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Wittgenstein's Remarks on Mathematics, Turing, and Computability

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I, Adam William Harwood, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Abstract:

Typically, Wittgenstein is assumed to have been apathetic to the developments in computability theory through the 1930s. Wittgenstein's disparaging remarks about Gödel's incompleteness theorems, and mathematical logic in general, are well documented. It seems safe to assume the same would apply for Turing's work. The chief aim of this thesis is to debunk this picture. I will show that:

a) Wittgenstein read, understood and engaged with Turing's proofs regarding the *Entscheidungsproblem.*
b) Wittgenstein's remarks on this topic are highly perceptive and have pedagogical value, shedding light on Turing's work.
c) Wittgenstein was highly supportive of Turing's work as it manifested Wittgenstein's prevailing approach to mathematics.
d) Adopting a Wittgensteinian approach to Turing's proofs enables us to answer several live problems in the modern literature on computability.

Wittgenstein was notably resistant to Cantor's diagonal proof regarding uncountability, being a finitist and extreme anti-platonist. He was interested, however, in the diagonal method. He made several remarks attempting to adapt the method to work in purely intensional, rule-governed terms. These are unclear and unsuccessful.

Turing's famous diagonal application realised this pursuit. Turing's application draws conclusions from the diagonal procedure without having to posit infinite extensions. Wittgenstein saw this, and made a series of interesting remarks to that effect. He subsequently gave his own (successful) intensional diagonal proof, abstracting from Turing's. He endorsed Turing's proof and reframed it in terms of games to highlight certain features of rules and rule-following.

I then turn to the Church-Turing thesis (CTT). I show how Wittgenstein endorsed the CTT, particularly Turing's rendition of it. Finally, I show how adopting a family-resemblance approach to computability can answer several questions regarding the epistemological status of the CTT today.

Impact Statement:

The chief impact of the thesis will be on its immediate academic environment. This is primarily Wittgenstein scholarship. I hope to have uncovered and explicated some new ideas from Wittgenstein and also provide new criticisms to the secondary literature that might be of further interest.

Some of the work contained herein may have pedagogical value in a broader context. In §2 I run through some of Wittgenstein's (relatively unknown) abstractions from some of Turing and Gödel's mathematical proofs. The original proofs themselves are highly complex and demanding, but Wittgenstein manages to spell them out simply without substantive loss of explanatory power. Shedding light on these passages may be useful for philosophers (or indeed anyone) interested in the staple proofs of recursion theory without a strong mathematical background.

Mathematicians may also find some use in the contents of this thesis. Wittgenstein's mathematical ability, or rather lack thereof, usually discourages a serious consideration of his work in a mathematical context. An underlying theme throughout this thesis is that this is unfair (and indeed unhelpful). My final and most lengthy section argues that a Wittgensteinian approach to the ordinary language associated with the Church-Turing thesis is useful to understanding its status in modern scholarship. If my analysis is correct, then this cements the Church-Turing thesis beyond doubt (and thus the proofs relative to the *Entscheidungsproblem*). This clearly has interesting and desirable implications in mathematics. Whilst I am not naïve enough to think I have presented any kind of breakthrough here, I hope that I have shown that an appeal to the Wittgensteinian approach to mathematics is worth serious consideration. Although my scope here is restricted to Wittgenstein's interest and approach to Turing's work, I am confident that an extension of this to recursion theory *tout court*, modern computability and related mathematical fields would be equally worthwhile.

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I would further like to thank Juliet Floyd. Her first-rate philosophical work and subsequent guidance are mutually responsible for the inception and spirit of this thesis.

My final thanks go to the long-suffering Christopher Holliday and Siân Whitby for rigorously proofreading various drafts of this thesis. Any remaining errors are, of course, my own.

Abbreviations:

The following are abbreviations that refer to works of Wittgenstein and Turing that are frequently cited throughout. Please see Bibliography for full entries.

AWL: *Wittgenstein's Lectures, Cambridge, 1932-1935.*

BB: *The Blue and Brown Books.*

CN: *On Computable Numbers, with an Application to the Entscheidungsproblem.*

LFM: *Wittgenstein's Lectures on the Foundations of Mathematics: Cambridge 1939*

MS + *n*: the *n*-th manuscript in the Wittgenstein *Nachlass.*

PG: *Philosophical Grammar.*

PI: *Philosophical Investigations.*

PR: *Philosophical Remarks.*

RFM: *Remarks on the Foundations of Mathematics.*

RPP: *Remarks on the Philosophy of Psychology.*

TLP: *Tractatus Logico-Philosophicus.*

WVC: *Wittgenstein and the Vienna Circle: Conversations Recorded by Friedrich Waismann.*

Schoolmaster: ‘Suppose x is the number of sheep in the problem’.
Pupil: ‘But sir, suppose x is not the number of sheep’.

[I asked Prof. Wittgenstein was this not a profound philosophical joke, and he said it was.]

——*J.E. Littlewood, A Mathematician’s Miscellany.*

Introduction

The turn of the twentieth century famously saw David Hilbert issue a call to arms for the modern mathematician. This took the form of 23 *Mathematische Probleme* (1900): a smorgasbord of major, then-unsolved problems extending across fields the solutions of which would shape the future of mathematics. The tenth of these called for a *decision procedure* for Diophantine equations. That is: find an algorithm which can take as an input any given polynomial equation with integer coefficients and decide whether that equation has a solution. The idea of *decidability* later generalised: are there procedures that can decide entire formal systems e.g. propositional logic, predicate logic, perhaps even arithmetic?

Questions of this nature formed the core of 'Hilbert's programme', which aimed to rigorously formalise mathematics by formulating it as a set of axioms that had been proved consistent by only finitary methods. Broadly, it was concerned with the limitations and qualities of axiomatic formal systems and *what can be done within them.* At a 1928 international congress Hilbert offered his new questions for the mathematical world with precision: Is mathematics *complete*? Is mathematics *consistent*? Is mathematics *decidable*?

A system is complete iff every sentence of the language of that system is either provable or refutable. A system is consistent iff there is no sentence of that system such that both the sentence and its negation can be derived from the axioms. Finally, and most importantly for our purposes, a system is decidable iff there is some algorithm to determine the provability of any given sentence. At this time, 'algorithm' was understood intuitively as an *effective method*—an *effectively calculable* or *computable* procedure. These give a finite list of instructions, capable of being followed step-by-step without creativity or insight, which guarantee an answer. In the *Tractatus*, Wittgenstein would notably invent an algorithm that demonstrated the decidability of the propositional calculus: the truth table. The truth table can take any sentence of propositional logic and through a simple method show whether that sentence is universally valid in a finite number of steps.

Hilbert thought, indeed hoped, that his questions would return in the affirmative.

In 1930, the young Kurt Gödel announced work that foreshadowed a shattering blow to Hilbert's programme. By 1931, the results were in: Gödel had proved remarkable theorems concerning incompleteness and consistency (cf. Gödel (1931)). Gödel showed that the formal system in *Principia Mathematica*[1]—the quintessential formal system—and other similar systems such as ZF[2], are incomplete assuming they are ω-consistent.[3] Gödel showed that any ω-consistent system capable of modelling a certain amount of arithmetic, whose axioms can be recursively defined[4], contains undecidable sentences—Gödel sentences. These sentences are neither provable nor disprovable within the system. Further, he showed that statements of consistency within a system cannot be proved within that system, if that system is consistent.

This still left Hilbert's *Entscheidungsproblem* [decision problem], which by then had become "one of the leading problems of mathematical logic" (Ramsey 1930, 264). The decidability of fragments of predicate logic had been established, such as a restriction of it to unary predicates and other special cases.[5] However, the *Entscheidungsproblem*—the question of the decidability of the predicate calculus *tout court*—remained open. More specifically, the target was Hilbert's *engere Funktionkalkul*—the restricted functional calculus incorporating propositional and first-order predicate logic (cf. Hilbert & Ackermann (2000)).

No solid answer appeared until 1936, when both Alonzo Church, working from Princeton, and Alan Turing, from Cambridge, independently published answers that proved that the *Entscheidungsproblem* has no solution. Church's proof came first (cf. Church (1936a), Church (1936b)). Usually this would preclude the publication of Turing's work. However, Turing's paper was so original and general that it warranted exposure to, and further development by, the mathematical community. Turing's paper—*On Computable Numbers, with an Application to the Entscheidungsproblem* (CN)—was published in the LMS *Proceedings* in November 1936. Turing was 24.

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[1] Cf. Russell & Whitehead (1910), (1912), (1913).

[2] ZF abbreviates Zermelo-Fraenkel set theory, the orthodox axiomatic system of set theory.

[3] This requirement is stronger than simple consistency; ω-*in*consistency occurs when every formula of a sequence $\phi(0), \phi(1), \ldots \phi(n)$ are provable, and also the formula ~$\forall x\ \phi(x)$.

[4] More on this later.

[5] This is due to the Löwenheim-Behmann theorem; see Boolos et al., (2007, Ch 21) for the proof and related theorems. Other 'special cases' include the Schönfinkel-Bernays-Ramsey class: the fragment, now sometimes called 'effectively propositional', with which Ramsey was concerned in Ramsey (1930).

Turing outlined a logical blueprint for computing machines, now commonly called 'Turing machines'. These, he argued, can compute any result calculable via an algorithm. He showed that there is no Turing machine capable of deciding whether or not any given sentence in the first-order predicate calculus is universally valid. Therefore, if his analysis is correct, there is no effective method that settles the *Entscheidungsproblem.*[6] This work was important not only for its own sake: formulating an acceptable notion of algorithm led directly to an absolute definition of an effectively axiomatisable formal system. This meant that—in conjunction with other developments in the literature[7]—Gödel's proofs could be extended to include any consistent, effectively axiomatisable system capable of modelling a certain amount of arithmetic. It is now common knowledge that Gödel's proofs entail that the *Entscheidungsproblem* has no solution. However, the true scope of Gödel's achievement was not apparent until long after the publication of his proofs.

There is an anecdote about Turing that I like. Sometime during the first months of the Second World War, a discussion arose in a cosy Parisian café between English and Polish cryptographers over which system of measurement and currency was more logical: the (famously chaotic) British imperial system or the European decimal system. Turing characteristically defended the former: the pound sterling, with its composition of 20 shillings divided into 240 pence, uniquely allowed three, four, five, six, or eight persons to precisely split a pub tab with a tip rounding off perfectly to a full pound (Hodges 2014, xxviii).

Turing found the system 'logical' in virtue of its utility—in this case, for its neat divisibility relative to pint pricing. This might well have been a Pavlovian response for Turing. Only months before this, Turing was sat in Cambridge attending a lecture series by Wittgenstein entitled *Lectures on the Foundations of Mathematics* (LFM). Wittgenstein would here attach the purpose of a given calculus to its application: calculi are inventions employed by us to make inferences in *everyday* scenarios.

The *Lectures* are a fascinating read. At times, they give the impression that Wittgenstein and Turing were intellectual enemies. Turing is charged with giving the typical mathematician's response against which Wittgenstein lays

[6] Church's proof used another formalism—the λ-calculus—in a similar way; the class of λ-definable functions coincides with Turing-computable functions.

[7] Notably Rosser's trick, which meant that formal systems need only be consistent, rather than ω-inconsistent for the incompleteness proofs to apply.

his arguments. Ray Monk claims that their approach to the foundations of mathematics at the time of the *Lectures* "could not have been more different" (Monk 1991, 417). The lectures saw Wittgenstein attack, and Turing defend, the importance of mathematical logic. They debated extensively on the nature of contradiction, Turing having to defend the (intuitively obvious) position that contradictions in mathematics are worrying (and indeed that they *matter* at all) (Monk 1991, 421). Turing then stopped attending the lectures: "convinced, no doubt, that if Wittgenstein would not admit a contradiction to be a fatal flaw in mathematics, then there could be no common ground between them" (Monk 1991, 421-422). Despite lengthy discussions on the nature of 'rule' in mathematics, Turing never gave a definition of rules in terms of Turing Machines. Monk does not find this curious: "surely, Turing realised that Wittgenstein would have dismissed such a definition as irrelevant"—his concerns were far more fundamental (Monk 2991, 422).

This picture is, in fact, misleading. I will show that in no way did Wittgenstein find Turing machines 'irrelevant'. Wittgenstein was not hopelessly unaware or unengaged with the developments in recursion theory in the 1930s. Rather, I will show that:

a) Wittgenstein read, understood and engaged with *Computable Numbers.*
b) Wittgenstein's remarks on this topic are highly perceptive and have pedagogical value, shedding light on Turing's work.
c) Wittgenstein was highly supportive of Turing's work as it was indicative of Wittgenstein's prevailing approach to mathematics.
d) Adopting a Wittgensteinian approach to Turing's proofs enables us to answer live problems in the modern literature on computability.

It is true that Wittgenstein and Turing disagreed over several issues, the most famous of which being machine-thinking. Turing famously defended the proposition that machines are capable of thinking, whereas Wittgenstein vehemently opposed it. These differences, however, are contained to the philosophy of mind and related areas. Wittgenstein was *not* opposed to Turing's logico-mathematical work. Rather, he endorsed it thoroughly. I will show that Turing's work is consistent with Wittgenstein's core philosophy of mathematics at the time of *Computable Numbers.* Instead of viewing Turing's work as steeped in linguistic confusion, Wittgenstein reconstructs some of Turing's discoveries in his own framework, and uses them to deduce conclusions concerning his own work on rules.

I consider d) my most interesting and novel claim. The Wittgensteinian approach is typically taken to be gormlessly incommensurable with the practices of mathematical logicians concerned with computability. The fact that adopting a Wittgensteinian approach to computability—especially in modern discourse—can prove fruitful should, I hope, be an exciting development.

This paper is divided into three sections:

§1—Mathematics—gives an excavation of Wittgenstein's philosophy of mathematics relevant to Turing's work. This is largely concerned with Wittgenstein's arguments in Part II of his *Remarks on the Foundations of Mathematics* (RFM). I give an exposition of Wittgenstein's treatment of Cantor's famous diagonalisation method. Understanding Wittgenstein's (quite curious) approach to Cantor's work is paramount for a correct interpretation of his remarks about *Computable Numbers.*

§2—Turing—offers my interpretation of Wittgenstein's interest in *Computable Numbers.* I will show how Wittgenstein understands Turing's proofs in terms of games. He also reconstructs his own version of Turing's application of the diagonal method. He uses this to show an interesting feature of rules and rule-following. My reconstruction of these remarks is largely similar to Juliet Floyd's interpretation, although I hope to go slightly further. I will counter Floyd's argument that Wittgenstein influenced the inception of *Computable Numbers.* Influence, if we are to posit any regarding *Computable Numbers*, flowed only in one direction.

§3—Computability—is concerned with the Church-Turing Thesis. Wittgenstein's makes a pithy remark about Turing machines being *humans.* I show that this is actually an insightful comment on Turing's contribution to the thesis. I argue this in opposition to Stuart Shanker, who erroneously argues that Wittgenstein objected to the thesis. After this, I examine the current status of the thesis moving forward. There is a philosophical debate to be had over the epistemological status of the thesis today. That is, whether it is (undoubtedly) true, provable, proved etc. I will make a case for the truth of the thesis by appeal to the Wittgensteinian notion of family resemblance. Although the truth of the thesis is largely accepted, I think that my account is the only approach that successfully accommodates the historical reception of the thesis.

1 Mathematics—Wittgenstein on Cantor and Foundations

1.1 Cantor

Cantor's diagonalisation method is perhaps the most widely used tool in mathematical logic. This simple technique underpins a host of influential proofs, including Gödel and Turing's. It dates back to 1891, where Cantor used it to prove the uncountability of the set of real numbers ($\mathbb{R}$). He had, in fact, already proved this in 1874 using a different method. This new method, however, was far neater and has since become a staple in mathematical logic. I will briefly run through it. Cantor's original formulation (1891) is not formalised, relying on the informal concept of *Mannigfaltig* (manifold). As such, I will instead (loosely) follow the presentation given in Boolos et al. (2007, Chs 1-2).

A set is enumerable, or countable, iff it can be arranged in some list such that every member corresponds to a natural number, these being 0,1,2,3... That is, a set is enumerable iff it is the image of some function of natural numbers.[8]

All finite sets are clearly enumerable, being the images of partial functions of natural numbers. Take $\{2,19,48\}$:

$$\begin{array}{l} 0 \to 2 \\ 1 \to 19 \\ 2 \to 48 \end{array}$$

So are many infinite sets. Take the positive even numbers:

$$\begin{array}{c} 0 \to 2 \\ 1 \to 4 \\ 2 \to 6 \\ \cdots \end{array}$$

Every even number will eventually appear on the list 2,4,6... Every even number can be associated with a natural number.

[8] A total/(partial) function is a relation between sets which takes arguments from a domain (arguments x of a set X) and maps every/(some) member(s) each to a single element y of a set Y (the range). The image of a function is the subset of the range that is the output of the function (the elements y of Y).

Not all sets can be arranged in this way. For instance, the set of all subsets of natural numbers—the *power set* of $\mathbb{N}$, $\mathcal{P}(\mathbb{N})$—is not enumerable. Assume for reductio, that the members of $\mathcal{P}(\mathbb{N})$ could be laid in the following list (i.e. assume it is enumerable):

$$S_1, S_2, S_3, \ldots$$

where each S_n is some subset of $\mathbb{N}$.

We can always define *another* subset of the natural numbers (hence another element of $\mathcal{P}(\mathbb{N})$)—S_0—which does not appear as any S_n. We simply define S_0 to be the set containing each natural number n iff n is not in S_n. From this definition we know that S_0 cannot be an entry on the list $S_1, S_2, S_3, \ldots$ for it differs with each of these by at least one member. S_0 cannot be any S_n because, by definition, either $n \in S_n$ and $n \notin S_0$, or $n \notin S_n$ and $n \in S_0$. Yet, S_0 is an element of $\mathcal{P}(\mathbb{N})$, being it a subset of $\mathbb{N}$, and so by the assumption *must* appear on the list $S_1, S_2, S_3, \ldots$ Let S_0 be S_m: then, $m \in S_m$ and $m \notin S_0$, or, $m \notin S_m$ and $m \in S_0$. Thus, we have derived a contradiction. Our original assumption is thus false and $\mathcal{P}(\mathbb{N})$ is uncountable (or non-denumerable).

We can make this clearer by interpreting $S_1, S_2, S_3, \ldots$ as functions $s_1, s_2, s_3, \ldots$ which return values of 0 or 1. We say for each S_n that for each natural number n:

$$s_n(n) = \begin{cases} 1 & \text{if } n \in S_n \\ 0 & \text{otherwise} \end{cases}$$

We can plot this spatially as follows:

$s_n(n)$	1	2	3	4	...
s_1	$s_1(1)$	$s_1(2)$	$s_1(3)$	$s_1(4)$	...
s_2	$s_2(1)$	$s_2(2)$	$s_2(3)$	$s_2(4)$	...
s_3	$s_3(1)$	$s_3(2)$	$s_3(3)$	$s_3(4)$	...
s_4	$s_4(1)$	$s_4(2)$	$s_4(3)$	$s_4(4)$	...
...	...	...	...	...	...

This represents an array of 0s and 1s, each dependent on whether the natural number n appears in S_n. For instance, if S_1 happened to be $\{1,3\}$, then s_1 would read horizontally 101000…

We can now represent S_0 in terms of the function s_0. For all *n*:

$$s_0(n) = 1 - s_n(n)$$

This takes the outputs diagonally across the table $(s_1(1), s_2(2), s_3(3), ...)$ and switches them. If $s_1(1) = 0$ then $s_0(1) = 1$, and vice versa and so on.

Now, we know s_0 must appear as a row on the table above. If $\mathcal{P}(\mathbb{N})$ were enumerable, its members could be laid out $S_1, S_2, S_3, ...$ such that each member corresponded to a natural number. By definition, S_0 must appear on the list as it is a set of natural numbers and thus an element of $\mathcal{P}(\mathbb{N})$. S_0 therefore corresponds to s_0 on the table.

However, s_0 cannot appear as any *m*-th row in the list above. If it did, then the following equation would hold:

$$s_m(m) = 1 - s_m(m)$$

This says that 0=1 as $s_m(m)$ returns either 0 or 1.

s_0 must appear, but its doing so results in a contradiction. Therefore, contra our assumption, $\mathcal{P}(\mathbb{N})$ is uncountable.

The aesthetic effect created by altering the values of $s_m(m)$ explains why this is commonly referred to as Cantor's *diagonal* proof, or proof by *diagonalisation.*

From the proof that $\mathcal{P}(\mathbb{N})$ is uncountable, if follows that $\mathbb{R}$ is also uncountable. The real numbers are those numbers that form the continuum: any number that can be represented as a point along an infinite line. These include any quantity that is not imaginary e.g. 0.00001, √2, π, 100.8, etc. In order words, the reals are those numbers that can be represented by an integer followed by a possibly infinite sequence of digits.

If r is a real number $0 < r < 1$, then r has an infinite binary expansion $.x_1x_2x_3...$ where each x_i is either 0 or 1. Some have two expansions i.e. $\frac{1}{2} = 0.1000... = 0.0111...$ If there is a choice, choose the one trailing in 0s rather than 1s. Every set S of natural numbers can be associated to some real number r between 0 and 1—just take the function s corresponding to the subset S of $\mathbb{N}$ to be the sequence of 0s and 1s in the binary expansion of *r*.

Thus, an enumeration of $\mathbb{R}$ would entail an enumeration of $\mathcal{P}(\mathbb{N})$ which we know from above is impossible. Therefore, $\mathbb{R}$ is uncountable.

Uncountability is a feature of the *cardinality* (size) of sets. The cardinality of A is greater than that of B—$|A| > |B|$—iff there is an injection from B into A but no bijection.[9] We know by diagonalisation that any $f\colon \mathbb{N} \to \mathbb{R}$ has real numbers in its range that are not the image of an element from the domain $\mathbb{N}$. Therefore, $|\mathbb{R}| > |\mathbb{N}|$. We denote $|\mathbb{N}|$ by $\aleph_0$. We denote $|\mathcal{P}(\mathbb{N})|$ as $2^{\aleph_0}$.[10] As there is a bijection between $\mathbb{R}$ and $\mathcal{P}(\mathbb{N})$, $|\mathbb{R}| = 2^{\aleph_0}$. By diagonalisation we have shown that $2^{\aleph_0} > \aleph_0$.

Cantor's work gave access to an entirely new branch of mathematics: transfinite arithmetic. The diagonalisation method delivered more still though. For its initial purpose, the method was virtually redundant: as I have already mentioned, Cantor had already proved the uncountability of the real numbers way back in 1874. However, this technique is so simple and elegant that it proved highly transferable.

I would like to analyse the remarks that Wittgenstein devoted to Cantor. I will distinguish Cantor's *proof*—that $\mathbb{R}$ is uncountable—from Cantor's *technique* (*method*, *procedure*). By this I mean the construction of a diagonal sequence as follows:

$$\begin{array}{l} a_{11}, a_{12}, a_{13}, a_{14}, a_{15}, \ldots \\ a_{21}, a_{22}, a_{23}, a_{24}, a_{25}, \ldots \\ a_{31}, a_{32}, a_{33}, a_{34}, a_{35}, \ldots \\ a_{41}, a_{42}, a_{43}, a_{44}, a_{45}, \ldots \\ a_{51}, a_{52}, a_{53}, a_{54}, a_{55}, \ldots \\ \qquad\quad \ldots \end{array}$$

New sequence: $b_1, b_2, b_3, \ldots$ such that $b_n \neq a_{nn}$.

For whatever reason, many authors have tried to poke holes in the legitimacy of Cantor's method. None of these attempts, however, have been treated particularly seriously. Wilfrid Hodges has written a rather amusing, if not slightly disrespectful, article entitled: 'An Editor Recalls Some Hopeless Papers' (1998). This paper is entirely devoted to highlighting (and in some cases mocking) errors in the many papers sent to him for publication that

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[9] A function is injective iff every element of the function's range is the image of *at most* one element of its domain. A function is surjective iff every element in the range is the image of some element in the domain. A total function that is both injective and surjective (i.e. there is a one-to-one correspondence) is *bijective*.

[10] This was, in fact, proved by Cantor. If $|A| = n$ then $|\mathcal{P}(A)| = 2^n$. Further, for any set (finite or infinite) $|\mathcal{P}(A)| > |A|$.

claimed to refute Cantor's proof. This goes to show, if only superficially, the esteem in which Cantor's proof is held in the mathematical community. Hodges actually mentions a court case from 1995 where William Dilworth, having published a 'refutation' of Cantor's proof, sued mathematician Underwood Dudley for libel after he called Dilworth a 'crank' (1998, 1). Perhaps it is telling that the case was dismissed.

Given this, Wittgenstein's remarks on Cantor should be immediately surprising: Wittgenstein criticises the results of Cantor's proof. These remarks are worth further inspection for their own sake. However, they will also be instrumental in understanding Wittgenstein's later remarks on Turing's application of the diagonal procedure. I do not find Wittgenstein's attack especially convincing. I will say this though: despite being, I dare say, the quintessential crank, Wittgenstein is absolved of that charge on this count.

1.2 Wittgenstein on Mathematics in 1938

The bulk of Wittgenstein's comments on Cantor's diagonalisation proof come from MSS 117 and 121, which have been collated into Part II of RFM. These originate mostly from 1938. These remarks are highly cryptic and require an understanding of Wittgenstein's prevailing philosophy of mathematics, of which they are symptomatic.

By 1938 Wittgenstein was well into the formation of his philosophy that became known as the 'later Wittgenstein'. This was characterised by a vehement rejection of the system of language and reality described in his first work: *Tractatus Logico-Philosophicus* (TLP), written during the First World War. The *Tractatus* describes a rigid relationship between language and the world—we describe facts by picturing them with language. The structure of language logically pictures the structure of reality—language is "laid against reality like a measure" (TLP 2.1512). Thus, language has sense if it pictures some possible or actual state of affairs. Any other utterance is nonsense.

The principal aim of the *Tractatus*—aside from solving all philosophical problems—was to resolve the core issues with which Russell was struggling at the time, such as the class-theoretic problems in *Principia* and the multiple-relation theory of judgement. Wittgenstein was highly suspicious of class and set theory. Thus, when it came to mathematics, the *Tractatus* offered alternative foundations that attempted to dispense with classes altogether. This turned on a theory of formal operations. Wittgenstein defines the natural numbers in terms of successive 'operations' upon a variable, much like a successor function (cf. TLP 6.01-6.02).[11]

Importantly, Wittgenstein's take on mathematics in the *Tractatus* is non-referential, and in many ways formalist—for Wittgenstein, mathematical propositions are purely syntactical and make no reference to independent mathematical objects or facts. Numbers are defined not by appeal to mathematical objects, but by successive applications of *rules.* Mathematics (or, at least, arithmetic) in the *Tractatus* consists only in the manipulation of signs. Wittgenstein is clear that mathematical statements are not *bona fide* propositions: they do not create a picture of the world (*qua* representing certain states of affairs and their relation to one another). Having said this, mathematical propositions are relevant in justifying inferences between propositions:

> Indeed in real life a mathematical proposition is never what we want. Rather, we make use of mathematical propositions *only* in inferences from propositions that do not belong to mathematics to others that likewise do not belong to mathematics (TLP 6.211).

Mathematics is not purely a manipulation of signs, then, but a set of operations yielding normative results used to reason about contingent propositions.

Wittgenstein subsequently came to reject most of the ideas in the *Tractatus.* His analysis of logic became less formalised, and he dropped his key idea that there is a pervasive logical form across all of language. By the early thirties, the 'language game' made its way into Wittgensteinian parlance—a tool for highlighting the use of language in certain isolated contexts. This emphasised that the meaning of expressions came from their use, which needn't be constant across language.

Wittgenstein's outlook on mathematics followed suit. His account became far less formalised and inquiry was always directed at the application of a given calculus. Having said this, his core approach remained substantively similar. He still maintained the formalist aspects of his philosophy of mathematics:

[11] See Marion (1998, Ch2) for a good exposition. Marion interestingly points out that, save notation, Wittgenstein's definition of the natural numbers is identical to Church's definition in the λ-calculus—the formalism used in the first proof of the unsolvability of the *Entscheidungsproblem.*

> Mathematics is always a machine, a calculus. The calculus does not describe anything. [...] The calculus is an abacus, a calculator, a calculating machine (WVC, 106).

For Wittgenstein, "mathematics consists entirely of calculations" (PG, 468). Calculations can be thought of only as the manipulation of signs like the manipulation of beads on an abacus. As such, mathematics does not *describe* anything. We cannot describe it; we can only do it (PR, §159). Mathematics is thus fundamentally *algorithmic*: "in mathematics *everything* is algorithm, and *nothing* is meaning" (PG, 468).

Around this time Wittgenstein developed a distinction between *calculus* and *prose*.[12] Only manipulating a calculus, like moving beads on the abacus, is doing mathematics. There is no place for prose in mathematical calculation. Having said this, we use calculations to reason over propositions in prose. He would continually emphasise the claim from the *Tractatus* that the *sine qua non* of a mathematical calculus is its use to reason about everyday propositions:[13]

> I want to say: it is essential to mathematics that its signs are also employed in *mufti*. It is the use outside mathematics, and so the *meaning* of the signs, that makes the sign-game into mathematics. (RFM V, §2).

That is, mathematics is a game we play to reason about extra-mathematical propositions. Only calculation is *doing* mathematics, but it is the *application* that makes the game mathematics. Similarly, we do not study mathematical calculi simply because they make an interesting pattern, but rather we use them *normatively* to reason about how things must be.

At the core of this is the tenet that (all) mathematics is an *invention* by us. Wittgenstein would insist that "the mathematician is not a discoverer: he is an inventor" (RFM I Appendix II, §2).

From this crucial point follow many important corollaries. There are no mathematical objects independent of our inventions. From this there can be no such thing as an 'infinite extension'. That would require, amongst other

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[12] See Shanker (1987a, Ch5) for a good exegesis of this.

[13] This tenet was seemingly dropped in the early thirties, where Wittgenstein talks cryptically in places about arithmetic 'taking care of its own applicability' (cf. PG, 308). The tenet reappears by RFM.

things, a superhuman capacity to invent infinitely many discrete objects. Wittgenstein makes a sharp distinction between 'extensions'—symbols, sets, axioms etc. (the mathematical objects we invent)—and 'intensions', which he understands as the mathematical *rules* for e.g. transformations, generating sequences etc.[14] The concept of infinity can only be understood in terms of intensions. Infinity is a property of a concept contained entirely within its rules for construction; it is never a question of 'large' extensions. For instance, Wittgenstein claims that "[a]n irrational number isn't the extension of an infinite decimal fraction,… it's a law" (PR, §181). Wittgenstein continually criticised the conflation of intensions and extensions in mathematics: "there isn't a dualism: the law and the infinite series obeying it" (PR, §180). When we talk of infinity, we must avoid thinking that we are talking about extensions: "only laws reach to infinity" (PR, §181). The crux of the point is that "'[i]nfinite' is not a *quantity*", it represents a possibility, and thus is a predicate of a certain property; its grammar is entirely distinct from that of finite numbers, which we can label as mathematical objects (WVC, 228).

> The infinite number series is only the infinite possibility of finite series of numbers. It is senseless to speak of the *whole* infinite number series, as if it, too, were an extension. (PR, §142).

This is characteristic of Wittgenstein's vehement anti-platonism, this following from his conviction that mathematics is invented by us. This restricts him to *finitism*: an insistence that there can be no such thing as infinitely many mathematical objects. This precludes the existence of infinite sets for Wittgenstein. He was heavily critical of set theory on these exact grounds, calling it a "fictitious symbolism" (PR, §174). It is fictitious due to a failure to heed the distinction between intension and extension. Sets do not have intensions, being intuitively just collections of objects. This leads to the labelling of an infinite set as if it were an actual extension. For Wittgenstein, there can be no such thing: infinity is only a property, or rather a possibility, of an intension. Further, this symbolism, in lieu of an enumeration of infinite sets, settles with describing them by signs. However, as mathematics is essentially algorithmic for Wittgenstein, there is no room for description of possibilities.

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[14] N.B. This is different to how the extension/intension distinction is commonly understood nowadays. Usually we use 'extension' to denote the class of objects denoted by a given expression, whilst the 'intension' is its meaning. For instance, 'the morning star' and 'the evening star' have the same extension—Venus—but different intensions.

For this reason, Wittgenstein's approach to mathematics is in various places referred to as 'intensionalist', or rather 'anti-extensionalist' (cf. Marion (1998)). This lies in opposition to what Wittgenstein would label the 'extensionalist' approach seen in his Cambridge peers such as Russell and Ramsey. Wittgenstein was highly suspicious of extensional talk; understanding mathematics involves only grasping the underlying rules in a given calculus or algorithm.

From this picture it is clear why Wittgenstein would oppose Cantor's set-theoretic proof that some infinite sets have a greater cardinality than others.

1.3 Wittgenstein on Uncountability

Wittgenstein rejected the results of Cantor's proof as a fundamental confusion: the proof claims more than its method allows because it is muddied by ordinary language.

Mathematics is simply a calculus, a game with signs. Following from his distinction between calculus and prose, Wittgenstein argues that any result of a calculation expressed *verbally* is to be regarded with suspicion (RFM II, §7). We should never try to confer meaning upon a calculus via prose. Rather, "the calculation illumines the meaning of the expression in words" (ibid.). That is, calculation should inform the meaning of the verbal expression, not the other way around:

> [T]he verbal expression casts only a dim general glow over the calculation: but the calculation a brilliant light on the verbal expression (ibid.).

This point is very important and explicitly directed at Cantor's proof, which is almost always explained using prose with a pre-existing meaning. Take the following quotation lifted from *SEP*:

> There are relatively small infinite sets like the set of even numbers, the set of integers, or the set of rational numbers. These sets can all be put into one-to-one correspondence with the natural numbers; they are called *countably infinite*. In contrast, there are much "larger" infinite sets like the set of real numbers, the set of complex numbers, or the set of all subsets of the natural numbers. These sets are too big to be put into one-to-one correspondence with the natural numbers; they are called *uncountably infinite*. Cantor's Theorem, then, is just the claim

> that there are uncountably infinite sets—sets which are, as it were, too big to count as countable (Bays 2014).

Wittgenstein would entirely oppose such an interpretation of Cantor's proof. Here, the pre-theoretical concepts of size (largeness, big-ness etc.) are being attached to Cantor's calculus as a means by which which to interpret it, with no reference to the actual proof. Because the diagonal procedure involves finding an 'extra' number not included in a given list, there is a temptation to follow an analogy with finite sets and declare that Cantor's proof shows that $\mathbb{R}$ is *larger* than $\mathbb{N}$. We are led to believe that we are comparing these sets in terms of magnitude.

This, however, is *not* what Cantor's method shows for Wittgenstein. What the method shows is that:

> [T]he *concept* 'real number' has much less analogy with the concept 'cardinal number' than we, being misled by certain analogies, are inclined to believe (RFM II, §22).

Traditionally, this disanalogy represents, "by a skew form of expression, a difference of extension" (ibid.). He declares this tactic "hocus pocus" (ibid.).

What he means by a disanalogy in the concept *real* versus *cardinal number* is not initially explicit. I take him to mean that there is some difference in kind in the *rule* for the generation of successive real numbers comparatively to natural numbers, cardinal numbers etc. This must be what he means as, for Wittgenstein, the infinity displayed in these series is entirely a feature of the rules for their construction. There is no difference in extension between these disanalogous concepts: both have the potential for infinite expansions. That is, the rules for generating them "lack the institution of an end" (RFM II, §45). Thus, the extensions in either case are not in question. The difference between the *concepts* that Cantor has shown is that it makes no sense to talk of an enumeration of the real numbers. That is, there is no definite rule or algorithm which will yield the series of real numbers. The generation of the 'next' real number makes no sense because the rule does not turn on a tangible, recursive operation such as '+1'. As such, there is no stepwise rule I can apply which will capture 'an enumeration of the real numbers'. *That* is what Cantor showed with the diagonal method. Disanaologously, all enumerable concepts can be enumerated stepwise. Take the rational numbers: even though there is no way in which this series can be ordered in magnitude[15], there is a *definite rule* for their enumeration which turns on a simple operation:

We can illustrate that the set of rational numbers ($\mathbb{Q}$) is enumerable by giving a rule for counting the rational numbers p/q in order of the sums $p+q$ i.e. begin with those where $p+q=2$ (i.e. 1/1), then those where $p+q=3$ (i.e. 1/2 and 2/1) and so on. To enumerate $\mathbb{Q}$ we just follow such a rule listing its positive and negative counterparts successively, avoiding repeats:

$$\mathbb{Q} = \left\{0, \frac{1}{1}, -\frac{1}{1}, \frac{1}{2}, -\frac{1}{2}, \frac{2}{1}, -\frac{2}{1}, \frac{1}{3}, -\frac{1}{3}, \dots\right\}$$

For Wittgenstein, the analogy between the concepts *natural number* and *rational number* is that there is a *rule* that can be followed which successively enumerates their extensions. The purpose of Cantor's proof, for Wittgenstein, is to show that a similar rule cannot be defined for the concept *real number*. Any rule intended to lay down all the real numbers in a sequence $E_1, E_2, E_3, \dots$ will exclude some real number E_0. Thus, there is a disanalogy between this concept and those of *cardinal*, *natural*, *rational* etc.

Wittgenstein argues on this point that the only proper use of the diagonal proof could be to dissuade someone from attempting to enumerate the irrational numbers. We could say: "leave it alone; it means nothing; don't you see, if you established a series, I should come along with a diagonal series!" and he might abandon his attempts (RFM II, §13).[16]

This is the true sense of Cantor's proof for Wittgenstein. He has shown something about the nature of the rules associated with these concepts. It is the interference of prose and our intuitive understanding of the concepts involved that skews the results creating misunderstanding. Of course, we may want to use ordinary language to explain the results of a given calculus. In fact, this is common practice and surely advisable. What is important is the direction in which meaning is fixed.

For instance, it makes perfect sense to say "I call number-concept X non-denumerable if it has been stipulated that, whatever numbers falling under this concept you arrange in a series, the diagonal number of this series is also to fall under that concept" (RFM II, §10). However, this is different to

[15] There is another rational number $(n+m)/2$ between any rational numbers n and m.

[16] It should be emphasised here that Wittgenstein uses 'series' to mean 'enumeration'.

declaring after Cantor's results: "therefore the X numbers are non-denumerable" (ibid.). It is admissible to label the results of a calculus with prose, thereby fixing its meaning and allowing us to export the result to reason about verbal propositions. What is inadmissible is reasoning about the calculus in terms of our everyday prose, importing its pre-existing meaning. This ultimately leads to confusion. The 'therefore' above cannot be seen as an appeal to some independent concept *non-denumerable* which we are now entitled to apply to this calculus, as if we have discovered that a certain number-concept satisfies our existing concept *non-denumerable*.

Wittgenstein stresses this point in the first of the *Lectures*:

> Suppose Professor Hardy came to me and said, "Wittgenstein, I've made a great discovery. I've found that [X]" I would say, "I am not a mathematician, and therefore I won't be surprised at what you say. For I cannot know what you mean until I know how you've found it." We have no right to be surprised at what he tells us. For although he speaks English, yet the meaning of what he says depends upon the calculations he has made (LFM, 17).

We are perfectly entitled to claim that Cantor has shown that some infinities are greater than others. However, this can only be understood by reference to the calculus. The meaning of 'greater' relative to Cantor's proof must have its meaning fixed therein. Furthermore, the use of the word 'greater' as applied to this proof bears no analogy to its use outside of this calculus. If someone claims that Cantor has proved that the real numbers are more numerous than the cardinal numbers, we cannot understand what this means without appeal to the proofs themselves, for the language is only meaningful relative to the calculi which it interprets. We cannot understand the claim until we understand how the word 'numerous' is being used to express Cantor's results.

Wittgenstein therefore asks: "what can the concept non-denumerable be used for?" (RFM II, §12). If we decide to label a certain concept as non-denumerable based on its susceptibility to diagonalisation, what can this tell us? The implication is: nothing. This point turns on Wittgenstein's conviction that mathematical calculi are inventions of ours which are used to make inferences about non-mathematical propositions. For instance, the algorithm which tells me that $2 + 2 = 4$ can be used to make inferences about the total number of bananas in my hands given that I have two in each. Non-denumerability cannot be applied outside of mathematics in this way. The difference between countable and uncountable infinities on this picture is useless. We cannot employ these concepts outside of abstract set theory. There is no picture outside of this calculus to which we can attach it.

Wittgenstein thus insists that, when told by Cantor's proof that we cannot arrange the irrational numbers in a series, we can insist: "I don't know...what it is that *can't be done* here" (RFM II, §16).

As non-denumerability is a concept fixed by its mathematical surroundings, we are just forcing a calculus to give us a certain result which we label 'non-denumerable', but which means nothing outside of that calculus. As such the statement that '$2^{\aleph_0} > \aleph_0$' simply "hangs in the air", it looks like an architrave but is "not supported by anything and supporting nothing" (RFM II, §35). He compares it to the proposition that 10^{10} souls fit into a cubic centimetre. We could say this, but we do not because the picture it conjures is of no use to us (RFM II, §36).

The real problem, of course, is that talking in this way misleads us to think that the claims about non-denumerability are substantive. Calling a set 'uncountable' because there are 'too many' elements to be placed in a one-to-one correspondence with an enumerable set conjures a picture that goes beyond what the calculus shows. It allows the proof to prove "more than its means allow it"; it becomes a "puffed-up proof" (RFM II, §21). The danger here is that it makes the determination of a concept "look like a fact of nature" (RFM II, §19). The error lies in allowing a conflation between calculus and prose.

> Ought the word 'infinite' to be avoided in mathematics? Yes; when it appears to confer meaning upon the calculus; instead of getting one from it (RFM II, §58).

1.4 Analysis

It should be clear that Wittgenstein is not one of the 'cranks' as described in Hodges' *Hopeless Papers*. Wittgenstein nowhere contends that Cantor's technique is guilty of some logical blunder. It isn't. Wittgenstein has not misunderstood the technique or what it claims to show. Rather, Wittgenstein objects to the way Cantor frames his results, due to differences at a more foundational level. For instance, Wittgenstein is opposed to the idea that Cantor has described a difference in extension; admitting this makes his proof look like a fact of nature. This would contradict Wittgenstein's conviction that mathematics is pure invention. Wittgenstein's objections all follow quite straightforwardly from his philosophy of mathematics in 1938 as per my explication.

If we take Wittgenstein's critique at face value, he is surely unsuccessful. Cantor's proof is framed set-theoretically. Sets do not have intensions. Of course, we may use rules as a means to talk about a set, but the set exists independently of them. A rule describing a set is thus ontologically irrelevant. What Cantor showed *must* be a difference in extension. Given Cantor's definitions, for a set to have the same cardinality as another set is for a one-to-one correspondence to pertain between their extensions. He proved that no bijection pertains between $\mathbb{R}$ and any enumerable set. The only way to cash out this result is in terms of the cardinality of the sets involved i.e. properties of their extensions.

Having said this, the source of Wittgenstein's critique on this score is clearly a product of other beliefs about mathematics i.e. his finitism, his extreme anti-extensional approach, etc. Wittgenstein resists the concept of a set altogether due to its 'fictitious symbolism'. Their disagreement is more fundamental than the proof of uncountability itself. They are, as it were, sat at different chessboards, so Wittgenstein is wrong to expect a game. In Cantor's terms, he really has shown a difference in extension. I do not think that Wittgenstein would deny that, but rather disagree with the terms in the first place. This is a different kind of objection to the ones seen in the *Hopeless Papers.* There is a distinction between *being framed in the wrong terms* and *being wrong in the terms set out*—Cantor's proof is only susceptible to objections of the former kind. To be sure, if we accept Cantor's set-theoretic framework then his results do follow; it is a *bona fide* proof. However, the question of whether to accept this framework goes to the root of what (one thinks) mathematics is.

One crucial point that I find successful is Wittgenstein's dichotomy between calculus and prose and his subsequent conviction that we should be wary of expressing results verbally, without inspection of the calculus. This is because we fix the meaning of verbal expression by association with a given calculus. That is why Wittgenstein said in the *Lectures* that he cannot understand Hardy's claim to have proved such and such, until he has seen *how* he proved it. He cannot know what it means to prove that X until he knows what X actually means; this can only be understood by reference to the calculus that proves X.

Now, it is clear how an expression of the uncountability results could be guilty of such confusion. For instance, my earlier quotation from *SEP* explains the results of Cantor's proof (in terms of numerousness) with no mention of how the results were obtained via the calculus. This, I think, is impermissible and exactly the kind of confusion Wittgenstein has in mind. The ordinary language associated with numerousness already carries its own meaning which must not be projected onto the results of the proof. This gives the (false) impression that we have the extensions laid out before us of, say, the naturals and the reals, and when pairing them off we somehow run out of natural numbers—some "sets are too big to be put into one-to-one correspondence with the natural numbers" (Bays 2014).

Despite this, Wittgenstein's criticism here seems to miss its target. His worry seems more applicable to the pedagogy of Cantor's proof rather than a practicing mathematician using transfinite arithmetic.

I cannot foresee any real confusion (for a mathematician) in the claim that '$2^{\aleph_0} > \aleph_0$'. The notation here is an expression of cardinality that elucidates the relationship between these sets and their respective properties. It describes which kinds of functions pertain between these sets and their behaviour relative to one another. We may still talk about the uncountability proofs verbally, and numerosity is arguably the neatest way of doing so. What must be avoided is importing the pre-theoretic meaning of the ordinary language onto the calculus and thereby misrepresenting the results. I think, however, that mathematicians are fully aware of this. Even Hardy—arch-extensionalist and Cantor supporter of the mathematician-as-discoverer persuasion—heeds Wittgenstein's point, refraining from explicating Cantor's proof to the lay-mathematician in *A Mathematician's Apology*:

> [T]he proof is easy enough, when once the language has been mastered, but considerable explanation is necessary before the *meaning* of the theorem becomes clear (1940, §13).

Mathematical discussion requires that we have means to talk about proofs in prose. This is entirely permissible so long as the distinction between prose and calculus is respected and no meaning is conferred upon the calculus by its verbal expression. Rather, meaning must be fixed in the other direction. We should be suspicious of verbal expressions of Cantor's uncountability results that do not reference the proofs. That being said, I do not think practicing mathematicians routinely fall into this trap. Wittgenstein's worry, although well founded, does not seem applicable to those who should be its primary target.

Further, I would resist Wittgenstein's conviction that the concept *non-denumerable* is somehow useless, or that the claim that $2^{\aleph_0} > \aleph_0$ 'hangs in the air' as a picture with no application. As I have insisted, these are distinctions that tell us about the behaviour of certain sets, and the relations pertaining between them. Uncountable sets are salient in their contrast to countable sets. The distinction between countable and uncountable sets can, in fact, be highly informative on occasions. For instance, once this distinction was clarified it led directly to a (constructive) proof that there are transcendental numbers.[17] It is fairly simple to show that the algebraic numbers are enumerable and there are several ways of giving a rule for their enumeration.[18] As the algebraic numbers are a countable subset of the (uncountable) real numbers, it follows directly that there are transcendental numbers. This is inferable directly from employment of the concept *non-denumerable*. What is more, a rule for the generation of a transcendental number can be given by employing the diagonal method. We could program a computer to begin enumerating the decimal expansions of algebraic numbers, then printing a new sequence by altering the n-th digit of the n-th number. By the standard diagonal reasoning, this number must be transcendental—it differs from every algebraic number by at least one digit. This strikes me as a proper and useful application of the concept *non-denumerable* and the diagonal method. I cannot see how Wittgenstein would object to the diagonal algorithm that successively generates the expansion of a transcendental number.

Wittgenstein was certainly aware of the diagonal proof concerning the transcendental numbers; he alludes to it in several places (cf. RFM II, §34). It strikes me as a counterexample to his claim that Cantor's results regarding uncountability are 'not supported by anything and supporting nothing'. Perhaps it is because the results still do not conform to Wittgenstein's tenet that mathematics should be used for reasoning about everyday propositions. I cannot think how Cantor's results could be used in this way. Having said this, Cantor's results surely inform *other* mathematical propositions (e.g. in topology or measure theory) which in turn have an extra-mathematical application. In any case, this criticism is not particularly threatening unless

[17] These are numbers that are not the root of any nonzero polynomial equation with integer coefficients. Numbers that *are* roots of these are called 'algebraic'.

[18] See Gray (1994), the basic idea is to enumerate equations of the form $a_n x^n + a_{n-1} x^{n-1} + a_2 x^2 + a_1 x + a_0$ and then generate from these their roots.

you already agree to that tenet. Once again, that question is more foundational than Cantor's results themselves. To say the least, I doubt Cantor or his disciples would find Wittgenstein's arguments problematic on this score. To paraphrase Hardy once more: 'very little of mathematics is useful practically, and that little is comparatively dull' (cf. 1940, §11).

1.5 Wittgenstein on Diagonalisation

It seems to me that Wittgenstein's critique, although free from the misunderstandings present in many objections to Cantor's proof, is not especially successful. It might well be convincing, but only if the reader is already sympathetic to Wittgenstein's general philosophy of mathematics.

Putting this to one side, there is one further point from RFM II that I wish to explore before moving onto Turing. There is a class of remarks in RFM II that I have hitherto neglected; these are Wittgenstein's remarks on the diagonal *technique* itself, rather than Cantor's *proof*. These roughly follow the spirit of Wittgenstein's critique of the proof. Wittgenstein neglects the orthodox extensional approach to the technique, instead discussing the prospect of the technique being fruitful under his peculiar intensional, finitistic gaze.

His remarks are largely confusing: the technique does not transpose naturally to an instensional reading. This is hardly surprising seeing as Cantor's proof is non-constructive, and the technique was developed especially to prove set-theoretic results. Wittgenstein is largely concerned with the nature of the rule which generates a diagonal sequence and what that rule can be used for. The general form of a diagonal sequence takes inputs (the n-th digit from the n-th sequence) and generates a sequence of which the m-th digit is different from each n-th digit where $m = n$. Wittgenstein compares this with another task:

> 'Name a number that [dis]agrees with √2 [precisely] at every second decimal place.' What does this task demand? The question is: is it performed by the answer: It is the number got by the rule: develop √2 and add 1 or -1 to every second decimal place?
> It is the same as the way the task: *Divide an angle into three* can be regarded as carried out by laying 3 equal angles together (RFM II, §2).[19]

[19] I have changed 'agrees' to 'disagrees', and added 'precisely'. Wittgenstein has made a mistake here as √2 already agrees with itself at every second decimal place. As Wittgenstein

The point here is: tampering with extensions is just a cheat-answer—a redraft of the question. You have not given an answer *per se*, but rather a template for what a real answer would look like. But this much was already contained within the task as described. He likens this tactic to laying three equal angles together. This is a reference to one of the classic Greek compass-and-straight-edge problems: there is no possible method for trisecting arbitrary angles using only an unmarked straight edge and a compass. Laying out three angles side-by-side as a solution to the task 'divide an angle into three' is a cheat-answer. It is not *technically* wrong, but it certainly would not be acceptable as a proof of the possibility of trisecting an angle. A natural response to this method might be: "But I didn't mean like that!" (RFM II, §3).

As for diagonal numbers, Wittgenstein says the method does not give us a 'number different from all of these', but rather "a rule for the step-by-step construction of numbers that are successively different from each of these" (ibid.). That is, we are given a rule that will generate sequences stepwise, successively dodging each expansion on a list. What he has in mind here are what Felix Mühlhölzer calls "a_{ik}-carpets" (Floyd & Mühlhölzer (forthcoming), 146).

We are confronted with an infinite list of sequences:

$$\begin{array}{l}a_{11}, a_{12}, a_{13}, a_{14}, a_{15}, \ldots\\ a_{21}, a_{22}, a_{23}, a_{24}, a_{25}, \ldots\\ a_{31}, a_{32}, a_{33}, a_{34}, a_{35}, \ldots\\ a_{41}, a_{42}, a_{43}, a_{44}, a_{45}, \ldots\\ a_{51}, a_{52}, a_{53}, a_{54}, a_{55}, \ldots\\ \qquad\quad\ \ldots\end{array}$$

Of course, given Wittgenstein's finitism, the ellipses here do not represent an infinite expansion, but the potential infinity indicated by the rules generating each a_{ik}-row, which have no end digit.

The construction of a diagonal sequence involves altering each n-th digit of the n-th row. Thus, to compute a diagonal number, we follow the rule for a_{1k}, calculate its first digit, then alter the result and write it down. We then follow the rule for a_{2k} and calculate its expansion up to the second place, then alter the result write it down. We do this for every a_{ik}, calculating it up to the i-th digit of its expansion and altering a_{ii}. This will generate the diagonal sequence. This computes successive carpets of results as follows:

certainly meant this as an analogy for the diagonal procedure, the correction illustrates Wittgenstein's point better anyway. I took this point from Floyd & Mühlhölzer ((forthcoming), 131).

$$
\begin{array}{l}
a_{11} \\
\\
a_{11}, \\
a_{21}, a_{22} \\
\\
a_{11} \\
a_{21}, a_{22} \\
a_{31}, a_{32}, a_{33} \\
\\
a_{11} \\
a_{21}, a_{22} \\
a_{31}, a_{32}, a_{33} \\
a_{41}, a_{42}, a_{43}, a_{44} \\
\ldots
\end{array}
$$

For Wittgenstein, there is no such thing as a mathematical object until we invent it. As such, it makes no sense to speak of extensions laid out unless we construct them via some rule. An intensional approach to the diagonal sequence requires an interpretation whereby we generate successive finite sequences of expansions and use our diagonal rule to successively avoid them. This is why Wittgenstein argues that the diagonal procedure does not give a result vis-à-vis 'a number different from all of these'; rather, it gives instructions on how to avoid a given system of expansions. Given the above picture, it is clear why Wittgenstein insists:

> Let us say—not: "This method gives a result", but rather, "it gives an infinite series of results" (RFM II, §5).

Now, one may retort: granted, we are given a rule by the diagonal procedure rather than a result, but we are *also* given a result (in some sense) i.e. a number—the rule shows how to construct an infinite diagonal number.

Wittgenstein's response is: "But what is the method of calculating, and what the result, here?" (ibid.). He follows this by asking:

> What can this number be *used* for? True, that sounds queer.—But what it means is: what are its mathematical surroundings? (ibid.).

What he means is: interpreting the diagonal sequence *qua* its general form as a rule for avoiding a given system of expansions, the number obtained by the rule is inseparable from that given system of expansions. We cannot take this number out of its mathematical context for use elsewhere; it is in the DNA of the diagonal number, as it were, that it be defined in terms of the system of expansions it is designed to avoid.

This is quite a bizarre point to pick up on, but Wittgenstein is correct in some sense. The rule we obtain for computing the transcendental number via the diagonal procedure is different in kind to the rule for, say, computing the expansion of π. The latter is inward looking, as it were. The diagonal transcendental is defined in terms of the system of algebraic numbers, and so the rule cannot be understood, less so applied, separately from that system.

What Wittgenstein has picked up on is that: "Cantor defines a *difference of higher order*, that is to say a difference of expansion from a *system* of expansions" (RFM II, §34). Of course, the difference of higher order here must be a reference to rules. What Cantor has shown, for Wittgenstein, is that whilst the generation of numbers by some rule on the surface appears to be the same thing, there can be a *difference in kind* in the rules that produce them. There is a higher-order difference between the generation of the diagonal sequence and the generation of the system of algebraic numbers. Due to this, he resists the diagonal *reasoning*:

> [W]e cannot very well say that the rule of altering the places in the diagonal in such-and-such a way is as such proved different from the rules of the system, because this rule is itself of 'higher order'; for it treats of the alteration of a system of rules (ibid.).

This argument is interesting and will be especially important for §2. Wittgenstein is arguing that, if we interpret the diagonal procedure as a *rule* for generating a sequence, it makes little sense to declare that this is different from the rule, or set of rules, which enumerate a particular list of expansions. This is because the rule for developing the diagonal sequence is of *higher order*: it is a rule cast over that system of expansions. As such, Wittgenstein claims that we *already know* that the diagonal sequence is of a different kind to the system of expansions. It is already obvious from its design that the diagonal rule could not appear as a rule for one of the expansions in the system. Wittgenstein thus claims it makes no sense to say you have shown that the diagonal sequence is different from all of these sequences in a given expansion: we are not left any clearer on what it means in general that a given expansion is 'different from all of these'. Or so it goes.

Obviously this worry bears no threat to Cantor's formulation, his being purely extensional. I think, though, we should read RFM II as an attempt by Wittgenstein to intellectually experiment with the diagonal procedure and *see what would happen* if we were to approach it intensionally. As I have shown, it does not work very well for Wittgenstein. The diagonal procedure does not seem to transpose well into the Wittgensteinian approach. Despite this, Wittgenstein devotes many remarks specifically to the technique. As such, he clearly thinks this is an interesting pursuit. However, there is a lack of clarity from Wittgenstein over what the intensional diagonal technique *shows.* This is why his remarks here are so cryptic. I contend that RFM II is an expression of Wittgenstein's nascent thoughts of the diagonal technique. He is attempting to derive some value out of the technique as an intensional exercise, but cannot find a clear way of cashing this out. I will show in §2 how Turing's application of the diagonal procedure accomplishes this task of RFM II. Abstracting from Turing's diagonal proof in *Computable Numbers* shows how an intensional diagonal procedure can deliver meaningful results about the nature of rules. Wittgenstein saw this, and later made a point of doing so.

2 Turing—On Computable Numbers and Wittgenstein's Diagonal

2.1 Enters Alan

The chief aim of this chapter is to show that Wittgenstein read, understood and endorsed Turing's results in *Computable Numbers.* I will show that Turing's diagonal proof realised the intensional diagonal procedure that Wittgenstein pursued in RFM II. Wittgenstein employed Turing's proof as a model for his own diagonal argument, which he used to shed light on the notion of a *rule.*

Before doing this, I must of course briefly run through the contents of *Computable Numbers.* There are three proofs in the paper. The first turns on an application of the diagonal procedure. The second proof draws important consequences from this to deliver the final proof, which shows that the *Entscheidungsproblem* has no solution. The first proof is the most relevant for my purposes. I will run through this in detail then give a brief sketch of the two proofs that follow.

Intuitively, the effectively computable (or calculable) functions are those for which a finite list of definite instructions can be given such that in principle every value of the function can be determined. Turing depicts computable sequences as those that "can be written down by a machine" (CN, 116). Turing then expands on what he means by this. He explicates what are now familiar as *Turing machines.*

These are abstract computing machines capable of a finite number of conditions ('*m*-configurations': $q_1, q_2, q_3, \ldots$). The machines can recognise finitely many easily distinguishable symbols (S_j). They are fed an abstract infinite tape divided into squares and a set of instructions that tell it what to do when a given symbol is scanned in a given *m*-configuration. The machine may scan one square at a time, which contains a symbol or is blank. The machine can erase/print/replace/leave a symbol in the square, move left/right to a different square and/or enter a different *m*-configuration. Machines are either *circular* (if it ever halts)[20], or *circle-free* (otherwise). The computable sequences are those computed by circle-free machines. At any stage of motion,

[20] Or continues indefinitely but only printing 'rough notes' (cf. CN, 118).

the number of the scanned square, all the previously printed symbols on the tape and the m-configuration will be called the *complete configuration* of that stage (CN, 118).

The machines can be arithmetised by coding their behaviour into natural numbers. Each machine is uniquely describable in terms of its *description number* (D.N.). Turing machines are describable as sets of quintuples of the form q_n, x, y, z, q_m, where x and y are some S_j (symbol or blank) and z is either R for *right* or L for *left.* They should read as 'when configuration n obtains and x is scanned, replace it with y and move right/left then adopt configuration m'. It is well known (proved by Cantor) that quintuples of symbols taken from an enumerable alphabet can be put in a one-to-one correspondence with the natural numbers, allowing the latter to function as codes for the former. It follows that the computable sequences are enumerable.

Turing defines a *universal machine* (**U**), which is capable of computing any computable sequence i.e. mimicking the sequence outputted by any Turing machine. It is fed the D.N. of any Turing machine and then successively prints its complete configurations. This machine underpins the proofs that follow.

2.1.1 The Proofs

Turing titles one section 'application of the diagonal process' (CN, 132). This should immediately be worrying: as I showed in §1, Cantor's method generally shows that a class of numbers is non-denumerable. However, as already stated, the computable sequences are enumerable.

Turing imagines an interlocutor:

> If the computable sequences are enumerable, let $\boldsymbol{\alpha}_n$ be the n-th computable sequence, and let $\phi_n(m)$ be the m-th figure in $\boldsymbol{\alpha}_n$. Let β be the sequence with $1-\phi_n(n)$ as its n-th figure. Since β is computable, there exists a number K such that $1-\phi_n(n)=\phi_K(n)$ all n. Putting $n=K$, we have $1=2\phi_K(K)$, i.e. 1 is even. This is impossible. The computable sequences are therefore not enumerable (ibid.).

Prima facie, this argument is pretty plausible. Assume the computable sequences (of 0s and 1s) are enumerable. From an enumerated list of the computable sequences, have a machine compute a new sequence by taking the

n-th digit from the n-th computable sequence and altering it. This computes a sequence that is not on the original list yet is itself a computable sequence and so must appear. We have a contradiction. The computable sequences are thus susceptible to diagonalisation and by reductio they are not enumerable.

However, this argument is delivered in quotation marks. It is not, in fact, sound: "the fallacy in this argument lies in the assumption that β is computable" (ibid.). As it happens, β is not computable. The problem is equivalent to the problem of finding out whether a number is the D.N. of a circle-free machine, and there is no general way of doing this in a finite number of steps (ibid.).

Turing proves this by giving the correct application of the diagonal method. This involves attempting to compute not the *anti*-diagonal[21] as above, but the *positive* diagonal—a sequence whose n-th figure is $\phi_n(n)$ (ibid.). Supposing this is possible, let's hypothesise that there is a machine **D** which, when supplied with the D.N. of any machine **M**, will test whether or not **M** is circular. It will mark 'u' if **M** is circular, or 's' if it is circle-free (CN, 133). We can then pair **D** with the universal machine to construct **H**—a machine that will compute the (positive) diagonal sequence (ibid.).

It should be clear that to enumerate the computable sequences, we need **D**. To compute a diagonal along all computable sequences, **H** must compute one figure from each computable sequence successively. To do this, it must be fed the D.N.s of machines that compute computable sequences; these are the circle-free machines. So, we need a general, finite way of determining which numbers to feed **H**, as it were.

Turing continues: **H**'s work is divided into sections. It begins with **D** testing every natural number starting from 1,2,3,… It marks all unsatisfactory numbers (numbers that are not the D.N. of a circle-free machine) with 'u'. It continues checking numbers until it reaches a satisfactory number, r, which it marks 's'. The next section of **H**'s behaviour is to compute the sequences determined by those satisfactory numbers; it must compute up to the r-th digit of the sequence defined by the r-th satisfactory number. It then continues to the next sequence. It does not matter in particular how these sections are divided. What matters is that these sections of **H**'s behaviour must work in tandem, as each section continues its work infinitely. From the

[21] I have hitherto called this just a diagonal sequence, where the rule is to *alter* the n-th digit of the n-th sequence successively.

behaviour described above, **H** is itself clearly circle-free by definition (ibid.). Each section can be completed in a finite number of steps and the rules of its formation allow it to move between sections infinitely. **H** has no final position: it will keep computing the diagonal sequence infinitely.

Eventually, **D** will run into a D.N. of **H**. What is the verdict of **D**: is the D.N. of **H** 's' or 'u'? **H**'s D.N. cannot be 'u'—it is circle-free. Interestingly though, it cannot be 's' either (ibid.).

If we say the D.N. of **H** is the k-th satisfactory number determined by **D**, then the rule for **H** is to start calculating the sequence of **H**, and then print its k-th digit. But how can the k-th digit be determined? **H** is a machine whose figures are computed by printing the n-th digit of the n-th computable sequence. The rule is empty for $n = k$. The instruction is to calculate the k-th figure by calculating the k-th figure. The machine cannot proceed, meaning it cannot be circle-free. **H** is both (and neither) circular and circle-free i.e. it is contradictory. We must conclude then that machine **D** is impossible (ibid.).

There is thus no general way of deciding whether a machine is circle-free. This is the first proof in *Computable Numbers*, and the one with which I am most concerned. This is, in fact, the most important proof in the paper; the others follow quite directly.

The second proof is contained to one page—(CN, 134)—and shows that there can be no machine **E** that, when supplied with the D.N. of any machine **M**, will determine whether **M** ever prints a given symbol (say, 0). Turing shows that if **E** is possible, then there is also a machine that determines whether **M** prints 0 *infinitely* often. If so, there is another machine that determines whether **M** prints 1 infinitely often. Combining these machines, we have a process that determines whether **M** prints an infinity of digits (i.e. whether **M** is circle-free). We know from the first proof that such a machine is impossible. Therefore, **E** cannot exist.

The third proof is slightly trickier, involving several lemmas and developments on the previous material. The basic idea is to show that there is no machine that will determine whether any formula of the functional calculus **K** is provable (CN, 145). Turing constructs a formula $Un(\boldsymbol{M})$ in **K** that says "in some complete configuration of **M**, [...] 0 appears on the tape" (CN, 146). He then proves two lemmas: first, that if 0 does appear in some complete configuration of **M**, then $Un(\boldsymbol{M})$ is provable; second, that if $Un(\boldsymbol{M})$ is provable then 0 appears in some complete configuration of **M**. After these, the proof of the unsolvability of the *Entscheidungsproblem* is simple via reductio. If the *Entscheidungsproblem* had a solution, i.e. there was a mechanical way to decide the provability of all formulas in **K**, then there is a mechanical method to decide whether $Un(\boldsymbol{M})$ is provable. From the lemmas we know that if there is such a process, this implies that there is a process for determining whether **M** ever prints a 0. This cannot be done as shown in the second proof. Therefore, the *Entscheidungsproblem* has no solution.

This third proof originally contained some problematic technical errors. These were subsequently addressed and corrected in Turing (1937b).

2.2 Wittgenstein's Reception

The default assumption—and, were it not for the evidence I am about to present, quite an astute one—should be that Wittgenstein would not have cared at all for Turing's paper. Wittgenstein was generally very dismissive of mathematical logic. Wittgenstein viewed it as merely another mathematical calculus, but he was not concerned with doing mathematics. Rather, he wanted to establish its grammar *from the outside*. It was paramount for him "not to interfere with the mathematicians" (LFM, 1):

> I must not make a calculation and say, "That's the result; not what Turing says it is." Suppose it ever did happen—it would have nothing to do with the foundations of mathematics (ibid.).

Further, Wittgenstein infamously made a series of disparaging remarks about Gödel's incompleteness theorems in RFM Appendix III.[22] Turing and Gödel's proofs are, to a large extent, variations on the same theme. As such, there is no reason to think that Wittgenstein's treatment of Turing's work would be any more sympathetic. It would not be surprising if Wittgenstein was not even aware of Turing's work, and if he were, it would be safe to assume he would have been either nonplussed or derisive.

However, this picture is quickly debunked by examination of the historical evidence. First, Turing had an offprint of *Computable Numbers* sent to Wittgenstein in 1937. Shortly after, Alister Watson formally introduced them,

[22] These remarks have received extensive attention cf. Kreisel (1958), Dummett (1959), Goodstein (1957), Floyd & Putnam (2000) & (2006). Most commentators take Wittgenstein to have misunderstood the incompleteness theorems, although some are more sympathetic than others. I do not intend to contribute to this debate.

and the three shared discussions in the Cambridge botanical gardens (Hodges 2014, 172). In Watson's paper on the foundations of mathematics in which he discusses, amongst other things, Gödel's proofs and Turing machines, he credits both Turing and Wittgenstein for helpful discussions (1938).

These discussions clearly had a lasting effect on Wittgenstein. A decade later, in 1947, he writes:

> Turing's 'Machines'. These machines are *humans* who calculate. And one might express what he says also in the form of *games*. And the interesting games would be such as brought one *via* certain rules to nonsensical instructions. I am thinking of games like the "racing game". One has received the order "Go in the same way" when this makes no sense, say because one has got into a circle. For that order makes sense only in certain positions. (Watson.) (RPP 1, §1096).

This reference to Watson at the end suggests that the remark might be a recollection of their 1938 discussions. For the moment, I will put to one side the first claim that 'these machines are *humans* who calculate'; I will give a rigorous interpretation of this in §3.

Unsurprisingly, Wittgenstein wants to interpret Turing's results in the form of *games*. We cannot be sure exactly what 'the racing game' refers to. Let's suppose it is a board game. We receive the instruction, by, perhaps, picking up a card that says 'go in the same way'. But, as it stands, this makes no sense; the order cannot be followed. The order cannot be followed because it only makes sense in certain positions. This particular instruction clearly only makes sense *if you are already doing something.* It can be read as: 'continue as you are'.

It is initially difficult to see how this is an expression of what Turing says, as Wittgenstein is suggesting. This is, in fact, a reference to Turing's application of the diagonal procedure in the first proof of *Computable Numbers.* More specifically, this is an expression of the behaviour of Turing's machine **H**—the contradictory machine that was supposed to compute the positive diagonal sequence over the computable numbers. Recall, the machine failed to compute the diagonal sequence because eventually it was fed its own D.N. as an input. Machine **D** runs through the natural numbers testing whether each is a satisfactory D.N.; for each natural number which is a satisfactory D.N., its sequence will be computed, corresponding to its place n on the list of computable sequences, up to its n-th digit. The instructions for **H** were thus to copy the outputs of *other* machines. The problem is that this sequence is not, in fact, computable. **H** eventually will stop printing when it is fed its own D.N. When computing its own n-th digit its instructions are: 'print the same as **H**'—that is, print the digit you are supposed to print. Absent some extra instruction, this cannot be followed. It has 'got into a circle', as Wittgenstein puts it.

This reference is quite cryptic. However, the subsequent remark elucidates this:

> A variant of Cantor's diagonal proof: Let $N = F(k,n)$ be the form of the law for the development of decimal fractions. N is the n-th decimal place of the k-th development. The diagonal law then is: $N = F(n,n) = Def. F'(n)$.
> To prove that F'(n) cannot be one of the rules $F(k,n)$. Assume it is the 100th. Then the formulation rule of
>
> $F'(1)$ runs $F(1,1)$
>
> of
>
> $F'(2)$ $F(2,2)$ etc.
>
> But the rule for the formation of the 100th place of $F'(n)$ will run $F(100,100)$; that is, it tells us only that the hundredth place is supposed to be equal to itself, and so for $n = 100$ it is *not* a rule.
> The rule of the game runs "Do the same as…"—and in the special case it becomes "Do the same as you are doing (RPP 1, §1097).

The original remark in MS 135 contained an extra sentence, which was later deleted:

> I have namely always had the feeling that the Cantor proof did two things, while appearing to do only one (MS 135, 60).[23]

Wittgenstein is here giving his own rendition of a diagonal proof. This requires some spelling out. He defines a general rule [$N = F(k,n)$] that tells us how to generate a list of decimal expansions. This would look something like this:

[23] See Floyd (2012, 36) for the history of the remark. I have followed her translation here.

	n_1	n_2	n_3	n_4	...
k_1	8	2	1	5	
k_2	1	4	6	6	
k_3	3	3	3	7	
k_4	9	1	2	1	
...					

On this diagram, for instance, our rule tell us that for F(2,4), $N = 6$. The positive diagonal sequence is accordingly defined as $F(n, n)$ $[df. F'(n)]$: take the n-th input from the k-th row where $k = n$, as I have highlighted.

Now, Wittgenstein wants to show that $F'(n)$ cannot be one of the rules for $F(k, n)$. That is, the diagonal sequence cannot appear as any k-th row. He shows this by reductio. Assume that $F'(n)$ is the 100th expansion, i.e. $F(100, n) = F'(n)$. The rule for $F(100, n)$ is thus: generate the n-th digit at every n-th place of $F'(n)$:

	...	n_{98}	n_{99}	n_{100}	...
...	...	...	...	...	
k_{98}	...	1	2	2	
k_{99}	...	3	4	9	
k_{100}	...	1	4	?	
...					

The rule ceases to be informative when $n = 100$. The rule says: take your own digit. There is no such digit though as the outputs for $F'(n)$ are defined by the outputs of *other* rules. In this exact position, the instructions are to take an output that is as yet undefined. As Wittgenstein rightly puts it, the general rule says 'do the same as...' but at this position it says 'do the same as you are doing'. The rule becomes circular. In the same way, I could comfortably play a game of chess (as black) by only copying the moves of my opponent, but if I tried to play by only copying my own moves I would not be able to open. The rule cannot accommodate play when used in this way.

The resonance with Turing's diagonal is striking. Of course, this is no accident. This is plainly a reconstruction of Turing's first proof. This is quite an achievement, given the clarity and brevity of Wittgenstein's rendition. He has, in a matter of lines, isolated exactly what 'goes wrong' in Turing's application of the diagonal and indeed any diagonal procedure of this form.

I want to compare Wittgenstein's comments here and those in RFM II. First, note the resemblance between both the behaviour of **H** and Wittgenstein's diagonal rule, and the 'a_{ik}-carpets' I mentioned earlier. Wittgenstein's finitism forced an interpretation of any diagonal sequence in RFM as successively longer, but always finite, outputs based on computations up to the n-th numeral of the n-th sequence in an expansion. This is exactly the behaviour exhibited in **H**, and subsequently Wittgenstein's diagonal. Here, there is no reification or manipulation of infinite extensions.

Further, it is clear that Turing's proof realises Wittgenstein's pursuit in RFM for an intensional diagonal application—that is, a diagonal procedure that does not rely on positing infinite extensions but can be interpreted only in terms of rules. I showed in §1 how Wittgenstein was experimenting with the possibility of such a diagonal procedure, but his remarks were not especially felicitous. Wittgenstein could not see how a diagonal rule could show anything: we already know from its construction that it is of higher order than the rules to generate a given expansion, so we know already it will not appear there (cf. RFM II, §34). In 1938, Wittgenstein did not see clearly how to cash this fact out in terms of a proof.

Compare this to the 1947 remark. Here, Wittgenstein *employs* the fact that the rule for computing a diagonal sequence is of a different kind to the rule for computing a system of expansions. His aim is to show that the positive diagonal rule *is* different from any on a given list. We assume it appears on the list and show this leads to a circle because the diagonal rule is felicitous only in certain positions. Wittgenstein concludes:

> I have namely always had the feeling that the Cantor proof did two things, while appearing to do only one (MS 135, 60).

We should correct this by deleting 'always'. From what I have shown, it is clear that Wittgenstein did not understand that there was a felicitous non-extensional interpretation of Cantor's proof in 1938. Sometime between 1938 and 1947 though, probably resulting from conversations with Watson and Turing himself[24], Wittgenstein was able to formulate an alternative approach to the diagonal procedure. He was hence able to infer that Cantor's proof shows *two* things. First, the diagonal method shows that a given diagonal extension is different from the extensions in a given list because it must differ with all of them by at least one place. Second, and more interestingly for Wittgenstein, it shows us something about rules. The method shows that the rule for computing a positive diagonal sequence cannot appear as a rule for computing an expansion in certain systems. This shows that some rules, although everywhere defined, surprisingly make sense only in certain positions.

I take this final part as key. I interpret this thought as similar to a key insight from *Computable Numbers*: the fact that definability is not identical with, and does not entail, computability. One striking result from the first proof of *Computable Numbers* is the apparent dissonance uncovered by completely (and constructively) defining a sequence that is not computable. Failing to heed this distinction is what accounted for the faulty reasoning in Turing's interlocutor, who claims the computable numbers are non-denumerable. Even though we can define the instructions for computing each digit of the diagonal sequence, it is not computable. This is true of any description number encoding the solution to an undecidable problem.

It seems Wittgenstein, after abstracting from Turing's proof, has concluded this fact more generally as a feature of certain rules: they are only informative in certain positions. Even though, *prima facie*, a rule seems to 'reach to infinity', it is sometimes the case that we cannot follow it. Not all scenarios are circumscribed by the rule—that is, attempting to follow it results in receiving nonsensical commands. A rule may be everywhere defined, yet there can be some positions where we cannot follow it. This is an interesting feature of certain rules, and our ability to follow them.

It is clear now why Wittgenstein's disdain for Cantor's proof regarding uncountability is not replicated for Turing's. The application of the diagonal procedure in the latter case was in no way 'puffed up'. That is, Turing does not confer meaning upon his calculus by means of confused verbal expression. In fact, Turing heeds Wittgenstein's advice in RFM II: he starts with a basic, intuitive notion of *computable* and constructs a calculus to represent it. He

[24] Given the reference to Watson in RPP 1 §1096.

then constructs a proof in this calculus that informs us on the intuitive concept. Turing lets his "calculation illumine the meaning of the expression in words"; thus, "the calculation sheds a brilliant light on the verbal expression" (RFM II, §7). Turing's formulation aligns exactly with Wittgenstein's core philosophy of mathematics. It embodies his tenet that mathematics is merely a calculus that we invent in order to reason about intuitive concepts. Granted, Turing in this case does not explicitly use his results to reason about *extra-mathematical* propositions as Wittgenstein might have hoped. This can easily be done by extending Turing's results to claim, say, that there is no computer program which will determine for an arbitrary computer program and an input whether that program will ever finish running—this problem is often called 'the Halting problem' and its unsolvability is derivable from Turing's results. Turing's method captures exactly how Wittgenstein pictured *doing* mathematics. Further, his calculi—Turing machines—being models to capture the intuitive notion of an algorithm, encapsulate Wittgenstein's notion of a mathematical calculus. Recall, Wittgenstein thought that:

> Mathematics is always a machine, a calculus. The calculus does not describe anything. [...] The calculus is an abacus, a calculator, a calculating machine (WVC, 106).

It seems Turing's paper embodies this first thought quite literally. The activity of a Turing machine i.e. a step-by-step mechanical manipulation of signs, is precisely how Wittgenstein describes mathematical calculation.

Of course, Turing's diagonal procedure also avoids the other pitfalls Wittgenstein associates with Cantor's proof. Turing's formalism does not require a choice between logics (e.g. classical, intuitionistic etc.). His proofs do not require us to posit infinite extensions in the way Wittgenstein resisted, and there is no recourse to higher-order infinities. As such, Turing's proof omits the 'hocus pocus' that Wittgenstein critiqued in Cantor.

2.3 Floyd's Account

Floyd (2012) reconstructs Wittgenstein's diagonal remark in rigorous detail. Floyd is keen to highlight the generality of Wittgenstein's argumentation. Wittgenstein has not given an application of the diagonal procedure, but the "general *form* of diagonal argumentation" (2012, 36). This is because Wittgenstein has formulated his diagonal in terms of abstract rules, so its specific inputs do not affect the proof. Read in this light, we can say that Turing's proof is an *ex ante token* of Wittgenstein's general proof. **H** is a machine manifesting the rule $F(n, n)$. The machine halts when its instruction is 'do the same as you are doing'. For this reason, Floyd later coined a nickname for **H**—'The Do-What-You-Do Machine' (2017, 130).

My reconstruction of the diagonal remark is similar to Floyd's. To my knowledge she was the first to bridge the gap between Wittgenstein and Turing via this remark. I agree with Floyd's reconstruction of the diagonal entirely. However, there are a few minor amendments required for her subsequent conclusions to be convincing.

First, Floyd is keen on establishing mutual influence between Turing and Wittgenstein, but does not explicitly connect this remark back to Wittgenstein's problems in RFM II. I think that the contrast between Wittgenstein's approach to the diagonal between 1938 to 1947 is as good an example as any of Turing influencing Wittgenstein. Wittgenstein was highly suspicious of the diagonal method, even when applied only with reference to rules. He was unconvinced that it had any meaningful application in RFM II. By reading Turing, however, he was able to derive meaning from an alternative diagonal procedure that chimed with his prevailing philosophy of mathematics. From this, he drew meaningful conclusions regarding features of certain rules and our ability to follow them. This marks a notable turn in Wittgenstein's philosophy of mathematics. I will return shortly to the question of influence.

Second, there are some misleading technical errors in Floyd's account that misrepresent the generality of Wittgenstein's achievement. For instance, Floyd claims:

> [Wittgenstein's diagonal argument] had a legacy. Wittgenstein was later credited by Kreisel with "a very neat way of putting the point" of Gödel's use of the diagonal argument to prove the incompleteness of arithmetic, in terms of the empty command, "Write what you write" (2012, 36).

This is not quite right. In brief[25], Gödel identifies a proof predicate, call it $Prov(a\ b)$, which is true iff a is the number of a proof of the formula with number b.[26] We also have $s(a, b)$: a function symbol for the (primitive

[25] For now, I will only give an outline of the first theorem. This involves appeal to notions such as primitive recursiveness, which I will not explicate until §3.

[26] 'Number' here means 'Gödel number', that being the unique natural number that is assigned to each expression and each sequence of expressions within a formal system (similar

recursive) function that maps each ordered pair given by (the code of) a formula a with one free variable and a number b to the (code of the) sentence that results from substituting the free variable in (the formula coded by) a with the numeral of b. Consider the matrix:

$$\begin{array}{ccc} (\exists y)\ Prov[y, s(0,0)] & (\exists y)\ Prov[y, s(0,1)] & (\exists y)\ Prov[y, s(0,2)] \ldots \\ (\exists y)\ Prov[y, s(1,0)] & (\exists y)\ Prov[y, s(1,1)] & (\exists y)\ Prov[y, s(1,2)] \ldots \\ \ldots & \ldots & \ldots \end{array}$$

The (positive) diagonal sequence is:

$$(\exists y)\ Prov[y, s(0,0)],\ (\exists y)\ Prov[y, s(1,1)],\ (\exists y)\ Prov[y, s(2,2)]\ldots$$

From it, we can define a diagonal formula $(\exists y)\ Prov[y, s(x,x)]$ and an *anti*-diagonal formula $\neg(\exists y)\ Prov[y, s(x,x)]$. Let m be the code of the latter. Then we have the following true identity:

$$s(m,m) = \ulcorner \neg(\exists y)\ Prov[y, s(m,m)] \urcorner$$

This is because $s(m,m)$ is—by definition—the code of the result of substituting, in the formula whose code is m (i.e. $\neg(\exists y)\ Prov[y, s(x,x)]$), the free variable with the numeral of m. Therefore, since $\neg(\exists y)\ Prov[y, s(m,m)]$ says of the formula coded by $s(m,m)$ that is not provable, it says so of itself.

This establishes undecidability because neither this sentence nor its negation is provable in the systems Gödel considers.[27] If the sentence were provable, the system would be inconsistent because we could derive its negation. If it were disprovable, the system would be ω-inconsistent because $(\exists y)\ Prov[y, s(m,m)]$ would be provable yet there is no natural number n that will satisfy $Prov[n, s(m,m)]$.[28] Therefore, assuming the system in which we construct the sentence is ω-consistent, it cannot also be complete.

In discussing the $Prov(a\ b)$ predicate, Kreisel footnotes:

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in kind to Turing's D.N's for machines). For each code ϕ, its numeral is denoted $\ulcorner\phi\urcorner$. For any number p, its numeral ($\underline{p}$) is 0 followed by p-many strokes i.e. $\underline{1} = 0'$, $\underline{2} = 0''$, etc.

[27] Systems must be recursively axiomatisable and have a certain amount of expressive power. The weakest system usually considered is Robinson arithmetic (denoted as **Q**)—a fragment of Peano Arithmetic. These incompleteness results apply to any extension of **Q** presuming it is recursively axiomatisable and ω-consistent.

[28] If there were such an n, we could prove the original sentence and the system would be simply inconsistent.

> A very neat way of putting the point is due to Prof. Wittgenstein: Suppose we have a sequence of rules for writing down rows of 0 and 1, suppose the *pth* rule, the diagonal definition, say: write 0 at the *nth* place (of the *pth* row) if and only if the *nth* rule tells you to write 1 (at the *nth* place of the *nth* row); and write 1 if and only if the *nth* rule tells you to write 0. Then, for the *pth* place, the *pth* rule says: write nothing! (1950, 281n).

This is quite a neat way of putting Gödel's proof. Gödel has isolated a sentence within systems that their rules cannot prove: if we try to derive a Gödel sentence, the rules say 'write nothing'. This also shows that Wittgenstein's variant diagonals were not restricted to his manuscripts: he was discussing them with, amongst others, Kreisel.

Notice though, this is not the same as Wittgenstein's rendition of Turing's diagonal, which computes the *positive* diagonal sequence. Kreisel goes on to quote this diagonal—the one which says 'write what you write'—but *not* as an explication of Gödel's diagonal application. Floyd's claim gives the impression that Wittgenstein has identified a common diagonal form present in Turing and Gödel's proofs. Although these proofs are obviously highly related, Floyd's presentation here is misleading. Gödel and Turing's diagonal arguments take different forms: the former identifies a sentence (via an *anti*-diagonal sequence) that says of itself that it is not provable; the latter involves attempting to compute a *positive* diagonal sequence before reaching an unfollowable instruction.

2.3.1 The Path to Computable Numbers

Floyd claims that Wittgenstein was instrumental in the inception of the content and ideas behind *Computable Numbers.*[29] I cannot find any convincing evidence for this:

Turing went up to Cambridge in 1931 to read for the mathematical tripos (Hodges 2014, 78). After graduating, he was then elected a fellow in 1935 (2014, 121). In the spring of 1935, he attended lectures by Max Newman on

[29] I attended a lecture by Floyd in Kirchberg (2018) in which she went as far to claim that "without his study of philosophy with Wittgenstein...Turing would not have designed the Turing Machine". Unfortunately I cannot find this claim replicated in publication.

the foundations of mathematics. These marked Turing's entry into mathematical logic, running through the Hilbertian problems, Russell's logicism, finishing with proofs of Gödel's theorems (2014, 116-117). By the early summer of that year, lying down on Grantchester meadows during a long run, Turing saw the answer to Hilbert's *Entscheidungsproblem* (2014, 123). In his lectures, Newman phrased the problem in terms of a "mechanical process"; it was reflecting on this that led Turing to envisage machines (ibid.).

Typically for him, Turing worked entirely on his own, not once discussing the idea for his machines even with Newman (Hodges 2014, 139). He shared a few words with Braithwaite on Gödel's theorems at King's high table, asked Watson about the nature of Cantor's diagonal, and briefly explained the idea of the universal machine to his close friend David Champernowne. This was all. His work then consumed him until April 1936, when he handed a draft typescript to Newman (ibid.).

Wittgenstein had been back in Cambridge since 1929 (Monk 1991, 255). He had realised that there were fundamental problems in the *Tractatus* and was now advancing his new method for philosophy. In the early thirties, Wittgenstein was thinking intensely about mathematics and its foundations. During Turing's undergraduate years Wittgenstein gave two series of lectures in Cambridge (1932-33) entitled simply 'Philosophy' and 'Philosophy for Mathematicians' (Monk 1991, 328). The latter consisted in uprooting even the most commonly held assumptions in mathematics, one of these being that mathematics had any *foundation* in logic (ibid.). In the next academic year 1933-34, Wittgenstein cancelled the lectures and instead dictated them to his favourite five students. Their notes were collated and bound, and then distributed amongst students (Monk 1991, 336). These have been known since as *The Blue Book*.

Now, one might expect that Turing had attended 'Philosophy for Mathematicians'—it had undergraduate mathematicians as its target audience. There is no evidence that he did. Nor is there evidence that Turing ever read *The Blue Book.* Given this, I find the chances of Wittgenstein having had any influence on the content of *Computable Numbers* very slim. The two did not meet formally until after the publication of *Computable Numbers* in 1937, when Watson introduced them. From the above evidence, it would seem unlikely that Wittgenstein influenced Turing's paper.

Let's analyse Floyd's argument. Floyd's first piece of evidence is that in 1933, Turing acquired a copy of Russell's famous *Introduction to Mathematical*

Philosophy (1919). In the final chapter 'Mathematics and Logic', Russell mentions Wittgenstein and his early work on notion of tautology, saying:

> The importance of "tautology" for a definition of mathematics was pointed out to me by my former pupil Ludwig Wittgenstein, who was working on the problem. I do not know whether he has solved it, or even whether he is alive of dead (1919, 206).

Floyd argues that the notion of tautology would not have escaped Turing here:

> Turing would import the use of a tautology-like construction into the heart of his argumentation in 'On Computable Numbers'. Turing would thus vindicate Russell's suggestion, drawn from Wittgenstein in 1918, that appeal to the laws of contradiction, excluded middle, and bivalence are no longer sufficient as a basis for an analysis of logic, whereas the idea of an empty, senseless, repetitive remark 'saying the same thing over again' (*tauto- logoi) is* (2017, 114).

What Floyd is suggesting is that this one-sentence snippet had some causal effect on the diagonalisation argument in *Computable Numbers*. 'Saying the same thing over again' here is an allusion to Turing's machine **H,** which comes across an empty rule—the 'Do-What-You-Do Machine'. In his proof, Floyd argues:

> [Turing] adapts Wittgenstein's notion of a tautology, central to the philosophy of logic at Cambridge since the *Tractatus* (ibid.).

This analogy between Wittgenstein's characterisation of tautology in the *Tractatus*-era and Turing's machine **H** is not especially cogent. Granted, Wittgenstein describes tautologies as empty: "they say nothing"—"they lack sense" (TLP 4.461). This is because the rule will always return true regardless of the way the world is. But machine **H** is not empty in *this* way. It better befits an analogy with a 'truth teller': a sentence that says of itself that it is true. It may either be true or false, but as nothing determines its truth-value we have no way of settling the matter.

Floyd is claiming that Turing has 'vindicated' Wittgenstein's notion that appeal to the laws of contradiction and excluded middle are now insufficient. Turing does not argue this anywhere nor even mention these concepts. The paper is consistent with the notion in that Turing machines are neutral between formal systems—they do not require a choice between logics—but there are obviously independent motivations for this aside from anything Turing knew about Wittgenstein. In any case, Floyd claims that these thoughts are drawn from one sentence of Russell which was read two years before Turing had the idea for *Computable Numbers.* This is not plausible.

Floyd continues:

> But why would Turing have been reading Russell at all? Two possible answers present themselves (2017, 114).
>
> The first may be gleaned from Turing's annotated copy of Littlewood's *Elements of the Theory of Real Functions* (1926). In his preface, [...] conceding some readers might be interested in the foundations of mathematics, he recommended that the reader consult Russell's Introduction to Mathematical Philosophy [...] So it is plausible to suppose that Turing turn toward logic in the spring of 1933, during his second undergraduate year (ibid.).
>
> There is a second possible answer as to why Turing would have been reading Russell in the spring of 1933. Wittgenstein's course "Philosophy for Mathematicians" was given 1932-1933 (Turing's second undergraduate year) [...] It may have influenced Turing (directly or indirectly), drawing him toward logic and foundations of mathematics. (2017, 115).

This, I think, is a false dichotomy. Turing reading Russell requires no substantive 'reason' whatsoever. Russell was one of the most famous mathematician-philosophers of the day, if not ever. His work was renowned worldwide, and of course Russell would have been well known and often discussed in the Cambridge mathematics faculty of Turing's time. It would be surprising if Turing *had not* read Russell. This is not convincing evidence of Wittgenstein influencing the ideas in *Computable Numbers.*

There is also no evidence that Turing attended 'Philosophy for Mathematicians'. Even if he did, Floyd's argument would require that Turing read Russell due to Wittgenstein's influence then, taking particular note of the ideas surrounding Wittgenstein's one-sentence mention in the *Introduction*, working these ideas into the heart of *Computable Numbers* to vindicate Wittgenstein's earlier views on logic and mathematics. Hopefully it is evident that this is far-fetched. Floyd's contention that Turing's "basic move was to utilise...the method of a 'language game'" remains quite unsupported (2017, 109). The notion of a language game was introduced in

‘Philosophy’, but does not appear at all in ‘Philosophy for mathematicians’ (cf. AWL). There is no plausible way Turing would have heard of language games before *Computable Numbers.* They do appear in the *Blue Book,* of course, but there is no reasonable basis on which to argue that Turing had read this. Wittgenstein’s influence on *Computable Numbers* goes no further than in the indirect sense in which Wittgenstein may have contributed to a culture of thinking about logic at Cambridge, which Turing later entered. Whilst Floyd is clear on what Wittgenstein is supposed to have added to *Computable Numbers*, her evidence is very slim and ultimately unconvincing.

2.4 Taking Stock

Before moving forward to the final section, where the focus will shift towards the Church-Turing thesis, it is worth recapitulating what I have shown so far. I have so far given my arguments for a) and b) in my introduction, and some of my arguments for c):

a) Wittgenstein read, understood and engaged with *Computable Numbers.*
b) Wittgenstein’s remarks on this topic are highly perceptive and have pedagogical value, shedding light on Turing’s work.
c) Wittgenstein was highly supportive of Turing’s work as it was indicative of Wittgenstein’s prevailing approach to logic and mathematics.

I hope that a) is evident now. Wittgenstein makes a clear reference to *Computable Numbers* in the 1947 remark (RPP I, §1096). He was not only aware of the paper, but gave it careful study, particularly its first proof and the application of the diagonal procedure. Wittgenstein ran through its argument and reframed it in his own terms—as a game. He used Turing’s proof as a prop out of which he derived new remarks on the nature of a rule, and our ability to follow them.

Following from a), I have also argued for the qualitative judgement about Wittgenstein’s remarks in b). In (RPP I, §1097), Wittgenstein reconstructs the general form of the diagonal application in Turing’s proof. His rendition does justice to Turing’s proof; I have shown that Turing’s machine **H** is a token of Wittgenstein’s general diagonal rule $F'(n)$. In a matter of lines, Wittgenstein diagnosed what goes wrong when computing the positive diagonal sequence across the computable numbers. The brevity and clarity of the remark surely have pedagogical value relative to the complexity of

Turing's paper. Further, by Kreisel's admission, it seems Wittgenstein made a point of discussing this diagonal proof elsewhere. He also had an equally simple rendition of the form of diagonal argumentation in Gödel's first incompleteness theorem.

This in itself supports my claim in c). Wittgenstein was not dismissive of Turing's work but enthusiastic. This much is clear from the fact he was still thinking about it a decade after the publication of *Computable Numbers.* What is more, Wittgenstein thought that Turing's work proved valuable not only in itself, but as an application to his own philosophy. He learnt from Turing that bare instructions are not enough to fully describe a rule; we also need contextual facts vis-à-vis our position in the game, as it were. It also seems that Wittgenstein changed his stance on the diagonal technique after reading Turing. I showed that in 1938 Wittgenstein explored the diagonal technique understood in only anti-extensional terms. He struggled to see how the technique could be informative in this way and was doubtful in 1938 that any use could be got from the diagonal technique. Sometime before 1947, Wittgenstein realised the impact that Turing's proof had on this prospect. He subsequently changed the spirit of his approach to the diagonal method, claiming he thought it showed two things while appearing to do only one.

I cannot be sure of when exactly Wittgenstein's change of heart towards the diagonal technique occurred; it was quite possibly a long time before 1947. It is somewhat surprising that these ideas are not fully formed in 1938, given his discussions with Turing and Watson. It seems Wittgenstein was still developing his approach, which was only realised in text by 1947. In any case, there is a clear change in mood between the arguments laid out in RFM II, in which the diagonal technique is met wholly with hostility, and the 1947 remarks, where Wittgenstein, abstracting from Turing, draws enthusiastic conclusions from it.

Turing's results are, in many ways, indicative of Wittgenstein's prevailing philosophy of mathematics. Turing's proof avoids having to posit infinite extensions, and reflects Wittgenstein's tenet that rules are essential to mathematics. Further, the Turing machine captures the abacus-, machine-like nature of calculation that Wittgenstein championed, portraying mathematics as a mere *algorithmic* manipulation of signs.

However, this fact is purely circumstantial. That is, I want to resist the suggestion that Wittgenstein directly influenced any of the ideas in *Computable Numbers.* We must not understand Wittgenstein's endorsement of Turing's proofs as being somehow triggered by the recognition of his own contribution. *Computable Numbers* is overtly original; its methods are highly general, and palatable to a broad mathematical audience—even Wittgenstein. The fact that Wittgenstein found them useful for his own purposes is a testament to the generality of Turing's methods. It is not indicative that the ideas originated from Wittgenstein himself.

In §3 I will give further evidence for c) by offering an interpretation of the first claim in RPP I §1096—'Turing's 'Machines'. These machines are *humans* who calculate'. I take this as a clear endorsement of the Church-Turing thesis, in particular Turing's rendition, once it is spelled out and placed in its historical context. I will then present an argument for:

d) Adopting a Wittgensteinian approach to Turing's proofs enables us to answer live problems in the modern literature on computability.

3 Computability—Wittgenstein and the Church-Turing Thesis

3.1 Humans Who Calculate

The Church-Turing thesis (hereafter CTT) has been heavily discussed in the literature on computability and is relevant even in on-going scholarship. I have two principal objectives in this section:

After giving a landscape of the thesis and the relevant debates, I will first argue that Wittgenstein himself endorsed Turing's rendition of the CTT. I will do so in opposition to arguments from Stuart Shanker, who claims that Wittgenstein objected to Turing's conception of the CTT and found it steeped in linguistic confusion. This is incorrect. Shanker's evidence turns heavily on Wittgenstein's comments on mechanism. Conflating the CTT with arguments concerning mechanism is a common fallacy. Wittgenstein's remarks on the CTT are perceptive and endorse Turing's own exposition.

Second, I will look at the status of the CTT moving forward. As mentioned, there are still several live debates related to the CTT. These typically study its truth and provability, or analyse exactly what type of claim the CTT actually is (a definition, consequence of induction, etc.). I hope to put at least some of these questions to bed. As far as I know, Wittgenstein himself never contributed to these debates substantively—evidence of Wittgenstein's attitude towards the CTT is reducible to just one sentence in the *Nachlass*. However, adopting a Wittgensteinian approach to the CTT yields interesting results. This, I will argue, gives a clear picture of the CTT and its status today. I will use this approach to give a case for the universal truth of the CTT. If I am correct, the Wittgensteinian approach to mathematics sheds light on modern debate on computability. The finale to this paper is thus a hymn to Wittgenstein scholarship, and a humble justification of a continued interest in his work.

3.1.1 The CTT at a Glance

There is no unique formulation of the CTT. It is testified in various ways throughout the literature. This is in part historical: it has been updated along with developments in recursion theory.

The thesis is a claim of equivalence between a cluster of informal concepts and a cluster of formal concepts. The informal concepts broadly fall under *effective calculability*. This is a notion inherited from Hilbert. As previously mentioned, a function is effectively calculable if there is some finite, definite method for its calculation i.e. an algorithm (intuitively understood). *Definite* here is understood as a *step-by-step* process in which every step is capable of being followed mindlessly. J.B. Rosser characterises an effective method quite neatly as a "method each step of which is precisely predetermined and which is certain to produce the answer in a finite number of steps" (1939, 225).

I will call the cluster of formal concepts *formal computability*, which (we now know) are all equivalent to *recursiveness*. The CTT began as simply CT—Church's thesis. Church proposed it as a definition of effective calculability, by identifying the notion with recursive functions of positive integers (1936, 100). He then parenthetically adds: "or a λ-definable function of positive integers", which is Church's formalism, equivalent to recursiveness (ibid.).[30]

Thus, the original CT equates effective calculability on the one side, and recursiveness and λ-definability on the other. Recall, late 1936 saw the publication of *Computable Numbers* and the introduction of Turing-computability. This added a further formal notion to the equivalence, and turned the CT into the CTT.

The CTT underpins all of the results showing that the *Entscheidungsproblem* has no solution. This should be fairly apparent. The *Entscheidungsproblem* demanded an effective procedure by which one could decide whether or not any given statement of Hilbert's restricted predicate calculus was provable. To show this is *not* possible, both Church and Turing designed formalisms that they argued could model *any* effective procedure, and proved that there is no procedure in the formalisms solving the *Entscheidungsproblem*. Therefore, there is no effective procedure to solve the *Entscheidungsproblem*. Of course, the proofs depend on whether these formalisms successfully capture all possible effective procedures.

Before moving on, I shall give a brief account of recursiveness. A basic grasp of recursiveness (and how it captures effective calculability) is essential to understanding the effect that Turing-computability had on the CTT.

[30] This was proved by Stephen Kleene (with the help of Church and Rosser) cf. Kleene (1936).

3.1.2 Recursiveness

There are several ways of presenting recursiveness; for continuity, I will again broadly follow Boolos et al. (2007, Ch7). The recursive (general recursive, or recursively computable) functions are a class of functions (partial and total) from finite tuples of natural numbers that return one natural number. The class of recursive functions arrived as an extension of the primitive recursive functions. These were the functions Gödel employed in his incompleteness proofs in 1931. As before, let's adopt a notation such that the numeral for any integer n is 0 followed by n strokes: $\underline{1} = 0'$, $\underline{2} = 0''$, and so on. The basic functions are defined as follows:

(I) *The Successor Function*: $s(x) = x'$
(II) *The Zero Function*: $z(x) = 0$
(III) *The Identity/Projection functions*: $id_i^n(x_1, \dots, x_i, \dots x_n) = x_i$

I take the first two definitions as self-explanatory. The identity functions are those that return the i-th argument of any n-ary projection. More primitive recursive functions can be defined by certain operations on the above basic functions; the operators are:

(IV) *Composition*:

$$h(x_1, \dots, x_n) = f(g_1(x_1, \dots, x_n)), \dots, g_m(x_1, \dots, x_n))$$

(V) *Primitive Recursion*:

$$h(x_1, \dots, x_n, 0) = f(x_1, \dots, x_n),$$
$$h(x_1, \dots, x_n, y') = g(x_1, \dots, x_n, y, h(x_1, \dots, x_n, y))$$

Composition is sometimes called *substitution*. We can abbreviate this to $h = Cn[f, g_1, \dots, g_m]$. For example, $Cn[s, z]$ is the function h where $h(x) = s(z(x)) = s(0) = 1$. We can abbreviate primitive recursion to $h = \text{Pr}\ [f, g]$. Put simply, these operations are templates for defining functions in terms of other functions.

A function is *primitive recursive* if it can be defined from the functions (I)-(III) by zero or more successive applications of schemas (IV) and (V).

For example, the addition function is primitive recursive. We can define addition by the following pair of equations:

$$x + 0 = x$$
$$x + y' = (x + y)'$$

These we can express as:

$$sum(x, 0) = x$$
$$sum(x, y') = sum(x, y)'$$

We can express these in terms of (I)-(V) as follows:

$$sum(x, 0) = id_1^1(x)$$
$$sum(x, s(y)) = Cn[s, id_3^3](x, y, sum(x, y))$$

That abbreviates to:

$$sum = \mathrm{Pr}\ [id_1^1, Cn[s, id_3^3]]$$

Clearly, all primitive recursive functions are effectively computable. The basic functions can be computed in one simple step: applying the successor function involves adding a stroke, applying the zero function only requires writing a zero, and the identity function requires counting to some *i*-th argument and returning it. Further, schemas (IV) and (V) preserve effective calculability. For composition, if functions *f* and g_i are effectively calculable then so is *h*. To compute $h(x_1, \ldots, x_n)$ will take the number of steps needed to compute $y_1 = g_1(x_1, \ldots, x_n)$ plus the number of steps needed to compute $y_2 = g_2(x_1, \ldots, x_n)$, and so on, plus the number of steps that compute $f(y_1, \ldots, y_m)$. There is thus a finite, definite list of instructions that can be given to compute any application of composition. Likewise, $h = \mathrm{Pr}\ [f, g]$ will be effectively calculable if f and g are. $h(x, y)$ can be computed in the same number of steps as required to compute $z_0 = f(x) = h(x, 0)$ plus the number required to compute $z_1 = g(x, 0, z_0) = h(x, 1)$ and so on to $z_y = g(x, y - 1, z_{y-1}) = h(x, y)$.

Adding one further operation to the primitive recursive functions, which are total, yields the (general) recursive functions, which can also be partial. For a function *f* of $n + 1$ arguments, *minimisation* gives a total or partial function *h*:

(VI) *Minimisation*:

$$h\left(x_1, \dots, x_n\right) = \begin{cases} y, & \text{if } f\left(x_1, \dots, x_n, y\right) = 0 \ \text{ and for all } t < y \\ & f\left(x_1, \dots, x_n, t\right) \text{ is defined and} \neq 0 \\ \mathit{undefined}, & \text{if there is no such } y. \end{cases}$$

To abbreviate, we say that $h = Mn[f]$. Intuitively, this searches for the smallest argument that causes *f* to return 0; if there is no such argument this continues indefinitely. This will be effectively calculable if *f* is. If *f* is partial, it is intuitively effectively calculable if there is a finite list of definite instructions such that when they are applied to any x in the domain of *f*, they will eventually arrive at the value $f(x)$. If applied to an x not in the domain of *f*, the procedure will continue infinitely with no result. The same goes for many-place functions. Writing x for $x_1, \dots, x_n$, we would compute $h(x)$ by successively calculating $f(x,0), f(x,1), f(x,2), \dots$ stopping if we reach a y such that $f(x,y) = 0$. If x is in the domain of h there will be such a *y*. If it is not, then this process will continue infinitely.

The recursive functions are those that can be defined by zero or more applications of the operations (IV)-(VI) on the basic functions (I)-(III). These are all demonstrably effectively calculable: the basic functions are intuitively effectively calculable and applications of any of the operations preserve effective calculability. The substantive claim in the CT is that the recursive functions (and λ-definability) capture *all* effectively calculable functions. I will assess this claim in detail shortly.

3.1.3 Turing-computability and the CTT

I have so far given a sketch of the CT until it became the CTT. Importantly, *Computable Numbers* did more for recursion theory than simply add another formal notion to the bandwagon. As it stood, there were problems with the CT and it was not entirely convincing. Gödel found the proposal of defining effective calculability in terms of λ-definability "thoroughly unsatisfactory" (Davis 1982, 9). Gödel did not find the λ-calculus a natural counterpart to effective calculability. He was more inclined to accept recursiveness as a formal counterpart, but still only as a *heuristic*.

Church does defend his CT, but the arguments are curious. Church first appeals to the empirical fact that thitherto for every effectively calculable function of positive integers, there was an algorithm for the calculation of its value (1936a, 100). Conversely, every recursive algorithm that inputs and outputs natural numbers is clearly effectively calculable. Although true, this is a curious argument for a mathematical *definition*, which is how Church framed his CT. A definition is an analytic statement; once drawn, this precludes the possibility of an effectively calculable function being found for which there is no algorithm, or vice versa. If there is no algorithm for it, then it is not effectively calculable—by definition. This objection can be avoided if, instead of a definition, we state the CT as a conjecture—a claim of the equivalence of extension of two classes. This, in fact, is how the CT is often read.

Regardless, Church's rendition of the CT still leaves room for doubt. Gödel, notably, was still unconvinced by the thesis even after an equivalence had been proved between recursiveness and λ-definability (Davis 1965, 40). He was not convinced that he had identified the most general form of recursiveness (ibid.). Even though recursiveness seemed to match effective calculability intuitively, he was not convinced that all possible recursion fell into this category. Furthermore, he still had issue with declaring an equivalence between an informal concept and a formal one. Recall, Gödel envisaged any equivalence between recursiveness and effective calculability to be only a heuristic. He thought that no equivalence could be stated "without first showing that "the generally accepted properties" of the notion of effective calculability necessarily lead to this class" (Davis 1982, 13).

As it stood, there was a theoretical gap in the CT. There was no strong connection between the relevant notions, only the evidence that they intuitively seemed to share extensions. This fell short of the rigour required for such an important development.

It was only after reading Turing that Gödel fully endorsed the (now) CTT. Turing's rendition of the CTT equates effective computability to Turing-computability, which is also mathematically equivalent to recursiveness in the sense that the class of recursive (partial) functions coincides with that of the Turing-computable functions. This managed to bridge the theoretical gap between effective procedures and formal computability—for Gödel at least. Turing did this by characterising the latter notion *in terms of* the former, thereby ensuring that the generally accepted properties of effective calculability led to Turing-computability. Turing machines are explicitly justified in §9 of *Computable Numbers* by reference to their analogy with human calculators.

Turing reflects on what is essential to *human* calculation. His machines are limited in calculative power in the same way as humans, other than those related to sluggishness. The machines are built in analogy to human calculation: a human computer's behaviour is determined by the symbols she observes and her “state of mind” at that moment (CN, 136). We may imagine a human running her calculations with a pen across a one-dimensional tape (divided into squares, like child’s arithmetic book), rather than the traditional two-dimensions of lines on paper (CN, 135). We know that human computers may observe only finitely many distinguishable symbols at once; there is some upper bound B to how many symbols may be observed (CN, 136). Observing more requires successive observations. The number of ‘states of mind’ required in a procedure must also be finite. We may imagine the behaviour of a human calculator being broken down into the simplest of steps. Each step involves some change of the physical system consisting of the human and her tape (ibid.). All changes may be split up into steps of this kind, where one symbol is changed at a time.

In analogy to these essential features of human calculation, Turing defines his machines. To each state of mind corresponds an *m*-configuration. Where the human observes squares, the machine scans. All moves on the machine are determined by its *m*-configuration and the symbols it scans. Turing machines are thus idealised human calculators. Any sequence calculable by a human can be broken down into these simple steps. Correspondingly, a machine can be described which allegedly will compute any sequence calculable by a human calculator. This would mean that a human calculator cannot *out-compute* a Turing machine. Conversely, Turing machines are limited such that they can only complete the most basic of operations at any stage. Therefore, they can only compute sequences theoretically calculable by a human (ignoring constraints on sluggishness or time). That is, their computing power is no stronger than an idealised human’s. So it goes.

Turing tried to bridge the gap between the elements of the CT by giving an insight into the central features of effective calculability *as a human enterprise*. Hereafter, I will call Turing’s justification of his machines *qua* abstract human calculators *the Turing analysis*.

Even Gödel would accept that the difficulties of the CT had been overcome by the CTT. In an address to the Princeton Bicentennial Conference, Gödel credited Turing with having “for the first time succeeded in giving an absolute definition of an interesting epistemological notion” (1946, 84). Turing had shown the connection between effective calculability and the notion of recursiveness. Accordingly, the 'generally accepted properties' of the informal notion of effective calculability could be mapped with sharpness and clarity via Turing machines onto their formal counterpart. Further, Gödel credits Turing with having given a thorough analysis of "mechanical procedure" or "algorithm" (1964 Postscriptum to 1934, 72). This proved invaluable to generalising his incompleteness results as it gave a "precise and unquestionably adequate definition of the general concept of formal system" (*op. cit.*, 71). It meant that the existence of undecidable propositions could be proved for *every* consistent, effectively axiomatisable formal system containing a certain amount of finitary arithmetic (ibid.).

3.1.4 Wittgenstein's Analysis

The above considerations are crucial to understanding Wittgenstein's response to the CTT. So far I have given a reconstruction of Wittgenstein's 1947 remarks on *Computable Numbers* in some detail. However, I have thus far neglected the first substantive claim of the remarks:

> Turing's 'Machines'. These machines are *humans* who calculate (RPP 1, §1096).

This is for good reason: the remark is self-standing and distinct from Wittgenstein's analysis of Turing's application of the diagonal procedure. I will analyse it here. This remark shows Wittgenstein's understanding of what a Turing machine actually is—it is therefore a direct comment on the CTT.

Now, this remark (or any other reference to the CTT) is not replicated anywhere else in the Wittgenstein *Nachlass*. Further, it is characteristically pithy and almost certainly written as an epigram. As such, we should be cautious about inferring too much from the remark. Having said this, the remark is highly perceptive and seems to go right to the heart of the developments in recursion theory and computability in the 30s.

Read at face value, Wittgenstein's remark simply notices what is quite explicit about Turing machines (given the Turing analysis)—that they are modelled on idealised abstract human computers. Turing himself would repeat this point: "A man provided with paper, pencil, and rubber, and subject to strict discipline, is in effect a universal machine." (1948, 416).

It is striking, though, that Wittgenstein picks this as his sole characterisation of Turing's machines. It is precisely this feature of Turing machines that allegedly allows the formalism to convincingly capture effective calculability as formal computability, thus proving results related to the *Entscheidungsproblem.* Jack Copeland puts the point well:

> [I]t was not some deficiency of imagination that led Turing to model his logical computing machines on what could be achieved by a human computer. The purpose...demanded it (2000, 11-12).

Wittgenstein identifies Turing's precise *philosophical* move that aimed to connect effective calculability and formal computability. It was this move that put Gödel's worries to rest, allowing him to accept a formal analysis of an informal notion. Only a direct model of human calculation could successfully capture all of the essential characteristics of effective calculability. Before Turing, there was an equation of two classes of extensions between which there was no explicit connection other than a lack of evidence for falsification and a weak intuitive appeal. Turing's machines *qua* abstract human calculators gave a non-*ad hoc* philosophical argument that (purportedly) led from the most essential characteristics of calculation to a formal notion equivalent to recursiveness. That Wittgenstein should identify this feature of Turing's machines (and *only* this feature) shows a remarkable depth of understanding. Such a remark can only be read as an endorsement of the CTT—Wittgenstein is affirming the very aspect of Turing's machines which accounts for the supposed convincingness of the CTT: that they are abstract humans.

There is no hint of objection. Rather, this philosophical move manifests the motto Wittgenstein would later advocate in RFM II: in 1936, the Princeton group were focussing their efforts solely on the formal side of the equivalence in the CTT whilst Turing did what was required to tackle the *Entscheidungsproblem*—"take a wider look round" (RFM II, §6).

This further endorses my contention that Wittgenstein took an active interest in Turing's work. Alongside the remark on Turing's application of the diagonal process, this remark on the CTT occurs over ten years after the publication of *Computable Numbers.* Similarly to Wittgenstein's diagonal remark, this comment draws out what is essential in Turing's work. This remark, once fully worked out, identifies the breakthrough idea in Turing's logic. It was his step outside of mathematics to analyse what *effective calculability* really meant that led to a galvanised CTT.

Wilfried Sieg puts my point quite neatly: the opening line of Sieg (1994) quotes the *humans who calculate* remark. He then begins:

> Wittgenstein's terse remark captures *the* feature of Turing's analysis of calculability that makes it epistemologically relevant (1994, 71).

I would echo this sentiment. Sieg uses the remark chiefly as a gobbet to introduce his discussion of Turing and mathematical experience. Its employment as such should serve as further evidence to my defence of the pedagogical value of Wittgenstein's remarks on Turing.

3.1.5 Shanker, Wittgenstein and Mechanism

As Wittgenstein affirmed the very premise that separates the CT from the CTT, I have argued that this shows Wittgenstein's endorsement of the CTT. However, I did also point out the epigrammatic nature of this, the only reference in the *Nachlass* to the CTT. As such, I will not labour my argument beyond the scope of the evidence available. Clearly one sentence cannot give sufficient substance for a knock-down argument as to Wittgenstein's approach to the CTT.

On this score, I would like to offer criticism against Stuart Shanker (1987b, 1998): he takes the *humans who calculate* remark as a full-blown objection to the CTT. Interestingly, both Floyd (2012) and Sieg (1994) note their opposition to Shanker's interpretation, but neither offers any sustained critical analysis of it. To be sure, if correct, his arguments pose a major problem for the thesis I am proposing. It also directly contradicts Sieg's reading, for which Floyd expresses sympathy (2012, 27). Shanker's interpretation pictures Wittgenstein and Turing at odds on *the* fundamental contention of *Computable Numbers*. This patently contradicts the kernel of my agenda.

Shanker's argument turns on Wittgenstein's famous opposition to the Mechanist thesis—the proposition that machines can or could think. Turing famously endorsed the Mechanist thesis. Shanker argues that Turing derived this from his computability results:

> One of the central points that Turing made in his 1947 'Lecture to the London Mathematical Society' was that the Mechanist Thesis is not

> just licensed by, but is in fact entailed by his 1936 development of CT (1998, 1).

Shanker lays this against Wittgenstein's refutation of the Mechanist thesis. He correctly draws out Wittgenstein's approach: saying of a machine that it 'thinks' would transgress the rules of logical grammar. It betrays what we *mean* when we use the word 'think':

> 'A machine thinks (perceives, wishes)' seems somehow nonsensical. It is as though we had asked 'Has the number 3 a colour? (BB, 47).[31]

On this basis he argues that for Wittgenstein, *Computable Numbers* represents "a misguided attempt to integrate independent issues in mathematical logic and the philosophy of mind" (1998, 3):

> Wittgenstein objects that the mathematical and philosophical strands in 'On Computable Numbers' are not just independent of one another but, indeed, that the epistemological argument misrepresents the mathematical content (1998, 4).

Shanker fleshes out his argument by reference to a passage where Wittgenstein reflects on calculating machines in RFM V:

> Does a calculating machine *calculate*? Imagine that a calculating machine had come into existence by accident; now someone accidentally presses its knobs (or an animal walks over it) and it calculates the product 25×20. I want to say: it is essential to mathematics that its signs are also employed in *mufti*. It is the use outside mathematics, and so the *meaning* of the signs, that makes the sign-game into mathematics. (§2)

The point here, for Shanker, is that a 'calculating machine' does not, in fact, calculate. What we call 'calculating' requires a host of normative concepts. We do not calculate simply because it makes interesting patterns. Mathematical signs are employed in *mufti* to reason about everyday propositions.

Shanker argues that these normative conditions are not applicable to Turing machines (them being 'calculating machines'). Thus, for him Wittgenstein's *humans who calculate* remark argues that Turing machines are not calculating

[31] See also (PG, 105).

machines at all. Wittgenstein argues that "if calculating looks to us like the action of a machine, it is the *human being* doing the calculation that is the machine" (RFM IV, §20).

Shanker argues that Turing's conception of mechanical calculation requires that "the rules of calculation have been broken into a series of *meaningless* sub-rules, each of which is devoid of cognitive content, and for that reason are such that a 'machine could carry it out'" (1998, 10). *This* is Turing's fallacy. Machines cannot calculate *precisely because* they act mechanically. To divide steps into meaningless sub-rules devoid of cognitive content is to violate the grammar of calculation. To Wittgenstein (and Shanker), *meaningless rule* is an oxymoron because rules must be followed normatively. Turing's CTT hence makes a conflation between following a mechanical 'rule', and following a rule mechanically. Machines follow mechanical processes, but this cannot be said to be rule-following because if each step is devoid of cognitive content then the essential normativity of calculation is lost. Conversely, humans may follow certain rules mechanically i.e. without thinking, but this does not involve merely following a mechanical process like a machine: a human may at any stage justify her actions by reference to the rule (1998, 31).

This feeds into Wittgenstein's alleged contention that Computable Numbers is a hybrid paper, confusing mathematical and epistemological concepts:

> The crux of Wittgenstein's response to Turing's interpretation of the epistemological significance of his mechanical version of CT is that the only way Turing could synthesise these disparate elements was by investing his machines with cognitive abilities *ab initio*: that is, by assuming the very premise which he subsequently undertook to defend. (1998, 32).

In other words, Turing's paper is steeped in confusion because his conclusions require that his machines calculate: they are, of course, intended as a model of calculation. However, Wittgenstein has shown that Turing's notion of algorithm precludes *bona fide* calculation because calculation is an essentially normative enterprise. Turing defines algorithms in terms of meaningless sub-rules devoid of cognitive content; the only way to bridge this gap was for Turing to inadvertently bestow cognitive abilities onto his machines, making them not calculating machines but *humans who calculate*. It was this characteristic that Turing went onto defend (via the Mechanist thesis) which he claimed was entailed by his formulation of the CTT. Thus, Wittgenstein's remark that Turing's machines are really humans who calculate in fact shows that Turing's CTT-*cum*-Mechanist-thesis begs the question.

3.1.6 Critique of Shanker

Recalling my remark about the lack of evidence on this debate (and my subsequent caution about over-interpretation), I would emphasise at this point that the above interpretation of Wittgenstein's approach to the CTT is based entirely on the sentence: 'Turing's 'Machines': these are humans who calculate'. The other quoted passages serve only as reference for Wittgenstein's opposition to the Mechanist thesis.

As it happens, Shanker's argument turns on fallacy. If we are careful about picking apart what the CTT entails and what it does not, this becomes clear. Overstating the scope of the CTT is highly common in the literature; Copeland labels this the *Church-Turing fallacy* (1998, 133). Usually this involves overstating the scope of Turing-computability.

It seems that Shanker has committed such a fallacy (although not one Copeland identifies). The claim Shanker makes is that Turing thought the CTT, a claim stating an equivalence between idealised human calculation and Turing-computability, entails that real machines can think. Shanker repeatedly claims that Turing himself was to "insist" this point in his 1947 'Lecture to the London Mathematical Society' (1998, 14). Despite its frequency, Shanker rehearses this claim without citation or quotation. Nor does he explain how such an argument would actually work—the possibility of machine thinking certainly does not trivially follow from the CTT.

I cannot find any such claim in Turing's lecture. This is hardly surprising: it would be fallacy.

Shanker's chapter includes a lengthy discussion of Gödel's response to deriving mechanism from the CTT. This seems misplaced to me. There are two senses in which 'mechanism' can be interpreted. Shanker uses it to denote an endorsement of the proposition that machines can/could think. Let's call this mechanism$_1$. There is another sense, which I take as the standard usage, which I shall dub mechanism$_2$; this is the proposition that human thought is entirely mechanical, or reducible to mechanical procedures i.e. that the extension of *thought* is a subset of mechanical calculation. There is yet another: let's call it mechanism$_3$—the claim that human cognitive capacities are bound by those of a Turing machine.

To be sure, these are not equivalent claims. Mechanism$_2$ may well imply mechanism$_1$: if human thought is totally reducible to mechanical procedures, logically a machine *qua* mechanical device could mimic this. However, mechanism$_1$ need not entail mechanism$_2$. One could consistently claim that machines can sufficiently resemble humans to warrant describing their behaviour as 'thinking' without limiting all human thought to mechanical procedures. That is, a machine could satisfy the sufficient conditions for (mechanical) thinking without all human thought being restricted to this. While Mechanism$_2$ does entail mechanism$_3$, the converse does not hold: it could be that humans and machines 'do things' differently (i.e. mechanically/non-mechanically) but that our results are limited by those obtainable by a Turing machine. Of course, mechanism$_3$ does not entail mechanism$_1$ either or vice versa.

Importantly, mechanism$_2$ is not entailed whatsoever by the CTT. If the CTT is true, when a human is calculating mechanically i.e. following a definite procedure, she cannot out-compute a Turing machine. A further premise could be added—that all human cognitive ability is mechanical—which might lead one to think that humans are totally Turing-computable, but this is not entailed by the CTT, which claims nothing about a human's faculty for non-mechanical behaviour.

Gödel's famously resisted mechanism$_2$ by denying this additional premise. To his postscript on Turing's notion of algorithm Gödel added:

> [N]ote that the question of whether there exist finite <u>non</u>-<u>mechanical</u> procedures not equivalent with any algorithm, has nothing whatsoever to do with the adequacy of the definition of "formal system" and of "mechanical procedure" (1964 Postscriptum to 1934, 72).

Mechanism$_3$ does not follow either from the CTT. Again, if humans are capable of non-mechanical behaviour (which is not under the purview of the CTT), it might be possible to obtain results not obtainable by a Turing machine. Gödel, again, took this tack towards mechanism$_3$. He took from Turing's work (and his own) the following disjunction:

> The human mind…infinitely surpasses the powers of any finite machine, or else there exist absolutely unsolvable diophantine problems (1995, 310).

However, Wang reports that Gödel found the second disjunct implausible, following Hilbert's famous motto that in mathematics there can be no *Ignorabimus* (Wang 1974, 324-325). Gödel thought it would be irrational for humans to set themselves questions that are unanswerable, and thus the human mind must surpass the scope of what is performable by Turing machines (ibid.).

Shanker states accordingly that "Gödel was persuaded to accept CT on the basis of Turing's Thesis…[but] he repudiated the consequences which Turing was to draw" (1998, 19). However, so far Shanker has only introduced the Mechanist thesis as a claim about mechanism$_1$. Gödel is interested in refuting mechanism$_2$ and mechanism$_3$. He does not mention mechanism in the sense Shanker is discussing. Shanker is unclear about whether Turing is supposed to derive mechanism$_1$, mechanism$_2$, or mechanism$_3$ from the CTT.

Although Turing himself endorsed mechanism$_1$ as Shanker claims, he never claimed to derive that belief from the CTT. Mechanism$_1$ bears no relation to the CTT; it is a question for the philosophy of mind. In fact, Turing wanted to avoid *philosophical* discussion of mechanism$_1$ altogether—the whole reason Turing devised the now-famous Turing test was to replace the question of whether machines can think with a more definite counterpart: can a machine pass the imitation game? (1950, 433-434).

In Turing's 1947 'Lecture to the London Mathematical Society' he does endorse mechanism$_1$ (as Shanker claims) (1947, 393-394). However, there is no mention of the CTT, let alone a claim that mechanism$_1$ is *entailed* by the CTT. There is also no reference to the ideas of mechanism$_2$, nor mechanism$_3$. So, not only is it unclear what Shanker means when he argues that Turing 'insisted' mechanism was entailed by the CTT, there is also no evidence for any of the possible interpretations.

Now to Shanker's claim that Wittgenstein found *Computable Numbers* a misguided hybrid that attempted unsuccessfully to fuse two disparate issues from mathematics and the philosophy of mind. I am not certain what evidence Shanker is using to support this claim. It seems to be a corollary of his exposition of Wittgenstein's *humans who calculate* remark. This interpretation reads Wittgenstein's remark as something like: 'Turing's machines are really humans who calculate, not machines'. According to Wittgenstein, machines cannot calculate. In order that his machines do, Turing has question-beggingly invested cognitive abilities into his machines. Turing's supposedly seamless analysis is thus confused, and fails to merge the psychological with the mathematical. Without assuming the point Turing is trying to defend via the Mechanist thesis, his machines do not in fact 'calculate' and thus cannot be the executors of algorithms (and counterpart of effective calculability), as he requires.

However, I do not think this is what Wittgenstein had in mind, nor is it a convincing criticism of Turing. Turing deliberately models his machines on human calculation; this is the entire point. Turing does not fall into a vicious circle by investing his machines with cognitive abilities. Shanker erroneously claims that the notion of mechanical calculation requires a given calculation to be broken down into *meaningless sub-rules* and the notion of an algorithm demands that each step is devoid of cognitive content, so a machine could carry it out (cf. 1998, 10).

This is not correct. Granted, Turing's notion of algorithm requires that steps amount to "simple operations" so elementary that "it is not easy to imagine them further divided" (CN, 136). This does not entail an omission of all cognitive content. The requirement, inherited from Hilbert, is that effective procedures involve a *definite* method. This means instructions must involve no ambiguity such that insight or reflection is required to follow them. This is why only one simple operation is performed at any step. Demanding absolutely no cognitive ability to follow an algorithm is too high a requirement—after all, the procedure for computing a recursive function presumably requires *some* cognitive ability from the agent, we just limit this complexity to ensure steps are *definite*. So, *pace* Shanker, Turing's machines are not required to follow *meaningless sub-rules*. They can quite consistently display a certain level of cognitive ability without departing from their algorithmic function. Obviously carrying out an algorithm requires some ability to recognise, interpret and print relevant symbols.

Turing's machines do not, of course, 'calculate' to Wittgenstein's standard as explicated by Shanker. But this is a misnomer: Wittgenstein's targets when he talks of calculating machines are *real* machines. His point is that mere sign manipulation is not sufficient for calculation; we are also required to use these normatively to reason. However, Turing machines need not adhere to any preconceptions of what real machines are capable of. Turing machines are not *machines* per se, but merely sets of mathematical signs. The only reason machines are used as the paradigm in the first place is because they are a neat intuitive trope for 'mechanical' calculation. The results of *Computable Numbers* could be replicated with no mention of machines.[32] Turing machines are not especially designed to capture machine calculation, rather *human* calculation.

Wittgenstein understood this. He was under no illusion that his arguments concerning 'calculating machines' applied to Turing machines. This is why there is no hint of objection. Note Wittgenstein's careful use of inverted commas when mentioning *Turing's 'Machines'* (RPP 1, §1096). Of course, if we built physical machines that resembled Turing machines, these would not calculate according to Wittgenstein. However, insisting this clearly misses the point. *Computable Numbers* is about real machines no more than in the trivial sense in which Orwell's *Animal Farm* is about livestock.

The question of whether *real* machines 'calculate' or 'think' to satisfy Wittgenstein's conception of these terms is entirely separate. On this score, Shanker is absolutely right: Turing and Wittgenstein were certainly at odds when it came to the question of mechanism$_1$. However, this dispute is not under discussion here. Such concerns are irrelevant to the CTT. Shanker's error is to derive import from Turing and Wittgenstein's arguments on mechanism$_1$ to inform their arguments concerning mathematics and the CTT. *This* is the fallacy in Shanker's argument. Discussion of mechanism$_1$ needs to be separated from mechanism$_2$ and mechanism$_3$, which in turn must be distinguished from the CTT.

Wittgenstein's *humans who calculate* remark successfully identifies the defining feature of Turing's rendition of the CTT, and what contributed to its plausibility. Taking heed of Wittgenstein's remark aids an understanding of the constitutive features of Turing machines: their plausibility as a formal analysis of effective calculability due to their being modelled on *human* calculation.

3.2 The Status of the CTT Moving Forward

So far I have given a landscape of the CTT, and what it would mean for it to be true. First and foremost, Turing's results draw limits on *humans who*

[32] This was actually done by Emil Post: he independently considered an idealised human 'worker' which is mathematically equivalent to a Turing machine; see Post (1936).

calculate following a definite method. This point is crucial to understanding the purpose of the CTT and, *a fortiori*, what a Turing machine is. Wittgenstein understood that it was the Turing analysis that was the *sine qua non* of Turing machines. This point follows from an understanding of the difference between the CT and the CTT—Turing did not simply develop another formal notion to boot. He gave the results epistemological substance.

I have shown that the Turing analysis was enough to convince Gödel of the absolute truth of the CTT. I have not yet committed to the status of the CTT myself. There are several issues that first require picking apart.

In some sense, the CTT has a clear status nowadays: there is a near-consensus in the mathematical community over the truth of the CTT. Having said this, it is less clear what this actually amounts to. What kind of a statement is it (definition, consequence of induction, etc.)? Is it true beyond doubt? If so, is it provable, or indeed proved? On these questions there is less clarity. I hope to put some of these issues to bed. I will do so by appeal to the Wittgensteinian notion of family resemblance. This, of course, turns on an interpretation of the ordinary language at play in the CTT i.e. 'computability', 'effective calculability' etc. I will follow Wittgenstein's tack in the *Lectures* and scrutinise the use of ordinary language in mathematics with the aim of 'dispelling the fog'. I will give a case for the CTT as a mathematical claim that has been established now beyond doubt.

There are several striking issues at play that I think a successful case for the CTT must accommodate, the most important of these being the protean historical reception of the CTT.

After its inception in 1936 there was an overriding consensus that the CTT was true, but not provable. The main reasons for this I have already covered in the previous subsection: it seems doubtful that an equivalence can be shown between an informal notion (for which no strict bounds are drawn) and a sharp formal notion delimited in its scope. The problem was not, of course, a fear that there exists a recursive function that is not effectively calculable. We can see by induction that all of the functions under the formal notions are effectively calculable. The worry was that some new phenomenon might arise that we intuitively describe as effectively calculable but which is not captured by the formal notions. A typical response to the status of the CTT was thus:

> While we cannot prove Church's thesis, since its role is to delimit precisely an hitherto vaguely conceived totality, we require evidence

> that it cannot conflict with the intuitive notion which it is supposed to complete (Kleene 1971, 318).

That is, the thesis is a working hypothesis, requiring evidence that every function contained in the intuitive notion is recursive.

Prima facie, this seems a pretty reasonable reception. Of course, any claim that the CTT is provable requires a decent account of proof. If we have in mind *formal proofs*—a sequence of sentences within a formal system that jointly entail a theorem via the axioms and rules of inference—then the prospect of proving the CTT seems slim. For example, it seems unlikely that we could establish the CTT within ZF. To do this would require a translation of effective calculability into set-theoretic terms. We would create some predicate for effective calculability alongside axioms governing its use. From here, we could go about deducing the CTT line-by-line using the axioms and rules of inference. However, this only starts a regress. Presumably the same problem of informality will likewise apply to this new characterisation of effective calculability. Axioms need to be self-evident, yet it is the characterisation of the intuitive notions that is dubious in the first place. A justification of the translation of effective calculability into set-theoretic terms cannot come from within ZF itself, so the situation seems unchanged.

This does not yet rule out the possibility of an *informal proof* of the CTT, whatever the conception of that may be. On this score, it seems Kleeneness is not next to Gödelness. I have shown that in his response to Church's CT, Gödel found the equivalence unsatisfactory. However, he did suggest that effective calculability might be stated as a set of *axioms* to include all its accepted properties, and "do something on that basis" (Davis 1982, 9). If effective calculability could be convincingly axiomatised, this would give the blueprint for a proof. Gödel's approach on the question of proof is quite unclear, but this *suggests* that, unlike Kleene, Gödel thought it at least possible that the CTT could be proved. If a plausible set of axioms were found to capture *all* of the general properties of effective calculability, then a proof of the CTT, maybe even a formal one, is surely possible. The problem, at least for Kleene et al., was whether such an axiomatisation is possible.

Later, the idea that the CTT might be provable started to gain traction. Robin Gandy argued in (1988) that the CTT had *been proved* by Turing. He argues that the Turing analysis showed how effective calculable functions cannot surpass the capability of a Turing machine. As such, Turing had *demonstrated* an equivalence between the concepts, rather than defined one. Thus, he labelled the CTT *Turing's theorem* in contrast to *Church's thesis.*

Shortly thereafter Elliott Mendelson (1990) published a reappraisal of the CTT against the traditional grain, also arguing that the CTT is provable. He argues that we are more than happy in other circumstances to accept rigorous proofs involving intuitive concepts, and there is no reason not to extend this to the CTT. He also attacks the suggestion that *effectively calculable* is vague. He argues that the concepts and assumptions supporting the notions on the formal side of the CTT are no less vague than the informal side. The former are simply more familiar and understood in terms of their connection to other parts of logic and mathematics: "functions are defined in terms of sets, but the concept of set is no clearer than that of function" (1990, 232). As to my point above, Mendelson claims:

> [T]he notion of effectively computable function could have been incorporated into an axiomatic presentation of classical mathematics, but the acceptance of CT[T] made this unnecessary (ibid.).

Both Gandy and Mendelson claim that the CTT *follows* from the Turing analysis. This is why for them the CTT is provable and proved.

This cannot be the whole story though.

Importantly, neither Gandy nor Mendelson mention that even after Turing's paper, there was still (some) dissent as to the *truth* of the CTT. The objections stand in stark tension with their claims of proof. The objections most often cited are due to Jean Porte (1960) and László Kalmár (1959), who argue in opposite directions. I will summarise them briefly:

Porte's objection is roughly that some recursive functions are not humanly computable, requiring capacities that go beyond not only one human but also those of the entire future human race. It is easy to identify recursive functions that grow so quickly that the number of steps required to compute them exceeds the number of electrons in the universe. $g(x) = 10^{1000^{1000^x}}$ would be an example (10^{80} being roughly the number of atoms in the universe). Such functions, Porte argues, can hardly be considered computable. Of course, the response to this is obvious: *effective calculability* and *human computability* are not equivalent. *Effective calculability* is a concept analogous to idealised mechanical calculation, so bounds on human limits vis-à-vis time constraints or resources do not affect the effective calculability of a function. The only requirement regarding the number of steps in an effective method is that it be finite; so long as the number of steps is fewer than $\aleph_0$ then there is no compromise on effective calculability.

Kalmár, conversely, argues that there are effective methods that are not recursive. He says that the *improper minimalisation* of some recursive functions is non-recursive.[33] This is true. He contends that there is nonetheless a method by which to calculate a given value of the improper minimalisation $[f'(p)]$ in a finite number of steps. That is:

> Calculate in succession the values $f'(p,0)$, $f'(p,1)$, $f'(p,2)$,... and simultaneously try to prove by all correct means that none of them equals 0, until we find either a (least) natural number q for which $f(p,q)=0$ or a proof of the proposition stating that no natural number y with $f(p,q)=0$ exists; and consider in the first case this q, in the second case 0 as result of the calculation (1959, 76–77).

The problem with this is that a 'step' in his 'algorithm' may require a proof, or at least the ability to detect one. The method is not *definite* in the sense that a human could follow it without ingenuity. Better still, it may be that there is no proof, in which case the procedure may not terminate. Kalmár regards as effective calculable:

> [A]ny arithmetical function, the value of which can be effectively calculated for any given arguments in a finite number of steps, irrespective how these steps are and how they depend on the arguments for which the function value is to be calculated (1959, 73).

This clearly departs from the more stringent mechanical requirements present in the CTT as stated, for which each step must involve no creativity.

Nowadays, the arguments from Porte and Kalmár are fairly easy to dismiss. Both arguments are swiftly undermined in Mendelson (1963), so he was definitely aware of them when he claimed the CTT has been proved. I raise these objections because they must be accommodated in any account claiming *proof* of the CTT. If the CTT has been demonstrated—that is, "leads me to say: it *must* be like this" (RFM III, §30)—then why is there such clear room for doubt?

[33] This is a function f' on a function f that returns the least natural number y such that $f(x,y)=0$ if there is such a y, or 0 if there is no such y.

Of course, in practice, the mathematical community may doubt *bona fide* proofs. Many proofs are difficult and for whatever reason are not immediately recognisable as proofs. However, the above objections are not similar to those contained in the *Hopeless Papers* 'refuting' Cantor's proof: Porte and Kalmár do not misunderstand the calculus. Rather, these objections are sincere disagreements over the use and scope of the concept *effective computability* from first-rate scholars. Nothing can be proved about *all* effectively calculable functions if the extension of this class is disputed. Further, Church and Turing have no grounds to claim theirs is the correct interpretation of *computable*—the terminology is publicly owned, as it were. The dispute over the terminology serves as evidence for the type of approach to the CTT that Kleene et al. have in mind, whereby we cannot equate a vague intuitive notion with a sharp formal one.

All of this is not to say that Gandy and Mendelson are *wrong* to claim the CTT is provable or proved. Rather, we require an account for the above. The objections cannot merely be parried, as they turn on the ambiguity and disagreement over the informal concepts involved, which was the reason for tentativeness in the first place. If the Turing analysis *proved* that effective calculability is equivalent to recursiveness, then there seems little room for objections based on a pre-existing conception of effective calculability which recursiveness does not capture. These at least require explanation.

3.2.1 Shapiro

To my knowledge, the only account that goes some way to accommodating the CTT's historical reception is due to Stewart Shapiro (2006, 2013).

Shapiro's basic idea is to appeal to Waismann's notion of the 'open texture of language'. This notion is introduced in a response he gave to crude phenomenalism in 1945. Empirical concepts, says Waismann, are open textured in that they always carry the possibility of indeterminacy—our language can never delimit an empirical concept in all directions. For instance:

> The notion of gold seems to be defined with absolute precision, say by the spectrum of gold with its characteristic lines. Now what would you say if a substance was discovered that looked like gold, satisfied all the chemical tests for gold, whilst it emitted a new sort of radiation? 'But such things do not happen.' Quite so; but they might happen, and that

> is enough to show that we can never exclude altogether the possibility of some unforeseen situation arising in which we shall have to modify our definition. Try as we may, no concept is limited in such a way that there is no room for any doubt. We introduce a concept and limit it in some directions; for instance we define gold in contrast to some other metals such as alloys. This suffices for our present needs, and we do not probe any farther. We tend to overlook the fact that there are always other directions in which the concept has not been defined [...] we could easily imagine conditions which would necessitate new limitations. In short, it is not possible to define a concept like gold with absolute precision; i.e., in such a way that every nook and cranny is blocked against entry of doubt. That is what is meant by the open texture of a concept (Waismann 1968, 42).

It is via open texture that languages change and evolve alongside, say, scientific developments. New phenomena appear which seem to fall under a certain concept, yet betray essential conditions of that concept. This forces a debate as to whether to update the concept to accommodate (or exclude) this new phenomenon. Another example Waismann uses is that of Einstein. The main tenets of the theory of relativity violated what was meant by the word 'simultaneous'. It's not that Einstein found some new underlying meaning in the original word. Rather, some results sufficiently warranted an application of the word, but to do so required a change in the application of 'simultaneous' thereafter. Intuitive notions are sharpened or relaxed over time, giving language an essential open texture.

Waismann applies this notion only to empirical concepts. He argues definitively that it should not apply to areas such as mathematics, which has a closed texture:

> In a formalized system the use of each symbol is governed by a definite number of rules, and further, all the rules of inference and procedure can be stated completely (1968, 51).

Shapiro's contribution is to deny this last part: he argues that, in fact, concepts in mathematics *are* susceptible to open texture. He uses as example the dialogue on Euler's theorem from Imre Lakatos' *Proofs and Refutations* (1976) (2006, 435-439):

A teacher puts Euler's theorem on the board: Consider any polyhedron. Let V be the number of vertices, E the number of edges, and F the number of faces.

Then V − E + F = 2. The teacher then proves this, but the exceptional students provide a barrage of counterexamples. They identify unforeseen (and weird) polyhedra for which the proof does not work, such as 'picture frames'—a cube with a cube-shaped hole in one of its faces—or the 'star polyhedron' where faces protrude from each other. Some argue that spheres and tori qualify as polyhedra (where V − E + F = 1).

The point is that there will always be hidden lemmas in the proof, stemming from the open texture of the intuitive concepts at stake. That Waismann was wrong, and Shapiro correct, regarding the application of open texture to mathematics should be clear to us immediately—I have already discussed at length what seems to be a clear-cut case of open texture in mathematics: Cantor's proof(s) of the uncountability of $\mathbb{R}$. Prior to Cantor, the concept *infinity* did not include parameters to distinguish between transfinite cardinalities. Then Cantor produced results which concerned the concept *infinity*, but violated its grammar—we had no tools to describe results 'greater than' infinity. The concept of infinity required updating, set-theoretically, in order for us to comprehend and explain Cantor's proof. The concept was duly sharpened, allowing for distinctions including that between $2^{\aleph_0}$ and $\aleph_0$.

Regarding the CTT, Shapiro's claim is that in the 30s, and for sometime thereafter, *computability* and *effective calculability* were subject to open texture. No bounds had been drawn on their application as a pre-theoretic notion. However, the work of Turing, Church, etc. and the subsequent acceptance of the CTT sharpened these concepts. For instance, the Turing analysis makes it clear exactly which features of the pre-theoretic notions are to be isolated. Thus, in a sort of self-fulfilling prophecy, the CTT becomes established as fact because it sharpens the relevant pre-theoretic notions such that they have a determinate meaning. This separates effective calculability from, say, human computability (*pace* Porte). What we are left with today are concepts "about as sharp as anything gets in mathematics...there is not much room for open-texture anymore" (2006, 451).

3.2.2 Critical Notice of Shapiro

Shapiro's analysis of the state of play nowadays seems to point in the right direction. The crucial advantage of this approach, for me, is that it gives an account of the various changes in the CTT's reception across the literature. Using open texture, we can accommodate all of the attitudes to the CTT so far considered. For Church and his contemporaries, it was true that they were working with an intuitive cluster of concepts rather than a single notion of calculability. This certainly carried the possibility of indeterminacy. Under a similar guise, Porte and Kalmár's objections turned on identifying concepts from that cluster, thereby included under the concept *computable*, but not the subject of the mathematical results of Church or Turing. However, these concepts have since been sharpened through continual discussion and development in the mathematical community. That is why, looking back now, these objections seem so trivially flawed. We now possess the tools with which to examine the intuitive notions with absolute precision, and so concepts which were once being sharpened by open texture are no longer susceptible to it.

As it stands, however, I find Shapiro's account ultimately unconvincing.

Open texture, for Waismann at least, is an essential feature of language. Thus, concepts can never lose their open texture. The reason he uses gold as an example is because this, on the face of it, seems to be rigorously defined in all directions. Even concepts such as gold display open texture. The entire purpose of introducing the notion rides on this feature. This is perhaps why Waismann does not consider mathematical concepts under its purview; the immediate consequence of extending open texture to mathematics is that even some of the most perspicuous proofs become subject to doubt. If mathematics has an open texture, we cannot be definite in our treatment of sets or polyhedra because we cannot accommodate for future developments which call into question our current results.

Of course, Shapiro is perfectly welcome to depart from Waismann and declare that open texture *can* be lost. He does not argue for this though, or explain how this might work. He claims that terms like 'computability' have lost their open texture by sharpening through informal rigour. I do not see on what authority Shapiro can claim this. Consider hypercomputation—computation that is Turing-uncomputable. If this were realised, given the close connection between digital computing and the mathematical notion of computability, and the fact that we talk in both cases of 'computing' or 'computation', the discovered process might intuitively be described as 'computable'. However, currently we have no parameters for treating such a process as computable. As such, capturing this phenomenon linguistically would require that we update what we mean by 'computable', and reassess and adjust the CTT. Now, the possibility of hypercomputation is unlikely and highly controversial.[34] It is nevertheless possible. Even Martin Davis, who has disparagingly dubbed hypercomputation a ‘myth’, admits he cannot absolutely exclude its possibility (2004, 206). It would require an overhaul of modern scientific knowledge and capability, but is no more far-fetched than naturally occurring radioactive gold. It is certainly conceivable that we may in the future encounter some procedure clearly apt for being described as computable or calculable, but which our current rules for the application of these concepts cannot accommodate.

I find that Shapiro misses the point of open texture. Its most salient ingredient is that it cannot be lost. Waismann has noticed an interesting internal feature of empirical language—the scope of its application is under persistent re-evaluation. Without this, open texture only highlights the (I think) less salient fact that not all concepts that we use have sharp extensions.

Shapiro’s goal is to establish that the CTT has been proved, on his informal Lakotosian picture. However, his analysis of the open texture of computability actually directs us away from the provability of the CTT, rather than towards it. Accepting that computability is susceptible to open texture introduces doubt about its universal truth. Shapiro’s account is a useful benchmark for taking account of the CTT’s historical reception, but is ultimately unconvincing. I therefore agree with the spirit but not the letter of Shapiro’s picture.

3.2.3 A Wittgensteinian Way Out

The chief problem with Shapiro’s account is that it does not, and given Waismann’s analysis arguably cannot, accommodate a sharpening of the informal concepts *effective computability* or *calculability* such that they are no longer subject to open texture. For this reason, his claim that the CTT has been established beyond doubt, let alone proved, is unsuccessful.

Having said this, given my earlier exposition, there are clear advantages to an account which views effective computability as a once-blunt notion that has been sharpened as a result of mathematical discourse. This would give a convincing story for the initial trepidation as to the CTT’s universal truth, and the alleged refutations, whilst consistently claiming that nowadays the CTT is established beyond doubt. All this requires is a notion similar to open

[34] Copeland is its primary advocate, having coined the word.

texture, but with a convincing, non-*ad hoc* argument for how the process can end. To this end, I will make two claims. First, that Waismann adapted the notion of open texture directly from Wittgenstein. Second, that an analysis of *computability* derived from the original source yields the successful account required to make a case for the indubitable truth of the CTT.

Open texture is a direct continuation on Wittgenstein's notion of family resemblance in all but name. Better, it is a label for the type of activity seen in language games that overtly host family-resemblance concepts. It should not be too surprising that Waismann's notion originated with Wittgenstein. The two were close collaborators from the late 1920s through the 1930s. What is more, Waismann's role in their collaboration was typically as Wittgenstein's amanuensis, drawing out Wittgenstein's thoughts and expressing them clearly to a wider philosophical community.[35] Because of this, much of Waismann's work can be seen as an extension of Wittgenstein's.

The most developed introduction of family resemblance is in the *Investigations*, derived from remarks dating to 1936 from MS 152. Wittgenstein asks what is common to the proceedings that we call 'games'. We have 'card games', 'board games', 'Olympic games' etc. Passing from analysis of one type of game to another, we see similarities, but many core features drop off and new ones appear. No single feature common to all games can be isolated. We see a "complicated network of similarities overlapping and criss-crossing" (PI §66). No feature is common to all, but many features are common to many. A cluster of concepts is connected by a cluster of features. Wittgenstein claims there is no better way to characterise this than 'family resemblance':

> [F]or the various resemblances between members of a family a build, features, colour of eyes, gait, temperament, and so on and so forth a overlap and criss-cross in the same way.—And I shall say: 'games' form a family (PI §67).

Scanning a family photograph one sees similarities crop up here and there. Each member of the photograph can be instantly recognised as a member of the family by reference to features shared by others: 'Dawn's children all have that smile', 'All the boys are giants', 'Tom's got Grandad's chin'. There is a

[35] The two were to co-author a book of this nature entitled *Logik, Sprache, Philosophie*—a presentation of Wittgenstein's prevailing ideas that was never completed.

nexus of commonalities displayed across the board, yet nothing common to all.

The point of Wittgenstein's analogy is that this is how we use and define concepts in language. In this way, concepts are not "closed by a boundary" (PI §68). Concepts are not rigidly limited: there is no boundary for being a game, no precise rule for what counts as a game and what does not. We *could* draw one, but none has been drawn. The way we use the concept has no boundaries. That is, "it is not everywhere bounded by rules" (ibid.). And so it is for most concepts according to Wittgenstein. Ordinary usage rarely requires a sharply defined concept, but when it does we can sharpen them:

> I say: "We are eating at 1 o'clock" and that is correct even though we do not lift our spoons simultaneously at the strike of 1. [...] You can play a game quite well by just making rules as they are required (MS 152, 77).[36]

Once the purpose of the family resemblance analogy is understood, the connection to open texture should become clear. The way we use ordinary-language concepts does not draw rigid boundaries on their application. An analysis of the use of a concept across the board gives a picture (albeit with blurred edges) characterising that concept. This is not delimited in all directions because a feature of a concept across applications need not be manifested in its *every* application. We do not draw rigid boundaries specifying when something falls under the extension of that concept.

Not only are these ideas similar, but it seems Waismann actually derived open texture directly from Wittgenstein. It is safe to assume Waismann would have read MS 152; Waismann was very familiar with Wittgenstein's work in the 30s. If there is any doubt, compare the following passages. The first is Waismann's second example demonstrating what he means by open texture. The second is a remark from MS 152 directly following the exposition of family resemblance that I have considered:

> [S]uppose I say 'There is my friend over there'. What if on drawing closer in order to shake hands with him he suddenly disappeared? 'Therefore it was not my friend but some delusion or other.' But suppose a few seconds later I saw him again, could grasp his hand, etc. What then? 'Therefore my friend was nevertheless there and his

[36] All quotations from MS 152 are my own translations.

> disappearance was some delusion or other.' But imagine after a while he disappeared again, or seemed to disappear—what shall I say now? Have we rules ready for all imaginable possibilities? (1968, 41).

> I say: "there is a chair there". But what if I approach it in order to pick it up and it disappears into thin air?—"so it wasn't a chair at all but some illusion."—But a few seconds later we see it again and touch it etc. So, there was a chair there after all and the disappearance was an illusion. And in an hour it disappears again, or so it seems, and so on. What should we say now? Do we have rules for such cases? Are we going to say that we do not know what the word "chair" means since we are not equipped with rules for its application in all imaginable cases" (MS 152, 79).

Needless to say, despite the nine-year gap, Waismann reproduces Wittgenstein's remark virtually verbatim.[37] Waismann's notion of open texture blatantly arose from close study of Wittgenstein, particularly MS 152.

Now, Shapiro was on the right track. However, his employment of open texture does not work as he had hoped. I will argue that taking stock from the original Wittgensteinian picture will give a clearer analysis of the CTT and its status today.

Shapiro's contribution to the debate is to apply Waismann's notion to mathematics. To be more precise, Shapiro claims that the ordinary language employed in mathematics is subject to open texture (e.g. 'polyhedron', 'computable'). He likens this to how *number* shows open texture:

> Are complex numbers numbers? Surely. But this was once controversial. If it is a matter of proof or of simple definition, why should there ever have been controversy? (2006, 434).

Wittgenstein had already foreshadowed this exact application in MS 152. Wittgenstein introduces family resemblance using this exact example:

> And likewise the types of number, for example, form a family. Why do we call something a number? Well, because it has a direct affinity with something else that has hitherto been called a number. And we expand

[37] Aside from Wittgenstein's (perhaps characteristic) choice to use an inanimate object as the example, rather than a friend.

> our notion as if we were spinning, twisting fibre on fibre. And the strength of the thread comes not from the fact that a single fibre runs through its entire length, rather that many fibres cross over one another (MS 152, 74) (cf. PI §67).

The problem is that Shapiro throws the baby out with the bathwater. His account requires that open texture can be lost. However, Waismann's concept is posited specifically as an internal, essential feature of language. Thus, open texture 'lacks the institution of an end', as Wittgenstein might put it (cf. RFM II, §45). Shapiro fails to account for how ordinary language in mathematics might lose its open texture. Therefore, he has no grounds to declare that computability nowadays is no longer susceptible to it. This, however, is not a problem for a picture given in terms of family resemblance.

The substantive difference between open texture and family resemblance is a methodological one. Waismann's notion is framed as a necessary condition: open texture is an essential feature of empirical concepts so they must be enduringly susceptible to it. Concepts subject to open texture can never be rigid. Conversely, Wittgenstein's notion is an empirical claim. Family resemblance is not description of how language *must* be, but how it is. Family resemblance is a feature of language *as used by us*. This much is emphasised in Wittgenstein's example of games as displaying family resemblance. He is disparaging about rhetoric such as: "[games] *must* have something in common, or they would not be called 'games'" (PI §66). His motto is: "don't think, but look!" (ibid.). We derive the notion of family resemblance by *looking at how we use language*.

Family resemblance is not an internal feature of language. Rather, it is a description of how we use certain concepts. For this reason, we are not forced to concede that we can *never* delimit a concept in all directions—quite the opposite. Wittgenstein argues this for the case of *number*:

> I *can* give the concept of number rigid boundaries [...], that is, use the word "number" for a rigidly bounded concept; but I can also use it so that the extension of the concept is *not* closed by a boundary (PI §68).

That *number* is characterised by a cluster of concepts is not necessary. This is just how we use it. This is advantageous in mathematics, of course. Having a porous concept allows us to accommodate new developments in mathematics. For example, at some point *number* had to be updated to accommodate the first use of complex numbers in mathematics. We want to say imaginary numbers are numbers due to their clear analogy with non-imaginary numbers. Drawing boundaries on numbers would have inhibited this type of accommodation. We *can* draw a boundary, but none has so far been drawn.

Mathematicians ostensibly draw boundaries at times, as required. Take *recursiveness*: there is no allowance for borderline cases here. Recursive functions can only be defined in terms of the basic functions and applications of the operations that I outlined. Either a sequence of signs satisfies the meticulously specified conditions, or else it is not recursive. The conditions leave no possibility of indeterminacy. The concept is everywhere circumscribed by rules. Thus, this concept is delimited in all directions. Its edges are sharp.

Mathematical concepts patently can be closed. The point is: they often are not. This gives a much clearer picture of how concepts are used in mathematics. Mathematical concepts are not slaves to open texture as explicated by Shapiro. Rather, some ordinary-language concepts like *number* are used with blurred edges as suits their purpose. However, concepts can be made rigid by drawing precise boundaries.

It is paramount, however, to understand how these boundaries are drawn. When drawing boundaries upon concepts that display family resemblance, we are susceptible to confusion. This point harks back to my discussion of sharpening mathematical concepts in RFM II and the *Lectures*. I emphasised earlier that the *direction* of fixing meaning is all-important. The calculus must inform the use of language, not the other way around. With Cantor's proof, confusion ensues when we try to use our ordinary-language concepts to confer meaning upon his results. Rather, the sharpening of indeterminate mathematical concepts must come from the calculus.

This, of course, was exactly why Wittgenstein had no objection to Turing's results, as I showed in §2. Turing's proofs do not clumsily describe a calculus in terms of pre-theoretic concepts. Turing presents his results, then the Turing analysis links these results back to our intuitive concept of *computability*. The calculus can be attached to effective computability, illuminating certain limitations on that concept. The Turing analysis shows that the properties of his chosen calculus necessarily lead back to the pre-theoretic concept. The result, therefore, casts "a brilliant light over the verbal expression" (RFM II, §7). He gave substance to a previously vaguely understood notion. After Turing, the intuitive notion was given a paradigm through which it could be sharpened. It gave sense and a counterpart to the notion of a ‘finite, *definite* method’ and thereby to the internal features of effective calculability.

My analysis of the CTT nowadays therefore runs as follows. The concepts *computability* and *effective calculability* had blurred edges in the 1930s. These terms picked out a cluster of intimately connected concepts, which included, for example, *humanly calculable*. There were no strict rules for the boundaries of these notions, but rather a family resemblance. The concept was not sharp because, until then, it had no need to be. There was not yet a rigid framework by which to sharpen the notions, and through which to understand them. No light had been shed on them. Hilbert’s programme, and his framing of the *Entscheidungsproblem*, gave some clues as to which features a formal framework should embody. That is, to capture effective calculability, sense had to be given to the concept of ‘finite, definite procedure’. These requirements, however, could not be understood *rigidly*.

Turing and Church, in 1936, then proved influential results concerning a subset of this cluster of concepts. The results required a subsequent sharpening of the intuitive concept, eliminating some of its uses. Their formal results jointly gave a paradigm for the notion of a finite, definite method. Importantly, the Turing analysis provided a sharp specification of which features these results concerned, and which they did not. Turing thus sharpened the informal notion by drawing its boundaries. As such, there was now no worry that further instances of effective calculability may be found which could not be captured by the formal frameworks: the Turing analysis demonstrated how the general properties of this informal notion necessarily led to his formal framework, and vice versa. As it was then shown how bearing the properties of computability *necessarily* led to falling under the concept of Turing-computability, the CTT was thereby demonstrated.

Gradually, the terms ‘effective computability’ and ‘calculability’ came to be understood *in terms of* the mathematical results; the notions were thus publicly sharpened and there is no longer room for future effectively calculable solutions which are not recursive. This sharpening did not sink in for some time after the publication of the proofs, which explains the initial doubt and few objections. The subsequent sharpening is what led Mendelson and Gandy to declare the CTT a *theorem*. That the CTT was demonstrated beyond doubt satisfied them sufficiently to label it a proof. I will not follow here: I have neither the space nor the inclination to offer a decent account of proof, as this requires. However, I contend that the CTT has been demonstrated as clearly as any other claim in mathematics, albeit informally. Whether there could be a satisfactory formal proof of the CTT is a moot point.

Of course, there may be future phenomena, say, the realisation of hypercomputation, which have sufficient analogy to computability that we might update these concepts so as to include the new phenomena. However, the point here is that we are no longer left with ‘blurred edges’—that is, without rules. The boundaries have now been drawn; hypercomputational procedures, if realised, would not be effectively calculable. Granted, if we extended our concept of computability, the *language* of the CTT would have to be updated to include caveats. This would not affect its claim though. The validity of existing number-theoretic proofs was not undermined upon the introduction of complex numbers into mathematics, even though many were originally stated as proofs about ‘all’ numbers. The content of the CTT is secure, but the language with which we express it may change.

To be sure, this argument would not save Shapiro’s account. Unlike mine, his account suggests that there may always be future phenomena for which we have no rules. As such, the *content* of the CTT is not secure. As he has given no adequate explanation of how the open texture of computability may cease, we cannot on this account declare that the CTT has been established beyond doubt, let alone proved as he claims. My Wittgensteinian analysis gives a far stronger case for the CTT and its status today.

3.2.4 Final Remarks

This concludes the arguments for my core claims a)-d). This section began by providing further support for my claim that Wittgenstein endorsed Turing’s work—c). A correct analysis of the *humans who calculate* remark shows that Wittgenstein not only endorsed Turing’s rendition of the CTT, but that he understood the intricacies of the thesis. The fact that Turing machines are explicit models of human calculators is exactly why Turing’s account is often lauded as so convincing. Hence, once spelled out, Wittgenstein’s remark recognises Turing’s account as the sort Gödel called for: a bridging argument that connects both sides of the CTT. *Pace* Shanker, Wittgenstein did not object to the CTT. This interpretation relies on fallacies related to mechanism and the CTT. The CTT may well inform debates on mechanism alongside other premises, but these considerations are not entailed by the CTT whatsoever.

In the latter half of this section I have argued for a Wittgensteinian interpretation of computability. This concept for some time clearly displayed a good deal of Wittgenstein's notion of family resemblance. As such, initially the CTT was susceptible to doubt. I contend that Turing then successfully sharpened this notion via his analysis. He determined a class of functions that necessarily corresponded to a specific conception of computability. He demonstrated that all computable functions understood in his way were necessarily Turing-computable. After these influential results the common understanding of the relative intuitive notions sharpened in turn. I contend that there is now no doubt as to the truth of the CTT. The key advantage of this approach is that it accommodates the initial doubt over the status of the CTT. Should we wish to update our understanding of *computable function* in the future, we are not left without rules for demarcation. This would not invalidate the content of the CTT as currently understood; we would simply need to update the language that we use to express it.

Conclusions

I began with the following claims:

a) Wittgenstein read, understood and engaged with *Computable Numbers*.
b) Wittgenstein's remarks on this topic are highly perceptive and have pedagogical value, shedding light on Turing's work.
c) Wittgenstein was highly supportive of Turing's work as it was indicative of Wittgenstein's prevailing approach to mathematics.
d) Adopting a Wittgensteinian approach to Turing's proofs enables us to answer live problems in the modern literature on computability.

My chief aim was to debunk the default assumption that Wittgenstein would not have cared about, understood, or even heard of Turing's early work on computability. Instead, I have depicted Wittgenstein as an engaged and engaging reader of Turing. He took diligent care to reproduce Turing's results in his own style. What he found was that Turing's proofs chimed harmoniously with his own approach to mathematics at that time. Further, Turing's proofs were general enough so as to be adapted to draw conclusions relevant to Wittgenstein's own programme vis-à-vis rules. This followed from a new intensional approach to Cantor's diagonal argument, with which I showed Wittgenstein previously struggled. The endorsement of Turing's work extended to his rendition of the CTT *qua* its being a model of human calculation. Along the way I have caveated this picture in various ways to separate it from other accounts. Notably, I have offered objections to Floyd's argument that Wittgenstein influenced the contents of *Computable Numbers*, Shanker's interpretation of Wittgenstein's approach to the CTT, and Shapiro's arguments for the provability of the CTT by appeal to open texture. I finished by offering a case for the truth of the CTT by interpreting computability in terms of family resemblance.

Despite initial appearances, the Wittgensteinian approach to mathematics is clearly useful to answering philosophical questions about computability. I hope this might serve as a justification, albeit modest, of its relevance to modern scholarship. I would speculate that Wittgenstein's work may have an even broader impact on computability theory than presented here. As a non-mathematician by trade, it is tempting to overlook Wittgenstein's approach when it comes to more technical inquiries. However, if Wittgenstein showed anything in the *Lectures*, it was that the most pernicious confusions, even in the most technical fields of mathematics, usually supervene upon the misuse of ordinary language.

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