From: "Raatikainen Panu A K" <Praatikainen@elo.helsinki.fi>
Date: Mon, 19 Mar 2001 11:00:05 +0200

On 16 Mar 01, at 10:01, charles silver wrote:

> *Are you the person who recently published an article debunking Chaitin's celebrated Gödel-like theorem? And, if so, I wondered whether you've received any response from him disputing your interpretation of his result.*

Yes, that's me. And yes, we had a short round of e-mail exchange with Chaitin on my first paper in JPL (Vol 27, No 6, Dec. 1998) on Chaitin's earlier incompleteness result. I found his reply rather unsatisfying, being at odds with the facts of recursive function theory. But there is also a second paper by me in **Synthese** Vol. 123 (2000) on Chaitin's later work on halting probability Ω etc. I have not received any responses to that. But anyway, I think I was just pointing out several logical facts challenging the standard interpretation of that work.

I would be very interested to discuss these issues, if there are people in FOM interested in them.

Panu

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From: "charles silver" <silver_1@mindspring.com>
Date: Mon, 19 Mar 2001 11:03:45 -0800

> *On 16 Mar 01, at 10:01, charles silver wrote:*
>
> > *Are you the person who recently published an article debunking Chaitin's celebrated Gödel-like theorem? And, if so, I wondered whether you've received any response from him disputing your interpretation of his result.*

> *Panu Praatikainen wrote:*
>
> *Yes, that's me. And yes, we had a short round of e-mail exchange with Chaitin on my first paper in JPL (Vol 27, No 6, Dec. 1998) on Chaitin's earlier incompleteness result. I found his reply rather unsatisfying, being at odds with the facts of recursive function theory.*

I'd be interested in FOMers' opinions on two things: (1) whether Chaitin's Theorem actually accomplishes much the same thing as Gödel's (while being different in several respects), and (2) whether anything can be inferred from Chaitin's Theorem about the limitations (or more neutrally put: the capabilities) of the human mind. (In this latter regard, I have to admit that I've never been able to see any connection between Gödel's result and the capabilities of the human mind, though Chaitin's result supposedly provides more information that allows us to jump from the mathematical result to a conclusion about the human mind [acc. to Rucker, for example].)

Charlie Silver

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From:  "Jeffrey Ketland" <ketland@ketland.fsnet.co.uk>
Date:  Mon, 12 Mar 2001 17:35:21 -0000
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Dear Panu,

> *Silver: Are you the person who recently published an article debunking Chaitin's celebrated Gödel-like theorem? And, if so, I wondered whether you've received any response from him disputing your interpretation of his result.*
>
> *Raatikainen: Yes, that's me [snip].*

As far as I recall, Chaitin defines a notion of program-size complexity for axiom systems, and defines the halting probability Ω and then shows that, roughly:

"It takes an axiom system of at least complexity $N + c$ bits to determine N bits of Ω (c being a fixed constant)."

Is that it? It seems rather neat to me. Chaitin's result seems to strengthen the result that no non-recursive function is numeralwise represented in a consistent r.e. system. Can you explain your argument about Chaitin's idea? I'm interested and maybe other fom people would be interested too.

Best - Jeff

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From: "Raatikainen Panu A K" <Praatikainen@elo.helsinki.fi>
Date: Wed, 21 Mar 2001 12:08:16 +0200

As there are some people here in FOM interested in my critical work on the interpretation of Chaitin's results, I'll try to explain shortly my main points.

There are two somewhat different results by Chaitin:

a) Chaitin 1974 (mentioned by Charlie Silver). For every formal system F, there is a finite constant c such that F cannot prove any true statement of the form $K(n) > c$ (even though there are infinitely many n for which this is true) - here $K(x)$ is the Kolmogorov complexity of x

b) Chaitin 1986 (mentioned by Jeff Ketland) Any formal system F can determine only finitely many digits of the halting probability Ω.

As I have (carefully) formulated them here, above results are just fine. What is problematic are the ambitious (fantastic?) philosophical conclusions one has drawn from them.

One has standardly assumed that the size of the limiting constant c for a theory F (in a) or the number of digits of Ω decided by F (in b) somehow reflects the power, or content, of F. Sometimes it is rather said that it is the size, or the complexity, of F which determines this finite limit.

I show, however, that all this is wrong. Actually, it is determined by a rather accidental coding of computable functions used. In particular, there are codings such that theories with highly different power (say, Q and ZFC) have the same finite limit. Also, the size and complexity of F are quite irrelevant. For any given finite collection of formal systems, however different in all respect, one can always fix a coding such that they all have the same limiting constant - one can even make it 0.

Futher, the interpretation is seriously confused with use and mention (e.g., the complexity of a theorem as a syntactical object (mentioned) vs. the compexity a theorem ascribes to an object (used)).

For every theory, there is indeed a finite limit, but that is all - the value of this finite limit does not reflect any natural or interesting property of F.

All this is argued in detail in my **JPL** paper – I repeat the essential argument for the Ω case in my **Synthese** paper.

Now speaking about Ω, my basic point in the later paper is rather simple. I attack (besides the above mentioned interpretation) the claims that this result is "the ultimate undecidability result", "the strongest possible version of incompleteness theorem" etc. I point out that Ω is actually Δ^0_2, (and thus even recursive in Turing's halting set), so it is all too easy to present undecidability and incompleteless results that are definitively stronger that Chaitin's. I give some natural examples.

I am glad to discuss these issues further here in FOM, but I also recommend my papers for those seriously interested in the issue. My papers are quite self-contained, and have useful introductions (and the later paper is even short). I repeat the references:

Panu Raatikainen: "On interpreting Chaitin's incompleteness theorems", Journal of Philosophical Logic 27 (1998), 569-586.

Panu Raatikainen: "Algorithmic information theory and undecidability", **Synthese** 123 (2000), 217-225.

> *Charlie Silver wrote:*
>
> *It seemed to me, despite your assertion at the end of the article about the theorem still being of value (or something like that), the points you make in the article were devastating. If we take away the standard interpretation of the theorem, what do you think of value is left?*

I am happy to hear that you think that my points were devasting – that was my intention. On what value is left: well, I was just trying to be not too devasting and merciless; anyway, I think that the result (a) shows that there is some difference whether one constructs an incompleteness result in terms of a simple set rather than in terms of a creative set, as the standard Gödelian proof does. But I think that this is the only lasting value that is left.

Panu

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From:  Stephen G Simpson <simpson@math.psu.edu>
Date:  Wed, 21 Mar 2001 17:02:32 -0500
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> *Raatikainen Panu A K, Wed, 21 Mar 2001 12:08:16 +0200:*
>
> *There are two somewhat different results by Chaitin:*
>
> ...
>
> *As I have (carefully) formulated them here, above results are just fine. What is problematic are the ambitious (fantastic?) philosophical conclusions one has drawn from them.*

Yes. Thank you for making these points. Chaitin's results are not without interest, but claims about their philosophical/foundational significance are greatly exaggerated.

Speaking of Chaitin's c, Raatikainen says:

> *there are codings such that theories with highly different power (say,* Q *and* ZFC*) have the same finite limit. Also, the size and complexity of F are quite irrelevant. ...*
>
> *For every theory, there is indeed a finite limit, but that is all - the value of this finite limit does not reflect any natural or interesting property of F.*

Yes.

Now clearly there is a serious foundational/philosophical problem here. A crude attempt at formulating the problem:

(*) For well known foundational theories F (e.g. $F = \mathsf{PA}, \mathsf{Z}_2, \mathsf{ZFC}, \mathsf{ZFC} +$ a large cardinal axiom, etc), find versions of the incompleteness phenomenon, e.g., mathematically natural statements independent of F, which are sensitive to F.

I invite other FOM participants to give a sharper formulation.

Obviously (*) is a key f.o.m. problem – some would say THE key f.o.m. problem. And this problem seems extremely difficult. Gödel's independent statements, $\mathrm{Con}(F)$, do not have the required properties, nor do Chaitin's statements $K(n) > c$. The Paris/Harrington theorem is a well-known major contribution to (*).

Many people underestimate the difficulty of (*) and overestimate Chaitin's contributions to it. For example, the New Scientist article at

`http://www.newscientist.com/features/features.jsp?id=ns22811`

is loaded with overblown hype.

Harvey Friedman's recent Boolean relation theory is a direct assault on (*). In my estimation this work of Harvey is much deeper and better than anything Chaitin has done, and it goes much farther than Paris/Harrington. Details are in Harvey's numbered FOM posting # 100 of today, Wed, 21 Mar 2001 11:29:43 -0500.

On the one hand, it is good that serious f.o.m. issues have attracted so much attention in the popular press. On the other hand, it is unfortunate that these issues are so badly misunderstood.

Steve

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From:  "charles silver" <silver_1@mindspring.com>
Date:  Fri, 23 Mar 2001 08:08:53 -0800
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Steve Simpson wrote:

Chaitin's results are not without interest, but claims about their philosophical/foundational significance are greatly exaggerated.

I would appreciate any detailed comments you might wish to make about what interest Chaitin's Theorem really has and what exaggerations you have found concerning their philosophical/foundational significance. Rudy Rucker, for one, considers it to be of greater philosophical interest than Gödel's Theorem. He says that Chaitin's Theorem gives us more information than Gödel's. (See the section of Mind Tools on "Algorithmic Information", beginning on page 279.) What I find of particular interest (independently of whether Chaitin's Theorem has real content) is Rucker's connecting Chaitin's Theorem to the philosophical notion of "conceivability". According to Rucker (p. 290), "a pattern is 'inconceivable' if it is too complex for me to reproduce in detail." Later, on the same page, he says, "Suppose I think of myself as being a Turing machine about to make marks on a blank tape. My brain has only finitely many components, and each of these components can be set in only finitely many ways...."

It seems to me that certain scientific or maybe pseudo-scientific notions fire up the public's imagination and are then translated into modern jargon, which then becomes used to reflect the "general intellectual interest" of the day. It would be useful to assess the scientific value of such notions and then to evaluate their application to diverse fields in the humanities.

Charlie Silver

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`From:  Joe Shipman <shipman@savera.com>`
`Date:  Fri, 23 Mar 2001 11:50:40 -0500`

In defense of Chaitin, I have always found his approach by far the easiest way of establishing incompleteness theorems, and his philosophical insight that the strength of theories is ultimately dependent on their algorithmic information content is important. Chaitin's result that the halting probability of a computation-universal system is maximally compressed information is also useful in various contexts.

On the other hand, Chaitin's more recent remarks about the "randomness" of mathematical truth seem muddled to me. Yes, there are parameterized exponential Diophantine equations (and probably regular diophantine equations too), the existence of solutions to which is an absolutely random function of the parameter in various senses, but it is not surprising that there are mathematical statements which are true for no particular reason. This would only become interesting if the statements themselves were small enough to be interesting.

The relevance of algorithmic information to the incompleteness theorems and to mathematics generally is obscured because the well-explored hierarchy of logical strengths of theories does not seem to correspond to amount of algorithmic information in the theory. Some very strong theories seem to have much simpler axiomatizations than much weaker ones. We need a better way to measure (an upper bound on) the algorithmic complexity of a theory.

The most straightforward way to do this is to start with the predicate calculus as a computational base, and define the complexity of a theory to be the length of the shortest axiomatization, converting non-finitely-axiomatizable theories into finitely axiomatizable conservative extensions ones by introducing new predicates to Skolemize axiom schemes. (Consider GB versus ZF.) But there may well be ways to represent the size of a theory involving other computational bases that are superior for relating "size" of a theory to logical strength.

PA and ZFC both seem to have the property that they are "simpler" (in some inexact sense) than any logically stronger consistent theories. I'd like to see someone either dispute this assertion or suggest other examples of the phenomenon.

Joe Shipman

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From:  "Alexander R. Pruss" <pruss@imap.pitt.edu>
Date:  Fri, 23 Mar 2001 11:52:47 -0500
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> *From: "charles silver" ¡silver_1@mindspring.com¿*
>
> *According to Rucker (p. 290), "a pattern is 'inconceivable' if it is too complex for me to reproduce in detail."*

Sorry to ask something naive, but I haven't been following this discussion or reading Rucker. Is "inconceivable" here an idiosyncratic synonym for "unimaginable"? Surely I can conceive of "the atomic structure of my left big toe" in some real sense of "conceive", even though it would be impossible for me to reproduce it in detail. When I entertain a thought of "the atomic structure of my left big toe", I entertain a thought I know to be coherent, though I can only imagine the content of that thought in general terms. But in any case, what is there that I can really imagine or reproduce in detail? I can't even imagine any given triangle in detail, because no triangle I imagine has determinate side-lengths and I cannot reproduce "in detail" the exact side lengths of any specific triangle.

Alex Pruss

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From:  "charles silver" <silver_1@mindspring.com>
Date:  Fri, 23 Mar 2001 19:14:02 -0800
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> > *From: "charles silver" ¡silver_1@mindspring.com¿*
> >
> > *According to Rucker (p. 290), "a pattern is 'inconceivable' if it is too complex for me to reproduce in detail."*
>
> *Sorry to ask something naive, but I haven't been following this discussion or reading Rucker. Is "inconceivable" here an idiosyncratic synonym for "unimaginable"?*

I don't wish to try to represent Rucker's views or to defend them. I suggest that people who are interested in this read Rucker on "conceivability" to see whether what he says seems interesting or not. My experience reading philosophy papers on the topic of conceivability and related notions (which is not very extensive, I admit) is that they are far far less interesting than Rucker's views and close to being completely vacuous. At least Rucker ties the notion of conceivability to a mathematical theorem. So, on the face of it, there **may** be some content to his views. However, Panu Raatikainen has challenged the standard interpretation of Chaitin's Theorem, so it may well be the case that there's a problem at the very base of Rucker's notion.

If I understand Joe Shipman's most recent post, he thinks Chaitin's notion of algorithmic complexity is worthwhile in enabling us to compare the strengths of theories. If I understand Panu Raatikainen, he thinks that the content of a theory is in no way related to any such measure. I am interested in understanding these issues better and would like to request that both Joe and Panu provide more details of their views (and that they correct anything I have wrongly attributed to them).

Charlie Silver

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From:  "Raatikainen Panu A K" <Praatikainen@elo.helsinki.fi>
Date:  Sat, 24 Mar 2001 18:44:36 +0200
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> *Joe Shipman wrote:*
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> *In defense of Chaitin, I have always found his approach by far the easiest way of establishing incompleteness theorems, and his philosophical insight that the strength of theories is ultimately dependent on their algorithmic information content is important.*

I strongly disagree. The easiest way is to use the notion of truth and show by diagonal argument that provable does not exhaust true. (see, e.g., Smullyan's book on incompleteness). Indeed, a rigorous proof of Chaitin's theorem requires one to arithmetize both Turing machines and the syntax of the theory in question (Gödel's proof requires only the latter) and move back and forth between these two codings (no wonder so many people have got lost).

Further, the claim that "the strength of theories is ultimately dependent on their algorithmic information content is important" is simply false, as Shipman's own comments later show when he admits that "Some very strong theories seem to have much simpler axiomatizations than much weaker ones"

Charlie Silver wrote that Rudy Rucker, for one, considers Chaitin's Theorem to be of greater philosophical interest than Gödel's Theorem. He says that Chaitin's Theorem gives us more information than Gödel's.

In fact, Chaitin's Theorem gives less information, in the sense that it holds only for theories that are, not only consistent (or 1- consistent) as in Gödel's proof, but also sound for the sentences "$K(n) > m$".

> *Joe Shipman also wrote:*
>
> *"We need a better way to measure (an upper bound on) the algorithmic complexity of a theory.*
>
> *The most straightforward way to do this is to start with the predicate calculus as a computational base, and define the complexity of a theory to be the length of the shortest axiomatization, converting non-finitely-axiomatizable theories into finitely axiomatizable conservative extensions ones by introducing new predicates to Skolemize axiom schemes. (Consider* GB *versus* ZF.*) But there may well be ways to represent the size of a theory involving other computational bases that are superior for relating "size" of a theory to logical strength.* PA *and* ZFC *both seem to have the property that they are "simpler" (in some inexact sense) than any logically stronger consistent theories. I'd like to see someone either dispute this assertion or suggest other examples of the phenomenon."*

I agree, except that one should not call it algorithmic complexity any more – it is a different notion. I've had some ideas to this direction, and Harvey Friedman has had too (in think it was a year ago or so when we had some discussion on them here in FOM).

Panu Raatikainen

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From:  Joe Shipman &lt;shipman@savera.com&gt;
Date:  Mon, 26 Mar 2001 10:38:18 -0500
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> > *Shipman:*
> >
> > *In defense of Chaitin, I have always found his approach by far the easiest way of establishing incompleteness theorems, and his philosophical insight that the strength of theories is ultimately dependent on their algorithmic information content is important.*
>
> *Rattikainen*
>
> *I strongly disagree. The easiest way is to use the notion of truth and show by diagonal argument that provable does not exhaust true. (see, e.g., Smullyan's book on incompleteness).*

THIS IS NOT SO EASY, EXCEPT FOR SPECIALLY CHOSEN FORMAL SYSTEMS LIKE SMULLYAN'S WHICH ARE DESIGNED FOR SELF-REFERENCE

> *(Rattikainen):*
>
> *Indeed, a rigorous proof of Chaitin's theorem requires one to arithmetize both Turing machines and the syntax of the theory in question (Gödel's proof requires only the latter) and move back and forth between these two codings (no wonder so many people have got lost).*

WRONG. FIRST OF ALL, ONCE YOU HAVE ARITHMETIZED TURING MACHINES YOU DON'T NEED TO ARITHMETIZE SYNTAX, YOU CAN SIMPLY **PROGRAM** THE SYNTAX. SECONDLY, YOU DON'T EVEN NEED TO ARITHMETIZE TURING MACHINES TO GET INCOMPLETENESS RESULTS, JUST TO GET INCOMPLETENESS FOR PEANO ARITHMETIC WHICH IS MUCH STRONGER.

CHAITIN'S APPROACH VIA THE HALTING PROBLEM MAKES IT VERY EASY TO SHOW THAT ANY SOUND THEORY WHICH CAN FORMALIZE THE STATEMENTS $K(m) > n$ IS INCOMPLETE. ZF OBVIOUSLY SUFFICES, AS DOES $\mathsf{ZF} + \neg(\mathsf{Inf})$ THIS LATTER THEORY IS

ALMOST TRIVIALLY ISOMORPHIC TO PEANO ARITHMETIC AUGMENTED BY EXPONENTIATION. THE ONLY TECHNICALLY DIFFICULT PART IS IF YOU INSIST ON NOT ALLOWING EXPONENTIATION. THEN YOU HAVE SOME HARD CODING TO DO TO PROVE THAT YOU CAN REPRESENT EXPONENTIATION IN TERMS OF $+$ AND $\cdot$, AND THAT WORK IS NECESSARY IN *any* PROOF OF THE INCOMPLETENESS OF PA.

> *(Rattikainen):*
>
> *Further, the claim that "the strength of theories is ultimately dependent on their algorithmic information content is important" is simply false, as Shipman's own comments later show when he admits that "Some very strong theories seem to have much simpler axiomatizations than much weaker ones".*

THAT IS WHY I USED THE WORDS 'ULTIMATELY' AND 'SEEM TO'. IT IS WRONG TO INTERPRET CHAITIN'S THEOREM TO MEAN THAT A STRONGER THEORY IS ALWAYS OF GREATER ALGORITHMIC INFORMATION CONTENT THAN A WEAKER ONE. THE POINT IS THAT THE ALGORITHMIC INFORMATION CONTENT GIVES AN UPPER BOUND ON THE STRENGTH.

Joe Shipman

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From:  Joe Shipman <shipman@savera.com>
Date:  Mon, 26 Mar 2001 11:14:39 -0500
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> > *Rattikainen:*
> > *I strongly disagree. The easiest way is to use the notion of truth and show by diagonal argument that provable does not exhaust true. (see, e.g., Smullyan's book on incompleteness).*
>
> *Shipman:*

THIS IS NOT SO EASY, EXCEPT FOR SPECIALLY CHOSEN FORMAL SYSTEMS LIKE SMULLYAN'S WHICH ARE DESIGNED FOR SELF-REFERENCE

Raatikainen:

I am not sure if I understand this comment. I don't think that PA *etc. are in any way specially chosen or designed for self-reference.*

I was referring to "Smullyan's Easy Language For Self-reference" ("SELF") and his language of Arithmetic SAr which he used to develop incompleteness very smoothly in J. Symb. Logic 22, no.1 (1957), 55-67. Smullyan's development is repeated in Manin's remarkable text "A Course in Mathematical Logic" (Springer GTM # 53). Those languages are carefully contrived to make the proofs of Incompleteness easy, and they are not the standard way of formalizing arithmetic (though they are reasonable considered as formal systems, it's not difficult to use them).

Raatikainen:
Indeed, a rigorous proof of Chaitin's theorem requires one to arithmetize both Turing machines and the syntax of the theory in question (Gödel's proof requires only the latter) and move back and forth between these two codings (no wonder so many people have got lost).

Shipman:

WRONG. FIRST OF ALL, ONCE YOU HAVE ARITHMETIZED TURING MACHINES YOU DON'T NEED TO ARITHMETIZE SYNTAX, YOU CAN SIMPLY **PROGRAM** *THE SYNTAX.*

Raatikainen:

I am sorry to disagree: I submit that it makes no sense to talk about programming the syntax - in the context of Turing machines reading and writing only zeros and ones, which is the case in the algorithmic information theory - independently of a binary coding of the syntax.

I think we differ about the meaning of "arithmetize". I am not requiring the Turing machines to have a binary alphabet, and I am certainly not requiring that they be formalized in a theory of arithmetic with only $+$ and $\cdot$ as operations. ZF, or $\mathsf{ZF} - (\mathsf{Inf})$, can *formalize* Turing machines perfectly straightforwardly, and the Turing-machine-coding needed to make a Universal Turing Machine and get results about the Halting Problem is MUCH easier than "arithmetizing TM's" in the sense of representing them in Peano Arithmetic.

> *Shipman:*
> *SECONDLY, YOU DON'T EVEN NEED TO ARITHMETIZE TURING MACHINES TO GET INCOMPLETENESS RESULTS, JUST TO GET INCOMPLETENESS FOR PEANO ARITHMETIC WHICH IS MUCH STRONGER.*
>
> *Raatikainen:*
>
> *in a sense, yes (if I got your idea right – at least, if one has a theory that is about Turing machines directly). But in order to have interesting and generalized incompleteness results, there is no choice.*

If you admit exponentiation into your arithmetic it is very easy to translate finite set theory into arithmetic using the enumeration of the hereditarily finite sets "$f(t) = \sum_{s \in t} 2^{f(s)}$". So you can get generalized incompleteness results about Peano Arithmetic augmented with exponentiation without difficulty. It is a separate fact that exponentiation can be represented in terms of $+$ and $\cdot$, and there was no a priori reason to suppose that the theory of $+$ and $\cdot$ was general enough to get incompleteness (the theory of $+$ and the theory of $\cdot$ are each decidable but the proofs are hard).

> *Shipman:*
> *CHAITIN'S APPROACH VIA THE HALTING PROBLEM MAKES IT VERY EASY TO SHOW THAT ANY SOUND THEORY WHICH CAN FORMALIZE THE STATEMENTS $K(m) > n$ IS INCOMPLETE.*
>
> *Raatikainen:*

to repeat myself a little: in order to formalize the statements "$K(m) > n$" in an ordinary mathematical theory one has to arithmetize Turing machines first.

No, you just have to *formalize* Turing machines which is easier than arithmetizing them. ZF certainly suffices, as do much weaker theories. The theorems of ZF are obviously enumerable, and someone who has learned enough about the theory of computation to understand that general programs can be executed by a Turing machine can therefore get the incompleteness of ZF quickly, and generalize it to PA+Exponentiation with only a little work.

Raatikainen:

But similarly, if one just assumes that a theory can formalize $\mathrm{Prov}(x)$ *and Diagonalization and is sound, it is extremely easy to prove Gödel's first theorem fo such a theorem (see e.g. pages 827-8 of Smorynski's Handbook survey; the proof takes just 6 lines).*

Yes, the hard part is formalizing $\mathrm{Prov}(x)$ and Diagonalization. I claim that for someone with any computer programming education, it is far easier to get $\mathrm{Prov}(x)$ and Diagonalization by going through Turing machines than by trying to work in PA directly. The only thing you sacrifice is the separate result that exponentiation is representable in terms of $+$ and $\cdot$.

Joe Shipman

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From:  Harvey Friedman <friedman@math.ohio-state.edu>
Date:  Mon, 26 Mar 2001 11:54:48 -0500
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Reply to Raatikainen 3/21/01 12:08PM:

The main issue that Chaitin's work does not address is this:

is there any reason to consider extending the usual axioms for mathematics (ZFC)?

Chaitin's work says, in various interesting ways, that there are certain things that would be nice to do that cannot be done in any reasonable formal system whatsoever.

But this cannot be a reason to extend the current axioms, because the problem will arise in the same way for any extension.

Also, Chaitin does not give us a specific task that we cannot accomplish in our present formal systems, or any reasonable formal system. In fact, Chaitin does give us an infinite list of tasks that we cannot accomplish all within any formal system. However, this is not all that surprising since formal systems are finitary. His example is "determine each digit of the halting probability Ω". However, this formulation of his result is presumably easier than his full result, as it depends only on the nonrecursivity of Ω.

Chaitin does give us an infinite list of tasks, and shows that we cannot accomplish infinitely many of these tasks within ZFC, or even within any reasonable formal system. Again, the tasks are determining the digits of the halting probability Ω.

Of course, the original Gödel work does give us a specific task that we cannot accomplish in our present formal systems, and which, historically, was sought after and believed to be obtainable, and has the highest significance – consistency.

It is true that the interesting tasks that Chaitin tells us we cannot hope to accomplish in various senses are of this character:

they are not at all like the kinds of things that mathematicians try to do, or even conceive of trying to do; in fact, it is intrinsically different than those kinds of things

This is partly because of the nonrobustness of the underlying notions used. The mathematical interest of detailed quantitative information about structures generally depend on their robustness.

This is presumably why Chaitin sometimes works with exponential Diophantine equations. But these are still rather disgusting mathematically. One cannot hope to use ordinary Diophantine equations because of the lack of knowledge about them, so that one does not get presentable quantitative information.

> *a) Chaitin 1974 (mentioned by Charlie Silver). For every formal system F, there is a finite constant c such that F cannot prove any true statement of the form $K(n) > c$ (even thought there*

> *are infinitely many n for which this is true) - here $K(x)$ is the Kolmogorov complexity of x*

I think that more is true:

For every consistent reasonable formal system F, there is a finite constant c such that F cannot prove any sentence of the form $K(n) > c$.

Under a standard presentation of K, roughly how big does c need to be for ZFC? And what effect is there if we use PA (Peano Arithmetic) instead of ZFC?

> *b) Chaitin 1986 (mentioned by Jeff Ketland) Any formal system F can determine only finitely many digits of the halting probability Ω.*

Again, under a standard presentation of Ω (i.e., a standard setup for Turing machines), roughly how many digits can be determined in ZFC? And what effect is there if we use PA (Peano Arithmetic) instead of ZFC?

> *One has standardly assumed that the size of the limiting constant c for a theory F (in a) or the number of digits of Ω decided by F (in b) somehow reflects the power, or content, of F. Sometimes it is rather said that it is the size, or the complexity, of F which determines this finite limit.*
>
> *I show, however, that all this is wrong. Actually, it is determined by a rather accidental coding of computable functions used. In particular, there are codings such that theories with highly different power (say, Q and ZFC) have the same finite limit. Also, the size and complexity of F are quite irrelevant. For any given finite collection of formal systems, however different in all respect, one can always fix a coding such that they all have the same limiting constant - one can even make it 0.*

But what if we fix the presentation of Turing machines to be reasonably natural, in advance, and then change the theories?

As a simplified example, suppose we are interested in the size of the smallest Turing machine TM which does not halt but cannot be proved to not halt in PA or ZFC. How do these sizes compare?

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From:  "Raatikainen Panu A K" &lt;Praatikainen@elo.helsinki.fi&gt;
Date:  Tue, 27 Mar 2001 11:17:27 +0200
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First, let me note that it seems there has been some misunderstandings between me and Shipman which became apparent in his last posting – it is obvious that on various issues, we've been talking on different issues.

But still, I have some comments

> *On 26 Mar 01, at 11:14, Joe Shipman wrote:*
>
> > *Raatikainen:*
> > *I am sorry to disagree: I submit that it makes no sense to talk about programming the syntax – in the context of Turing machines reading and writing only zeros and ones, which is the case in the algorithmic information theory – independently of a binary coding of the syntax.*
>
> *I think we differ about the meaning of "arithmetize". I am not requiring the Turing machines to have a binary alphabet, and I am certainly not requiring that they be formalized in a theory of arithmetic with only + and · as operations.* ZF, *or* ZF − (Inf)*, can *formalize* Turing machines perfectly straightforwardly, and the Turing-machine-coding needed to make a Universal Turing Machine and get results about the Halting Problem is MUCH easier than "arithmetizing TM's" in the sense of representing them in Peano Arithmetic.*

Yes, I took it for granted that we focus on Turing machines with binary alphabet. But it seems to me that this is needed for various key definitions and results in Algorithmic Information Theory. A possible more genralized approach would be interesting but is non-standard.

I must admit that I don't recall that I have ever seen the theory of Turing machines developed in Set Theory (do you have any good references?) - it is interesting to hear that it makes UTM etc. much easier (by the way, there is an unpublished textbook manuscript by M. Fitting where he developes the

Gödelian approach in Set Theory – also it turns out to be somewhat simpler in that context ...)

But I still wonder whether one need not to somehow code Turing Machines to sets before one can formalize them in Set Theory ?

One final clarification: the approach to the incompleteness results via Turing Machines has certain appeal – I do not intend to deny this. But wouldn't it then be better to credit Turing (rather than Chaitin) - for the proof of the first incompleteness theorem by using the Halting Problem was given already by Turing himself in 1936...

All the best

Panu Raatikainen

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`From: "Raatikainen Panu A K" <Praatikainen@elo.helsinki.fi>`
`Date: Tue, 27 Mar 2001 12:58:08 +0200`

I would like to thank Harvey Friedman for his thoughtful comments on our issue. There is really nothing I disagree with him. I'll only try to comment the specific questions Harvey raised

Panu Raatikainen

> *On 26 Mar 01, at 11:54, Harvey Friedman wrote:*
>
> > *a) Chaitin 1974 (mentioned by Charlie Silver). For every formal system F, there is a finite constant c such that F cannot prove any true statement of the form $K(n) > c$ (even thought there are infinitely many n for which this is true) – here $K(x)$ is the Kolmogorov complexity of x*
>
> *I think that more is true:*
>
> *For every consistent reasonable formal system F, there is a finite constant c such that F cannot prove any sentence of the form $K(n) > c$.*

I think that both your formulation and mine are slightly inexact, namely, the proof of Chaitin's result actually requires that F proves a sentence of the form "$K(n) > m$" only it is true – that is, we need some amount of soundness and not just consistency (my wording was even worse here) - or perhaps you meant just this by "reasonable" ...

> *Under a standard presentation of K, roughly how big does c need to be for* ZFC*? And what effect is there if we use* PA *(Peano Arithmetic) instead of* ZFC*?*
>
> *Again, under a standard presentation of Ω (i.e., a standard setup for Turing machines), roughly how many digits can be determined in* ZFC*? And what effect is there if we use* PA *(Peano Arithmetic) instead of* ZFC*?*
>
> *But what if we fix the presentation of Turing machines to be reasonably natural, in advance, and then change the theories?*
>
> *As a simplified example, suppose we are interested in the size of the smallest Turing machine TM which does not halt but cannot be proved to not halt in* PA *or* ZFC*. How do these sizes compare?*

The problem here is that in general, we cannot compute these values (for a particular theory, with a particular coding of TMs and a particular Gödel numbering of its syntax, it may turn out to be possible to determine it) – actually Chaitin's methods only provide relatively loose upper bounds for them, contrary to what the standard interpretation seems to suggest. Indeed, if there were any kind of effective correspondence between F and the minimal c (etc.) for F, one could decide the Halting Problem.

Compare: G2 provides an effective upper bound for the length of the shortest unprovable Π^0_1 sentence in a given theory F, i.e., Length(Cons(F)) – but it gives absolutely no information about the simplest such unprovable sentence. And again, if there were a general method for finding the minimal unprovable Π^0_1 sentence of a theory, one could decide the undecidable.

And as I have pointed out, there are acceptable codings in which these finite limits are the same for, say, PA and ZFC (or, for Q and ZFC + MC, or whatever) – and we simply do not know what happens with various "natural" or "standard" codings – as far as we know, they may still be the same, at least for some of them. Or maybe not.

Anyway, I guess –if it interests anybody– that for a standard coding technique, these values would turn out to be very large.

Harvey's "simplified example" (the size of the smallest Turing machine TM which does not halt but cannot be proved to not halt in F) is actually not at all simple: it is it which in fact determines the value of these limiting constants (well, in the first case: the smallest TM which does not halt, cannot be proved to not halt in F, and further: it cannot be proved in F what would be the output of TM if it halted. NOTE: this is a small correction to my **JPL** paper - thanks to Daniel Leivant for pointing out the gap)). But the reply to Harvey's question is: we do not know whether there is a difference, or whether it is the same TM.

But in any case, my arguments do show that in general, there is no correspondence between these finite limits and the proof- theoretical strengths of theories. And my own view is that there are quite many rather different ways of coding Turing machines and syntaxes all which could with equal right be called "a standard coding", so that sticking to one and concluding something about the resulting values of the limiting constants seems to me quite speculative.

I think that the most natural question of this sort with a real foundational interest still is (for some natural F):

What is the shortest true sentence (perhaps: Π^0_1 sentence) unprovable in F ? How large it is ?

Best

Panu Raatikainen

$*$

`From:  "Don Fallis" <fallis@email.arizona.edu>`
`Date:  Tue, 27 Mar 2001 08:34:13 -0700`

Hi,

Setting aside the difficulties of coding things, I also think that Chaitin's "$K(m) > c$" result is very cool and a very intuitive way of proving incompleteness. However, I have never been able to fathom the claims that the

"$K(m) > c$" result (or the Ω number) have serious implications for mathematical practice.

It has become popular in recent years to argue that mathematicians should make significant changes in the way that they do business (e.g., by accepting a bunch of new axioms or, more radically, by using non-deductive methods of proof.). First, it has been argued (on philosophical grounds) that they will suffer no epistemic loss by making these changes. And, second, it has been argued that there is a significant mathematical gain to be had by making these changes.

Many discussions of Chaitin's work seem to fall under this second category. However, as far as I can see, the only really compelling arguments (in this category) do the following: They show that there is something that *we would want to be able to prove* (or disprove), but that is impossible (or computationally infeasible) to prove given our existing techniques. (For example, a proof of the undecidability of CH is an argument of this sort.) It is not clear that Chaitin has done anything like this.

take care, don

P.S. Here are some other articles that claim that the implications of Chaitin's result are somewhat overstated:

Michiel van Lambalgen, ALGORITHMIC INFORMATION THEORY, Journal of Symbolic Logic. 1989; 54,1389-1400.

Don Fallis, The Source of Chaitin's Incorrectness, Philosophia-Mathematica. 1996; 4(3), 261-269.

P.P.S. I believe that Boolos once gave a talk where he claimed that Chaitin's incompleteness result was a riff on the Berry paradox (i.e., "the least integer that cannot be named in fewer than thirteen words") in the same way that Gödel's incompleteness result is a riff on Russell's paradox. Does this sound familiar? Was it ever published?

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From:  Richard Heck <heck@fas.harvard.edu>
Date:  Tue, 27 Mar 2001 13:03:59 -0500
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P.P.S. I believe that Boolos once gave a talk where he claimed that Chaitin's incompleteness result was a riff on the Berry paradox (i.e., "the least integer that cannot be named in fewer than thirteen words") in the same way that Gödel's incompleteness result is a riff on Russell's paradox. Does this sound familiar? Was it ever published?

See "A New Proof of the Gödel Incompleteness Theorem", in Logic, Logic, and Logic, pp. 383-8, at p.386. Boolos attributes the comparison to Chaitin himself in "Computational Complexity and Gödel's Incompleteness Theorem", AMS Notices17 (1970), p.672.

Richard

$*$

`From:  Alasdair Urquhart <urquhart@cs.toronto.edu>`
`Date:  Tue, 27 Mar 2001 16:16:17 -0500`

FOM subscribers who have been following the Chaitin thread might be interested in the very thoughtful review of Chaitin's work by Peter Gacs in the JSL, Vol. 54 (1989), p. 624. In particular, Gacs argues, based on an information conservation theorem of Leonid Levin, that Chaitin's results do not support the idea of empirical mathematics.

Alasdair Urquhart