Some Reflections on Alan Turing's Centenary

P. D. Welch (University of Bristol)

We review two of Alan Turing's chief publications in mathematical logic: the classic 1936 paper On Computable Numbers [9] *and the less well known paper* Systems of Logic based on Ordinals [10]. *Whilst the former has rightly received enormous attention the latter is really only known amongst logicians. We outline some of the history and background to the first, whilst emphasising a viewpoint often forgotten in discussions of the so-called 'Church-Turing thesis'; we sketch the development of the second paper and see why its results were equivocal and perhaps somewhat disappointing to Turing.*

Early Life

Alan Mathison Turing was born on 23 June 1912 in London to parents of whom his biographer Andrew Hodges [7] aptly conjectures the English novelist George Orwell would have described as "lower upper middle class", his father holding a position in the Indian Civil Service (ICS). This meant that Turing, like many boys of this time and status, would be educated in England either living with relatives or at boarding school. His father eventually retired from the ICS at a relatively senior position in the Presidency of Madras but then for tax reasons continued to live in France.

Turing was thus sent to Sherborne School from the age of 13, which, whilst not Eton or Harrow, would have provided the required respectable education. He seems to have shown early interest in all matters mechanical, chemical and biological and this persisted throughout his life. He showed strong promise in mathematics and a strong ease and facility but without any Gauss-like precocity. His mathematical abilities won him a Scholarship to King's College, Cambridge, which he entered in the Autumn of 1931.

Alan Mathison Turing (1912 London – 1953 Manchester, England)

The intellectual atmosphere in Cambridge at that time, at least in the areas of interest to Turing, would have been dominated by G. H. Hardy and A. Eddington. Of his own peer group he became friends with the future economist David Champernowne. At Sherborne he had read Eddington's "*Nature of the physical world*" and at Cambridge Hardy and also von Neumann's "*Mathematische Grundlagen der Quantum Mechanik*".

He attended Eddington's lectures entitled *"The distribution of measurements in Scientific Experiments"* and this must have engaged him as he found for himself a mathematical problem to work on, leading him to rediscover and prove the Central Limit Theorem in February 1934. It seems to have been typical of him to work things out for himself from first principles and he was thus quite unaware that this had already been proven in a similar form by Lindeberg in 1922.

Notwithstanding this his tutor, the group theorist Philip Hall, encouraged him to write up this work as a Fellowship Dissertation for the King's College competition in 1935, which was done, being entitled *On the Gaussian Error Function*. This was accepted 16 March 1936, Hall arguing that the rediscovery of a known theorem was a significant enough sign of Turing's strength (which he argued had not yet achieved its full potential). Turing thus won a three-year fellowship, renewable for another three, with £300 *per annum* with room and board. He was 22 years old.

His first published work was in group theory and was finished in March 1935,[1] this being a contribution to the theory of almost periodic functions, improving a result of von Neumann. By coincidence von Neumann arrived the very next month in Cambridge and proceeded to lecture on this subject, and they must have become acquainted from this time.

Probably more decisive than meeting von Neumann was his contact with Max Newman. In Spring 1935 he went on a Part III course of Newman's on the Foundations of Mathematics. (Part III courses at Cambridge were, and are, of a level beyond the usual undergraduate curriculum but preparatory to undertaking a research career.) Newman was a topologist and interested in the theory of sets. Newman attended Hilbert's lecture at the 1928 International Congress of Mathematicians. Logic at this time had disappeared at Cambridge: Russell was no longer present, having left in 1916, and Frank Ramsey had died in 1931. Wittgenstein had moved on from his logical atomistic days and the concerns of the Tractatus to other things (although Turing did attend a Wittgenstein seminar series and conversed with him). Hence Newman was more influenced by Hilbert and Göttingen.

Hilbert had worked on foundational matters for the previous decades and would continue to do so. His aim to obtain a secure foundation for mathematics by finding proofs of consistency of large parts (if not all) of mathematics by a process of systematic axiomatisation, and then showing that these axiomatisations were safe by providing finite consistency proofs, looked both reasonable and possible. By systematic effort Hilbert and his school had reduced the questions of the consistency of geometry to analysis. There seemed reasonable hope that genuinely finitary methods of proof could render arithmetic provably consistent within finite arithmetical means.

The address that Hilbert gave at the 1928 Congress (when Germany had been re-admitted to the International Congress of Mathematicians after being denied this in 1924) not only gave a plea for the internationalist, apolitical nature of mathematical research but also formulated several important questions for this foundational project.

Hilbert's programme and the *Entscheidungsproblem*

- (I. Completeness) His dictum, concerning the belief (engraved as the famous *non ignorabimus* on his gravestone) that any mathematical problem was in principle solvable, can be restated as the belief that mathematics was *complete*. That is, given any properly formulated mathematical proposition P, either a proof of P could be found or a disproof.
- (II. Consistency) The question of *consistency* – given a set of axioms for, say, arithmetic, such as the Dedekind-Peano axioms, PA, could it be shown that no proof of a contradiction can possibly arise? Hilbert stringently wanted a proof of consistency that was finitary, that made no appeal to infinite objects or methods.
- (III. Decidability – the *Entscheidungsproblem*) Could there be a finitary process or algorithm that would *decide* for any properly formulated proposition P whether it was derivable from axioms or not?

Of course the main interest was consistency but there was hope (discernible from some of the writings of the Göttingen group) that there was such a process and therefore a positive solution to the *Entscheidungsproblem*. From others came expressions that it was not:

> Hardy:
> *"There is of course no such theorem and this is very fortunate, since if there were we should have a mechanical set of rules for the solution of all mathematical problems, and our activities as mathematicians would come to an end."* [6]

> von Neumann:
> *"When undecidability fails, then mathematics as it is understood today ceases to exist; in its place there would be an absolutely mechanical prescription with whose help one could decide whether any given sentence is provable or not."* [12]

Gödel's Incompleteness Theorems block Hilbert's programme

Theorem 1. (Gödel-Rosser First Incompleteness Theorem – 1931) *For any theory T containing a moderate amount of arithmetical strength, with T having an effectively given list of axioms, then: if T is consistent then it is incomplete, that is, for some proposition neither $T \vdash P$ nor $T \vdash \neg P$.*

The theorem is, deliberately, written out in a semi-modern form. Here, it suffices that T contain the Dedekind-Peano axioms, PA, to qualify as having a 'moderate amount of arithmetical strength'. The axioms of PA can be written out as an 'effectively given' list, since although the axioms of PA include an infinite list of instances of the Induction Axiom, we may write out an effective prescription for listing them. Hence PA satisfies the theorem's hypothesis. Gödel had used a version of the system of *Principia Mathematica* of Russell and Whitehead but was explicit in saying that the theorem had a wide applicability to any sufficiently strong "formal system" (although without being able to specify completely what that meant).

This immediately established that PA is incomplete, as is *any* theory containing the arithmetic of PA. This destroys any hope for the full resolution of Hilbert's programme that he had hoped for.

However in a few months there was more to come:

Theorem 2. (Gödel's Second Incompleteness Theorem – 1931) *For any consistent T as above, containing the axioms of* PA*, the statement that 'T is consistent' (when formalised as 'Con_T') is an example of such an unprovable sentence.*

Symbolically:

$$T \nvdash \mathrm{Con}_T$$

The first theorem thus demonstrated the incompleteness of any such formal system, and the second the impossibility of demonstrating the consistency of the system by the means of formal proofs within that system. The first two of Hilbert's questions were thus negatively answered. What was left open by this was the *Entscheidungsproblem*. That there might be some effective or finitary process is not ruled out by the Incompleteness Theorems. But what could such a process be like? How could one *prove* something about a putative system that was not precisely described, and certainly not *mathematically* formulated?

Church and the λ-calculus

One attempt at resolving this final issue was the system of functional equations called the "λ-calculus" of Alonzo Church. He had obtained his thesis in 1927 and, after visiting Amsterdam and Göttingen, was appointed an assistant professor in Princeton in 1931. The λ-calculus gave a strict, but rather forbidding, formalism for writing out terms defining a class of functions from base functions and a generalised recursion or induction scheme. Church had only established that the simple number successor function was "λ-definable" when his future PhD student Stephen Cole Kleene arrived in 1931; by 1934 Kleene had shown that all the usual number theoretic functions were also λ-definable. They used the term "effectively calculable" for the class of functions that could be computed in the informal sense of effective procedure or algorithm alluded to above.

Church ventured that the notion of λ-definability should be taken to coincide with "effectively calculable".

Church's Thesis (1934 – first version, unpublished) ***The effectively calculable functions coincide with the λ-definable functions.***

At first Kleene tried to refute this by a diagonalisation argument along the lines of Cantor's proof of the uncountability of the real numbers. He failed in this but instead produced a theorem: the *Recursion Theorem*. Gödel's view of the suggestion contained in the thesis when Church presented it to him was that it was "thoroughly unsatisfactory".

Gödel meanwhile had formulated an expanded notion of primitive recursive functions that he had used in his Incompleteness papers; these became known as the *Herbrand-Gödel general recursive functions*. He lectured on these in 1934 whilst visiting the IAS, Princeton.

Church and Kleene were in the audience and seem to have decided to switch horses. Kleene:

> "I myself, perhaps unduly influenced by rather chilly receptions from audiences around 1933–35 to disquisitions on λ-definability, chose, after [Herbrand-Gödel] general recursiveness had appeared, to put my work in that format . . . "

Preliminary solutions to the *Entscheidungsproblem*

By 1935 Church could show that there was no λ-formula "*A* conv *B*" iff the λ-terms *A* and *B* were convertible to each other within the λ-calculus. Moreover, mostly by the work of Kleene, they could show the λ-definable functions were co-extensive with the general recursive functions. Putting this "non-λ-definable-conversion" property together with this last fact, there was therefore a problem which, when coded in number theory, could not be solved using general recursive functions. This was published by Church [2]. Another thesis was formulated:

Church's Thesis (1936 – second version) ***The effectively calculable functions coincide with the [H-G] general recursive functions.***

Gödel still indicated at the time that the issue was unresolved and that he was unsure that the general recursive functions captured all informally calculable functions.

"On Computable Numbers"

Newman and Turing were unaware of these developments in Princeton. The first subject of Turing's classic paper is ostensibly 'Computable Numbers' and is said to be only "with an application to the *Entscheidungsproblem*". He starts by restricting his domain of interest to the natural numbers, although he says it is almost as easy to deal with computable functions of computable real numbers (but he will deal with integers as being the 'least cumbrous'). He briefly initiates the discussion calling computable numbers those 'calculable by finite means.'

In the first section he compares a man computing a real number to a machine with a finite number of states or '*m*-configurations' $q_1, \ldots, q_R$. The machine is supplied with a 'tape' divided into cells capable of containing a single symbol from a finite alphabet. The machine is regarded as scanning, and being aware of, only the single symbol in the cell being

King's College Rowing Team 1935 (2nd from the left, rear row) after his election to a Fellowship

viewed at any moment in time. The possible behaviour of the machine is determined only by the current state q_n and the current scanned symbol S_r which make up the current configuration of the machine. The machine may operate on the scanned square by erasing the scanned symbol or writing a symbol. It may move one square along the tape to the left or to the right. It may also change its *m*-configuration.

He says that some of the symbols written will represent the decimal expansion of the real number being computed, and others (subject to erasure) will be for scratch work. He thus envisages the machine continuously producing output, rather than halting at some stage. It is his contention that "these operations include all those which are used in the computation of a number". His intentions are often confused with statements such as 'Turing viewed any machine calculation as reducible to one on a Turing machine' or some thesis of this form. Or that he had 'distilled the essence of machine computability down to that of a Turing machine'. He explicitly warns us that no "real justification will be given for these definitions until Section 9".

In Section 2 he goes on to develop a theory of his machines giving and discussing some definitions. He also states:

> "If at each stage the motion of the machine is *completely* determined by the configuration, we shall call the machine an 'automatic' or *a*-machine."

> "For some purposes we may use machines whose motion is only partly determined. When such a machine reaches one of these ambiguous configurations, it cannot go on until some arbitrary choice has been made . . . "

Having thus in two sentences prefigured the notion of what we now call a *non-deterministic Turing machine* he says that he will stick in the current paper only to *a*-machines, and will drop the '*a*'. He remarks that such a non-deterministic machine 'could be used to deal with axiomatic systems'. (He is probably thinking here of the choices that need to be made when developing a proof line-by-line in a formal system.) The succeeding sections develop the theory of the machines. The theory of a *"universal machine"* is explicitly described, as is in particular the conception of program as input or stored data and the mathematical argument using Cantor's diagonalisation technique, to show the impossibility of determining by a machine, whether a machine program was 'circular' or not. (Thus, as he does not consider a complete computation as a halted one, he instead considers first the problem of whether one can determine a looping behaviour.)

Section 9 "The extent of the computable numbers" is in some ways the heart of the paper, in particular for later discussions of the so-called 'Turing' or 'Church-Turing' theses. It is possibly of a unique character for a paper in a purely mathematical journal of that date (although perhaps reminiscent of Cantor's discussions on the nature of infinite sets in *Mathematische Annalen*). He admits that any argument that any calculable number (by a human) is "computable" (i.e., in his machine sense) is bound to hang on intuition and so be mathematically somewhat unsatisfactory. He argues that the basis of the machine's construction earlier in the paper is grounded on an analysis of what a human computer does when calculating. This is done by appealing to the obvious finiteness conditions of human capabilities: the possibilities of surveying the writing paper and observing symbols together with their writing and erasing.

It is important to see that this analysis should be taken *prior to* the machine's description. (Indeed one can imagine the paper re-ordered with this section placed at the start.) He had asked:

> *"What are the possible* processes *which can be carried out in computing a real number."* [Author's emphasis]

It is as if the difference between the Princeton approach and Turing's is that the former appeared to be concentrating on discovering a definition whose extension covered in one blow the notion of effectively calculable, whereas Turing concentrated on process, the very act of calculating.

According to Gandy [5] Turing has in fact proved a *theorem* albeit one with unusual subject matter. What has been achieved is a complete analysis of human computation in terms of finiteness of the human acts of calculation broken down into discrete, simple and locally determined steps. Hence:

> **Turing's Thesis: Anything that is humanly calculable is computable by a Turing machine.**

(i) Turing provides a philosophical paradigm when defining "effectively calculable", in that a vague intuitive notion has been given a unique meaning which can be stated with complete precision.
(ii) He also makes possible a completely precise understanding of what is a 'formal system' thereby making an exact statement of Gödel's results possible (see the quotation below). He claims to have a machine that will enumerate the theorems of predicate calculus. This also makes possible a correct formulation of Hilbert's 10th problem. It is important to note that Turing thus makes expressions along the lines of "such and such a proposition is undecidable" have mathematical content.
(iii) In the final four pages he gives his solution to the *Entscheidungsproblem*. He proves that there is no machine that will decide of any formula φ of the predicate calculus whether it is derivable or not.

He was 23. His mentor and teacher Max Newman was astonished and at first reacted with disbelief. He had achieved what the combined mental resources of Hilbert's Göttingen school and Princeton had not, and in the most straightforward, direct, even simple manner. He had attended Newman's Part III course on the Foundations of Mathematics in Spring 1935 and within 14 months had solved the last general open problem associated with Hilbert's programme.

However, this triumph was then tempered by the arrival of Church's preprint of [1] which came just after Turing's proof was read by Newman. The latter however convinced the London Mathematical Society that the two approaches were sufficiently different to warrant publication; this was done in November 1936, with an appendix demonstrating that the machine approach was co-extensional with the λ-definable functions, and with Church as referee.

Gödel again:[2]

> "When I first published my paper about undecidable propositions the result could not be pronounced in this generality, because for the notions of mechanical procedure and of formal system no mathematically satisfactory definition had been given at that time ... The essential point is to define what a procedure is."

> "That this really is the correct definition of mechanical computability was established beyond any doubt by Turing."

Turing's "Ordinal logics"

In 1937 Turing went to Princeton but was somewhat dismayed to find only Church and Kleene there. He first asked von Neumann for a problem, and von Neumann passed on one from Ulam concerning the possibility of approximating continuous groups with finite ones which Turing soon answered negatively.

With this and some other work he published two papers on group theory (described in a letter to Philip Hall as 'small papers, just bits and pieces'; nevertheless they appeared in Compositio and Annals of Mathematics).

He stayed on in Princeton on a Procter Fellowship (of these there were three, one each for candidates from Cambridge, Oxford and the Collège de France). He decided to work towards a PhD under Church. He still had a King's Fellowship and thus a PhD would not have been of great use to him in the Cambridge of that day. He completed his thesis in two years (even whilst grumbling about Church's "suggestions which resulted in the thesis being expanded to appalling length" – it is 106 pages). The topic (probably suggested by Church) concerned trying to partially circumvent incompleteness of formal theories T by adding as axioms statements to the effect that the theory was consistent.

To illustrate the thesis problem with an example (where we may think of T_0 as PA again) set:

$$T_1 : T_0 + \mathrm{Con}(T_0)$$

where "$\mathrm{Con}(T_0)$" is some expression arising from the Incompleteness Theorems expressing that "T_0 is a consistent system"; as $\mathrm{Con}(T_0)$ is not provable from T_0, this is a deductively stronger theory; continuing:

$$T_{k+1} : T_k + \mathrm{Con}(T_k) \text{ for } k < \omega, \quad \text{and then:} T_\omega = \bigcup_{k<\omega} T_k.$$

Presumably we may still continue:

$$T_{\omega+1} = T_\omega + \mathrm{Con}(T_\omega) \text{ etc.}$$

We thus obtain a transfinite hierarchy of theories. As would occur to many people who have spent even a moderate amount of time pondering the Second Incompleteness Theorem, one could ask of this sequence of theories of increasing deductive strength, what can one in general prove from a theory in this sequence? (Indeed this is just one question one can see about the incompleteness results that bubble up from time to time on MathOverflow.)

Turing called these theories "Logics" and used the letter "L" but I shall use the modern convention. He was thus investigating the question as to what extent such a sequence could be 'complete':

Question: Can it be that for any problem A there might be an ordinal α so that T_α proves A or $\neg A$?

Actually he was aiming at a more restricted question, namely what he called *number theoretic problems* which are those that can be expressed in an '$\forall\exists$' form (the twin primes conjecture comes to mind). He does not clarify why he alights on this particular form of the question.

There are several items that must be discussed first, in order to give this sketch of a progression of theories even some modicum of precision. To formally write down in the language of PA a sentence that says "Con(PA)" one really needs a formula $\varphi_0(v_0)$ that defines for us the set of Gödel code numbers n of instances of the axiom set T_0 = PA. There are infinitely many such formulae but we choose one which is both simple (it is Σ_1, meaning definable using a single existential quantifier) and *canonical* in that it simply defines the axiom numbers in a straightforward manner. Assuming we have a φ_0, we then may set $\varphi_{k+1}(\bar{n}) \longleftrightarrow \varphi_k(\bar{n}) \vee \mathrm{Con}(\varphi_k)$ where $\mathrm{Con}(\varphi_k)$ expresses in a Gödelian fashion the consistency of the axiom set defined by φ_k.

But what to do at stage ω? How you choose a formula for a limit stage depends on how you approach that stage, but the problem even occurs for stage ω: how do you define a formula that uniformly depends on the previous stages so that you can express the "union" set of axioms correctly?

Notation and progressions

Turing solved this and devised a method for assigning sets of sentences, so theories, to all constructive (also called *recursive* or *computable*) ordinals by the means of *notations*. In essence a notation for an ordinal is merely some name for it but a system of notations (which Turing used) was invented by Kleene using the λ-calculus. Nowadays we also use the idea of being able to name the ordinal α by the natural number index e of a computable function $\{e\}$ which computed the characteristic function of a well-order of $\mathbb{N}$ of order type α.

This essentially yields a tree order with infinite branching at all and only constructive ordinal limit points.

The set of notations $O \subset \mathbb{N}$ thus forms a tree order, with $n <_O m \leftrightarrow |n| < |m|$, where $|\cdot|$ is the ordinal rank function (defined by transfinite recursion along $<_O$) satisfying:

$$|0| = 0; \quad |2^a| = |a| + 1; \quad |3^e| = lim_{n\to\infty}|\{e\}(n)|.$$

However O is a *co-analytic* set of integers and is thus highly complex. Let $suc(a) =_{df} 2^a$ and let $lim(e) =_{df} 3^e$.

Definition 1. *A* progression based on a theory T *is a primitive recursive mapping $n \longrightarrow \varphi_n$ where φ_n is an $\exists$ formula such that* PA *proves:*
(i) $T_0 = T$*;*
(ii) $\forall n(T_{\mathrm{suc}(n)} = T_n + \mathrm{Con}(\varphi_n))$*;*
(iii) $T_{\mathrm{lim}(n)} = \bigcup_m T_{\{n\}(m)}$.

Thus one attaches *in a uniform manner* formulae φ_a to define theories T_a to *every* $a \in \mathbb{N}$ of the form $suc(a), lim(a)$. However this does not tell us how to build progressions which can be justified by the Recursion Theorem.

An *explicit consistency sequence* is then defined to be the restriction of a progression to a path through O.

With these tools Turing proved a form of an enhanced Completeness Theorem.

Theorem 3 (Turing's Completeness Theorem)**.** *For any true $\forall$ sentence of arithmetic, ψ, there is a $b = b(\psi) \in O$ with $|b| = \omega + 1$, so that $T_b \vdash \psi$. The map $\psi \rightarrowtail b(\psi)$ is given by a primitive recursive function.*

Thus we may for any true ψ find a path through O of length $\omega + 1$,

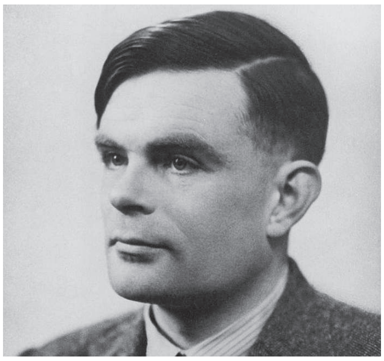
$$T = T_0, T_1, \ldots, T_{\omega+1} = T_b$$

with the last proving ψ. At first glance this looks like magic: how does this work, and can we use it to discover more $\forall$-facts about the natural number system?

However, there is a trick here. As Turing readily admits, what one does is construct for *any* $\forall$ sentence ψ an extension $T_{b(\psi)}$ proving ψ with $|b(\psi)| = \omega + 1$. Then *if ψ is true* we deduce that $T_{b(\psi)}$ is a consistent extension in a proper consistency sequence (notice that conditional in the antecedent of the theorem's statement). However *if ψ is false* $T_{b(\psi)}$ turns out to be merely inconsistent, and so proves anything. In general it is harder to answer ?$b \in O$? than the original $\forall$ question and so we have gained no new arithmetical knowledge. The outcome of the investigation is thus somewhat equivocal: we *can* say that some progressions of theories will produce truths of arithmetic but we cannot determine which ones they will turn out to be.

He regarded the results as somewhat disappointing. He had only succeeded in proving a theorem for '$\forall$' problems and not for his chosen 'number theoretic problems'. He had, moreover, proven another theorem that stated that there would be $b, c \in O$, with, for example, $|b| = |c| = \omega + 1$, such that T_b and T_c would prove different families of sentences. Thus *invariance* would fail even for theories of the same "depth".

It does contain a remarkable aside however. Almost as a throw-away comment he introduces what has come to be called a *relativised Turing Machine* or (as he called it) an *oracle machine*. This machine is allowed an instruction state that permits it to query an 'oracle' (considered perhaps as an infinite bit-stream of information about the members of $B \subseteq \mathbb{N}$ written out on a separate tape) whether ?$n \in B$? An answer is received and computation continues. With this one can develop the idea of 'relative computability' – whether membership of m in set A can be determined from knowledge of finitely many membership questions about set B. This notion is central to modern computability theory. However, Turing



Royal Society Election portrait 1951

introduces the concept, (dubbing it an 'oracle' or *o*-machine) and uses it somewhat unnecessarily to prove the point that there are arithmetic problems that are not in his sense number theoretic problems. And then ignores it for the rest of the paper; it is unused in the sequel.

The paper, duly published in 1939, lay somewhat dormant until taken up by Spector and Feferman some 20 years later. Feferman did a far reaching analysis of the notion of general progressions, using not just formalised consistency statements as Turing had done but also other forms that, roughly speaking, ensured the preservation of truth. Note that a *general* consistency sequence step will not necessarily preserve truth of even say existential statements. However, a properly formulated 'existential soundness' statement – that existential sentences provable from the theory are true – when iterated or progressed in the above manner, can result in a '$\forall\exists$'-completeness statement of the Turing kind. Indeed, it can be shown that there are paths through $\mathcal{O}$ along which *all* true sentences of arithmetic are provable. However, finding a path through $\mathcal{O}$ is no simpler than determining whether a single b is in $\mathcal{O}$, so again there is this equivocal feeling to the results. It is compounded by the fact that there are also paths through $\mathcal{O}$, as Spector and Feferman found, which do *not* establish all truths of arithmetic.

The photograph shows Turing on his election as Fellow of the Royal Society in London in 1951 with a citation for "On Computable Numbers"; he was not the youngest at an age of 38 (Hodges notes that Hardy had been elected at 33 and Ramanujan at 30). Three further small articles appeared on the lambda-calculus, but otherwise Turing published nothing further on mathematical logic.

This article does not aim to discuss his contributions to the wartime decoding effort, the development of actual computers or to morphogenesis but in all these areas he displayed an open mind to ideas no matter whence they came and a startlingly fresh, lucid, when not even slyly mischievious, writing style that is exemplified by his *Mind* paper [11]. He had an ability to get to the heart of a problem and express it in simple, clear terms. Robin Gandy told an anecdote of Turing entering the room where two engineers were laboriously testing the permeability of the cores in certain transformers of radio receivers. Robin marvelled to see Turing take a clean piece of paper, write at the top Maxwell's equations and then proceed to derive what they wanted *ab ovo*.

I'll conclude on a more visionary note with a quotation from an interview he gave following a discussion of a famous British neuroscientist's well publicised lecture on the impossibility of the brain being a mere machine. It shows that he was indeed visionary in what computers would be capable of.

Whilst reports to the US Government or military at about this time supposedly emphasised the rarefied nature of the new or even nascent machines, that they would only be used in university (or presumably government) laboratories or that "five or six machines would suffice for the whole country", Turing's view could not have been more different: he suggested that computers would permeate all walks of life and that in 100 years a machine would pass what has come to be called the "Turing Test."

> *"This is only a foretaste of what is to come, and only the shadow of what is going to be. We have to have some experience with the machine before we really know its capabilities. It may take years before we settle down to the new possibilities, but I do not see why it should not enter any of the fields normally covered by the human intellect and eventually compete on equal terms."*
>
> (Press Interview with *The Times*, June 1949)

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To the interested reader the following are also recommended; they were consulted once more during my preparation of this lecture: Davis' anthology [3] of the early fundamental papers in the subject, Gandy's paper [5], Soare's article on the early history of computation theory [8] and, for a very readable account of progressions in theories, [4].

Notes

1. *Equivalence of left and right almost periodicity*, J. of the London Math. Society, 10, 1935.
2. There are several approving quotes from Gödel; this is taken from an unpublished (and ungiven) lecture in the *Nachlass* Gödel, Collected Works, Vol. III, p. 166–168.

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Philip Welch [p.welch@bristol.ac.uk] is Professor of mathematical logic at the University of Bristol. From 1997 to 2000 he was Professor of mathematical logic at the graduate school, Kobe University, setting up a new research group in set theory. He has held a Mercator Professorship at Bonn University and is a regular visitor at the Kurt Gödel Research Center in Vienna.