Besides a view of the world that supports the SSE research program and methods, *The Black Swan* may be of secondary interest to SSE members for its skewering of the academic establishment. What he offers here, serves both to amuse and to provoke thought.

As for amusement, that is partly a matter of taste. I, for one, smiled broadly at Taleb's discussion of "peer cruelty," from which I extract a characteristic sentence:

> If you are a researcher, you will have to publish inconsequential articles in "prestigious" publications so that others say hello to you once in a while when you run into them at conferences (p. 87).

Taleb provokes thought through comments that are skeptical of the incentives, traditions and institutions of academia. He believes that the best science and philosophy are done by those, like himself, who understand the practical significance of their research. In contrast, he believes that many academics, like the early medical doctors, are pompous frauds, whose theories have no record of practical success. (In modern times, he especially goes after economists, whose failures to predict, result in excuses, but not in major revision of theories, or greater modesty in making further predictions.)

In the end, I fear that I have not done justice to a book that I believe is full of important, and well-defended, insights. I thoroughly enjoyed the nonlinear style of the book; and the style, arguments and evidence, produced a substantial cumulative case. But the lack of linearity, makes that case very hard to adequately summarize; or, at least, such is my excuse.

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J. Review by Ordman

Thinking about Gödel and Turing: Essays on Complexity, 1970–2007, by Gregory J. Chaitin. World Scientific, 2007 (hardcover). $98.00 ISBN 978-981-270-895-3; (paper) $48.00 ISBN 978-981-270-896-0.

On the occasion of Gregory Chaitin's 60th birthday, World Scientific has published a two volume festschrift. The book reviewed here is a collection of survey papers and lectures by Chaitin, showing how his thinking progressed over a period of 37 years. The other volume, writing mainly by others about Chaitin's work, is "Randomness and Complexity, from Leibniz to Chaitin", edited by Cristian S. Calude.

As a collection of papers and talks, the book is highly repetitive; Chaitin feels strongly about the importance of a few of his central results, and thus explains them over and over. But the book builds, things get clearer and new ideas enter in as the papers progress from 1970 to 2007, and the repetition does help the reader understand things better than a much shorter exposition might do.

Chaitin works at the IBM TJ Watson Research Center, for which I have the highest respect. He has had books published by such respected publishers as Springer (4 books!) and Cambridge University Press, and articles in, e.g., Scientific American. Could such a person claim not to be taken seriously, not to be listened to? But he writes in the year 2000, (p. 149–150) "But I must say that philosophers have not picked up the ball. I think logicians hate my work, they detest it! And I'm like pornography, I'm sort of an unmentionable subject in the world of logic, because my results are so disgusting."

I've been a long-time teacher and researcher in theoretical mathematics and computer science. Many of my best students have gone to work solving large, highly complex, real-world problems. I find Chaitin's work fun, interesting, enlightening, conceptually helpful–and almost useless in the real world. I hope in a limited space I can explain why I hold some of those seeming contradictory views.

In several of his talks and essays, Chaitin refers to the unsolved problem of whether P = NP. He cites it as an example of a problem that might be formally unsolvable, a candidate for being a new axiom. Bear with me a moment while I tell what this problem is about, because it is a problem of major practical importance. There are a great many problems that a computer can solve in "reasonable" time. Sorting n items on a computer can be done in time proportional to $n \, log(n)$, although some of the naive algorithms take time proportional to n^2. Harder problems take longer. If a problem can be solved in *Polynomial time*, that is, in time proportional to some fixed power of the problem size n, we say the problem is in the class P.

There turn out to be significant and practical problems that no one has found a way to solve in polynomial time. A simple example is: given a large graph (a network, with some points connected by edges) find a maximal independent set of points (a largest set of points no two of which are connected by an edge). A more practical sounding one: Given a set of cities, find the shortest set of roads that connects all of them. These problems typically have the property that if you could guess very well, you could guess the answer very quickly. Many also have the property that if you somehow knew the answer, you could convince someone it is right in polynomial time. Checking an answer is often far easier than finding it: think of finding a prime factor of a very large number. Finding the factor may take many, many, trials; checking it is a simple matter of division.

Loosely, a problem is in the class NP (the initials stand for *Nondeterministic Polynomial*) if you could guess and check an answer in polynomial time, if you made a lucky guess. There are a great many problems in this class, and many of them are *polynomially equivalent*: that is, they are similar enough that if you could solve any one of them in polynomial time, you could solve the others too. Surprisingly, a large class of these are "at least as hard as any other problem in NP," in the sense that if you could solve any one of them in polynomial time, you could also solve the others in polynomial time. No one has ever found a polynomial time solution for any of these so-called *NP-Complete problems*, and no one has shown that there is no polynomial time solution. So, since the problems are important, people spend a lot of time seeking fast approximate solutions, or seeking solutions that are fast enough given the size of the actual problems their organization must solve. A major basic reference on NP-completeness is the monograph by Garey and Johnson [1].

The subject we have just been talking about is called *computational complexity*. Chaitin's main thrust, *algorithmic information theory*, is a little different. He is concerned not with how fast a computer program is, but with how large it is–that is, the length of the computer program, in bits or bytes. If we care about speed, shouldn't we care also about size? I'll return to that question later.

Why is Chaitin such fun to read? He asks some conceptually interesting questions, and phrases them to give unexpected and interesting answers. Some of his answers are short and simple enough that I can't resist repeating them. I can compare the joy I felt in seeing some of his ideas to the experience I had at age 11 or 12 reading George Gamow's introduction to mathematical ideas, *One Two Three ... Infinity* [2] (as Chaitin would say, anything that has stayed in print for over 50 years deserves mention occasionally; anyone who has read this far and hasn't read Gamow's book, ought to.)

For example, you may well have encountered "random number generators" as a class of computer programs. Depending on your use of random numbers, you may need a bigger and more complex random number generator, one that passes "more tests" for randomness. Why is making a random number generator hard? Why can't you get a perfect one? One possible explanation is an insight that Chaitin reports he had at the age of fifteen.

Consider as an example of a random sequence a very long (e.g., ten thousand digit) binary string. We would consider "010101... .01" (the repetition of "01" many times) not random. By contrast, "010010111011001010 ... 0" might be random. Chaitin gives a definition. Call such a string "random" if you cannot describe it much more briefly than by stating it in full. Or more precisely: if a computer program to generate the string is almost as long as the string. Then almost all numbers are random, for the simple reason that the number of short descriptions (or of short computer programs) is much less than the number of long strings.

On the other hand, you can never prove that any particular long binary string is random! The proof is only slightly more technical. Loosely, "The first string over 10000 digits that you can prove is random" would be a short description of that string, so it wouldn't be random. A more formal proof involves defining

what you mean by a "proof" (a string of symbols following certain rules, that can be checked by a specified procedure.) You can write a computer program, of fixed length, that simply examines all possible proofs (shortest ones first) until it finds a proof (necessarily very long) that some very long string is random. If there were such a proof, the program could stop and print the string in question. But then the program constitutes a description (of fixed length) of that string. As long as we set the target string length much longer than the program, we'd have a contradiction if the program ever found a proof. So we know that it never does.

Chaitin explains even more clearly Turing's proof of Gödel's Incompleteness Theorem. It uses the fact that there is no general method for determining whether a given computer program will ever stop (as opposed to running forever). Suppose we had a program G that can test any program and say whether it terminates. Then we could in principle list in order of length all possible computer programs that take an integer as input and either never stop, or stop and produce one integer as output. Define F as the function that such that $F(N) = 3$ if program number N run with the integer N as input never stops (F can determine this by running G), and $F(N) = K+1$ if program N given input N stops with output K. If we have a G that works, it is easy to program F. So a program for F must be in the list. But it cannot be in the list–it differs in output from program number N for every N.

Now, if we are given a finite axiom system and symbolic rules of inference, we can write a computer program which produces in order, and checks, all possible proofs. If Program P stops and can be proved to stop, our program will find that proof (and thus determine that P stops.) But we've already seen that we can't systematically check whether each program stops. So there must be some program for which there is no proof whether it stops or not. Hence there is a true statement "Program P does not stop" which cannot be proved using our axiom system. This is Gödel's *Incompleteness Theorem:* For any strong enough axiom system, there are true statements that cannot be proved.

Chaitin goes on–using slightly more technical strategies, he defines a number omega, Ω, which expresses the probability that a computer program of a given length will terminate. He proves (not in this book) that it is supremely random–and also supremely noncomputable. In fact he feels it is useful as a measure of how strong a set of axioms is–the more and stronger axioms you have, the more digits of Chaitin's Ω you can compute.

Ok, in what sense do I find this fun, useful, instructive, and yet of no practical value? Why are Chaitin's ideas not as well followed up by others as he might like? Four points:

One: While strict constructivism isn't much followed by many working mathematicians, those working in the real world do want a certain amount of constructibility. Would an axiom about whether P = NP help? In practice, working computer scientists act as if P is not equal to NP–that is, as if problems known to be NP-complete cannot be solved quickly, and we should look for good approximate solutions. Making an axiom "P = NP" would not give us any quicker way of solving those problems. If making a new axiom doesn't give us a handle on a practical problem, why bother?

Two: Yes, pure mathematicians are interested in the size of axiom systems. Papers of the form "in axiom system A1, A2, A3, A4, A2 can be derived from the others", are sought after, enjoyed, and valued. One was in a recent issue of the widely-circulated *American Mathematical Monthly* [3]. Do such results have much practical importance? Not usually–they are simply fun and increase our understanding. So are concrete proofs that axiom A2 cannot be derived from the others. Are these often cited in other papers? In most cases, no. Obviously, there are critical exceptions: non-Euclidean geometry, assuming something other than the usual parallel axiom, has very practical applications in the real world. Similarly, a recent issue of the *Notices of the American Mathematical Society* [4] has an article showing how to construct undecidable problems in number theory. It helps the reader understand undecidability, but probably isn't very helpful in doing number theory. In any event, Chaitin's results on unprovability have not, as yet, found the practical applications that would make them widely valued.

Three: Does solving a problem with a small computer program have value? Again, it is often a great deal of fun. I have enjoyed assigning such things as programming exercises. Such exercises are a big help to students in understanding numerous techniques in practical programming (recursion, self-reference, time of evaluation of functions and variables). But it has never caught on as a "mainstream" area of computer science. Why? It has very little practical significance. Given a (typically large) real-world problem, there are two actual main constraints in the real world: how much time does it takes to solve, and how much space in memory or on disk does it take while being solved? With some rather technical exceptions, taking huge amounts of working space also takes huge amounts of time; so computation time is the subject on which most research concentrates. Finding a computationally fast solution to a large practical problem is of great value, so polynomial time solutions with low exponent are valued. Showing that a problem is NP-complete (and hence, presumably, not in P) shows that we should look for approximate solutions or other ways to attack the practical problem. Making the program source code a great deal smaller (as Chaitin seeks to do) is of no practical use, especially if it dramatically increases the time and space needed to run the program.

Four: Knowing that there are a great many more theoretically unsolvable problems out there, and describing a class of them, is certainly of philosophical interest, but doesn't much affect day-to-day work. Chaitin believes that it is a further justification for experimental mathematics–for example, extensive computer calculations looking for examples in number-theoretical questions that we cannot at present solve. That may be, but the people doing those calculations typically justify them by the hope of finding an example that will disprove the conjecture, or the hope that knowing more cases will lead to an insight that will prove it. Even if Chaitin's work led them to feel it was more likely that the problem is unsolvable–that the result they seek is unprovable with our presently accepted axioms–it doesn't as yet give them tools to prove that. If Chaitin's methods are used to demonstrate that some long-standing problem is in fact unsolvable with the standard axioms, his work will get much broader attention than it does now.

In the latter part of the book, Chaitin speculates about the implications of his thoughts for biology and physics. Paul Davies, in his introduction, goes even further. Modern physics is characterized in part by the very high computational demands of some current theories. If one regards the universe as a large computer, one may believe that only a finite time and finite number of states have been available since the Big Bang to carry out computations. Can a physical law be so demanding that the required computation cannot have been carried out? Of course, applying Chaitin's ideas to this presumes that the universe is a digital, not an analog, computer–that space and time are made of small discrete quanta. Even so, we are a far cry from being able to see whether the properties of the real world required to produce the facts we care about (the "axioms", as it were) are large or small in size and complexity. (The smaller they are, the fewer and simpler the basic principles are, the less "random" the universe is.) We are even farther from being able to understand how much computer power is needed by the universe to carry out the computations. Speculation of this sort may be fun, but it is far too early in the process to regard it as part of science.

There is another sense, however, in which speculative scientists may draw support from Gödel's incompleteness theorem, and Chaitin's exposition of it. There is an image of the scientific method that is well summarized in a recent essay by Paul Grobstein in *Soundings* [5], p.10: "[I believed as a student] that there actually was a well-defined and unique set of properties and rules, the discovery of which would eventually make the mysterious and not yet understood more predictable and ultimately completely so." He goes on to explain why he became skeptical of this program as his career progressed. The discussion here suggests very strongly that *the behavior of physical systems cannot all be deduced from any finite set of rules.* For a computer with a program can, after all, be reasonably realized as a physical system: and if mathematical logic cannot tell us if the program terminates or not, we can't tell if the computer will ever stop running–unless it perhaps fails due to lack of power, mechanical breakdown, or perhaps the end of the universe. So at the scale of mechanical or electronic computing devices, our axioms systems can never be complete. Even at finer scales–so long as the system allows for a potentially infinite sequence of units of time–we cannot expect all problems to be solvable, and we can have mathematical certainty that any "complete theory" will leave some questions unanswered.

Those interested in more will find a great deal of material on Chaitin's website [6]. The table of contents and the first chapter of the book under review are on the World Scientific website [7].

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Voodoo Science: The Road from Foolishness to Fraud by Robert L. Park. Oxford University Press, 2001. 230 pp. $17.95 (paper). ISBN-10: 0195147103.

Dr. Robert Park is recognized by many people as an advocate for science and for the integrity of science. He does not like to see it abused and misrepresented by unethical opportunists, and he tries to serve the public good by alerting the public to what he believes is false science. Park is also well-known for using wit, sarcasm and name-calling to make his points.

This review looks only at the parts of Park's book that covers cold fusion. In many sections of this book, Park expresses himself with derision and invective, likening himself to a hostile juvenile in a schoolyard.

The hypothesis of fusion, as the underlying process to the research ascribed to as "cold fusion," is suspect and unproved; it is now and has been from the day Martin Fleischmann and Stanley Pons made their pronouncement in a press conference in 1989 at the University of Utah. On the other hand, proof of nuclear reactions exists and has existed in this field for many years, long before Park published this book in 2001. This review will pick out important sections of his book that mention "cold fusion" and will provide related comments.

On pp. 13–14 Park writes, "Each year at the cold fusion conference there is great excitement over new results that are said at last to show incontrovertible proof that fusion is taking place at low temperatures. Perhaps it's new evidence of neutrons or gamma rays characteristic of deuterium fusion; or helium . . . But by the time of the next meeting, many of these papers will have been discredited or withdrawn."

It is clear that when Park writes this, he is expressing his disbelief. That fact is that each year, there have been new, exciting results. Neutrons, helium and energy gains have been reported – rigorously. As far as his assertion that many of these papers "will have been discredited or withdrawn," Park does not provide any reference for this statement and it is inconsistent with the facts known to this author. Park comments at the end of his book about scientists' responsibility to society. Journalists or people who write about science also have a responsibility to society. Park's comment about these papers indicates a failure on his part to report accurately.

On p. 14 he writes: "Cold fusion is no closer to being proven than it was the day it was announced." If Park is referring to a fusion mechanism, he is correct, that theory is not proven. If he is referring to a novel nuclear reaction, he is incorrect; evidence for nuclear reactions is plentiful and was so at the time he wrote his book.

On p. 14 he writes, "These are scientists; they are presumably trained to view new claims with skepticism. What keeps them coming back each year with hope in their breasts? Why does this little band so fervently believe in something the rest of the scientific community rejected as fantasy years earlier?" And on p. 27 he speculates that they "found in cold fusion relief from boredom."

The questions Park asks are good questions. Unfortunately, he unilaterally divines the answer and assesses the researchers as merely foolish and given to fantasy. This author asked the same question but asked the researchers directly. In general, they responded that they persisted because they saw a positive result in their experiment(s), they checked their instrumentation carefully and they found no source of error. They trusted their methods and instruments, despite their own or others' preconceived notions.

On p. 18 Park makes a snide remark about his presumption that Fleischmann and Pons were ignorant about the fundamentals of their work. "How," Park wondered, "could Pons and Fleischmann have been working on their cold fusion idea for five years, as they claimed, without going to the library to find out what was already known about hydrogen in metals?"

Fleischmann began his long investigation in Pd/D effects when he was 20, in 1947, reading, among other works, that of Percy Bridgman, a Harvard professor of physics and a Nobel Prize winner. Park's comment couldn't be more wrong. Fleischmann spent his entire life studying hydrogen in metals and was awarded nearly every prize in the field. Not only that, but Fleischmann and Pons were working in a regime beyond that which was known. This is what pioneering science is all about.

On p. 18 Park talks about the inexplicable fact that the expected fusion byproducts should have killed Fleischmann and Pons, and there they were,

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