MATHEMATICS IS DRAMATICALLY INCOMPLETE*

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ABSTRACT

We state and comment our recent results on the incompleteness of elementary real analysis and their relevance for the axiomatized sciences.

K. Gödel of Vienna Seems to have proved that a specific contradiction could be deduced from any proof of the impossibility of the occurrence of contradictions in mathematics. It seems, in fact, that systems like pure mathematics cannot be completely symbolized (...) This is a very important result, if correct (...)

Max Black, *The Nature of Mathematics* (1933).

§1

Chaos theory has been a fast-growing research area since the early 70's, a decade after the discovery of (an apparently) chaotic behaviour in a deterministic nonlinear dynamical system by E. Lorenz (for references see [3]). Chaos scientists usually proceed in one of two ways: whenever they wish to know if a given physical process is chaotic the usual starting point is to write down the equations that describe the process and out of them to check whether the process satisfies some of the established mathematical criteria for chaos and randomness.

However those equations are in most cases intractable nonlinear differential equations; moreover, they in general have no analytical solutions. Therefore, chaos theorists turn to computer simulations. Usually a Mac or a PC will do the trick: the simulation is easily done and for most nonlinear systems one sees a confusing, tangled pattern of trajectories on the screen. The system looks random, chaotic. Better: there are statistical tests such as the Grassberger-Proccacia criterion that guarantee the existence of randomness in a computer-

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simulated system given certain presuppositions and within a margin of error. Yet statistical tests furnish no *mathematical* proof of the existence of chaos in a dynamical system. There is always the chance that the system is undergoing a very long and complicated transient state, before it settles down to a nice and regular behaviour. Therefore how can we prove that a dynamical system that *looks* chaotic is, in fact, chaotic?

This problem has been around for some time since the discovery and early exploration of what is now called «deterministic chaos». Researchers in the area either try to explore (as we have just explained) through computer simulations well-known systems that can be mathematically described, to see if they have the looks of a random system, or try to develop finely-tuned formal criteria for chaos that can be mathematically checked in a system, not just inferred out of a disordered appearance of the system's trajectories.

In a 1983 conference (published in 1985) Morris Hirsch stated that time was ripe for a marriage between the «experimental» and «theoretical» sides of chaos research: after discussing the Lorenz equations, Hirsch remarks [17]:

> (...) By computer simulation Lorenz found that trajectories seem to wander back and forth between two particular stationary states, in a random, unpredictable way. Trajectories which start out very close together eventually diverge, with no relationship between long run behaviours.
>
> But this type of chaotic behaviour has not been proved. As far as I am aware, practically nothing has been proved about this particular system (...)
>
> A major challenge to mathematicians is to determine which dynamical systems are chaotic and which are not. Ideally one should be able to tell from the form of the differential equations.

More recently, in a 1990 conference, S. Smale formulated a closely related problem about the Lorenz system and general chaotic dynamical systems [23]:

> Are the dynamics of the Lorenz equations described by the geometric Lorenz attractor of Williams, Guckenheimer and Yorke?
>
> Also, the general problem of establishing and analyzing strange attractors of differential equations of physics and engineering is still wide open.

Smale asks for a proof that the Lorenz system has an attractor related to the Williams-Guckenheimer-Yorke (WGY) attractor; he also asks for a general characterization of chaos in «concrete» (or «naturally occurring») dynamical systems. Hirsch asks for a *decision procedure* to test for chaos in a system. If such a procedure were available, given the WGY attractor one would immediately solve Smale's problem for the Lorenz equations just by applying it.

However we showed [3] that no such a decision method exists. Moreover, for any nontrivial characterization of chaos in a dynamical system there will always be systems where proving the existence of chaos is unattainable within reasonable standard axiomatizations. Chaos theory (and dynamical systems theory) is both undecidable—there is no general algorithm to test for chaos in an arbitrary dynamical system—and incomplete—there are infinitely many dynamical systems that will look chaotic on a computer screen, for they are chaotic in an adequate class of standard models for axiomatized mathematics, but such that no proof of that fact will be found within the usual formalizations of dynamical systems theory. Similarly, there are systems whose properties are (formally) equivalent to the proof of intractable problems such as Fermat's Conjecture, or Riemann's Hypothesis, or the *P?NP* question, and those systems are densely dispersed in a natural topology among all dynamical systems. Classical mathematics is dramatically incomplete in the sense of Gödel, and full of extremely difficult problems, that can arise in innocent-looking contexts.

Worse yet: all our first examples for undecidability and incompleteness within axiomatized physics could be formally reduced to elementary arithmetic problems [3] [11]. However we later discovered that that reduction cannot always be made, as we can obtain examples of intractable problems in the axiomatized sciences which are *not* elementary number-theoretic problems in disguise. They stand beyond the pale of arithmetic; they are much more difficult than any arithmetical problem, and yet they look like ordinary mathematical statements.

There are even weirder situations: we can obtain formal expressions that describe physical systems such that *nothing* but trivialities can be proved about them. And again those systems may be shown to lie fully outside the arithmetical hierarchy, since they belong to the non-arithmetical portion of set theory (if we are working, say, within Zermelo-Fraenkel set theory). Those are truly faceless systems, very much like generic sets in forcing models; however their construction shows no relation to the usual forcing tools and can be proved to be outside the reach of the usual forcing techniques.

Our results are consequences of general incompleteness theorems that are to be found in our papers [3] [4] [5] [6] [7] [8] [11] [12] [13] [14] [15]; other references are [19] [24]. In the present paper we summarize and state without proofs our chief results, with a few comments to clarify their meaning.

§2

Mathematical sentences that are undecidable with respect to sensible axiomatic systems are known since the 19th century proof of the independence of the Parallel Postulate from the remaining axioms and postulates of geometry. Here we have a meaningful and «intuitively true» assertion, in a «natural» model for geometry which cannot be deduced from the then available axiom system for that discipline. So, nothing new here.

However Euclid's system is notoriously inadequate according to our current criteria for mathematical rigor. The main surprise that stemmed from Gödel's 1931 incompleteness theorems is the conclusion that, even if we adhere to contemporary standards in the formulation of mathematical proofs, all the usual axiom systems strong enough for most of mathematics turn out to be incomplete; it is enough that they include arithmetic for undecidable sentences to creep up within them.

Yet, due to Gödel's weird examples of undecidable sentences, the hope remained that undecidability and incompleteness would always be peripheral to mainstream mathematics. That is to say, everyday mathematics, as practiced by the professional mathematician, would be untouched by Gödel's stormy results. (In a recent interview René Thom expressed that same hope, when he said that Gödel's results were to be seen as «road signs,» «warning posts,» meaning that one shouldn't go further in that direction, but that they had no meaning for the practicing mathematician [27].)

Cohen's independence proof of the continuum hypothesis from the axioms of Zermelo-Fraenkel set theory shattered that hope, since the continuum hypothesis affects innumerable important results in topology, analysis and even algebra. As it is well known, forcing techniques led to the independence proof of several open questions in mathematics, such as Whitehead's problem in the theory of abelian groups [12], but the application of forcing demands high mathematical ingenuity, while being restricted to nonabsolute assertions in set theory. Thus, finite objects remained outside the scope of forcing, while its success strengthened the feeling that the domain of the finite should be regarded as the ground plan from which arose the whole of mathematics.

§3

Around 1987 the authors started a research program whose main goal was to fully axiomatize physics and to apply modern techniques from mathematical logic to problems in physics. The starting point was a venerable one: the 1900 list of 23 problems that David Hilbert presented to the Second International

Congress of Mathematicians in Paris. The sixth problem in Hilbert's list asks for an axiomatic formulation of physics; the tenth problem asks for a decision procedure to verify whether a polynomial Diophantine equation with integral coefficients does have solutions. Both problems are fused in our results.

We had a twofold goal in that program. First, we wished to place physics (and, if possible, any mathematically-formulated areas in other empirical sciences) in a firm and rigorous footing (according to current conceptions). Second, we wished to obtain «meaningful» undecidable sentences within those theories. Somehow we hoped that formally undecidable assertions in an empirical science might turn out to be, let us say, «empirically» decidable. In order to proceed we drew up a list of a few problems that might lead to our goal. We believed that classification schemes for spacetimes in general relativity might allow the construction of unsolvable problems and of undecidable sentences within the corresponding theory, and we had the feeling that Hirsch's decision problem for chaos theory [17] was algorithmically unsolvable, and that its investigation might prove fruitful. As our tool we only had forcing at the beginning of our efforts, and forcing was applied with modest results to the first question and related areas [7] [12]. The breakthrough came out of a suggestion of P. Suppes who told us about Richardson's [21] examples of unsolvable problems in analysis.

We immediately noticed that Richardson's constructions were in fact realizations of a functor from the theory of formal systems (here coded in Diophantine equations) into classical elementary analysis. We had some previous intuition that such a fully algorithmic functor might exist, one that would translate metamathematics into questions about elementary functions and their properties. However we had no example of such a functor before we learned of Richardson's results [9] [10].

§4

Out of Richardson's examples we immediately obtained almost by chance something that had seemed impossible, an expression for the halting function within a rather simple mathematical language, the language of classical analysis: let $M_n(q)$ be the Turing machine of index n that acts upon the natural number q [22]. Granted that $\theta(n,q)$ be the *halting function* for $M_n(q)$, $\theta(n,q) = 1$ if and only if $M_n(q)$ stops over q, and $\theta(n,q) = 0$ if and only $M_n(q)$ doesn't stop over q.

We suppose that our theories T are *arithmetically consistent*, that is, that the model we are interested in represents arithmetical assertions by the standard model for arithmetic. (One might take $T \supseteq$ ZFC.) Let $p_{n,q}(x_1, x_2,\ldots,x_n)$ be a universal Diophantine polynomial [18]. Let σ be the sign function, $\sigma(\pm x) = \pm 1$, $\sigma(0) = 0$. Then:

Proposition 4.1 (The Halting Function.) *If T is arithmetically consistent, then:*

$$\theta(n, q) = \sigma(G_{n,q}),$$

$$G_{n,q} = \int_{+\infty}^{-\infty} \frac{C_{n,q}(x)\, e^{-x^2}}{1 + C_{n,q}(x)}\, dx,$$

$$C_{n,q}(x) = \lambda p_{n,q}(x_1, \ldots, x_\tau).$$

□

(λ is one of Richardson's maps from the Diophantine polynomials into elementary real analysis [3].)

Out of that we proved a first general undecidability and incompleteness theorem: we say that a predicate P in our language is *nontrivial* if there are term-expressions ξ, ς in our theory T such that $T \vdash P(\xi)$ and $T \vdash \neg P(\varsigma)$. Then we have proved a 3-step undecidability and incompleteness theorem for any nontrivial P in theories that included the language of analysis:

1. In those theories, given any nontrivial predicate P there is a countably infinite family of term-expressions ξ_m such that there is no general algorithm to decide, for an arbitrary m, whether or not $P(\xi_m)$.

2. Even if we can prove that $T \vdash P(\xi_m)$, the function $g(m)$ that bounds those proofs (whenever they can be done) isn't recursive. Therefore those proofs may be arbitrarily difficult.

3. There are (denumerably) infinite many term-expressions ξ in our language so that, in an adequate model **M** it is true that $P(\xi)$, while our theory T neither proves nor disproves that assertion.

Things go much farther; as an example we now quote a recent result on the incompleteness of the theory of *finite* noncooperative games with Nash equilibria, a result that has immediate relevance for neoclassical economics [14].

(As it is well-known, the theory of games was developed by John von Neumann out of some ideas and a major conjecture by Émile Borel with a view towards its applications in economics; in the early 50's Kenneth Arrow and Gérard Debreu translated the theory of competitive markets into the language of game theory, which allowed them to prove the central result of Walrasian neoclassical economics: every competitive market has a set of equilibrium prices. So, when one talks about games, one is talking about competitive markets.)

Proposition 4.2 *If T is arithmetically consistent then:*

1. *Given any nontrivial property P of finite noncooperative games, there is an infinite denumerable family of finite games* Γ_m *such that for those m with* $T \vdash$ «$P(\Gamma_m)$,» *for an arbitrary total recursive function g:* $\omega \to \omega$, *there is an infinite number of values for m such that the length of the proof of* $P\Gamma_m$ *from the axioms of T is strictly larger than* $g(\| P\Gamma_m \|)$, *where* $P\Gamma_m\|$ *is the length of the formal expression that describes* $P\Gamma_m$ *in the language of T.*

2. *Given any nontrivial property P of finite noncooperative games, there is one of those games* Γ *such that* $T \vdash$ «$P(\Gamma)$» if and only if $T \vdash$ *«Fermat's Conjecture.»*

3. *There is a noncooperative game* Γ *where each strategy set* S_i *is finite but such that we cannot compute its Nash equilibria.*

4. *There is a noncooperative game* Γ *where each strategy set* S_i *is finite and such that the computation of its equilibria is T-arithmetically expressible as a* Π_{m+1} *problem, but not to any* Σ_k *problem,* $k \leq m$.

5. *There is a noncooperative game* Γ *where each strategy set* S_i *is finite and such that the computation of its equilibria isn't arithmetically expressible.*

Everything turns out to be undecidable; each nontrivial property, even the simplest one, leads to an incompleteness proof. There are natural problems that turn out to be as difficult as Fermat's problem; there are natural problems that are equivalent to arithmetic problems as high as one wishes in the arithmetical hierarchy; and there are natural problems that lie outside the arithmetical hierarchy.

Our constructions essentially arise from the existence of expressions for the halting function and for characteristic functions in all arithmetic degrees of unsolvability within elementary analysis. No forcing is required; in fact we still wonder why those results weren't discovered earlier. The main constructions are pretty straightforward; however in order to obtain them we had to believe from the very beginning that incompleteness is something that belongs to the way we conceive mathematics. Incompleteness is a natural phenomenon according to our current views about mathematics. And yet everybody seemed to shy away from that fact of mathematical life.

§5

The following problems have been dealt with our methods:

- *The integrability problem in classical mechanics.* There is no general algorithm to decide, for a given hamiltonian, whether or not it is integrable. Also there will be both integrable and nonintegrable hamiltonians in **M** but such that T is unable to prove it [3].

- *The Hirsch problem.* Is there an algorithm to check for chaos given the expressions of a dynamical system? No: there is no such a general algorithm, and there will be systems that look chaotic on a computer screen (that is to say, they are chaotic in our model **M**) but such that proving their chaotic behaviour is impossible in T [3] [17].

- *Penrose's thesis.* Penrose conjectured that classical physics offers no examples of noncomputable phenomena. We gave a counterexample to that assertion [4] [24].

- *«Smooth» problems equivalent to hard number-theoretic problems.* We gave an explicit example of a dynamical system where there will be chaos if and only if Fermat's conjecture is provable. We also showed that (given some conditions) those 'nasty' problems are dense in the space of all dynamical systems [11].

- *Arnol'd's problem.* Arnol'd formulated in the 1974 AMS Symposium on the Hilbert Problems [1] a question dealing with algorithmic decision procedures for polynomial dynamical systems over Z. We showed that again there is no general algorithm available, and that their theory is incomplete [8] [13].

- *Problems in mathematical economics.* Lewis [19] pointed out that our results entail the incompleteness of the theory of hamiltonian models in economics. They also entail the incompleteness of the theory of Arrow-Debreu equilibria and (what is at firs sight surprising) the incompleteness of the theory of *finite* games with Nash equilibria [8] [14].

- *Problems worse than any number-theoretic problem.* They can be constructed (and look «natural») with our techniques [8].

§6

Let's reduce our ideas and techniques to their bare essentials. Suppose that we start from classical axiomatic set theory, which is a framework big enough to contain all of everyday mathematics within it. Let's look at set theory as an abstract construction, a collection of strings of symbols from an alphabet—the collection of all sentences of set theory. We know that we have here a recursively enumerable sequence of objects which can be effectively mapped onto the natural number sequence, so that we can get the whole of our axiomatic sequence coded (through a Gödel numbering) into an infinite recursively enumerable subset of the natural number sequence. Such a sequence can be embedded into several *continuous* mathematical structures within set theory itself! One of those maps is Richardson's, which we have just exploited, but there are infinitely many others.

Once we have thus coded the whole of mathematics into itself, a whole new family of questions appears: say, since we have mapped an axiomatic system into a much larger structure, can we now go back and define out of our embedding some «hyperaxiomatic» structure with brand-new (and sensible) truths and theorems? Do we get something really new here, or can we reduce our hyper-extensions to the traditional setting of first-order recursively enumerable theories?

We know the answer: there definitely are several new results to be found in those maps of mathematics redrawn over its own belly.

§7

However we would like to emphasize a more modest albeit very important point here. We do it by repeating the conclusion in the first of our papers [3]:

> What can we make out of all [those manifold incompleteness theorems]? We cautiously suggest that the trouble may lie not in some essential inner weakness or flaw of mathematical reasoning, but in a too narrow, too limited concept of formal system and of mathematical proof. There is a strongly mechanical, machinery-like archetype behind our current formalizations for the idea of algorithmicity that seems to stem from an outdated 17th century vision *à la* Descartes (even if our current notion of proof is traced back to Greek mathematics). Also a first-order

language such as the one for Zermelo-Fraenkel theory is too weak: even if we can prove all of classical mathematics within it, it is marred by the plethora of undecidability and incompleteness results that we can prove about it, and which affect interesting questions that are also relevant for mathematically-based theories such as physics.

The authors certainly do not know how to, let us say, safely go beyond the limits of the presently available concepts of computability, algorithmicity, and formal system, but they feel that if there are so many quite commonplace things that 'should' somehow be provable or decidable within a sensible mathematical structure, and which however turn out to be algorithmically undecidable or unprovable, then one cannot blame the whole of mathematics for that. Mathematics isn't at fault here. The problem lies in our current ideas about formalized mathematics. They are too weak.

We must look beyond them.

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