ALAN MATHISON TURING

1912-1954

THE sudden death of ALAN TURING on 7 June 1954 deprived mathematics and science of a great original mind at the height of its power. After some years of scientific indecision, since the end of the war, Turing had found, in his chemical theory of growth and form, a theme that gave the fullest scope for his rare combination of abilities, as a mathematical analyst with a flair for machine computing, and a natural philosopher full of bold original ideas. The preliminary report of 1952, and the account that will appear posthumously, describe only his first rough sketch of this theory, and the unfulfilled design must remain a painful reminder of the loss that his early death has caused to science.

Alan Mathison Turing was born in London on 23 June 1912, the son of Julius Mathison Turing, of the Indian Civil Service, and of Ethel Sara Turing (née Stoney). The name 'Turing' is of Scottish, perhaps ultimately of Norman origin, the final g being an addition made by Sir William Turing, of Aberdeenshire, in the reign of James VI and I. The Stoneys, an English-Irish family of Yorkshire origin, produced some distinguished physicists and engineers in the nineteenth century, three of whom became Fellows of the Society; and Edith A. Stoney was one of the early women equal-to-wranglers at Cambridge (bracketed with 17th Wrangler, 1893).

Alan Turing's interest in science began early and never wavered. Both at his preparatory schools and later at Sherborne, which he entered in 1926, the contrast between his absorbed interest in science and mathematics, and his indifference to Latin and 'English subjects' perplexed and distressed his teachers, bent on giving him a well-balanced education. Many of the characteristics that were strongly marked in his later life can already be clearly seen in remembered incidents of this time: his particular delight in problems, large or small, that enabled him to combine theory with the kind of experiments he could carry out with his own hands, with the help of whatever apparatus was at hand; his strong preference for working everything out from first principles instead of borrowing from others—a habit which gave freshness and independence to his work, but also undoubtedly slowed him down, and later on made him a difficult author to read. At school homemade experiments in his study did not fit well into the routine of the house: a letter from his housemaster mentions 'Heaven knows what witches' brew blazing on a naked wooden window sill'. But before he left school his abilities,

and his obvious seriousness of purpose, had won him respect and affection, and even tolerance for his own peculiar methods.

In 1931 he entered King's College, Cambridge, as a mathematical scholar. A second class in Part I of the Tripos showed him still determined not to spend time on subjects that did not interest him. In Part II he was a Wrangler, with '*b**', and he won a Smith's Prize in 1936. He was elected a Fellow of King's in 1935, for a dissertation on the Central Limit Theorem of probability (which he discovered anew, in ignorance of recent previous work).

It was in 1935 that he first began to work in mathematical logic, and almost immediately started on the investigation that was to lead to his best known results, on computable numbers and the 'Turing machine'. The paper attracted attention as soon as it appeared and the resulting correspondence led to his spending the next two years (1936-8) in Princeton, working with Professor Alonzo Church, the second of them as Procter Fellow.

In 1938 Turing returned to Cambridge; in 1939 the war broke out. For the next six years he was fully occupied with his duties for the Foreign Office. These years were happy enough, perhaps the happiest of his life, with full scope for his inventiveness, a mild routine to shape the day, and a congenial set of fellow-workers. But the loss to his scientific work of the years between the ages of 27 and 33 was a cruel one. Three remarkable papers written just before the war, on three diverse mathematical subjects, show the quality of the work that might have been produced if he had settled down to work on some big problem at that critical time. For his work for the Foreign Office he was awarded the O.B.E.

At the end of the war many circumstances combined to turn his attention to the new automatic computing machines. They were in principle realizations of the 'universal machine' which he had described in the 1937 paper for the purpose of a logical argument, though their designers did not yet know of Turing's work. Besides this theoretical link, there was a strong attraction in the many-sided nature of the work, ranging from electric circuit design to the entirely new field or organizing mathematical problems for a machine. He decided to decline an offer of a Cambridge University Lectureship, and join the group that was being formed at the National Physical Laboratory for the design, construction and use of a large automatic computing machine. In the three years (1945-8) that this association lasted he made the first plan of the ACE, the N.P.L's automatic computer, and did a great deal of pioneering work in the design of sub-routines.

In 1948 he was appointed to a Readership in the University of Manchester, where work was beginning on the construction of a computing machine by F. C. Williams and T. Kilburn. The expectation was that Turing would lead the mathematical side of the work, and for a few years he continued to work, first on the design of the sub-routines out of which the larger programmes for such a machine are built, and then, as this kind of work became standardized. on more general problems of numerical analysis. From 1950 onward he

turned back for a while to mathematics and finally to his biological theory. But he remained in close contact with the Computing Machine Laboratory, whose members found him ready to tackle the mathematical problems that arose in their work, and what is more, to find the answers, by that combination of powerful mathematical analysis and intuitive short cuts that showed him at heart more of an applied than a pure mathematician.

He was elected to the Fellowship of the Society in 1951.

For recreation he turned mostly to those 'home-made' projects and experiments, self-contained both in theory and practice, that have already been mentioned: they remained a ruling passion up to the last hours of his life. The rule of the game was that everything was to be done with the materials at hand, and worked out from data to be found in the house, or in his own head. This sort of self-sufficiency stood him in good stead in starting on his theory of 'morphogenesis', where the preliminary reading would have drowned out a more orthodox approach. In everyday life it led to a certain fondness for the gimcrack, for example the famous Bletchley bicycle, the chain of which would stay on if the rider counted his pedal-strokes and executed a certain manœuvre after every seventeen strokes.

After the war, feeling in need of violent exercise, he took to long distance running, and found that he was very successful at it. He won the 3 miles and 10 miles championships of his club (the Walton Athletic Club), both in record time, and was placed fifth in the A.A.A. Marathon race in 1947. He thought it quite natural to put this accomplishment to practical use from time to time, for example by running some nine miles from Teddington to a technical conference at the Post Office Research Station in North London, when the public transport proved tedious.

In conversation he had a gift for comical but brilliantly apt analogies, which found its full scope in the discussions on 'brains *v.* machines' of the late 1940's. He delighted in confounding those who, as he thought, too easily assumed that the two things are separated by an impassable gulf, by challenging them to produce an examination paper that could be passed by a man, but not by a machine. The unexpected element in human behaviour he proposed, half seriously, to imitate by a random element, or roulette-wheel, in the machine. This, he said, would enable proud owners to say 'My machine' (instead of 'My little boy') 'said such a funny thing this morning'.

Those who knew Turing will remember most vividly the enthusiasm and excitement with which he would pursue any idea that caught his interest, from a conversational hare to a difficult scientific problem. Nor was it only the pleasure of the chase that inspired him. He would take the greatest pains over services, large or small, to his friends. His colleagues in the computing machine laboratory found him still as ready as ever with his help for their problems when his own interests were fully engaged with his bio-chemical theory; and, as another instance, he gave an immense amount of thought and care to the selection of the presents which he gave to his friends and their children at Christmas.

His death, at a time when he was fully absorbed in his scientific work, was a great and sad loss to his friends, as well as to the wider world of science.

Scientific work

The varied titles of Turing's published work disguise its unity of purpose. The central problem with which he started, and to which he constantly returned, is the extent and the limitations of mechanistic explanations of nature. All his work, except for three papers in pure mathematics (1935*b*, 1938*a* and *b*) grew naturally out of the technical problems encountered in these inquiries. His way of tackling the problem was not by philosophical discussion of general principles, but by mathematical proof of certain limited results: in the first instance the impossibility of the too sanguine programme for the complete mechanization of mathematics, and in his final work, the possibility of, at any rate, a partial explanation of the phenomena of organic growth by the 'blind' operation of chemical laws.

1. *Mathematical logic*

The Hilbert decision-programme of the 1920's and 30's had for its objective the discovery of a general process, applicable to any mathematical theorem expressed in fully symbolical form, for deciding the truth or falsehood of the theorem. A first blow was dealt at the prospects of finding this new philosopher's stone by Gödel's incompleteness theorem (1931), which made it clear that truth or falsehood of A could not be equated to provability of A or not-A in any finitely based logic, chosen once for all; but there still remained in principle the possibility of finding a mechanical process for deciding whether A, or not-A, or neither, was formally provable in a given system. Many were convinced that no such process was possible, but Turing set out to demonstrate the impossibility rigorously. The first step was evidently to give a definition of 'decision process' sufficiently exact to form the basis of a mathematical proof of impossibility. To the question 'What is a "mechanical" process?' Turing returned the characteristic answer 'Something that can be done by a machine', and he embarked on the highly congenial task of analyzing the general notion of a computing machine. It is difficult to-day to realize how bold an innovation it was to introduce talk about paper tapes and patterns punched in them, into discussions of the foundations of mathematics. It is worth while quoting from his paper (1937*a*) the paragraph in which the computing machine is first introduced, both for the sake of its content and to give the flavour of Turing's writings.

'1. *Computing machines*

'We have said that the computable numbers are those whose decimals are calculable by finite means. This requires rather more explicit definition. No real attempt will be made to justify the definitions given until we reach §9. For the present I shall only say that the justification lies in the fact that the human memory is necessarily limited.

'We may compare a man in the process of computing a real number to a machine which is only capable of a finite number of conditions q_1, q_2, ..., q_R which will be called "m-configurations". The machine is supplied with a "tape" (the analogue of paper) running through it, and divided into sections (called "squares") each capable of bearing a "symbol". At any moment there is just one square, say the r-th, bearing the symbol $\mathfrak{S}(r)$ which is "in the machine". We may call this square the "scanned square". The symbol on the scanned square may be called the "scanned symbol". The "scanned symbol" is the only one of which the machine is, so to speak, "directly aware". However, by altering its m-configuration the machine can effectively remember some of the symbols which it has "seen" (scanned) previously. The possible behaviour of the machine at any moment is determined by the m-configuration q_n and the scanned symbol $\mathfrak{S}(r)$. This pair $q_n, \mathfrak{S}(r)$ will be called the "configuration": thus the configuration determines the possible behaviour of the machine. In some of the configurations in which the scanned square is blank (*i.e.* bears no symbol) the machine writes down a new symbol on the scanned square: in other configurations it erases the scanned symbol. The machine may also change the square which is being scanned, but only by shifting it one place to right or left. In addition to any of these operations the m-configuration may be changed. Some of the symbols written down will form the sequence of figures which is the decimal of the real number which is being computed. The others are just rough notes to "assist the memory". It will only be these rough notes which will be liable to erasure.

'It is my contention that these operations include all those which are used in the computation of a number. The defence of this contention will be easier when the theory of the machines is familiar to the reader.'

In succeeding paragraphs he gave arguments for believing that a machine of this kind could be made to do any piece of work which could be done by a human computer obeying explicit instructions given to him before the work starts. A machine of the kind he had described could be made for computing the successive digits of π, another for computing the successive prime numbers, and so forth. Such a machine is completely specified by a table, which states how it moves from each of the finite sets of possible 'configurations' to another. In the computations mentioned above, of π and of the successive primes, the machine may be supposed to be designed for its special purpose. It is supplied with a blank tape and started off. But we may also imagine a machine supplied with a tape already bearing a pattern which will influence its subsequent behaviour, and this pattern might be the table, suitably encoded, of a particular computing machine, X. It could be arranged that this tape would cause the machine, M, into which it was inserted to behave like machine X. Turing proved the fundamental result that there is a 'universal' machine, U (of which he gave the table), which can be made to do the

work of any assigned special-purpose machine, that is to say to carry out any piece of computing, if a tape bearing suitable 'instructions' is inserted into it. The machine U is so constructed that, presented with a tape bearing any arbitrary pattern it will move through a determinate, in general endless, succession of configurations; and it may or may not print at least one digit, 0 or 1. If it does, the pattern is 'circle-free'. It is therefore a problem, for which a decision process might be sought, to determine from inspection of a tape, whether or not it is circle-free. By means of a Cantor diagonal argument, Turing showed that no instruction-tape will cause the machine U to solve this problem, i.e. no pattern P is such that U, when presented with P followed by an arbitrary pattern Υ, will print 0 if Υ is 'circle-free', and 1 if it is not. If Turing's thesis is accepted, that the existence of a *method* for solving such a problem means the existence of a machine (or an instruction-tape for the universal machine U) that will solve it, it follows that the discovery of a process for discriminating between circle-free and other tapes is an insoluble problem, in an absolute and inescapable sense. From this basic insoluble problem it was not difficult to infer that the Hilbert programme of finding a decision method for the axiomatic system, $\mathcal{Z}$, of elementary number-theory, is also impossible.

In the application he had principally in mind, namely, the breaking down of the Hilbert programme, Turing was unluckily anticipated by a few months by Church, who proved the same result by means of his 'λ-calculus'. An offprint arrived in Cambridge just as Turing was ready to send off his manuscript. But it was soon realized that Turing's 'machine' had a significance going far beyond this particular application. It was shown by Turing (1937*b*) and others, that the definitions of 'general recursive' (by Gödel in 1931 and Kleene in 1935), 'λ-definable' (by Church in 1936) and 'computable' (Turing 1937*a*) have exactly the same scope, a fact which greatly strengthened the belief that they describe a fundamentally important body of functions. Turing's treatment has the merit of making a particularly convincing case for the acceptance of these and no other processes, as genuinely constructive; and it turned out to be well adapted for use in finding other insoluble problems, e.g. in the theory of groups and semi-groups.

Turing's other major contribution to this part of mathematical logic, the paper (1939) on systems of logic based on ordinals, has received less attention than (1937*a*), perhaps owing to its difficulty. The method of Gödel for constructing an undecidable sentence in any finitely based logic, L, i.e. a sentence expressible, but neither provable nor disprovable, in L, has led to the consideration of infinite families of 'logics', L_a, one for each ordinal a, where L_{a+1} is formed from L_a by the adjunction as an axiom of a sentence undecidable for L_a, if such exist, and L_a for limit ordinals a has as 'provable formulae', the union of the sets P_β ($\beta < a$), where P_β is the set of provable formulae in L_β. The process must terminate for some $\gamma < \omega_1$, since the total set of formulae (which does not change) is countable. This procedure opens up the possibility of finding a logic that is complete, without violating Gödel's principle, since

L_a may not be finitely based if a is a limit ordinal. Rosser investigated this possibility in 1937, using the 'classical' non-constructive theory of ordinals. Turing took up the proposal, but with the proviso that, although some non-constructive steps must be made if a complete logic is to be attained, a strict watch should be kept on them. He first introduced a new theory of constructive ordinals, or rather of formulae (of Church's λ-calculus) representing ordinals; and he showed that the problem of deciding whether a formula represents an ordinal (in a plausible sense) is insoluble, in the sense of his earlier paper. A formula $\mathbf{L}$ of the λ-calculus is a *logic* if it gives a means of establishing the truth of number-theoretic theorems; formally, if $\mathbf{L(A)}$ conv. $\mathbf{2}$ implies that $\mathbf{A(n)}$ conv. $\mathbf{2}$ for each $\mathbf{n}$ representing a natural number. The *extent* of $\mathbf{L}$ is the set of $\mathbf{A}$'s such that $\mathbf{L(A)}$ conv. $\mathbf{2}$, i.e. roughly speaking, the set of $\mathbf{A}$'s for which $\mathbf{L}$ proves $\mathbf{A(n)}$ true for all $\mathbf{n}$. An *ordinal logic* is now defined to be a formula Λ, such that $\Lambda(\Omega)$ is a logic whenever Ω represents an ordinal; and Λ is *complete* if every number-theoretic theorem that is true, is probable in $\Lambda(\Omega)$ for some Ω, i.e. if given $\mathbf{A}$ such that $\mathbf{A(n)}$ conv. $\mathbf{2}$ for each $\mathbf{n}$ representing a natural number, $\Lambda(\Omega,\mathbf{A})$ conv. $\mathbf{2}$ for some Ω (depending on $\mathbf{A}$). It is next shown, by an example, that formulae Ω_1, Ω_2 may represent the same ordinal, but yet make $\Lambda(\Omega_1)$ and $\Lambda(\Omega_2)$ different logics, in the sense that they have different extents. An ordinal logic for which this cannot happen is *invariant*. It is only in invariant logics that the 'depth' of a theorem can be measured by the size of the ordinal required for its proof. The main theorems of the paper state (1) that complete ordinal logics and invariant ordinal logics exist, (2) that no complete and invariant ordinal logic exists.

This paper is full of interesting suggestions and ideas. In §4 Turing considers, as a system with the minimal departure from constructiveness, one in which number-theoretic problems of some class are arbitrarily assumed to be soluble: as he puts it, 'Let us suppose that we are supplied with some unspecified means of solving number-theoretic problems; a kind of oracle, as it were.' The availability of the oracle is the 'infinite' ingredient necessary to escape the Gödel principle. It also obviously resembles the stages in the construction of a proof by a mathematician where he 'has an idea', as distinct from making mechanical use of a method. The discussion of this and related matters in §11 ('The purpose of ordinal logics') throws much light on Turing's views on the place of intuition in mathematical proof. In the final rather difficult §12 the idea adumbrated by Hilbert in 1922 of recursive definitions of order-types other than ω received its first detailed exposition.

Besides these two pioneering works, and the papers (1937*b*, *c*), arising directly out of them, Turing published four papers of predominantly logical interest. (A) The paper (1942*a*), with M. H. A. Newman, on a formal question in Church's theory of types. (B) A 'practical form of type-theory' (1948*b*) is intended to give Russell's theory of types a form that could be used in ordinary mathematics. Since the more flexible Zermelo–von Neumann set-theory has been generally preferred to type-theory by mathematicians, this paper has received little attention. It contains a number of interesting ideas,

in particular a definition of 'equivalence' between logical systems (p. 89). (C) The use of dots as brackets (1942*b*), an elaborate discussion of punctuation in symbolic logic. Finally, (D) contains the proof (1950*a*) of the insolubility of the word-problem for semi-groups with cancellation. A finitely generated semi-group *without* cancellation is determined by choosing a finite set of pairs of words, (A_i, B_i) $(i=1, \ldots k)$ of some alphabet, and declaring two words to be 'equivalent' if they can be proved so by the use of the equations $PA_iQ = PB_iQ$, where P and Q can be arbitrary words (possibly empty). The word-problem for such a semi-group is to find a process which will decide whether or not two given words are equivalent. The insolubility of this problem can be brought into relation with the fundamental insoluble machine-tape problem. The table of a computing machine states, for each configuration, what is the configuration that follows it. Since a configuration can be denoted by a 'word', in letters representing the internal configurations and tape-symbols, this table gives a set of pairs of words which, when suitably modified, determine a semi-group with insoluble word problem. So much was proved by E. L. Post in 1947. The question becomes much more difficult if the semi-group is required to satisfy the cancellation laws, '$AC = BC$ implies $A = B$' and '$CA = CB$ implies $A = B$', since now a condition is imposed on account of its mathematical interest, and not because it arises naturally from the machine interpretation. This was the step taken by Turing in (1950*a*). (For a helpful discussion and analysis of this difficult paper see the long review by W. W. Boone, *J. Symbolic Logic*, **17** (1952) 74.)

2. *Three mathematical papers*

Shortly before the war Turing made his only contributions to mathematics proper.

The paper 1938*a* contains an interesting theorem on the approximation of Lie groups by finite groups: if a (connected) Lie group, L, can for arbitrary $\varepsilon > 0$ be ε-approximated by a finite group whose multiplication law is an ε-approximation to that of L, in the sense that the two products of any two elements are within ε of each other, then L must be both compact and abelian. The theory of representations of topological groups is used to apply Jordan's theorem on the abelian invariant subgroups of finite groups of linear transformations.

Paper (1938*b*) lies in the domain of classical group theory. Results of R. Baer on the extensions of a group are re-proved by a more unified and simpler method.

Paper (1943)—submitted in 1939, but delayed four years by war-time difficulties—shows that Turing's interest in practical computing goes back at least to this time. A method is given for the calculation of the Riemann zeta-function, suitable for values of t in a range not well covered by the previous work of Siegel and Titchmarsh. The paper is a straightforward but highly skilled piece of classical analysis. (The post-war paper (1953*a*) describes an

attempt to apply a modified form of this process, which failed owing to machine trouble.)

3. *Computing machines*

Apart from the practical 'programmer's handbook', only two published papers (1948*a* and 1950*b*) resulted from Turing's work on machines. When binary fractions of fixed length are used (as they must be on a computing machine) for calculations involving a very large number of multiplications and divisions, the necessary rounding-off of exact products introduces cumulative errors, which gradually consume the trustworthy digits as the computation proceeds. The paper (1948*a*) investigates questions of the following type: how many figures of the answer are trustworthy if k figures are retained in solving n linear equations in n unknowns? The answer depends on the method of solution, and a number of different ones are considered. In particular it is shown that the ordinary method of successive elimination of the variables does not lead to the very large errors that had been predicted, save in exceptional cases which can be specified.

The other paper (1950*b*) arising out of his interest in computing machines is of a very different nature. This paper, on computing machines and intelligence, contains Turing's views on some questions about which he had thought a great deal. Here he elaborates his notion of an 'examination' to test machines against men, and he examines systematically a series of arguments commonly put forward against the view that machines might be said to think. Since the paper is easily accessible and highly readable, it would be pointless to summarize it. The conversational style allows the natural clarity of Turing's thought to prevail, and the paper is a masterpiece of clear and vivid exposition.

The proposals (1953*b*) for making a computing machine play chess are amusing, and did in fact produce a defence lasting 30 moves when the method was tried against a weak player; but it is possible that Turing underestimated the gap that separates combinatory from position play.

4. *Chemical theory of morphogenesis*

For the following account of Turing's final work I am indebted to Dr N. E. Hoskin, who with Dr B. Richards is preparing an edition of the material for publication.

The work falls into two parts. In the first part, published (1952) in his lifetime, he set out to show that the phenomena of morphogenesis (growth and form of living things) could be explained by consideration of a system of chemical substances whose concentrations varied only by means of chemical reactions, and by diffusion through the containing medium. If these substances are considered as form-producers (or 'morphogens' as Turing called them) they may be adequate to determine the formation and growth of an organism, if they result in localized accumulations of form-producing substances. According to Turing the laws of physical chemistry are sufficient to

account for many of the facts of morphogenesis (a view similar to that expressed by D'Arcy Thompson in *Growth and form*).

Turing arrived at differential equations of the form

$$\frac{\partial X_i}{\partial t} = f_i(X_1,...,\ X_n) + \mu \nabla^2 X_i, \ (i = 1,...,n)$$

for n different morphogens in continuous tissue; where f_i is the reaction function giving the rate of growth of X_i, and $\nabla^2 X_i$ is the rate of diffusion of X_i. He also considered the corresponding equations for a set of discrete cells. The function f_i involves the concentrations, and in his 1952 paper Turing considered the X_i's as variations from a homogeneous equilibrium. If, then, there are only small departures from equilibrium, it is permissible to linearize the f_i's, and so linearize the differential equations. In this way he was able to arrive at the conditions governing the onset of instability. Assuming initially a state of homogeneous equilibrium disturbed by random disturbances at $t = 0$, he discussed the various forms instability could take, on a continuous ring of tissue. Of the forms discussed the most important was that which eventually reached a pattern of stationary waves. The botanical situation corresponding to this would be an accumulation of the relevant morphogen in several evenly distributed regions around the ring, and would result in the main growth taking place at these points. (The examples cited are the tentacles of *Hydra*, and whorled leaves.) He also tested the theory by obtaining numerical solutions of the equations, using the electronic computer at Manchester. In the numerical example, in which two morphogens were supposed to be present in a ring of twenty cells, he found that a three or four lobed pattern would result. In other examples he found two-dimensional patterns, suggestive of dappling; and a system on a sphere gave results indicative of gastrulation. He also suggested that stationary waves in two dimensions could account for the phenomena of phyllotaxis.

In his later work (as yet unpublished) he considered quadratic terms in the reaction functions in order to take account of larger departures from the state of homogeneous equilibrium. He was attempting to solve the equations in two dimensions on the computer at the time of his death. The work is in existence, but unfortunately is in a form that makes it extremely difficult to discover the results he obtained. However, B. Richards, using the same equations, investigated the problem in the case where the organism forms a spherical shell and also obtained numerical results on the computer. These were compared with the structure of *Radiolaria*, which have spikes on a basic spherical shell, and the agreement was strikingly good. The rest of this part of Turing's work is incomplete, and little else can be obtained from it. However, from Richards's results it seems that consideration of quadratic terms is sufficient to determine practical solutions, whereas linear terms are really only sufficient to discuss the onset of instability.

The second part of the work is a mathematical discussion of the geometry of phyllotaxis (i.e. of mature botanical structures). Turing discussed many

ways of classifying phyllotaxis patterns and suggested various parameters by which a phyllotactic lattice may be described. In particular, he showed that if a phyllotactic system is Fibonacci in character, it will change, if at all, to a system which has also Fibonacci character. This is in accordance with observation. However, most of this section was intended merely as a description preparatory to his morphogenetic theory, to account for the facts of phyllotaxis; and it is clear that Turing did not intend it to stand alone.

The wide range of Turing's work and interests have made the writer of this notice more than ordinarily dependent on the help of others. Among many who have given valuable information I wish to thank particularly Mr R. Gandy, Mr J. H. Wilkinson, Dr B. Richards and Dr N. E. Hoskin; and Mrs Turing, Alan Turing's mother, for constant help with biographical material.

M. H. A. NEWMAN

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[A second paper on morphogenesis is being prepared for publication by N. E. Hoskin and B. Richards, based on work left by Turing.]

* Received four years earlier (7 March 1939).