Chapter 1:
In the Margins

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> Many will pass by, and knowledge will increase.
> —Francis Bacon 1620.

1. It will be useful to have before us an apparent example of mathematical testimony. Why not the most famous? Amongst the many notes that Pierre de Fermat scribbled in the margins of his copy of Diophantus' *Arithmetica* was the following:

> …it is impossible to separate a cube into two cubes, or a biquadrate into two biquadrates, or generally any power except a square into two powers with the same exponent. I have discovered a truly marvelous proof of this, which however the margin is not large enough to contain. (c.1637. Heath 1910: 144–145.)

Although Fermat's marginalia were in the first instance intended for his eyes only, they seem to have been designed to form the basis for one or another form of transmission or publication, and a number of them found their way into Fermat's correspondence. However, this particular note was shared for the first time only after Fermat's death, when his son Clément-Samuel republished *Arithmetica* with Fermat's commentaries in 1670. In the note, Fermat appears to make two claims. The first is to the effect that, for $n \geq 3$, $x^n + y^n = z^n$ has no natural number solutions. That claim later became known as *Fermat's Last Theorem*, because it was the last of Fermat's numerous marginal theorems or conjectures whose fate had not been publicly decided. (The vast majority had been decided in Fermat's favour.) The second is to the effect that Fermat had demonstrated, or proved, that result. As is well known, no explicit record of Fermat's purported demonstration remains. As Michael Mahoney observes, Fermat's fondness for the aphorism from Bacon that serves as this chapter's epigraph didn't translate into volubility. (Mahoney 1973: 341.) The first published proof of the theorem, due to Andrew

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Wiles with assistance from Richard Taylor, appeared in 1995. (Wiles 1995; Taylor and Wiles 1995. For a useful overview of the history of the theorem, see Singh 1997.)

The purpose of this chapter is to raise a series of questions about Fermat's first apparent claim, all centred on the question whether, prior to the appearance of Wiles' proof, we could have come to know that Fermat's theorem is true on the basis of accepting Fermat's testimony to that effect. Having raised a number of such questions, I'll return at a more leisurely pace to some of them, or generalisations thereof, in the following chapter. (Readers who think that we have reason to give a fast, negative answer to the question about Fermat's case are requested to be patient.) My primary purpose is not to assess Fermat's capacity to provide us with knowledge of the theorem. Rather, my aim is to present a specific case of apparent mathematical testimony in order to see some of the questions that might naturally arise in seeking to determine its epistemic credentials. As we'll see, some of these questions are more or less specific to the case of mathematical testimony, whilst others concern the epistemic power of testimony more generally.

2. The first question that I want to raise is very general. Is it ever possible to come to know things by accepting another's claims? For if it isn't, then a trivial corollary is that it is not possible to come to know Fermat's theorem by accepting his claim. I shall simply assume that the answer to the first question is that, yes, it is possible to come to know things by accepting another's claims. As a number of theorists have observed, scepticism with respect to that possibility is not much less virulent than near global forms of scepticism. (See especially Coady 1992.) To that extent, we might reasonably ask for an account of how it is possible to come to know on the basis of accepting another's testimony, but it would be pointless to raise the general question whether it is ever possible. (Notice, for example, that if it were impossible to gain knowledge by accepting testimony, then we would know nothing about Fermat, and few of us would know anything about the standing of his theorem.) I shall therefore assume that it is at least sometimes possible to come to know things by accepting another's claims. And I shall assume, moreover, that answers to my other questions that would cast a shade over that possibility are to that extent disfavoured.

Assuming, then, that it is sometimes possible to come to know things by accepting another's claims, we can inquire into the conditions under which it is possible. If we were able to specify such conditions, that in turn would enable us to address the question whether we, and Fermat, meet those conditions, and so to make progress in deciding whether it would be possible for us to come to know, by accepting his claim, that there are no natural number solutions to the target equation.

The next most general question that I want to raise concerns whether it is possible to come to know specifically mathematical truths by accepting another's claim. Scepticism here is apt to seem more constrained than blanket scepticism about the epistemic power of testimony and, to that extent, more reasonable. That is, if it were possible to have grounds for scepticism about mathematical testimony that were not thereby grounds for scepticism about testimony more generally, then the unacceptability of the latter wouldn't immediately foreclose on the acceptability of the former. Furthermore, scepticism about the epistemic power of mathematical testimony seems, perhaps by contrast with global scepticism about testimony, to have some reasonable adherents. (See e.g. Kant 1992: 31, 69, 297, 245, 340–341, 897; Williams 1972. As I'll explain in the following chapters, some apparent expressions of more global forms of scepticism about testimony, for example by Plato and Locke, are better construed as expressions of more limited forms of scepticism about the power of testimony—for example, as expressions of scepticism about the power of testimony to deliver specifically mathematical knowledge, or other more specific epistemic goods.) One very general motive for this type of scepticism about the power of testimony to deliver mathematical knowledge would be provided by endorsement of views according to which our relationship to mathematical truths never amounts to knowing those truths. I shall assume, however, that it is possible to come to know mathematical truths, at least on the basis of familiarity with proofs of those truths. My interest is in a more local form of scepticism according to which mathematical knowledge is attainable, but not on the basis of testimony. Motives for adopting this sort of stance must rely on what are taken to be special features of mathematical knowledge, for example that it is apriori knowledge, or bound up essentially with possession of proof, or intrinsically universal in a way that precludes the possibility of differential expertise. A central challenge facing the proponent of this type of scepticism is to furnish their position with principled limits. At the very least, they will need to mark out a clear boundary around the cases for which they hold testimony to be epistemically impotent, on pain of sliding into global scepticism about testimony. And if their position is to carry conviction, they will need in addition to provide an acceptable explanation for why testimony is impotent with respect to all and only the cases that fall within that boundary. This is one of the issues to which we will return in the following chapter.

A further question that I want to raise about Fermat's case is whether there are epistemic goods or standings other than knowledge that cannot be acquired on the basis of accepting someone's word. For it seems clear that there may be broadly epistemic goods or standings distinct from, or not obviously equivalent to, the possession of knowledge *per se*. For example, there may be forms of ability or understanding that are not simply cases of knowing; and it might be possible to know a mathematical truth without knowing it apriori. In that case, it would be important to distinguish the claim that one cannot come to know by accepting mathematical testimony from claims to the effect that one cannot come on the same basis to have those other goods or standings—say, forms of mathematical ability or understanding, or distinctively apriori knowledge of mathematical truths. It might be, for example, that someone acquainted with a proof of a mathematical theorem is in an epistemically superior position to someone who has simply accepted the theorem on someone else's say so, not because only the former knows the theorem, but rather because only the former has those distinct goods or standings. In the following chapters, we'll consider in more detail the following special cases: first, the question whether one of the distinctive goods or special standings that someone might come to have on the basis of familiarity with a proof of a theorem is apriori knowledge of the theorem; and second, the question whether it is possible for us to come to have apriori knowledge simply by accepting another's claims.

Returning to the main theme, let's assume for the moment that it is sometimes possible to acquire knowledge of a mathematical theorem by accepting another's claim to the effect that it is a theorem. In that case, the next question is whether the case of Fermat is a witness to that sometime possibility.

One reason that we might take ourselves to have for doubting that it is possible to come to know the theorem by accepting what Fermat wrote is the following. We might hold that it is in general possible to come to know something by accepting what someone says only on condition that their saying what they did was in the service of their performance of a more specific type of speech act: minimally, an assertion or telling, as opposed, say, to a musing or a conjecture. (See e.g. Moran 2005a, 2005b; Owens 2006; Williams 2002.) And we might hold, in addition, that we lack evidence for thinking that Fermat meant to assert or communicate the theorem, as opposed to noting it as worthy of further investigation, presenting it as a reasonable conjecture, or the like. Furthermore, we might reasonably hold that Fermat would have been more circumspect in offering up the theorem for public consumption and that, at the least, he would have been liable more carefully to check his derivation of the theorem before seeking to tell others of the result. However, pulling in the opposing direction, we have that Fermat presented himself as in possession of a proof of the theorem. That seems incompatible with his at the same time continuing to view the theorem as a musing or conjecture. Whilst it may be true that Fermat would have done more work on his derivation prior to publication, it doesn't follow that he didn't take himself to be in a position to assert the theorem in advance of undertaking that work. Although Fermat never went public with the theorem, it was anyway unusual for him to offer others more than hints at any of his results. The evidence that we have suggests that Fermat's reticence was due more to an aversion for controversy than to a lack of confidence about his results. (Mahoney 1973: 22–25.)

If we assume that Fermat asserted his theorem, and would at the time have taken himself to be in a position to tell others of the result, we might nonetheless take ourselves to have other reasons for holding that we couldn't come to know the theorem by accepting what he asserted.

One such reason might arise from a view to the effect that the preservation of knowledge acquired on the basis of testimony depends upon the continued willingness of one's interlocutor to address challenges to the item of knowledge. Thus, for example, it might be held that knowing depends upon being able satisfactorily to answer questions about how one knows. And it might be held in addition that stating that you heard it from so and so would not count as a satisfactory answer to such a challenge, since it would open one to further questions about how so and so knows. Since it would ordinarily be allowed that one can acquire knowledge from another's testimony without scrutinising their credentials, one might not autonomously be able to answer those further questions. However, one's claim to know might be allowed to stand just insofar as one is in a position to pass such challenges back to the testifier. Accordingly, preservation of one's claim to knowledge might be taken to depend upon the continued willingness of one's interlocutor to shoulder the burden of addressing such challenges. And that, in turn, might lead us to take Fermat's incapacitation to undermine the possibility of our acquiring knowledge from his testimony.

However, the claim that someone can preserve a claim to know only insofar as they maintain a capacity satisfactorily to answer, or to defer, challenges to their credentials seems fanciful. We ordinarily take ourselves to know a vast number of things without having maintained any detailed view about how we came to know them. ("How do you know that there are more than thirty bus routes in London?") Relatedly, we would ordinarily allow that someone might retain a piece of knowledge that was acquired on the basis of testimony despite their being unable, or unwilling, to detail their sources, and despite those sources being unable, or unwilling, to shoulder challenges to their credentials. Indeed, we would ordinarily allow that someone might retain a piece of knowledge that was acquired on the basis of testimony even after the initially knowledgeable source of that testimony had ceased to know. Rescinding any of those ordinary concessions would have dramatically sceptical consequences: minimally, that no knowledge gained on the basis of testimony could outlast its original source.

Another reason for holding that we couldn't come to know the theorem by accepting what Fermat asserted is that we might take it that in order for us to come to know the theorem by accepting Fermat's assertion, we would need to have independent evidence that Fermat was a reliable guide to this tract of mathematical reality. More minimally, we might take it to be required that there aren't any, or aren't too many, grounds for thinking Fermat unreliable. Are there such grounds?

3. We know that Fermat was not quite infallible. For example, in a letter of August 1640 to Bernard Frenicle, Fermat conjectured that $2^{2^n} + 1$ is prime for all n:

> But here is what I admire most of all: it is that I am just about convinced that all progressive numbers augmented by unity, of which the exponents are numbers of the double progression, are prime numbers, such as
>
> $$3, 5, 17, 257, 65537, 4\ 294\ 967\ 297$$
>
> and the following of twenty digits
>
> $$18\ 446\ 744\ 073\ 709\ 551\ 617, \text{ etc.}$$
>
> I do not have an exact proof of it, but I have excluded such a large quantity of divisors by infallible demonstrations, and my thoughts rest on such clear insights, that I can hardly be mistaken. (Mahoney 1973: 296–297.)

By contrast with his (private) mention of the theorem, Fermat explicitly presented his claim to Frenicle as a conjecture for which he lacked a proof, albeit one about which he was decisively confident. However, in later correspondence with Pierre

Carcavi (meant ultimately for Christiaan Huygens), Fermat asserted that he now had a proof of the conjecture, based upon his method of infinite descent. (Incidentally, the method of infinite descent is viewed by some as a highly plausible source of Fermat's confidence in the theorem.) (Mahoney 1973: 337–348.) Fermat's confidence turned out to be misplaced. Leonhard Euler disproved the conjecture in 1732, showing that $2^{2^5} + 1$ is divisible by 641. (Euler 1733.)

Be that as it may, it would be implausible to require in general that one can come to know something by accepting someone's word only if they are infallible. So, it would require special pleading with respect to mathematical testimony, or testimony specifically to the theorem, to parlay Fermat's slight fallibility into grounds for disallowing the possibility of our acquiring knowledge of the theorem by accepting his assertion of it.

A more reasonable concern at this point arises from a different putative condition on coming to know from another's testimony, the requirement that the testifier possesses requisite knowledge. In this case, it might be held that it would be possible for us to know the theorem by accepting Fermat's assertion only if Fermat knew the theorem. And—finally rewarding the patience of those who thought this an obvious bar on our coming to know the theorem from Fermat—it is arguable that Fermat did not know the theorem. (It is important not to confuse that concern with a near cousin, a concern arising from the fact that we lack independent evidence that Fermat knew the theorem. For the general demand for independent evidence of knowledge on behalf of our interlocutors leads fairly directly to general forms of scepticism about the epistemic power of testimony.) The concern raises two questions. First, do we have reason to think that Fermat didn't know the theorem? Second, is it a general requirement on coming to know something by accepting another's word that they know it?

In pursuing the first question, we might begin by asking whether, as he claimed, Fermat was able to demonstrate the theorem. For possession of an ability to demonstrate a theorem would ordinarily put someone in a position to present, and to acquire familiarity with, a demonstration of the theorem. And that would, in turn, ordinarily be sufficient to put them in a position to know the theorem. However, we should avoid assuming at the outset that appropriate familiarity with a demonstration is the only way in which someone could come to know a theorem. In order to avoid identifying possession of knowledge of a theorem with possession of a demonstration of the theorem, we can impose additional conditions on proof or demonstration. Let's assume, then, that a demonstration of the result would be either such as to be viewed as such by a reliable and appropriately competent, but otherwise typical, contemporary mathematician (or mathematicians), or such that a mathematician (or mathematicians) with similar characteristics could fairly easily develop on its basis what they would then regard as a proof. Thus, we don't require that a proof be fully formalised or perfectly rigorous, in the sense of appealing only to atomic inferential steps, or being amenable to mechanisation. (For discussion of some of the more exigent demands that might be imposed here, see Hales 2008; Paseau forthcoming; Rav 1999, 2007.) However, we do require that proof be in principle communicable to appropriately, but not distinctively, competent mathematicians—roughly, to mathematicians able to understand, and to follow the steps of, the proof. (See

Thurston 1994.) Given that assumption about conditions on demonstration, there are a number of reasons to think that Fermat didn't have one.

One transitory ground for thinking that Fermat lacked a demonstration has, as noted above, now lapsed. In advance of Wiles' (1995) proof of the theorem, we might have held proof of the theorem to be impossible because the theorem is false. However, we now know that the theorem is true. (What about independence? If the theorem were false, then there would be at least one counterexample. But if that existential claim were true, it would be provable, since the systems responsible for its truth are complete. So, if the theorem were independent of Peano Arithmetic, it wouldn't be false. For relevant discussion, see Smith 2007: 51–70.)

Another ephemeral ground was the spread of gossip about Fermat's mathematical ability that emerged during his controversy with Descartes about optics and algebraic geometry. The gossip, to the effect that Fermat's mathematical judgements were grounded in lucky guesses rather than knowledge, appears to have been instigated by Descartes. (Gaukroger 1995: 323; Mahoney 1973: 143–213.)

Although Wiles' proof decides the issue in favour of provability, the nature of his proof might be taken to present grounds for thinking that Fermat was not in a position to construct a proof. For we might hold that any proof of the theorem must be similar to the one we now have and that Fermat lacked the mathematical resources to construct any similar proof. As Wiles puts it,

> Fermat couldn't possibly have had this proof. It's a 20th century proof. There's no way this could have been done before the 20th century. (PBS 1997.)

However, while we should agree that there is good reason to think that Fermat didn't possess a proof similar to Wiles', we lack grounds of the same sort for thinking that Wiles' proof strategy is unique. (For the latter, our grounds are weaker, inductive grounds to the effect that a large number of other strategies are known to fail. And those grounds are in partial conflict with Harvey Friedman's grand conjecture, according to which any provable theorem can in principle be proved using fairly elementary resources. (Friedman 1999.))

Better evidence for thinking that Fermat lacked a proof is to be found in his own corpus. Although Fermat preferred not to publish his results, he nonetheless occasionally revealed his hand to correspondents, often by setting theorems or conjectures as challenges. However, despite challenging others to prove special cases of the theorem, for $n = 3$ and $n = 4$, and despite presenting the basis of a proof of the latter special case in the atypically forthcoming missive to Carcavi that was mentioned above, the marginal note of the general theorem is the only mention of it amongst his papers. Furthermore, Fermat's explicit attacks on the two special cases post-dated the marginal note, suggesting that Fermat had lost confidence in the general theorem. Finally, his method of attack on the special cases was the one mentioned above, the method of infinite descent. The evidence that we have makes it plausible to conjecture that it was the application of this method to special cases that provided the basis for Fermat's early confidence in the general theorem. (There is some evidence that suggests that Fermat's discovery of the method, around 1640, post dates the marginal note, but the evidence isn't sufficiently strong to rule out the conjecture.) However, as Mahoney observes,

> …just as the prime number conjecture breaks down for $n = 5$, so too the demonstration by infinite descent of the "Last Theorem" makes a quantum jump in difficulty for $n \geq 5$. For $n \geq 23$, the method of infinite descent fails altogether. (Mahoney 1973: 345.)

There is good reason, then, to think both that Fermat lacked a proof of the theorem and that he came to realise that he did. Mahoney aptly summarises the situation at it stood in 1973:

> The "proof" probably was no proof, because Fermat could not be bothered by detailed demonstration of theorems his superb mathematical intuition told him were true. The theorem probably is true—no one to this day has doubted it—because that intuition seldom erred. (Mahoney 1973: 347.)

Although it is reasonable to think that Fermat lacked a proof of the theorem, it doesn't follow immediately that Fermat didn't know the theorem. As Mahoney suggests, Fermat seems to have had a reliable route to true mathematical beliefs other than the way of demonstration. The next question, then, is whether that route functioned, or could have functioned, as a source of knowledge of the theorem.

4. The issues here are delicate, and this is not an appropriate venue to try to decide them. Nonetheless, I shall make a few observations.

The first observation is the formal point that, insofar as the demonstration of a theorem is taken to require linear derivation from premises (e.g. axioms or other theorems) that are distinct from the theorem, it is impossible that every claim be demonstrated. Specifically, at least some of the axioms or theorems, and at least some of the relations of consequence exploited in a demonstration, must lack a demonstration. Insofar, then, as we can come to know anything by means of proof, it seems that our doing so cannot depend on appeal only to things that we have proved. And if, as seems plausible, such acquisitions of knowledge are conditional on our knowing the axioms, theorems, or relations of consequence on which our demonstrations ultimately depend, then mathematicians must be capable of knowing such axioms, theorems, or relations of consequence in the absence of proof. (See e.g. Aristotle *Posterior Analytics* 84a.)

How might mathematicians attain such knowledge in the absence of proof? One natural and appealing answer—appealing, at least, to mathematicians—is that they do so via a faculty closely akin to that which sponsors basic perceptual knowledge. Kurt Gödel presents a version of this proposal in the following passage, with specific application to mathematicians' knowledge of the axioms of set theory:

> …despite their remoteness from sense experience, we do have something like a perception of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decided in the future. The set-theoretical paradoxes are hardly any more troublesome for mathematics than deceptions of the senses are for physics….
>
> …It by no means follows…that the data of this second kind, because they cannot be associated with actions of a certain things on our sense organs, are something purely subjective, as Kant asserted. Rather they, too, may represent an aspect of objective reality, but, as opposed to the sensations, their presence in us may be due to another kind of relationship between ourselves and reality. (Gödel 1964: 268.)

Whatever the fate of Gödel's specific proposal, his claim that mathematicians can have intuitive—that is, proof independent—knowledge of axioms has a great deal of plausibility. Furthermore, having allowed that knowledge of axioms can be attained without proof, it is not obvious what precludes knowledge of at least some theorems being attainable on the basis of the same sort of power. Indeed, Gödel's account is designed specifically to allow for the acquisition of knowledge of novel (independent) axioms, and it is not straightforward to see what would prevent such a power from extending also over novel theorems. So, the case for alleging that Fermat didn't know the theorem must go beyond his want of a proof. What is wanted is, first, a principled boundary around the bits of mathematics that can be known by intuition and, second, reasons to think that the theorem falls without that boundary.

Minimally, then, the prosecution must appeal to a weaker requirement on mathematical knowledge than the global requirement of proof. Perhaps, then, instead of the requirement that one can know a theorem only if one knows (or once knew) a proof of the theorem, they might seek to impose only the weaker demand that one can know a *provable* theorem only if one knows (or knew) a proof of the theorem. That would avoid the formal difficulty. And since Fermat's theorem is provable, the consequence would be the same: Fermat didn't know the theorem. (See e.g. W. D. Ross 1930: 16–47 for the idea that at least some propositions knowable without proof—namely, the self-evident propositions, which Ross appears to identify with the propositions that are knowable without proof—are not susceptible to proof.)

The main difficulty with the weaker requirement is its implausibility. Individual mathematicians typically take themselves to know an enormous quantity of mathematics that they take to be provable without yet knowing the proofs. Thus, the imposition of a requirement to the effect that such mathematical knowledge is attainable only via proof would have dramatically sceptical consequences. One cause of this circumstance is the role of testimony in the spread of mathematical knowledge, a cause to which we will return. But another equally important cause is that competent mathematicians typically take many mathematical facts to be obvious or available on reflection in advance of proof (perhaps on the basis of some combination of intuition, training, and inductive reasoning). (That is especially so given our working assumption that a proof must be convincing, or approximately so, to other mathematicians who are competent to understand it.) Consider, for one sort of example, that the simple equation $2 + 2 = 4$ is susceptible of proof. With respect to examples of that sort, what is proved seems not only to be knowable in advance of proof, but also often to be known more securely than are the axioms that figure in the proof. Indeed, in such cases, proof isn't exploited in order to provide deductive support for theorems, but rather to provide abductive support for axioms. (See e.g. Gödel 1964; Russell 1907.) For another sort of example, consider so-called folk theorems, results that "everyone quotes…but no one can seem to cite a complete and accurate proof". (Schattschneider 1981: 141. "Everyone" here is retricted to professional mathematicians. See also, Harel 1980.) Phillip Kitcher gives the example of Stokes' theorem as a folk theorem that is widely known and used but for which no proof exists. (1984: 42) Although Kitcher is wrong to hold that no proof exists, a weaker corollary of his claim, that the extant proof is not well known by mathematicians who nonetheless know and exploit the theorem, seems to be correct. (Katz 1979.)

More generally, many mathematicians allow an extensive role in the acquisition of mathematical knowledge to broadly intuitive sources. G. H. characterises his view of the respective epistemic powers of intuition and proof via an analogy:

> I have always thought of a mathematician as in the first instance an *observer*, a man who gazes at a distant range of mountains and notes down his observations…. There are some peaks which he can distinguish easily, while others are less clear. He sees A sharply, while of B he can obtain only transitory glimpses. At last he makes out a ridge which leads from A, and following it to its end he discovers that it culminates in B…. If he wishes someone else to see it, he *points to it*, either directly or through the chain of summits which led him to recognise it himself….
>
> The analogy is a rough one, but I am sure that it is not altogether misleading. If we were to push it to its extreme we should be led to a rather paradoxical conclusion; that there is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*; that proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils. This is plainly not the whole truth, but there is a good deal in it. (Hardy 1929: 18.)

Like Gödel, Hardy seems to propose that mathematicians can acquire knowledge of the layout of mathematical reality in advance of knowledge of proof. (Strictly, one might distinguish the sort of observation of mathematical reality to which Hardy appeals from knowledge about that reality. But for present purposes it seems safe to assume that Hardy would allow that such observation can issue in knowledge.) But Hardy explicitly extends the range of the proposal in order to take in cases of knowledge of theorems. (Indeed, Hardy includes proofs within the scope of intuitive knowledge, and sometimes (e.g. 1940: 113) collapses the distinction between knowing a theorem and knowing the theorem together with its proof. Furthermore, the proposal is consistent with allowing that intuitive knowledge of a theorem often depends upon knowledge of other axioms, theorems, and relations of consequence. All that the proposal rejects is the further claim that the further knowledge must provide the basis for independently convincing proof of the theorem.) And the extension is grounded in the difficulty of drawing a plausible and principled boundary to the extent of intuitive knowledge that includes all and only the axioms and basic relations of consequence. (We might speculate that Hardy's sympathy for the extension was shaped, in part, by his experience of working with the renowned intuitive mathematician Srinivasa Ramanujan, on which see C. P. Snow's introduction to Hardy 1940: 30–38 and Kanigel 1992.)

It shouldn't be thought that allowing for sources of knowledge of theorems other than familiarity with proofs would deprive proof of its most significant role in mathematics. (Hardy is pretty clear that he doesn't think that it carries that consequence.) That proofs are not always necessary for knowledge of mathematical theorems does not entail that they are never necessary for such knowledge, or that they are not always sufficient. Furthermore, as I observed earlier, the ultimate search for a proof of a theorem (for Gödel, a decision) is motivated, not by simple ignorance of the theorem, but rather by other goals. For example, the goal of developing a proof might be the tracing of connections amongst theorems or domains, the testing and developing of methods of proof (perhaps including the articulation of axioms suitable to be incorporated into those methods), the generalizing of theorems, and, more generally, the deepening of our understanding of mathematical reality. Or, the goal might be that of improving our confidence in the theorem beyond the level strictly required merely to know it. Furthermore, it is, as Hardy suggests, consistent with the proposed role for mathematical intuition in underwriting knowledge of theorems that amongst the objects of that epistemic power are proofs, or facts about the consequence relations obtaining amongst axioms and theorems. (For further discussion of the possibility of mathematical knowledge without proof, and values of proof over and above furnishing knowledge of theorems, see e.g. Dawson 2006; Fallis 2003; Kitcher 1984: 36–48; Rav 1999; Thurston 1994.)

5. Let's suppose, then, that at least some theorems can be known intuitively. Could Fermat's theorem be amongst them? And if it could be, did Fermat have the resources to come to know the theorem in that way?

Here, I think we reach the limits of what we can say with any confidence. Given the extreme difficulty of the only proof that we have, it is not unreasonable to view the theorem itself as impossible to know in advance of proof. The evidence we have about Fermat himself suggests that his initial confidence in the theorem was grounded, at least in part, on a hasty induction from one or two special cases. The evidence suggests, moreover, that he quickly lost confidence in his ability to prove the general theorem. However, that evidence is consistent with his nonetheless knowing the theorem without knowing a proof. We also have the evidence, noted earlier, that Fermat's faculty of mathematical intuition was not quite infallible. However, faculty infallibility is no more a requirement on its being a potential source of knowledge than it is a requirement on the transmissibility of such knowledge by testimony. The fact that a faculty is subject to occasional misfire is consistent with its successful operation on other occasions. So, I think that we lack decisive evidence for or against the claim that when Fermat wrote the marginal comment he could have had intuitive knowledge of the theorem. Of course, even if we had solid reason to think that Fermat had neither intuitive knowledge nor proof of the theorem that would still leave open whether he knew the theorem in some other way. In particular, it would leave open the question whether Fermat knew the theorem on the basis of testimony.

Since we do not know whether or not Fermat knew the theorem, another earlier question assumes importance, whether we would be required to have such knowledge in order to come to know by accepting what he said. At least two sorts of reasons might be offered for judging that we would not be required to know that Fermat knew the theorem.

The first putative reason depends on the view that, although one can acquire knowledge by accepting what someone says only if they have that knowledge, there is no additional requirement to the effect that one must also know, or have evidence for believing, that they know. As long as one's interlocutor knows, and one lacks evidence for believing that they don't know, one can acquire knowledge from them by accepting what they say. If that position were defensible, then an attempt might be made to argue that we lack sufficient evidence to trigger the negative clause and, so, that *if* Fermat knew the theorem, then we could come to know the theorem too by accepting his assertion of the theorem. As noted above, the imposition of any stronger demand for positive evidence in favour the belief that one's interlocutor knows tends towards scepticism about testimony.

The second putative reason depends on the view that it is sometimes possible to acquire knowledge by accepting what someone says even in cases in which they lack the knowledge that one thereby acquires. If that were so, then it would follow as a trivial corollary that one might acquire knowledge in that way without knowing that one's interlocutor knows. An example of the sort of case often taken to support such a view would be the following. An intuitionist mathematician might be familiar with only a non-constructive proof of some theorem. Since familiarity with a non-constructive proof can be a source of knowledge, it would be possible for others to gain knowledge of the theorem through familiarity with the proof. However, the intuitionist's logical scruples lead them mistakenly to refuse to accept the conclusion of the proof. (At most, they are willing to accept the double negation of the theorem.) The intuitionist therefore fails to know the theorem. Nonetheless, they are (perhaps uncharacteristically) no evangelist, and so are willing to tell others the full-strength conclusion of the proof. A common judgment about cases of broadly this form is that although the intuitionist does not know the theorem, it would be possible for others to come to know the theorem by accepting the intuitionist's statement of it. (The case is modelled on cases presented in Graham 2000; Lackey 1999. An earlier example in a similar vein appears in Thompson 1970.)

I don't propose to try to evaluate either of the views just sketched at this point. We will have cause to return to them in the following chapter. For now, I'm content with the provisional conclusion that it is not entirely straightforward to close the case against Fermat's testimony.

6. The primary purpose of the foregoing chapter has not been to assess Fermat's capacity to provide us with knowledge of the theorem. Rather, my aim has been to present a specific case of apparent mathematical testimony in order to see some of the questions that might naturally arise in seeking to determine its epistemic credentials. Some of those questions have partially been addressed in the foregoing, and some will be addressed more fully in the following chapters. In the following chapters, we will consider in some more detail the following questions. (1) Are there general reasons to be sceptical about the epistemic power of mathematical testimony? (2) If one can come to know a mathematical truth by accepting another's testimony, what general conditions must be met in order for one to do so? For example, must one have independent evidence that one's interlocutor knows the truth, or will it suffice that one lacks independent evidence of their ignorance? (3) What is the status of mathematical belief acquired by accepting another's testimony? For example, can it ever amount to apriori knowledge?

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