

Turing's Pre-War Analog Computers: The Fatherhood of the Modern Computer Revisited

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Alan Turing has often been praised as the foremost figure in the historical process that led to the rise of the modern electronic computer. Particular attention has been devoted to the purported connection between a Universal Turing Machine (UTM) as introduced in Turing's article of 1936 [27] and the design and implementation in the mid-1940s of the first stored-program computers (with particular emphasis on the respective proposals both of von Neumann for the ED-VAC [30] and of Turing himself for the ACE [26]).

In some recent accounts, von Neumann's and Turing's proposals (and the machines built on them) are unambiguously described as direct implementations of a UTM as defined in 1936. Most noticeable in this regard are the writings of Jack Copeland and his collaborators, as stated in the following example:

What Turing described in 1936 was not an abstract mathematical notion but a solid three-dimensional machine (containing, as he said, wheels, levers, and paper tape); and the cardinal problem in electronic computing's pioneering years, taken on by both 'Proposed Electronic Calculator' and the 'First Draft', was just this: How best to build a practical electronic form of the universal Turing machine? [9, p. 73]

Similar is the following statement by Andrew Hodges:

[The] essential point of the stored-program computer is that it is built to implement a logical idea, Turing's idea: the Universal Turing machine of 1936. [18]

This statement is of particular interest because, in his authoritative biography of Turing, Hodges typically follows a much more nuanced and careful approach to this entire issue. For instance, when referring to a mocking 1936 comment by David Champernowne, a friend of Turing, to the effect that the universal machine would require the Albert Hall to house its construction, Hodges commented that this "was fair comment on Alan's design in *Computable Numbers* for

if he had any thoughts of making it a practical proposition they did not show in the paper." [21, p. 139] Or, even more cautiously, in the following quotation:

Did [Turing] think in terms of constructing a universal machine at this stage? There is not a shred of direct evidence, nor was the design as described in his paper in any way influenced by practical considerations. ... My own belief is that the 'interest' [in building an actual machine] may have been at the back of his mind all the time after 1936, and quite possibly motivated some of his eagerness to learn about engineering techniques. But as he never said or wrote anything to this effect, the question must be left to tantalize the imagination. [21, p. 685–686]

Discussions of this issue tend to be based on retrospective accounts, and sometimes even on hearsay. The most-often quoted one comes from Max Newmann. Newmann had been Turing's teacher and mentor back in the early Cambridge days and, later on, became a leading figure in the rise of the modern electronic computer, sometimes collaborating with Turing. In an obituary published in 1954, he wrote:

The description that [Turing] gave of a 'universal' computing machine was entirely theoretical in purpose, but Turing's strong interest in all kinds of practical experiment made him even then interested in the possibility of actually constructing a machine on these lines. [6, pp. 15-16]

This and other similar testimonies have been repeatedly cited as solid historical evidence but they are invariably vague and unsupported.¹ Similar is the case with the anecdotes about the purported early influence of Turing's paper on von Neumann (See, e.g., [21, p. 184, p. 382]).

The present article is intended as a further contribution to the historical ongoing debates about the actual role of Turing in the history of the modern electronic computer and, in particular, about the putative connection between the UTM and the stored-program computer. I contend that in order to attain a complete and balanced historical picture one must explicitly abandon the idea of a straightforward (let alone necessary) transition from the mathematical idea of 1936 to the physical machine (or even the design of that machine) in

¹Newman repeated this claim again in an often cited oral interview of 1976, but curiously, in 1955 he wrote a memoir on Turing for the Royal Society where this point was not mentioned at all. See [21, p. 686].

1945. More specifically, by exploring the details of Turing’s pre-war involvement with various fields of mathematics both at Cambridge and Princeton, and with the actual construction of two calculating machines, I claim that to the extent that early stored-program computers of the mid-forties can be seen as physically embodying ideas discussed in ‘Computable Numbers’, this is mostly a result of hindsight and it says little about Turing’s ideas before the war.

The purported connection that I call into question involves, in the first place, a *technical* claim, namely that the UTM as defined in 1936 comprises a representative mathematical model of the stored-program electronic computers of the late forties. Turing in 1947, for example, stated that digital computing machines such as the ACE “are in fact practical versions of the universal machine.” [6, p. 383] But the full validity of this technical claim is debatable in various ways. For one thing, a stored program machine is only one way to construct a practical realization of a UTM. For another thing, even when the connection was mentioned in relation with the new machines, reference was to re-cast versions of Turing ideas rather than to the original ones. [12]

Notoriously, neither von Neumann nor Turing himself even suggested this connection back in 1945, in their original proposals. In a brief note apparently drafted in 1945 while working on his proposal, Turing implicitly clarified that the original context of the ideas of 1936 could not allow for thinking about a physical calculator as now proposed on the basis of electronic components. He did not mention the issue of instructions stored as data, which might be seen, at least retrospectively, as connecting the current design with the idea of a UTM. Rather, Turing referred only to the issue of accessing the data in reasonable time:

In ‘Computable numbers’ it was assumed that all the stored material was arranged linearly, so that in effect the accessibility time was directly proportional to the amount of material stored, being essentially the digit time multiplied by the number of digits stored. This was the essential reason why the arrangement in ‘Computable numbers’ could not be taken over as it stood to give a practical form of machine. [7, p. 456]

But beyond the questionable parallel between a UTM and a stored-program machine, there are more purely *historical questions* that require clarifications. Of particular interest is the *actual, direct* influence of Turing’s paper on von Neumann at the time when the latter wrote his famous “First Draft”. Some authors have recently approached this issue and have shown (convincingly in my opinion) that, to the extent that von Neumann (or even Turing himself) actually took inspiration from Turing’s 1936 paper when engaged in the design of a stored-program computer, these ideas provided at most additional input (arguably not the decisive one) that was incorporated to a broader, complex array of (mostly engineering and only partly mathematical) considerations (see, e.g., [12, 15, 16]).² To what has been said in works such as these, I will only add some specific remarks in this article. But I think that my analysis, by focusing on the earlier part of the story, naturally connects to the views expressed therein, and gives them further credence.

²It should be remarked that the term ‘stored-program’ was introduced in 1949 by IBM engineers working at Poughkeepsie. See [11]

Considerations of space require that my analysis be brief and at times schematic. I do not explore the important issue of contemporary developments in different national settings. [3, pp. 379–524] I limit myself to Turing’s work before being recruited to Bletchley Park, and I do so by relying on a variety of already published, mostly well-known, primary sources. I argue that the ‘machines’ that Turing discussed at the time were purely mathematical constructs. They were not conceived as possible blueprints for building physical calculators. Moreover, I claim the very idea of a modern computer in the sense of either von Neumann’s “First Draft” or of Turing’s “Proposed Electronic Calculator” was in 1936 not only beyond the scope of Turing’s capabilities, but also of his concerns. This is true in the obvious (yet crucial) sense that the specific electronic technology that would allow their construction was then beyond Turing’s horizon, but it is also true in the less obvious sense of the question what should an automatic calculating device be in the first place.

Turing did become involved during this time in the construction of two actual devices and in both cases the idea of a UTM was of no use, provided no inspiration and was not even remotely mentioned or hinted at. Neither did Turing suggest that the right approach to building a real computing machine would be along the lines of the UTM, and that he would not pursue this direction just for reasons related to technical limitations or to lack of time.

1. TURING’S COMPUTERS

Let us start with the 1936 paper itself. Beyond a superficial appearance, there is nothing in the original text of *Computable Numbers* that may indicate that Turing was referring to, or had in mind, actual physical devices as part of his analysis. The ‘computers’ that he referred to in the article are humans who calculate. The aim was to “construct a machine to do the work of this computer”, and this is what his famous ‘machines’ are meant to do. Clearly, ‘construct’ was not intended in this text as ‘physically construct’, just as it was not intended there when Turing speaks about ‘constructing’ a proof “by the methods of Hilbert and Bernays”, or ‘constructing’ a number β about which he asks whether or not it is ‘computable’.

The non-physical spirit of Turing’s conception of the ‘machines’ is highlighted by specific comments that do involve what could be taken on first sight to mean physical components such as ink or a square in the (infinite) paper ribbon, but which are actually treated as truly abstract entities. Thus, for instance, when he explains the assumption that the number of symbols which may be printed is finite. The reason for this assumption is that otherwise we would have “symbols that differ from each other to an arbitrarily small extent”. A footnote explaining this comment leaves no doubt that, in spite of the wording, Turing is thinking here not as an engineer, but purely as a mathematician analyzing the situation with conceptual tools taken from measure theory and topology:

If we regard a symbol as literally printed on a square we may suppose that the square is $0 \leq x \leq 1, 0 \leq y \leq 1$. The symbol is defined as a set of points in this square, viz. the set occupied by printer’s ink. If these sets are restricted to be measurable, we can define the “distance” between two symbols as the cost of transforming

one symbol into the other if the cost of moving unit area of printer's ink unit distance is unity, and there is an infinite supply of ink at $x = 2$, $y = 0$. With this topology, the symbols form a conditionally compact space. [27, p. 249]

Some time around May of 1936 Turing prepared a two-page French summary of *Computable Numbers*. This time he did not mention the human computers, and he directly stated that a number may be called 'computable' if its decimals can be written by a machine. In describing what he meant by a machine, he explicitly characterized the various configurations of which the machine is 'susceptible' as different arrangements of "the levers, the wheels, etc."³ But even when he used this suggestive wording, it is clear that he was speaking figuratively, and that the machines in question were to his mind purely mathematical entities. We can see this from what he writes further:

- A real 'computing machine' should be able to write as many digits as one wishes,
- a machine M is called 'malicious' (*méchant*) [what in English he called 'circular'] if there is a number N such that M will never write N digits,
- an application of Cantor's diagonal argument proves that there exists no machine that, if provided with a description of an arbitrary machine M , can decide if M is malicious.

It would make little sense to state that by appealing to this abstract argument, used to tackle situations where infinite sets are involved, Turing intended to say something about an actual physical device.

We can take the analysis one step forward by considering Turing's ideas in the context of the well-known contemporary debates of the mid-1930s. They involved other logicians who came up with their own attempts to provide rigorous mathematical formulations of ideas related to the general notion of 'effective computability' or 'mechanical procedure'. Gödel, Church, Kleene, Bernays, and Post are among the most prominent names associated with this 'period of confluence'. Their motivations, the specific problems they were addressing, and the approach they followed, were slightly different in each case, and for lack of space I cannot delve into them in any detail (see, e.g., [13, 22]). For all of them, the search for *mathematically precise concepts*, corresponding to what were then *informal ideas*, only vaguely understood, was crucial.

Turing was the first to introduce into this kind of discourse the word 'machine'. The more specific term 'Turing Machine' was coined by Church in a famous review written in 1937. Church, as is well known, had completed at roughly the same time—but following a rather different approach—his own contribution to solving Hilbert's *Entscheidungsproblem*, which was at the focus of Turing's paper. After becoming aware of this, Turing sent his paper for publication in August 1936, with an appendix proving the equivalence of both approaches and of the ensuing results. In the review, Church described the 'Turing Machines' as follows:

³<http://www.turingarchive.org/browse.php/K/4>. In the above quotation of [9], Copeland and Sommaruga refer to this French summary as evidence for their statement about a "three-dimensional machine".

[Turing] proposes as a criterion that an infinite sequence of digits 0 and 1 be "computable" that it shall be possible to devise a computing machine, occupying a finite space and with working parts of finite size, which will write down the sequence to any desired number of terms if allowed to run for a sufficiently long time. As a matter of convenience, certain further restrictions are imposed on the character of the machine, but these are of such a nature as obviously to cause no loss of generality—in particular, a human calculator, provided with pencil and paper and explicit instructions, can be regarded as a kind of Turing machine. [4]

Notice that Church used the term 'computing machine' to mean *any* calculating machine of finite size, rather than the specific kind of 'machines' introduced by Turing. The latter are then characterized by further restrictions, and Turing's human calculator becomes here a particular example of the Turing machine. This goes just the opposite way from the direction Turing followed in his original formulation. Moreover, as Hodges [20, p. 246] has indicated, Turing had not referred to machines of finite size as Church did here, and he certainly did not define computability in terms of the alleged power of finite machines. Such machines would eventually repeat themselves, and Turing had attempted precisely to show how a machine with finite specifications would not be constrained to do so. The finiteness of Turing's machines concerned only the number of configurations, but the tape, for instance, could *not* be limited. So, we see that Church's account somewhat obscured rather than clarified Turing's powerful, original point of view.

Turing, however, never seems to have explicitly reacted negatively to Church's characterization. Neither did he react to similar remarks by Gödel, who characterized Turing's work in the 1930s as a general analysis of arbitrary machines.[22] It is likely that Turing did not consider such accounts of his ideas as totally unreasonable. But, as Hodges further suggested [20, p. 247], it is likely that the participants in this discourse about computability were using the word 'machine' in a loose manner and without qualifications to signify 'mechanical processes' in general. The machines and the mechanical procedures they referred to were conceived as part of the metamathematical attempt to provide the rigorous mathematical characterization of the informal idea of computing rather than suggesting, in any way, some practical indication on how to build a physical machine.

Yet another contemporary account emphasizes from a different direction the purely mathematical character of Turing's view of his machines. This one is by Alister Watson. Watson was, like Turing, fellow at King's College, and he was also the person who introduced Turing to Wittgenstein in the summer of 1937. What interests us here is Watson's description of Turing's machines in an article written in 1938.

Turing's theory of computable numbers is essentially that of mathematical expressions, but he has put it in a rather striking way in terms of machines, which would calculate decimals in accordance with rules which correspond to different mathematical expressions for sequences of this kind. He shows how each such machine can be given a number, different for each machine, and

so concludes that the machines and therefore the numbers calculated by them form an enumerable set. Although we can give every machine a number, it is impossible to give a mechanical method by which we can ascertain whether any particular machine is really [circle-free] . . . [31, pp. 448-449]

2. TURING'S THESIS

Invited to take his PhD under the direction of Church, Turing worked at the Institute for Advanced Study in Princeton from September 1936 to July 1938. His dissertation provides further insight into the relationship between his 'machines' and any thoughts he may have had about building an actual physical calculator.

A main innovation of the thesis is the idea of an 'oracle', that "cannot be a machine" and which, by definition, involves "some unspecified means of solving number theoretic problems". With the help of the oracle, Turing said, "we could form a new kind of machine (call them *o*-machines), having as one of its fundamental processes that of solving a given number theoretic problem."

Turing used here the term "number theoretic problem" with a precise meaning, namely, problems involving statements of the form "for all . . . , there exists . . .". Turing proposed the metamathematical task of establishing the completeness of this kind of problems. The twin prime conjecture and the statement of Fermat's last theorem fall within the scope of such problems. But of special importance for Turing was the fact that the Riemann Hypothesis was also a number theoretic problem in his sense. As Solomon Feferman has indicated, [14, p. 6] it is not really clear why Turing concentrated here specifically on this kind of statements rather than considering arithmetical statements in general. After all there are other important problems, such as the finiteness of the number of solutions of a diophantine equation or the statement of Waring's problem, that do not fit into this definition.

At this stage Turing was already involved in the design of two actual calculating devices, as we will see below. And yet, even more than in *Computable Numbers*, there is nothing in the way that 'machines' are referred to in Turing's thesis that may be taken to suggest the idea of building an actual device. Much less does the text suggest that a UTM should be taken as the most appropriate basis for building some kind of 'general purpose', or 'stored-program' calculator. On the other hand, since the 'oracle', by its very nature, cannot be a machine in the restricted sense of the Turing machine, it brings to the fore the idea of various possible ways to conceive appropriate models of addressing different mathematical situations. So, in exploring the capabilities of the *o*-machines, Turing actually meant to explore aspects of mathematical proof and of calculation that would not be covered by the machine as defined in 1936.

3. TURING'S PRINCETON

Turing's encounter with von Neumann at Princeton is one of the topics that arises repeatedly in texts that argue for the connection between a UTM and the stored-program computer. A most explicit example of this appears in the following passage, cited from a text of Jack Copeland and Diane Proudfoot:

John von Neumann shared Turing's dream of build-

ing a universal stored-program computing machine. Von Neumann had learned of the universal Turing machine before the war—he and Turing came to know each other during 1936–1938, when both were at Princeton University. [8]

Indeed the range of the mutual mathematical interests of these two bright men was very broad. Von Neumann in the 1920s had been a leading figure in the Hilbert circle and Hilbert's collaborator in matters related to the foundations of mathematics. In 1933 he became professor of mathematics at the IAS. Turing met him in the summer of 1935 in Cambridge, when von Neumann lectured there on 'almost periodic functions'. This was a topic of interest to Turing at the time and he most certainly attended the course.

Given the later prominence of both Turing and von Neumann in the story of the modern computer, it may seem natural to assume that their encounter in Princeton was a period of intense intellectual interchange, particularly around the possibility of building calculating machines. A closer look at the evidence, however, tells a completely different story of the encounter and of its relevance to our story. Take for example this passage from Turing's letter written in October 6, 1936, soon after his arrival in Princeton:

The mathematics department here comes fully up to expectations. There is a great number of the most distinguished mathematicians here. J.v.Neumann, Weyl, Courant, Hardy, Einstein, Lefschetz, as well as hosts of smaller fry. Unfortunately there are not nearly so many logic people here as last year. Church is here of course, but Gödel, Kleene, Rosser and Bernays . . . have left. I don't think I mind very much missing any of these except Gödel. [6, p. 127]

Turing did not include von Neumann among the "logic people", and with good reason. Right after becoming aware of Gödel's results in 1930, von Neumann definitely abandoned his previous, very active and important involvement with the foundations of mathematics. [1, pp. 8-16] The prominent mathematicians listed in Turing's letter showed little interest in the newcomer from Cambridge and in his work on logic. Von Neumann was no exception, nor was Hardy, who was on visit at the IAS. In fact, it is fair to say that Church was Turing's only real interlocutor on logic while at Princeton.

Both Turing and Church were far from the overly extroverted style of von Neumann and all evidence indicates that there was no personal or friendly relationship with him. We do know that Church's few active attempts to make Turing's work better known in Princeton were not particularly successful. Shortly before *Computable Numbers* was about to be published, Church urged Turing to deliver a talk before the distinguished local mathematical community. Obviously, Turing was thrilled about the opportunity and thought it might bring his work to greater attention. However, it all ended up in disappointment, as we read in one of his letters:

There was rather bad attendance at the Maths Club for my lecture on Dec.2. One should have a reputation if one hopes to be listened to.[6, p.130]

Turing was also disappointed by the rather limited reaction—besides Church's review essay—aroused by the publication of his paper at the end of 1936. We know that only

two persons requested off-prints. Even Hermann Weyl, who had been a most prominent member of Hilbert’s inner circle and was a main figure in the late-1920s debates around the Hilbert program, made not a single remark about the paper. Naturally, Turing was particularly disappointed by Weyl’s lack of reaction [21, p. 158]. And it seems that he did not even expect von Neumann to react in any way to his paper. To be sure, besides the letter quoted above, von Neumann is not mentioned in any of the letters that Turing wrote from Princeton in 1936–37, to either his mother or to his teacher, Philip Hall.

On April 1938, von Neumann approached his younger colleague to offer him a job as assistant. Turing turned down the offer. His fellowship at Cambridge had been just renewed, and he was not eager to remain in the USA anyway. These may have been the main reasons for Turing’s decision. But what about von Neumann’s motivations for approaching Turing? Hodges [21, pp. 183–184] suggests, that by this time von Neumann “was aware of *Computable Numbers*, even if he had not been a year earlier.” This is likely, though there is no direct evidence for it. But what is more than evident is that the offer had nothing to do with a direct interest in Turing’s work on computability and logic, either as developed in the now famous article of 1936 or as then pursued in his PhD dissertation [28].

Indeed, back in June 1937, von Neumann had written a letter of recommendation on behalf of Turing for the Procter Fellowship and there he indicated that Turing “had done good work in branches of mathematics in which I am interested, namely: theory of almost periodic functions and theory of continuous groups.” Von Neumann, let me emphasize once again, had by then completely abandoned his interest in logic and there is no indication that at the time (and indeed anytime before he became involved in the war effort) he had in any way started to think about computing machines or even about mathematical topics related to massive calculations. [1, pp. 25–34]

If, as Copeland and Proudfoot emphasize, “von Neumann had learned of the universal Turing machine before the war”, there is no indication that he devoted special attention to it. While obviously “he and Turing came to know each other . . . at Princeton”, their interaction was rather limited in scope and intensity. There is no indication that the two devoted any time to discussing Turing’s ideas on the topic and much less that they shared (or discussed) during these years anything like a “dream of building a universal stored-program computing machine”.

4. TURING’S ANALOG MACHINE

Between the publication of *Computable Numbers* and the recruitment to Bletchley, Turing was involved in the design and possible construction of two different physical calculators. The first one, in the fall of 1937, was essentially an electric multiplier. This was for Turing an early and rather rudimentary (though no doubt original) foray into machine-based cryptanalysis. On the mathematical side, it appealed to a simple but theretofore not very well known noticed parallel between binary arithmetic and Boolean algebra. On the physical side, it brought to bear the possibility of using, as the basis for a computing device, electromagnetic relays which had been in use for about hundred years in a different context, since the early days of telegraphy.

In his account of this interesting episode, Hodges explains

it as an attempt on the side of Turing, to build a physical embodiment of a specific ‘Turing machine’ —meant to deal with a specific mathematical problem—with the network of relay-operated switches acting as material counterparts of the ‘configurations’:

The idea would be that when a number was presented to the machine, presumably by setting up currents at a series of input terminals, the relays would click open and closed, currents would pass through, and emerge at output terminals, thus in effect ‘writing’ the enciphered number. [21, p.177]

We have no evidence that Turing himself would have described in these terms what he was doing here, or that in his view, with the relay multiplier, “Turing machines’ were coming to life”, as Hodges further remarks. But one way or another, as Hodges himself has clearly stated elsewhere [19, p. 5], “this offbeat amateur engineering was the closest Turing came to developing his ideas of general computation in a practical direction.” This episode underscores, in my view, the unlikelihood of seeing Turing’s 1936 UTM as a blueprint for a counterpart physical device, that would be general-purpose, digital, and, more importantly, stored-program. An engineering project involving such ideas just was well-beyond the horizon of what Turing had in mind at this point.

But all of this becomes even more evident when we take a close look at a second calculating device, which Turing designed and started to build in 1939. This was a device specifically conceived for calculating approximate values of the Riemann zeta-function on its critical line. Hodges’ Turing website, that foremost repository of valuable information for anyone interested in Turing’s life and work, displays the application submitted by Turing to the Royal Society for a grant support for building the device, as well as a blueprint with some details about its technical design.⁴ This is not the place, of course, to give any details about the mathematical significance of the Riemann Hypothesis (RH), of Turing’s overall involvement with this problem, and of his specific contribution to it. This can be found in [2, 17]. But there are some specific points that are highly relevant to our concerns here, and I proceed to discuss them now.

Turing’s interest in RH was sparked shortly after matriculating at King’s College in 1931. There he attended a course by Albert E. Ingham, who in 1932 published an important text on the distribution of prime numbers. At King’s, Turing also befriended Stanley Skewes, who in 1933 made a remarkable contribution to research on RH. Very briefly stated, Skewes calculated an upper bound for the smallest value of x for which $\pi(x) > \text{Li}(x)$. Here $\pi(x)$ represents the number of primes which are smaller than a given integer x , while $\text{Li}(x)$ is the value of the integral $\int_2^x \frac{dx}{\ln x}$. It is well known that RH concerns the question of the nontrivial zeros of the Riemann zeta-function $\zeta(s)$ and its relation to the estimation of the value of the difference between the two above functions. John E. Littlewood had proved in 1912, contrary to the common belief at the time, that the difference $\pi(x) - \text{Li}(x)$ changes sign infinitely many times, both if RH is true or if it is false. Skewes’ proof involved two different values of the upper bound for the respective cases, and both values were amazingly high. For instance, if RH is true, then he

⁴<http://www.turing.org.uk/sources/zetamachine.html>

proved that the smallest number x where this change of sign happens is smaller than $10^{10^{34}}$.

While at Princeton, and during his visit in Cambridge in the summer of 1937, Turing actively pursued research related to RH. Among other things, he improved the value of Skewes' bound, and, more importantly, he improved the existing methods for calculating zeros of $\zeta(s)$. Recently, Edward Ch. Titchmarsh had been involved with such calculations at Oxford and he was able to establish that the first 1041 nontrivial zeros of $\zeta(s)$ all satisfy RH.[24, 25]. Turing's plan of 1939 for building a calculating device is directly connected to Titchmarsh's work.

Titchmarsh's calculations were based on approximation formulas that required massive iterations of addition and multiplication, as well as the use of cosine tables. For "planning and supervising the calculations, which were carried out with Brunsviga, National, and Hollerith machines," Titchmarsh explicitly thanked J.L. Comrie.[25, p. 261] Comrie had since 1929 been 'Secretary of the British Mathematical Tables Committee', and he was the driving force behind a great amount of important projects of scientific table-making conducted in the UK at the time. [10, pp. 242 ff.]

Astronomical tables were of particular importance among such projects, and their preparation involved repetitive summations of circular functions with different frequencies for plotting the positions of planets. Comrie's recent introduction of Hollerith punch-card techniques to scientific table-making [5] signified a remarkable innovation in the field. As it happened, the calculations required in Titchmarsh's approach to calculating zeros of $\zeta(s)$ were quite similar to these. Comrie was clearly the right person to provide the necessary technical assistance.

Typically, Comrie did not actually perform any calculations himself. Rather, he had teams of 'human computers' (mainly women) to do that work. Each computer received data to be worked out, and, with the help of "Brunsviga, National, and Hollerith machines," she would perform a specific task, clearly defined in advance according to the kind of input presented to her. She would then deliver the result to another person in the team who also carried out a respective task, well-defined in advance, again depending on the kind of input at stake. Notice, then, that while Turing's 'machines' of 1936 provided a mathematical model meant to analyze the nature and scope of the calculations that *individual* human computers could and did effectively perform, they can also be taken to describe adequately (and perhaps even better) the model of what *coordinated teams* of human computers were actually doing in Turing's time, when addressing heavy computational tasks that went beyond what an individual could actually achieve.

Now, when Turing in 1939 undertook to perform relatively massive calculations in one of his main mathematical topics of interests other than logic, he was aware that precisely at that time, one of Comrie's teams had recently been involved in the same task, aiding themselves with mechanical calculators. The conceptual similarity between Turing's model and Comrie's activities could not have escaped Turing's mind. And yet, for his own calculations, he did not choose to follow or improve the direction previously pursued by Titchmarsh. Much less did he take a step further towards anything like a UTM. Rather he went into the totally different direction of designing an *analog* machine, specifically conceived for supporting his work on what for him was such an important

mathematical task.

Turing's design relied on a machine built some years earlier for tide prediction at Liverpool. It performed trigonometrical summations based on a combination of pulley wheels, each representing one of the gravitational effects that give rise to tidal phenomena. A thin nickel tape connected the various pulley wheels and 'summed up', as it were, their separate movements. The tidal highs and lows were thereby registered on a chart located at the bottom of the machine.

Turing thought that similar principles would be useful for constructing an apparatus for calculating trigonometric sums that were at the heart of his method for the zeros of $\zeta(s)$. In some cases, he thought, the apparatus would not be accurate enough and then it would be necessary to work out the calculations manually. But he believed that such cases would be extremely rare. He specifically stressed that the apparatus would be so closely analog to the simulated mathematical situation to the extent that he could not think "of any application that would *not* be connected with" $\zeta(s)$! The project, at any rate, was never completed because of the outbreak of war, and no parts of it have survived.

I find it quite interesting that when Hodges describes the project on his website, he speaks about "a special machine to calculate approximate values for the Riemann zeta-function on its critical line." This is not the only place where the term 'special machine', which was not used by Turing, is used in this context (e.g., [2, p. 29],[17, p. 270]). I take it to be a revealing, subtly misleading description of what Turing had in mind in 1939, particularly because the project is often mentioned in conjunction with Turing's later efforts of 1950 to attack the same problem using now a 'general-purpose' electronic computer, the Manchester University Mark I.[29]

My point is that in 1939 Turing approached the actual construction of an apparatus the way he did, not because—for lack of time or resources—he was compelled to make do with it. This was not a 'special', limited or rudimentary version of what for him would be the real thing, namely, a general-purpose, stored-program, digital and electronic computer (presumably being a physical embodiment of a UTM). Rather, it is that the latter alternative was not within the horizon of possible or convenient approaches to be followed. To the contrary, what the evidence shows is that at the time, Turing considered the *analog approach* to be the most *intrinsically* appropriate one for the task at hand.

As a matter of fact, the tremendous success of modern digital computers has negatively affected the way in which the history of analog computers in general has been told. The case of Turing's 1939 project is just one example of this, though seldom mentioned in this context. Analog computers were not only a natural choice in many situations before the war, but even after the emergence of digital computing in the post-war period they were not immediately displaced.[23] And a most remarkable example of this is precisely the Liverpool tide-predicting machine, which remained in use until the 1960s before being superseded by electronic computers.

5. INTUITION AND TURING'S MACHINES

Turing left Cambridge in September 1939 for Bletchley Park. Over the next few years, he would devote all of his intellectual energies to war-related projects, while temporarily setting aside mathematical research in his other fields of interest. By 1945 he would have added important skills, as well as familiarity with electronic valves and with ma-

chines of various kinds, to the already impressive arsenal of knowledge he had brought with him at the time of his recruitment. His activities after 1945—including of course all of his involvement with electronic computers—were deeply influenced by the years at Bletchley.

But over the first months after his recruitment, Turing and Newman continued to correspond and to discuss issues connected to their pre-war activity. This correspondence bears witness to the way in which the two went on to speak about ‘Turing machines’ at a time when Turing had already gained actual experience not only with the two calculating devices that he had been involved with, but also with what he had now started to learn at Bletchley. Remarkably, they stuck to the language of ‘machines’, not in the sense of physical devices, but rather as the relevant, purely mathematical idea on the basis of which they would discuss issues related with the foundations of arithmetic.

In a particularly interesting passage, under the heading “Ingenuity and Intuition”, Turing replied to a previous letter of Newman where the latter seems to have commented on matters related to the Hilbert program:

I think you take a much more radically Hilbertian attitude about mathematics than I do. You say ‘If all this whole formal outfit is not about finding proofs which can be checked on a machine it’s difficult to know what it is about’. [D]o you have in mind that there is (or should be or could be, but has not been actually described anywhere) some fixed machine . . . and that the formal outfit is, as it were about this machine. If you take this attitude . . . there is little more to be said: we simply have to get used to the technique of this machine and resign ourselves to the fact that there are some problems to which we can never get the answer. . . . If you think of various machines I don’t see your difficulty. One imagines different machines allowing different sets of proofs, and by choosing a suitable machine one can approximate ‘truth’ by ‘provability’ better than with a less suitable machine, and can in a sense approximate it as well as you please. The choice of a machine involves intuition, . . . or as [an] alternative one may go straight for the proof and this again requires intuition.[6, p. 215]

The fact that still in 1940, when the classical debates on the foundations of arithmetic had almost totally faded away, Newman and Turing continued their interchanges on such matters, is worthy of attention in itself. But no less interesting is the subtle twist that Turing introduced into this discussion when he mentioned the possibility of having various kinds of machines, according to different kinds of intuitions which are relevant to different mathematical situations. The UTM was not for Turing ‘universal’ in this important sense.

It seems that now—in those few opportunities when he could think about the foundations of mathematics, and about questions of ‘truth’, or ‘provability’—Turing also incorporated new directions such as he had explored in his PhD. This included no doubt the oracle, but also, so it seems, alternatives to the basic ‘machine’ defined in 1936.

6. CONCLUDING REMARKS

I conclude with a final, somewhat conjectural suggestion. By its very nature, Turing’s oracle could not be a standard Turing machine. “Solving a given number theoretic problem” is one of “its fundamental processes”. And the Riemann Hypothesis is one such problem. Now, it seems to me that Turing’s construction of his analog machine and the variety of machines he mentioned in the letter to Newman shed interesting light, retrospectively, on that passing, somewhat unclear comment that Turing advanced in his thesis. From the letter we learn that each mathematical situation calls for the choice of a suitable machine and these choices rely on the right intuition to do so in each case. The UTM had been a highly successful, *specific* choice for dealing with the *Entscheidungsproblem*, but that would not mean—even in principle—that it would provide the model for a physical universal machine, suitable for *all* mathematical tasks. If we may somehow think of these mathematical models as suggesting blueprints for designing physical devices, then an analog machine such as planned by Turing in 1939 would come much closer than anything built along the lines of a UTM to embodying specific, “fundamental processes” associated with a particular number theoretic problem, in the sense suggested in the PhD thesis.

When Turing in 1950 retook the task of calculating zeros of $\zeta(s)$, the sea-changes that had affected the world of automatic computation had rendered all those pre-war considerations obsolete. The natural approach to follow for Turing was now to write a specific program to run in a stored-program machine. But the Mark I, as all other similar machines at the time, were not only stored-program. They were also electronic, large-scale, high-speed, general-purpose and digital. In 1939, all these crucial components of the machines that started to be built in the late 1940s were far beyond the horizon.

7. ACKNOWLEDGMENTS

This article is a somewhat belated elaboration of a talk given at the “Turing in Context II” conference, Brussels, 10-12 October 2012 (see <http://www.dijkstrascry.com/Corry-Post> submitted on 10/09/2013). I want to thank Edgar Daylight for transcribing the talk, for his continued encouragement to publish my ideas on this topic, and for comments on an earlier version of this text. I am also thankful to three anonymous referees for valuable comments that helped prepare an improved final version of the paper.

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