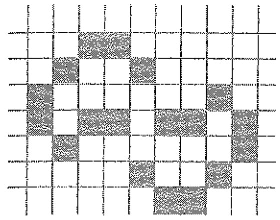
chapter 3

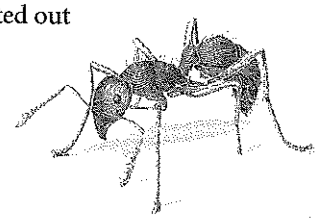
Historic Conjectures: More Sequence Mysteries



Conjectures

Among notable recent achievements in mathematics are the resolutions of some celebrated long-standing conjectures: the proof by Deligne of the Weil conjectures; by de Branges of the Bieberbach conjecture; and by Faltings of the Mordell conjecture. On the other hand, the Fermat problem,* the Riemann hypothesis, and the so-called Poincaré conjecture remain unresolved, though for brief periods it was claimed that they too had been settled. In any case, it seems timely to present some scattered facts about the origins of some of these problems. For most of what follows in this section, aside from the next paragraph, I am indebted to Professor M. R. Choudhury of the University of Dhaka, Bangladesh.

The statement commonly referred to as the Poincaré conjecture is the assertion that the only simply connected compact 3-manifold is the 3-sphere. However, as has been pointed out (see e.g., S. Smale, *Mathematical Intelligencer,* Vol. 12, No. 2, 1990) Poincaré never put this forward as a



*Now no longer a conjecture!

conjecture. He writes [Poincaré, H., Cinquième complément à l'analysis situs, *Œuvres VI*, Gauthier-Villars, Paris (1953), p. 498]:

> Il resterait une question à traiter: Est-il possible que le groupe fondamental de V se réduise à la substitution identique, et que pourtant V ne soit pas simplement connexe?
>
> There remains a question to be treated. Is it possible that the fundamental group of V reduces to the identity even though V is not simply connected?

There follows a paragraph in which the question is rephrased in terms of some of the concepts introduced in the paper, and then there is a final one-line paragraph: "Mais cette question nous entranerait trop loin," which roughly translated says, "But that question would carry us too far afield." Thus the question is presented neither as a conjecture nor as an open problem. Poincaré does not even guess at the answer.

By contrast, Weil is absolutely explicit in believing that his unproved results are true. He writes [Numbers of solutions of equations over finite fields, *Bulletin of the American Mathematical Society* 55(1949), p. 498],

> This, and other examples which we cannot discuss here, seem to lend some support to the following conjectural statements, which are known to be true for curves, but which I have not so far been able to prove for varieties of higher dimension.

As we now know from the work of Deligne, Weil's conjectures turned out to be correct. It is interesting then to read what Weil has written on the subject of conjectures in general [Two lectures on number theory, past and present, *L'Enseignement mathématique* (2), 20 (1974), 87–110]:

> Here I may point out that in the old days, when we used the word "hypothesis" or "conjecture" (in German, *Vermutung*), this was not to be taken as simply a form of wishful thinking. Nowadays the two are often confused. For instance, the so-called "Mordell conjecture" on Diophantine equations says that a curve of genus at least two with rational coefficients has at most finitely many rational points. It would be nice if this were so, and I would rather bet for it than against it. But it is no more than wishful thinking because there is not a shred of evidence for it, and also none against it.

So Poincaré clearly did not make his conjecture, Weil definitely did. We leave it to the reader to decide whether Mordell's statement [On the rational solutions of the indeterminate equations of the third and fourth degrees, *Proceedings Cambridge Philosophical Society* 21 (1922/23), 191–192] is a conjecture or wishful thinking:

> In conclusion, I might note that the preceding work suggests to me the truth of the following statements concerning indeterminate equations, none of which, however, I can prove....

(3) The equation

$$ax^6 + bx^5y + \cdots + fxy^5 + gy^6 = z^2$$

can be satisfied by only a finite number of rational values of x and y, with the obvious extension to equations of higher degree.

(4) The same theorem holds for the equation

$$ax^4 + by^4 + cz^4 + 2fy^2z^2 + 2gz^2x^2 = 2hx^2y^2 = 0.$$

(5) The same theorem holds for any homogeneous equation of genus greater than unity, say $f(x, y, z) = 0$.

More Mysteries

Many mathematicians have observed that the main impact of computers on mathematics has been to raise new problems rather than solve old ones. I will suggest that some of these new problems, though easy to formulate, may in fact be impossible to solve.

Among problems that would probably never have been posed but for the existence of computers, one of the simplest and probably the best known is the so-called $(3n + 1)$ conjecture due to Lothar Collatz. Let f be the function on the natural numbers $\mathbf{N}$ where

$$f(n) = \begin{cases} n/2 & \text{for } n \text{ even,} \\ 3n + 1 & \text{for } n \text{ odd.} \end{cases}$$

The conjecture is that for any n there is some k such that $f^k(n) = 1$, or in the language of dynamical systems, the orbit of every n contains 1. This has been verified for all $n < 10^9$.

A general class of questions of which this is a special case is easily described. For any number k and numbers a_i, b_i in $\mathbf{N}$, $0 \le i < k$, let f from $\mathbf{N}$ to $\mathbf{N}$ be defined by

$$f(kn + i) = a_in + b_i.$$

One can then ask questions about the orbits of points under f. For example, do they all contain the number 1? The Collatz problem corresponds to the case $k = 2$, $a_0 = 1$, $b_0 = 0$, $a_1 = 6$, $b_1 = 4$.

The main result on the general problem is due to John H. Conway [Unpredictable Iterations, *Proceedings Number Theory Conference, Boulder,* 1972] and asserts that it is undecidable. More precisely, Conway shows, even for the case when all the b_i are zero, that there is no algorithm for deciding whether the orbit of a given n contains the number 1. Of course this says nothing about the decidability of the Collatz problem, but it does show that there exist specific problems with numbers k and a_i that could in principle be calculated but for which the problem is undecidable. Further, Conway has come up with some interesting examples, referred to by Richard Guy as permutation sequences. The simplest of these appears in Guy's *Unsolved Problems in Number Theory* [Springer-Verlag, New York, 1981] and is given by the mapping T defined by

$$T(2n) = 3n,$$
$$T(4n + 1) = 3n + 1,$$
$$T(4n - 1) = 3n - 1.$$

T is easily seen to be a bijection, so all orbits are either cycles or bi-infinite sequences that approach infinity in both directions. One finds easily the cycles (1), (2, 3), (4, 6, 9, 7, 5), and (44, 66, 99, 74, 111, 83, 62, 93, 70, 105, 75, 59). The smallest missing number among these is 8, whose orbit starts out 73, 55, 41, 31, 23, 13, 10, 15, 11, *8*, 12, 18, 27, 20, 30, 45, 34, 51, 38, 57, 43, 32, 48, 72, ... The orbit shows no signs of cycling after several thousand iterations in both directions although there are a number of "near misses." Note the 73 and 72 at the beginning and end of the sequence above. This happens again with 153 and 154, 161 and 162, 500 and 501, 790 and 791.

Question 1: Are there any other finite orbits under T?

Question 2: Is the orbit of 8 finite?

A striking feature of this last question is that it concerns a single sequence, as contrasted with the Collatz problem, which asks about the behavior of an infinite number of sequences. Of course it is meaningless to say that this question is undecidable, but later I argue that it may well be "unprovable"; that is to say, it might be that the orbit of 8 is in fact infinite, but that there is no proof of this from our usual system of axioms.

Further experimentation leads to further speculation. The smallest number not in any of the preceding orbits is 14, whose orbit appears to be heading resolutely for infinity at both ends. The next missing number is 40, and so on. Using *Mathematica* we found that all numbers up to 1000 belong to 54 disjoint orbits. We call the smallest number in an orbit the seed s. The elements of $T^n(s)$ are called *forward numbers* for n positive, and *backward numbers* for n negative. Of course, just as we have no proof that these orbits are not parts of cycles, we also don't know that they are disjoint. It is conceivable, for example, that some forward iterate $T^n(8)$ might eventually hit a backward iterate of $T^m(14)$. A crude statistical study indicates that the forward iterates contain roughly the same number of even and odd numbers. A consequence of this is that roughly half of the forward numbers are divisible by 3, since from the definition of T, a number is divisible by 3 if and only if its predecessor is even. Among the backward numbers, on the other hand, the odd numbers seem to outnumber the evens by about 2 to 1. This must clearly be the case, for an even backward number follows a down-jump of 2/3, while an odd backward number follows an up-jump of only 4/3. So there must be many more odds than evens, since the numbers $T^{-n}(s)$ must remain positive.

With the data at hand, one can prove that if there are any cycles other than those listed, they must have length at least 360. Further, one can intuitively argue that the existence of cycles becomes very "improbable." First note that any odd number gives a forward down-jump of (approximately) 3/4, while an even number gives an up-jump of 3/2. So if there are m odd and n even numbers, then for a cycle we must have $(3/4)^m(3/2)^n \approx 1$, and if the lowest number in the cycle, the seed, is around 1000, then this approximation must be close, that is, the ratio m/n must approximate 0.70951 to four decimal places. If the seed is greater than 10,000, then the smallest cycle would have length 665 and could occur only for $m = 276$ and $n = 389$. What are the "chances" of this happening?

What then is the moral of this story? We are all aware from the work of Gödel that no matter what system of axioms we work with, as long as just a bit of number theory is available, there are true propositions for which there is no proof. Yet we continue as diligently as ever in looking for proofs, and frequently we find them, mainly, I think, because of the problems we choose to attack. But problems like the one discussed here seem to be of a special sort. It seems to me overwhelmingly "likely" that the orbit of 8 is infinite, an[d] correspondingly "unlikely" that there is a proof of this fact. Indeed, why should ther[e]

Whitehead Wit

The topologist J. H. C. Whitehead was often asked for his views on the work of his u[ncle] the renowned philosopher Alfred North Whitehead. Eventually he developed a [s...] answer. When asked, "What do you think of your uncle's philosophy?" he would rep[ly ...] really haven't thought much about it—but what do you think of *your* uncle's philosop[hy?"]