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THE COLLATZ CONJECTURE
A Case Study in Mathematical Problem Solving*

1. Introduction

In previous papers (see Van Bendegem [1993], [1996], [1998], [2000], [2004], [2005], and jointly with Van Kerkhove [2005]) we have proposed the idea that, if we look at what mathematicians do in their daily work, one will find that conceiving and writing down proofs does not fully capture their activity. In other words, it is of course true that mathematicians spend lots of time proving theorems, but at the same time they also spend lots of time preparing the ground, if you like, to construct a proof. A first tentative list of these "extras" comprises at least the following items:

[I1] Informal proofs: "proofs" that do not satisfy the formal standards, e.g., a non-justified rule is used, say, an extrapolation from a finite case to an infinite case, but that nevertheless arrives at a correct result, thus pointing the way to a possibly correct proof.

[I2] Career induction: to get a hold on a problem ranging over, e.g., all natural numbers, one studies the separate cases of an initial fragment to get ideas about possible techniques that could lead to a proof of the general statement.

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[I3] Mathematical "experiments": computer visualisations are the best known example. Although in several respects unreliable, as it often involves the reduction of the (infinite) continuum to a finite, discrete set, they do produce "clues" that serve as a guide for a proof.

[I4] Probabilistic considerations: although proofs in the genuine sense of the word, what they establish is not that a mathematical object (say, a natural number) has a certain property (say, being a prime), but has that property with a certain probability.

[I5] Computer proofs: to be distinguished from computer visualisations, these proofs involve the checking of a finite, though huge amount of separate cases such that human checking is either impossible or too prone to errors and hence a computer program performs the task. The result is not a proof in the classical sense, since unavoidably a human cannot check the proof, one of the basic standards to call a proof a proof.

[I6] Metamathematical considerations: although one has a proof satisfying the required standards, the result is seen as paradoxical, counter-intuitive, in conflict with expectations, and hence it is questioned. It can also involve formal metamathematical results, e.g., in showing that a particular problem is unsolvable.

Usually given a specific case, i.e., a particular theorem and its proof history, one will see that one item or a few of the above list will actually be used in the proof search. It is rather exceptional to have a case where (nearly) all these elements are present. The topic of this paper is quite simply the presentation (to a certain depth) of one such case study. All elements, save [I1], of the list are present in one way or another. It can thus be considered an *exemplar* (in the Kuhnian sense), and, perhaps more importantly, as far as I know, a *new* exemplar. As is so often the case, in many philosophical discussions, the same typical example keeps coming back, wrongly suggesting that no other examples are available[1]. In addition, the problem is fairly easy to state, although the mathematics that are used in search of a proof reach formidable heights. And, finally, it is also a problem that many mathematicians consider absolutely *not* interesting. As will be shown here, the problem definitely is interesting, but then the question is

[1]Think, e.g., about thought experiments. A tiny set of examples keeps coming back over and over again: Galileo's thought experiment about heavy and light masses, Newton's bucket experiment concerning absolute properties such as acceleration, and Einstein's thought experiment about travelling on a light wave. It has led some philosophers to mistakenly claim that there is no real problem about thought experiments as they are exceptional and, hence, not important.

why so many think otherwise. In Section 3, I will provide some suggestions, relating to this matter.

This paper is primarily based on the overview article of Jeffrey Lagarias [2004][2] that provides an extremely detailed presentation of the problem and the attempts to deal with it. Additional sources are used to highlight details of the main story. The contrast between Lagarias' presentation and mine is that I focus on the philosophically interesting features, not necessarily the "pure" mathematical aspects. However, as should be clear, this paper is heavily indebted to the excellent work done by him.

2. The problem

Consider a function from $\mathbb{N}_0$ to $\mathbb{N}_0$, defined as follows:

$$\mathrm{T}(n) = \begin{cases} n/2 & \text{if } n \text{ is even} \\ (3n+1)/2 & \text{if } n \text{ is odd} \end{cases}$$

Next define the iterate of T as usual:

$$\begin{cases} \mathrm{T}^{(0)}(n) = n \\ \mathrm{T}^{(i+1)}(n) = \mathrm{T}(\mathrm{T}^{i}(n)) \end{cases}$$

The question is now to show that for every $n \in \mathbb{N}_0$, there is a finite k, such that

$$\mathrm{T}^{(k)}(n) = 1.$$

A straightforward example: take $n = 7$, then we have the following sequence

$$\begin{array}{cccccccccccccccccccccccc} 7 & \to & 11 & \to & 17 & \to & 26 & \to & 13 & \to & 20 & \to & 10 & \to & 5 & \to & 8 & \to & 4 & \to & 2 & \to & 1 \\ 0 & & 1 & & 2 & & 3 & & 4 & & 5 & & 6 & & 7 & & 8 & & 9 & & 10 & & 11 \end{array}$$

therefore $\mathrm{T}^{(11)}(7) = 1$ and $k = 11$.

3. The origin of the problem

It is easy to understand why, if one has only the above information and is asked whether or not this is an *interesting* problem, the answer will most likely be negative. Why?

[2] This paper available on the Internet is an update of a previous webpaper from 1996, see Lagarias [1996], and itself a further elaboration of Lagarias [1985]. The most recent paper is an annotated bibliography whereas Lagarias [1996] retraces the history of the problem, proofs included.

Firstly, it is quite easy to "invent" similar problems, so why should this particular case attract our attention? As a matter of fact, this type of argument has been used on several occasions by mathematicians, the most famous case no doubt Gauss' comment on the problem that was to become Fermat's Last Theorem. In 1816 he wrote to Heinrich Olbers (known as the originator of the Olbers' paradox) that "he could easily lay down a multitude of such propositions, which one could neither prove nor dispose of" (see Ribenboim [1979], p. 3).

Secondly, suppose we do manage to show the theorem to be correct, what have we gained? Are there other problems around that would get solved in the process as well? At first sight not.

Thirdly, on the level of proof methods, it is not guaranteed at all that interesting things will come out of it. Is it likely that some ingenious new proof method could solve this problem, but is it to be expected? These are all very good reasons to consider the problem not interesting (as the author of this paper believed for a very long time, up to the point that he actually wrote that because the problem has no connections with other problems, it was perfectly acceptable to consider it uninteresting; so this paper is at the same time a correction on one of my former views).

In fact, notwithstanding the observation that not that many mathematicians are actually involved with this problem, it is definitely an interesting problem. Let me say a few words about its origin. When one is dealing with number-theoretic functions, say functions f from $\mathbb{N}_0$ to $\mathbb{N}_0$, then one of the particular problems one has to deal with is *notation* and *representation*. What I mean is the following.

Suppose that the function f from $\mathbb{N}_0$ to $\mathbb{N}_0$ is a permutation. Then there are several ways to represent this function:

(a) One of the classical forms is in tabular form:

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 & \dots \\ \mathrm{f}(1) & \mathrm{f}(2) & \mathrm{f}(3) & \mathrm{f}(4) & \mathrm{f}(5) & \dots \end{pmatrix}$$

Note that this representation supposes to have the necessary knowledge on how to continue the table.

(b) Obviously, as for any function, we can have an explicit form:

$$\mathrm{f}(n) = \text{some symbolic expression involving } n.$$

(c) A variation on (b) is a function defined implicitly by some recurrence relations:

$$\mathrm{f}(n) = \mathrm{g}(\mathrm{f}(n-1), \mathrm{f}(n-2), \dots, \mathrm{f}(3), \mathrm{f}(2), \mathrm{f}(1)),$$

where not all of f(i), $n-1 \leqslant i \leqslant 1$ need occur and where g is some specified function.

(d) Another form that differs radically from the three above, but just like (a) supposes that one has sufficient knowledge on how to continue the figure, is a graphical representation.

$$\underleftarrow{1 \to 2 \to 3} \quad \underleftarrow{4 \to 5 \to 6} \quad 7 \to \dots$$

where an arrow represents an application of the function f (in this case, the simple function f(n), defined by $\mathrm{f}(3n+1) = 3n+2$, $\mathrm{f}(3n+2) = 3(n+1)$ and $\mathrm{f}(3(n+1)) = 3n+1$)). Although this example is rather trivial, the importance of a well-chosen representation must be obvious. The graphical representation shows immediately that f is composed of an infinite number of 3-cycles. One could very well imagine that if f becomes more complex, the graph can tell more things than an algebraic of analytical expression. (Note at the same time the connection with visualisations; although there is no computer involvement here, it does show the importance of an image).

Note also that different graph representations are possible. Instead of simply listing the natural numbers and drawing the appropriate arrows, we can start with 1 and list the iterates of 1:

$$1 \to \mathrm{f}(1) \to \mathrm{f}^2(1) \to \mathrm{f}^3(1) \to \dots$$

All of this shows that if we want to understand what permutations are all about, what their properties are, then it is a useful approach to examine the graphs of such functions. In addition, it allows to rephrase some questions into graph-theoretical questions. This is actually the area that the "creator" of the problem, Lothar Collatz, was working on. Although his examples are different from what is now known as the Collatz Conjecture (CC), they raise the same problems. His original question was whether, for a particular function f, the trajectory starting with 8 and the iterates of 8, contains 1 or not. (I use here the term "trajectory" because it need not be a cycle). One now sees the relation to the CC. Rephrased in terms of trajectories, the CC claims:

> For any natural number n, the trajectory starting with n, contains the number 1.

Of course, no mathematician doubts the importance of permutation theory. It is so deeply entrenched in number theory and beyond, that is must be considered one of the core parts of mathematics. Although one might perhaps consider the CC as a “spin-off”, it is clear that the general question that is raised by it is an interesting one. What seems to have been at play is that there are several gaps in the research of the CC. The problem disappears for some years only to reappear at some other moment in the hands of another mathematician. The fact that it was not easy to locate the “true” origins of the problems is supported by the observation that the very same problem is known under different names: Hasse’s algorithm, the Syracuse problem, Kakutani’s problem, Ulam’s problem, and sometimes it is even referred to as the Hailstone problem. The last name is a reference to the behaviour of the sequence of $\mathrm{T}^i(n)$. It tends to move upwards and downwards much in the way that hailstones hit the ground and bounce back up again.

4. Mathematical induction, number crunching and pictures

An important feature to notice in the search for a proof of the CC is that, at first sight, it seems not very useful to invoke mathematical induction as a proof method. One of the obvious problems is that it does not help to start from the assumption that the CC has been proven for all cases up to a number n in order to prove the case for $n+1$, as the iterates for $n+1$ can go well beyond $n+1$. In the above example for $n = 7$, the highest value one reaches is 26. This would shift the problem to the question whether one can show that:

> For all n, there is a finite number $N(n)$, such that for all i, $\mathrm{T}^{(i)}(n) \leqslant N(n)$.

In addition, one would need some connection between $N(n)$ and $N(n+1)$ to be able to get the induction process working. However, it is clear that this new task looks every bit as difficult as the original task. Of course, one might try an induction on some other parameter of the problem, but it becomes soon clear that either one keeps coming back to the original problem itself or one ends up worse off. E.g., one might try an induction on k, such that $\mathrm{T}^{(k)}(n) = 1$, if at all. However, one needs a way to enumerate the n such that k forms a sequence 1, 2, 3, ... (with or without gaps?). But that seems an even harder question to answer:

> Given a natural number k, what are the numbers n such that $\mathrm{T}^{(k)}(n) = 1$?

If we had an answer to this question and, for every k, we could list the numbers n, then of course if we could prove that some number n is missing for all k, then we would have a disproof of the CC. Clearly, this is not an interesting strategy and so, in short, one does well (initially) to forget about mathematical induction.

As one might expect with this kind of problem, it is very tempting to collect *numerical evidence*, corresponding to a mixture of career induction [I2] and computer proof (a mix of [I3] and [I5]). The CC has been checked up to a staggering 3.24×10^{17}. One might wonder what the relevance of such evidence could possibly be.

One argument is rather trivial: one might come up with a counterexample, thereby settling the problem by producing a disproof. However, oddly enough, in many cases where such evidence is collected, the mathematicians tend to believe that there are no counterexamples. So why do they do it?

A possible answer is that mathematicians sometimes do what scientists in general do: you collect evidence hoping that some pattern appears that tells you something about the problem your studying. As it happens in this case, the only thing that appears is complexity and more complexity. Table 1 shows the maximum value reached of the number n, (indicated by the variable N) as n ranges from 1 to 100.000. Note, e.g., that between 1.819 and 4.254, the highest value remains 1.276.936 but at 4.255 it jumps straight away to 6.810.136. Even in this case, however, it is clear that the numerical evidence is interesting for it is shows that we are most likely dealing with a problem that is intrinsically complex and therefore we should not be surprised that the problems resists attempts to prove it.

As to the computer aspect of this numerical search, it is clear that we are dealing here not with a mere enumeration of cases; the size of the set of checked cases is simply too large to be checked one by one. Hence a whole range of mathematical techniques and computer engineering is involved and, therefore, it becomes interesting. Note that for the computer checking a distributed network had to be created to have sufficient computational power.

5. Enter probabilities and statistics

5.1. A probabilistic argument

What is more interesting is the fact that there exists a probabilistic heuristic argument, a perfect illustration of [I4], that (at least some) mathematicians seem to find convincing enough to believe the CC to be provable. This is the argument:

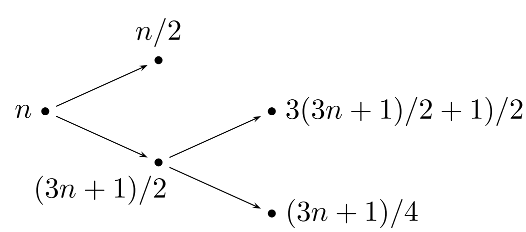
N	Path length	Maximum value
1	0	1
2	1	2
3	7	16
7	16	52
15	17	160
27	111	9,232
255	47	13,120
447	97	39,364
639	131	41,524
703	170	250,504
1,819	161	1,276,936
4,255	201	6,810,136
4,591	170	8,153,620
9,663	184	27,114,424
20,895	255	50,143,264
26,623	307	106,358,020
31,911	160	121,012,864
60,975	334	593,279,152
77,671	231	1,570,824,736

Table 1. Sequence of peak values up to $N = 100{,}000$
(© *Scientific American*, see Hayes [1984])

(a) You do not have to worry about even numbers $2n$, because in the next step, you will have n, so you go "down", i.e., the numbers are becoming smaller.

(b) Therefore look at what happens when you start with an odd number $2n + 1$. Either in the next step you will have an odd number or an even number. *Assume that the probability is* $1/2$ *in both cases.*

(c) Repeat the process. This produces the following picture:



(each arrow has a probability $1/2$ and note that $3(3n + 1)/2 + 1)/2$ is an even number, since by construction $(3n + 1)/2$ is odd).

(d) Consider now a trajectory from one odd number to another odd number. Suppose that in between there are $N - 1$ odd numbers. In total this produces N transitions from an odd number to the next. What we expect is that $N/2$ of these transitions will happen in one step, $N/4$ in two steps, and so on. This leads to a growth factor:

$$(3/2)^{N/2}.(3/4)^{N/4}.(3/8)^{N/8}\dots$$

So the average growth factor per transition is:

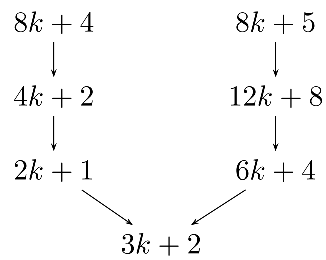
$$(3/2)^{1/2}.(3/4)^{1/4}.(3/8)^{1/8}\dots$$

(e) A simple calculation shows that the numerator is nothing but 3 to the power $1/2 + 1/4 + 1/8 + \cdots = 1$, therefore 3; and the denominator is $2^{1/2}.4^{1/4}.8^{1/8}\cdots = 2^2 = 4$. (Here a simple inductive reasoning will do the trick). Hence the average growth factor per transition is 3/4 which is smaller than 1, so on average the numbers “shrink”, therefore the CC should be correct.

Of course, this beautiful argument stands or falls with the assumption made in (b) (in italics). Is there reason to assume that there is just as much chance to have an odd or an even number in the next step? Actually not and, in addition, there are many interesting problems in number theory where one expects certain probabilities but amazingly enough, the mathematical “facts” show otherwise. A famous example to illustrate this point concerns a conjecture put forward by Georg Polya. Think about the prime decomposition of natural numbers. Count the number of primes, that need not be distinct. Call $r(n)$ = number of primes in n. Then either $r(n)$ is even or odd. Does it not seem likely that if we pick an arbitrary number the probability that $r(n)$ is even or odd is 1/2? As it happens this is not the case, and the behaviour of the function $r(n)$ turns out to be quite complex. In that sense, it is quite understandable that for some mathematicians these probabilistic considerations carry little weight.

5.2. Gathering statistical evidence

Related to the above are what one might call *statistical analyses* of the problem. Here the objective is to explore and hopefully to understand and explain particular features that appear in the numerical tables, not necessarily to find arguments for or against the correctness of the conjecture.



Consider, e.g., the fact that consecutive numbers have trajectories of the same length (and other properties). In some cases this phenomenon can be easily explained. The diagram shows why numbers of the $8k + 4$ end $8k + 5$ must have the same trajectory length.

Although, as said, it is not clear in what way such results could contribute to a final answer, i.e., a proof satisfying the usual standards, there seems to be a very clear analogy to be drawn with scientific practice. If it is meaningful to speak of a *Collatz-universe*, meaning thereby all the numerical material related to the conjecture, then these probabilistic and statistical analyses correspond to an exploration of that universe. One is not really expecting to find laws or the like, but rather indications that suggest what possible laws one could look or aim for. In a sense the mathematician is trying to get a "grip" on the problem by wandering through the territory.

6. Digression: generating concepts to tackle the problem

The heading of this section seems to suggest that its topic is of minor importance. Such is definitely not the case, but there are two reasons why I want to treat it separately: firstly, because it is a common feature of the whole mathematical enterprise and in that sense it occurs in [I1] up to and including [I6], and, secondly, because the topic and its related literature is too vast to treat here in a thorough way. What is this feature? For want of a better notion, I propose to call it *generating concepts* (GC). Let me first of all illustrate what I mean using CC.

Take a look at the original problem. What concepts occur in the problem formulation? We talk about functions, natural numbers, about elementary arithmetical operations (addition, multiplication, division) and about iteration. Those are roughly the "ingredients" of the problem. The striking feature when one goes through the history of CC is that the concepts as formulated in the original problem statement play hardly any role at all. Instead, and techniques such as listed in [I1]-[I6] promote this process, a whole range of derived concepts is introduced and in some theorems none of the original concepts actually occur. For CC, what follows are some of the derived concepts:

(a) The notion of iteration leads rather naturally to the idea of a *trajectory*, i.e., the sequence of numbers, starting with n, and ending with the first 1 to occur.

(b) An obvious correlate of (a) is the *length of the trajectory.*

(c) Given a trajectory, let k be the least positive number such that $T^{(k)}(n) < n$, then k is called the *stopping time* of n, or, $\sigma(n) = k$.

(d) Derived from (c) is $\sigma_\infty(n)$, this is the *total stopping time*, i.e., that k such that $\mathrm{T}^{(k)}(n) = 1$, (this relates of course to (b)).

(e) The *expansion factor* $\mathrm{s}(n)$ is defined as the division of the largest value reached in a trajectory by n, i.e., $\mathrm{s}(n) = \frac{\sup_{k \geqslant 0} \mathrm{T}^{(k)}(n)}{n}$.

(f) The *parity vector* $v_k(n)$, basically corresponding to the trajectory, where all the numbers are reduced modulo 2.

As an illustration, consider once more the example $n = 7$, then the properties are:

(a) *Trajectory of* $n = 7$: $\langle 7, 11, 17, 26, 13, 20, 10, 5, 8, 4, 2, 1 \rangle$,

(b) *Length of the trajectory* $= 12$,

(c) $\sigma(7) = 7$,

(d) $\sigma_\infty(7) = 11$,

(e) $\mathrm{s}(n) = 26/7 \approx 3{,}7$

(f) $v_{11}(7) = \langle 1, 1, 1, 0, 1, 0, 0, 1, 0, 0, 0, 1 \rangle$

On the one hand, it seems obvious that these new concepts should emerge, as it is easy to see how they are related to the original problem and, hence, how they can be helpful in the search for a proof. However, this is only part of the story. Besides the concepts mentioned above, many others could have been proposed, but apparently have not been proposed. As an example, take this personally thought-up concept:

$M_7 =$ the set of all trajectories such that the length of the trajectory is a multiple of 7

and related to that:

$N_7 =$ those numbers that belong to a trajectory in M_7.

It is my estimated guess that no mathematician will find the notions of M_7 and N_7 the least bit interesting. But then the question must be: why? No doubt the answer will be: the mathematicians' practice, but that does not help to fill in the details. What is it in that practice that allows mathematicians to make such a selection? Let me reformulate that question in slightly more abstract terms. Suppose that:

(a) we are given a set X, and

(b) a property corresponds to a subset of X,

then,

(c) we have a total of $2^{|X|}$ possible properties.

If X is of infinite size, so is 2^X. Hence we are faced with a double question:

(Q1) How is a *finite* subset of the interesting properties chosen?

(Q2) How are uninteresting properties avoided?

Note the importance of (Q1). Computer programs such as *Automatic Mathematician*, developed in the eighties by Douglas Lenat, were indeed capable of generating interesting concepts, but, as time went on, they tended to drown in them. Somehow, real-life mathematicians seem to avoid this pitfall. Apart from general considerations about concept generation and selection as studied in cognitive psychology[3] (involving the study of metaphors, analogies, conceptual blending, and the like), mathematics is in this sense a special case in that concept generation and proof are tied together. E.g., in the case of CC, $\sigma(n)$ is more interesting than $\sigma_\infty(n)$ because the first theorems one could prove about CC involved the stopping time function and not the total stopping time function. Thereby the concept is reinforced and all concepts that can be easily linked to it. If a derived concept does not turn up somewhere in a proof, then it will most likely disappear. As the production of proofs is a rather difficult and often slow process, it explains why so few derived concepts survive.

As a further support of this thesis—the link between concept generation and proof production—it is worthwhile to look at so-called "seminal" papers in the history of mathematics, i.e., those contributions that either set in motion a new branch of the mathematical tree or relaunched a research that had arrived at a standstill. One such famous example is Bernhard Riemann's paper "Uber die Anzahl der Primzahlen unter einer gegebenen

[3]The literature in this field is too extensive and too varied to be reported here, but, obviously, for mathematics a fine example (although many, such as myself, tend to disagree with the authors) is the recent work of Lakoff and Nunez [2000].

Grösse" [1859], ("On the Number of Prime Numbers Less than a Given Quantity"). I will not go into details here, but one, if not the most striking feature of the paper is that there are hardly any proofs and if so, they tend to be "over-summarized", making it a tough job to reconstruct what the author might have meant[4]. On the other hand, what the paper does is to introduce a range of new functions that get connected to existing and well-studied functions, thereby offering a new range to explore. As the paper is generally acknowledged as a fundamental contribution, it is reasonable to conclude that such concept generation attempts are considered as important as proofs themselves.

However, let me now return to the main story of this paper and look into item [I6] on the list.

7. Metalevel considerations

In 1972 John Conway published a short paper with a curious and important result: a generalization of CC is undecidable. In that sense, it is a beautiful illustration of a type [I6] kind of argument. It implies that *perhaps* CC itself is undecidable, although at present no such result has been found[5].

The generalization is the following:

Consider a function g from integers to integers (note that this is not an essential extension as the integers can always be mapped one-to-one onto the natural numbers[6]), such that

$$\mathrm{g}(n) = a_i n + b_i \qquad \text{for } n \equiv i \pmod{p},$$

and where a_i and b_i are rational numbers such that $\mathrm{g}(n)$ is always an integer.

[4]One of the best sources about Riemann's paper is Edwards [1974]. The statement on the low proof quality of the paper is based on this quote of Edwards: "The real contribution of Riemann's 1859 paper lay not in its results but in its methods. The principal result was a formula [...] However, Riemann's proof of this formula was inadequate [...]". (p. 4)

[5]If CC would turn out to be undecidable, then it would most certainly replace the "busy beaver" as the simplest undecidable problem. The "busy beaver" concerns Turing machines producing a string of '1'-s on an empty tape. See Boolos et al. [2002], pp. 41–44, for a clear and concise exposition of the "busy beaver" problem.

[6]The reason for the extension from natural numbers to integers has to do with the problem of encoding a problem known to be undecidable into this generalization of *CC*. In that sense the construction can be reformulated restricted to natural numbers, however the result would be definitely 'ugly'.

CC then corresponds to the special case, where:

$$g(n) = (1/2)n + 0 \qquad \text{for } n \equiv 0 \pmod 2\text{, and}$$
$$g(n) = (3/2)n + 1/2 \qquad \text{for } n \equiv 1 \pmod 2.$$

So $a_0 = 1/2$, $b_0 = 0$, $a_1 = 3/2$ and $b_1 = 1/2$.

The undecidability comes down to the fact that, given a function g, and given a number n, there is no algorithm that decides whether there is a number k such that $g^{(k)}(n) = 1$. Actually, Conway proved an even stronger result, viz. all rational numbers b_i may be equal to 0.

Obviously, what this result implies is, at least, that one should not be amazed by the complexity of the original problem, the CC. The fact that the statement resisted and continues to resist proof for quite some time now, is perhaps something to be expected, given Conway's result. In that sense, it does have an influence on mathematicians' expectations. However, the story does not end there. There are links between CC and ergodic theory (see Lagarias [1985], Section 2.8), thus introducing considerations about stochasticity and randomness into the proof search. These considerations are clearly not purely mathematical, witness this quote from the conclusion of Lagarias [1985]:

> Is the $3x + 1$ problem intractably hard? The difficulty of settling the $3x + 1$ problem seems connected to the fact that it is a deterministic process that simulates "random" behaviour. We face this dilemma: On the one hand, to the extent that the problem has structure, we can analyse it—yet it is precisely this structure that seems to prevent us from proving that it behaves "randomly." On the other hand, to the extent that the problem is structureless and "random," we have nothing to analyse and consequently cannot rigorously prove anything. Of course there remains the possibi1ity that someone wil1 find some hidden regularity in the $3x + 1$ problem that allows some of the conjectures about it to be settled. The existing general methods in number theory and ergodic theory do not seem to touch the $3x + 1$ problem; in this sense it seems intractable at present. Indeed all the conjectures made in this paper seem currently to be out of reach if they are true; I think there is more chance of disproving those that are false.

It seems obvious, at least to me, that such statements do not only go beyond mathematics proper, but at the same time contain (a) philosophical ideas about the structure of the mathematical universe, (b) the expectations one might reasonably have concerning the likelihood of proving a theorem, and (c) the connection(s) between these two elements. In a sense this could be considered a form of philosophy emerging out of mathematical practice itself, and hence, produced by mathematicians themselves. This explains to a certain extent the contrast with philosophical explanations by philosophers about mathematics, that tend to focus on "end-products", i.e., "finished" proofs. Let me explore this idea a bit further in the conclusion of this paper.

8. Conclusion

A first minor remark to make is that the reader surely will have noticed that an illustration of [I1] is missing. There are indeed, as far as I know, no examples of "sketchy proofs" that could possibly be translated or transformed into an acceptable proof. On the whole, occurrences of [I1] seem to be rather rare. However, the presence of all the other elements do show that the Collatz Conjecture deserves to be called an "exemplar".

Secondly, and more importantly, the reader will also have noticed that I have given no "real" proofs of partial results. After all, see Lagarias [2004], as one might expect, there is a multitude of proofs dealing with bits and pieces of the CC, but I did not want to pay attention to that part of the mathematical process. I did want to focus on all those elements that are at the same time not proofs, but essential to guide the search for a proof. My claim is that these considerations are part and parcel of mathematical practice and, by implication, that a philosophy of mathematics that claims to deal with the essential features of what mathematics is all about, should include these elements.

Thirdly, as a consequence of the observation above, it follows that mathematics — or the mathematical building, to use the best known metaphor— need not be an integrated whole or a unity in some sense. After all, not only will proof methods differ from mathematical domain to mathematical domain—think, e.g., about the difference between "diagram chasing" in category theory and mathematical induction in number theory (see Van Bendegem [2004])—but the additional elements [I1] up to [I6] will most certainly differ from domain to domain—in number theory number crunching is obviously possible but visualisations, equally obviously, seem more suited to geometrical and topological problems. Note that this form of 'disunity' I am pleading for, is not in contradiction with the existence of the foundations of mathematics, such as set theory. From the foundational point of view, we look at the end-products, i.e., mathematical theories, leave out the details of the process that has led to the theory, and then integrate these theories by constructing a common language wherein these theories can be translated, thus creating a new universe that has a uniformity that the daily practice of mathematicians seems to be lacking. In terms of languages, foundational work corresponds to designing an artificial language such as Esperanto. Whereas in this paper I am suggesting that we should also have a look at the languages we daily speak. In the same manner that Esperanto did not become the world language, working mathematicians know that there is this special group of "foundational speakers" that seem to have trouble to convince everyone else to speak as they do. In addition, the better we understand our daily languages, the more likely we will understand what kind of artificial languages will have any rate of success or not.

As a final closing remark, let me just mention that at the moment of writing—February 2005—the problem remains unsolved.

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