On Unsettleable Arithmetical Problems

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Abstract. It has long been known that there are arithmetic statements that are true but not provable, but it is usually thought that they must necessarily be complicated. In this paper, I shall argue that these wild beasts may be just around the corner.

1. INTRODUCTION. Before Fermat's Last Theorem was proved, there was some speculation that it might be unprovable. Many people noticed that the theorem and its negation have a different status. The negation asserts that for some $n > 2$ there is an nth power that is the sum of two smaller ones: Exhibiting these numbers proves the negation and disproves the theorem itself. So if one shows that the theorem is not disprovable, then one also shows there exist no such nth powers and therefore that the theorem is true.

However, the theorem could conceivably be true without being provable. In this case, its unprovability could not itself be proved since such a proof would imply the nonexistence of a counterexample.

The same sort of arguments applied to the Four Color Theorem and still apply to Goldbach's Conjecture, that every even number greater than 2 is the sum of two primes. (In fact, Goldbach asserted this of every positive even number since he counted 1 as a prime.) There has never been any doubt that Goldbach's conjecture is true because the evidence for it is overwhelming.

What are the simplest true assertions that are neither provable nor disprovable? I shall use the term un**sett**leable because for more than a century the ultimate basis for proof has been set theory. For some of my examples, it might even be that the assertion that they are not provable is not itself provable and so on. Of course this means that you shouldn't expect to see any proofs! My examples are inspired by

2. THE COLLATZ $3n + 1$ PROBLEM. Consider the *Collatz function* $\frac{1}{2}n \mid 3n + 1$, whose value is $\frac{1}{2}n$ if this is an integer and otherwise $3n + 1$. I shall call this a "bipartite linear function" because its value is one of two linear possibilities. The Collatz $3n + 1$ problem is, "Does iterating this function always eventually lead to 1" (starting at a positive integer)? It certainly does if we start at 7:

$$7 \to 22 \to 11 \to 34 \to 17 \to 52 \to 26 \to 13 \to$$
$$40 \to 20 \to 10 \to 5 \to 16 \to 8 \to 4 \to 2 \to 1.$$

Tomás Oliveira e Silva has verified [**4**] that it does for all numbers less than 5×10^{18}. There is a slight chance that this problem itself is unsettleable—some very similar problems certainly are.

I generalize it by considering multipartite linear functions and the associated games and problems. The value of the k-partite linear function

$$g(n) = g_1(n) \mid g_2(n) \mid \cdots \mid g_k(n)$$

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is the first one of the k linear functions $g_i(n) = a_i n + b_i$ that is integral (and is undefined if no $g_i(n)$ is integral). The corresponding *Collatzian game* is to repeatedly replace n by $g(n)$ until a predetermined number (1, say) is reached, or possibly $g(n)$ is undefined, when the game stops.

3. ARE THERE UNSETTLEABLE COLLATZIAN GAMES? There certainly are. The proof is more technical than the rest of the paper, but the message is simple: There is an explicit game with 24 simple linear functions for which there are numbers n for which the game never stops, but this is not provable. Gödel's famous Incompleteness Theorem, published in 1931, shows that no consistent system of axioms can prove every true arithmetical statement. In particular, it cannot prove an arithmetized version of its own consistency statement. Turing translated this into his theorem about computation—that the Halting Problem for an idealized model of computation is undecidable.

Given these stupendous results, it is comparatively trivial to produce an unsettleable Collatzian game. In a 1972 paper "Unpredictable Iterations" [**1**], I showed that any computation can be simulated by a Collatzian game of a very simple type, namely a *fraction game*, where the multipartite linear function involved has the form $r_1 n \mid r_2 n \mid \cdots \mid r_k n$ determined by a sequence $r_1, r_2, \ldots, r_n$ of rational numbers. The later paper "Fractran: a Simple Universal Programming Language for Arithmetic" [**2**], shows that the game whose fractions are:

$$\frac{583}{559}\ \frac{629}{551}\ \frac{437}{527}\ \frac{82}{517}\ \frac{615}{329}\ \frac{371}{129}\ \frac{1}{115}\ \frac{53}{86}\ \frac{43}{53}\ \frac{23}{47}\ \frac{341}{46}\ \frac{41}{43}$$
$$\frac{47}{41}\ \frac{29}{37}\ \frac{37}{31}\ \frac{299}{29}\ \frac{47}{23}\ \frac{161}{15}\ \frac{527}{19}\ \frac{159}{7}\ \frac{1}{17}\ \frac{1}{13}\ \frac{1}{3}$$

is universal in the sense that for any computable (technically, general recursive) function $f(n)$, there is a constant c such that the game takes $c \cdot 2^{2^n}$ to $2^{2^{f(n)}}$. In this case we define $f_c(n)$ to be $f(n)$. Moreover, the result includes all partial recursive functions (those that are not always defined) when we say that $f_c(n)$ is undefined if this game does not stop or stops at a number not of the form 2^{2^m}.

From this it follows fairly easily that whatever consistent axioms we use to define "settleable," there is some number for which the game with one more fraction,

$$\frac{583}{559}\ \frac{629}{551}\ \frac{437}{527}\ \frac{82}{517}\ \frac{615}{329}\ \frac{371}{129}\ \frac{1}{115}\ \frac{53}{86}\ \frac{43}{53}\ \frac{23}{47}\ \frac{341}{46}\ \frac{41}{43}$$
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never gets to 1, but this is not settleable. Instructions to the writer of a computer program: If the machine succeeds in proving $0 = 1$ from the nth axiom system, define $f(n) = 0$, otherwise leave $f(n)$ undefined. Then, precisely when the system is inconsistent, the 23-fraction game stops at 2, since 0 is the only possible value for $f(n)$, and so the 24-fraction one stops at 1.

What are the simplest Collatzian games that we can expect to be unsettleable? I think I have one answer.

4. THE AMUSICAL PERMUTATION. The *amusical permutation* $\mu(n)$ maps $2k \mapsto 3k$, $4k+1 \mapsto 3k+1$, and $4k-1 \mapsto 3k-1$. This is obviously a tripartite linear function, since every number is uniquely of one of the three forms on the left-hand side. Since every number is also uniquely of one of the forms on the right-hand side, μ^{-1} is equally a tripartite linear function and so μ is a permutation. In the abbreviated notation, the amusical permutation is $\frac{3n}{2} \mid \frac{3n+1}{4} \mid \frac{3n-1}{4}$ and its inverse is $\frac{2n}{3} \mid \frac{4n+1}{3} \mid \frac{4n-1}{3}$. Using $\{r\}$ for the nearest integer to r, we could abbreviate the permutation μ still further to $\frac{3n}{2} \mid \{\frac{3n}{4}\}$ and μ^{-1} to $\frac{2n}{3} \mid \{\frac{4n}{3}\}$, but this might obscure the fact that μ and μ^{-1} are tripartite rather than bipartite linear functions.

In the usual cycle notation (including possibly infinite cycles), μ begins

(**1**) (**2**, 3) (**4**, 6, 9, 7, 5) (**44**, 66, 99, 74, 111, 83, 62, 93, 70, 105, 79, 59)

(..., 91, 68, ..., 86, ..., 97, 73, 55, 41, 31, 23, 17, 13, 10, 15, 11, **8**, 12, 18, 27, 20, 30, 45, 34, 51, 38, 57, 43, 32, 48, 72, ...)

(..., 77, 58, 87, 65, 49, 37, 28, 42, 63, 47, 35, 26, 39, 29, 22, 33, 25, 19, **14**, 21, 16, 24, 36, 54, 81, 61, 46, 69, 52, 78, ..., 88, ..., 94, ..., 89, 67, 50, 75, 56, 84, ...)

(..., 98, ..., 100, ..., 95, 71, 53, **40**, 60, 90, ..., 76, ...)

(..., 85, **64**, 96, ...) (..., **80**, ...) (..., 92, ..., **82**, ...)

wherein the smallest element in each cycle is highlighted. I have shown what seem to be all the finite cycles and the first six infinite ones, so as to include all numbers up to 100.

Strictly speaking, I do not know that these statements are true. For instance, the cycle containing 8 might be finite, or might be the same as the one containing 14. However, the numbers in both of these cycles have been followed in each direction until they get larger than 10^{400} and it's obvious that they will never again descend below 100. We need a name for this kind of obviousness: I suggest *probvious*, abbreviating "probabilistically obvious."

Figure 1 makes this even more clear. It shows the cycles containing 8, 14, 40, 64, 80, and 82 on a logarithmic scale against applications of μ. These six curves have been

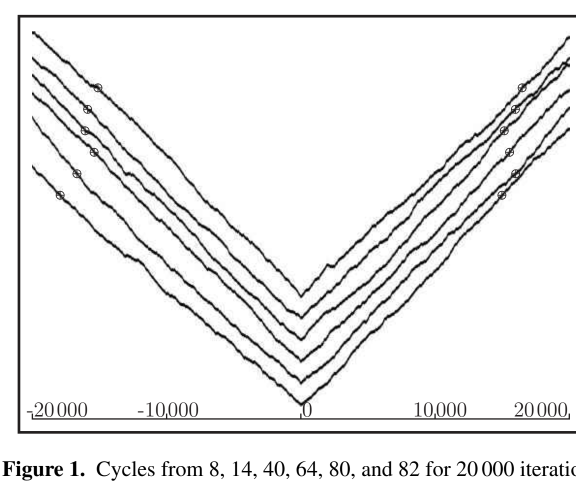

Figure 1. Cycles from 8, 14, 40, 64, 80, and 82 for 20 000 iterations

separated since on the scale displayed their least points are all indistinguishable from 1.* The spots indicate where they pass 10^{400}. In both directions the growth is exponential, and μ has a slightly faster rate than μ^{-1}. How can these facts be explained?

Let's consider what is probably the case when the numbers get large. Since a number n is equally likely to be even or odd, it will be multiplied on average by $\sqrt{\frac{3}{2} \times \frac{3}{4}} = \sqrt{\frac{9}{8}}$ per move. In twelve moves the expected factor is:

$$\frac{3^{12}}{2^{18}} = \frac{531\,441}{262\,144} \approx 2.027.$$

For μ^{-1} we multiply by $\frac{2}{3}$ in one case out of three and $\frac{4}{3}$ in the other two cases, so the expected increase in three moves is $\frac{32}{27}$ and the expected increase in twelve moves is:

$$\frac{32^4}{27^4} = \frac{2^{20}}{3^{12}} = \frac{1\,048\,576}{531\,441} \approx 1.973.$$

Taking these two numbers to be 2 explains the name "amusical." On a piano there are twelve notes per octave, which represents a doubling of frequency, just as twelve steps of the amusical permutation approximately doubles a number, on average. A frequency ratio of

$$\frac{3^{12}}{2^{19}} = \frac{531\,441}{524\,288} \approx 1.0136$$

is called the "Pythagorean comma," and is that between B-flat and A-sharp and other pairs of "enharmonically equivalent" notes. So there really is a connection with music. However, since the series always ascends by a fifth modulo octaves, it does not sound very musical, and it has amused me to call it amusical.

5. AMUSICAL UNSETTLEABILITIES? The simplest assertion about μ that I believe to be true but unsettleable is that 8 belongs to an infinite cycle.

Why is this true? Because the assertion that the logarithm of $\mu^n(8)$ increases linearly is amply verified by Figure 1, and nobody can seriously believe that $\mu^n(8)$, having already surpassed 10^{400}, will miraculously decrease to 8 again (Figure 2, produced after this text was written, shows that after 200 000 iterations it even surpasses 10^{5000}). Being true, the assertion will not be disprovable.

If a Collatzian game does not terminate, is there a proof that it does not terminate? The 24 fraction game of Section 3 (which was improved to 7 fractions by John Rickard [**3**]) shows that in general the answer is no. In general, if a Collatzian game does not stop, then there is no proof of this. So one should not expect the cycle of 8 to be provably infinite in the absence of any reason why it should be. After all, there is a very small positive probability that for some very large positive numbers M and N, $\mu^M(8)$ might just happen to be the same googol digit number as $\mu^{-N}(8)$.

Some readers will still be disappointed not to be given proofs, despite the warning in the Introduction that this is clearly impossible. I leave such readers with the intriguing thought that the proportion of fallacies in published proofs is far greater than the small positive probability mentioned in the previous paragraph.

*The visible kink in the graph of the 82 cycle corresponds to the remarkable decrease (by a factor of more than 75989) from $\mu^{1981}(82) = 5\ 518\ 82\ 09\ 452\ 689\ 749\ 562\ 442\ 051\ 558\ 599\ 474\ 342\ 616\ 171\ 049\ 802\ 024\ 438\ 847\ 761 \approx 5.519 \cdot 10^{63}$ to $\mu^{2208}(82) = 72\ 625\ 599\ 594\ 039\ 327\ 995\ 887\ 556\ 149\ 205\ 597\ 399\ 175\ 812\ 389\ 461\ 574\ 936\ 396 \approx 7.263 \cdot 10^{58}$. Admittedly this decrease by a factor of more than 2^{16} where an increase of almost 2^{19} was to be expected casts some doubt on the probabilistic arguments in the text.

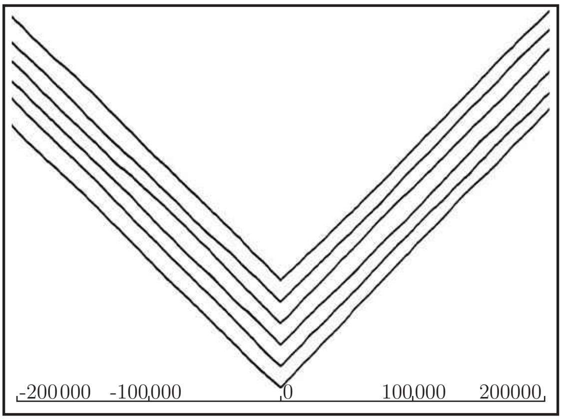

Figure 2. The same cycles for 200 000 iterations, showing much greater regularity in the long run, and further confirming the probabilistic predictions

APPENDIX 1: Is the $3n + 1$ problem settleable? The $3n + 1$ game presents special features, in that the probabilistic arguments suggest that large numbers decrease, rather than increase as in the amusical permutation. If this were provable, the conjecture would be settled by being provable. There is some slight hope that this might happen. The celebrated Hardy-Littlewood circle method often makes it possible to prove results that are predicted probabilistically.

Its most spectacular application has been Vinogradov's proof that every sufficiently large odd number is the sum of 3 primes. The method applies more generally to find the number of representations of a number n as a sum of a given number of numbers of some special form (primes, kth-powers, ...). Their estimate for this number takes the form $P + E$, where P is a probabilistic estimate and E an error term. One hopes to prove that $|E| < P$ so that there is a representation.

P turns out to be a product containing factors P_p, where P_p (for prime p) is the probability that n is p-adically (i.e., modulo all powers of p) the sum of the given number of numbers of that form. (There is also a factor P_∞, which is the proportion of numbers near to N that are representable.) In other words, P is just what one would naively expect from probabilistic considerations analogous to the ones we used for the amusical permutation.

It is not entirely inconceivable that such a method might one day prove the Collatz $3n + 1$ Conjecture, since all one has to do is prove that large enough numbers eventually reduce. However, I don't really believe it.

These remarks do not apply to the amusical permutation, whose behavior would not be established even if one proved that almost all large numbers tend to increase, since, for instance, the number obtained by applying μ a million times to 8 might just be the same as the number obtained by applying μ^{-1} rather more times to 8 or 14, in which case the cycle containing 8 would be either finite or the same as the cycle containing 14. This probviously doesn't happen, but we can't expect to prove it, and there's no reason to expect that either it or its negative follows the Zermelo–Fraenkel Axioms or any likely extension of them. In other words, it's probviously unsettleable.

Some other things are probvious but with a slightly smaller probability. For instance, there is probviously an algorithm for telling whether n belongs to a finite cycle. Just ask whether n is one of the twenty numbers:

$$1, 2, 3, 4, 5, 6, 7, 9, 44, 59, 62, 66, 70, 74, 79, 83, 93, 99, 105, 111;$$

if so, say "yes," if not, "no." If there is another finite cycle this algorithm fails, but the answer will still be computable unless there are infinitely many finite cycles, which there very probviously aren't.

I've already suggested that the assertion that 8 is in an infinite cycle, although probvious, is unsettleable. I now propose that this unsettleability assertion is itself unprovable, and therefore unsettleable and so on arbitrarily far into the metatheory.

Even if this is wrong, mathematics is *not* defined by any system of set theoretical axioms. In particular, it is likely that some simple Collatzian problems (possibly even the $3n + 1$ problem itself) will remain forever unsettleable.

APPENDIX 2: Some amusical paradoxes. With some relief let's put deep problems aside to discuss some simple puzzles about the behavior of the amusical permutation. We have already noticed the "Either-way-up paradox," that the numbers in the typical cycle increase no matter which way we move along the cycle. It's not really paradoxical, as Figure 1 shows. No matter where we start on the cycle and no matter which direction we move, we'll eventually pass the minimum and after that we go up.

Here is the "Congruence Paradox." Since $n < \mu(n)$ just if n is even and $n < \mu^{-1}(n)$ just if n is not a multiple of 3, it satisfies both these inequalities (and so is a local minimum) just if $n \equiv \pm 2 \pmod 6$, which happens in exactly one third of cases: Right? Maybe not. It satisfies neither inequality just if $n \equiv 3 \pmod 6$ and so is a local maximum in exactly one sixth of the cases. But in any sequence local minima and maxima alternate, so there should be just as many of each. So which is right: Do we get these turning points every third term or every sixth term?

Let's think again. Whenever an increase is followed by a decrease we get a maximum, and since increases and decreases are equally likely, we should get a maximum one quarter of the time and the same argument applies to minima, which happen when a decrease is followed by an increase. So these things both happen once in four moves rather than once in either three or six! We can get yet another answer by thinking backwards, when the two probabilities are $\frac{2}{3}$ and $\frac{1}{3}$, leading to the conclusion that maxima and minima both occur once every $4\frac{1}{2}$ moves.

What these arguments prove is not really paradoxical. If one follows a typical number one sees both maxima and minima equally often, namely once every four moves going forwards or once every $4\frac{1}{2}$ backwards. We leave it to the reader to explain why neither of these answers (once in 4 or $4\frac{1}{2}$) agrees with either of the answers (once in 3 or 6) given by the Congruence Paradox.

Since the apparent contradictions are based on our experience with finite cycles, one might think that they could be turned around to prove that most cycles are infinite, or that at least there are some infinite cycles. However, having thought about it, I still believe that these problems are unsettleable.

If you disagree try to prove or disprove either of the following statements.

1. There is a new finite cycle.
2. There is an infinite cycle.

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POSTSCRIPT. *Added June 8th, 2012.* The following argument has convinced me that the Collatz $3n + 1$ Conjecture is itself very likely to be unsettleable, rather than this merely having the slight chance mentioned above. It uses the fact that there are arbitrarily tall "mountains" in the graph of the Collatz game. To see this, observe that $2m - 1$ passes in two moves to $3m - 1$, from which it follows that $2^k m - 1$ passes in $2k$ moves to $3^k m - 1$. Now by the Chinese Remainder Theorem we can arrange that $3^k m - 1$ has the form $2^l n$, which passes by l moves to n. There is a very slight possibility that n happens to be the same as the number $2^k m - 1$ that we started with. Let's suppose that the starting number $2^k m - 1$ is about a googol; then the downward slope of the mountain certainly contains a number between one and two googols, so the chance that this is the same as the starting number is at least one googolth. (This is justified by observations for smaller n showing that the first iterate that lies in the range $[n, 2n)$ is approximately uniformly distributed in this range.) In my view the fact that this probability, though very small, is positive, makes it extremely unlikely that there can be a proof that the Collatz game has no cycles that contain only large numbers. This should not be confused with a suggestion that there actually *are* cycles containing large numbers. After all, events whose probability is around one googolth are distinctly unlikely to happen!

I don't want readers to take these words on trust but rather to encourage those who don't find them convincing to try even harder to prove the Collatz Conjecture!

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John Conway was always engaged in discussions with students, such as here during the excursion.