facts; they may go through construction processes that are similar to other scientific facts, but their particular flat and dry ecology makes them relevant for the formation of nonmathematical objects and facts. They make undefined entities easier to handle, more shareable, more comparable, more malleable, and more enrollable within claims they precisely help to *formulate*.

It is not mathematical facts and their correlated objects that give, by themselves, some additional strength to the transformed entities they sometimes encounter. Rather, it is the flat ecology within which mathematical knowledge deploys itself that, sometimes, provides advantages to the entities that acquire the same *form*. This last element allows me to finally define the activity of *formulating* more technically; for the remainder of this part III, I shall call *formulating* the empirical process of translating an undefined entity until it acquires the same form as already defined mathematical object. The encounter between a "made-flat" entity and a mathematical object—that previously had to be constructed in a laboratory and presented in a claim whose conviction strength made it a polished fact—will, in turn, help scientists to further characterize the behavior of the entity and present its reified version in a written claim. Just as any scientific claim (including those formulated by mathematicians), this written claim will still have to overcome publication, citation, captation, and posterity trials to become, eventually, a certified fact. A circle has been drawn; we are now back to where we started. With all these elements in mind, it is high time to return to computer science in the making and engage with ethnographic materials.

This is a section of doi:10.7551/mitpress/12517.001.0001

The Constitution of Algorithms

Ground-Truthing, Programming, Formulating

By: Florian Jaton

Citation:

The Constitution of Algorithms: Ground-Truthing, Programming, Formulating
By: Florian Jaton
DOI: 10.7551/mitpress/12517.001.0001
ISBN (electronic): 9780262363235
Publisher: The MIT Press
Published: 2021

The open access edition of this book was made possible by generous funding and support from Arcadia – a charitable fund of Lisbet Rausing and Peter Baldwin

The MIT Press

The open access edition of this book was made possible by generous funding from Arcadia—a charitable fund of Lisbet Rausing and Peter Baldwin.

This book was set in Stone Serif and Stone Sans by Westchester Publishing Services.

Library of Congress Cataloging-in-Publication Data

Names: Jaton, Florian, author. | Bowker, Geoffrey C., writer of foreword.
Title: The constitution of algorithms : ground-truthing, programming, formulating / Florian Jaton ; foreword by Geoffrey C. Bowker.
Description: Cambridge, Massachusetts : The MIT Press, [2020] | Series: Inside technology | Includes bibliographical references and index.
Identifiers: LCCN 2020028166 | ISBN 9780262542142 (paperback)
Subjects: LCSH: Algorithms--Case studies. | Computer programming--Case studies. | Algorithms--Social aspects. | Mathematics--Philosophy.
Classification: LCC QA9.58 .J38 2020 | DDC 518/.1--dc23
LC record available at https://lccn.loc.gov/2020028166