INTUITIONS IN THE MATHEMATICAL PRACTICE OF CONJECTURING

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ABSTRACT

The topic of this paper is the role intuition can play in the mathematical practice of conjecturing. More precisely, it addresses the question of *how intuitions can serve to rationally form fruitful conjectures*. To succeed in making fruitful conjectures, various conditions must be fulfilled. Syntactic capacities paired with unwarranted truth claims are not sufficient. Conjecturing is not guesswork and not all intuitions are of the same relevance to it. The kind of intuition that can and often does play a leading role in the practice of conjecturing is typically invoked by experts. But even if a statement is suggested by expert intuition, the attempt to conjecture it may still fail due to insufficient justification. In such cases, one has to offer other reasons why the statement should be considered probably true and worth investigating. Although an established conjecture may of course still prove to be false, this need not undermine its fruitfulness. For in mathematics, the refutation of an interesting claim can be the proof of an equally intriguing statement. And when it comes to identifying what is potentially fruitful, intuition has again a crucial role to play. This paper aims to show that the unwritten rules, implicit criteria and intellectual powers at work in the art of conjecturing are indeed a valuable field of investigation for epistemology. It is also an attempt to contribute to the larger issue of how intuition relates to knowledge and truth in mathematics.
Keywords: conjecturing, mathematical intuition, philosophy of mathematical practice

1. Introduction

To make conjectures is an essential part of mathematical practice. A good conjecture can captivate generations of mathematicians and shape the development of entire fields. Just think of the Riemann hypothesis or the Langlands program. Despite its obvious importance, the practice of conjecturing has not always received the attention it deserves in the philosophical literature. Perhaps, this is because conjecturing is typically associated with what are called contexts of discovery. But the philosophy of mathematics, it has been maintained, can only be concerned with contexts of justification. Questions pertaining to discovery would have to be relegated to other disciplines such as psychology or sociology.[1]

This dismissive attitude may seem all the more plausible when one considers the importance that is often attached to intuition in forming conjectures. For philosophy has a long history of separating intuition from discursive rationality. Intuition has often been, and not seldom still is, understood as the faculty of the mind to *directly* apprehend truth without recourse to reasons. Intuitions, as products of this faculty, are thus considered self-sufficient and in no need of further justification, be it in the form of proof or less rigorous arguments: Either you see the truth, or you don't. But that seems to leave nothing for philosophy to investigate. Justifications, on the other hand, are supposed to rely as little as possible on intuitive insight if they are to bring the kind of certainty typical of modern mathematics.[2] Here, rigorous proof can be required even of the most trivial truth. It may seem compelling, therefore, to deny intuition any essential role within mathematical contexts of justification. One might concede that intuitions are involved in contexts of discovery: in developing new concepts, in forming hypotheses, even in exploring possibilities of proof. But in the finished proofs themselves as well as in any other justificational work, intuitions seem inessential and dispensable.

Against this I would like to maintain not only that conjecturing, as it occurs in mathematical research, is indeed a rational practice but also that one can grant intuition a crucial role in this practice without jeopardising its rationality.[3] The question I would like to address, therefore, is *how intuitions*

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[1] A clear commitment to this view can be found, e.g., in Dummett 1991, 305. For an early and resolute critique of this view, see Lakatos 1976. Influential papers discussing the context distinction in general include Blackwell 1980 and Hoyningen-Huene 1987. For more recent contributions, see Schickore and Steinle 2006.

[2] For a classical formulation of this view, see Hahn 1933, 93; for a critical reassessment, see Feferman 2000, 319-322.

[3] The fact that intuition plays an important role in conjecturing is widely accepted. Carlo Cellucci's refusal to assign intuition any significant role even in contexts of discovery is rather exceptional. His rejection is directed at what he calls "the dominant view" according to which "mathematical discovery is an irrational process based on intuition, not on logic" (Cellucci 2006, 28). Against this view, he rightly maintains that in mathematics even discovery is a "rational activity". Where he is mistaken is when he infers from this that intuition "does not provide an adequate explanation as to how we reach new hypotheses" (28), emphasising the separation of intuition from discursive rationality. However, as pointed out in Hersh 2011, 42, Cellucci's dismissive stance is due to his narrow use of the term 'intuition', taking it as synonymous with 'immediate knowledge', thus implying direct apprehension of truth without recourse to reasons (see also Cellucci 2017, 232). In keeping

can, and do, serve to rationally form fruitful conjectures eventually leading to truth.

Furthermore, I would like to emphasise that it is very much a job for philosophy to provide an epistemological account of conjecturing. This is not to deny that questions belonging to other disciplines may be touched upon. On the contrary, it is one of the main tasks of the philosophy of mathematical practice to prepare the ground for non-philosophical investigations by clarifying conceptual landscapes, dissolving possible misunderstandings, and setting out well-structured accounts of different sub-practices. To achieve this, however, the strict separation of mathematical activity into contexts of discovery and contexts of justification should be abandoned. The example of conjecturing makes this particularly clear.

As liberal as the practice of conjecturing may seem, anyone who wishes to succeed in making a conjecture must abide by certain rules. In particular, no statement known to be true or known to be false will be considered a serious candidate for conjecturing. Although those who advance a conjecture typically tend to believe that its statement is true, they do not know it. To gain certainty, the conjecture has either to be proved or disproved. This, however, can be extremely difficult and time consuming. If the statement appears to be of the more elusive kind, one might be compelled therefore to convince oneself or others that it is worth to investigate its truth nevertheless. And here, intuitions about the statement's being true and possibly fruitful may be offered as reasons for taking up an investigation. But this will not always suffice. For the conjecture to be accepted as worthwhile, it will sometimes be necessary to corroborate such intuitions with arguments, ideally of a mathematical nature. In cases where the conjectured statement goes against common intuition, it is even more likely that a justification for investigating its truth will be demanded. As a result, the initial intuition may not only be rejected, but also replaced by an improved one. It is precisely in such contexts of conjecture forming – which are clearly contexts of justification – that intuition and discursive rationality come together. That the philosophy of mathematics should refrain from investigating such processes seems to me rather misguided.

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with common usage, the term 'intuition' is taken here in a broader sense, as will become clear in the next section.

Before turning to the question that has just been raised, the next section will first introduce some basic distinctions to clarify different uses of the term 'intuition'. The aim here is to identify the notion of intuition most relevant to our investigation. The third and main section, then, offers elements of an epistemological account of conjecturing as practised in mathematics. This includes discussions of necessary conditions for successfully making conjectures and for their becoming fruitful. Along the way, it will be seen in which different ways intuition can contribute to the rational activity of forming conjectures. The results of this investigation are intended to provide a suitable starting point for shedding further light on the practice of conjecturing by means of in-depth analyses of historical examples. Such analyses cannot be carried out within the scope of this paper.

2. Mathematical intuition

The history of the term 'intuition' and the various meanings that have been attributed to it throughout the centuries is a vexed one. Surely, there is a colloquial use of the word with which the use in philosophy is connected. But, as Charles Parsons rightly remarks, neither the pre-philosophical nor the philosophical uses of the term 'intuition' seem to point to a fundamental notion on which there is sufficient agreement (Parsons 2008, 138-9). Different acceptations of the term stand side by side, and, unfortunately, they are thrown together or mixed up more often than not.[4]

For the philosophy of mathematics, this has not always been the case. Through Immanuel Kant's influence on philosophy in general and on the philosophy of science in particular, his technical use of the term 'Anschauung', translated into English as 'intuition', dominated philosophical discourse on mathematics for a long time. Having lost its dominant status in the meantime, aspects of the Kantian notion are still present, if often only implicitly and alongside other uses of the term. In order

[4] In Davis and Hersh 1981, 391-392, six different ways in which the term 'intuition' is used by mathematicians are listed. Yet the authors note that in "all these usages the notion of intuition remains rather vague" (392). See Burgess 2014, 31, for a more philosophically based taxonomy of intuition. Disagreement about how to understand the term 'intuition' when applied in mathematical contexts is not limited to philosophy. The term's ambiguity and the difficulty of grasping the phenomena it is supposed to denote cause problems also in the psychological literature. Jean Piaget once said that "[n]othing is harder for a psychologist to understand than what mathematicians mean by intuition" (Beth and Piaget 1966, 208). For a recent account of the situation in mathematics education research, largely confirming Piaget's *bon mot*, see Lajos 2023.

to delineate the notion relevant to our investigation, it is contrasted in what follows with a broadly Kantian notion.

2.1. *Objectual intuition in the Kantian tradition*

An intuition, in the Kantian sense, is the direct representation of a particular object. Kantian intuitions are always intuitions *of* an object, never *that* something is or is not the case. It makes no sense, therefore, to ask whether an intuition, taken in the Kantian sense, is true or not. Only judgments, or their statements, are capable of being true or false. When an assertive judgement contains an intuition, it says something true or false about the object directly given in the intuition. For example, I could judge that the thing in front of my eyes is a table. This would be a judgment joining an empirical intuition with the concept of a table which, as a concept, is a *general* representation. According to Kant, the judgements of pure mathematics are special in that they contain only *pure* intuitions, that is no intuitions of particular empirical objects. Contrary to empirical objects which are given through sensation, the objects of pure intuitions are constructed by the imagination alone. In constructing the objects of mathematics, the imagination conforms with the general forms of intuition, *i.e.* with space and time. This is why mathematical judgments typically contain an intuitive element and Kant classifies them as *synthetic* a priori. In this they contrast with philosophical judgments which are the result of discursive rationality unsupported by intuition (see the beginning of Kant's *Transcendental Doctrine of Method*, which forms the second part of his *Critique of Pure Reason*, A 712-738 / B 740-766).

From a Kantian perspective, our initial question – as to the possibility of rationally forming conjectures based on intuition – would seem obviously misguided. A Kantian would rather ask in return how intuition could *not* play a crucial role in conjecturing, given that the bulk of mathematical propositions are synthetic truths and thus by definition based on intuition. From these short considerations already, it is clear that the notion of intuition relevant to our undertaking must be at some distance from the Kantian tradition. What are the main differences?

2.2. *Propositional intuition in experts*

First, and foremost, intuitions in the sense intended here are not objectual, but directed at propositions (or statements). For mathematical propositions are, of course, what one is after in the practice of conjecturing. And if intuition is to be of any great value for this practice, then primarily (although not exclusively) as a source from which such propositions flow. An intuition, in the sense intended here, is therefore a propositional attitude that may be denoted by expressions like “I have the feeling that ...” or “It seems to me that …” or “I tend to think that ...” etc. Note that there is nothing artificial about this way of using the term ‘intuition’. In fact, it is closer to common usage than the objectual variant derived from Kant.

Another important difference is that intuitions in our sense are not bound to spatial or temporal imagination. They need not depend on images or diagrammatic representations at all. A group theorist, for example, could have a highly abstract intuition about the existence of certain structures without being able to visualise the states of affairs at hand. Mathematical intuitions of this sort seem to be more akin to linguistic intuitions. And in this sense, even set theorists or logicians can be said to have intuitions about their subject matter. Note that such a wide use of the term ‘intuition’ is not as unusual as some philosophers of mathematics might be inclined to think.[5]

This, of course, is not to deny the fact that spatial intuition is an important source for conjecturing in many areas of mathematics. However, its usefulness also has its limits. There are enough cases known from history in which mathematicians were misled by the inaccuracy of spatial intuition. (The Banach-Tarski paradox, which will be discussed in the next section, is arguably a case in point.) Conversely, if the intuition is so clear and distinct that the statement flowing from it appears evident enough to make a proof almost superfluous, it will hardly lead to a fruitful conjecture. Take for example the statement that every simple polygon divides the plane into an interior and an exterior region. More precisely:

> Let P be a simple polygon in the plane $\mathbb{R}^2$. Then its complement, $\mathbb{R}^2 \setminus P$, consists of exactly two connected components. One of these components is bounded (the

[5] See, for a *locus classicus*, Henri Poincaré’s *The Value of Science*, where a distinction is drawn between intuition based on concrete sensory experience and pure intuition (Poincaré 1958, 19-25). A more recent elaboration can be found in Chudnoff 2014, where the view of intuition “as concrete illustration” is distinguished from the “perceptualist view of intuition” (175-176), which in turn is divided into a Kantian and a non-Kantian branch. The latter view, which the author endorses, is described as non-Kantian, for it concedes that “at least some mathematical intuitions are cognitive and not limited by our capacity for sensory representation” (184). The Cantor set is offered as an example of a mathematical object that is intuitable, although it outstrips our sensory capacities. Feferman 2000 discusses in detail various intriguing cases where “common-sense geometrical intuition” and “set-theoretical intuitions” seem to clash (327).

interior) and the other is unbounded (the exterior), and the polygon P is the boundary of each component.

Our intuition of Euclidean planes and simple polygons leaves no doubt as to the truth of this statement. While it may have been a sensible conjecture to make at a certain point in the distant past, it surely did not have the potential to become a *fruitful* conjecture. It is not, in any way, a deep result and a proof, even a rigorous one, is too easily at hand.[6] If contemporary textbooks contain a proof at all, it is only for didactic reasons: as an occasion to practice the art of proving.

To be clear, the same does not hold true for the generalised version of this statement, which became Camille Jordan's famous curve theorem. The Jordan curve theorem states the same as the lemma above, but for Jordan curves in general. A Jordan curve is any image of an injective continuous map of a circle into the plane $\mathbb{R}^2$.[7] In its full generality, this theorem is surely not trivial as in the special case of polygons. This is because "a Jordan curve can be quite fantastical in the sense that there are some bizarre properties such a curve might have (jagged at every point, space filling, etc.) or that

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[6] See Hales 2007, 46. However, this is not to say that a statement which is intuitively very plausible or even evident could not be of major importance, *e.g.* in virtue of its important applications. A case at hand here is Desargues's theorem about which Gian-Carlo Rota writes the following: "The proof of Desargues's theorem of projective geometry comes as close as a proof can come to the Zen ideal. It can be summarized in two words: 'I see!' Nevertheless, Desargues's theorem is far from negligible, despite the simplicity of its proof. It has found far more applications, both in geometry and beyond, than any theorem of number theory, maybe more applications than all the theorems of analytic number theory put together" (Rota 1997, 189). The reason for this, Rota tells us, is the theorem's connection with a certain geometric structure that is now called the Desargues configuration: "The role of Desargues's theorem was not understood until the Desargues configuration was discovered. [...] The value of Desargues's theorem, and the reason why the statement of this theorem has survived through the centuries, while other equally striking geometrical theorems have been forgotten, lies in the realization that Desargues's theorem opened a horizon of possibilities that relate geometry and algebra in unexpected ways" (192).

[7] Bernard Bolzano seems to have been the first to judge the statement in its general form to be worthy of proof. This is why he went on to conjecture it. Jordan gave a proof that was criticised and replaced by Oswald Veblen. For a long time, the *opinio communis* relied on Veblen's claim that Jordan's original proof was substantially incomplete. Thomas Hales who gave the first formalised version of a proof, showed, however, that Jordan's original proof was not as problematic as supposed, but rather satisfactory and more elegant than Veblen's proof (Hales 2007). Either way, the Jordan curve theorem was, in turn, generalised by Arthur Schoenflies and later by Luitzen Brouwer to become the separation theorem. Because of this subsequent history alone, it would be wrong to deny fruitfulness to the conjecture first put forward by Bolzano and proven by Jordan.

such a curve can have a difficult to discover inside and outside" (Ross and Ross 2011, 213). Yet the Jordan curve theorem is often cited as an example of a statement whose truth is intuitively obvious but not easy to prove rigorously. I suspect that the intuitiveness of simple cases, such as polygons, is here transferred to the general case without much caution. If one considers the sheer variety of possible Jordan curves that the theorem deals with, its alleged obviousness fades.

For our purposes here, it is sufficient to note that a statement fails to exemplify the kind of intuition most relevant to the practice of conjecturing if its truth is as intuitively evident as in the case of the simple lemma above. To be established as a conjecture, a statement suggested by intuition must be worth proving or disproving – and not only for didactical reasons. That means that even its simplest proof or refutation should be "hard" enough as to have the potential to spark the interest of the relevant mathematical community. Trivialities do not meet this requirement and it is very unlikely that it could be met by the sort of mathematical intuition most humans share. What we are after here is therefore advanced intuition: the sort of intuition typically invoked by experts in any given field.

In order to elucidate how expert intuitions have proven themselves in the mathematical practice of conjecturing, it is not necessary to posit them as the product of some mysterious faculty of the mind. When using the term 'intuition' in the singular, one speaks sometimes as if there were a special faculty responsible for producing different intuitions. Although this way of speaking is convenient, it should not be taken to imply the assumption that there is in fact a single such faculty. It seems more likely that a variety of different faculties are involved. This is true even if one considers only those more abstract intuitions that do not flow from spatial or temporal imagination, as can be the case in set theory. The assumption of a single faculty at work here – of a purely intellectual intuition detached from the senses – does not appear to be sufficiently justified.[8] It would be all the more

[8] This alleged faculty of the intellect, which plays a prominent role in Kurt Gödel's philosophy of mathematics, but also has an ancient philosophical pedigree, is sometimes called 'rational intuition', see Parsons 2008, ch. 9. It corresponds to what in Chudnoff 2014 is described as the non-Kantian version of the perceptualist view of intuition (see note 5). John Burgess rightly emphasises the plausibility of alternative explanations: "The crucial philosophical question is simply whether there is any real need to posit a special intellectual faculty in order to account for the experiences of the kind Gödel describes […] or whether, on the contrary, such experiences can be explained in terms of faculties already familiar and less problematic. For there are other, more mundane, varieties of nonsensory intuition, and a

dubious to deduce from the occurrence of such non-sensory intuitions not only the existence of a special intellectual faculty, but also the existence of a Platonic realm of entities providing this faculty with its fitting objects. For the purposes of the present investigation, however, the ontological question can be set aside without prejudice to any particular position on the matter. As to the question of which cognitive faculties are involved in the production of mathematical intuitions, it is ultimately a psychological one.

While avoiding the dogmatic assumption of a purely intellectual faculty of intuition that would allow direct access to mathematical truth without recourse to discursive rationality, we may safely assume that expert intuitions are grounded in experience. The intuitive familiarity with mathematical concepts, and the deep understanding of their interconnections, that is required for good conjecturing usually implies years of dedicated work as a specialist in a chosen field (although in geniuses, acute expertise may manifest itself without the usual amount of practice). In this respect, such intuitions are the opposite of common intuitions, which are shared by most and border triviality. Expert intuitions will strike others, especially non-experts in the field, as too difficult to venture guessing their truth value or even their meaning. As one purpose of using them is to allow for mathematical research to advance into unknown territory, such intuitions may seem bold when they are first advanced. And, of course, they may deceive their holders and turn out to be false. Expert intuition is fallible, which is why the mathematical statements it suggests may require some justification in order to qualify as worth investigating. But typically, what emerges from expert intuition looks interesting enough to spark the community's interest. This makes it a favourite source for forming fruitful conjectures.[9]

Contrary to what one might think, the fact that expert intuitions are not readily shared by many does not preclude the possibility of transmission. As pointed out by Elijah Chudnoff, experts can make the content of (at least some of) their intuitions accessible to others, even to novices, by guiding

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skeptic might suspect that one or another of them is what is really behind Gödelian experiences" (Burgess 2014, 22-23). A further reason that speaks against the assumption of rational intuition is the lack of criteria to distinguish it either from linguistic or from heuristic intuition (26-30).

[9] The kind of intuition intended here is thus a species of what is called "heuristic intuition" in Burgess 2014, 27, and described as "plausible in the absence of proof" and "reasonable as a conjecture" in Davis and Hersh 1981, 391.

them to see for themselves what they, the experts, can see, *i.e.* by helping them to "represent superior problem spaces, recognize the kind of problem that they face as one with a proven strategy, or pursue superior strategies for finding new solutions on their own" (Chudnoff 2020, 476). This does not mean, of course, that expertly guided novices instantly acquire the expertise of their guides. But, as an example in the next section will show, such guidance can be an important step in improving one's own intuitions and developing the kind of advanced capacities needed to make meaningful contributions to the practice of conjecturing. Let us now turn to this practice and the role intuition may play in it.

3. The rational practice of conjecturing in mathematics

By describing the activity of conjecturing as a *rational practice*, I would like to emphasise two things. First, that there are certain rules by which anyone who wishes to succeed in making a conjecture has to abide. And secondly, when an attempt is made at establishing a particular statement as a conjecture, there is always the possibility of demanding reasons for investigating its truth. Moreover, it should also be kept in mind that not only one can succeed or fail in conjecturing, but one can do better or worse. For this reason, I sometimes refer to this practice as the *art* of conjecturing.

Being essentially a *social* practice, the art of forming mathematical conjectures emerges from the cooperative activity of groups of experts. As such, it is governed by various norms, the description of which would require an empirical approach, not a purely philosophical one. Particularly, whether a given conjecture will become fruitful or not is in part a matter of contingency. To become fruitful, the conjecture must not only be accepted by a group of sufficiently influential mathematicians (a group which, in borderline cases, may consist of only one member), it also needs to be acknowledged as worthwhile investigating and actually spark research. This, of course, does not imply that fruitfulness is *solely* a matter of contingency. Not every mathematical statement has the potential to spark the interest of the relevant community. Typically, the fruitfulness of a conjecture is associated with features such as generality or simplicity, depth or inferential power, beauty or boldness, and so forth.[10] But what is more

[10] Note that what counts as general enough, as deep, as elegant, etc., will vary over time. What captured the imagination of earlier generations might appear dull to later mathematicians. And what mathematicians find promising today could have been rejected as empty or unwarranted by earlier generations.

important for the present investigation, is that there seem to be *necessary conditions that any statement must fulfil in order to be successfully established as a potentially fruitful conjecture*. It is only these conditions of the possibility for success and fruitfulness that I will try to articulate. Therefore, the constraints that are of interest here concern mainly the *statements* themselves and their relations to present and future states of knowledge and belief in mathematics. Most other aspects of good conjecturing cannot be taken into consideration here or are merely touched upon.

3.1. *Conditions of successful conjecturing*

As already pointed out in the introduction, not every meaningful mathematical statement is suitable for conjecturing. A statement that is intuitively evident, requiring no proof to be known as true, cannot normally be made a conjecture. The same holds, of course, for any statement that is obviously false. If someone wanted to advance as conjecture that there is more than one even prime number, this would not count as a serious attempt. Furthermore, if someone wanted to turn a statement for which a proof is known into a conjecture, this would also not be considered an acceptable attempt. Theorems as well as statements, the negation of which have been knowingly proved, cannot be made conjectures. If someone tried to advance as conjecture either the statement that there are infinitely many prime numbers or the statement that the primes are only finitely many, neither attempt would be acceptable today.

From these simple considerations, one can already discern a first principle that underpins the practice of conjecturing: A mathematical statement can only become a conjecture if it is neither known to be true nor known to be false. Therefore, whether an attempt to establish a conjecture succeeds depends on what is known to be true and what is known to be false.

One might ask, now, whose knowledge is meant here. Is it the individual knowledge of the mathematician trying to make a conjecture or rather the collective knowledge of the relevant community? Usually, the decisive factor is the community's collective knowledge, or to be more precise, the collective knowledge of its competent part. If an individual tried, for example, to advance a well-known theorem as conjecture, that individual would simply be made aware of the fact that the corresponding statement has already been proven. The only interesting case would be if an individual had such superior intuition, or knowledge, that they could advance important statements yet incomprehensible to the members of the competent community or far too remote to arouse their interest. In such a case, one would probably be inclined to admit that the individual has succeeded in making a conjecture, although it would not be accepted as such by that individual's contemporaries under the given circumstances.

To some extent, this seems to have been the case with the early, and highly original, work of Srinivasa Ramanujan. While it was initially met with rejection, incomprehension, or indifference, by certain mathematicians, it was later widely recognised (see Berndt and Rankin 1995). But even Godfrey H. Hardy, who prided himself on having discovered Ramanujan's genius, was surprised by the depth of his results. He admitted to having been "defeated […] completely" by some of the equations Ramanujan had sent him, stating that he had "never seen anything in the least like them before" (Hardy 1940, 9). Unlike others, Hardy immediately realised the importance of these results. "A single look at them" was enough for him to know "that they could only be written down by a mathematician of the highest class" (9). Some equations seemed as though they had to be true, for "if they were not true, no one would have had the imagination to invent them" (9). However, Ramanujan was often unable to provide rigorous proof for the statements he reached, and some of them turned out to be false. In forming what were in fact conjectures, Ramanujan was guided, as Hardy, who worked with him for years, concluded, by "a mixture of intuition and computation" (229).

A somewhat different case was described by William P. Thurston who, like many mathematicians, attached great importance to trusting his own intuitions (see Thurston 1994, 165). After years of studying the relationship between three-dimensional manifolds and hyperbolic geometry, building up his "intuition for hyperbolic three-manifolds", Thurston conjectured that "all three-manifolds have a certain geometric structure", thus advancing what eventually became known as the geometrization conjecture (174). Having developed not only his intuition, but also "a repertoire of constructions, examples and proofs", he managed to prove the conjecture for Haken manifolds a few years later. This relentless work placed him in a position of such epistemic superiority over his peers that it took them "a while just to understand what the geometrization conjecture meant, what it was good for, and why it was relevant" (175). One difficulty Thurston had in passing on his results to others was that the mental models he used to think about the subject matter were rather idiosyncratic. At times, he explains, there was "a huge expansion factor in translating from the encoding in my own thinking to something that can be conveyed to someone else" (175). This experience led him to invest more time and effort in transmitting to larger parts of the community not only his results and their proofs, but also his intuitive familiarity with and deep understanding of the ideas involved and the advanced intuitions that flowed from them (168-172, 175-176). In other words, he used his expertise to guide others, including novices, towards these intuitions, helping them to develop and improve their own until they themselves were in a position to advance fruitful conjectures.

These examples already illustrate various ways in which intuition contributes to the practice of conjecturing. What exactly its different roles can be will become clearer in the next section, and then again in the conclusion, when it comes to answering our initial question. For now, let us turn to other, more philosophical questions that arise from the same considerations. In particular, one would like to know what kind of knowledge and understanding is required, how much of it is necessary, and what must be yet unknown for statements to be valuable candidates for conjecturing. It will not be possible here to answer these questions conclusively. But by considering a few examples, I will try to give first hints. The focus will be on cases where the attempt to turn a statement into a conjecture would fail or require atypical justification to be accepted as such.

Imagine a child who has just mastered the language of arithmetic. The child knows now how to assemble numbers, letters and other signs in order to form meaningful formulas, *e.g.* equations. Imagine further that this child jotted down the equation $a^n + b^n = c^n$ and was now claiming that no natural numbers a, b, and c satisfy the equation for any integer value of n greater than 2. Of course, we would not accept this as serious conjecturing.[11] Not because the statement, *i.e.* Fermat's so-called Last theorem, has been proven in the meantime. But because a superficial and merely syntactic understanding of statements paired with a truth claim is not sufficient to make conjectures. Serious conjecturing is not guesswork. It presupposes the

[11] By adding the attribute 'serious' to the term 'conjecture', I do not intend to indicate a further constraint on conjectures – as if in addition to normal, acceptable conjectures, there were also serious conjectures. Rather, the attribute 'serious' is intended to demarcate mathematical conjecturing in the more technical sense of the word from conjecturing as a freer form of guessing corresponding to the non-technical use of the word 'conjecture' outside scientific contexts. To avoid confusion, I use the adjective 'successful' and its adverbial variant exclusively as attributes to the *act* of conjecturing, and refrain from speaking of successful or unsuccessful conjectures. What may or may not be successful is an *attempt* at conjecture. If the attempt succeeds, a conjecture results. But if it fails, no conjecture is established at all.

establishment of a certain conceptual environment around the notions involved in the conjecture. This environment should be such that its mastery allows for a serious attempt at proof, *i.e.* an attempt that would bring some progress and not immediately end in perplexity. Making guesses out of the blue, just because one has the syntactical skills to do so, is not the kind of conjecturing that has proven itself in mathematics.

Admittedly, if the child in our example was called Carl Friedrich Gauß, we would perhaps take a second look. Gauß made his private conjecture about the prime number theorem when he was only 15 years old. But even a statement advanced by a genius or a recognized expert in the field may not be accepted immediately as a conjecture. This could be the case, for example, if the claim involved seemed overly speculative, such that no serious attempt at proof were foreseeable. In such cases, the mathematical community would be entitled to ask for reasons why this proposed claim should be believed to be true. If no reason can be given, the attempt at conjecture may be rejected as not sufficiently justified. It is unknown, possibly no longer knowable, exactly how Gauß arrived at his conjecture and whether intuition was involved.[12] What is certain, however, is that merely alluding to an unshareable intuition would not have been very helpful in convincing others. Perhaps, Fermat's conjecture of his so-called Last theorem was such an overly speculative move, considering that it took

[12] Gauß mentioned his juvenile conjecture only much later in a letter to Johann Franz Encke (for an English translation of the letter, see Goldstein 1973, 612-614). In this letter, he is eager to justify the plausibility of his conjecture by computations and other mathematical arguments. As to how he came to form this conjecture in the first place, he only says that when counting prime numbers on logarithm tables, he soon recognised that their "frequency is on the average inversely proportional to the logarithm" (612). How can we know from this, and more generally in studying the history of mathematics, whether or not intuition was at work in any particular instance of conjecture forming? The truth, of course, is that we may not know for sure. We must rely on the testimony of the mathematicians involved or of their contemporaries. And even if someone like Gauß assured us that intuition was at work when he came up with his conjecture, it is still not clear to what kind of cognitive process, if any, he was alluding to. Should we therefore refrain from using the term 'intuition' when studying the past? Is the only way to find out whether particular mathematicians had the relevant kind of intuition to examine them psychologically or, better still, to measure their neural activity at the precise moment when they come up with their fruitful ideas? This cannot be the case. From these brief considerations, I conclude rather that the term 'intuition' can have another function: a function linked to our attempts to explain the process of forming fruitful conjectures. It is used to fill the gap in our reconstructions of how rational enquiry ultimately leads to mathematical truth. In other words, the appeal to intuition is a valid step in a scientist's search for truth as well as in our reconstructions of it.

360 years of revolutionary developments in mathematics before a proof was achieved. Alternatively, one could say that Fermat's conjecture came too early to be fruitful from the outset – that the conjecture, like an exceptional wine, needed time to mature. In neither of these views, it is a paradigmatic example for the rationality at work in the practice of conjecturing.

Unsuccessful conjecturing is not always due to a lack of knowledge or to an insufficiently developed conceptual environment, as in the example of the child just discussed. On the contrary, if too much is known and understood about the concepts involved in a statement, this can also prevent it from being conjectured. This would be the case, for example, if someone wanted to conjecture a statement that borders on truism or for which a proof is too easy at hand. In either case, the mathematical community may refuse to accept the statement as a valuable candidate for conjecturing. (The same holds, *mutatis mutandis*, for obvious and easily disproved falsehoods, of course.) Alternatively, the individual who made the proposal could be asked to explain why the search for a proof is nevertheless worthwhile. And here, depending on the situation, various reasons could be given. The remainder of this section explores several such reasons as they may occur in strictly axiomatic settings.

One reason of this kind could be to know what more general truths a particular statement, however trivial, depends on. The question can either be whether a proposed set of axioms is *sufficient* to prove that statement, or whether a certain axiom from this set is *necessary* to do so. Neither of these questions involves conjecturing the particular statement whose dependency from the axioms is to be examined. In the first case, the aim is to find a very specific proof for this statement, namely one that, besides the admitted rules of inference, relies on nothing but the given axioms. Providing a proof that is valid but does not have the required connection to the axioms would not be accepted. But usually, when it comes to proving a conjecture, it is sufficient to show that it can be derived in any acceptable way, *i.e.* by using techniques and propositions that are well known in the field, the latter rarely being axioms alone.[13] In ordinary conjecturing, one is moreover less

[13] The kind of practical, non-axiomatic reality referred to here is described by Thurston in the following terms: "Within any field, there are certain theorems and certain techniques that are generally known and generally accepted. When you write a paper, you refer to these without proof. You look at other papers in the field, and you see what facts they quote without proof, and what they cite in their bibliography. You learn from other people some idea of the proofs. Then you're free to quote the same theorem and cite the same citations. You don't

convinced of the truth of the conjectured statement than of the truth of the premisses one would appeal to in an attempt at proof. In the axiomatic setting just considered, exactly the opposite can be the case, for confidence in the truth of the statement one wishes to deduce is sometimes greater than confidence in the truth of the proposed axioms.[14]

The second setting mentioned above, which is about showing the *necessity* of a certain axiom for the proof of a statement, is a case for reverse mathematics. The main method of reverse mathematics consists in trying to derive not the statement from the axiom, but conversely, the axiom from the statement by assuming not more than a specified base theory.[15] It is obvious, then, that the statement is not being conjectured but assumed. What instead is being conjectured, one could be tempted to say, is the axiom itself. But again, the analogy fails, and for the same reasons as before. What perhaps becomes even clearer than before, is how different the aim here is from that of ordinary conjecturing. It is not about establishing the truth of a particular statement, which can be done by producing some proof or other, but rather about determining an explicitly specified deductive dependency, which imposes stronger constraints on what kind of proof will be accepted.

The same holds for questions of axiomatic independence where attempts are made to deduce an axiom from the others, either directly or indirectly, by assuming its negation. What is conjectured here is not the axiom itself, even less its negation. The real conjecture is either that the axiom in question follows from the others, or that it does not, *i.e.* that its negation is compatible

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necessarily have to read the full papers or books that are in your bibliography. Many of the things that are generally known are things for which there may be no known written source. As long as people in the field are comfortable that the idea works, it doesn't need to have a formal written source" (Thurston 1994, 168). Most of the premisses that are explicitly assumed in proofs of that kind do not have the degree of generality typical of foundational axioms. Much of the work carried out by mathematicians can do without explicit reference to a specific foundational theory.

[14] For simple examples from arithmetic, see Parsons 2008, 322. Besides such obvious examples, there is also, more generally, a certain view of mathematics according to which foundational axioms receive their special status not in virtue of their being intuitively evident or particularly plausible, but because it is possible to deduce from them familiar propositions, the truth of which nobody doubts. Paolo Mancosu has labelled this view, which he identifies in Russell, Gödel and Lakatos, among others, as "hypothetico-inductivism", see Mancosu 2001, 102-108. On this view, mathematical axioms are, of course, not conjectures in the sense intended here, but rather resemble hypotheses in the natural sciences. For the question of how intuition might be at play when it comes to the axioms of arithmetic and set theory, see Parsons 2008, §§ 54-55 (328-342).

[15] See Eastaugh 2024.

with the conjunction of the other axioms. As long as the expectations regarding consistency and dependency of the axioms hold, it is not their truth that is in question, but rather their logical interdependencies. However, as the famous fate of the parallel postulate shows, expectations can be drastically deceived. And if a statement loses its status as an axiom, or never had it in relation to an alternative system, it can, of course, be advanced as a conjecture in the normal sense. Whether preserving its syntactical identity is sufficient for being the same statement across different conceptual environments is another question. For now, it is sufficient to note that not every request for proof is a case of conjecturing.

From these brief considerations alone, it should have become clear that it would be mistaken to expect a general and yet precise answer to the question of how much knowledge or ignorance is required to successfully establish a statement as a conjecture. The variability of conceivable situations is far too large for this. But it would certainly be worthwhile investigating this question more closely in various individual cases.

3.2. *Conjectures true and false*

A conjecture is an invitation to investigate and eventually demonstrate the truth of the statement being conjectured. By making a conjecture, a challenge is set up to produce a proof. And what is usually expected is a proof of the statement involved – not of its negation or of a substantially different statement. Accepting a conjecture implies therefore accepting the challenge to come up with a proof. It would be strange, perhaps irrational to accept, let alone pose this challenge if one had better reasons to believe that the statement being conjectured is false. Typically, then, to make or accept a conjecture implies the belief that the conjectured statement is probably true.

Again, simple considerations suffice to yield a second principle that underpins the practice of conjecturing: A mathematical statement is only to be conjectured if it is believed to be more likely true than false. Therefore, whether a statement can be made a conjecture, or accepted as such, depends on what is believed to be true and what is believed to be false.

One might ask, now, what kind of belief is supposed to be involved here, or whether belief is even the right notion. Is it really necessary to *believe* a statement to be true in order to conjecture it? And if so, how strong has that belief to be? Would it not be sufficient to merely *assume* the statement to be true?

In mathematical contexts, it is particularly important to distinguish conjecturing from making an assumption for the sake of proving a conditional or producing an indirect proof. When assuming a certain statement – whether it is to refute it or to infer another statement from it –, the steps to be taken are quite different from those to be taken when the same statement is being conjectured in order to prove its truth. Conjecturing therefore involves more than mere assumptions. But of course someone could advance a conjecture without actually believing it to be true, and others might accept it and investigate its truth. However, this would be a deviant case of conjecturing, if only because the reasoning behind it remains obscure. It is clear what conjecturing in mathematics normally aims at: to prove the truth of the conjectured statements. In order to allocate limited resources in a way that increases the chances of success, the obvious thing to do is to only conjecture statements that seem more likely to be true than false. Acting contrary to this principle would, in general, undermine conjecturing as it is practised in mathematics. And without conjecturing, mathematics would be a completely different matter, if at all.

What is also clear is that mathematicians do have beliefs about the truth of certain unproven statements, and that the confidence accompanying their beliefs may vary in degree. As Timothy Gowers puts it in a recent paper, mathematicians "are extremely confident that Goldbach's conjecture is true and that π is a normal number, they are quietly confident but with not quite 100% certainty that the Riemann hypothesis is true, they think that almost certainly P≠NP (though a few outliers think that this confidence is misplaced), and think it is probably not possible to factorize a large integer in subexponential time but would not be unduly surprised if it turned out to be possible" (Gowers 2023, 58). Conjectures, therefore, can be linked to beliefs of various strength, although not all differences of degree have the same relevance for conjecturing. After a certain lower threshold, the degree of confidence in a statement's truth becomes secondary to the question of whether it should be advanced as a conjecture or not.[16] On the other hand, when it comes to the question of abandoning an established conjecture, an initially higher degree of confidence means that stronger evidence for the falsity of the statement must be presented.

[16] In view of this, one might be inclined to treat conjecturing as a *sui generis* attitude. However, this would not spare us the work of clarifying how this attitude is connected with the various beliefs mathematicians hold.

The pressing problem now is to make sense of this notion of believing a mathematical statement to be more likely true than false or *vice versa*. How can we even speak of probabilities if mathematical statements are either necessarily true or necessarily false? Since this intricate issue cannot be discussed here in the detail it deserves, what follows is limited to a few points that are central to our investigation.

The first thing to note is that the sort of probability mathematicians may be inclined to assign to a statement's being true or being false is usually not expressible in terms of precise numerical values. A statement could be deemed 'plausible', 'very probable' or 'highly unlikely', for instance. But, as George Pólya convincingly argued in his classic *Mathematics and Plausible Reasoning*, it would be nonsensical to ask for an exact percentage, at least in most cases (Pólya 1954, 68-70; 1968, 109-111).[17] One reason for this is that whether a statement is considered to be more likely true than false, or whether one is prepared to assign it any probability at all, depends heavily on one's own knowledge, beliefs, and even preferences and predispositions – all of which are hardly quantifiable. Yet, the slightest shift in one of these factors can lead to significant changes in related probability judgements.

Take for example the Banach-Tarski paradox, a theorem in set-theoretic geometry proved in 1924 by the two mathematicians from which it received its name. In one version, it affirms the possibility of decomposing a ball in three-dimensional Euclidean space into a finite number of pieces such that they can be re-assembled into two balls, each of equal size to the first (Banach and Tarski 1924, 260-262). At first glance, and with not much more knowledge than what is required to superficially understand the mathematics involved, the statement appears almost certainly false, as it seems to contradict our common spatial intuition. Thus, if at some point in the early 1920s Tarski had told me that, against all odds, he wanted to conjecture this very statement, clearly, I would have been entitled to ask for reasons why it should be believed to be more likely true than false. After all, the truth of a conjecture has to be plausible enough as to convince others to

[17] This is in line with much of the more recent literature on plausible reasoning in mathematics. For further discussions, see Franklin 2016 and Corfield 2001 (as referred to in Gowers 2023, 76). For an elaborate defence of imprecise credences and their rational permissibility, see Isaacs, Hájek and Hawthorne 2022.

put efforts into proving it. And if Tarski had been unable to provide any evidence, the conjecture could have been rejected as insufficiently justified.

In fact, Tarski would have been perfectly able to corroborate his belief by invoking what Pólya calls “patterns of plausible reasoning” and backing them up with mathematical certainties. For example, Tarski could have pointed out that his conjecture implies a well-known theorem proved a few years earlier by Hausdorff (for the mathematical details, see Tomkowicz and Wagon 2016). In doing so, he would have activated the pattern described by Pólya as “verification of a consequence”: If A implies B and B turns out to be true, this makes A more credible (Pólya 1968, 3-5). The fact that Hausdorff’s theorem appears to be almost as paradoxical as Tarski’s own statement is not detrimental to his conjecturing it. On the contrary, the credibility of Tarski’s conjecture is even increased by this fact, the relevant pattern of reasoning now being that of the “verification of an *improbable* consequence” (Pólya 1968, 7-9; for further explanations, see Gowers 2023, 76-77). To bolster his position, Tarski could also have gone the other way round by referring to a more general statement from which his conjecture is seen to follow (see Gowers 2023, 78-81). In particular, he could have tried to show how to derive his paradox from the axiom of choice by an ingenious use of it. If successful, he would not have merely provided further evidence for his conjecture, but arrived at a sufficient proof, at least in the eyes of most mathematicians. Those who prefer to reject the axiom of choice, on the other hand, would have seen Tarski’s argument as a *reductio ad absurdum*, thereby reinforced in their rejection of the axiom. All of these are possible ways in which opinions about Tarski’s conjecture could have been influenced.[18]

The recent literature largely agrees with Pólya’s account of how mathematicians tend to, or should, revise their beliefs in view of changing evidence. Often, the emphasis is placed on the objectivity of the relations between evidence and belief that are involved in such processes (see Franklin 2016, 15-16; Gowers 2023, 83-92). And it is certainly true that the forming of conjectures cannot be understood as a merely subjective phenomenon that manifests itself in arbitrary ways in different individuals.

[18] For an insightful discussion of the Banach-Tarski paradox, its mathematical history and its bearing on issues related to intuition, see Feferman 2000, 323-328. For further information on the historical background against which Banach and Tarski developed their proof, see Feferman and Feferman 2008, 43-52. It seems, however, that not much is known about the conjecturing phase.

In accordance with Pólya, however, I would like to emphasise here the "double nature" of the reasoning involved, making it appear "sometimes as 'objective' and sometimes as 'subjective'" (Pólya 1968, v).[19] This means that in our elucidations of the practice of conjecturing, some space should be allowed for subjective factors which cannot be shared with others or for which objectivity cannot always be claimed. Diverging preferences in the choice of admissible axioms could be a case in point, dispositional differences in weighting evidence when assigning probabilities another one (see Pólya 1968, 113-116).

This distinction between objective and subjective factors is all the more pertinent to the present investigation, as expert intuition can be seen to fluctuate between the two categories. Contrary to common intuition, it is typically not present in many. And, in exceptional cases, it may only manifest itself in a single expert, with no one else being able to share it. However, it does not follow from this that expert intuition can only serve as subjective evidence, even in such exceptional cases. First, the expert can reveal that her conjecture is based on a certain intuition she has, without enabling others to experience the intuition for themselves. Those who are unable to share the expert's intuition can still include it as objective evidence in their evaluation of probabilities. To return to our earlier example, if Tarski had told me that his conjecture was based on some advanced intuition he had, I would have taken his word for it and thereby accepted his conjecture as sufficiently probable. Secondly, and more importantly, it is quite certain that Tarski, with all his expertise, would have been able to guide others, even novices, to see for themselves what he saw, *i.e.* to make accessible the content of the intuition that led him to consider such counterintuitive decompositions in the first place. This, in turn, would have potentially helped others to improve their own intuitions, more precisely to learn how to properly constrain their common geometric intuitions and to clearly distinguish them from the more abstract intuitions needed in set theory (see Feferman 2000, 325-330). Indeed, this may have been one of the effects that results such as the Banach-Tarski paradox had on parts of the mathematical

[19] James Franklin elaborates Pólya's observations in terms of a logical theory of probability, emphasising that Pólya "rightly saw" that the plausibility relevant for mathematical conjecturing "was not a matter of subjective impressions, but it was a matter of the degree of belief that was *justified* by the evidence" (Franklin 2016, 15). But Pólya's remarks about the double nature of plausible reasoning in mathematics make him seem more like a proponent of a position between subjective and objective Bayesianism. On the question of where Pólya fits into the spectrum of Bayesians, see Corfield 2001.

community, especially on the younger generation.[20] Under the right circumstances, then, expert intuition not only trumps other kinds of evidence, including common intuition, it can also help to refine it and improve its use.

But what about statements on which all our intuitions are silent? And more generally, what about statements for which there are no reasons to believe them to be more likely true than false? Is it not possible to conjecture such statements? Well, rather than advance a conjecture, one would simply formulate an open problem. Sometimes the difference between conjectures and open problems is blurred by the fact that some believe the statement in question to be true, while others prefer to stay agnostic about it. But this should not conceal the fact that the strategies for proving a specific claim will most often differ from the strategies for disproving that claim.

What I would like to maintain, therefore, is that to advance a conjecture is not simply to say: "Take a look at this statement. It is interesting and it could be fruitful to investigate it further." Conjecturing essentially contains a truth claim. Like every science, mathematics strives for truth. However, due to the intimate connection between truth and falsity in mathematics, the truth claim involved in a conjecture can in certain cases become next to negligible. As we shall see in the next section, this has to do with the fact that whether a statement has the potential to become a fruitful conjecture does not depend, in general, on its truth value. False conjectures can become as fruitful as true ones.

3.3. *Forming potentially fruitful conjectures*

When doing mathematics, mathematicians are not always only interested in getting certainty about the truth value of statements. Often, what is sought after is more than just knowing *that* something is true or *that* something is false. Understanding *why* it is true, or even *why* it is false, can be at least as important and worthwhile an investigation. Accordingly, one does not only want to conjecture a true statement, but a statement whose investigation will turn out to be fruitful by improving mathematical understanding. So what is being sought after are conjectures that help to

[20] See Moore 1982, 284-290. As noted in Tomkowicz and Wagon 2016, 311, the Banach-Tarski paradox also "has a historical importance for mathematics that is completely independent of foundational questions and the axioms of set theory", since it proved fruitful for the development of various mathematical ideas, in particular for the notion of amenability in groups.

deepen or broaden our understanding of the statement's conceptual environment by triggering the development of new concepts, methods, even theories. And for this, the statement conjectured needs a certain potential of becoming fruitful.

But what exactly is meant here by fruitfulness? And what does it take for a conjecture to become fruitful? What does it mean to say that a conjectured statement has the potential of becoming fruitful? And how does one know whether a given statement has or lacks that potential?

Admittedly, I use the term 'fruitful' in a rather unspecific way here. I try to cover a vast multitude of aspects that would have to be disentangled again in more elaborated discussions of the topic. What can be said now is just so much: For a conjecture to actually become fruitful it needs to be investigated, to actually spark the interest of mathematicians. Therefore, whether a conjecture becomes fruitful depends, at least in part, on contingencies like the preferences of dominant groups within the mathematical community, or the authority of an individual advancing the conjecture, etc. But clearly, not every mathematical statement has the potential of becoming fruitful. To show that a statement lacks that potential, one could, for example, expose its triviality or narrowness (see, however, the example of Desargues's theorem mentioned above). On the other hand, to conclusively show that a statement has the potential of becoming fruitful, there is no way around investigating the statement and coming up, eventually, with a proof or disproof. Fruitfulness is then manifested in the fact that such investigations improve understanding, for example by revealing a "beautiful" uniformity across a wide range of variation, or that if, on the contrary, these investigations only exacerbate an existing mystery, they at least prompt further research.[21]

These considerations suggest a third principle at work in the practice of conjecturing: Often, one of the aims of advancing a conjecture is to help improve mathematical understanding in significant ways rather than just attaining truth. For making this possible, the conjectured statement must possess the potential of becoming fruitful. Therefore, whether a conjecture will actually spark the interest of the competent community often depends on whether it appears potentially fruitful or not.

[21] For this distinction between "two flavors of judgments of fruitfulness" – one that emphasises the usefulness of concepts or statements, as opposed to one that might be more closely related to aesthetic judgements –, see Tappenden 2012, 216-219.

Obviously, this third principle is not as constraining as the first two. It is certainly possible to make a conjecture that clearly could not become fruitful. One might want to investigate its truth for reasons unrelated to matters of understanding, *e.g.* because it had been stated a long time ago, or in order to provide rigorous proof for all truths of a given domain within a project of formalisation. However, uninteresting statements, although they may be accepted as conjectures, will most likely not attract the necessary attention to be investigated much. In a certain way, then, conjectures are better off when they are held to be false rather than unfruitful. Better be wrong than boring, one could say. Why is this so?

To doubt the truth of a conjecture is not the same as to deem it unfruitful. One might believe the conjecture to be false and yet be highly interested in settling the question of its truth value once and for all – even if this means proving the conjecture to be false. In such a case, one could be said to investigate the negation of the statement first advanced as conjecture. This is not to dismiss the initial conjecture totally, but to try to decide the question of its truth in the negative, so to speak. Moreover, a false conjecture can become fruitful in two different ways. Either, by disproving the statement initially conjectured, one has proved the truth of an equally interesting statement, namely its negation. Or, the negation of the initial statement is uninteresting, but the reasons why it is false are worth investigating.

A good example for a false conjecture, the negation of which turned out to be an equally interesting statement, is the decidability of first-order logic. Hilbert and members of his school conjectured, believed and clearly hoped in the 1920s, and some still in the 1930s, that a decision procedure for first-order logic not only existed, but could perhaps be given one day. The basic intuition behind it was rather philosophical in nature, and Hilbert had articulated it in his famous axiom of solvability, according to which every mathematical problem must have a solution. But after Gödel had presented his theorems, the conjecture became very improbable. Accordingly, the question of decidability did not turn into an open problem, but instead the negation of the initial statement became the conjecture sought after. Neither Church nor Turing hoped to come up with proof for the existence of a decision procedure, let alone the formulation of such a procedure. Instead, what they tried to do, and eventually succeeded in doing, was to show that and, to some extent, why no decision procedure could exist. Their conjecture proved to be enormously fruitful in that it triggered the development of new theories, arguably even of new disciplines.

The second case mentioned above is when the negation of the conjectured statement is not interesting in itself, but the reasons for the statement's being false are. Imagine that, against all odds, a counterexample to Goldbach's conjecture was found. Although the specific instance for which the conjectured statement was proven false could be uninteresting, one would still want to better understand why the statement holds for so many even numbers but almost miraculously not for others (see Gowers 2023, 93). One might also want to know whether it is false of finitely or infinitely many even numbers, etc. The investigation of such questions could very well lead to the result that the initial conjecture – although wrong and its negation in itself not very interesting – becomes fruitful in unexpected ways.

Since false conjectures can become just as fruitful as true ones, the ability to pick out the statements that have the right potential regardless of their truth value is in a sense even more important for the advancement of mathematics than having a keen eye for its truths. As should have become clear now from various examples, intuition, particularly that of an expert, can be a decisive factor when it comes to aiming at mathematical truths. But just as it provides orientation in the search for truth, intuition can also help when it comes to identify potential fruitfulness. In this role, intuitions will act neither as a source of evidence nor as evidence themselves. They will rather serve to filter out from the countless sterile statements those that are of mathematically valuable content, be it true or false. And the way in which this selection is made will reflect personal predispositions, aesthetic preferences and cultural influences to a far greater extent than if it were only a matter of truth. It is here that the individuality of the conjecturer can fully express itself, revealing their mathematical style.

4. Conclusion

Let us now take stock of the main findings and answer the question raised at the beginning of this paper. In mathematical conjecturing, intuition serves primarily as a source of potential candidates for conjecture. Since forming and advancing conjectures in mathematics is a rational practice, it is rooted in a manifold background of knowledge and beliefs about mathematical matters. What will be accepted as a serious conjecture depends on what the community concerned knows and ignores, what it believes to be true and believes to be false. Here already, various preferences and dispositions may come into play. These latter factors, whether due to personal temperament or mathematical culture, will weigh even more heavily when it comes to the decision of allocating time and effort in order to actually investigate the truth of a particular conjecture. The challenge is not only to pick a true statement, but one which will prove fruitful in that its investigation will yield a better understanding of its conceptual environment or prompt further research. Although this mixed background puts certain constraints on what constitutes good conjecturing, it still leaves open a wide range of possible lines for research. The main role of intuition is to help mathematicians find their way through this sheer multitude and to "see the end from afar".[22] This heuristic role has two distinct and, to some extent, independent aspects. The first is to help identify those statements that, when put in the right light, appear more likely to be true than false. And the second aspect is to grasp what has the potential to become fruitful. But since it can be just as revealing to show the falsity of a statement as it is to show its truth, false conjectures can become just as fruitful as true ones. And conjectures that show the right potential, even if they turn out to be false, will generate more interest than conjectures that appear unfruitful from the outset. Intuition for the fruitful may therefore claim a certain priority over intuition for the true.

Acting as guiding light when navigating the unknown is not the only role that intuition plays in conjecturing. Even though intuition, especially that of experts, is generally considered more reliable than random guesswork, it is not blindly followed. As the knowledge and beliefs in the background constantly change, and the preferences and dispositions may vary too, the process of forming conjectures can be quite dynamic. New evidence, or shifts in the weighting of existing evidence, can lead to changes in probability assignments such that the initial statement of a conjecture must be adjusted to claim the truth more convincingly. In some cases, this can lead to the statement's negation becoming the new conjecture. In such processes, the intuitions themselves, whether they are common or of the rarer kind typically found in experts, will sometimes be counted as evidence or counterevidence. And so, as a factor alongside others, they become entangled in the dialectic of conjecture forming and, ultimately, in the search

[22] As Henri Poincaré phrased it: "Pure analysis puts at our disposal a multitude of procedures whose infallibility it guarantees; it opens to us a thousand different ways on which we can embark in all confidence; we are assured of meeting there no obstacles; but of all these ways, which will lead us most promptly to our goal? Who shall tell us which to choose? We need a faculty which makes us see the end from afar, and intuition is this faculty. It is necessary to the explorer for choosing his route; it is not less so to the one following his trail who wants to know why he chose it" (Poincaré 1958, 22).

for proof. Again, it is expert intuition which, in this justificatory role too, will normally prevail over more common kinds of intuition. Where it is not shared, the intuition of an expert can still be included as objective evidence by others in their evaluation of probabilities. But to significantly increase the justificatory weight of their intuitions, experts will try to guide others so that they can intuit for themselves how things probably are. This will be particularly useful in cases where the expert's intuition contradicts current beliefs and requires a re-evaluation or refinement of more common intuitions.

Besides persuasion, the effort to make advanced intuitions accessible to others can also serve a different purpose. For it offers others the opportunity to develop and improve their own intuitions, even in ways that may seem counterintuitive, thus helping to spread the kind of intuitive familiarity with the subject matter that enabled the expert to make a good conjecture in the first place. Here, then, is another role for intuition which is essential to the mathematical practice of conjecturing: to instill that close acquaintance with and deep understanding of conceptual environments from which alone fruitful forays into unknown regions of mathematical truth can be attempted.[23]

[23] I wish to thank the participants of the *Nancy-Liège Workshop on Mathematical Intuition* (November 2023), of the *Colloquium in Theoretical Philosophy and History of Philosophy* at the University of Zurich (November 2023), and of the 25th *Rheinisch-Westfälisches Seminar zur Geschichte und Philosophie der Mathematik* (July 2024), as well as the members of the group *Origins of Contemporary European Thought 1837-1938* at the University of Geneva and two anonymous referees for their helpful comments.

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