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IBE can masquerade as enumerative induction in the confirmation of mathematical conjectures—A case study: Gauss's AGM conjecture

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1. Introduction

Although proof occupies a privileged place in mathematics, mathematicians lacking a proof (or disproof) of some conjecture routinely use mathematical evidence to confirm it. This confirmation, though falling short of proof, is sometimes strong enough to justify mathematicians in placing great confidence in the conjecture. This confirmation is then crucial to justifying mathematicians in regarding the conjecture as sufficiently promising to be worth their expending considerable effort in trying to prove it.[1] In this paper, I will pose and resolve a philosophical problem presented by one common kind of non-deductive reasoning in mathematics by which conjectures are confirmed.

The kind of reasoning in question seems analogous to enumerative induction in science. Roughly, mathematicians notice some pattern in the mathematical facts they already know and, on the basis of that evidence, regard as plausible a conjecture consisting of a straightforward generalization of the observed pattern. However, if we understand these extrapolations as straightforward cases of enumerative induction (or hypothetico-deductive reasoning—i.e., where a proposition is confirmed by its successful predictions), then we find ourselves with a problem. It is very difficult to see how this kind of plausibility argument in mathematics can ever be as strong as mathematicians often take it as being.

In this paper, I will argue that in many examples of this kind, mathematicians are actually using "inference to the best explanation" (IBE). In this way, we can better understand the crucial role played in each of these mathematical arguments by evidence beyond the conjecture's positive instances (or other successful predictions). This additional evidence is the key to resolving the problem that we encounter when we interpret a mathematician's extrapolation as an enumerative induction. The solution is that mathematicians' additional evidence justifies their having various expectations regarding the observed pattern's explanation and thereby justifies mathematicians in regarding their conjecture as strongly supported by the mathematical pattern they have observed. That mathematicians are using IBE ultimately makes a big difference (I will argue) to the conjectures they can justly make.

My argument that enumerative induction (and hypothetico-deductivism more generally) cannot do justice to these sorts of plausibility arguments in mathematics, whereas IBE can, will consist primarily of a detailed account of one prominent historical example that is often cited as typifying the sort of enumerative induction common in mathematics. The example is Gauss's famous 1799 conjecture (which he subsequently proved) that the reciprocal of the arithmetic-geometric mean (AGM) of $\sqrt{2}$ and 1 is equal to $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. Gauss's reason for making this conjecture (that is, his argument for its plausibility) seems at first glance to be an uncomplicated example of enumerative induction (or hypothetico-deductive reasoning). Gauss's argument is frequently so portrayed. However, I will argue in section 2 that as an enumerative induction, Gauss's argument could not have been nearly strong enough to justify Gauss's high confidence in his conjecture. (Textbooks typically regard Gauss's evidence as having fully warranted his high confidence.)

In sections 3 and 4, I will elaborate in detail Gauss's actual reasons for his conjecture. I will show that these reasons went far beyond the positive instances he discovered; before discovering them, Gauss had a great deal of additional, circumstantial evidence for his conjecture. His additional evidence did not consist of anything like additional successful predictions that his conjecture makes. Therefore, this evidence's confirmatory relevance to the conjecture cannot be captured

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[1] Brown (2008:164; 2017:48) and Franklin (2013:12) have emphasized this feature of mathematical practice. Pólya (1954) is a classic discussion of plausibility arguments in mathematics.

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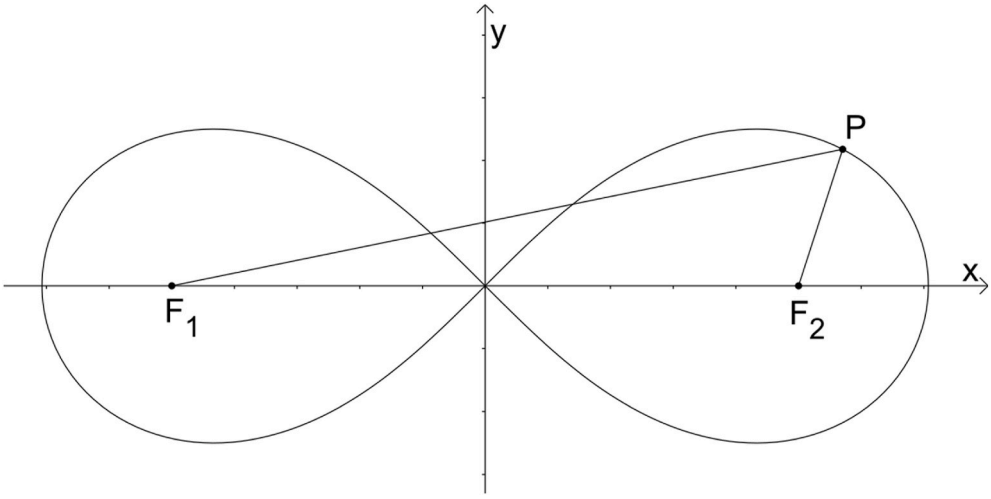


Fig. 1. A lemniscate showing a point P's distances from each of the two foci F_1 and F_2.

hypothetico-deductively or by enumerative induction.[2] I will argue that this evidence's confirmatory relevance to Gauss's conjecture is best understood in terms of IBE. Roughly speaking, his earlier evidence made the positive instances that Gauss later discovered seem suspicious—made them "cry out" for explanation. More precisely, Gauss's earlier evidence made it plausible that the positive instances he later discovered have a certain sort of explanation. Without that earlier evidence, Gauss would not even have looked for those positive instances and, more importantly, would not have been entitled to regard them as strongly confirming that they have a certain sort of explanation.

In section 5, I will take Gauss's antecedent mathematical knowledge (as described in sections 3 and 4) and show how the opinions it underwrote about the existence of mathematical explanations entitled Gauss to regard his conjecture as strongly confirmed (in both the incremental and absolute senses) by the positive instances he discovered. I will use Bayesian terms to reconstruct Gauss's non-deductive argument for his conjecture. In doing so, I will argue that Gauss's reasoning should be understood as IBE.

Finally, in section 6, I will argue that Gauss later gave a proof of his conjecture that explains why the conjecture (by then a theorem) holds. Gauss's earlier expectations about the sort of explanation the conjecture possesses were thereby borne out.

Thus, I will argue that Gauss's reasons for placing such confidence in his conjecture are very much like the reasons that scientists sometimes have for their confidence in various theories. In regarding all of these arguments as similar, I will agree with those who have understood Gauss's argument as an enumerative induction. However, I will take Gauss's argument not as enumerative induction, but rather as similar to scientific IBE's in that they all use explanatory considerations to underwrite the evidence's confirmatory significance. It is by virtue of these explanatory considerations that Gauss's evidence for his conjecture manages to be so powerful.

Of course, this analysis of Gauss's reasoning is merely a single case study. But it studies a case that (as we will see) has often been portrayed as typical of many cases of non-deductive reasoning in mathematics and as consisting of straightforward enumerative induction. It is important, then, that this non-deductive reasoning is not enumerative induction, but rather IBE.[3]

2. Gauss's conjecture and why his plausibility argument for it cannot be enumerative induction

On May 30, 1799, as (we will see) the story is often told, Gauss recorded in his mathematical diary a mathematical fact that he had just discovered: the numerical agreement of two, seemingly unrelated mathematical quantities.

The first quantity is an "arithmetic-geometric mean". For any two real numbers $a \geq b > 0$, their arithmetic-geometric mean $M(a,b)$ is the common limit of the two linked sequences $\{a_n\}$ and $\{b_n\}$ generated by taking successive arithmetic means and geometric means, respectively:

$$a_0 = a \quad b_0 = b$$

$$a_{n+1} = \frac{a_n + b_n}{2} \quad b_{n+1} = \sqrt{a_n b_n}$$

That is, the two linked sequences begin with a and b, respectively, and then a_{n+1} is the arithmetic mean of a_n and b_n whereas b_{n+1} is their

[2] Pólya (1954:4) builds his account of the confirmation of mathematical conjectures around "the fundamental inductive pattern" that "theories are confirmed by their [nontrivial] consequences or predictions." Franklin (2016:16) likewise takes hypothetico-deductive reasoning to be central to plausibility arguments in mathematics. So hypothetico-deductivism with regard to non-deductive reasoning in mathematics is no straw man. By way of the Gauss example, I will argue that some mathematical plausibility arguments that seem clearly to follow this pattern in fact do not.

[3] My argument that Gauss's confirmation of his conjecture cannot be by enumerative induction is usefully compared to and contrasted with Baker's (2007) argument that the strong confirmation that mathematicians regard Goldbach's Conjecture as having received from their evidence (namely, the verification that it holds for even numbers to 6×10^{16}) cannot be by enumerative induction. Baker and I both argue that further evidence beyond mere positive instances must be playing a major role in underwriting mathematicians' justified high confidence in their confirmed conjectures. But unlike me, Baker does not invoke IBE. Rather, he sees the key further evidence in the case of Goldbach's Conjecture as showing that any "close calls" for the conjecture are overwhelmingly likely to be among the smaller even numbers, which are the ones that have been checked.

(In addition, I disagree with Baker's argument that any enumerative induction in mathematics would be fatally flawed because it would inevitably suffer from "small-number bias" in the cases examined. Such bias could easily be avoided in the instance confirmation of a conjecture covering, say, exactly 16 six-digits numbers (such as the "calculator number" conjecture in Lange, 2016, pp. 276-7). That conjecture's holding in any given, as yet unexamined case might well be confirmed by the discovery that each of several examined cases, drawn from among the 16 cases in the conjecture's scope, accords with the conjecture. But these examined cases need not all be smaller than the confirmed unexamined case. So contrary to Baker, small-number bias is not inevitable.).

geometric mean.[4] Gauss laboriously computed $M(\sqrt{2},1)$. Here is his table[5]:

n	a_n	b_n
0	1.414 213 562 373 905 048 802	1.000 000 000 000 000 000 000
1	1.207 106 781 186 547 524 401	1.189 207 115 002 721 066 717
2	1.198 156 948 094 634 295 559	1.198 123 521 493 120 122 607
3	1.198 140 234 793 877 209 083	1.198 140 234 677 307 205 798
4	1.198 140 234 735 592 207 441	1.198 140 234 735 592 207 439

Gauss had already proved that for any real numbers $a \geq b > 0$, the two sequences $\{a_n\}$ and $\{b_n\}$ have a common limit; that each successive a_n is smaller than (or equal to) its predecessor a_{n-1}; that each successive b_n is larger than (or equal to) its predecessor b_{n-1}; and that for any n, $a_n \geq b_n$. In this way, Gauss inferred that $M(\sqrt{2},1)$ lies between a_4 and b_4 in his table. Since $\{a_n\}$ and $\{b_n\}$ come together so rapidly that a_4 and b_4 agree to 19 decimal places, Gauss had shown that $M(\sqrt{2},1)$ equals (to 19 decimal places) 1.198 140 234 735 592 207 4 ….

Gauss is known (Cox, 1984, pp. 313,316) to have read James Stirling's 1730 *Methodus Differentialis*, where Stirling (1749:51 – example IV of proposition XI) computed the following integral to 18 decimal places:

$$\int_0^1 \frac{dz}{\sqrt{1-z^4}} = 1.31\ 110\ 287\ 771\ 460\ 598\ 7\ldots$$

Gauss computed $\frac{2}{\pi}$ times this integral. He also computed the reciprocal of $M(\sqrt{2},1)$. He noticed that they agreed to 11 decimal places (which were all of the places that he computed). As I mentioned, he deemed this result sufficiently remarkable to record it in his mathematical diary on May 30, 1799, conjecturing that

$$\frac{1}{M(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$$

and that a proof of this equation "will surely open up an entirely new field of analysis" (Gauss 1866-1933:X,1,541; Cox, 1984, p. 281). By December 23, 1799, Gauss had found at least two proofs (Gauss 1866-1933:X,1,544; Cox, 1984, pp. 282-3,324).

In May 1799, then, Gauss regarded the above two quantities' agreement in all 11 of their computed digits as strongly confirming that they agree in all of their other digits. Commentators have generally interpreted Gauss's argument here as an enumerative induction: that the 11 examined cases all turn out to be positive instances of the conjecture (namely, that the two quantities agree in their 1st digit, in their 2nd digit, …in their 11th digit) strongly confirms, of each remaining case (i.e., each further digit), that it also accords with the conjecture. For example, Borwein and Devlin (2009:3-4) depict Gauss's reasoning as straightforward:

> One day in 1799, while examining tables of integrals provided originally by James Stirling, he noticed that the reciprocal of the integral $\frac{2}{\pi}\int_0^1 \frac{dt}{\sqrt{1-t^4}}$ agreed numerically with the limit of the rapidly convergent arithmetic-geometric mean iteration (AGM): $a_0 = 1$, $b_0 = \sqrt{2}$…Based on this purely computational observation (which he made to 11 places), Gauss conjectured and subsequently proved that the integral is indeed equal to the common limit of the two sequences.

By depicting Gauss's conjecture as "based on this purely computational observation," Borwein and Devlin depict Gauss as directly generalizing from his "observation" rather than engaging in any more complicated reasoning based on more sophisticated prior mathematical knowledge. Gauss is often portrayed (by, e.g., Borwein & Bailey, 2008, p. 13; Borwein & Borwein, 1987, pp. 5,7; Bühler, 1981, pp. 88-89; Rice, 2009, pp. 14-15) as having employed a kind of simple induction by enumeration ("seeing meaningful patterns in numerical data", Borwein and Bailey (2008:13) call it) that is common not only in mathematics, but also in science. Drawing the analogy to science more explicitly, Borwein and Devlin describe Gauss as engaging in a typical instance of "experimental mathematics"; Rodriguez Villegas (2007:ix) portrays Gauss as likewise engaged in a canonical case of "experimental number theory."

Gauss evidently took his evidence as entitling him to considerable confidence in his conjecture. But (I will now argue) it is difficult to see how his evidence, if it consisted solely of these two quantities' agreement in their first 11 digits, could have entitled Gauss to have had so much confidence in his conjecture.

After all, there are infinitely many real numbers that agree with $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ in their first 11 digits but diverge from $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ somewhere thereafter—at the 12th decimal place, or millionth, or billionth. The likelihood, then, that $\frac{1}{M(\sqrt{2},1)}$ hits $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ right on the nose seems to remain mighty small even given that they agree in their first 11 digits.[6]

One could express this thought more formally, if one wished to do so. Suppose that Gauss had begun by believing correctly that $\frac{1}{M(\sqrt{2},1)}$ falls somewhere within a given, reasonably wide range of real numbers (lying somewhere roughly in the middle between $\frac{1}{\sqrt{2}}$ and 1). Suppose further that as between any two equally wide subintervals within that range, Gauss was indifferent; suppose he was as confident that $\frac{1}{M(\sqrt{2},1)}$ falls within one of these subintervals as he was that it falls within the other. In other words, suppose that Gauss's initial degrees of confidence regarding the value of $\frac{1}{M(\sqrt{2},1)}$ are represented by a prior probability density distribution that is uniform over some reasonably wide interval of real numbers (containing the true value: $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$). Any such distribution assigns no finite nonzero probability to $\frac{1}{M(\sqrt{2},1)}$'s having the particular precise value taken by $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. Then Gauss discovered that $\frac{1}{M(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ agree in their first 11 digits. So Gauss ascertained that $\frac{1}{M(\sqrt{2},1)}$ is within one hundred-billionth (one $10^{-11\text{th}}$) of $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$.[7] This evidence confirmed that $\frac{1}{M(\sqrt{2},1)}$ is within, say, one-trillionth (one $10^{-12\text{th}}$) of $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. In particular, conditionalization on this evidence raised Gauss's confidence in this proposition to 10%, since the interval $[\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}} - \frac{1}{10^{12}}, \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}} + \frac{1}{10^{12}}]$ is 10% of $[\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}} - \frac{1}{10^{11}}, \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}} + \frac{1}{10^{11}}]$.[8] Nevertheless, this evidence fails to confirm, by any finite (nonzero) increment at all, that $\frac{1}{M(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. Even after conditionalization on the evidence, no finite nonzero probability is assigned to $\frac{1}{M(\sqrt{2},1)}$'s taking the particular value $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. This is merely a trivial

[4] The geometric mean of n and m is the length of the side of a square having the same area as a rectangle with sides n and m.

[5] From Gauss (1866-1933:III,364), reprinted in Cox (1984:277).

[6] Guy (1988, 1990) gives many examples of patterns in mathematics that continue for *many* cases but then suddenly stop holding.

[7] Of course, Gauss really learned merely something weaker: that his careful numerical calculations placed the two quantities so nearby. He could not justly have put total confidence in those calculations; an error might have crept in. This merely strengthens my argument.

[8] Any discussion in terms of probability distributions representing Gauss's degrees of confidence assumes that we can make sense of a probability distribution that does not assign 1 to the probability of a necessary truth (such as that $\frac{1}{M(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$). For one way that this might be done, see Garber (1983). But I will set aside the question of how the Bayesian apparatus can be applied to epistemic agents who are non-ideal in departing from "logical omniscience".

consequence of the fact that a uniform probability distribution over the real numbers in some finite interval cannot assign any finite nonzero probability to any particular number in that interval.

The upshot is that Gauss's prior opinions could not have been represented by such a uniform probability distribution in order for Gauss's numerical evidence to have justly yielded Gauss's considerable confidence in his conjecture that $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. Gauss must have assigned his conjecture greater prior probability than such a uniform distribution does.

This sort of argument is not confined to uniform prior probability distributions; the same considerations undercut our ascribing to Gauss many other prior probability distributions. Let *EQ* (for "equal") be the hypothesis that $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ and let *UN* be the hypothesis that these two quantities are unequal but agree in their first (say) 25 decimal places. Let *e* be what Gauss discovered: that they agree in their first 11 decimal places. Then $\mathrm{pr}(e|EQ) = \mathrm{pr}(e|UN) = 1$, so by Bayes's theorem,

$$\frac{pr(EQ|e)}{pr(UN|e)} = \frac{pr(EQ)}{pr(UN)}\frac{pr(e|EQ)}{pr(e|UN)} = \frac{pr(EQ)}{pr(UN)}.$$

Therefore, for Gauss's *posterior* probabilities to heavily favor *EQ* over *UN*, Gauss's *prior* probabilities must have heavily favored *EQ* over *UN*.[9] What would have entitled Gauss to such priors?

My point (whether or not it is dressed up in terms of prior probabilities) is that Gauss must have had some antecedent reason to privilege $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s hitting $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ right on the nose over $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s failing to do so but coming within (say) one hundred-trillionth of doing so. That reason, whatever it was (and I'll be focusing on it below), eludes any depiction of Gauss's argument in terms of a simple enumerative induction on the two quantities' agreement over their first 11 decimal places. After all, Gauss found that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ agrees with $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}} - 10^{-25}$ over their first 11 decimal places. But Gauss did *not* regard this agreement as strongly confirming that they are equal. Gauss's reasons for doing so were good ones but cannot be captured by enumerative induction (or hypothetico-deductive reasoning). They can be captured (I will argue) by IBE.[10]

My argument here is not motivated by any general inductive skepticism. In particular, my argument fails to generalize to what might seem to be analogous cases of confirmation in science. Consider Newton's confirmation (in Book III, Proposition IV of the *Principia*) that the downward force on falling bodies near the Earth's surface is the same force as produces the centripetal force keeping the Moon orbiting the Earth. Newton's evidence (the "Moon test") is roughly that the strength of the centripetal force on the Moon, "brought down" to the Earth's surface in accordance with an inverse-square law, closely matches the strength of the downward force on terrestrial falling bodies. Similarly, consider Maxwell's confirmation (in part VI of "A Dynamical Theory of the Electromagnetic Field") that light is an electromagnetic wave. Maxwell's evidence is that the velocity of electromagnetic waves (according to Maxwell's electromagnetic theory together with experimentally determined values of various electromagnetic constants) turns out to closely match the measured velocity of light. These arguments are like Gauss's in that they involve confirming that two things are the same from an observed close agreement between two quantities ascertained "experimentally" (broadly speaking).

However, there is a key respect in which Gauss's argument differs from Newton's and Maxwell's, making the basis of Gauss's argument more mysterious. Newton's and Maxwell's arguments could both be founded on a presumption of ontological parsimony. Having *two* forces of equal strength is more than we need to account for Newton's evidence; one (inverse-square) force would suffice. Likewise, having distinct luminiferous and electromagnetic aethers (and distinct natural phenomena involving light and electromagnetic waves) is more than we need to account for the evidence that Maxwell cites; a single one would suffice. A privilege granted to ontologically parsimonious hypotheses could thus underwrite Newton's and Maxwell's high confidence in their hypotheses.[11] But Gauss's argument could not be founded on a presumption favoring ontologically parsimonious hypotheses. Such a presumption does nothing to favor Gauss's conjecture that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ hits $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ right on the nose over $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s failing to do so but coming very close. Unlike the existence of a single, unified force (or aether) being more ontologically parsimonious than the existence of two distinct forces (or aethers), having $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ equal $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ is no more ontologically parsimonious than having the two quantities be unequal but very nearby. Whether these two quantities are equal or not, all of the real numbers still exist (in whatever way numbers exist). There are no fewer numbers in one case than in the other. Two forces turning out to be one leaves us positing fewer forces, but $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ turning out to equal $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ does not leave us positing fewer numbers. (At the end of this paper, I will be in a better position to compare Gauss's argument to Newton's and Maxwell's.)

There is another argument for concluding that Gauss's argument cannot be captured as a simple enumerative induction (or case of hypothetico-deductive reasoning). Gauss computed a specific arithmetic-geometric mean $\mathrm{M}(a,b)$: $\mathrm{M}(\sqrt{2},1)$. Furthermore, he equated its inverse with a specific integral $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ multiplied by a specific quantity $\frac{2}{\pi}$. Why did he investigate those particular values for a and b? Why did he consider that particular integral? Why did he try the factor $\frac{2}{\pi}$ in particular? Surely Gauss did not decide arbitrarily to compute $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ in particular to so many decimal places; he must have had some antecedent reasons (beyond eyeballing that these two quantities are in the same vicinity) to suspect that these two quantities are equal. These reasons for privileging $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ in particular presumably also played a role, after he computed these two quantities to many decimal places, in helping to justify his regarding their agreement over their first 11 decimal places as strong evidence that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ hits $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ right on the nose (rather than equaling some other,

[9] The same argument applies if as *EQ*'s foil we stop using *UN* (which holds that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ is unequal to $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ but falls within some specified interval around $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$) and instead we use that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ is unequal to $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ but equals a given particular value agreeing in its first 11 digits with $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$.

[10] Pólya (1954:II,95-96) portrays Euler as having strongly confirmed that the sum of the inverse-squares of the natural numbers equals $\frac{\pi^2}{6}$ by having found (by numerical calculation) that they agree to the 10^{-6th} decimal place. But again (I maintain) Euler's argument cannot be captured in this way since this way of thinking would equally apply to agreement with $\frac{\pi^2}{6} - 10^{-25}$. Polya says (appealing to a uniform probability distribution) that the "odds" of the sum's agreeing (to this many digits) with $\frac{\pi^2}{6}$ "by chance" is extremely low. But that argument applies equally to the sum's agreeing with $\frac{\pi^2}{6} - 10^{-25}$ "by chance." Explanatory considerations (I will argue) makes such agreement with $\frac{\pi^2}{6}$ much more suspicious than such agreement with an arbitrary number.

[11] Of course, we would still need to understand why Newton and Maxwell were entitled to privilege more ontologically parsimonious hypotheses. Perhaps there is some *a priori* basis for doing so, or perhaps the basis is empirical. Perhaps the basis applies generally or perhaps it is very narrow and differs in connection with different subjects. That the justification for favoring ontological parsimony remains unspecified does not affect my point, which is that a presumption of ontological parsimony (however justified) could not underwrite Gauss's argument.

undistinguished nearby quantity). But there is no role for any such reasons to play in Gauss's argument if that argument is a simple enumerative induction (or case of hypothetico-deductive reasoning). By contrast, they can play important roles (I will now argue) if Gauss's argument operates by IBE.

3. No ordinary integral

Let's now begin describing how the mathematics that Gauss already knew on May 30, 1799 entitled him to regard his new evidence (that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ agree in their first 11 digits) as making his conjecture ($\frac{1}{\mathrm{M}(\sqrt{2},1)}=\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$) highly confirmed. We will then begin to better appreciate why that confirmation cannot be understood as simple induction by enumeration (or as hypothetico-deductive reasoning) and should be understood as IBE.

Gauss recognized a strong analogy between $2\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ and π. Gauss and his contemporaries knew well that $\pi = 2\int_0^1 \frac{dz}{\sqrt{1-z^2}}$ —the same integral as Gauss's except with z squared rather than to the fourth power. Gauss's own personal notation emphasized this analogy: he represented $2\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ by a varpi (ϖ), a variant of the letter π (there is no phonetic difference between them) that was developed in post-classical scribal cursive. ("Varpi" is shorthand for "VARiant PI.")[12] In fact, according to Cox (1984:316), many eighteenth-century math texts used the varpi to denote the quantity that we represent by π. Thus to Gauss:

$$\varpi = 2\int_0^1 \frac{dz}{\sqrt{1-z^4}}$$

$$\pi = 2\int_0^1 \frac{dz}{\sqrt{1-z^2}}$$

Gauss knew that the analogy between ϖ and π goes far beyond the similarity between these two equations. Since π is standardly defined as the ratio of a circle's circumference to its diameter, the circumference of the unit circle (i.e., circle of radius 1) is $2\pi = 4\int_0^1 \frac{dz}{\sqrt{1-z^2}}$. If centered at the origin, this circle is given in Cartesian coordinates by $x^2+y^2=1$ and in polar coordinates by $r=1$. The π-ϖ analogy is strengthened by the existence of a ϖ-analogue to the unit circle: a lemniscate, which is a ∞-shaped curve first described by Jacob Bernoulli in papers from 1691 to 1694 that Gauss is known to have read (Cox, 1984, pp. 281,310,313, 316). The particular lemniscate, centered at the origin and having the x-axis as its major axis, that is analogous to the unit circle (that is, the simplest lemniscate—often called "Bernoulli's lemniscate") is given in Cartesian coordinates by $(x^2+y^2)^2 = (x^2-y^2)$ and in polar coordinates by $r^2 = \cos 2\theta$ for $0 \le \theta \le 2\pi$. Just as $2\ \pi = 4\ \int_0^1 \frac{dz}{\sqrt{1-z^2}}$ gives the circumference of the unit circle, so likewise $2\ \varpi = 4\ \int_0^1 \frac{dz}{\sqrt{1-z^4}}$ gives the total arc length of Bernoulli's lemniscate.

Gauss knew that the π-ϖ analogy carries even further. The unit circle is, of course, the standard basis for defining the trigonometric functions. For instance, for $0 \le \theta \le \pi/2$, $\sin\theta = b$ iff θ (in radians) is the length of the unit circle's arc from the point (1,0) to the point on the unit circle with b as its y-coordinate. That is (using the above formula for the unit circle's arc length), $\sin\theta = b$ iff $\int_0^b \frac{dz}{\sqrt{1-z^2}} = \theta$. By January 1797, Gauss had extended the π-ϖ analogy by defining analogues to the trigonometric functions where the unit circle's role is played by Bernoulli's lemniscate (Cox, 1984, p. 316). For example, Gauss (1866-1933:III,404) defined the "lemniscatic sine" (which he abbreviated "slθ"): $\mathrm{sl}\theta = b$ iff b is the length of the arc of Bernoulli's lemniscate from the origin to the point on the lemniscate with b as its x-coordinate. That is (using the above formula for the lemniscate's arc length), $\mathrm{sl}\theta = b$ iff $\int_0^b \frac{dz}{\sqrt{1-z^4}} = \theta$.

Thus, just as $\sin 0 = 0$ and $\sin\frac{\pi}{2} = 1$, so likewise $\mathrm{sl}0 = 0$ and $\mathrm{sl}\frac{\varpi}{2} = 1$. Just as $\sin\theta$ is a periodic function with period $2\ \pi$, so likewise $\mathrm{sl}\theta$ is a periodic function with period $2\ \varpi$. Gauss discovered that he could further extend the π-ϖ analogy by defining $\mathrm{cl}\theta$ (the "lemniscatic cosine") by analogy with cos:

$$\cos\theta = \sin\left(\frac{\pi}{2}-\theta\right)$$

$$\mathrm{cl}\theta = \mathrm{sl}\left(\frac{\varpi}{2}-\theta\right)$$

Then (Gauss discovered) not only would $\mathrm{cl}\theta$ share $\mathrm{sl}\theta$'s period just as $\cos\theta$ shares $\sin\theta$'s period, but also a vast number of trigonometric identities would have lemniscatic analogues (Gauss 1866-1933:X.1,147; Cox, 1984, p. 316). For instance:

$$\sin^2\theta + \cos^2\theta = 1$$

$$\mathrm{sl}^2\theta + \mathrm{cl}^2\theta + \mathrm{sl}^2\theta\mathrm{cl}^2\theta = 1$$

$$\sin(\alpha+\beta) = \sin\alpha\cos\beta + \cos\alpha\sin\beta$$

$$\mathrm{sl}(\alpha+\beta) = \frac{\mathrm{sl}\alpha\mathrm{cl}\beta + \mathrm{cl}\alpha\mathrm{sl}\beta}{1-\mathrm{sl}\,\alpha\,\mathrm{sl}\,\beta\,\mathrm{cl}\,\alpha\,\mathrm{cl}\,\beta}$$

In this way, Gauss found that by extending the π-ϖ analogy, "it is possible, by beginning with … Bernoulli's lemniscate …, to define interesting and important functions analogous to the trigonometric functions" (Markushevich, 1966:ix).

We have seen so far that by the time Gauss discovered that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ agree in their first 11 digits, he was already justified in regarding $2\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ as no ordinary integral, but rather as analogous in many ways to π. Therefore, Gauss had reason to treat his conjecture that $\frac{1}{\mathrm{M}(\sqrt{2},1)}=\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ (i.e., that $\frac{1}{\mathrm{M}(\sqrt{2},1)}=\frac{\varpi}{\pi}$) very differently from a hypothesis that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ equals some *other* quantity agreeing with $\frac{\varpi}{\pi}$ in its first 11 digits. In what respect differently? Let's now see why Gauss's recognition of the π-ϖ analogy underwrote Gauss's having a prior probability distribution *favoring* $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s landing on $\frac{\varpi}{\pi}$ over its landing on nearby quantities, rather than having a prior probability for $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ distributed uniformly over these quantities (as discussed in the previous section).

The rationale for favoring $\frac{\varpi}{\pi}$ is not as simple as the fact that $\frac{\varpi}{\pi}$ is a ratio of analogous quantities. (Why should that characteristic favor $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s landing on $\frac{\varpi}{\pi}$?) Part of Gauss's rationale for favoring $\frac{\varpi}{\pi}$ is that π was well known to Gauss and his contemporaries as turning up in all sorts of mathematical places, not solely in problems that transparently concern circles (or other simple geometric figures). As the mathematician Tim Gowers (2008:261) writes: "When π appears in a calculation, one is not unduly surprised." Among the diverse places Gowers (2008:262) cites where π makes an appearance are these:

$$\pi = \sqrt{6\left(\sum_{n=1}^{\infty}\frac{1}{n^2}\right)} = \sqrt{6\left(1+\frac{1}{4}+\frac{1}{9}+\ldots\right)}$$

$$\pi = 4\left(1-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\frac{1}{9}-\ldots\right)$$

$$\pi = \text{the smallest positive number such that } x-\frac{x^3}{3!}+\frac{x^5}{5!}-\ldots = 0$$

[12] Thanks to Theodore Graham for information on the varpi.

$$\pi = \frac{1}{2}\left(\int_{-\infty}^{\infty} e^{-x^2/2}dx\right)^2$$

And there are plenty more, such as these:

$$\pi = \sqrt[4]{90\left(1+\frac{1}{2^4}+\frac{1}{3^4}+\frac{1}{4^4}+\ldots\right)}$$

$$\pi = 2\left(\frac{2\cdot2\cdot4\cdot4\cdot6\cdot6\cdot8\cdot\ldots}{1\cdot3\cdot3\cdot5\cdot5\cdot7\cdot7\cdot\ldots}\right)$$

$$\pi = 3+4\left(\frac{1}{2\cdot3\cdot4}-\frac{1}{4\cdot5\cdot6}+\frac{1}{6\cdot7\cdot8}-\ldots\right)$$

$$\pi = 16\left(\frac{1}{5}-\frac{1}{3\cdot5^3}+\frac{1}{5\cdot5^5}-\frac{1}{7\cdot5^7}+\ldots\right) - 4\left(\frac{1}{239}-\frac{1}{3\cdot239^3}+\frac{1}{5\cdot239^5}-\ldots\right)$$

Gowers emphasizes that when π turns up in a new place, mathematicians naturally ask *why* it arises there—and that there exists a reason why, an explanation. For example, regarding Euler's discovery that $\pi = \sqrt{6\left(\sum_{n=1}^{\infty}\frac{1}{n^2}\right)}$, Gowers (2008:261) writes:

> What on earth, one might wonder, has π to do with adding up reciprocals of squares? This is a perfectly legitimate question, but the idea that there could in principle be a connection is not, to an experienced mathematician, a surprise.

It is no coincidence when π turns up again in some new place; as Gowers says, there is some "connection" to be revealed between that new place and π that explains why π turns up there. For example, Gowers says, there is indeed a reason why the ratio of the circle's circumference to its diameter figures in the formula for the sum of the reciprocals of squares. Gowers gives a derivation of the formula by using Fourier analysis; π enters this derivation by appearing in the formula for the nth Fourier coefficient a_n. Concerning that derivation, Gowers (2008:262) says:

> Now we have a reason for the appearance of π [in the formula for the sum of the reciprocals of squares]: it comes up in the formula for the Fourier coefficients. What is more, its appearance there can be explained as well. A periodic function on $\mathbb{R}$ is more naturally thought of as a function defined on the unit circle. The Fourier coefficient is a certain average defined on the unit circle, so we have to divide by the length of the circle, which is 2π.[13]

There are many ways to prove that $\sum_{n=1}^{\infty}\frac{1}{n^2} = \frac{\pi^2}{6}$ (Chapman (2003) gives 14 ways.). But a proof that *explains why* π figures in this sum cannot generate $\frac{\pi^2}{6}$ by brute force or by some algebraic trick or by π's appearing unexpectedly in the final step as various terms magically cancel out and the remaining ones just happen to combine so as to yield an expression for π. Rather, a proof that (not only proves that but also) explains why π appears in this sum must reveal (what Gowers called above) a "connection" to π lying within $\sum_{n=1}^{\infty}\frac{1}{n^2}$. That is, an explanatory proof of the formula must first find a circle buried somewhere within $\sum_{n=1}^{\infty}\frac{1}{n^2}$ and then use that circle to solve for the sum. In other words, a proof explaining why π figures in this sum must begin by expressing $\sum_{n=1}^{\infty}\frac{1}{n^2}$ in terms of a circle and then must exploit that expression to arrive at the total. That is what Wästlund (2010) does; when his proof was presented in a *YouTube* animation (Hambrecht, 2018), that video was aptly entitled "Why is π here?" (i.e., in the sum of the reciprocals of squares). Such a proof explains π's presence in the solution by tracing it back to π's hidden presence in the original problem being solved.[14]

I am emphasizing not merely that π turns up repeatedly in new mathematical places, but also that it turns up in each new place *non-coincidentally* (i.e., there is an *explanatory* proof of its turning up there). This is the key feature distinguishing π (and, by analogy, ϖ) from other numbers in its vicinity. Ultimately (I will argue), Gauss's background knowledge of this feature is responsible for making his evidence (that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ agrees with $\frac{\varpi}{\pi}$ to 11 decimal places) strongly confirm his conjecture (by strongly confirming that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ agrees with $\frac{\varpi}{\pi}$ in, say, their 12th decimal place).

Obviously, each new place where π turns up also provides a new place where there turns up some number agreeing with π in every digit except (say) its 12th digit. For example:

$\pi = 3.141\ 592\ 653\ 589\ 793\ 238\ 462\ 643\ 383\ldots$
Let $\rho = 3.141\ 592\ 653\ 583\ 793\ 238\ 462\ 643\ 383\ldots$

So since $\pi = \sqrt{6\left(1+\frac{1}{4}+\frac{1}{9}+\ldots\right)}$, it follows that $\rho = \sqrt{6\left(1+\frac{1}{4}+\frac{1}{9}+\ldots\right)} - 0.000\ 000\ 000\ 006$; they both turn up in connection with the sum of reciprocals of squares. So ρ turns up in exactly as many mathematical places, having exactly as much diversity, as π does. However, π and ρ differ in the *reasons why* they turn up where they do. When ρ turns up in these new places, it does so coincidentally: only because π turns up nearby. By contrast, when π turns up in some new place, there is a proof *explaining why* it does—roughly, a proof tracing its appearance in that place back to some circle buried within whatever formula is being evaluated. For ρ's turning up, there are no such explanatory proofs—only proofs that piggyback on proofs of π's turning up.[15]

Gauss knew all of π's behavioral tendencies mentioned above. Gauss's recognition that ϖ is analogous to π entitled Gauss to have considerable confidence that ϖ, like π, will turn up non-coincidentally in many mathematical places. We will see how this confidence helped to underwrite Gauss's having had a prior probability distribution favoring $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s landing on $\frac{\varpi}{\pi}$ over $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s landing on other, nearby quantities. An additional contribution to Gauss's justification for this distribution came from Gauss's having already found that the combination $\frac{\varpi}{\pi}$, in particular, turns up in many unexpected places. For example, Gauss (1866-1933:X.1,169; Cox, 1984, p. 319) had proved in July 1798 that

$$\frac{\varpi}{\pi} = \frac{\sqrt{2}}{2}\left(1+\left(\frac{1}{2}\right)^2\frac{1}{2}+\left(\frac{3}{8}\right)^2\frac{1}{4}+\left(\frac{5}{16}\right)^2\frac{1}{8}+\ldots\right)$$

As another example, Gauss (1866-1933:III,418; X.1,535; Cox, 1984, p. 320) had proved in October 1798 that

$$\sqrt{\varpi/\pi} = 1 - 2e^{-\pi} + 2e^{-4\pi} - 2e^{-9\pi} + \ldots$$

[13] Gowers (2008:262) also says that there is an explanation for π's turning up in $1 = \sqrt{\frac{1}{2\pi}}\int_{-\infty}^{\infty} e^{-x^2/2}dx$: This formula "tells us what constant to put in front of $e^{-x^2/2}$ to make it into the famous normal distribution. (Why should π come into it? … Another fundamental property of $e^{-x^2/2}$ is that the function $f(x,y) = e^{-(x^2+y^2)/2}$ is *rotationally invariant*, and rotations involve circles, which involve π.)".

[14] Lange (2016) argues at length that many explanatory proofs in mathematics derive their explanatory power in this way.

[15] For our purposes, it will make no difference whether we characterize ρ's turning up in a given mathematical place as having no explanation (as I generally will put it) or instead as having an explanation that piggybacks on π's turning up (namely, the reason is that ρ is near π and the reason why π turns up here is …). Under either characterization, ρ and π differ sharply in the reason why they turn up as they do. I will aim to identify this difference.

So by December 1799, Gauss already knew that $\frac{\varpi}{\pi}$ really gets around.

Thus, Gauss's recognition that ϖ is analogous to π entitled Gauss to have considerable confidence that ϖ, like π, will turn up in many, diverse mathematical places—and Gauss was also entitled to have considerable confidence that $\frac{\varpi}{\pi}$, in particular, will do so. Crucially, the analogy to π likewise entitled Gauss to have considerable confidence that ϖ's turning up in those various places will (like π's turning up) be no coincidence. Rather, there will be reasons why ϖ turns up where it does. In this key respect, ϖ differs from the other numbers in its vicinity. From Gauss's recognition of this explanatory difference, it will follow that Gauss's discovery that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ agree in their first 11 digits entitled Gauss (by IBE) to high confidence that they agree in their 12th digit.

As I emphasized in the previous section, agreeing with $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ in the first 11 digits fails to distinguish $\frac{\varpi}{\pi}$ from infinitely many other numbers in its vicinity. But Gauss was entitled to expect, based on the π-ϖ analogy, that $\frac{\varpi}{\pi}$ is distinguished from all of those other numbers in that only for $\frac{\varpi}{\pi}$ is there a *reason why* it agrees in its first 11 digits with $\frac{1}{\mathrm{M}(\sqrt{2},1)}$—that is, a proof of this agreement exploiting a "connection" between them rather than merely what Gauss had on May 30, 1799: a calculation of $\frac{\varpi}{\pi}$ and a separate calculation of $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ revealing them to agree in their first 11 digits. That is, Gauss was entitled to believe that if the value of some expression is unequal to $\frac{\varpi}{\pi}$ but (like $\frac{\varpi}{\pi}$) agrees with $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ to 11 digits, then the agreement will turn out to be a coincidence. If Gauss knew already that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and some unspecified other expression's value agree in their first 11 digits (known also to be the first 11 digits of $\frac{\varpi}{\pi}$), then if Gauss were to discover that this agreement is no coincidence, then that discovery would strongly confirm (indeed, would justify Gauss's having high confidence) that the other expression equals $\frac{\varpi}{\pi}$ rather than some other value agreeing with $\frac{\varpi}{\pi}$ in its first 11 digits. Gauss was entitled to believe that if $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ equals some quantity near $\frac{\varpi}{\pi}$ *coincidentally*, then that quantity might just as well be any of the numbers near $\frac{\varpi}{\pi}$. But Gauss was entitled to priors according to which $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ is probably equal to $\frac{\varpi}{\pi}$ if $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ equals some quantity near $\frac{\varpi}{\pi}$ *non-coincidentally*.

All of Gauss's pieces of antecedent knowledge that I have just described (e.g., of π's behavioral tendencies, of the π-ϖ analogy) were important to making his May 1799 discovery that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ agree in their first 11 digits confirm so strongly that $\frac{1}{\mathrm{M}(\sqrt{2},1)}=\frac{\varpi}{\pi}$. None of that antecedent knowledge was knowledge of the confirmed conjecture's instances or its other consequences. So the role played by this knowledge in the conjecture's confirmation cannot be understood in terms of induction by enumeration or hypothetico-deductive reasoning. As we have just seen (and will see further), the confirmatory relevance of this knowledge can be understood in explanatory terms—that is, in terms of IBE.

As I just mentioned, Gauss was entitled to priors according to which $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ is probably equal to $\frac{\varpi}{\pi}$ if $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ equals some quantity near $\frac{\varpi}{\pi}$ *non-coincidentally*. In section 4, I will describe some of Gauss's reasons for his high confidence that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s equaling some quantity near $\frac{\varpi}{\pi}$ (its falling within one hundred-billionth of $\frac{\varpi}{\pi}$) is too suspicious to be a coincidence—i.e., that it has an explanatory proof. In section 5, I will show that all of these prior expectations regarding explanation entitled Gauss to regard his May 1799 discovery as strongly confirming his conjecture.

4. What is so suspicious about the $\sqrt{2}$,1 pair?

A question that we have not yet addressed is whether Gauss had a good reason in May 1799 for computing the arithmetic-geometric mean of $\sqrt{2}$ and 1 in particular (rather than of some other pair of numbers) or whether he simply got lucky. The answer (I will now argue) is that for $\sqrt{2}$ and 1, unlike for nearly any other numerical pair, Gauss was entitled to think that if certain expressions involving them turned out to approximate certain expressions involving ϖ, then that agreement would be too suspicious to be plausibly coincidental. Rather, the agreement would probably have an explanation—that is, a proof tracing the agreement back to the $\sqrt{2}$,1 pair being buried within ϖ.

This opinion on Gauss's part was justified by Gauss's knowing already in May 1799 that the $\sqrt{2}$,1 pair is unique in being buried within ϖ. So for this pair also to appear in another, seemingly unrelated expression (such as $\frac{1}{\mathrm{M}(\sqrt{2},1)}$) that has a value closely approximating the value of some expression involving ϖ would tend to suggest strongly that there exists a "connection" (as we saw Gowers put it) between the two expressions—in other words, that there is an explanatory proof of their close approximation rather than (for instance) merely proofs that evaluate each of the two expressions separately, showing that they end up nearby. These justified explanatory expectations for $\mathrm{M}(\sqrt{2},1)$—by contrast with $\mathrm{M}(a,b)$ for other a,b pairs—were (I believe) Gauss's reason for taking the trouble to compute the result of plugging $\sqrt{2}$ and 1 in particular into the algebraic-geometric mean iteration.

What did Gauss already know in May 1799 about the $\sqrt{2}$,1 pair's being uniquely hidden within ϖ? Consider again Bernoulli's lemniscate, which (as we saw in the previous section) Gauss already knew to have an arc length of 2 ϖ. A lemniscate can be defined (see Fig. 1) as the locus of points where any point's distance to one of the lemniscate's two "foci" multiplied by its distance to the other focus is constant. (The lemniscate is thus the brother of the ellipse: the locus of points where any point's distance to one of the ellipse's two foci *added to* its distance to the other focus is constant.) For Bernoulli's lemniscate $\left(x^2+y^2\right)^2=\left(x^2-y^2\right)$, the two foci's coordinates are $\left(-\frac{1}{\sqrt{2}},0\right)$ and $\left(+\frac{1}{\sqrt{2}},0\right)$. Thus, Gauss knew before May 1799 that the $\sqrt{2}$,1 pair is uniquely buried within ϖ.

Gauss already knew another unique relation between the $\sqrt{2}$,1 pair and ϖ that helps to make the 11-digit agreement between $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ suspicious (i.e., helps to make plausible that this agreement has an explanatory proof). In the 1730 work that Gauss read where Stirling computed $\int_0^1\frac{dz}{\sqrt{1-z^4}}$ $\left(=\frac{\varpi}{2}\right)$ to 18 decimal places, Stirling (1749:51) also proved that this integral, added to $\int_0^1\frac{z^2dz}{\sqrt{1-z^4}}$, equals half of the circumference of an ellipse having major and minor axes of lengths (wait for it!) $\sqrt{2}$ and 1, respectively (Cox, 1984, p. 312). In a work appearing in 1786 that Gauss is also known to have read (Cox, 1984, p. 313), Euler (1912-3) proved another relation between these two integrals:

$$\left(\int_0^1\frac{dz}{\sqrt{1-z^4}}\right)\left(\int_0^1\frac{z^2dz}{\sqrt{1-z^4}}\right)=\frac{\pi}{4}$$

(Another place where π turns up!) So (combining these two equations and introducing ϖ) Gauss already knew that $\varpi+\frac{\pi}{\varpi}$ equals the circumference of an ellipse having axes of lengths $\sqrt{2}$ and 1. This is another of Gauss's pieces of circumstantial evidence that the 11-digit agreement he discovered in 1799 between an expression involving the $\sqrt{2}$,1 pair $\left(\frac{1}{\mathrm{M}(\sqrt{2},1)}\right)$ and an expression involving ϖ (and π) is unlikely to be coincidental. Again, this evidence's confirmatory relevance to Gauss's conjecture (that $\frac{1}{\mathrm{M}(\sqrt{2},1)}=\frac{\varpi}{\pi}$) can be captured in explanatory terms but not in terms of enumerative induction or hypothetico-deductivism.

The ellipse result's confirmatory relevance to Gauss's conjecture is

enhanced by a relation (that Gauss certainly knew) between M(a,b) and an ellipse with a and b as its semiaxes. It is an elementary result that for $x = \frac{\sqrt{a^2-b^2}}{a}$, M(1 + x,1 − x) = $\frac{M(a,b)}{a}$.[16] This result forges a relation between M(a,b) and the ellipse since x equals the eccentricity of an ellipse with a and b as its semiaxes. So before 1799, Gauss knew that the arithmetic-geometric mean is related to the ellipse, which in turn (when its axes are $\sqrt{2}$ and 1) is related to ϖ and π. In the context of these relations, Gauss was entitled to regard his discovery in May 1799 as strongly suggestive of the existence of an explanatory proof.

5. A Bayesian reconstruction of Gauss's reasoning as IBE

I have argued that Gauss's May 1799 discovery of the 11-digit agreement strongly confirmed his conjecture only in the context of his prior mathematical knowledge, most importantly his knowledge of the π-ϖ analogy and the $\sqrt{2}$,1 pair's relations to lemniscates and ellipses. I have argued that this antecedent knowledge's confirmatory relevance to Gauss's conjecture (and, hence, the confirmatory power of Gauss's May 1799 discovery) can be understood in explanatory terms, but not in terms of enumerative induction or hypothetico-deductivist reasoning. In particular, this antecedent knowledge bore upon the likelihood that Gauss's May 1799 discovery has an explanatory proof.

In studies of confirmation in science, enumerative induction and hypothetico-deductivism have been succeeded by Bayesian confirmation theory. In this section, I will show that the account of Gauss's reasoning developed in sections 3 and 4 can be expressed in Bayesian terms. In the course of doing so, I will argue that Gauss's reasoning can also be well understood as IBE.

Here is one way to use the results of sections 3 and 4 to reconstruct Gauss's non-deductive argument for high confidence in his conjecture. In particular, let's use Bayesian terms to show how Gauss was entitled to high confidence in his conjecture c (that $\frac{1}{M(\sqrt{2},1)} = \frac{\varpi}{\pi}$) in light of his evidence e (that $\frac{1}{M(\sqrt{2},1)}$ agrees with $\frac{\varpi}{\pi}$ to 11 decimal places) because of explanatory considerations:

1. In section 4, we saw that by the time that Gauss discovered e, Gauss already knew various unique relations between the $\sqrt{2}$,1 pair and ϖ. This antecedent knowledge made e (the 11-digit agreement between $\frac{1}{M(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$) highly suspicious— i.e., made it highly likely that the relation between $\frac{1}{M(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ (whether it turns out to be equality or merely close approximation) is no coincidence, but rather has an explanatory proof. Letting f be that the relation between $\frac{1}{M(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ is no coincidence, we can express Gauss's background opinion as that his pr(f|e) was high.
2. In section 3, we saw that by the time that Gauss discovered e, he already knew the π-ϖ analogy. This antecedent knowledge entitled Gauss to priors according to which $\frac{1}{M(\sqrt{2},1)}$ is probably equal to $\frac{\varpi}{\pi}$ if $\frac{1}{M(\sqrt{2},1)}$ equals some quantity near $\frac{\varpi}{\pi}$ non-coincidentally. In other words, Gauss was entitled to the background opinion that pr(c|e&f) was high.
3. Since pr(e) ≠ 0, pr(f|e) = pr(e&f)/pr(e). Since pr(e&f) ≠ 0,
   pr(c|e&f) = pr(c&e&f)/pr(e&f).
   So pr(f|e) pr(c|e&f) = [pr(e&f) pr(c&e&f)]/[pr(e) pr(e&f)] = pr(c&e&f)/pr(e)
   ≤ pr(c&e)/pr(e) = pr(c|e).
   Hence, pr(f|e) pr(c|e&f) ≤ pr(c|e).

So we have a floor for pr(c|e). Since (from steps 1 and 2 above) Gauss's pr(f|e) and pr(c|e&f) were each high, Gauss's pr(c|e) was high (though presumably not quite as high as either pr(f|e) or pr(c|e&f)).

Thus, Gauss's background knowledge entitled him to have already assigned a high value to pr(c|e) by the time he discovered e. By Bayesian updating, then, his discovery that e justified his having high confidence that c.

This Bayesian reasoning can be captured nicely as IBE. Lipton (2001:97; 2004:63) aptly characterizes IBE as operating when we use our judgment of a potential explanation's "loveliness" (that is, our judgment of the quality that it would have as an explanation) as a guide to judging its likeliness. Gauss's background knowledge justly influenced what he judged would be the loveliest kind of explanation of the 11-digit agreement he observed—namely, a proof of $\frac{1}{M(\sqrt{2},1)} = \frac{\varpi}{\pi}$ that works by first revealing some sort of "connection" between ϖ and M$(\sqrt{2},1)$ and then exploiting that connection to yield the conjectured equality. By IBE (i.e., by taking loveliness as a guide to likeliness), Gauss could conclude that the existence of such a proof and perforce the conjecture's truth is likely. Such a proof would be a lovelier explanation than (for example) a proof that first evaluated $\frac{1}{M(\sqrt{2},1)}$, then entirely separately evaluated $\frac{\varpi}{\pi}$, and finally found these two quantities to be equal—or to be nearly but not quite equal. Another less lovely explanation of Gauss's evidence would be a proof that, while not treating $\frac{1}{M(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ entirely separately, brought them together only by the grace of several separate cancellations, each arriving "out of the blue" (i.e., "fortuitously", as mathematicians say).

As we saw, by the time Gauss discovered the 11-digit agreement, he already knew various ways that the $\sqrt{2}$,1 pair is connected to ϖ. Therefore, Gauss justly regarded a potentially explanatory proof of the 11-digit agreement between $\frac{1}{M(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ as explaining it better insofar as the proof works by exploiting the $\sqrt{2}$,1 pair as buried somehow within the integral defining ϖ. Likewise, we saw that by the time Gauss discovered the 11-digit agreement, he already knew various respects in which π differs from other numbers nearby— respects having to do with the existence of reasons why π turns up in so many and such diverse mathematical places. In addition, Gauss had already discovered various respects in which ϖ is analogous to π. Therefore, Gauss justly believed that an explanatory proof of the 11-digit agreement between $\frac{1}{M(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ would be enhanced in its explanatory quality (i.e., "loveliness") by proving that $\frac{1}{M(\sqrt{2},1)}$ hits $\frac{\varpi}{\pi}$ on the nose rather than that $\frac{1}{M(\sqrt{2},1)}$ is near but unequal to $\frac{\varpi}{\pi}$. In this way, Gauss's expectations regarding the explanation of the 11-digit agreement he observed can be understood as guided by loveliness (i.e., by what would make for the best explanation) and so as operating by IBE.

The above reconstruction of Gauss's reasoning concerned e's highly confirming c in the "absolute" sense—that is, e's discovery justly leaving Gauss highly confident that c (i.e., Gauss's justly having high pr(c|e)). But to fully appreciate e's confirmatory relevance to c, we must also look at e's confirming c in the "incremental" sense—that is, at e's discovery justly raising Gauss's confidence that c (i.e., Gauss's justly having pr(c|e) > pr(c)).[17] Let's now reconstruct this incremental confirmation in Bayesian

[16] This result follows from two lemmas:
(i) Since the arithmetic-geometric mean is the common limit of the sequences $\{a_n\}$ and $\{b_n\}$, M(a,b) [= M(a_0,b_0)] is equal to M(a_1,b_1) [= M($\frac{a+b}{2}$, $\sqrt{ab}$)].
(ii) For any positive λ, M(λ a, λ b) = λ M(a,b).
To prove the result, let $a_0 = 1 + x$ and $b_0 = 1 - x$. Then $a_1 = 1$, $b_1 = \sqrt{a_0 b_0} = \sqrt{1-x^2} = \sqrt{1-\frac{a^2-b^2}{a^2}} = \sqrt{1-1+\frac{b^2}{a^2}} = \frac{b}{a}$. So M(1 + x,1 − x) equals (by (i)) M(1, $\frac{b}{a}$), which (by $1 = \frac{a}{a}$ and (ii)) equals $\frac{1}{a}$M(a, b).

[17] This distinction between "incremental" and "absolute" confirmation (and this terminology) are familiar from Salmon (1975).

terms and see how explanatory considerations enter into it.

The (incremental) confirmation of Gauss's conjecture c (that $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{\varpi}{\pi}$) by Gauss's evidence e (that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ agrees with $\frac{\varpi}{\pi}$ to 11 decimal places) might appear to be easily expressible in Bayesian terms. (Easily, that is, if all of these probabilities are non-zero—which raises the issues from section 2, to which I will shortly return.) Since c entails e, $\mathrm{pr}(e|c) = 1$, so by Bayes's theorem, $\mathrm{pr}(c|e) = \mathrm{pr}(c)\ \mathrm{pr}(e|c)/\mathrm{pr}(e) = \mathrm{pr}(c)/\mathrm{pr}(e) > \mathrm{pr}(c)$. But this is too easy. Firstly, it fails to account for the confirmation of c's *predictions* of agreement between $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ in the decimal places after the 11 covered by e; such a prediction (unlike c itself) does not entail e. For instance, let u (for "unexamined") be that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ agrees with $\frac{\varpi}{\pi}$ in the 12th decimal place (the first decimal place that was not computed by Gauss). Then since u does not entail e, the above Bayesian approach does not apply. Secondly, the above Bayesian approach would work just as well were c replaced by c': that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ is equal to a given arbitrary number n that agrees with $\frac{\varpi}{\pi}$ to 11 decimal places while differing in the 12th. Gauss's reasoning must somehow favor c over c'. This is essentially the same problem as we saw in section 2: hypothetico-deductivism cannot account for Gauss's antecedent reason for privileging $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s hitting $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ right on the nose over $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s failing to do so but coming within one hundred-billionth of doing so.

These two problems are closely related, since the difference between c and c' lies in their predictions regarding the decimal places after the 11 covered by e. I will now reconstruct Gauss's incremental confirmation of u, thereby reconstructing confirmation that discriminates between c and other conjectures that agree with c on $\frac{1}{\mathrm{M}(\sqrt{2},1)}$'s first 11 digits but disagree on the 12th.

I have argued that Gauss's reasons for discriminating between c and c' involve explanatory considerations. Of course, Bayesian conditionalization makes no explicit reference to explanation. But a *hypothesis* being confirmed *can* expressly concern explanation; it can concern the sort of explanation that a given discovery would have.[18] I will now show how the explanatory considerations that underwrote Gauss's confirmation of his conjecture (as we saw in the previous sections) can be captured by a Bayesian account that includes the confirmation of a hypothesis positing the existence of an explanatory proof.

Let's continue taking e to be the observed 11-digit agreement between $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{\varpi}{\pi}$ and u to be that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ agrees with $\frac{\varpi}{\pi}$ in their 12th decimal place (the first unexamined decimal place). Suppose $0 < \mathrm{pr}(e)$, $\mathrm{pr}(u) < 1$. Let h be a hypothesis about explanation: that either there is a proof that explains why $e\&u$ holds or there is a proof that explains why $e\&{\sim}u$ holds. Let's see how the Bayesian apparatus can capture the role played by Gauss's expectations about the (non)existence of certain explanations (ultimately based on his prior mathematical knowledge) in underwriting his taking e as incrementally confirming u. That is, let's see how the Bayesian apparatus can capture the roles played in e's incremental confirmation of u by Gauss's judgments of the "loveliness" of various sorts of potential explanations—judgments that guided Gauss's judgments of the likeliness that there exist such explanations (under IBE, according to which loveliness is a guide to likeliness).

Use the theorem of total probability to compare $\mathrm{pr}(u|e)$ to $\mathrm{pr}(u)$:

$$\mathrm{pr}(u|e) = \mathrm{pr}(u|e\&h)\ \mathrm{pr}(h|e) + \mathrm{pr}(u|e\&{\sim}h)\ \mathrm{pr}({\sim}h|e)$$

$$\mathrm{pr}(u) = \mathrm{pr}(u|h)\ \mathrm{pr}(h) + \mathrm{pr}(u|{\sim}h)\ \mathrm{pr}({\sim}h)$$

The right side of each of these equations is a weighted average of two $\mathrm{pr}(u)$ terms; the weights are the $\mathrm{pr}(h)$ and $\mathrm{pr}({\sim}h)$ terms. Let's get a grip on these various terms.

Gauss had some nonzero prior confidence $\mathrm{pr}(h)$ that e and the fact (whatever it is) about whether or not u is true have a common explanation. Plausibly, for Gauss any confirmatory relevance of e to u (or to ${\sim}u$) was entirely by virtue of the prospect that e and u (or ${\sim}u$) have a proof that explains why their conjunction holds, making it no coincidence. That is, the only route for Gauss by which e could bear confirmationwise on u is by bearing on h; there are no other, potentially interfering routes. Therefore, if Gauss had discovered that ${\sim}h$, then this route to u's confirmation via h's confirmation would have been blocked and so e would have failed to bear confirmationwise on u. Thus ${\sim}h$ screens off u from e: $\mathrm{pr}(u|e\ \&\ {\sim}h) = \mathrm{pr}(u|{\sim}h)$.[19] We can plug this equality into the two equations above.

Since h entails e, $\mathrm{pr}(e|h) = 1$, so by Bayes's theorem, $\mathrm{pr}(h|e) = \mathrm{pr}(h)\ \mathrm{pr}(e|h)/\mathrm{pr}(e) = \mathrm{pr}(h)/\mathrm{pr}(e) > \mathrm{pr}(h)$. With e confirming h, obviously e disconfirms ${\sim}h$: $\mathrm{pr}({\sim}h|e) < \mathrm{pr}({\sim}h)$. Furthermore, since h entails e, $\mathrm{pr}(u|e\&h) = \mathrm{pr}(u|h)$. Plug this all into the two equations above.

Thus, each of the $\mathrm{pr}(u)$ terms being averaged in one of the above equations is equal to its corresponding $\mathrm{pr}(u)$ term in the other equation. The only difference between the equations lies in the weights given to these terms in the average:

$$\begin{array}{ccccccccc} \mathrm{pr}(u|e) & = & \mathrm{pr}(u|e\&h) & & \mathrm{pr}(h|e) & + & \mathrm{pr}(u|e\&{\sim}h) & & \mathrm{pr}({\sim}h|e) \\ & & \| & & \vee & & \| & & \wedge \\ \mathrm{pr}(u) & = & \mathrm{pr}(u|h) & & \mathrm{pr}(h) & + & \mathrm{pr}(u|{\sim}h) & & \mathrm{pr}({\sim}h) \end{array}$$

Therefore, $\mathrm{pr}(u|e) > \mathrm{pr}(u)$ exactly when e's discovery increases the weight placed on the *greater* of the two averaged terms—exactly when $\mathrm{pr}(u|e\&h) > \mathrm{pr}(u|e\&{\sim}h)$. That is precisely what happened with Gauss, as we saw in section 3. There I argued that the confirmatory relevance of the π-ϖ analogy should be understood in explanatory terms. In particular, I argued that Gauss's knowledge of this analogy entitled Gauss to a prior probability distribution with the following feature: Given Gauss's knowledge that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and some unspecified other expression's value agree in their first 11 digits (known also to be the first 11 digits of $\frac{\varpi}{\pi}$), if Gauss were then to discover that this agreement is no coincidence, that discovery would confirm for Gauss that the other expression equals $\frac{\varpi}{\pi}$ rather than some other value agreeing with $\frac{\varpi}{\pi}$ in its first 11 digits. Therefore, $\mathrm{pr}(u|e\&h) > \mathrm{pr}(u|e\&{\sim}h)$—enabling the above reasoning to yield incremental confirmation of u by e.

That is, Gauss judged (in view of his prior mathematical discoveries) that the loveliest sort of explanation that e could have would be a proof that would also prove (and explain) u. Under IBE, loveliness is a guide to likeliness. Under IBE, then, $\mathrm{pr}(u|e\&h) > \mathrm{pr}(u|e\&{\sim}h)$.

Thus, Gauss's antecedent mathematical knowledge, in underwriting expectations about explanations, put Gauss in a position to regard his May 1799 discovery as incrementally confirming that his conjecture holds in unexamined cases.

6. Our Conclusion—and what Gauss concluded

I have argued that Gauss's justified high confidence that $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$, based on his discovery that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ agree in their first 11 decimal digits, depended on some of his antecedent mathematical knowledge. That knowledge consisted principally of the π-ϖ analogy together with the suspicious ways that the $\sqrt{2}$,1 pair is bound up not only with Bernoulli's lemniscate, but also with an

[18] I have argued elsewhere (Lange, 2022) that one way to reconcile Bayesian confirmation theory with IBE is to give a Bayesian treatment of hypotheses that expressly concern the sort of explanation that a given discovery would have.

[19] Although this "only route" (i.e., screening-off) condition seems realistic to Gauss, it could be relaxed and e still confirm u incrementally. However, that confirmation would depend on whether the explanatory route by which e bears confirmationwise on u is reinforced or outweighed by the other routes.

ellipse—two shapes that, in turn, are connected to $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. His antecedent knowledge entitled Gauss to the opinion that if $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ come close, then probably their relation (whatever it turns out to be) is no coincidence, but rather has an explanatory proof—and that if they come close and their relation has an explanatory proof, then they are probably equal. Thus, in May 1799, Gauss was in a position to draw large conclusions, were he to discover that $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ come close.[20] Indeed, this is surely why Gauss bothered in the first place to calculate $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ to so many digits.

In this way, I have argued that Gauss's background knowledge enabled his May 1799 discovery of the 11-digit agreement to confirm his conjecture very strongly. I have argued that his background knowledge (expressly concerning explanations) could play this role only because Gauss's reasoning consisted not of simple enumerative induction, but rather of IBE. His background knowledge suggested strongly that were $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ to come close, then this fact would have an explanation. That explanation would consist of a proof of $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ that works by first revealing that the $\sqrt{2}$,1 pair and the AGM are both buried somehow inside $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ and by then exploiting that "connection" (as Gowers puts it).

That is, such a proof would not consist of (for instance) a brute force calculation of $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ that, after a welter of algebra and several unanticipated cancellations, ultimately becomes $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$. Rather, the loveliest (and hence, by IBE, the likeliest) explanatory proof would begin by expressing $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ in terms of the $\sqrt{2}$,1 pair and the AGM. It would then exploit that expression in evaluating the integral. In this way, the proof would reveal why $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ by uncovering and exploiting the way that the $\sqrt{2}$,1 pair and the AGM were hidden in $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ in the first place.

By December 23, 1799, Gauss had two proofs that $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ (Cox, 1984, pp. 281-3). Both begin by expanding $\frac{1}{\mathrm{M}(1+x,1-x)}$ in even powers of x (that is, as $1 + c_1x^2 + c_2x^4 + c_3x^6 + \ldots$) and both continue by finding ingenious ways to solve for the coefficients c_i.[21] But Gauss evidently wanted more than such a brute force approach could provide. Evidently, he believed (justly, I have argued) that a lovelier explanation existed.

He published a very different proof in 1818 (Gauss 1866-1933:III, 331-55)—in (significantly) the only paper on the AGM that he published in his lifetime (Cox, 1984, p. 281). That proof begins with Bernoulli's 1691 reformulation of $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ as $\int_0^{\pi/2} \frac{d\phi}{\sqrt{2\cos^2\phi + \sin^2\phi}}$.[22] This change-of-variables reveals where the $\sqrt{2}$,1 pair from $\mathrm{M}(\sqrt{2},1)$ was hiding: this integral is the special case of $\int_0^{\pi/2} \frac{d\phi}{\sqrt{a^2\cos^2\phi + b^2\sin^2\phi}}$ for $a = \sqrt{2}$ and $b = 1$. So Gauss's conjecture $\frac{1}{\mathrm{M}(\sqrt{2},1)} = \frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$, i.e., $\mathrm{M}(\sqrt{2},1)\int_0^1 \frac{dz}{\sqrt{1-z^4}} = \frac{\pi}{2}$, is the special case for $a = \sqrt{2}$ and $b = 1$ of the generalization:

$$\text{For any real } a \geq b > 0, \mathrm{M}(a,b) \int_0^{\pi/2} \frac{d\phi}{\sqrt{a^2\cos^2\phi + b^2\sin^2\phi}} = \frac{\pi}{2}.$$

Gauss proved this generalization of his conjecture by first revealing how the AGM is buried inside this integral and then exploiting that connection.

Since the arithmetic-geometric mean is the common limit of the sequences $\{a_n\}$ and $\{b_n\}$, M(*a*,*b*) [i.e., M(a_0,b_0)] is equal to M(a_1,b_1) [i.e., $\mathrm{M}(\frac{a+b}{2}, \sqrt{ab})$]. This constitutes the AGM's "invariance" when a is replaced by $\frac{a+b}{2}$ and b is replaced by $\sqrt{ab}$. Gauss showed that the above integral exhibits the same invariance! That is, Gauss showed that the integral

$$\mathrm{I}(a,b) = \int_0^{\pi/2} \frac{d\phi}{\sqrt{a^2\cos^2\phi + b^2\sin^2\phi}}$$

is invariant (i.e., its value is unchanged) under the change of parameters[23] corresponding to the AGM:

$$(a,b) \to \left(\frac{a+b}{2}, \sqrt{ab}\right).$$

That is (with a_i and b_i as the ith members of the two linked sequences $\{a_n\}$ and $\{b_n\}$, each converging to M(*a*,*b*)),

$$\mathrm{I}(\mathrm{a},\mathrm{b}) = \mathrm{I}\left(\frac{a+b}{2}, \sqrt{ab}\right) = \mathrm{I}(\mathrm{a}_1, \mathrm{b}_1).$$

This invariance is the hidden connection between the integral and the AGM that ultimately explains why Gauss's conjecture holds.

Gauss exploited this connection to prove the above generalization of his conjecture as follows:

$$\mathrm{I}(a,b) = \mathrm{I}(a_1,b_1) = \mathrm{I}(a_2,b_2) = \ldots = \mathrm{I}(\mathrm{M}(a,b), \mathrm{M}(a,b)).$$

So abbreviating M(*a*,*b*) as μ,

$$\int_0^{\pi/2} \frac{d\phi}{\sqrt{a^2\cos^2\phi + b^2\sin^2\phi}} = \mathrm{I}(\mathrm{a},\mathrm{b}) = \mathrm{I}(\mu,\mu) = \int_0^{\pi/2} \frac{d\phi}{\sqrt{\mu^2\cos^2\phi + \mu^2\sin^2\phi}} = \frac{1}{\mu}\int_0^{\pi/2} \frac{d\phi}{\sqrt{\cos^2\phi + \sin^2\phi}}$$

Substituting $\sin^2\phi + \cos^2\phi = 1$, the above integral becomes

$$\frac{1}{\mu}\int_0^{\pi/2} d\phi = \frac{\pi}{2\mu}$$

Thus, $\int_0^{\pi/2} \frac{d\phi}{\sqrt{a^2\cos^2\phi + b^2\sin^2\phi}} = \frac{\pi}{2\mu} = \frac{\pi}{2\mathrm{M}(a,b)}$, i.e., $\mathrm{M}(a,b)\int_0^{\pi/2} \frac{d\phi}{\sqrt{a^2\cos^2\phi + b^2\sin^2\phi}} = \frac{\pi}{2}$, proving the generalization of Gauss's original conjecture. This proof not only proves the conjecture but also shows "where it comes from" in that it shows the source of the integral's connection to the AGM. In this way, Gauss gave an explanatory proof of his conjecture.

In section 2, I argued that Gauss's high confidence in his conjecture differs from Newton's and Maxwell's high degrees of confidence in their hypotheses (about gravity and light, respectively) in that Gauss's confidence could not have been underwritten by a presumption of

[20] In this paper, I do not discuss all of the large conclusions that Gauss was in a position to draw. In particular, I do not discuss his antecedent work on the AGM of complex numbers.

[21] In characterizing these ways as "ingenious" or "not the easiest development" (Almkvist & Berndt, 1988, p. 588; Borwein & Borwein, 1987, pp. 5-6), mathematicians mean that they are a tedious algebraic slog, not explanatory.

[22] Proof: Change variables by letting $z = \cos\phi$, so $z = 0$ corresponds to $\phi = \cos^{-1}0 = \frac{\pi}{2}$ and $z = 1$ corresponds to $\phi = \cos^{-1}1 = 0$. Differentiate $z = \cos\phi$ to yield $dz = -\sin\phi\, d\phi$. Substituting into $\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ yields $\int_{\pi/2}^0 \frac{-\sin\phi\, d\phi}{\sqrt{1-\cos^4\phi}}$. From $\sin^2\phi + \cos^2\phi = 1$, $\sin\phi = \sqrt{1-\cos^2\phi}$. Substituting into the numerator and factoring the denominator yields $\int_{\pi/2}^0 \frac{(-1)d\phi\sqrt{1-\cos^2\phi}}{\sqrt{1-\cos^2\phi}\sqrt{1+\cos^2\phi}} = \int_0^{\pi/2} \frac{d\phi}{\sqrt{1+\cos^2\phi}}$. Replacing 1 with $\sin^2\phi + \cos^2\phi$ yields $\int_0^{\pi/2} \frac{d\phi}{\sqrt{2\cos^2\phi + \sin^2\phi}}$.

[23] This change of parameters is often called "the Landen transformation" for historical reasons given by Almkvist & Berndt, 1988.

ontological parsimony. Nevertheless, these three cases seem alike. In each case, an observed close agreement between two quantities is used to justify high confidence that two things are the same.

Having now understood Gauss's argument as IBE, we have better prospects of understanding all three of these plausibility arguments as operating in the same way. Gauss's reasons for his high confidence involve explanatory considerations underwriting the confirmatory power of the 11-digit agreement he discovered. Gauss's antecedent knowledge entitled him to the opinion that if $\frac{1}{\mathrm{M}(\sqrt{2},1)}$ and $\frac{2}{\pi}\int_0^1 \frac{dz}{\sqrt{1-z^4}}$ come close, then their relation (whatever it is) is too suspicious to be plausibly coincidental, but instead probably has an explanation—and that if they come close and their relation has an explanation, then they are probably equal; that would make for the loveliest explanation. Newton's high confidence that the downward force on falling bodies near the Earth's surface is the same force as produces the centripetal force on the Moon plausibly also rests on the opinion that if the lunar centripetal force (brought down to the Earth's surface in accordance with an inverse-square dependence on distance) comes close to the force experienced by falling terrestrial bodies, then the relation between the strengths of these forces (whatever it turns out to be) is too suspicious to be plausibly coincidental, but instead probably has an explanation—and that if they come close and their relation has an explanation, then the two phenomena are probably caused by the same force. That would make for the loveliest explanation. (Analogous considerations apply to Maxwell's reason for taking the agreement between the speeds of light and electromagnetic waves as strong evidence that light is an electromagnetic wave.)

Thus, by taking Gauss's argument as IBE, we can understand Gauss's reasoning as similar to the reasoning that scientists such as Newton and Maxwell gave in apparently similar cases. A philosophical explanation of why the confirmation in each of these cases is so strong is made lovelier by its unifying these cases rather than having to reconstruct them in dissimilar ways. That is another good reason to interpret Gauss's argument as IBE.

Declaration of competing interests

I have nothing to declare.

Data availability

No data was used for the research described in the article.

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