Math Wars

About This Section

This section is not about my attempts to solve the two very difficult problems I had set out to solve in the late seventies, but rather about my attempts to get mathematicians to read my results. Some readers will find parts of the section tedious, and to these readers I say: Feel free to read only what you find interesting! But I wanted to leave a detailed record, including quotes from emails, of what I now regard as the often inexcusable behavior of the academic mathematicians I approached.

One mathematician exhibited none of this behavior. He did his best to provide valid criticisms of my efforts. He will not be discussed in this section. I was appalled by the behavior of the rest — not the least by their lack of curiosity and insight. Since I was not an academic, not one of them, apart from the above three, up to the time of this writing (May, 2016), has had the courage to actually read, sentence by sentence, what I have written, and to do so without prejudice while at the same time making a genuine attempt to understand my arguments.

There is no question in my mind but that this behavior was a direct result of the current academic mathematics Culture, with its emphasis on credentials, and on communicating only with the tenured few. I cannot explain why the above-mentioned three managed to put aside the Culture in their dealings with me.

There was also a stern prohibition against working on problems that the mathematics Establishment decreed should not be worked on. One of these problems was that of finding a simple proof of Fermat's Last Theorem. Andrew Wiles had constructed, in the early '90s, a masterful proof of well over 100 pages of the most advanced mathematics. I had been wondering, even before his proof, if a simple one existed. But no mathematician, especially after Wiles's proof, would consider such a thing.

I cannot believe that this stifling elitism can be good for the discipline in the long run. But I also cannot say that I see any hope for change.

Ed the Physicist

In March 2004 I posted an ad on a Physics Dept. bulletin board at the University of California at Berkeley (UCB) asking for help in understanding Special and General Relativity. I received an email reply from a fifth-year graduate student who was working on a PhD in string theory, which was then considered the most promising means of unifying quantum mechanics and general relativity. He answered my physics questions in an exchange of emails. I then thought: "Why not have him take a look at my Fermat's Last Theorem (FLT) and Syracuse Problem papers?" He was willing to do this, and thus began a consulting relationship that lasted until early summer 2010. His name was Ed and he proved to be the sharpest, the most acute, critic of my work that I had found. He cauggt errors that even the graduate students and a couple of other professional mathematicians overlooked. He also had none of the pedantry that drove me to distraction in professional mathematicians. And he was by far the most loyal consultant I ever had. After receiving his degree, however, he said (Aug. 26, 2004) in reply to my asking him what universities he was applying to:

> were i staying on my current track, one or two postdocs would be absolutely necessary (there are no examples otherwise in my field in the past 20 or 30 years that i know of). however, i am going to step away from physics for the time being and return to some fiction writing projects that have been on hold for several years (and start some new ones).

I encouraged him to stay with physics, citing Einstein's working in the Swiss patent office, and telling him how I lamented the all-or-nothing attitude that the mathematics and physics cultures bred into graduate students; I told him about the two recent PhDs in mathematics I had once talked to, who had decided to give up mathematics altogether when they failed to obtain tenure tracks at Princeton or at one of the other top math departments in the nation. He replied:

> i have tried to examine in myself the reasons for leaving the field, looking in particular for the sort of all-or-nothing attitude you speak of. i did have very high expectations going in, and to some extent they have been disappointed — research has not been nearly as rewarding as i had hoped, and i have yet to achieve anything i would call significant success therein. my primary motivation, however, is to return to what has been my interest for a long time now, namely writing. i spent two years in england reading literature as preparation for this eventual move, and it became increasingly clear throughout the phd process that the move needed to come sooner rather than later. in short, i found my self staring out the window writing in my head when i should have been doing physics. i imagined that perhaps physics would be the way i supported my writing habit, but the will to write took over too soon. so there i go. i still love thinking about and talking about physics, but i need to make a career elsewhere. it is always difficult to tease apart the feelings that go into such a decision, and for that reason, i really appreciate your comments.

One of the pleasures of a trip to New York City to visit Gaby was my morning coffee break at a little restaurant a few blocks from her apartment. It was called Le Pain Quotidien (The Daily Bread). It had an interior of bright wood, and a long, wooden table on one side, where diners could sit and eat communally. I would have a cup of their delicious, rich coffee, which was served in a white cup the size of a small soup bowl, and read in the company of the local upper class. One morning in 2005, the man sitting next to me asked what I was studying. I said, "tensor calculus" (I think the book was Dirac's *General Theory of Relativity*[1], which I found very difficult). He said he was a math professor and that he could "do that stuff with my eyes closed". I didn't ask him what exactly he meant by that because I could see that he regarded his purpose in life to be the demonstrating to one and all how brilliant he was, and I had no patience with mathematicians like that.

The following year, while obtaining help from Ed on tensor calculus, I told him what the mathematician had said, and asked how many of the basic operations in this calculus are algorithmic, meaning, can be performed by a computer. In an email of April 9, 2006, he said:

> no real math professor, at least at a top-notch institution, would say something like that. it's sort of like bragging about an ability to do addition. sort of embarrassing for the guy, even putting aside how rude he was. console yourself with the fact that he was insecure and had a reason to be....

On April 11 he added:

> The big computer programs (Maple, Mathematica) that do symbolic manipulation of mathe-

1. Princeton University Press, Princeton, N.J., 1996.

matical structures do a good job at handling tensors. Maple has everything built in; with Mathematica, you have to fiddle a little with their array structures. I used Mathematica a lot to compute curvatures etc for General Relativity problems.

So I felt a little better after yet another humiliation at the hands of the professional mathematicians.

A Graduate Student

Apart from Ed, the best consultant I had, for several months, was a graduate student in mathematics at a major university in California. He was from Eastern Europe; I will call him N. He was always overworked, and so needed frequent prodding to complete the readings (on both the Syracuse Problem and Fermat's Last Theorem) that I asked him to do, but he did a good job. It turned out that he was subjecting his PhD candidacy to a genuine risk by consulting for me: the professors made it clear to all the graduate students that outsiders were invariably crackpots, especially if they were working on problems that had so far not been solved by professional mathematicians. I naturally guaranteed N. complete confidentiality, except that one day I accidentally sent an email intended for him to the entire math department mailing list, which I used in order to advertise for consultants. The uproar was great enough to get me barred from placing any more ads. I pleaded with the man who was in charge of managing the mailing lists, offering to make a public apology. He refused. I explained how desperately I needed to be able to advertise for consultants, reminded him that I had been doing so for years without causing any trouble. Eventually he relented. N. continued to read what I asked him to for a few more months. Then he told me that the head of his department had notified all graduate students that any student found to be consulting on Fermat's Last Theorem, risked losing his PhD candidacy. Soon after, he said he had to stop because of the press of work on his thesis.

Another Me

On June 23, 2005, I received the following email:

> Just Googled on FLT[1] and came upon your very recent paper on the possibility of the existence of a simple proof of FLT.
>
> I'm one of those amateurs who never gave up looking. I've had a rationale that I have been looking at for 47 years and I knew there had to be a piece of the puzzle I was missing. I've been like the little boy digging in the big mound of horse manure saying, 'There's got to be a pony in here somewhere.'
>
> Well, I'm pretty sure I found the piece of the puzzle. It was hiding in plain sight for 350 years. For every exponent, there is a mathemetical [sic] relationship between x, y and z that, when you finally see it, you cannot believe that it is there.
>
> I've come up with an independent proof for n=3 which relies on the fact that 3 is a prime number, and then used the same methodology (congruences) to develop a proof for all prime val-

1. Standard abbreviation of "Fermat's Last Theorem"

> ues of n. A math professor at a local university has checked and rechecked the proofs and said that he cannot find any fatal flaw. He urged me to send it to the College Mathematical Journal for review and, hopefully, publication. It's 16 double-spaces [sic] pages.
>
> He didn't believe I had anything at first and was reluctant to spend any time on it, but something apparently intrieged [sic] him and he plowed on. When he finally realized that I had a new proof for n=3 he began to get a little excited, because all I needed to do was to do the same thing with a binomial expansion and I had a proof for all primes. I've now done that. He says that he will not believe it himself until he sees it in print. The CMJ [College Mathematical Journal] has had it since 6/13/05. Haven't heard anything yet, but I'm sitting on pins and needles.
>
> Except for the math professor and my immediate family, you are the only one who I've shared this with. That's because you never gave up on us amateurs. I'll let you know the outcome, one way or the other.
>
> Richard

We exchanged a few emails while he waited for the verdict from the Journal. Then, in December 2005 a referee informed him of an error. At first he thought that the referee had merely made up the error in order to keep him, an amateur, from receiving any credit for finding a simple proof, but then he realized the error was a real one. He asked me to take a look at the paper. I told him what I had told other amateurs who asked me to read their papers, namely, that I was not about to help someone else render null some thirty years of my own hard work on the problem. If he would guarantee me in writing that I would get shared authorship if my efforts on his paper resulted in publication, I would be glad to take a look at his paper. He sent me the guarantee.

There and then began an exchange that continued for several years, although with gaps of several months or more. Over the next few months, I learned a little about him. He was in his sixties, and had an invalid wife he had to take care of. He wrote:

> My professional experience has been primarily in Army facilities maintenance management - structures, housing, utility systems, roads etc. Budget wise, maintenance is always underfunded, the priorities going to training and weapons systems. When unprogrammed events pop up like Katrina, Bosnia, Irac [sic] etc. they steal the money out of maintenance and then never put it back. My job was to keep the infersturcture [sic] functioning with inadequate resources. This forced me to take short cuts as a way of life. That has been my training. So, I am interested in problem solving, and doing it in the simplist [sic] possible way. While I see and appreciate the beauty of pure mathematics, I am programmed to take short cuts. My last 3 page paper was a short cut. The formal paper was an attempt to explain the solution to a problem so that a layman like myself could understand it, someone who didn't know what a lemma meant."

His paper was clearly the work of a man who had tried hard to write in clear, simple prose. The trouble was that he had very little knowledge of the *form* in which mathematical statements are expressed. The only way I can convey to the layman the difference between writing that does not follow this form and writing that does is by making an analogy with algebraic equations. It is

perfectly possible to write, for example,

> Let the unknown quantity be augmented by five and then this entire quantity multiplied by itself twice. Now let the unknown quantity be multiplied by itself and the result subtracted from the previous product, and the whole then decremented by thirty-five, and set equal to zero. Find the unknown quantity.

This is not wrong, it is simply a long-winded and potentially ambiguous way of saying:

Solve the following equation for x:

$(x + 5)^2 - x^2 - 35 = 0$.

In 47 years, he seemed not to have mastered the equivalent of a freshman course in elementary number theory. And yet he was a thoroughly decent sort, and was clearly glad to have someone, after all these years, pay serious attention to his life's work. He did not argue when I pointed out errors in his ongoing attempts to find a proof. I made what effort I could to fix them.

The reader must understand that he and I were in a long line of losers who had tried to find some meaning for their empty lives by proving Fermat's Last Theorem. This line went back to the late 1600s, when Fermat's son, Samuel, first announced his father's Theorem in the course of editing his deceased father's works. Proving the Theorem held an irresistible appeal for amateurs because it was so easy to understand: The Theorem asserts simply that there does not exist a positive integer n greater than 2, and positive integers, x, y, and z, such that $x^n + y^n = z^n$. If n is 1 or 2, there exist lots of such positive integers x, y, z. For example if $n = 1$, we have $x = 8$, $y = 5$, and $z = 13$ because $8^1 + 5^1 = 13^1$. If $n = 2$, we have $x = 3$, $y = 4$, and $z = 5$, because $3^2 + 4^2 = 5^2$. All you needed to do to prove Fermat's Last Theorem was to prove that no such equalities could occur if the exponent n was greater than 2. Amateurs assumed that, because the problem was easy to understand, it therefore must be easy to prove. Nothing was farther from the truth. Some of the best mathematicians in the world worked at a solution for over three centuries. Fermat himself had proved the Theorem for the case $n = 4$, but it wasn't until 1770 that a proof (by Euler) for the case $n = 3$ was published, and that proof contained a major gap. However, the gap could be filled by a lemma that Euler already had published, and so he is given credit for the first proof of the case $n = 3$.

It wasn't until the mid-1800s that mathematicians succeeded in proving that the Theorem was true for all but three n less than 100. Still an infinite number of n to go! Finally, in 1994, the Princeton mathematician Andrew Wiles was able to prove the Theorem for all exponents n greater than 2. His proof was part of a much larger proof of another theorem, and Fermat's Last Theorem could legitimately be said to be merely a footnote to that larger theorem. But the amateurs did not give up. Since Wiles's proof covered well over 00 pages, the amateurs, including me, now set out to find a "simple" proof.

Various prizes were offered over the years for a proof of the Theorem. One was the Wolfskehl Prize, which in 1958 was worth about 7,600 German marks. One of the professional mathematicians who was charged with reading and responding to submitted proofs wrote:

> Nearly all "solutions" are written at a very elementary level (using the notions of high school mathematics and perhaps some undigested papers in number theory), but can nevertheless be

> very complicated to understand. Socially, the senders are often persons with a technical education but a failed career who try to find success with a proof of the Fermat problem. I gave some of the manuscripts to physicians who diagnosed heavy schizophrenia.[1]

The reader will recall the proud contempt of my co-worker, the mathematician Ian McGregor at HP Labs (first file of Chapter 2 in Vol. 3), for any amateur attempting to prove the Theorem. Having myself received a few crackpot "proofs" of the Theorem over the years via the Internet, I could not be unsympathetic with the attitude of professional mathematicians. And yet I made it a practice never to humiliate these poor souls. Instead, if their work was not written in anything close to standard mathematical format, I always replied, "It is entirely possible you have discovered something that no one else has thought of, but I'm afraid I don't have time to convert your paper into something that I can understand." If the paper showed the slightest sign of being coherent and readable, I offered to look at it in return for shared authorship, explaining that otherwise I would be risking helping someone to nullify the many years of my own work on the problem. As of 2010, I think a total of three people had taken me up on my offer. One of them, after I had told him of several errors, and made numerous suggestions for the improvement of his argument, suddenly stopped replying to my emails, and didn't reply even after I wrote him threatening to state, on my web site, his name and the fact that he had gone back on his word. He didn't reply, but I never carried out the threat.

A Flicker of Hope

In November of 2005, I made a series of corrections to errors in my Syracuse Problem paper. N., the graduate student described above, read them over, sent an email with "Great", "Good", "OK" following each correction. Then he said:

> OK. I think that's all. I still haven't found a reason why I shouldn't believe the first possible proof but I'll go over it once again and I'll let you know.

He never raised any further objections. This was the first time anyone had said words to the effect that one of my solutions to the Syracuse Problem might be correct.

Another Flicker of Hope, Soon Extinguished

I continued to work on papers concerned with Fermat's Last Theorem and the Syracuse Problem. I advertised for consultants on the electronic bulletin board of a major university in the area and once in a great while I got a response. One was from an Indian student who was in the final months of completing his PhD thesis. But he had discovered errors in one of his proofs, and so it was only months later that his work on the thesis was finished, and he was awarded his PhD. Finding a job proved difficult, but by fall he had a one-year contract to teach mathematics at a small college in Southern California. By then he had had time to look at one of my Fermat papers. On Oct. 26, 2005, I received the following email:

> Subj: VERY IMPORTANT!!! READ ASAP
> Date: 10/26/2005 2:28:51 PM Pacific Standard Time

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1. Schlichting, F., letter of 3/23/74 quoted in Ribenboim, Paulo, *13 Lectures on Fermat's Last Theorem*, Springer-Verlag, N.Y., 1979, p. 16.

From: ...
dear john,

i have spent the last hour looking at p.28-29. those pages appear to have no contradiction. which means you have a good chance at FLT. i say chance because i don't think that the lemma 0.85 on p.39 is right. at least the proof seems faulty.

here is the correct version.
...

now that i have written the above out, it seems very similar to what you did. you had some (what i assume to be) typos in your proof.

now the lemma is fixed (i think).

let me know what you think. i applaud you (maybe a bit early... but it looks okay to me) that you have a proof.
...

So at the age of 69, I had received an email from a professional mathematician congratulating me on having found a simple proof of a theorem that had baffled the best mathematicians for more than 300 years. In the hours following, I walked around saying to myself, "See? You were justified in believing in yourself! You knew you would win despite the years of anguish and self-contempt. You have a right to live after all!"

Of course, good practice demanded that I obtain corroboration of my achievement. So I sent an email to Ed the Physicist and asked him to take a look at the proof. It, like all my papers, was in a paper on my web site. Within a day he replied, saying that my argument had a fundamental flaw, which he pointed out. There was no denying that he was right. So I had no right to live after all.

A Nasty Professional

Around 2001, a young mathematician who had just received his PhD and his first university appointment, saw the offer on my web site to pay $500 to anyone who could find an error in my solution to the Syracuse Problem. He wrote a cordial email saying he would bring the offer to the attention of graduate students and a few faculty members, adding, as I recall, that he didn't have time to check the solution himself. In April, 2006, the Indian mathematician whose email is quoted in the previous section had to end his consulting for me because of lack of time, and since by then I was receiving no reply to my electronic bulletin board ads, I decided to write to the mathematician who had contacted me around 2001.

Subj: Syracuse Problem
Date: 4/26/2006
To: ...

...:

You may remember that we communicated in early 2001 about my paper on the Syracuse Problem.

Since then, I have done a great deal of work to develop the basic ideas in that paper... , all the while paying graduate students to critique my efforts.

Now, at least one graduate student believes I have a solution. Since you seemed to have a genuine interest in my approach, and since I definitely need the help of a professional mathematician at this point, I would like to offer you shared authorship to help me to bring what I have into publishable form.

I'm sure you realize that, if I really do have a solution, shared authorship could be the coup of a lifetime.

I ask only that you read the rest of this email before replying. I will do my utmost to be brief.

First I will outline my strategy for solving the Problem, then I will give what I hope are the appealing conditions of my offer (including a very low requirement of your time), and then finally I will list the pages in my paper that you would need to read to understand the proposed solution.

<u>Outline of Strategy</u>

Here is an outline, informal of course, of the strategy underlying my possible solution. So far, out of the many dozens of grad students who have gone over the paper, none has said he has seen this strategy attempted elsewhere.

...

"Conceptually it is that simple, although as you can imagine, the machinery to make it all work is a bit more complex.

<u>"Conditions of My Offer of Shared Authorship</u>

You would be allowed to withdraw at any time;
I would guarantee complete confidentiality: no one would know that you are working on the paper unless you told them;
You would be paid any reasonable hourly fee you desired for the time you spent;
You would be allowed to put in as little as one hour a week;
You would be free to continue to work on your own Syracuse research.

Needless to say, I can supply references as to my ability to work cordially with others, and, of course, as to my not being a crackpot.

<u>"Pages Describing Proposed Solution</u>

The pages that need to be read to understand the proposed solution are the following...
I urge you to take a little time to think over my offer before you reply.

Hoping that all is going well with your research and teaching.

Regards,

-- John Franklin

Almost immediately I received the following reply.

Subj: Re: Syracuse Problem (2)
Date: 4/26/2006 12:14:23 PM Pacific Standard Time
From:...

Hi,

I'm not interested in taking a look at your strategy for the Syracuse problem.

Good luck, ..."

I was beside myself with rage and self-contempt and shame at this rejection by a young mathematician who had once been civil toward me. I wrote to Gaby, "Do you wonder that I live much of the time in suicidal despair?" I couldn't bear not knowing why he had so curtly dismissed my labors, so I wrote to him:

Subj: Syracuse Problem (2)
Date: 4/26/2006
To: ...

...:

I certainly respect your decision, but it would be a big help to me if I had some idea of your reasons. Below are a few that occur to me. You only need to list the appropriate numbers in a return email.

You have my word that I will not bother you again, regardless which numbers you give, or if you don't reply to this email.

1 -- You think I am a crackpot.
2 -- You simply can't believe that I have a promising strategy.
3 -- You have your own solution to the Problem.
4 -- You don't have time to work on another paper.
5 -- Something I said in 2001 offended you.

Regards,

-- John Franklin"

Again, his reply was prompt:

Subj: Re: Syracuse Problem (2)
Date: 4/26/2006 8:57:27 PM Pacific Standard Time
From: ...

I don't like to use the word crackpot. My opinion is that if our [sic] strategy actually works, you would have known it by now, so probably it doesn't work.

I Attempt to Write in the Approved Style

As I said earlier (in the section "I Was Meant to Be an Amateur" in the first file of Vol. 5), I always felt that in mathematics, what counted most was *ideas* —

ideas like the one underlying the calculus, namely, that if you want to find the area under a curve, you can line up a bunch of vertical rectangles side by side under the curve, and compute their area, which is easy; then you can make the rectangles thinner so you can put more of them in the same space, and again compute the area; and proceed in this way so that "in the limit" you have the area under the curve (calculus gives a method so that you don't have to repeat the process an infinite number of times);

ideas like the fact that if you want to know if the number of things in one set is the same as in another set, you don't have to count them, all you have to do is see if the things in the first set can be matched one-for-one with the things in the second set; if they can, then both sets have the same number of things (even though you may not know what that number actually is); if not, then they don't (no counting needed!);

ideas like Cantor's marvelous, and simple, proof (based on the previous idea) that there are more decimal numbers than there are fractions (rational numbers);

ideas like the one that lies at the basis of probability, namely, that the probability of something is the number of ways the something can happen divided by the number of ways anything (in the set of things under consideration) can happen;

ideas like jagged lines and rough surfaces that have the peculiar property that if you look at a piece of them under a powerful magnifying glass, the piece looks just like the big piece (self-similarity, the basis of fractal geometry);

and many others.

Of course ideas are not in themselves mathematics, they only become mathematics when they are placed on a firm logical foundation, but for me and at least a few other mathematicians, the idea comes first.

""Mathematics is not about symbols and calculations. These are just tools of the trade — quavers and crochets and five-finger exercises. Mathematics is about *ideas*. In particular, it is about the way that different ideas relate to each other."— Stewart, Ian, *The Problems of Mathematics*, 2nd ed., Oxford University Press, N.Y., 1992, p. 10.

"Logic is the means by which I convince the world of the correctness of my intuitions." (Jacques Hadamard, I believe).

Surely, I kept thinking, there will be a mathematician who will recognize the originality and insight of my ideas, and disregard my inept writing style, knowing that that is something that can be corrected once the concepts and the arguments are correct. I could simply pay someone to convert my papers into the approved style, as I had done on several occasions in the past.

But no such mathematician appeared. Instead, overworked mathematicians and graduate students used my inept style as a way of avoiding trying to understand the ideas I had in mind — anything to avoid that kind of labor! Criticize the style and find an error in the logic and you are done! You can preen yourself on your rare kindness to those less fortunate (amateur mathematicians) and do it with a minimum of time and effort!

In 2006, I decided that, no matter how much I hated wasting my time on what I regarded largely as pedantry, I would have to start learning how to write in the approved style, if only to force my few readers to do what they were supposed to do. So I bought four of the leading style guides and began working through them, building an annotated index to all four as I went. The guides were *How to write mathematics*, by Paul Halmos et al., *A Primer of Mathematical Writing*, by Steven G. Krantz, *Handbook of Writing for the Mathematical Sciences*, by Nicholas J. Higham, and *Mathematical Writing*, by Donald E. Knuth, Tracy Larrabee, and Paul M. Roberts. Halmos and Knuth I regarded with particular contempt because of their pedantry, but I tried to spend at least an hour a week on these books, and on slowly correcting the style of the papers I was working on.

One of the above-mentioned style guides contained several exercises — poorly-written proofs that the reader was asked to improve upon. I thought I found a logical error in one of these proofs and wrote a cautious email to one of the authors, an imperious woman I had worked with at HP in the eighties, and who I have no doubt always looked down on me. She never replied. So I wrote to the author himself.

He sent back a reply showing that I was mistaken — in fact, that I had made a truly stupid blunder. By today's standards his reply was civil, but one sentence cut me to the core, and that was where he said that I should get a textbook on basic mathematics and learn how a set is described. (I had known that for thirty years.)

I hated myself more than I can possibly express. I felt again the contemptuous look of the woman I had worked with, the look that said, "John, you really aren't very bright, are you."

One thing that surprised me in going through the books was their emphasis on clarity. Throughout the eighties and nineties, as I struggled with the few math problems I had chosen to work on, I worried that my writing might not be *difficult enough* for the professionals. I worried that, if they found that my writing was easy to understand, they would dismiss what I had to say. This concern was not as bizarre as it may sound. A friend of Gaby's who worked for many years as a mathematician at Bell Labs, one of the nation's leading research labs, told me that if a mathematician in the Labs published a paper that others outside his specialty could understand, they dismissed it as not being important work. But the experts said that my goal should be to help the reader as much as possible, and this I now set as my goal.

Another Final Blow

On 9/7/2006, I submitted my paper on the Syracuse Problem to the same journal that had rejected it several years earlier.

On 9/18/2006, I received the following email:

Dear John Franklin,

Thank you very much for the submission of your paper, "A Solution to the Syracuse Problem," to [journal name].

Below I have appended a message from [name], the editor-in-chief. We regret that we cannot accept your paper at this time in its current form.

We appreciate your consideration of [journal name].

Sincerely,

[managing editor's name]
----------"

The appended message was:

Dear Dr. Fraenklin[sic],

I have been going over your submission. It is certainly an intriguing result, but I am afraid that your logic is flawed.

As you point out, the argument seems to indicate that there are no examples of the results, which is patently false.

Unfortunately, the paper is written in an [sic] style that makes checking very hard.

Mathematical writing has certain standards designed to make checking possible. In particular, the definitions have to be made with precise language. Your definitions of ... are done only with examples.

The whole argument looks somewhat suspect since you seem to conclude from it also something which is false. The fact that this is a contradiction only means that the argument is incomplete.

I know that [name of former editor of journal] put forth a lot of effort helping with this paper.

Best wishes.

[name of editor-in-chief]"

I was shocked to find that the editor-in-chief's criticisms made no sense. My email attempting to explain this to him is given below. But first I must remind the reader, who may be inclined to

regard anything I say as the defensiveness of an author who can't bear criticism — I must remind the reader that I was spending several thousand dollars a year to have people read and critique my papers (usually paying at a rate of $50 an hour). I doubt if any one of those readers would say, "Franklin can't take criticism." Of course, if I felt that a reader hadn't understood an argument, I would say so, and I would try to make the argument clearer. But there are dozens of emails of mine that say, in so many words, "You are right. My argument doesn't work." It was clear to me that the editor-in-chief's criticisms arose from a superficial perusal of the paper with the sole purpose of finding reasons to justify a decision of rejection that the editor-in-chief had made before he even looked at the first page. The following is my email to the managing editor and to the editor-in-chief:

> Dear Dr. [name of managing editor]
>
> Naturally, I am disappointed about your rejection of my paper, "A Solution to the Syracuse Problem". I am even more disappointed about the reasons that [name of editor-in-chief] gave for the rejection, since I don't understand two of them, and since one I think I can legitimately disagree with. My (respectful) reply to [name of editor-in-chief] is given below, in the form of bracketed comments to his email. If you feel it is appropriate to forward the reply to him, then I hope you will do so. Please assure him that I will not reply to his response without his permission.
>
> My paper deserved far better. Frankly, I am appalled.
>
> Thank you for considering the paper.
>
> Best regards,
>
> -- John Franklin
>
> [My responses to the editor-in-chief's comments are given below in square brackets.]
>
> Dear Dr. Fraenklin[sic],
>
> I have been going over your submission. It is certainly an intriguing result, but I am afraid that your logic is flawed.
>
> As you point out, the argument seems to indicate that there are no examples of the results, which is patently false.
>
> [I have no idea what you mean by this. What "examples of the results"?]
>
> Unfortunately, the paper is written in an style that makes checking very hard.
>
> Mathematical writing has certain standards designed to make checking possible. In particular, the definitions have to be made with precise language. Your definitions of ... are done only with examples.

[It is flat-out not true that the definitions are done only with examples. Furthermore, these definitions, and indeed all definitions in at least the first seven pages, have been read and found acceptable by well over a dozen mathematics graduate students and several professional mathematicians. Certainly if any of these readers had felt that the definitions of ... were done only with examples, they would have told me, and I would have changed the definitions appropriately.]

The whole argument looks somewhat suspect since you seem to conclude from it also something which is false. The fact that this is a contradiction only means that the argument is incomplete.

[I simply don't understand what this means. The proof of the Syracuse Conjecture is by contradiction. In the Remark that follows, I give another proof by contradiction, easily derived from the first. Both contradictions arise from the assumption that counterexamples exist.]

I know that [name of former editor of journal] put forth a lot of effort helping with this paper.

[He did several years ago in a much earlier version of the paper. I am deeply thankful for his help and have expressed my gratitude in the Acknowledgements. But he did not see the version of the paper I submitted because he was too involved with other work.]

[I cannot conclude this email without a few additional words. In brief, I don't think the paper got anything remotely like a fair review. If the definitions of... were so poor as to prevent you from understanding the rest of the paper, why didn't you stop there and say so? So I must assume that you felt that despite my poor writing style, you understood the intended meaning of the definitions.]

[You mentioned no other objections to definitions (or to the proofs of the lemmas). So I must assume that you felt that you understood enough to examine the proof of the Conjecture. But then, if you found a flaw in my proof by contradiction, why didn't you point to the exact place in the proof where the flaw occurs?]

[My strong impression is that you made no effort to understand the argument on which the proof is based. (One can understand an argument even if it is flawed.) If you had, you wouldn't have needed to resort to language about "examples of results".]

[Finally, I need to tell you that well over a dozen mathematics graduate students, and several professional mathematicians have gone over earlier versions of the paper in the past few years. I assure you I have no problem in being told that I have made an error, or that my exposition should be changed.]

[In connection with the latter, I have made it a rule to spend an hour a day reading, re-reading, and attempting to apply, the advice contained in Higham's "Handbook of Writing for the Mathematical Science", Krantz's "A Primer of Mathematical Writing", and Knuth et al.'s "Mathematical Writing". In addition, as Dr. [name of managing editor] will tell you, I am con-

stantly attempting to find writing style consultants.]

-- John Franklin

On 9/21/2006 I wrote:

Dear Dr. [name of managing editor]:

Before I terminate communication with the editorial staff of [journal name], I feel I have to do what I can to prevent another author from receiving the kind of treatment I did, because, in the long run, such treatment can only hurt the reputation of the journal. (I will certainly never recommend the journal to any mathematician or computer scientist.)

Let me begin by taking a wild guess as to what really happened: you received a paper from an unknown author claiming a proof to a very difficult unsolved problem. You were skeptical about the claim -- as you had every right to be. You then looked through your records and found that in the late nineties, I had submitted a paper claiming a proof, and that a referee had rejected it, and that, after being given a chance to resubmit it, I withdrew it.

You decided that you could not risk wasting a referee's time again. What to do? What you could have done was to write me and simply have said that, on the basis of my past history with the journal, you felt you could not give me the benefit of the doubt a third time, and therefore had to reject the paper. But if I could find a professional mathematician who was willing to state to you that he felt I had in fact solved the problem, you would then be glad to reconsider the paper.

That would have been the honest, the decent thing to do. I would have been sad, of course, about your decision, but I would not have objected to it.

Instead, your editor-in-chief decided to fake a reading of the paper and come up with a few bogus reasons for rejecting it (one of them patently not true, two of them incomprehensible and revealing zero understanding of the solution).

I plead with you, for the sake of the reputation of the journal, and for the sake of common decency, to make it a rule from now on to be honest with authors like me. That's all that is required. A policy of deceit and humiliation does no one any good.

Regards,

-- John Franklin

On 9/22/07 I received the following email:

Dear John Franklin,

It goes quite too far to accuse us of a “policy of deceit and humiliation.” I forwarded your pre-

vious message to [name of editor-in-chief], who replied:

Upon a second reading, it became clear that [journal name] is not the proper journal for your paper, because it does not contain any experimentation.

Further, if you do have some professional mathematicians who have verified the proof, it would be good to mention them in the acknowledgments.

I find myself disinclined to explain our editorial policy because of the hostility of your latest message. However, I will state that [name of editor-in-chief] did not "fake" a reading of your paper, and to assume that he did so because you disagree with his comments seems to reflect some defensiveness on your part.

Sincerely,

[name of managing editor]

That same day I responded:

Dear Dr. [name of managing editor]

Thank you for responding to my email, and for forwarding Dr. [name of editor-in-chief]'s comments. Had he taken a few more minutes with the paper the first time, he would have noticed it contains no experimental data, and could then have rejected the paper for a perfectly legitimate reason.

Re my "defensiveness": as I emphasized in my reply to Dr. [name of editor-in-chief]'s comments, I have done my utmost to find readers of earlier drafts of the paper. These readers have pointed out errors and I have fixed the errors. I do not have a problem with persons criticizing my work! But it is always abundantly clear that the criticisms are based on an actual reading of the parts of the paper I have asked to have read.

Since receiving Dr. [name of editor-in-chief]'s rejection, I asked a mathematician with a high reputation for his expository skill to look over the first few pages of my paper. I did not mention Dr. [name of editor-in-chief]'s comments about the definitions of... -- in fact, I did not mention [journal name] at all. The mathematician pointed out a number of improvements that could be made, but none of them had anything to do with the definitions of these two terms!

In the last analysis, you will have to make up your own mind about Dr. [name of editor-in-chief]'s objections. I repeat: I spend a major amount of time throughout the year asking for and receiving criticisms of the paper, so I think I have become very good at judging if all or part of the paper has received a fair reading or not.

Thank you again for your email.

Regards,

-- John Franklin

I immediately began searching for another journal. I don't recall how, but I found one that, from its title, seemed ideally suited for my paper. Furthermore, on searching the list of editors, I saw that one of them was a mathematician whose work I had done my master's thesis on, many years before, and for which I had the highest respect. So I sent an electronic version of my paper to him, mentioning in my email how delighted I was to find that he was an editor of the journal, and telling him about my extensive study of his work. In keeping with the journal's policy, I sent a copy of the paper to the editor-in-chief. I quickly received an email back from the editor-in-chief — "ok, good luck with the referees" — but received no acknowledgment from the editor. I assumed this was because he knew the editor-in-chief would send the acknowledgment.

I waited three months, pleased that the paper had survived at least the initial perusal by the editor and probably a referee. But I felt I should be sure that, in fact, the editor had received the paper, so I wrote him, apologizing for bothering him and, to reduce that bother to a minimum, asking him to inform me only if my paper was no longer under consideration. (If that were the case, he by rights should have informed me already, but I wanted to give him the benefit of the doubt.) I received no reply. I waited another two months, then wrote the editor-in-chief asking him how long it typically took for an author to receive a decision on his paper. The editor-in-chief replied, "when you choos[sic] an editor, you may ask him: usually 4-5 months".

I waited another month, wrote the editor-in-chief again, telling him that I had no heard a word from the editor in more than six months. He replied:

> both authors and editors have full autonomy in [name of journal]: the first may chose any editor, the editor makes up autonomously his decision. [editor's name] is a top scientific personality, used to a very high standard, he will surely decide and act for the best

I began writing to a professional mathematician who was a friend of Gaby's, and to a recent PhD in computer science, and to a former PhD candidate, asking for their advice. The professional mathematician and the recent PhD said I should wait another month, then write the editor again, asking for a status report. The former PhD candidate said I should wait longer; that no news is good news; that sometimes a decision on a paper is not reached for a year or two.

I waited another month, then wrote the editor again, copying the editor-in-chief, saying that if I didn't receive a reply, I would have to assume that the paper had been rejected, and that I was free to submit it to another journal. When I didn't receive a reply from either of them in five days, I knew that it was all over for me.

I wrote a final email to the editor-in-chief, pleading with him to tell me what I had done wrong, so that I wouldn't repeat it with the next journal. I told him that if the editor regarded a claimed solution to a hard problem that was submitted by an unknown author to be worthless, then all he had to do was send me an email saying that my style did not meet the journal's standads, and so the paper had been rejected, and that the decision was final. Surely he could have done that much.

The editor-in-chief never replied.

It was the final blow. To be deemed so inferior, so much a crackpot, that editors do not feel your papers justify even the dignity of a one-sentence rejection, filled me with unbearable shame

and self-contempt. I had been living in the worst, the most pitiable self-delusion for 30 years, but now the truth had been conveyed to me. I went from a daily life of suicidal depression to one of each day being burned at the stake.

As I lay tossing and turning in bed, I tried to figure out what had happened. The answer I came up with was this: probably there was a grapevine among journal editors, and so when my paper arrived, the editor checked it and found my response to the rejection by the previous editors. He then went to my web site, saw that it contained papers on some of the hardest problems in mathematics, including Fermat's Last Theorem, and that it also contained a book that strongly criticized the contemporary academic mathematics culture (though written by a person with a different name) and decided that I didn't even deserve the few seconds it would have taken to send a one line rejection email.

My opinion of the editor's mathematical work did not change: I kept in mind what I considered one of the fundamental abilities that intellectuals needed to have, namely, the ability to accept the fact that brilliant work is sometimes done by first-class sons-of-bitches.

Ironically, during those terrible days, I wrote an email to another world-famous mathematician I had admired for many years, telling him how much I had enjoyed his latest book, and offering a few additional thoughts of my own. He wrote back:

> "...let me say that I've read your e-mail word by word, and your remarks are all remarkably perceptive. Of course, we are both going against the current zeitgeist/esprit des temps/spirit of the times. But who cares!
>
> "You may not be a mathematician, but you are my kind of intellectual. Ignore the madness of the world... "

Why he made a point of saying I was not a mathematician, I don't know, but his praise cheered me briefly. But only briefly. Then the Life Unbearable resumed.

A Mathematrician Who Cared About Amateurs

In June 2007 I did a Google search on "amateur mathematician", got the expected hundreds of hits. I started going through them, and came across a web site that offered advice to amateurs — advice that had been written by a professional mathematician. There were probably not ten professional mathematicians in the entire world who had anything but contempt for amateurs. I assumed that one explanation for this exception was that, although this mathematician had a PhD from one of the best mathematics departments in the country, he was not an academic, but instead worked for a software company.

He said he had no time to read any of my papers, so I picked his brain for advice on dealing with the culture of the professional mathematician. And, on two occasions, he resolved a dispute I had with Ed the Physicist, my most loyal reader. In both cases, he decided I was right.

A Real Son-of-a-Bitch

In January 2008 I received the following email:

> Subj: the link to your papers posted on the [name of web site] discusion [sic] page Date: 1/18/2008 9:02:25 PM Pacific Standard Time

From: ...
To: [My name as derived from my email address]

...where I have added my comments.

There was no signature nor indication of professional affiliation. There was also no link in the email, so I wrote him back asking him to explain what the email meant. He sent a long reply from which I gathered that on a certain web site someone had referred to my Syracuse Problem paper, and that in the course of reading part of my paper, the author of the email decided that my paper was wrong, and on the web site he had posted an extensive explanation why.

I eventually found my way to the web site, and was shocked to see that his critique began with some nasty language indicating his contempt for my entire approach. He then argued that my proof was wrong because it applied equally well to functions related to the Syracuse function in which counterexamples were known to exist, and thus that my paper proved a falsehood. He provided several pages of data and equations about these other functions. He then took me to task for not dealing with all the generalizations of the problem. In other words, any attempt at solving the Problem was invalid unless it provided a complete analysis of all similar functions.

He then rewrote several of my definitions so that they would fit the general case, then, using these revised definitions, argued that my proofs of lemmas were invalid. This is of course a completely illegitimate practice. My first impulse was to reply to his criticisms in kind, using even nastier language, but Ed the Physicist and one of my computer consultants advised against this, urging me instead to post on the web site a calm, reasoned, rebuttal of his remarks. This I did. I said that mathematics would come to a standstill if a new rule went into effect that every proof must be accompanied by a proof that it does not also prove a known falsehood in a similar problem, whatever "similar" might mean. Several months later, I received another unsigned email claiming that my proof was invalid, and using the same arguments, if even more frenetically, as the first author had. I strongly suspected the email was written by the same man.

He was not a crackpot[1]. He was simply a man who was obsessed with a problem but who didn't have any good ideas for solving it. He reminded me of my old nemesis (see section, "My Old Nemesis" in the first file of this volume). . If you can't solve the problem yourself, if you don't have a good idea, then become an expert, and make sure that no one else solves it either — or, at the least, make sure that no one else solves it except on your terms, which include that the author of any proposed solution has the proper credentials, and demonstrate wide reading in the literature on the problem. My nemesis published a number of respectable papers. (There is no limit to the publishable papers that a competent mathematician can come up with while circling around a problem he can't solve.) He also developed a reputation for catching errors in other proposed proofs and, as I can attest, in keeping out of consideration anyone that he felt did not fit the mold of a professional mathematician with proper credentials.

In 2009, a graduate student told me that he had written to my nemesis and, among other things, asked for his opinion about my approaches to the Problem. He was told that my proofs

1. As it turned out, his challenge regarding the related function that had known counterexamples, when it was repeated by a mathematician, led to my realization that my strategy, in fact, had a fundamental flaw. But in the process of preparing an announcemnt of that fact to be posted on my web site, I discovered a proof that, as of this writing, I think, and at least one graduate student thinks, solves the Syracuse Problem.

had errors (which was true, although the important question is not, Does a proof have an error?, but, Does the idea on which it is based have promise?) and that he didn't see why, if I believed I had a proof, I would bother to supply other proofs (whch I usually did).

In reply to this first opinion, I must quote from an email sent me by the above-mentioned mathematician who cared about amateurs. I had apologized for sending him an unconvincing argument.

> Don't worry about it. I've certainly sent equally unconvincing proofs to my collaborators. I really like Norber [sic] Wiener's comparison of mathematics to chess: in chess, your skill as a player depends mainly on the weakest moves you make, and one mistake can lose the entire game, no matter how well you played the rest of the time. By contrast, in mathematics success comes from your best ideas, and nobody cares how many attempts it took you to succeed.

My nemesis"s second opinion revealed an appalling ignorance of the history of mathematics on his part, because the greatest of all mathematicians, Karl Friedrich Gauss (1777-1855), produced at least seven proofs of what he called "my golden theorem" (a theorem known technically as "quadratic reciprocity"). The desire to find other proofs of difficult or surprising theorems is, I daresay, instinctive in many of the best mathematicians, because new proofs bring new and deeper understanding. My nemesis also discouraged the student from working on the Syracuse Problem because "it's a waste of time." Which I suppose is true, if you haven't got any good ideas. The lack of depth and creative ability of this man were revealed once again.

I know the reader will say, regarding my nemesis's requirement that researchers know the literature on a problem, "What can be more rational — if you don't have a good idea for solving a problem — than to try to read everything that has been written on the problem?" The reader will remind me that there are journals that will not even send a paper out for refereeing unless the author has demonstrated, in his list of references, that he knows the literature on the problem. In the search for a cure for cancer, people try to read as much as they can about past attempts. I can only say that I have never been able to work like that. Temperamentally, I am unsuited for such a plodding approach. Late in life I found that at least one great mathematician, Henri Poincaré (1854-1912), had a similar aversion to plowing through all the previous literature on a problem. I can summarize my attitude by saying: If you haven't got an idea that your aesthetic sense tells you is a good idea, then read the literature. If you do have a good idea, then don't waste your time reading the literature except as necessary to develop your idea.

Another Shameful Refereeing Job

After the year-and-a-half ordeal with the journal whose editor didn't respond to authors' emails, the above-mentioned mathematician who had sympathy for amateurs recommended another journal. And so, after fixing all the errors the previous referee had found, I submitted my paper (May, 2008). Unlike the previous journal, this one sent me an immediate acknowledgment of receipt of the paper, with an identification number for it. I was so overwhelmed that I wrote to the editorial office expressing my appreciation, and even received a thank you email from one of the office staff.

But in three weeks an email arrived from the editor saying the paper had been rejected. He sent a copy of the referee's report. It was the same old story and then some. The referee had given the paper the usual superficial reading, but in addition he or she had decided to redefine the Syra-

cuse function according to another definition that was well-known in the literature, then base his criticisms on that redefinition. Just as the reader with the nasty language had done. The referee openly admitted he or she had not understood several key concepts, as was clearly evident by the outright falsehoods he claimed the paper asserted. I immediately wrote an email to the editor.

> Dr. [name of editor]:
>
> First, I would like to express my appreciation for the promptness with which [name of journal] reviewed my paper....
>
> Naturally, I was disappointed to learn that the paper had been rejected, but when I read the referee's report, I was more than disappointed -- I was astounded. Although the paper has been read and commented on by dozens of qualified readers, never before has anyone read the paper with such a lack of comprehension as that of the referee.
>
> My first reaction was that somehow I must have sent a garbled version of my paper, with pages missing, and so I immediately checked the file I had sent. But it seemed to be entirely intact.
>
> I fully realize that your rejection decision must be final. Nevertheless, I simply cannot allow the matter to end without sending you a detailed response to some of the referee's comments. A glance at the attached .pdf file will reveal that I am not arguing about trifles. The referee admits (see start of file) that he or she did not understand [two] of the most fundamental and important concepts in the paper ... , even though all this is spelled out clearly and precisely on the pages indicated in the attached file.
>
> Then the referee insists on basing his or her criticisms on a definition of the Syracuse function that is different from the one used in the paper! Numerous other misconceptions and flat-out false statements are cited in the attached file. Perhaps most serious of all, the referee gives no sign of having understood the basic idea on which the proof of the main theorem is based.
>
> Obviously, you are under no obligation to forward the attached file to the referee, or read it yourself, or have any other member of the editorial staff read it, but I am hoping that someone will at least spend a few minutes seeing just how bizarre -- there is no other word for it -- the referee's review of my paper was.
>
> Best regards,
>
> -- John Franklin

I didn't know if I should be happy or sad that the referee also said in his or her report that the structure I had discovered that underlay the Syracuse function was "too simple" to be of any use in solving the Problem — not that it was wrong but that it was too simple. This from a person who had not understood most of the paper to begin with.

The reader may perhaps be wondering why journals hadn't long ago instituted the practice of not revealing to the referee the name and affiliation of the author(s) of each paper, thus ensuring

that the referee's decision would be based on the merits of the paper alone. The following quote gives an answer:

> The state of refereeing is revealed by the reactions to a recent[1] decision of the American Mathematical Society. Up to 1975 all papers submitted for publication in the several journals supported by the Society were sent to referees with the names and affiliations of the authors recorded on the papers. The Society decided to try, for one of its journals, blind refereeing, that is, submitting the paper to the referee without the name and affiliation of the author. The protests of referees and even of two of the associate editors of that journal were vehement. They pointed to the thanklessness of the work, the difficulty in finding competent referees, and the problem of judging the correctness and worth of a paper. In the ensuing debate, partly through published letters, the opponents of blind refereeing admitted that the name and affiliation of the author helped immensely in the refereeing process. What these opponents were really saying is that they were not judging papers on their merits but were relying on the reputation of the author and his institutional affiliation to aid in determining the correctness and value of his work. If one may judge by the protests, many referees used no more than this information to make their decision. This debate brought into the open all the weaknesses of the refereeing process. — Kline, Morris, *Why the Professor Can't Teach*, St. Martin's Press, N.Y., 1977, p. 63.

The Fundamental Disappointment

When, in the early eighties, I began working on the most difficult math problems I could find, and came up with several ideas that I felt would solve them, I thought, "The mathematicians will see immediately what I am driving at and will finish the work in return for shared authorship." As the reader knows by now, nothing could have been further from the truth. The mathematicians did *not* see what I was driving at (the two main reasons being my abominable writing style and my lack of credentials), nor did they have any interest in doing so. In 2009 I came across an observation by the physicist Lee Smolin that would have done me enormous good if it had been uttered 25 years earlier and if I had known about it then: "No one but you can develop your ideas, and no one but you will fight for them." [2]

I am always bothered when an intellectual's tenacity is admired, because I strongly suspect that people imagine he somehow knew he was right, and that is what drove him on. Equally important, perhaps more so, at least in some cases, is the power of vanity and the determination not to let years of work come to nothing, and the craving for revenge against those who slighted him.

A Couple of Briefly-Useful Undergraduates

In early 2009, two undergraduates wrote me an email saying they had found an error in my Syracuse paper. They were right. I repaired it, gave them credit in the paper for having reported the error (after checking with them that it was all right to mention their names), then offered them a chance to do some consulting. They accepted. But after a week, one of them dropped out, with no reason given. The second said he wanted to continue and asked only $15 an hour, a very low rate. I offered him $20. But he didn't get back to me with his comments by the deadline we had

1. as of 1977.
2. Smolin, Lee, *The Trouble With Physics*, Houghton Mifflin Company, N.Y., 2007, p. 291

agreed upon (I asked for as little as one hour a week of work). I offered to double his hourly rate if he would guarantee a five-day turnaround. He agreed, but always missed the deadline. I began reminding him the day before his comments were due. (I was privately furious that he couldn't even maintain a simple schedule.) He missed another deadline. I wrote him. He said that at first he had been optimistic about my proof, but now he was not.

You Gotta Have a Sense of Humor (1)

In fall of 2009 I placed a notice on one of the math department mailing lists that I had finally been allowed access to after years of pleading and cajoling. I said I was looking for someone to read a paper-in-progress, and that I would pay any reasonable hourly fee, etc. I received two replies, both from Asian graduate students. I told them where they could find, on my web site, the proofs I wanted checked, and gave them the terms of a potential contract, which included that they spend at least one hour a week on the reading and that they send me their comments at least once a week. We agreed on a fee ($50 an hour), I gave them the go-ahead. Incredibly, in less than a week they both replied, giving thoughtful comments, albeit in broken English, on the background material in the paper that was needed to understand the proofs. Both students lectured me in no uncertain terms on the need to improve my writing style. When the students got to the proofs themselves, they had difficulty understanding the concepts, which is not surprising, since almost certainly the concepts were unlike anything they had run into before. The students made suggestions as to ways to better express what I had not said. And there were again strong urgings, in broken English, that I learn to write in a professional manner.

Prof. X Agrees to Check My Proofs

The reader may recall Prof. X, from the first file of Vol. 5 — the mathematician who for several weeks in the late nineties consulted with me on my paper. Thereafter I tried not to bother him more than twice a year so that I wouldn't wear out my welcome. He usually said he had no time to spend on my paper at the moment, and he never commented on my repeated offer of shared authorship if he helped me get the paper published.

I wrote him in June 2007, more than a year since I had bothered him last, asking him if he would take a look at the latest version of my paper. I was surprised to receive a reply saying that he might have time in late summer or fall to return to the paper. In my email expressing thanks, I asked him if he had any thoughts on the bad treatment I consistently received from journal editors. His reply is worth quoting at length:

> Concerning the lack of response from journals: I assume the explanation is that you have gotten a reputation as someone who repeatedly comes up with invalid proofs of the Syracuse conjecture, and that it is better to ignore such people than to get into long arguments about whether a proof is valid. You also have to understand that journals decide whether a paper is worth publishing by sending it to a referee -- a person in the field who is asked to read it and critique it, but is not paid for that work. So journal editors do not want to lose the good will of people willing to serve as referees by repeatedly sending out material that turns out to be invalid, or too unclear to make sense of.
>
> You write that "Last September, half a dozen graduate students and professional mathematicians" went over "all or part of my paper", but you don't say what conclusion they came to! If reputable mathematicians concluded that your proof is correct (not "too vague for me to tell",

or the like), then they could certainly write to friends who are editors of journals, saying "This time, Mr. Franklin really seems to have a valid and comprehensible proof", and those editors should then be willing to put it through the refereeing process. If the people who read your paper agreed it was correct, have you spoken with them about this?

If the current situation is that you believe the proof is correct _but_ that you still don't have it in a form that other people can evaluate, you might try auditing a course in "proof-writing" at some local college or university...

Anyway, if, at the point when I have the time, the situation is still that you think you have a paper worth reading, but haven't been able to get it looked at by a journal, then I will try to read it, and if I find it readable and correct, I will certainly inform people who are editors, and/or are important in the area and whose opinion would be decisive with editors. Incidentally, having had it on the web certainly will establish your priority if there should be any question about this.

> ... I am willing to refrain from submitting the paper to any journals until you have made a
> decision regarding co-authoring, ...

I would only be a coauthor if I thought I had contributed some original mathematical content to the paper. But as I say, if I think you have a valid proof, I will inform others of this, and there should then be no problem with getting it published. I would only be a coauthor if I thought I had contributed some original mathematical content to the paper. But as I say, if I think you have a valid proof, I will inform others of this, and there should then be no problem with getting it published.

I thanked him profusely. No other mathematician had ever written to me at such length, or in such a helpful manner. In December 2007 I received an email from him that began:

I had written that I would send you general suggestions on writing "after the question of the correctness or correctability of your present proof is settled between us". Since I haven't heard from you for a long time, I suppose that you are either struggling with whether you can get your arguments into an acceptable form, or doing something else; but in any case, that it may be a long wait before you send another version of your argument. Since I have a bit of time at the moment (I'll be out of the country for a week and a half starting Thursday, and I've done almost all the preparations for my departure, and this isn't the time to start something bigger like a new paper), I thought I would take an hour or so now and write up for you some of the notes I had jotted down.

There followed several pages of notes, both on writing style and on matters I might look into in the course of working on my proof. I set to work implementing all his recommended changes, and meantime continued to try to make the proof meet the approval of Ed the Physicist.

At the end of June of 2008, I wrote the mathematician again. There ensued a month of email exchanges that resulted in a major improvement to my paper. His skepticism did not diminish, and I had no reason to believe that he understood the idea underlying the proof. But there was none of that thinly-veiled contempt that I had grown accustomed to from professional mathemati-

cians. There was also no encouragement. I had the impression that he had a grudging respect for someone outside the univeristy who wasn't a mathematician but who had put a great deal of effort into trying to solve a very difficult problem. He seemed to believe that the most honorable way to treat such a person was to help him improve his paper until he saw that his proof would never work.

Already in 2007 he had made the same criticism as the nasty reader described above, namely, that I needed to show that my proof didn't prove a falsehood about one of the functions similar to the Syracuse function. In July 2008 I decided that I had to respond to this criticism. I was surprised to discover that not only could I prove[1] that my proof didn't apply to two of the similar functions that both men had explicitly mentioned, but that my proof didn't apply to an infinity of functions that could reasonably be called similar to the Syracuse function! Our exchange of emails during June was one of the most productive in my life.

At the end of July, I wrote up everything carefully and sent him an email telling him the revised paper was on my web site. He didn't reply. I did not think he would simply curtail communication the way others had, without an explanation. Perhaps he had not received my email, or had gone on vacation, or could find no errors in my argument but didn't want to go on record, despite my guarantee of confidentiality, that I had solved the Problem.

I decided to give him a little rest, and went back to Ed the Physicist, who had returned from travels in Japan and China, and asked him to look at the latest revision of the paper. Like Prof. X, he had not accepted my proof, but he also had never understood the underlying idea. Perhaps my latest revision would change his mind. It didn't.

Months later, in response to an email from me, Prof. X said he had become involved in other work and had forgotten to reply to my email of the end of July. He had no time to look at my paper during the fall semester of 2008, but perhaps after that. In November and December of 2008 I made what can legitimately be called a major improvement in my solution of the Syracuse Problem. I wrote him early in January 2009, when I knew he would have completed end-of-semester grading. He wrote back a courteous email saying that he had to write several letters of recommendation, one of them for a student whose PhD advisor he had been and who was coming up for tenure, then he had to prepare for a talk at a conference, then he had to prepare for the next semester's courses, but that he would get back to my paper when he could.

In July 2009, he stated that from then on, he would review no more than one proof of mine per calendar year. This was understandable, I suppose, since after some 11 years he still had not been able to agree to the validity of any of my proofs.

In the summer and early fall of 2009, I tried to get him and Ed the Physicist to agree to the validity of a new lemma (a statement that might prove useful in later proofs) that, at first sight, seemed that it couldn't be true. Many weeks were spent arguing with Prof. X and with Ed over my logic. The mathematician's objections became more and more abstruse, involving logical subtleties that I simply couldn't understand. But the main point of contention for both readers was that I had used the phrase "whether or not" in the lemma statement, and they considered this illegitimate.

Ed was growing more and more stubborn. He no longer allowed metaphors in my explanations — no use of double quotes, which is a normal way to signal that a word or phrase is not meant literally, but only metaphorically. I should use formal language far beyond what I thought

1. This proof turned out to have a major gap, as the reader will see.

was necessary. I gave him numerous examples of the use of "whether or not" that no one, to my knowledge, ever challenged. He refused to change his mind. Neither he nor Prof. X had a shred of doubt that they were right and I was wrong.

But the more we struggled, the clearer my argument became (both to them and to me). Eventually they allowed me to use the lemma in other proofs, but neither one of them could bring himself to say, "You are right. I was mistaken."

In order to overcome the consistent misunderstandings of my proofs, I had begun asking Ed and graduate student readers to *stop* at the first sentence in a proof that they didn't understand or that they disagreed with, and let me know about it. All of these readers seemed to find great difficulty in doing this, but sometimes they managed to. In November 2009 I suggested to Prof. X that he proceed in this way also. He wrote a long reply which was summarized in his paragraph:

"... I think that asking [readers] for 'the first sentence that they disagree with in a proposed proof' without seeing what follows is a hopeless task. Even when the reader does get to see what follows, if an earlier statement is ambiguous, one can't say decisively whether it is that earlier statement which is wrong, or the later point where it is used."

Although I didn't say so to him, I strongly disagreed. If a sentence was not clear or was wrong in the mind of the reader, why should he continue reading in hopes that these matters would be resolved later to his satisfaction? Why shouldn't he ask the author, then and there, to clarify his meaning or to fix his error? The mathematician's reply reflected the standard practice among academic mathematicians of placing utmost importance on *prose* — on *interpreting* and *coming to an understanding*. But in my book that showed students a new way to keep math notes and to do proofs, I emphasized that the goal should be to reduce the importance of prose to a minimum. Every proof in mathematics can be structured according to a simple model — a few numbered steps at each level, then the proof of the validity of each step at a lower level, etc., as in a structured computer program. At each level, the steps must constitute a valid logical argument if each step is correct. And similarly for the proof that each step is correct, etc. A beautiful, simple model that works no matter how many pages long the entire proof is. Yet at the time of this writing (March, 2016) it is still rare even to find numbered steps in proofs in mathematical papers and textbooks, much less the structure I have just described. The reader is expected to understand and hold in his mind the entire idea underlying the argument, even though as the lengths of proofs increase, this becomes more and more difficult. Computer programmers originally had the same expectation for other programmers reading their programs, but by the seventies they realized that the task was hopeless, and so they promoted an approach to programming in which long programs were broken down into manageable, understandable, pieces, whose correctness could much more easily be ascertained.

Every paper in mathematics has the same set of entities: definitions, statements of lemmas and theorems, proofs, with interspersed informal explanations of strategies and aids to understanding (if the reader is lucky). So in my book I argued that, as much as possible, standard forms should be used for these. Since the subject is based on a strict logic, everything should be done to eliminate the importance of the literary factor — to eliminate the kind of ambiguity the mathematician referred to in his email. However, let me hasten to make clear that I was in no way advocating the presentation of proofs in strict symbolic format. (I had tried that in the topology course at San Jose State that I took in the seventies, and found how hopelessly difficult it made understand-

ing.)

There was no interchange with Prof. X until after the end of the spring semester. During the spring months, I thought I had discovered a much better argument for a proof that was based on my original intuition, 30 years before. In the summer, as usual, I didn't understand many of Prof. X's objections. I sometimes felt as though he were a father who hopes to cure his son of a false belief by simply finding reasons, time and time again, why the belief is wrong.

He continued to gently insist, as he had for several years, that I always check each new proof against a certain other function for which counterexamples were known. If my proof worked for that function, then my proof must be wrong, for it proved something false. But, as usual, I was reluctant to make this check, first and foremost because I was afraid it might reveal that there was indeed a fundamental flaw in my argument.

In late July 2010, sensing his growing impatience, I decided I really had to start making the check he so persistently recommended. Although I often doubted that he understood my strategies, much less my arguments, it was clear from one of his emails that he understood the strategy that was based on my original intuition. So I began checking that strategy against this second function, and realized that my intuition had been wrong. There indeed was a fundamental flaw in it that I had not been aware of — or, rather, that I hadn't allowed myself to be aware of. The error was something that a professional mathematician who had been reading my papers for 12 years could easily have pointed out if he had spent any time thinking about just a few of the lemmas in my paper. But he hadn't. Once again I felt that all he was doing was assuming that I was wrong, and then coming up with arguments, any arguments, to assert that. But as it turned out, in this particular case, he had been right — I had been wrong! While I was resigning myself to the loss of all hope, and preparing a new, short, paper that would set out for the online world what was wrong with that original intuition, there suddenly emerged a proof that, at this writing, I do not hesitate to call *elegant*. And it emerged directly from the counterexamples to the conjecture concerning the second function that the mathematician had been urging me to take into account in my proofs. Furthermore, it was another version of the basic intuition of 30 years before.

I wrote him telling him I had found the error, but that in the process I had also found a remarkable new proof, one that arose directly from this other function he had urged me to check my proof against. In his reply he merely said he would be willing to look at the new proof the following January, when I would be allowed to show him a new proof.

I didn't know what to think. On the one hand, his prodding me to check my previous proof against the second function had forced me to discover what seemed to be an elegant new proof that I almost certainly wouldn't have found for months or years if ever. On the other hand, I was appalled that a mathematician would tell someone who might have stumbled on a major discovery as a result of following the mathematician's own advice — would tell that person he would have to wait five months before the mathematician would read it.

And yet, here was a respected mathematician who had been willing to spend a few weeks each year for more than 12 years reviewing and commenting on my proofs. True, during that entire time he had not once offered me a word of praise or encouragement, or made a single suggestion as to how to go about improving my approaches to a proof. But on the other hand, there wasn't another mathematician in the world[1] who was willing to critique my work for that length of time — or even for a year. I tried to figure out what his motivation was. If he thought none of my

1. Apart from Prof. Y, who will be introduced later

ideas had any merit, he wouldn't have been willing to continue to review my work. Was I an annual charity contribution he made to the public, just as he might have each year set a certain amount of time aside to help people with no mathematical ability who were struggling to pass required math courses?

Since he wouldn't read my new proof for five months, I wrote him asking what he thought of the idea of my offering a prize of $500, possibly $1,000, plus shared-authorship to the first mathematics graduate student (not necessarily at his university) who (1) could correct my latest proposed proof of the Syracuse Conjecture and (2) could get at least one professional mathematician to declare, in writing, that he believed the proof was correct. The student could win the prize even if the revision were minor. He wrote back as follows:

> I am not comfortable with the premise of the prizes that you ask me about. A [graduate student] looking at a proposed proof should be trying to determine whether it is valid (or can be fixed so that it becomes valid). He or she should not be prejudiced toward finding it valid or fixable, whether or not it is.

I didn't know what to make of this reply, and wrote back telling him so. In his response, he remarked almost casually that he didn't think there was anything worthwhile in my paper, that even though he didn't know the literature on the Problem, he was quite sure that all the lemmas in my paper except a few were known to other researchers. (A veteran researcher later told me he had not come across any of my ideas in the literature (see "A Retired Member of the Research Community" on page 1345 below).) He then quoted a statement in a 1985 paper by my nemesis that was a passing observation of the structural fact that I had developed in my papers. Finally, he remarked that a well-known phenomenon in infinite sets probably invalidated one of my key strategies.

I was appalled. These remarks confirmed all my worst suspicions: for more than 12 years he had made no effort to think about the content of my papers — about the ideas —, and his remark about the well-known phenomenon revealed an almost unbelievable failure to understand the nature of one of my key strategies. If he were the PhD advisor of a graduate student who revealed that shallow a grasp of one of the mathematician's papers, the student would have been told to seek his PhD elsewhere.

I didn't reply to his email and resolved never to contact him again. But I did. Once a year or so, I would ask him to take a look at one of my latest proofs, and he would always find a reason to object to it. On the other hand, his emails never had the ill-concealed contempt that emails from other mathematicians did.

A Useful Nut-Case

I described earlier in this section the reader who did not merely criticize my proof, but used some rather nasty language in doing so. I remarked there that some people who were interested in the Syracuse Problem — in fact were obsessed with it — but who didn't have any ideas as to how to solve it, found consolation in discovering real or imagined errors in the attempted solutions of those who *did* have some ideas. One of these people, a German who apparently had some lower level mathematics teaching position, wrote me in April 2010, claiming to have found an error in the proof of the most fundamental lemma in my paper, a lemma that I had used in many proof attempts. I didn't want to hear this, but I went over the proof, and, sure enough, he was right. In the process of repairing the error, I came up with a much better proof. I thanked him for having

pointed out the error to me. He went through the proofs that at the time I believed solved the Problem, claimed to find other errors. But these claims turned out to be invalid, and I told him so. Nothing daunted, he continued turning up other non-errors. Each time I pointed out his error, he went on to find another one. He was engrossed in logical subtleties that were so complicated that even he couldn't see the errors they contained. Eventually, I told him I didn't have time to continue our interchange.

A Retired Member of the Research Community

In July 2010 I began sending emails to Syracuse Problem experts who had attended a conference on the Problem. Only one of these experts replied. He was a retired professor in his eighties who had published several papers on the Problem. From the start he seemed to be interested in my paper, in particular, in one of my lemmas that he thought might help in his own efforts. He sent me a summary of a paper he was working on (it contained a reference to the online draft of the full paper), and proposed that we collaborate. (Ironically, though the writing style of his summary and of his paper was thoroughly professional, his emails were full of typographical errors.)

I told him that I felt I was very near a solution to the Problem, and so didn't want to dilute my efforts at this point. Instead, I repeated my offer of shared authorship if he helped me prepare my paper for publication. He declined. I am sure the reason was that, like every person I had ever contacted who had worked on the Problem, he did not want to face the possibility that someone else had solved it. But he repeatedly promised to go through my paper as soon as he had time. I was confident that that meant he would probably never go through it, at least not in sufficient depth to actually check my proof.

I asked him if he had seen other papers with the approach I had taken to solving the Problem. He said no.

His paper seemed to me to be more of the kind of thing that constituted most of the literature on the Problem, namely, lemma after lemma of the form, "If x has the properties P, then the Syracuse function will return a number having the properties Q." I felt that my discoveries were way ahead of efforts like this. Also, he seemed not to deal with the fact that, if the lemma applied to both "desirable" x's (those that were not counterexamples to the Syracuse Conjecture) and "undesirable" y's (those that were counterexamples), it was hard to see what use such lemmas could be in solving the Problem.

Nevertheless, his treating me as an equal was an enormous relief, and was encouragement in itself. When I wrote to him, I once again felt I was doing what I had been born to do, that working on a very difficult problem with someone who respected me, was my natural home. At first the difference between working with him and with Prof. X, made me even angrier at the latter. But when I thought about it, I decided it was not fair to compare the behavior of the two, because the latter had never indicated any interest in solving the Problem.

The Sweet Wife of a Careerist

In the summer of 2007, my student renter, Debbie, to be introduced in the next file, was off on another trip to Europe and the Middle East. In keeping with our agreement, she was allowed to find a temporary renter (a "sublet") for me. She performed her usual magic with Craigslist and found a young German woman named M. who was a visiting scholar at a local university so she could do further research on the subject of her PhD thesis, black literature.

She was tall, graceful, a bit shy, very attractive, and spoke perfect English, with only the slightest trace of an accent. She was always a pleasure to talk to. We would sit at the kitchen

table, and I would ask her about the black authors she was studying. But, knowing she was from Leipzig — she had met her husband there, as he was completing his PhD at a local university — I couldn't help asking her about performances of Bach in that city. I asked her if Zimmerman's Coffee House was still in existence. (This was where Bach's little orchestra, the Collegium Musicum, gave some of the first performances of his works, including the Coffee Cantata, which was first performed there around 1734.) She said oh yes, it was still there, and I should definitely plan to come to Leipzig for a visit. Bach was performed throughout the year.

I developed a genuine affection for her, as I think she did for me. I of course passed in front of her bedroom door several times each day and once she said, "I could hear you sigh." I was deeply troubled when she told me that she had an eye disease that could result in a sudden attack that, if not caught within less than an hour, could result in permanent blindness. She asked if I would take her to the hospital if the attack occurred and, of course I said yes. She gave me the name of the hospital and the number I should call. (I can't remember now why she didn't tell me to call 911.)

While M. was here, I naturally asked her husband, who had recently received his PhD in mathematics, to read my paper on the Problem. He said he would. He gave it what seemed to me the usual dismissive review of the academic mathematician, although he was willing to read and reply to my attempts to explain where I felt he was wrong. I sensed no insight into what I was trying to do, and no interest. Nor did I sense the spark of creativity that I always looked for in any mathematician who read my work. He seemed to be a hard-working future member of the mathematics Establishment and I had no doubt that he would succeed. He had a driven quality that I speculated might be a result of his being the son of a well-known scientist. But I may be completely wrong in all this.

After M. left, she always sent me a Christmas card, and sometimes just a little note during the year to say hello. Then suddenly, no more cards or notes. After several months I contacted her husband, asked him if I had said something to offend her. He replied that she had had a baby several months before, and had no time. Knowing her, I didn't believe him. I was convinced that he had decided to break up our little relationship out of fear that I would keep asking him to read revisions of my paper. I made a promise to myself that when my solution to the Syracuse Problem was published, I would track him down and send him an envelope containing the paper and nothing else. Meantime, every now and then I find myself remembering how much I enjoyed talking with M.

A Son-of-a-Bitch

In September, 2010, I decided to write to a mathematician who years earlier had published a paper that contained an idea that lay at the basis of my approach to the Syracuse Problem. I thought that I might be able to arouse his interest in my latest proof if I pointed out to him that his paper had an important part in it.

I looked him up on Google. He had a list of publications a mile long — books and papers not only in mathematics but also in various branches of physics. Plus numerous honors and a very impressive title at his university. I wrote him a two-page email. He didn't reply. Some two weeks later, I wrote him the following email:

Prof. — :

No one could blame you for dismissing my email of 9/8, considering the number of false

claims of a possible solution to the Syracuse Problem that have been put forth.

But it would help me enormously if I knew if that was the only reason. I give you my word that I will not argue with anything you say, and in fact will not reply to your email at all without your permission.

Was there something about the writing style of my email? Was it the fact that I do not hold a tenured position at an accredited university? Was it an error in my proposed proof so egregious that you could not bring yourself to even bother replying?

Any suggestions will be appreciated. Again: I will not reply without your permission.

Regards,

-- John Franklin

Within an hour I received the following email:

You will have to find someone else to analyze your work.
Please do not contact me again.

[his name]

And in my shame and fury, I wondered yet again why so many mathematicians are so nasty. Was this man angry that he hadn't been able to solve the Problem, and therefore wasn't about to read anything that even suggested that someone else had solved it — even if that person offered him shared-authorship in the paper setting forth the solution? I thought again with disgust how the academic system practically nurtures the kind of close-minded, prestige-obsessed, knowledge-hoarding, obscurity-loving behavior that I had hated from my youth.

Torments by the Foreign Less-Than-Bright

In the summer of 2011, I received an email from a Finnish student who told me in no uncertain terms that my proof was wrong. It soon became clear that he did not understand the difference between form and content. I tried to explain it using the example of a binary tree (a tree with two downward branches from each node): the tree is the form, how we label each node is the content. It did no good. His difficulty with English, and his stubborn belief that he was right and I was wrong, eventually led me to terminate the exchange.

A little later, I received two more emails from Eastern Europe. The status of one sender I never found out, but the other described himself as a mathematics PhD who prided himself on being an outsider in the mathematics community but who nevertheless served as referee for journals. The names of these two were almost the same, and so it took a while to straighten out which was which. Both were convinced my proof was utterly wrong. It soon became clear the second was obsessed with logic. At one point he said he didn't have to "understand" my proof. All that mattered was the logic. (It is true that a correct proof can be checked by machine, but I don't think any mathematician would say that it was unimportant for the reader to understand his paper.) The discussion went on, and then, one day, an email from him said that my proof was correct after all!

I thanked him profusely, wrote to several friends announcing the good news. His next email said, no, the proof was wrong, he was only trying to teach me a lesson. I kept asking him to tell me the first sentence in my proof that was incorrect. He replied that it didn't matter if *all* of the sentences in my proof were correct: the proof was nevertheless wrong. His convoluted logical arguments were hopeless to understand, and so I terminated the exchange.

And then there was the German computer scientist, who sent me a preprint of a paper by a German mathematician in which the author claimed a proof of the Conjecture. The computer scientist said he had found a flaw in the proof, and that the author had acknowledged it. The paper was based on two papers of the nineties which attempted to apply the branch of mathematics known as *analysis* to a proof. (This is the branch of mathematics that developed from fundamental concepts in the calculus, for example, limits.) But the author made no attempt to save the reader from having to try to find copies of those papers, and so the logic was full of gaps that could only be filled by someone who had read the papers. It was a paper written for insiders, and I recalled what the mathematician in the first file of this volume had told me[1], namely, that in his research lab, a mathematics paper that was not difficult to understand was dismissed with contempt. Several times I urged the computer scientist to read my proof, offering to pay him for his time. He had no interest, making clear that he thought the proof was faulty but never telling me where the errors lay.

Prof. Y, A Veteran Researcher on the Syracuse Problem

Around this time I had what I thought was a brilliant idea: write to the head of the Math Dept. at Lehigh University, my first alma mater, and tell him what prestige he, his department, and Lehigh would gain if an alumnus succeeded in solving a very difficult math problem. He was receptive and asked a member of his department to read the fourteen pages that I told him were all that were necessary to understand my proof. But that professor had once had, as a graduate student, a man who by then had spent more than ten years trying to solve the Problem. I had communicated with the latter on several occasions. I will call him Prof. Y[2]. He was always courteous, even respectful, and he always replied to my emails within a day or two. But he made clear from the start that he was convinced that my proposed solutions to the Problem would not work.

The Lehigh professor contacted this latter professor, and the reader will not be surprised to learn that the Lehigh professor quickly came to the conclusion that my latest proof had no merit. When I pressed him to tell me the first sentence that he felt was wrong, he said that the first two steps were correct, but that the third (the last) step was not. It became clear that he had not even understood what the first two steps proved. It was also clear that he had no interest in having his mind changed, so I wrote to his former student, Prof. Y. Once again, the same inability to see what was on the page, much less to think about it. When I said, in the least offensive way, that perhaps it was not a good idea for me to ask a man who had spent years trying to solve the Problem, to tell me if I had solved it, he said:

> ...your concerns about math researchers being prejudiced against your work is not, in my opinion and experience, the way mathematicians work. Keep in mind that most research mathematicians are also college teachers, and thus we are very much used to reading and correcting proofs done by non-professional mathematicians... our students. So most mathemati-

1. See footnote in section, "The Meaning of 'Idea' in Mathematics"
2. "Y" is not the first letter in his first name or his last name.

cians are quite happy to explain a fine point or praise a correct solution, no matter who it is from.

I couldn't bring myself to continue communicating with a person that naive.

Superficial Readings Followed By Rejections

In the above section, "A Mathematrician Who Cared About Amateurs" on page 1334, I introduced a mathematician who actually was willing to communicate with me. In fact, he agreed to allow me to write him once every three months provided I didn't ask him to read more than two or three pages. This I did. Up until early 2011 he replied within a week or two of receiving my email, although it was clear he had not thought deeply about my argument, and seemed willing to use any kind of superficial argument that came to his mind as to why my logic was wrong. But I replied to his objections, and made changes that would, I believe, overcome them. At the end of the next three-month period it took me more and more emails to get him to reply to my emails (he said he had had jury duty, and new mathematicians to help get established in his lab). Finally, in August 2011 he no longer replied, even though I made clear to him that I had removed all the sources of his previous objections, and that three qualified readers had said my proof was correct and that a mathematician had said that the first two steps were correct, and that I corrected the mathematician's objections to the third step, and that some readers had said that the two steps were sufficient in themselves. No response. I sent him an email headed, "I can't believe that you would go back on your word". No response.

In July 2011, a mathematician on the staff of a national organization of mathematicians agreed to archive my paper. I almost couldn't believe his willingness to do this, since the one agency that existed for the purpose of archiving mathematics papers, namely, arxiv.org,, would not accept papers from non-academic mathematicians without an endorsement from a mathematician. When he received the archive copy, he went through my proof and declared it without merit. His remarks — delivered from on high, with not a trace of a doubt that he might be wrong in his judgment — made clear that he had only done a quick, superficial reading, and that he had not understood the argument. I asked him several times to tell me the first sentence that he considered wrong. Eventually, he told me the sentence, and the reason why he considered it wrong. Unfortunately, that reason made clear he had not understood the preceding material in the proof. After a few more emails, I as usual bowing and scraping and thanking him for being willing to discuss the proof and for archiving the paper, he said he had to terminate the exchange. I thought again: if you were a PhD advisor and one of your graduate students had given a paper of yours as superficial and uncomprehending and disrespectful a reading as you gave mine, you would have asked him to pursue his PhD elsewhere.

The Mathematics Culture

As the years passed I continued to wrack my brain trying to figure out why graduate students and mathematicians alike seemed determined to have as little as possible to do with my papers. I asked myself again and again why my offer of shared authorship had no appeal for professional mathematicians. I could certainly understand that a mathematician who had spent years on the Syracuse Problem would not want to receive an email from someone claiming a solution. But if, in exchange for the mathematician's help in making the paper publishable, the someone offered him shared authorship in the paper? I always made this a condition when someone wrote me saying they had a solution to Fermat's Last Theorem: I would be glad to read the paper (if the bizarre

arguments in the home-made writing style of the person ignorant of mathematics can be called a "paper") but only if I received a written statement that I would be offered shared authorship in return, since the sender couldn't expect me to help him to wipe out years of my own work on the problem. Sometimes I received such a statement.

When I asked mathematicians why my offer was no incentive for professionals to work with me, I got a two-part answer: first, the Culture only allowed shared authorship if each of the authors had contributed equally to the solution. I replied, "But suppose one of the authors wants to reward the other for stylistic guidance that would make the difference between the paper being accepted and not being accepted? Why should that be prohibited?" The answer was: It just was. The second part of the answer that I received was that no professional would want to spend time on a paper that would not obviously lead to a solution.

So, it was a Catch-22: if the mathematician believed I had solved the Problem, he could not accept shared authorship, and because he couldn't accept shared authorship, he would have no incentive to make constructive criticisms that might lead toward publication of the paper. If the mathematician was not convinced (after a brief, superficial look at the paper) that I had solved the problem, he would have no incentive to spend his time working on it with me, since I was an unknown.

All these excuses. All this monumental determination not to have anything to do with anything the Culture frowned on or prohibited. The countless hours I had spent writing and rewriting letters and emails to mathematicians, the working over each and every word: is it possible this will offend? does this exalt his status sufficiently? is there a grammatical error, a stylistic error, anywhere? (When they wrote to me, there were often typographical errors.) Have I expressed sufficient humility and shame for bothering them?

I thought of the deadly competition among the PhDs to get on a tenure track, then to publish sufficiently many papers to get the confirmation, namely, tenure, that they have a place among mathematicians. I thought of the ideal of a successful mathematician that the Culture always kept before aspiring students: a prodigy who wins prizes in competitive exams, who can afford to go to the best schools, who learns quickly what he is expected to learn, has a phenomenal memory for mathematical facts and details, is able to perform calculations in his head without effort, who grinds out papers (at least two or three) year after year. Above all, a person who never questions the Culture. And yet the history of mathematics (the careerists regarded the study of the history of their subject with contempt) showed that there had been creative mathematicians who by no means possessed all of these traits.

But by early 2011, I could no longer avoid the conviction that what was driving all this was not the love of accomplishment, of new knowledge, of excellence, but something far more mundane, namely, the determination that the Club should above all preserve its prestige. No outsiders! No one who doesn't look, sound, behave, write, exactly as we do! No one who works on problems we do not approve of! No one who lacks the credentials we require! No one who in any way threatens our exclusiveness!

Catastrophic News

The four worst days of my life were: the day my father died, the day my brother was killed in a horrible car accident, the day that Marcella said she wanted a divorce, and the day that a German computer science professor pointed out an error in my proposed solution to the Syracuse Problem that, as far as I could tell, nullified my 30 years of work on the Problem. That day was Thursday, Nov. 24, Thanksgiving, 2011.

Let no one say the gods lack a sense of humor. The professor had contacted me a few weeks earlier, praising the results (proven facts) in my paper. Part of his first email was the following:

Respected Mr Franklin,

I have just found your highly interesting papers concerning the [Syracuse] Problem.
It looks like your latest writing contains a possible proof based on ... and Exponents.

Please understand that I have been deeply into the topic for a year now, so I would say I have a certain expertise.

Your methods, concepts and considerations are clear, convincing and well elaborated.
In contrast to others' approaches your work is connected with my own findings and therefore especially appreciated.
I have just begun to carefully read your papers.
It will take some days (or a few weeks) until I can decidedly judge about "right or wrong".
It is my intention to support your work by preferably confirming that your proof is correct.
I don't have any problem at all with starting a sentence by "If counter-examples exist, ..." :-)
Promise: If your proof is valid, it will soon be accepted by many experts.
If not, I am willing to do anything (in support) to fill possible gaps or to make corrections.

Let me just offer you to give feedback as soon as possible (if welcome).

Gladly knowing there is someone out there who does exactly the right thing here (you),
with best regards

...

In his next email he said:

Please keep in mind: **I do not doubt** (in contrast to others) **the validity of your proof**.
I just recognize that there are difficulties to find broad acceptance, so some more clarifying work seems unavoidable.

Short version of my current judgment:

I know that your proof is essentially correct **by content**.
I think that your proof is to some extent inaccurate **by form**.

It must be possible to go a step further and find a strictly convincing argument which is incontrovertible by anyone!
If you agree, this is what I would like to do together with you: find whichever additional and clarifying evidence.

He had legitimate reservations about one part of my proof. He tried to convince me of the validity of these, and then, on Thanksgiving Day, he succeeded.

I wrote him, admitting he was right. I offered him shared authorship if he would help me fix the problem. He gladly accepted. There then followed an exchange of emails about several functions which are related to the Syracuse function. I was amazed at his insight and creativity and enthusiasm and, above all, the complete absence of the hauteur and contempt I had grown used to from academics. He was unlike every other person with whom I had communicated over the past 30 years. His enthusiasm for my work continued. At one point he invited me to come to his university as a "visiting scientist". He listed his academic history (degrees in physics and a branch of electrical engineering, none in mathematics.) He said that, once we started working together, he wanted me to call him by his nickname, not, "Prof. — ", as I always did in these cases. When I told him I had been a jazz musician, he expressed his delight, because he was one also (piano).

And then, soon after I had admitted the fundamental error in my proof, he said he had discovered an elegant solution to the Problem that he was 100% convinced was correct, and that furthermore made clear why there could be no other possible solutions. I asked him about the co-authorship. Yes, he still wanted me to be co-author, as he had not yet completed all the proofs supporting his solution. But first, he said, he wanted to send the outline of it to two leading mathematicians.

I asked Dr. D., my former housemate Zoe's father, a famous surgeon who had also been a professor for many years, what he thought. He said that he was from Missouri in matters like this[1]. He then said, in so many words, that the professor sounded too good to be true. Others to whom I told the story, said the same thing.

The professor's emails became more infrequent. In my despair, I began putting all my Syracuse papers in order, and getting used to the fact that I had wasted 30 years of my life. But while I was doing this, I went over my proposed proof again and again. By February 2012, I had decided that I was right after all. As I write this, I haven't the faintest recollection of what made me think my proof was hopelessly wrong. The emails in my exchange with the professor still exist in my email archive[2], and so I could try to find out exactly what caused me to lose all hope, but I have not attempted that, one reason being that it would be a huge labor to try to figure out which segments of the hidden text[3] in my paper were the ones that the computer scientist based his criticism on. I am skeptical that the proof he claimed to have was valid, because if it was, I would have heard of it by the time this was written (late June, 2012).

By May, 2012, after many unsuccessful attempts, I had found a second proof, using the second structure I had discovered that underlay the Syracuse function. I asked the professor for his opinion. After a heated exchange, in which he firmly insisted the proof was invalid, he said that he would show it to ten colleagues with international reputations. Soon after, he wrote that all his colleagues had said the proof was worthless. I told him I would like to see the comments that at least one of them had written; the computer scientist could certainly send it to me without revealing the name of the colleague. He refused. Later, he insisted that I not write him any more.

I Offer $10,000 For Help in Publishing My Paper: No Takers

The reader will recall that the dilemma I faced was this: no journal would consider a paper on

1. Referring to the saying, "I'm from Missouri. You'll have to show me."
2. The four-month ordeal of trying to preserve that archive will be described in a later version of this chapter.
3. In the FrameMaker word processor, there is a way to hide text from view without actually deleting it. It is called "Conditional Text".

the Syracuse Problem that was not written or co-written by an academic mathematician, or at the very least endorsed by one. But it was virtually impossible to find a mathematician to read my paper because I was not an academic mathematician. In April, 2012 I hit on a new idea: I would offer a contribution of $10,000 to the first mathematics or computer science department a member of which helped me prepare my paper for publication. In addition, I made the usual offer of shared-authorship, and assumed that, even if that offer were declined, the mathematician or computer scientist would be willing to endorse the paper in a letter to a journal editor. I wrote a letter and sent it, along with an abbreviated version of my paper, to the chairman of one of the nation's leading mathematics departments, stating that if I didn't hear from him in three weeks, I would approach another department.

I received not a word in reply — not so much as a written rejection.

I next tried the editor of the electrical engineering and computer science journal that had published my paper on Occam's Razor in the eighties. But even though it was easy to find, via Google, the university where he was a professor, for some reason no one in the secretarial staff seemed to know what department he was in. I was given an address to try, and sent my letter and paper to that address. No reply. Further probing revealed that it was not the right address. I tried to reach the editor via email. Finally, after several weeks, I received an email from him stating that my envelope had become temporarily lost in the university postal system, but in any case he didn't think my paper was appropriate for the journal.

By this time, the online version of the paper was receiving close to 200 downloads a month. None of the readers wrote to me, however.

I next decided that, given my credentials, and despite the experience with the journal that had published my earlier paper, perhaps I should approach computer science departments. So I sent a letter and a copy of the abbreviated version of the paper to the chairman of the computer science department of one of my alma maters. At the end of the two-week period I had said that I would wait for a reply, I received an email from the chairman. He rejected my offer, but stated that if he had the expertise, he would have been glad to join me in preparing the paper. I replied with an appreciative email.

Next I tried the computer science department at the same university as the math department I had first approached. Another courteous rejection, although it was clear the chairman had not bothered to read most of my letter. His advice was that I submit the paper to a journal, or try to find a graduate student to be co-author.

I kept trying. I approached the chairman of another computer science department in the same university system as the previous. I received no reply.

The Dazzling Pedantry and Devious Logic of Prof. Y

I mentioned, in the section "Prof. Y, A Veteran Researcher on the Syracuse Problem" on page 1348, a veteran researcher with whom I had communicated occasionally.

He was cheerfully convinced that none of my efforts to solve the Problem had a chance of succeeding, and so he had no interest in working with me in exchange for shared authorship. He had been unable to solve the Problem himself. The one paper of his that I had looked at gave no sign that he had any good ideas, it being yet another collection of new lemmas (facts) about the Syracuse function. Like all the other Syracuse researchers, he believed that if one keeps grinding out these lemmas, a solution to the Problem might suddenly appear. I did not believe that. I felt, and feel as I write, that only an original, unorthodox approach would succeed in solving such a difficult problem. But the professionals wanted to continue being published, they wanted to show

they were deserving members of the Club, they wanted to demonstrate their mathematical skills, and so they continued to produce their dull facts.

We carried on our correspondence at a rate of two or three short email exchanges a year. He always replied promptly to my emails, with none of the nastiness I had experienced from other mathematicians. There was something paternal in his manner, as though I were an errant child who needed to be corrected through patience and loving kindness. I kept trying to find something we could agree on, so that I could begin my argument for the validity of my proof from there. But he simply refused to accept that it is legitimate to compare two mutually-exclusive cases, namely, (1) a counterexample to the Syracuse Conjecture exists, and (2) a counterexample does not exist. I argued that such comparisons are made every day, both inside of and outside of mathematics, and gave numerous examples, including, e.g., "If the Higgs boson exists, then ... but if it does not exist, then ..." (The Higgs boson is a physical particle whose existence had been conjectured in 1964. Its existence was confirmed in 2013.) I gave him a scenario in which a professor in one room in a university, presents what he believes the consequences will be if the boson exists, and a professor in another room presents what he believes the consequences will be if the boson does not exist. I asked him if such a scenario contains a contradiction. If not, does the contradiction suddenly come into being if recordings of each presentation were then played in the same room? But I got nowhere, since his counterargument was so bizarre I didn't feel like continuing the discussion.

In one email he said that it was perfectly legitimate to *consider* the two cases, counterexamples exist and counterexamples do not exist, but it was not legitimate to *compare* the cases. I regarded that as rubbish.

At one point I came up with a statement regarding the legitimacy of my comparison strategy. I challenged him to show it to his colleagues and ask them for their opinion. He replied with several pages on the theme, "What does 'legitimate' mean?" He gave no sign he would do as I had requested.

He was a prime example of what can be accomplished in the way of avoiding unpleasant facts through the simple rule, "Know thyself *not.*" He had the psychological naiveté that allowed him to believe that academic mathematicians were completely open-minded about the efforts of non-academics. (See quote from his email in the section "Prof. Y, A Veteran Researcher on the Syracuse Problem" on page 1348.) It was clear that he had been able to convince himself that any proposed solution to the Problem that he didn't regard as correct, was simply not correct, no matter who had devised the solution, and no matter what other mathematicians said about the solution. He reminded me of the constructionists, in the early 20th century, who believed that no proof by contradiction (a standard proof technique) was valid. As a result, they dismissed numerous theorems that were established parts of the mathematical literature.

He had managed to wall himself into a comfortable, impregnable fortress, that I imagined as having soft mattress-like walls.

Another Mathematician Says My Proof Is Correct, Then Changes His Mind

In May 2013 a classified ad I had written, asking for a consultant to help me complete preparation of my Syracuse paper (I didn't name the problem in the ad), appeared in a publication of one of the national mathematics societies. Although the circulation was 14,000, I received exactly two replies. One was from a graduate student who didn't seem qualified to provide any help with my paper, the other was from a young mathematician who had held several teaching positions, but who was not yet on a tenure track. I asked him to read the paper. After several

emails of questions, he sent me an email with the following words:

"I am stumped as to any reason your proof is incorrect... It is very original."

I told him that I felt the comparison strategy I used to solve the Problem also seemed to work for a simple proof of the famous Fermat's Last Theorem (FLT, which had been proved in the mid-nineties, but with a long, complex proof). He was hesitant, but he said that he thought that the function I had used in my proof, and the structure of the Syracuse function I had used in my solution of the Problem, were worthy of further investigation. He said he would send a copy of my Syracuse paper to his former thesis advisor. He also offered to co-author a paper on my FLT discoveries.

I didn't hear from him for several weeks; I wrote several emails asking if something was wrong. Eventually he replied, saying that he had been taken to the hospital after an attack of a mental illness that he had suffered from for most of his life. He gave no further details. We resumed our email exchange, but it soon became clear that he had lost his previous understanding of my paper.

I asked him several times what his former advisor had said about my paper, but received no reply. Weeks later, he wrote: "Unfortunately, my adviser is dismissing your proof on the grounds that a conjecture's being true or false has nothing to do with a set of positive integers. In a later email, he said: "My advisor asks that you not mention him or his comments on your proof or otherwise try to contact him."

I was furious. I wrote the young mathematician:

> ... please assure [your advisor] that I will not mention him or his comments in my proof, or otherwise try to contact him.
>
> You might also tell him that I regard his treatment of my paper a disgrace. No other mathematician, in years of having had versions of the paper read by professional mathematicians, has been remotely as rude as your adviser. If one of his graduate students gave such a superficial reading and response to one his papers, the student would have been asked to seek his PhD elsewhere -- or perhaps consider a different academic field. If your adviser didn't want to read my paper (I think 'read' is way too complimentary a term for what he actually did), then he should have said so. If he decided to read it, then at the very least he should have had the common decency of adhering to the request made early in the [preliminary remarks section], namely, that he read [the proof] one sentence at a time, and notify me of the first sentence that he felt was wrong. He did nothing of the sort.

The young mathematician then revealed that he had not actually sent his advisor my paper: "...I gave him very little to go on. He did not read your proof. If anything, it is my fault for incorrectly interpreting your method of proof, and his response may have been the natural one."

I could only imagine what garbled version of my proof the advisor received from a former student who had recently suffered a mental breakdown, and who gave no sign thereafter of having any clear understanding of my reasoning.

But there was no one else with whom to communicate about my efforts, and so I pressed on,

hoping that eventually he would regain the mental acuity he had had before his illness. I asked him to read five pages describing an approach to a proof of Fermat's Last Theorem. No reply for many days. Then, when he finally wrote, it was clear that even these few pages were at the limit of his comprehension.

He seemed to want to return to my Syracuse solution, which he repeatedly called "circular". I began sending him one or two sentences *per email*, asking each time, "Do you agree with this?" He said yes several times. I wrote in the Subject field of my emails things like "Great! We're almost there!"

The third-from-last step said in essence that if $a + d = b$, and $a + d = c$, then b must equal c. , But my good luck had run out. He felt the reasoning was not legitimate. My heart sank. I was strongly tempted to tell him, "Well, we have gotten this far in six months, perhaps in another six months you will agree I am right for the final three steps." But I didn't. Instead I resigned myself to more days, weeks, months, of trying to understand why in God's name he couldn't understand my simple reasoning.

A Mathematician With Not One Hour of Spare Time Per Year

In February, 2011, I wrote to a mathematician whose name I had come across in a list of attendees at a conference on the Syracuse Problem. I asked him if he would have time to read my paper, emphasizing that it would take him no more than an hour to read not only all the necessary supporting material to understand my proof of the Conjecture, but the proof as well. I reminded him about the prestige awaiting any mathematician who had anything to do with the publication of a correct proof, and offered him shared-authorship if he made a contribution to the content of the paper.

He replied that he doubted he would have one hour of spare time *for the next year*. I waited a year. No response. I waited another half year. Still no response. So I wrote him. He claimed to have read my paper, but like all other recent readers who had communicated with me, he said that my Comparison Strategy was invalid[1]. It was clear that he had done little more than glance at the paper, probably spending less than half an hour in the entire year-and-a-half on it.

Of course, the no-spare-time excuse was routine among mathematicians who wanted nothing to do with a paper written by someone who wasn't a member of the Club. When I told one mathematician that he would be able to recognize in 15 min. that the paper was not a crackpot effort, he said he didn't have 15 min. spare time in a month.

There is no doubt that academic mathematicians were under constant pressure to produced research, as part of the disgraceful publish-or-perish rule.

Years later, I came across the following words: "The secret to doing good research is always to be a little underemployed. ... You waste years by not being able to waste hours."[2] And, despite the very high cost I have had to pay, I was, and am, thankful that I had the courage to go my own way and stay out of the university, and thus have the abundance of idle time that made it possible for me to think the thoughts I wanted to think, at the pace I wanted to think them.

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1. Years later, an expert on mathematical logic wrote me stating that the Strategy was in fact valid. See "An Expert on Mathematical Logic Pronounces In My Favor — But That Is Not Good Enough for Profs. X and Y" on page 1367.

2. Psychologist Amos Tversky, quoted in a column by David Leonhardt in the *New York Times*, Apr. 18, 2017, p. A19.

Mathematicians Who Spoke Only Broken English

I placed an ad in a publication of another national mathematics society. Circulation was way over 14,000. As with the previous ad, I received two replies. One was by phone message, but the English of the obviously-foreign-born speaker was too difficult to understand, so I couldn't even get his phone number. The second was from a professor at a third-rate university. He, too, was foreign-born and also had difficulties with English. "I consider very unfair that journal do not take your paper on your own," "I have not idea how much to charge."

But I wasn't in a position to be choosy, and so I gave him the online reference to my paper. It soon became clear that he was having considerable difficulty understanding the initial definitions, despite the fact that for years these had been understood with little difficulty by several mathematicians and graduate students. I answered all his questions, and in fact agreed that a sentence could be added to one of the definitions to make it clearer. I put up with his repeated criticisms of my poor writing style[1] without complaint. But I was stunned at the slowness, the lack of insight and fundamental intuition of this member of the mathematical Establishment. After a week or two he said he just remembered that he had previously accepted a refereeing job, and so would not be able to continue reading my paper.

I must mention that, like several mathematicians in the past, he had no concept of what programmers call a "top-down" approach to a reading task. He felt that to read a paper was to read and check the proofs of all the lemmas (supporting statements) at the outset, this despite a note on the first page of my paper stating that all these proofs had been checked and deemed correct by several mathematicians in the past, so that *initially* — on a first reading of the paper — it was not necessary to check them again. But he would have none of it. I thought: if mathematicians had been programmers in the 1960s, the craft would have choked on its own complexity and come to a dead stop.

Another of these English-challenged professors was the one I mentioned in the section, "A Retired Member of the Research Community" on page 1345. In November 2013 I wrote him again, saying that my Syracuse paper was much improved and would he like to take a look at it. He said he would do so within a week. More than a week passed without a word. Then he wrote saying he was unable to access the paper on my web site. I told him that on the home page was a link to my Syracuse papers. He had only to click on that link, and then, following the title of the paper containing my proposed solution were the words, "to download a copy click here". All he had to do was click where it said. (I didn't think such appalling ignorance was possible in a mathematics professor, active or retired.)

Even though he had read at least parts of my paper a couple of years previously, he had apparently forgotten everything except that he had regarded my description of the structure underlying the Syracuse function was "obfuscating". (No other reader had ever said that.) I controlled my temper and expressed my fond hope that we might possibly co-author a paper, since he had written several on the Problem.

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1. It dawned on me that the reason these incompetent speakers, or at least writers, of ordinary English lectured me so sternly about my inferior writing style was that they had difficulty understanding any written English, and lacking all facility at introspection, they blamed the author for their difficulties, especially if he was not an academic.

Refusing To Give Up

I was 77 years old — an age so preposterously beyond the age at which, according to the prevailing mathematics Culture, mathematicians have passed their peak creativity, namely, the age of 25, that no mathematician who knew my age would have spent five seconds on anything I wrote. Of course one reason, perhaps the main reason, for the Culture's belief was that mathematicians had not the slightest knowledge of the history of mathematics. If they did, they would have known that three of the greatest mathematicians who ever lived — Archimedes, Euler, and Gauss — had remained creative into old age.

I refused to give up, either in my attempt to find a mathematician who would agree that I had solved the Syracuse Problem (or at least who would be able to convince me that, if he felt my proofs contained an error, he had actually read them carefully), or in my attempt to develop my idea for a simple proof of FLT into an actual proof.

Late in 2013, I found a proof that I was convinced solved the Syracuse Problem. It showed that there was no difference between non-counterexamples and counterexamples to the Syracuse Conjecture, which implied that counterexamples did not exist. I published an announcement of the solution in a mathematics magazine in early 2014. Not long after, I found a way to apply the same strategy to what I believed was a proof of FLT. But in order not to be accused of being a crackpot, I merely called it an "approach" to a proof of FLT in my paper.

The Incomprehensible Dr. Z

Around this time, a waitress at one of my favorite restaurants had a boyfriend who was a graduate student at a European university. I will call him "Dr. Z", because he eventually got his PhD. She asked him if it would be all right if I wrote him; he said yes. I asked him to look at my Syracuse proofs. His reply was a multi-page email most of which was incomprehensible to me, and which invoked concepts, for example, non-standard Peano arithmetic, that no other reader had ever even mentioned. I asked him to look at my latest FLT attempt. Here, he rightly caught an error, although it took several emails for me to understand exactly what it was. I meantime had hit on a much better idea — one that required that a certain statement be true. He came up with a proof of the statement and the result was — in my opinion — the above-mentioned simple proof of FLT. He did not accept it, but as far as I could tell, the reason was that his extraordinary learning prevented him from considering the possibility that such a simple argument might be valid.

A month or so later in 2014 I found — or thought I had found — a another solution to the Syracuse Problem — one that justified an intuition I had had almost from the beginning of my efforts on the Problem, but that I had been unable to make logically convincing.

I asked Dr. Z to take a look at my Syracuse proofs. He dismissed them out of hand — I got the impression from his remarks that he had merely glanced at my paper, if that. It was clear that he regarded any idea of mine as something to be used to demonstrate his extraordinary, deep, learning, which of necessity he felt must be expressed in the most abstract terms. He never gave any sign that he had actually thought about what I had written (one simply doesn't do that with the work of outsiders).

Later, I sent a copy of one of his counterarguments to two mathematicians and a computer scientist, mentioning neither the name or the university of Dr. Z. All three said the counterargument was invalid.

In succeeding months and then years, I occasionally asked him questions about what I was reading, for example, the incompetently-written popularization by Saunders Mac Lane, *Mathematics Form and Function*[1], or a calculus text. He would reply promptly and incomprehensibly. I

asked him to keep his answers as brief and simple as possible, but I still couldn't understand him. Eventually, instead of asking questions, I wrote statements of what I understood, following each with "Right?" This usually worked. He seemed to have the entire undergraduate mathematics curriculum — and a good deal of graduate knowledge — at his finger tips. It was the old story: vast knowledge and zero curiosity about anything that wasn't approved by the mathematics Establishment.

He ignored my comments and criticisms about popularizations and texts and the presentation of mathematics, saying only that a text I had mentioned was a useful adjunct to a course. This too was the old story among mathematicians I communicated with.

Around November, 2015, I came across a paragraph in Mac Lane that began clearly enough, but then concluded with an equation that utterly baffled me. Since there may be a few readers with a mathematical background, I will reference the paragraph here[1], but the non-mathematical reader need not worry, as I will not go into any mathematical detail, with one exception.

In the middle of the paragraph Mac Lane suddenly introduced the term "T^*v". As was so often the case, he gave no definition. But he had explained that T^* was a function, and that v was a symbol for a vector, so I naturally assumed that the term stood for "$T^*(v)$", namely, that the function T^* was taking the vector v as its input. The trouble was that T^* only took certain other functions as inputs, and v was not a function. Yet this undefined term "T^*v" appeared on the left-hand side of the equation at the end of the paragraph. Another term on the right-hand side he also hadn't bothered to define.

I wrote to Dr. Z, received the usual incomprehensible reply. But I couldn't let go of the matter. And so, incredible as it will seem, I labored, in free moments, for *more than 1½ years*, sometimes just lying in bed, staring at the ceiling, and trying out possible meanings of that term. Every once in a while I would write to Dr. Z — "Is it ...?" — and he would reply no.

Eventually, in early April, 2017, I decided I would just ask him for a formal definition of each of the terms in the equation, including "T^*v". Why I hadn't done this on the very first day I didn't understand the meaning of the term, I have no idea. In any case, I didn't understand his response. I kept trying possible formal definitions for the term, and others in the equation. His response was always negative. And then, on April 20, 2017, I came up with a definition of "T^*v" that seemed to work in the equation. I sent it to Dr. Z, asking if it was correct. His email reply contained a single word: "Yes."

Looking back on that long ordeal, which already seems unbelievable to me, and which I know will have mathematicians scratching their heads over my willingness to spend such time and energy over a trivial matter, I can only think of the words that I would say to myself in childhood when confronting an apparently hopeless task: "*I do not give up*!"

Prof. X Reads My Solution

Prof. X read my new solution to the Syracuse Problem, didn't understand it though it was less than two pages long, and sent me a claim of an error. I wrote him back showing why his claim was wrong. He never responded to my correction.

He continued to refuse to read my proofs one sentence at a time, as I asked, and then tell me the first sentence that was wrong. Instead he sent me long, usually incomprehensible criticisms

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1. Springer-Verlag, N.Y., 1986.

1. It is the last paragraph on p. 200, under the section title, "7. Adoint".

that did not even specify which of my proofs they were supposed to apply to!

His usual criticism boiled down to the assertion — which most mathematicians made — that to compare two mutually-exclusive possibilities —for example, that the Syracuse Conjecture is true, or that it is false — implies that both possibilities exist at the same time (which, of course, would be contradiction). I had written several pages in my Syracuse paper showing that the comparison is legitimate, but readers almost never bothered to read these pages — I doubt if Prof. Y did. I kept trying to find more arguments to show that comparison is legitimate. In March, 2014 I came up with what I later called "The Two Sentences". Here they are:

(1) If a mathematician writes on a sheet of paper, "If *p*, then ...", where *p* is a statement, and below that, on the same sheet, he then writes, "If not *p*, then ..." he has *not* thereby written a contradiction!

(2) Furthermore, if in the first "...", he then shows that the integer *w* has the property *U*, and in the second "..." he shows that the integer *w* has the property *not-U*, he has *not* thereby asserted that *w* has both the properties *U* and *not-U* (which would be a contradiction)[1].

I sent an email to two mathematicians I thought might reply to my question, "Are the two Sentences both valid?" Amazingly, they both said Yes, although they both indicated clearly they thought the fact unimportant. I then sent the same email to Prof. X. He too agreed the Sentences were valid, and likewise said the fact was unimportant, and in any case that they did not free my proofs from error. (I thought: somewhere in the world there are a few mathematicians who are sharp enough to recognize that these Sentences open the door to a new kind of proof even without seeing mine.)

With the verdict of the two mathematicians I thought that I was done — that I had completed the goals that I set for myself some 35 years before, namely, to solve the Syracuse Problem and to come up with a simple proof of Fermat's Last Theorem. Of course, not a soul in the mathematics community would have agreed with me, not the least reason being that none of these souls would have deigned to actually read and understand what I had written.

I told Prof. X about my realization, and added that if I were asked how I wanted all my work in mathematics to be judged, I would say unhesitatingly: on my latest proof of the Syracuse Conjecture, and on my latest proof of Fermat's Last Theorem. He said not a word in reply.

In May 2015, he said he did not want to read any more of my proofs, claiming that, despite the validity of the The Two Sentences, they still implied that to consider a case and its negation implied that both cases existed simultaneously, a contradiction.

I accepted his decision (I had no choice) and promised him I would go back and re-read all his criticisms for the year. He said he would be willing to hear reports of other mathematicians' readings of the paper.

In re-reading his earlier criticisms, I came upon one that I had overlooked that revealed how bizarrely indifferent he was not only to anything I said but to some things that *he himself* said about my efforts. Specifically, a couple of years earlier, after several months of arguing, he had reluctantly agreed to the truth of a statement — which I will call "statement *s*" — that said that if a number maps to 1 via the Syracuse function, then it does so regardless if some other number is found not to map to 1. (The phrase "maps to 1" just means that 13 yields 1 at the termination of the sequence of calculations that are dictated by the Syracuse function when it is given 13 as

1. The "if"s in "If *p*, then ..." and in "If *not-p*, then ..." prevent this.

input.)

This had always seemed to me obvious, but other mathematicians, and the above-mentioned European graduate student, had claimed confidently that it was false. I had asked them when exactly the change in the behavior of the first number would occur: for example, if 13 maps to 1 today (and it does), and some other number is found not to map to 1 next week, when does 13 start to no longer map to 1: when the mathematician first makes the discovery in his mind? when he first writes down the proof? when he submits the proof to a journal? when the proof is first published? To me, it was absurd to imagine that 13 could stop mapping to 1, given that it does so via the rules of basic arithmetic. But the other mathematicians and the graduate student remained firm in their conviction.

In any case, Prof. X had agreed to the truth of the statement *s* that no such change in behavior can occur. And yet there, in one of his emails (March, 2014), was his calm assertion that my reasoning (he never referred specifically to any of my proofs) was invalid because the statement is *meaningless*!

I was appalled.

Following is my analysis of his psychology. When I first wrote him in the late nineties, he probably felt, "Here is this outsider, who, of course, can have discovered nothing of importance. But since he has an obvious interest in mathematics, I will spend a few minutes looking over what he sends me, then try to show him why it is wrong. There is no use my actually trying to understand the details of what he says because he can't possibly have discovered anything important. I will show how good-hearted I am, while at the same time spending as little as possible of my time in the effort, and while making sure that the prestige of the profession remains secure (because it does not allow outsiders to enter)."

As the years went by, I was occasionally astounded by how little he had understood of what I had sent him. On the rare occasions when one of his criticisms turned out to be valid, I thanked him, and reminded him thereafter that this was evidence that I was not afraid to admit I was wrong. Once I asked him whether he thought I was a crackpot. Very reluctantly, he said no, but that my stubborn refusal to admit that his criticisms were valid and that my proofs were worthless, threatened to put me in that category. I managed to refrain from asking him why his stubborn refusal to actually read my paper with understanding, and even to admit he had been wrong in his judgment about the validity of what I had done, did not make him, if not a crackpot, then at least a narrow-minded, unimaginative, uncreative, member of his profession.

Even though I was confident that I had in fact solved the two problems I had set out to solve so many years before, I continued to try to get other mathematicians, and the European graduate student, to agree that my solutions were correct. In the summer of 2014 I contacted two mathematicians who in the past had replied to my emails. I also contacted the graduate student. There ensued several weeks of email exchanges, in which the mathematicians, betraying no sign they had understood my Syracuse proofs, insisted more firmly than ever that I was wrong. The graduate student became nearly hysterical in his determination, at one point referring to my "stinking mathematics". But I remained confident that I was right.

A Bizarre Rejection of My Solution to the Problem

By this time, I thought I had experienced every possible rejection and snub by the academic mathematicians, but I was wrong. In 2014 I published announcements, as classified ads, of my solution, in publications of two mathematical societies. Although the total circulation of the publications was about 43,000, I received only one claim of errors in my solution. It came from a pro-

fessor emeritus. He said that my solution could not be considered correct until I had it verified by a proof checking program, but he was sure that it contained many errors. I asked him to tell me the sentences in which the errors occurred — as I requested, in the paper itself, of all readers who believed the paper contained errors. He quoted four or five such sentences. However, they seemed strange to me, so I had my word processor do a string search. Not one of the sentences was in the paper.

Prof. Y: A Mathematician With No Aesthetic Sense

I continued my occasional email exchanges with Prof Y. But his skill at finding objections, by the subtlest, most devious reasoning, to all my arguments, drove me to come up with a list of rules that I would now require any reader of my paper to follow if he wanted me to consider his criticisms. One of them was that the reader must give me clear, if informal, evidence that he understood my proof before he sent me any criticisms. I notified several earlier readers of this. The only one who bothered to reply was Prof. Y. He sent me a description of his understanding — but in it he arbitrarily changed the definitions of two variables in my paper! Doing so, of course, gave him a reason to reject my proof. A mathematician who would go to these lengths just to make sure that there was still a possibility that he could come up with his own proof, deserved not another moment of my attention. But I had no one else to communicate with, and so I controlled my temper, and merely informed him of his shameful tactic.

I asked him what his own, current approach to a solution of the Problem was. He sent me a brief description of it. It was, like most of the writings on the Problem that I had read, an all-too-obvious, pedestrian strategy that I was sure had been thought of by at least a few researchers in the past. But I said nothing.

I asked him what he thought had merit in my paper. He said that the diagram of a small part of the structure of the Syracuse function, had some interest, but even though he did not dispute my claim that no one had ever said the structure was invalid, it didn't say anything new, because everything in it was implied by the tree graphs that were well-known in the literature and were based on the original definition of the function. This definition was far more detailed, and the tree graphs were a classical case of the trees obscuring the forest.

I was astounded. What he said was the equivalent of telling a computer scientist that the programming language LISP, generally regarded as the greatest, though not the most practical, of all programming languages, was unimportant because everything that could be expressed in it, could be expressed in Turing machines (the most primitive of all computer languages). I realized I had been dealing, for more than 10 years, with a mathematician who had no aesthetic sense. And yet as every top-rank mathematician knows, a strong aesthetic sense is essential for any truly creative work in mathematics.

Still trying to find some way to make our email exchange productive, I asked him what he thought I could do to improve the paper. He said that I should get rid of the structure!

Then he said that it was far more important that I take some logic courses, because I clearly did not understand mathematical logic. I lost my temper, saying those words were surprising indeed, coming from a man who had no hesitation in arbitrarily changing the definitions of variables in a proposed proof.

Around this time, I discovered an error in what I had called the "most recent proof" of the Conjecture. (I realized that I had been unconsciously ignoring one of the basic rules in the comparison strategy described above.)

I wrote to Prof. Y and told him about the error. He said, in so many words, "There, you see?

All those mathematicians who had tried to convince you the proof was invalid had been right, but you had stubbornly refused to believe them." Here again I came very close to losing my temper, because the error I had discovered could be explained in two or three short sentences, and didn't need the pages of blather that the uncomprehending mathematicians had used to convince themselves and me that an outsider could not possibly have solved such a difficult problem

Later, he struck off in a new direction: he said that he had years of experience dealing with students who had difficulty with mathematics, and would I mind answering some questions? These asked about experiences early in life, a parent who might have destroyed what little ability I had with the subject, feelings of intimidation in school, etc. His previous suggestion that I start taking logic courses was insult enough, but now this latest was an outrageous humiliation. However since I had no one else to talk to in anything like a conversational manner, I suppressed my rage.

The End of the World — And Then ...

On Thursday, April 14, 2016, I realized that each of the two remaining proofs that I had thought solved the Syracuse Problem, contained a fatal error, because they applied to another function in which what I was trying to prove, was known to be false. It was one of the five worst days of my life. I could imagine the smirks and condescending smiles of the mathematicians who had told me for years that my efforts were worthless (even though they had understood none of the results). I removed the paper from my web site, and returned to the previous paper to see if that at least could remain. In going through it, and through a still earlier paper, I thought: I have nothing to be ashamed of. I have discovered facts about a beautiful structure that underlies the Syracuse function, and have made insightful observations about it. Furthermore there wasn't another mathematician in the world who had pursued this investigation.

A few days later, while trying to resign myself to spending the remaining years of my life doing nothing, I came up with another proof that at the very least had none of the flaws of the three that had failed. I sent a version to Prof. Y. As usual, he covered pages with reasons why I was wrong, none of them indicating he had understood my argument. I then sent a revised version to Prof. X. He said I should know by now that he would reject any proposed proof that used certain phrases that he had forbidden. He still refused to believe that the phrases could be easily replaced by non-forbidden phrases, as I had explained to him several times. Then, to show me how wrong I was, he issued a little challenge, claiming that if I used my reasoning, I would wind up proving a known falsehood. But the first sentence of his challenge contained the kind of blatant contradiction to my new proof that showed he had not understood it at all. I didn't bother to reply.

More Insults from Prof. Y, and Then the Light Dawns About His Approach to Mathematics

I continued coming up with new proofs. He dismissed them all. Then in an email he said:

"You can stop sending me proposed proofs. Instead I will teach you what a mathematical proof is from scratch. I will give you small lessons by email and assign some easy elementary proofs for homework and we will see if you can do them correctly and then work our way up to more challenging proofs and reasoning. That's really what you need to make progress. Think about it."

I decided to write him an email he would never forget. But just to have all my ammunition at hand before I began, I asked him to send me again what he had called , a month or so earlier, "John's Fallacy". He referred me to an April 2016 email he had written. It began:

"All of the variations of all of the proofs you have proposed over the year have had different wording and different symbols, but suffer from the same underlying logical error. So for efficiency I'm going to give it a name, and refer to it in the future when you send me a proposed proof. I'll refer to it as John's Fallacy.

"John's Fallacy:
"If a statement is true then it can't be false. So it must be true.

"That's it. That's exactly the fallacy in logic that, when you strip away all the business about [your discoveries and ideas concerning the Syracuse function]. It is what has been wrong about every argument you have tried to make. "

He gave an example of his own creation of this Fallacy of mine. The trouble was, it was a colossal misunderstanding of arguments I had used on several occasions.

I began writing my email. I said that the main reason he and I are so far apart in mathematics is that for me, ideas come first, logic comes second. I quoted the, for me immortal, words of the 20th century French mathematician, Jacques Hadamard"

"Logic is the means by which I convince the world of the correctness of my intuitions."

I said that the great mathematicians I have read who have commented on the nature of discovery in mathematics have all agreed with this. But it was abundantly clear that Prof. Y believed just the opposite.

I thought of his dismissal of my discovery of the structure underlying the Syracuse function, as described above, he saying in effect that the structure had no importance. I thought of his dismissal of a simple answer to a question I was sure had bothered many researchers, namely, why was the behavior of the function so seemingly chaotic? (He said the question had never bothered any researcher.)

I thought of the fact that he had never had the slightest curiosity about my papers on the Problem, and thus was ignorant of numerous discoveries I had made. I thought of the fact that he had never once made a suggestion as to how I might go about fixing an error he had pointed out, and that I had agreed was an error. I thought of the dull, uninspired line of research he and a colleague had embarked upon in an attempt to solve the Problem.

And it dawned on me that all this was the behavior of a man who simply couldn't handle ideas that had not been explained to him in classroom environments. An idea that he had never come across, and, that was the creation of an outsider, was beyond him. And so he had decided that the way to protect himself from his inability to handle original, unapproved ideas, was to place logic first (not second) — to use logic to demolish the ideas that were beyond his grasp, and to go so far as to claim that anyone who kept coming up with new ideas, suffered from a fundamental inability to understand logic and needed the ministrations of a kind person like himself, who knew that logic always comes first.

I never wrote the email, since I knew it would make no impression on him. He was a man who was incapable of believing he could be wrong about anything in mathematics.

I had no one else to communicate with about my work except Prof. X, who by then had imposed strict rules: I was not to send him any revisions of proofs I had sent him in the past; in fact I could only send him a proof that I felt could fairly be described as a "breakthrough"; if he felt there was a flaw in what I sent him he would probably not reply. If he did reply, I was prohibited from responding to his criticism.

So once in a while I wrote to Prof. Y, full well knowing he would bend all the rules of logic to convince himself, and try to convince me, that I was wrong. In February, 2017, after several weeks of false starts, I him a new possible proof of the Syracuse Conjecture. Here was his reply.

"Hi John,

"The entire argument is replete with pseduo [sic]-mathematical mumbo-jumbo starting with the very first sentence...

"... I agree we have been over this many times and I'm starting to concede that there is possibly no hope of you ever understanding this. I'm not sure why it is so important to you to prove yourself at mathematics. Did you have a bad experience as an undergraduate? Maybe you failed out of calculus and had to switch from a STEM major to a liberal arts major? I've encountered a lot of students like that over the years. Sometimes they end up with a chip on their shoulder and a strong need to convince everyone (including themselves) that it wasn't that they were just bad at math but rather that they were 'so good at it that they were misunderstood by their teachers', or 'they are like Einstein', or 'beyond their textbooks', etc. Maybe your father wanted you to be an engineer but you couldn't pass the classes and ended up graduating as an English major or something? I'm sure you are very talented in lots of ways, but having talked to you so extensively for so many years, I am curious on a personal level why, psychologically you are so obsessed with how people view your mathematical skills. What made you have such a strong need to try to prove yourself mathematically? Am I right about your educational career path? You said you worked for HP, but never say what it was you did for them. Did you start out pursuing a STEM career and unable to complete it?

Best regards,

..."

I felt that I had certainly earned the right to let this pompous third-rater know what I thought of him. But once again I decided that self-control was the wiser course. So I simply wrote him:

Prof. ...

"I am deeply hurt by your email, but rather than give you my opinion of YOUR mathematical abilities, or lack thereof, I think I should say nothing for the time being.

-- John"

Another Master of Logical Subtleties Reveals His Ignorance of Logic

I cannot resist closing this section without reporting on a truly embarrassing error in logic by Prof. X, who had spent years trying to convince me that my proofs suffered from errors so subtle that they could only be described in pages of emails. One of these claimed errors, I eventually figured out, was that a sentence of the form, "Whether or not the proposition *p* is true, the proposition *q* is true" is meaningless, or at least ambiguous. And yet there is nothing meaningless or ambiguous about sentences like, "Whether or not there is life on Mars, there is life on Earth," "Whether or not the Riemann Conjecture is true, Fermat's Last Theorem is true."[1] The only way that sentences of this form can be ambiguous is if the proposition *q* can be true *or* false. But that was never the case in my paper, since the proposition *q* was always true (it was a statement that was proved true in my paper.). But Prof. X was convinced he was right. Eventually, he forbade me to use any sentence of the form "Whether or not ..." in any email I sent to him.[2]

In May, 2016, I wrote him about a question unrelated to the Syracuse Problem. It asked if the number of truths is the same as the number of proofs. He said in an email:

"Logicians typically deal with statements that can be expressed in a given symbolic language, and proofs that can be written out using such a language and a set of rules of deduction. Such languages typically involve finite strings of finitely or countably many symbols, so one has countably many truths and countably many proofs -- the same number of each."

Which was true. He went on:

"However, though that is what they typically work with, they also consider lots of variants; and one of these could be a language with, say, a symbol for each real number, in addition to finitely many operation symbols, and, say, countably many variable symbols. In that system, there would be continuum many (hence, in particular, uncountably many) true statements."

Any first-year undergraduate math major knows about Cantor's famous (and beautiful) proof that there are more decimal numbers (real numbers) than there are integers. It is easy to show that there cannot be more proofs than there are integers. By a similar argument, it is easy to show that there cannot be more symbols than their are integers. Hence the idea that there can be a symbol for each real number, is nonsense.

I pointed this out as gently and obsequiously as I could, but Prof. X never replied.

You Gotta Have a Sense of Humor (2)

In early 2017, I told Prof. X and Prof. Y that my online paper had received more than 6,000 visits in 2016. At least 500 of them were almost certainly visits by professional mathematicians,

1. Riemann's Conjecture was asserted by Bernhard Riemann, one of the great mathematicians of the 19th century. As of the time of this writing (July 20, 2016) it has not been proved or disproved. Finding a proof of Fermat's Last Theorem was the most famous mathematical problem from the late 1600s to the mid-1990s, when a proof was found by Andrew Wiles.

2. Around June, 2017, I wrote to a leading expert on mathematical logic and asked if sentences of the form "Whether or not *p*, *q*" are meaningful and unambiguous if *q* is true. He said they are.

since that was the increase over the average number of monthly visits that followed the publication of two classified ad about my paper that I had placed in a popular mathematics magazine.

I proudly told the professors that I had received *not one claim of an error* in the paper.

Both professors replied immediately that the reason was that readers, and especially mathematicians, had recognized that the paper was worthless and hadn't wanted to waste time writing to tell me. Prof. X urged me not to mention, in future ads, that I had received no claims of errors, because that would only drive mathematicians away, since they would know that could only mean the paper was worthless.

An Expert on Mathematical Logic Pronounces In My Favor — But That Is Not Good Enough for Profs. X and Y

In late February/early March of 2017, I sent Prof. Y yet another possible proof of the Conjecture, this one based on my Comparison Strategy. He rejected it as before, saying that a comparison between two possibilities for a property of a given entity implies that there are two entities, but this is false, there is only one entity. I decided to write to a leading expert on mathematical logic and put my case to him, along with the counterargument of Prof. Y. The next morning I received an email from the expert. It said, "You are right." I told Prof. Y, but he attempted to wiggle out of accepting the expert's word.

Since Prof. X and I had spent countless hours arguing about the Strategy, I sent him an email telling him what the expert had said. Here is part of his reply.

"Certainly one can correctly reason that "If X is true then A" and that "If X is false then B" for various X, A, B -- I've never denied that! [But he had!] What I have pointed out is that one can't say "The *only* answer to such-and-such a question that would follow from X being true is A", unless one knows that X is true. [I had never made such a claim!]

"For instance, one can correctly reason that if 1+1=3, then 1+2=4, but one cannot conclude that 4 is the only value one can put in that statement -- one can also correctly reason that if 1+1=3, then 1+2=5."

[But no comparison of the type I had advocated for, applied to such obvious falsehoods. My Strategy applied only to cases where it was not known if a statement *p* was true or not, and so it was perfectly legitimate to say, "If *p*, then ..." followed by "If *not-p*, then ..."]

Prof. X's distortion of what I had said repeatedly is an example of his and Prof. Y's and other mathematicians unending determination to regard me as a failure.

Prof. X Allow Me Only One Proof <u>Per Year</u>

I had no one to communicate with about my work except Professors X and Y. By 2016 Prof. X had imposed strict rules: I was not to send him any revisions of proofs I had sent him in the past; in fact I could only send him a proof that I felt could fairly be described as a "breakthrough"; if he felt there was a flaw in what I sent him he would probably not reply. If he did reply, I was prohibited from responding to his criticism. I was not to use statements of the form "Whether or not *p*, *q*" in any emails or proofs, even if *q* was true (in which case, as I have explained above, the statements are perfectly legitimate).

In April, 2017, he wrote me saying that one of his family members was ill, and that therefore he would not have time to look at more than one proof of mine *per year*. Since the proofs I sent him were usually about a page long, and since I am confident that he never spent more than half

an hour on any of them (and that time was mostly devoted to writing out criticisms that usually had nothing to do with what I had sent him), he was in effect telling me that he would have less than an hour *per year* to spend reading and replying to my arguments.

For a few months, he was willing to answer an occasional question about the wording of classified ads concerning my paper, but in late August, 2017, he said he could no longer afford even the few minutes' time that that required.

Web Sites That Were Determined To See Me Fail

In 2015, I came across a web site that contained a long criticism of my solution to the Problem that the author claimed showed that my proof was invalid. The criticism had been written by a mathematician with whom I had communicated. It was clear that, like all the other mathematicians I had communicated with, his no. 1 goal was to satisfy himself that I had accomplished nothing. But by the time I came across the web site, I had removed that particular solution to the Problem from the paper. Furthermore, the mathematician's criticism contained the same errors, based on his superficial reading of my proof, that he had communicated to me via email.

It was not possible for me to inform visitors to the web site that the proof in question had long since been removed from my paper, because the first page of the web site was marked "closed". I wrote the web site managers, asking them to either remove the web site from Google or else to add a note saying that the proof had been removed from the paper. I told them that as it stood, the web site was unfairly discouraging visitors to my paper.

The managers refused, saying that the membership of the organization that sponsored the web site would not tolerate the web site being removed or altered in any way at the request of an outsider. I asked what the point was in keeping an obsolete criticism in Google. They didn't reply. And so every few months I would write them again, pleading with them to at least add a note to the web site.

They refused Eventually, in 2017, I told them I was going to inform other mathematicians of their practice. They told me to go ahead.

Around this time, I finally succeeded, after numerous attempts, in getting a mathematician who had read parts of a much earlier version of my paper, to take another look at it. He replied with an email that began, "I pretty much agree with the following found in Math Overflow..." I am not sure if that was the same web site as the one just mentioned, but from his invalid criticism, it was clear that he had not bothered to actually look at — much less read — any parts of my paper. When it came to the work of outsiders, some mathematicians felt that it wasn't necessary to even look at the paper: they knew it was worthless, and therefore any criticism at all would be correct.

In that same year, I came across a web site with a long contribution by a man who gave no indication of having any mathematical background; he was apparently a documentation designer. He gave his confident opinion that no one who was not an academic mathematician could have any hope of solving such a difficult problem, and he wondered why amateurs didn't recognize this. Here again, there was no way I could post a reply on the web site. I was unable to find, by searching Google, his email address.

My analysis in both cases — and in similar cases in the past — is that those who have never had an original mathematical idea in their lives, much less one about such a difficult problem, were determined to make sure that no one else whom they considered to be in their tribe (non-academic mathematicians) was going to show them up for the losers they were.

I Meet My Waterloo As a Self-Teacher of Mathematics Subjects

I had always been able to teach myself, from textbooks, the rudiments of any mathematical subject I chose. By "rudiments" I mean the principal ideas and the structure of the subject. I do not mean the ability to work all the exercises. In the late '90s, for reasons I have long since forgotten, I decided to look into algebraic topology. I had studied point-set topology on my own back in the early '70s prior to starting on my master's degree in computer science.

Since I had used James R. Munkres's point-set topology text, I bought his text on algebraic topology. But now I found things all but impenetrable from the very start. None of the questions that arose in my mind were answered. His examples of how to solve problems seemed to come out of the blue, and to rely solely on tricks. This was confirmed years later by an online lecture course taught by Norman Wildberger that I watched, in which the solutions to the problems, for example, the computing of what are called "homology groups", were described in all their laborious detail. I was getting nowhere, though I continued to try. The years passed.

I put my notes in the Environment form I had developed, but there were so many terms and symbols, many hundreds of them, that it would have required several large 3-ring binders. For the first few years, I simply berated myself for my stupidity, feeling that this was finally the proof that I had no business in mathematics. But then I began asking myself, "Why is this so difficult?" And, in answering the question, I realized how appallingly badly written the textbook (and one I bought later, by Alan Hatcher) was. The indexes were a disgrace. In most cases, if you didn't know what a specific symbol meant, you had no choice but to start searching for the definition staring at page 1.

I wrote up the many shortcomings and included them in an appendix of a chapter of my book about the Environment concept. The shortcomings, I realized, were only extreme versions of those that existed in the vast majority of mathematics texts — the atrociously-inadequate indexes, the omission of steps in the exposition, the frequent omission of justifications for the steps that were included, the stubborn adherence to the antiquated paragraph presentation of proofs, as opposed to the structured presentation described in the chapter "Proofs" in this book. Just about every fault that Morris Kline mentions in the chapter, "Follies of the Marketplace: Tirade on Texts" in his book, *Why the Professor Can't Teach*"[1] was in these two texts.

Even though I resolved several times to give up on the subject, I found that I couldn't stop trying to make progress. I bought other books, and found that most of them were devoted to the easier branch of the subject from a conceptual point of view, namely, homotopic groups. And so, to this day, I spend a few hours each week struggling, regarding it as my final burden in mathematics, a kind of lesson by the Fates that, even though I may believe I have solved two very difficult problems, I really have no talent for the subject.

Calculus Tutoring

One of my favorite restaurants was Rick & Ann's, located across the street from the tennis courts at the Claremont Hotel, in the lower Berkeley Hills — there is more about the restaurant in the next file. One of my favorite waitresses was a tall, slim, beautiful young woman in her mid-

1. St. Martin's Press, N.Y., 1977. It is no exaggeration to say that this book kept me going through some of my worst days in the long struggle with professional mathematicians. It is a thorough-going critique, by an insider, of most of the practices I had come to despise in these academics.

20s named Gaba. She usually waited on me for Sunday breakfast. I kidded her about the size of the hoop earrings she often wore. "Small children are going to get caught in those!" She was majoring in Earth Sciences at San Francisco State University, and had to take a semester of pre-calculus and then a semester of elementary calculus. She explained, somewhat sheepishly, that throughout primary school and secondary school she had spent most of her time on ballet, and so she had very little knowledge of basic algebra. Naturally, I offered to help her, and did so once in a while, in the restaurant, during the fall 2011 semester. When we started, she did not know how to add fractions, did not know that a negative number multiplied by a negative number is a positive number. I considered it a challenge to help her get a good grade. I didn't charge her.

She persevered, and got a B- in the course. She then asked for once-a-week tutoring during the spring semester course in calculus. We met, typically around 4 p.m., at Espresso Roma, on the corner of Ashby and College avenues in South Berkeley. Our meetings were friendly and not devoted entirely to school work. We talked about her family (one of her parents was Jewish, she had been raised in Philadelphia), about gossip at the restaurant, I told her a little about myself.

But I did too much talking when we were working on the homework problems, did too much of the algebra ahead of her instead of letting her do it and then correcting her, and so she did not do well on the final exam, and only got a C+ for the course. I felt very bad, and told her (truthfully) that she deserved at least a B–, and offered to write to her professor.

Nothing daunted, she then asked me to tutor her in the required course in physics, which was given in the fall semester. My practice of occasionally reviewing basic physics *concepts*, did no good here, as all that counted was her ability to work problems correctly and rapidly, and I had forgotten most of the techniques. I hated myself for letting her down.

She invited me to have Thanksgiving dinner with her and her boyfriend (he was a good cook and was to do all the cooking). I was very reluctant to accept, since it would mean that I would have to take my hat off, and she would see my baldness for the first time. But I went. There were several other of their young friends present. The dinner was not bad, and afterward, her boyfriend built a fire in the fireplace, and everyone sat around trying to fly paper airplanes into the flames.

She got a B in the course. But after that it seemed, she suddenly lost interest in talking to me. She was polite when I came for breakfast, but the old warmth was gone. Naturally, I had expected she would invite me out for a modest dinner by way of thanks for more than a year of tutoring, but she didn't. I told Gaby, "It's nothing personal! She is a natural user of people. Once I was no longer needed, I became merely another person she waited on."

Working full time and going to school full time unquestionably took its toll on her. In spring 2014, she no longer allowed me to ask how she was doing, and what courses she was taking. The mere thought of school made her depressed. After she got her degree, in June 2014, she brightened a little, became her warm, chatty old self. But I was not allowed to ask about her job prospects. Too depressing. She just wanted to enjoy not having to go to classes.

Amazingly, she eventually *did* get around to buying me that thank-you dinner. She happened to be working one evening in that same June. I asked to sit at the community table. She said that a group was coming in half an hour and they had reserved most of the table. I said that would be fine, I would just move to another table if I was still eating when they arrived. Which I had to do. She moved my plates and silverware to the table in the little waiting room, and I finished my meal there. When I asked for the bill, she said, no, she would pay for it — it was the least she could do by way of thanks for all that tutoring. It had been a year-and-a-half since the tutoring ended.

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I Find an Answer to the Calculus Questions That Tormented Me in My Youth

As I told Gaba several times, I got a lot out of the tutoring because it forced me to review mathematics that I had not worked on for many years. The old questions arose again (see chapter, "RPI", in Vol. 1), although as far as I know not in her mind: What is a number that is "arbitrarily small but not zero" (that is, an infinitesimal)? Is it a decimal number? If not, then what kind of a number is it? How can the quotient of two infinitesimals ($\Delta y/\Delta x$, representing the change in y divided by the change in x) eventually "become" the tangent to a curve as Δx becomes smaller and smaller (in calculus, the tangent is called "the derivative" and is designated "dy/dx"). I went over the standard drawing that is given in calculus textbooks[1]. Several weeks after the course ended, I suddenly saw a very simple geometric fact that answered all my questions — a geometric fact so obvious that I could well imagine why it had been overlooked. I added a description of my discovery to a chapter in one of my math books, and wrote up a one-page summary of the part dealing with $\Delta y/\Delta x$, added a drawing, and mentioned that questions about the nature of Δx, Δy, dx and dy had bothered some of the best mathematicians for at least 150 years after the calculus was discovered in the late 1600s. I sent it to G —'s professor. Naturally, I did not mention her name, or even that I had tutored a student in one of his classes. Here is his response:

> I don't know "best mathematicians" who were "bothered" with the question "What are dy and dx?". There is clear difference between the solid symbol dy/dx and two variables dy and dx related by the equation dy = f'(x)dx. This is explained in calculus textbooks.
>
> In regards to your text, I note only that the tangent to a curve is defined (!) by the derivative. It does not exist beyond differential calculus. You cannot prove calculus theorems by geometry. In fact, the situation is opposite - geometric concepts are introduced analytically using theorems from calculus.

I sent him the following reply:

> Prof. — :
> ...
> Your saying, "I don't know "best mathematicians" who were "bothered" with the question "What are dy and dx?"." reveals a depth of ignorance of the history of mathematics and, in particular, of the calculus, that makes any further communication a waste of time. I can only recommend what is probably the best history of the subject that was written in the 20th century, namely, Morris Kline's "Mathematical Thought from Ancient to Modern Times". Pages 385, 429, 433, and 954 of the 1972 edition are just a few of the pages describing the struggles of the best mathematicians of the time to understand dy, dx, dy/dx and related matters.

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1. Present and past calculus students will recall the drawing that shows the perpendicular x and y axes, the curve representing a function, the straight line that is the tangent touching the curve at one point, then the triangle with one vertex at the point and sides representing Δx and Δy (change in x and change in y as a point on the curve approaches the point at which the tangent touches). The hypotenuse of the triangle whose sides are Δx and Δy "becomes" the tangent when the point on the curve reaches the point where the tangent touches, and Δx and Δy are then said to "become" dx and dy.

As for the rest of your email, there are so many legitimate objections to what you say, that I don't even want to try to list them.

...

Best regards,

-- John Franklin

I then sent my one-page description to a post-doc teaching a summer calculus course at another university. He didn't reply. So I sent him my definition of the infinitesimal. I mentioned that I seemed to recall reading that mathematicians in the 19th century had experimented with a similar definition, and that I wondered why it was not included in calculus textbooks. The essence of the definition is that an infinitesimal is not some kind of single strange number that is always smaller than any number you can imagine, though never zero. It is a *set* of numbers that get smaller and smaller without ever becoming zero. The reader may find it hard to believe that a *set* of numbers (an infinite set, in fact) can be regarded as *a* number, but this was accepted already by the mid-19th century. And in fact we regard a set of numbers as being equivalent to a single number all the time without even thinking about it. For example, we regard the infinite set of numbers 2/4, 3/6, 4/8, 5/10, etc. as all being "the same as" 1/2.

This time the post-doc replied, explaining that the spam filter on the Mathematics Dept.'s email facility was so sensitive that it often discarded perfectly legitimate emails. As far as my ideas were concerned, he said that only the most advanced students asked about the infinitesimal[1], and that he felt it was best to pretty much avoid discussing it, so that students could spend their time mastering the techniques for solving the standard calculus problems.

A Physics War

Early in his career, Einstein devised a thought experiment to show that two events can occur simultaneously for one observer, but not for a second observer. The thought experiment is described in virtually all popularizations of Special Relativity. In the experiment, a lightning bolt at a distance d in front of a train moving in a straight line on a level track at constant speed v, strikes at the same moment as a lightning bolt at a distance d behind the train. An external observer on a nearby hill sitting on a line perpendicular to the center of the train at the moment of the flashes, sees the flashes as occurring simultaneously, while an observer sitting in the middle of the train sees the flash in front as occurring before the flash behind. The reason for this difference is that the closing speed of photons: from the back of the train and the center of the train, is $c - v$, where c is the speed of light and v is the speed of the train, whereas the closing speed of photons: from the front of the train and the center of the train, is $c + v$. Since the distances traveled by the two sets of photons are the same, the photons from the front of the train reach the observer sooner than the photons from the back[2].

1. The reader may recall how I was tormented, from my very first semester of calculus, at RPI, when I was 18, by my inability to understand what an infinitesimal number was. I naturally assumed that my inability was due to my being one of the least bright students in the class, that no other student, much less the professor, was bothered by this strange number. And now, the post-doc informed me that my torment was due to my being one of the advanced students!

Around 2015, I began wondering what the observer would see if instead of lightning bolts, two flashes of light were emitted by light sources attached to metal booms *on the train*, one extending a distance *d* in front of the train, the other extending a distance *d* from the rear. I felt that the observer in the center of the train would see exactly what he had seen before. I then realized that if this were true, it would be an exception to a basic precept of Special Relativity, which states that it is impossible for a person inside a closed vehicle to test if the vehicle is moving or not. Yet if we regard the train, including the metal booms, as a closed vehicle, the observer would know that the train was moving by the fact that the flashes seemed to occur at different times.

I started writing to physicists asking them if they agreed with my conclusion.

Responses of Physicists Who Were Told About the Possible Exception to the Precept

Several physicists said in no uncertain terms that any suggestion that there might be an exception to a basic precept of Special Relativity could only come from a crackpot.

Two said that the error in my thinking could only become clear via higher mathematics. The error could not be explained at the photon level of Einstein's original model.

But several other physicists said that higher math was not required. I was simply confusing the various inertial frames (the technical term for a closed vehicle) involved. (But I was using only one inertial frame, namely, that of the train.)

Others said that the error in my thinking was well-known, and was explained in most textbooks. (I looked at six textbooks, plus a variety of Google articles, and found nothing about my version of Einstein's model.)

One physicist said he performs "frequently" in the lab in his apartment, an equivalent experiment and always finds that the photons from the front and the back arrive at the observer at the same time. (But his apartment was not moving at a constant speed *v*. When I questioned his experiments, he said I was never, ever to write him another email.)

Another physicist said that my error lay in my claim that light has two speeds, $c + v$ and $c - v$, whereas it is known that light has only one speed. (But I made no such claim: the two speeds are *closing speeds* of photons and the observer, as I clearly explained. The speed of light, *c*, is the same in both. I pointed this out to the physicist. He ignored my argument. He then said that I was merely re-inventing "Galilean invariance"[1]. When I asked him to explain how I was doing that, he said that my refusal to accept his criticisms as correct was "breathtakingly arrogant" and that I was never to write him again.)

---

2. If two objects are moving directly toward each other in a straight line, then their closing speed is r + s, where *r* and *s* are the speeds of the objects. If one object is moving at a speed *r* in a straight line behind another object moving in a straight line away from it at a speed *s*, and $r > s$, then the closing speed of the two objects is $r - s$. The closing speed is in general *not* the same as the speed of either of the objects. So we are *most certainly not* making the false claim in the above that light has two speeds, $c + v$ and c – v. Light has only one speed, namely, *c*.

1. This is the principle, set forth by Galileo in the early 1600s, that it is not possible to tell, from within a closed space moving at a constant speed, if the space is moving or not. Galileo's closed space was the interior of a ship moving at a constant speed on completely calm water.

Possible Explanation for Physicists' Criticisms Having Little To Do With What I Actually Wrote

No physicist ever bothered to explain where the error in my assertion lay. It seemed clear that once a physicist knew that I was setting forth a possible exception to a basic precept of Special Relativity, he felt no need to actually read and think about the modified version of Einstein's model that I presented. Since there could be no exceptions to the basic precepts of Special Relativity, virtually anything could serve as a valid criticism. For example, I was amazed to learn that, after some six months of communicating with me about my assertion, and presumably thinking about it, one physicist revealed that he wasn't sure if the train in my version of the model, was moving!

How I Got the Physicists To Agree I Was Right Without Admitting It

In early 2017, I began sending emails to physics professors in which I only described my variation of Einstein's thought experiment, and then said that if the physicist did not reply to my email, I would assume that he agreed that the observer in the train would see exactly what he saw in Einstein's original model. I made no mention of what their agreement implied regarding the basic precept of Special Relativity.

Ten out of 12 of the physicists I wrote to did not reply, so I assumed they agreed I was right. I wrote to two of them, informing them of the implication, and asking for their thoughts. I guaranteed complete confidentiality. Neither replied. Then, one morning at Peet's Coffee and Tea store near Rick and Ann's restaurant in Berkeley, where I often went for breakfast, I was sitting at the high, narrow table facing a front window. A man came up, asked me what I was studying. I said mathematics. I asked him if he was a mathematician. He said no, a physicist. He worked at a major lab in the area. I immediately told him that I would welcome his answer to a physics question I had. I described my modification of Einstein's model, then asked him what the observer would see, and without a moment's hesitation he said he would see the same thing as before. I then told him what this meant for a basic precept of Spacial Relativity. He smiled, and nodded thoughtfully. We shook hands, I told him my name, he told me his, which I immediately forgot, and he left. I forgot to ask him for an email address, so I have been unable to contact him again.

But in any case, at the time of this writing (August, 2017) I believe I may have discovered something important.

A Philosophy War

In spring of 2016, I began communicating with a woman philosophy professor at a second-rate university. She spoke favorably about "po-mo" (post-modernism), and in particular had great admiration for Jacques Derrida, a philosopher I utterly despised. She had published a book on a lesser-known 18th century philosopher, but I found it incomprehensible.

She had no knowledge of classical music, but was "willing to learn", and, as far as I could tell, no knowledge of literature.

We exchanged emails for a year-and-a-quarter, at which time I told her I felt there was no point in our continuing, since neither of us understood what the other was saying.

I was appalled that a person of her shallowness, her ignorance of mathematics and formal logic and science, could have had a successful academic career (she was near retirement). Let me hasten to say that I certainly did not expect her to have taken courses in mathematics, logic, or any of the sciences, but I most certainly did expect her to know how these disciplines "worked", in

particular how truth was defined in them. But she dismissed the truth concepts in these disciplines on the grounds that they were derived from the faulty metaphysics of the ancient Greeks.

She also had little use for the Correspondence Theory of Truth, which states that an utterance is true if it corresponds to some reality. The modern world takes the Theory for granted, for not only is it the basis of all of mathematics and the sciences, it is also the basis of everyday life: we check our bank statements to see if they correspond to the reality that is the amount of money in our accounts; we teach children to tell the truth (to say what corresponds to this or that reality); the courts are mightily concerned with the truth, that is, statements that assert what actually happened, college professors check homework and exam papers to see if the students have written what corresponds to the realities that have been presented in the course, etc.

But she dismissed it all, arguing instead for the Pragmatic Theory of Truth, which asserts that what is true is what is "useful". The Theory was promoted by the American philosopher William James, and the American mathematician Charles Peirce, and others in the early 20th century. Bertrand Russell, in his *A History of Western Philosophy*, points up some of its shortcomings. \

She claimed that I merely asserted my beliefs, without justifying them with philosophical arguments. I asked her time and again to quote passages from her favorite philosophers in which these justifications were given. She eventually said:

"All of Nietzsche's and Heidegger's writings lay out their assumptions, premises etc. But they don't put them in the form of a "justification," (ie logical or rational justifications) because they understand such justifications are simply a function of having accepted assumptions different from theirs. I'm beginning to think you don't understand what "premises" or "presuppositions are unless they're put in a logical or scientific argument. Which only proves my point that you don't understand the difference between philosophy, analytic philosophy or science."

For me, this paragraph was nonsense. I told her one of our problems was that I understood the difference far better than she did.

I also told her that there are no valid philosophical arguments, because there are no universally-agreed-upon criteria in philosophy for judging whether an argument is valid. I told her that of all philosophers, Spinoza, in his *Ethics,* probably made the most concentrated effort to produce valid philosophical arguments, since each of his statements was backed by "proofs" in the form of Euclid's mathematical proofs. Yet no one considers those proofs valid today. She made no reply.

She frequently said that one of my problems was that I knew nothing about the history of philosophy. I replied:

"I have a six-foot by three-foot bookshelf filled with books on philosophy. Over the years, I have read at least portions of the works of Socrates/Plato, Aristotle, St. Augustine, Aquinas, Roger Bacon, William of Occam, Descartes, Francis Bacon, Pascal, Leibniz, Locke, Berkeley, Hume, Kant, Hegel, Schopenhauer, Nietzsche, Bergson, Russell, Wittgenstein, Heidegger, Sartre, Camus, Foucault, and Barthes, plus an overview of Derrida. I have read at least portions of at least six histories of philosophy, by far the best, in my opinion, being Russell's "History of Western Philosophy", which I continue to re-read, and Passmore's excellent "100 Years of Philosophy".

"Have you read an equivalent amount of writings on math and science?"

She said that all I had done was "read around" in philosophy, but that I had not made an in-depth study of it. She felt no need to spend any time on mathematics or science. As a result she had no idea of the difference between philosophers who thought in terms of Objects (Descartes,

Kant, and Russell are examples), and philosophers who did not (Nietzsche and Heidegger are examples). Heidegger's lifelong crusade against technology was in fact a lifelong crusade against the Object — against the way of thinking that occurs in mathematics and science and engineering and in disciplines derived from them. Hence, for example, the absence of formal definitions in his writings, especially in his major work, *Being and Time*.

And on it went, until I decided to terminate the exchange.

It seemed to me that the best summary of her philosophical thinking was that it was entirely *word-based — perhaps I should say, "literary" — and concept-free*. For her, philosophy was a discipline in which one used big, important words in order to express one's feelings (though feelings were never mentioned) about certain aspects of the world. Truth was something that lived in words; the slightest change in the wording of a phrase or paragraph meant a change in the truth being expressed. Concepts, in the sense of ideas that exist independently of the precise words in which they are expressed, — ideas such as matter, mind, sense perception, truth, reason, idealism, empiricism, ... — did not really exist for her. Only the words existed, to be used in the sense that her feelings required.

I sent her an email containing the following:

> In Lewis Carroll's "Through the Looking Glass", is the following passage:
> "When I use a word," Humpty Dumpty said, in a rather scornful tone, "it means just what I choose it to mean -- neither more nor less."
> "The question is," said Alice, "whether you can make words mean so many things."
> "The question is," said Humpty Dumpty, "which is to be master -- that's all."
>
> In our long interchange, I feel that for you, words mean what you want them to mean. For example, consider the words, "assumption" and "justification" as you use them in your email [of 8/17/17]. I am (A) utterly baffled by what you mean, and (B) am still waiting for you to quote a sentence or passage in either Nietzsche or Heidegger that exemplifies what you mean.

(Her response to (B) was given above, in the quoted paragraph beginning "All of Nietzsche's and Heidegger's writings...")

Her lifelong abhorrence of the technical disciplines arose, I firmly believe, from her recognition that here the concepts — the Objects, t*he abstractions* — exist separately from the precise words in which they are expressed. She was essentially an English professor, and people like me were essentially as annoying and irrelevant as engineers.

On 9/5/17 I sent her the following email:

> Dear ---
>
> I realize we are not communicating any more, but my struggle to understand some of the things you have said, goes on.
>
> I have written to a philosopher asking for references to books and/or papers that might give arguments supporting your claim that mathematical and scientific truth is suspect because it is

derived from the faulty metaphysics of the ancient Greeks. He has not replied, which suggests to me that such books and papers are not widely known, if they exist at all.

I know that you need to believe that I know nothing of the history of philosophy, but I assure you I do know something about the history of mathematics. -- e.g., that mathematics was discovered (invented) by Greek thinkers in the 500s b.c. Long before Plato was born. These thinkers laid down the rules governing the process that begins with premises (axioms, postulates) from which are derived, by stated rules of inference, the facts known as theorems and lemmas.

So the assertion -- which I believe is the one you are making -- that mathematicians began by building on the faulty metaphysics of Plato, is simply wrong. The mathematicians had defined their truth long before he was born.

She never replied.

But despite my repeatedly saying there was no point in our continuing our email exchange, I kept trying to find out the reason for her prejudices against mathematical and scientific truth. Since she was not about to explain why in her own words, I asked her for references to authors who shared her views. Eventually, she mentioned the American philosopher Richard Rorty (1931-2007), who was a proponent of the pragmatist theory of truth.

I found several interviews with him, and talks by him, on YouTube. But after a while, I wrote her the following email.

Dear S —:

I have been watching those Rorty videos on YouTube, and although I will continue to do so for a while, to me all this is just words.

But saying that immediately makes me think of the philosophy in which I first immersed myself, namely, existentialism.

I read a lot of Sartre, and a considerable amount of Camus. This was in a day when all the young intellectuals were doing the same.

The phrase "just words" never occurred to me -- not once! -- during those years.
Sartre, like Nietzsche, was a literary genius. His story, "The Wall", is unquestionably one of the world's great short stories. "Nausea" and "Roads to Freedom" are exceptionally good novels. At least some of his plays are in the same category.

However, I was unable to understand much of "Being and Nothingness".

I'm not sure I had any real criticism of Sartre (surely that surprises you!), but I did have a question I would have loved to ask him in response to his assertion that "man is a useless passion" because human life is "meaningless", namely, why were there few, if any, existentialists among physicists and mathematicians? Could it be that physicists were opening the heavens

and the sub-atomic world, and mathematicians were making earth-shaking discoveries that no one had believed possible, e.g., that there are mathematical truths that cannot be proved. For physicists and mathematicians, life was certainly not a useless passion!

I repeat: for me existentialism was not "just words". Nor were the writings of Nietzsche or Heidegger (or Hannah Arendt).

You have heard my criticisms of Foucault and of Derrida, so I won't repeat them here.

Holiday best wishes,

-- John

Readers who would like a more extensive presentation of my philosophical views should go to the chapter “Philosophy” in my book *Thoughts and Visions,* on thoughtsandvisions.com.

Spurned Even by Two Linguistics Professors

Around noon one day in April, 2006, while waiting in line at the counter of the little restaurant at the Musical Offering, just across the street from the U.C. campus, I noticed an attractive young woman next to me. I asked her if the book she was holding was a linguistics text, as that was what the title suggested, and she said yes, it was. (She had black hair, bright eyes, and, at least in memory, she wore a blue dress.) We got to talking and after we ordered each of us somehow found it all right if we sat at the same table.

I thought she was a graduate student. I am not sure how much I asked about her research, but — unable not to take full advantage of my opportunity — I was soon telling her about some of the linguistics ideas I was working on. She listened attentively, seemed genuinely interested, and at one point remarked that some of these ideas were subjects of resarch at Stanford. At the end of the conversation, she told me her name.

When I got home, I was so inspired by her having listened to me that I felt I had to send her an email that would present my ideas in a more organized fashion. I called the linguistics department at the university and was told that she wasn’t a graduate student, she was a professor. The department secretary gave me her email address. A few weeks later, I wrote her the following email:

Dear Prof. — :

You may remember that we had a conversation around noon at the Musical Offering during the week of Apr. 24: it began with my asking you if a book you were holding was about linguistics. I found it a very pleasant conversation, and I hope you did too.

Since then I have wanted to present, in a clearer and more concise form, the main ideas I brought up. I will certainly understand if you have no time to reply. In any case, please be assured I have no intention of dragging you into a long email exchange. I will do my utmost to be brief.

To begin: several years ago, it occurred to me that Wittgenstein's famous assertion, "The

meaning is the use", could be interpreted as "Semantics is pragmatics". I then began wondering if syntax could not also be interpreted as pragmatics, and decided that the answer might be yes: a natural language grammar describes how one says what one wants to say in the circumstances that it is appropriate to say it.

At the time, I was trying to come up with a better way of approaching the learning of a new foreign language (I realize that learning foreign languages is of only incidental interest to the academic study of linguistics, but please bear with me for a moment), since I had long felt that the traditional grammatical approach was "backwards": it seemed to me that grammatical rules are something that we come to intuit after we have learned sufficiently many words and phrases and sentences in a language. It seemed to me that a grammar is not a set of instructions for how to speak a language, but rather an abstract summary -- a report -- of the words, phrases, sentences, etc., that a certain speakers (e.g., native, educated speakers) use all, or nearly all, of the time.

And that led directly to the idea of a frequency-of-occurrence approach to language, rather than a grammatical approach. I remembered how I was bothered, in jr. high school Latin courses, by having to memorize cases of nouns, verbs, and adjectives, e.g., "hic, haec, hoc, huius, huius, huius, huic, huic, huic, hunc, hanc, hoc, ..." I thought even then: "How often did the Romans say these sequences of words? Probably never, unless they were grammarians." On the other hand, we students never found out how to say sequences of words that Romans probably said every day, e.g., the Latin equivalents of "How are you?", "I am hungry", "Which way is the forum?", etc.

You might reply that we weren't learning conversational Latin, but rather literary Latin. But I feel the same argument applies. We never learned how to write a letter or an essay by merely selecting from a list of words and sentences. What we learned was a complicated set of rules which, even if we followed them carefully, offered no promise that the resulting sequence of words and phrases would be in fact what a Roman would have used.

(Speaking of conversational Latin, you might enjoy Henry Beard's amusing "Latin for All Occasions" ("Lingua Latina Occasionibus Omnibus").)

So I decided, a few years ago, that it would be far better if the form of a book for learning a foreign language were closer to that of the phrase book that tourists use. All the most common phrases and sentences in the language would be listed, in alphabetical order, by their equivalents in the native language. Thus, under "H" would be found "How are you?", followed by the equivalent phrase in the foreign language. Under "W" would be found, "What time is it?", followed by the equivalent phrase in the foreign language, etc. (Of course, in some languages, e.g., German, in which the noun typically comes first, and the verb last, a "frequently-occurring phrase" might well have other words between the elements of the phrase.)

(I believe there are now electronic devices available on the market that allow the user to type in or otherwise select a phrase in a foreign language, and the equivalent words are then heard through a loudspeaker. I think such devices have been used by the military in the Iraq war.)

I said above that I was aware that the academic discipline of linguistics is only incidentally concerned with the learning of foreign languages, and so now let me get to more abstract matters. One test of the academic value of a set of ideas in linguistics can be obtained by considering the problem of computer translation of natural languages. Using the above ideas, a computer program for this purpose would consist of a data base containing a large number of the most-frequently-occurring phrases in the target language along with, possibly, a representation of the corresponding semantics. Parsing would be a last resort, when the above data base failed. Furthermore, the program would make it easy for new phrases and sentences to be entered manually, with appropriate semantics. (I am simplifying here, for clarity. Certainly something along the lines of "grammatical rules" would be required even with the data base.)

Which brings us to the subject of Chomsky's contributions to linguistics. There is no question but that he made a seminal contribution to the theory of formal languages. In particular, there is no question that what he claimed for five-year-old human beings -- that they can, in principle, generate an infinity of strings in their language -- is true of formal grammars: such a grammar is a finite set of strings ("productions") that, if the productions are of a certain form, e.g., "A can be replaced by bA", can generate an infinity of grammatical strings. As I'm sure you know, Chomsky's ideas proved invaluable to the design of compilers for high level computer languages.

But I think he did major damage to linguistic thinking in arguing that what was true for formal languages was true for natural language. I no longer believe that any speaker of a native language can "in principle" generate an infinity of grammatically correct strings in the language. Instead, I believe that native speakers learn a relatively small set of strings of words and phrases which they then assemble in a limited number of ways, over and over, as daily experience demands (pragmatics again).

But we are again talking about frequency-of-occurrence, and that means that we are in the realm of information theory, where a string with a high frequency of occurrence has low information, a string with low frequency of occurrence has high information. So it seems to me that one of the most important properties of any natural language -- or, I should say, any context in any natural language -- is its information content, in the formal sense (which is not the semantic sense). I believe that this property is every bit as important as grammatic rules.

To summarize: "Grammar is for grammarians"; frequency-of-occurrence, i.e., information-theoretic content, should be of fundamental importance in linguistics; Chomsky was right about formal languages, wrong about natural languages.

Let me say again how much I enjoyed our conversation. But I had no idea you were a professor! (I only found out after I called the Linguistics Dept. The person answering the phone said you were, and gave me your email address.) Instead I thought, "Boy, some of these grad students are really sharp!"

All the best,

-- John Franklin

No reply. But then I thought: "Of course! She is still in the glow of beatitude at having been appointed a professor at one of the nation's — the world's — great universities! She is not going to want to reply to an informal *email* from a guy she met at a lunch counter, for God's sake." So I wrote a ten-page paper, presenting all my points as formally as I could, including appropriate references, and sent it to her. She never replied.

But within a month after I put the paper on my web site, it became one of the most frequently visited each month — I'm sure for the wrong reason, namely, because the word "pragmatics" appears in the title.

In late 2012 or early 2013, one of the music store clerks at The Musical Offering in Berkeley happened to remark during a conversation that her father was a professor of linguistics. Never one to miss an opportunity, I asked her if she thought he would be willing to read a short essay on linguistics I had written. She gave me his email address, I wrote him, and he agreed to look over the essay, which was about 15 pages, and then get together with me and talk about it. I sent him the essay and he set a time to meet. As I recall, he said, after we had shaken hands and ordered our coffees and sat down, "I read your essay,". He made a few perfunctory remarks, and then for the next 40 minutes he talked about his travels to conferences around the world. At one point I asked him if he knew if my idea regarding frequently-occurring phrases was being used in computer translations. He said he believed it was, then resumed talking about his travels.

An Electronic and Mechanical Genius

One of the most remarkable men I ever met was the computer consultant for my next-door neighbor Steve, a man named Art S. —. Although I had an excellent consultant, Aaron, I used Art when I wanted a second opinion and then, later, when problems arose with my wireless connection to the Internet. (As was increasingly common from 2006 on, one neighbor would have a device called a "router" in his house, or, in Steve's case, in the garage he used as an office for his bookkeeping business. Other neighbors would then transmit and receive the necessary signals for their Internet connection to and from that router via small antennas on the backs of their computers. The router in turn was connected to the Internet via cable or the phone company's DSL line.)

Physical Description, Speech

He was in his late fifties, thin, with a pig-tail hanging down from the back of his head, though he wasn't bald. He had a wispy goatee and walked with a bit of an old man's stoop, although in a quick manner that would make you describe him as "spry". He had a Tennessee accent, an old-man voice, and spoke rapidly, with frequent repetitions of the phrases "what happens is..." and "basically" and "make a long story short..." since much of his talk was explanations of machines and technical concepts. When a mechanical or electrical part was no longer available, he would say it was "made of unobtainium". He often referred to security software, with its ever-present warnings about the customer's susceptibility to viruses, worms, spyware, Trojan horses and the rest unless he or she bought the latest upgrade to the company's software, as "nagware", or "nannyware", or "scareware". His term for Microsoft's ungainly, poorly-constructed software that was always requiring more and more memory was "bloatware". He had particular contempt for this company, often railing against the stupidity of some of their design decisions. He respected

Linux's Open-Source software which, although the users could add and modify the system software on their own (with suitable approvals and checks) was much more reliable and better engineered than Microsoft's products, which were kept under tight secrecy and control.

When I expressed disbelief that a piece of software could do something he had just described, and he really didn't have time to go into the details, he would wave his hands and say, "Don't worry. It's fairy magic." When we got onto a subject which revealed the stupidity of the government or of the software manufacturers, and he didn't want to allow it to take away time from the work at hand, he would shake his head and say, "Don't get me started". Sometimes he would point to his head and say, "See these gray hairs? I got them from dealing with these idiots. I'm really only 28."

For some reason, he didn't like the women whose recorded voices accompanied various events in the operation of a piece of software. For example, "Welcome to CompuServe!" "You have mail!", etc. When one of these messages occurred while he was working on my computer, he would say, "Thanks for sharing, *bitch*". In conversation, he referred to her as "Robo Bitch". I know of no reason for his hostility toward this woman with a pleasant voice who, after all, had only been trying to earn a living.

I managed to maintain a sense of humor even when, as a result of the latest computer problem, I had the gun loaded and had written my farewell note to Gaby . During one of these black periods, the subject of Mel Brooks' film *High Anxiety* came up, and he said that it featured a place that was ideally suited to people like me, namely, the Psychoneurotic Institute for the Very, *Very* Nervous. But then he caught himself, said no, what I needed was the *Cyber*neurotic Institute for the Very, *Very* Nervous, and I thought the term was perfect, and could well be the source of yet another branch of psychotherapy.

Another time, when I nervously called him about a noise in the computer (probably due to the fan blade having expanded slightly in the hot weather and scraping on something) and at one point asked him, "Am I going to have a crash?", he replied, laughing, "Probably, but your computer will be fine."

Whenever I spoke of my fears, for example, of earthquakes, and attributed them to my neurosis, he commented that he was neurotic too, and had many of the same fears. (He thought it possible that earthquakes might be caused by the moon.) And, like me, he was driven to distraction by the racket of power saws and leaf blowers in his neighborhood.

Brief Biography

He had been born in Kentucky. The family then moved to Tennessee where they lived for his first 11 years, then moved to Montgomery, Ala. He said once that when he was 12, his father had taught him the binary code (the one used in computers, which requires only the two digits 0 and 1 to represent numbers) and the binary powers (powers of 2) up to 2^{16}, whereupon he promptly rattled them off at breakneck speed: 2, 4, 8, 16, 32, 64, 128, 256, 512, 1024, 2048, 4096, 8192, 16384, 32768, 65536. He said his father had also taught him how a superheterodyne radio receiver works. I thought of my father, who had always been glad to teach me and my brother how to use mechanical tools — the correct way to drill and saw and fasten pieces of wood together (use screws, not nails!). And yet all my life I have had a morbid inferiority complex about my ability to use tools. On the other hand, the teachings of Art's father had resulted in a man who delighted in working with mechanical and electrical things, and in fact a genius for doing so. If I were forced to explain the difference in us, I would say that, as a boy, he probably didn't hold his father in the same awe as I held mine. Maybe it was no more than a case of his

father just happening to know some neat stuff. I never asked Art about this, but I am guessing that his father was not an engineer who was the president of a company and who commuted to a big city every day and published papers in important journals and spent his evenings working on inventions.

Art said that in high school he had become involved in broadcast radio and had tried to get rid of his Southern accent, but had been unsuccessful. He was a stand-up comedian briefly (two performances). After two years of college, he got married (this was during the Vietnam War years), had a daughter and a son. His brother was the head of a plastics firm, he having previously made a reputation for himself by applying W. Edwards Deming's ideas of statistical quality control, which were then new, to the manufacture of a plastic product that until then had proved difficult to make.

Perhaps one reason I felt comfortable around Art was that he was a natural self-teacher. He told me that one of his early projects when he was learning programming was a program to generate prime numbers efficiently. (A prime number is one that has only itself and 1 as factors. Thus, for example, 5 is a prime number because its only factors are 5 and 1, whereas 6 is not, since its factors are 6, 3, 2, and 1.) He immediately recognized the importance of what mathematicians call a "closed form" representation of the prime numbers, that is, a formula which was capable of generating all the prime numbers, as opposed to a mere program that would generate successive primes by trial-and-error. Not long after — in the eighties — such a closed form was discovered by two mathematicians.

Then a few years later he decided to learn to fly, and earned his single-engine pilot's license. Although he had long given up flying small planes when I knew him, a relic of his pilot days remained in his speech, for, when he repeated back the digits of a phone number or a computer code you had read off to him, he always pronounced "nine" as "niner". Despite, or perhaps because of, his experience as a pilot, he had a deep-seated fear of flying in commercial airliners. One reason may have been that he knew two of the mechanics who had been on duty at an Alaska Airlines terminal the night before a fatal Alaska crash. He knew that one of the mechanics was a chronic drunk.

He also had had close calls on several commercial flights — the planes having to make emergency landings. Regarding his fear of flying, he said, "I think too much and I know too much."

In intellectual matters, his learning had a rough-hewn, home-made quality. He knew what Maxwell's equations[1] were about, but he hastened to admit that it had been many years since he had studied them. One day, in November, 2008, as he was using an aerosol can of compressed air to blow the dust off the fan and various parts in the back panel of my computer, he paused and held the can toward me. "Feel it!" he said. I did, and commented on how cold it was. He (delighted): "Right! Boyle's Law!" But, as I found later on checking one of my physics texts, the phenomenon was not due to Boyle's Law (which was first published in 1662 and which relates the volume and pressure of a gas) but rather to Gay-Lussac's Law (discovered in 1802 and which relates temperature and pressure — in particular, it states that if pressure goes down, which it does when some of the compressed air is allowed to escape, then so does temperature). Both laws were later incorporated into what is known as the ideal gas law. But he was in the right ball-park.

About the same time I discussed earthquake-damage protection and pointed out the rope I had run over the top of my current computer (a Dell), old LaserJet printer on the left, and Beyond

1. These set forth the laws of electromagnetism. They were derived by the physicist James Clerk Maxwell in the 1860s.

2000 clone computer from the nineties on the right. The rope ran under the work table, was pulled tight, and the ends were tied. Its purpose was to prevent my computer from dancing off the table during a 'quake. He was a little skeptical. "Let's see, kinetic energy is 1/2 *m v* squared..." He then argued that although the rope would prevent my equipment from falling off the table, it wouldn't prevent it from wobbling on top of the table, and the energy behind that lateral movement would increase as the square of the velocity induced by the shaking. I told him I wanted to move the computer screen between the printer and computer, thus wedging in the computer, which he did.

He sometimes got anecdotes wrong about famous thinkers, and sometimes mispronounced their names ("Lefnitz" instead of "Leibniz", "Oak-um" instead of "Occam"). He had the automatic skepticism of the self-taught who know that, regardless of how much they know, they are on the bottom in the eyes of professional scientists. For example, he had no doubt that global warming does not exist, and that the scientific establishment systematically prevents any papers that show the truth about this subject, from ever being published. I asked him why the establishment would do this. He replied that the purported existence of the problem gave jobs and prestige to hundreds, if not thousands, of scientists.

On the other hand, he possessed a startling array of incidental cultural knowledge. One day when he was working on my computer, and the classical music station started to play selections from movie scores, he immediately recognized the first one: "Max Steiner!" he said, naming the composer. "*Gone With the Wind*! 1939!" and then he quoted one of Scarlet O'Hara's famous lines — something about tomorrow[1].

I don't know how much reading he actually did. He remarked once, in passing, that it was possible that he suffered from a mild dyslexia. His replies to my emails were always short — a line or two, and if what I had written him about was not urgent, he seldom bothered to reply at all. But he never seemed to mind talking on the phone as long as he wasn't racing to deal with a customer's emergency.

Example of His Extraordinary Ability

Since the computer problems he solved will be less easily understood by the reader, I will give an example of his remarkable mechanical intuition.

One time when I came back from a visit to Gaby, the battery in my car was dead, the car being a 1988 Toyota Camry I bought in 2003 from Jason, who is described above. I had the car towed to Art's Automotive, no relation, probably the best car repair service in the East Bay, and also introduced to me by Steve who, as the reader has no doubt gathered by this time, had an uncanny ability to find top-notch experts. They repaired it, I brought it home, and a few days later, when I went to start the car in the morning, the battery was dead again. I brought it back, again they fixed it, and a week or so later, the battery again died. I casually asked Art if he had any ideas about the problem. He said he was coming down to do some work on Steve's computer and would take a look at it.

He went about his sleuthing task in the intense way I had observed when he worked on my computer, clicking his teeth (which I also do, throughout the day), but nevertheless responding to my questions without annoyance. Among his many gifts was knowing how not to lord it over those whose technical knowledge was incomparably inferior to his. Once, when I told him I had

1. According to Google, possibly "I can't think about that right now. If I do, I'll go crazy. I'll think about that tomorrow," or "After all... tomorrow is another day."

neither the time for, nor the interest in, learning the details about how the software on my computer worked, he merely replied that I was what his trade called an "appliance user". There wasn't the trace of a suggestion in his voice or manner that this was a bad thing to be.

I think he had brought a meter to measure the battery strength. He noticed that when I shut the engine off, the battery immediately started slowly discharging. Yet all the wires in the engine compartment seemed to be connected properly. After apparently running out of ideas, he crawled behind the steering wheel, felt around above the ignition where the key went in, did something, climbed back out, and found that this time the battery wasn't draining.

"Aha!" he said. "What you've got is a broken ..." and he gave the technical name. "All you have to do is, make sure — here, come here...", and he had me slide behind the wheel and feel for a little button above the ignition key. "Before you shut the engine off, hold that button down Then release it after the engine stops." He explained that in these old cars, sometimes the key lock stops shutting off all the power circuits, as it should, but that holding down the button accomplishes the same thing. So in the space of an hour or so he had solved a problem which had baffled the mechanics at the best auto repair shop in the East Bay.

When I asked him where he had picked up his mechanical knowledge, he said, Well, country boys in Tennessee starting messing around with cars at an early age.

He not only knew everything he needed to know about the problems he was called on to fix, he also had an extraordinary amount of historical knowledge about technology. If I brought up the Army surplus receivers I had used in my ham radio days in the fifties, he would give me the history of these receivers. When he was explaining the signal interference that occurred because too many neighbors were on the same frequencies with their wireless routers, he remarked in passing that the Hollywood actress Hedy Lamarr[1] had been co-inventor of a device in World War II which prevented the enemy from intercepting the signals to radio-controlled torpedoes, the device arbitrarily changing the frequency continually on which the control signals were transmitted, she having gotten the idea from thinking about the fact that pressing different keys on a piano represented changes in frequency. When he was fixing the second phone line in the house, which had been wired incorrectly by a phone company technician in the process of an attempt to repair the first phone line, he remarked in passing that the acronym for standard telephone service was "POTS" ("Plain Old Telephone Service") and that the impedance on the lines was 600 ohms, and that the names of the two wires for each line were "tip" and "ring", the latter term referring to a circular part, not to the ringing of the phone; a third wire he said was called the "sleeve".

But he was not always right. He told me once that the word "cop" was an abbreviiation for "constable on patrol", and that "tip" was an abbreviation for "to insure promptness". But he was aware that authorities were not all in agreement on these etymologies.

Analysis of His Ability

More than once, after he had solved yet another problem, I would say something like, "Sheer genius!" He would shrug, say it was nothing but experience. But on one occasion he said that there are four types of intelligence: analytical, deductive, common sense, and artistic He said he was not gifted in any one category but he was able to go from one to the other quickly. (He had not heard of Howard Gardner's seven types of intelligence: linguistic, logical-mathematical, spa-

1. "An amazing woman, who actually was an inventor but better known for running through the woods in the buff in the 1933 film *Ecstasy*. She and composer George Antheil had a patent on this device but apparently never made a dime from it."

tial, bodily-kinesthetic, musical, interpersonal and intrapersonal.) I gradually came to conclude that what put him so far ahead of others in his field were three things: first, a logical mind, which of course others had; second, always having the right tools; and third, an extraordinary memory for technical details.

Regarding the second: he always arrived with a black bag containing his tools, including various electronic measuring devices, and sometimes a few pages downloaded from the Internet. His tools included not merely the expected screwdrivers and wrenches but also tiny versions of the same that he had picked up who-knew-where. With this latter set, he told me, he had been able to remove a circuit board from a computer whose disk drive had failed, resulting in the potential loss of much valuable information for one of his clients. The manufacturer apparently felt that the circuit board should not be removed by ordinary repair technicians, and so had made the screws so small that no ordinary screwdriver could be used on them. But Art happened to have this little set, carefully stored in a red plastic holder, which enabled him to remove the screws. He then removed the same circuit board from the second drive of the computer, replaced the first board with it, and lo and behold, the drive worked sufficiently well that all the information on it could be copied. "It was just an idea," he said. A lesser talent would have told the customer that the only hope was to bring the computer to a drive salvaging shop and pay the hundreds, perhaps over a thousand, dollars to have the disk removed and run on another machine.

When he worked on the second phone line, as described above, he had devices to see if a signal sent from my housemate's room went to the connection box at the front of the house; and a special phone he could attach to wires in the connection box to see if it rang when he dialed the number associated with the wires. Regarding his extraordinary memory for technical details: as I have said, this was by no means limited to technical details of the present, but included the history of numerous products. I am convinced that at least some of his intuition came from a deep, abstract understanding of how things work, by which I do not mean what is normally found in manuals and textbooks, but instead the subconscious design habits of engineers: "I bet they designed it this way..."

He collected old machines, electronic and mechanical. When I told him how sad I was that I would soon have to scrap my 1979 Toyota Celica (my car prior to the 1988 Toyota mentioned above), because I couldn't stand the thought of this fine piece of machinery, this loyal servant of two decades, being crushed into a cube and tossed into a vat of molten scrap, he offered to take it if I didn't charge him anything. I agreed on condition that, when he no longer wanted it, he would give me right of first refusal on taking it back. He fixed it up and drove it for a year, but then said that the cost of keeping it going any longer would have been more than $1000, even for him, and so I took it back. As this is written (April 2007) the car is parked in front of my garage. I am no longer able to start it, but every once in a while, as I pass it on the way to the garage, I place my hand on it as a token of affection so it will know that, even in its old age, it is still loved.

Political Beliefs

As I mentioned in the first chapter of Vol. 4 ("A Mutt and Jeff Duo"), Art and Steve became good friends, even though they were on opposite ends of the political spectrum. When I pointed this out to Art, he said that the main reason was that he and Steve both wanted a just society. Art's second wife , a professor of psychology at a nearby state university, was, like him, a libertarian-anarchist, and I think had written at least one book on the history of the movement. In discussions with Art, he always emphasized the evils of government, and repeated the libertarian-anarchist criterion for just laws: "Unless you would be willing to take a gun and threaten to kill anyone who

disobeyed the law, the law shouldn't exist." Thus, it was permissible to kill a burglar or someone on the street who was threatening to kill you, and therefore laws against burglary and murder were legitimate. But if you weren't willing to threaten to kill someone who didn't pay their taxes, or who refused to serve in the armed forces, or who drove too fast, or who committed embezzlement, there should be no law prohibiting these things. I told him (during the swine flu outbreak in April, 2009) that I personally was glad that some of my taxes supported the Health Dept. If a large number of people with the same symptoms began showing up at hospitals, and if tests revealed that the cause was salmonella, I wanted there to be an agency that attempted to find out the source of the salmonella, and then to shut it down and make sure all products that were likely to be infected were withdrawn from the market. He said this was perfectly reasonable, but that Health Dept.s could be supported by private contributions because people would recognize the importance of such an agency. I replied that you couldn't run an efficient Health Dept. if funding was unpredictable. He said that we often hear stories of government meat inspectors accepting bribes, but never of rabbis who inspect kosher meat taking bribes. Thus private citizens with a personal interest in preventing bad meat from reaching the market, would be much better meat inspectors than the government.

I always concluded by saying that my mind could be changed if the libertarian-anacharists could point to a case where their philosophy worked in practice. I repeated what I had said earlier, namely, that I was enthusiastic about the idea that had been circulating several years before, of the libertarians taking over the state of New Hampshire. He said that a town or city or state could not serve as a convincing example, and cited the case of Amsterdam, in which liberal drug laws have resulted in drug dealers from throughout Europe descending on the city. Only an entire nation could serve as an example, and this could only happen when the people gradually began to realize that anarchist-libertarianism was the best foundation on which to construct a society.

Success of His Business

I don't know what his annual income was. He was charging $125 an hour in 2006-2007, and always billing for less time than he actually spent, I suppose in part because he felt a little guilty about the time we spent talking about the history of technology, and about philosophy and political theories. He and his wife lived in a rented two-bedroom, two-floor condo in Martinez, a nondescript patch of flat suburbs about 45 min. from Berkeley, always hot in the summertime. Incredibly, he had had clients who were completely indifferent to his remarkable ability. He told me he had been fired by a firm of lawyers in Oakland because he routinely showed up not wearing a tie (and he was not about to start wearing one for people he could not respect). He had been fired by another company because he did not have Microsoft Certification. He said he had never seen the need to spend the several hundred dollars and months of study required to get it. Furthermore, he said, "Some of the people I've known who had it were real idiots who didn't know their butt from a reboot key."