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REVIEWS

The Association for Symbolic Logic publishes analytical reviews of selected books and articles in the field of symbolic logic. The reviews were published in ***The Journal of Symbolic Logic*** from the founding of the Journal in 1936 until the end of 1999. The Association moved the reviews to this Bulletin, beginning in 2000.

In a review, a reference "JSL XLIII 148," for example, refers either to the publication reviewed on page 148 of volume 43 of the Journal, or to the review itself (which contains full bibliographical information for the reviewed publication). Analogously, a reference "BSL VII 376" refers to the review beginning on page 376 in volume 7 of this Bulletin, or to the publication there reviewed. "JSL LV 347" refers to one of the reviews or one of the publications reviewed or listed on page 347 of volume 55 of the Journal, with reliance on the context to show which one is meant. The reference "JSL LIII 318(3)" is to the third item on page 318 of volume 53 of the Journal, that is, to van Heijenoort's *Frege and vagueness*, and "JSL LX 684(8)" refers to the eighth item on page 684 of volume 60 of the Journal, that is, to Tarski's *Truth and proof*.

References such as 49***5*** or 280***1*** are to entries so numbered in *A bibliography of symbolic logic* (the Journal, vol. 1, pp. 121–218).

David Christensen. ***Putting logic in its place: formal constraints on rational belief.*** Oxford University Press, Oxford, 2004 xii + 187 pp.

As its subtitle indicates, this is a book on epistemology; as the title intimates, classical logic does not come off well. The author distinguishes two models of belief, a binary approach according to which an item is either believed or not believed, and a graded approach, in which each item of belief is allocated a certain degree. A binary approach to rational belief seems to call for the imposition of logical constraints: One's set of rational beliefs should be *consistent* and it should be *deductively closed*. Chapters One and Two set the stage for making a choice between the binary approach to rationality and the graded approach to rationality.

The two constraints on rational belief of consistency and deductive closure are lumped together as *deductive cogency*. The first serious section of the book (Chapters Three and Four) consists of an exposition and discussion of Makinson's paradox of the preface (***Analysis***, vol. 25 (1965), pp. 205–207), and of Kyburg's lottery paradox in ***Probability and the logic of rational belief***. The discussion takes place on the level of examples and intuitions ("pre-theoretic intuitions"). Although much of the relevant literature is cited, the author has nothing new to add: " … it seems to me that if logic has a role to play in shaping epistemic rationality, it will not be the one of subjecting binary belief to deductive cogency" (p. 105). There is no discussion of what we might call the Fregean view in ***The foundations of arithmetic***, recently pushed by Gil Harman in his ***Change in view***, according to which logic is a theory of what follows from what, not a theory of constraints on what one should believe.

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Psychologism, the view that logic concerns belief, has recently spawned a number of conditional logics that attempt to formalize the nonmonotonic connection between evidence and belief. There is no discussion of the various forms of conditional logic that are now available. Indeed, there is no discussion at all of diachronic rationality—that is, of the rationality of changing your mind in the face of new evidence.

The main thesis of the book is to be found in the remaining two chapters. These contain the most interesting and original material in the book. The author accepts the general idea that belief comes in real-valued degrees, and that the only constraints on those degrees of belief are to be found in the probability calculus. Belief is subjective, and only *systems* of degrees of belief can be assessed as rational or irrational.

There are two main kinds of argument in favor of the thesis that degrees of belief *should* satisfy the probability calculus: Dutch Book arguments, and Representation Theorem arguments. These arguments, well known and studied since Ramsey, are understood as arguments for *probabilism*, the doctrine that "Ideally rational agents have probabilistically coherent degrees of belief" (p. 125).

Each kind of argument is laid out and examined in meticulous detail, and while neither is " ... close to being a knockdown argument for probabilism ... ," each is alleged to show that " ... probabilism fits well with (relatively) pre-theoretic intuitions about rationality" (p. 142). At base, it is our pre-theoretic intuitions that determine our epistemology.

In the end, though probabilism is " ... nothing more than a way of imposing traditional logic on belief ... When we see ... logic as governing the degree to which we believe things ... probability theory is the overwhelmingly natural choice for applying logic to belief" (p. 141).

There is, however, something going on here that seems wrong, whatever one's "pre-theoretic intuitions" may be. That is that "logic" is being used in two contrary ways. On the one hand, probability theory includes traditional logic, in the sense that any contradiction has a probability of 0, and any tautology has a probability of 1. (An enriched system of axioms for probability can include sentential logic explicitly, as the author notes.) Probability theory encompasses traditional logic.

On the other hand, the author speaks of " ... taking logic to constrain ideally rational belief by way of the probability calculus ... " as though logic were constraining partial belief. But while logic can constrain the probabilities of tautologies and the probabilities of contradictions, it can have nothing to say about the partial belief a person has in any single statement. It can have something (but not much) to say about the relations between probabilities (the probability of a disjunction must be at least as great as the probability of a disjunct), but this is not much help. Put more precisely, a sentence that is neither a tautology nor a contradiction can be assigned probability r, for arbitrary r $(0 < r < 1)$, in some coherent system of beliefs. To be fair, the author intends this result, however much it seems to conflict with some people's pre-theoretic intuitions.

While not informative about logic, this little book is politically correct: the masculine and feminine singular pronouns are distributed in the text at random.

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ANITA BURDMAN FEFERMAN AND SOLOMON FEFERMAN. ***Alfred Tarski, Life and Logic.*** Cambridge University Press, Cambridge, 2004, vi + 425 pp.

Alfred Tarski, "one of the greatest logicians of all time," was "a charismatic teacher, ... bon vivant and a womanizer," with supreme confidence in his talent and vision. Born in Warsaw in 1901 to Jewish parents, he changed his name and converted to Catholicism, but never obtained a professorship in Poland. Shortly after he arrived in the USA for a conference in 1939, Germany invaded Poland. Tarski was jobless, could not return, and his family could not leave. How he eventually was reunited with his family and built an empire of logic in Berkeley is quite a dramatic story.

Tarski spoke with a heavy Polish accent, smoked incessantly, and lectured with "chalk in one hand, cigarette or cigarillo in the other. . . . Would he try to write with the cigarette and smoke the chalk?" (During class in 1973, he really did write on the blackboard with his cigarillo.) By one account he set fire to a wastebasket and was thereafter forbidden to lecture in the wooden buildings on the Berkeley campus where this happened. His seminar standards were "exasperatingly high and memorably rigid". His students and assistants always addressed him as "Professor Tarski," but a student who had completed a Ph.D. with Tarski might receive the admonition, "Now, you must call me Alfred." In his own work, and with the women he pursued, he "was brilliant, charismatic, proud, and ceaselessly persistent." Five of twenty four Ph.D. students listed at the end of the book were women. With some of them and with several others, Tarski had affairs. Tarski is therefore a good subject for a biography. Even Tarski thought so. In one story he says, "you make me so angry I will make sure you are not mentioned in my biography."

The authors are exceptionally well qualified to write about Tarski. Anita Burdman Feferman is the author of ***Politics, Logic and Love: The Life of Jean van Heijenoort*** (in paperback, ***From Trotsky to Gödel: The Life of Jean van Heijenoort***). Solomon Feferman, professor of mathematics and philosophy at Stanford University, was editor-in-chief of ***Kurt Gödel: Collected Works*** and received his Ph.D. from Tarski in 1957. The Fefermans were in social and professional contact with Tarski for the last thirty years of his life. They conducted interviews, solicited letters, searched archives, and received considerable help from others. In the end they thanked more than 130 people. The book is graced with about seventy photographs, including three of Tarski with Gödel.

The story of Tarski's life and loves is told in 15 chapters, while his work in philosophy, logic, and mathematics is described in 6 interludes. All mathematical and logical symbolism is confined to the interludes. This structure makes this book accessible to the widest possible audience. The general reader may simply omit the interludes without losing the story. For the technically knowledgeable reader, these interludes are interesting and full of potentially surprising tidbits for those not intimately familiar with the subjects.

The first third of the book covers the first half of Tarski's life. Alfred Tarski was born on 14 January 1901 in Warsaw as Alfred Teitelbaum (or Tajtelbaum, the Polish spelling). He was the first son of the brilliant, imposing, and well-educated Rosa Prussak (1879–1942), from a prominent and wealthy family in the textile industry in Lódź, and the kind and gentle Ignacy Teitelbaum (1869–1942) of Warsaw, who was in the lumber business. Tarski's parents were married on 16 January 1900. Tarski's younger brother Wacław was born in 1903 and became a lawyer. The family lived in a large apartment on Koszykowa Street, near the Botanical Garden, one of Tarski's favorite places and perhaps the source of his intense life-long interest in gardening and botany.

The language of instruction in school was Russian, but Tarski also learned enough German to translate a story for his parents' 13th wedding anniversary. In 1915, just before Tarski entered high school, Warsaw was occupied by German forces and the language of instruction switched to Polish. Tarski studied Latin and French at the Szkola Mazowieka, but he also managed along the way to learn Greek. He eventually published papers in Polish, French, German, and English. Tarski received the highest possible marks in all subjects. "A student of extraordinary ability," wrote his teachers, "not to be compared with others." Tarski finished high school in June 1918.

His earliest memory, at age four, of watching workers parading in support of the Russian revolution of 1905, started his life-long interest in politics. World War I and the Russian revolution of 1917 brought about independence for Poland after 150 years. Tarski entered the University of Warsaw, now Polish and no longer part of the Russian Imperial University, on 15 October 1918, just a month before the formal declaration of independence for Poland. Between 1918 and 1921 Poland fought six wars, each time regaining territory lost in previous periods of history. Lectures were cancelled at the University of Warsaw for the academic year 1918/19, and Tarski was called up for service, working in a unit that provided food, equipment and medical help. At the university he had initially enrolled for courses in zoology, botany, anatomy, chemistry, and physics, but he changed his major to mathematics and logic.

Mathematics was about to blossom in Poland. In 1915, Zygmunt Janiszewski and Stefan Mazurkiewicz were appointed to the faculty of mathematics at the University of Warsaw. Janiszewski founded Fundamenta Mathematicæ but died in the flu epidemic of 1920. Tarski's professors in mathematics were Mazurkiewicz (chair of the department and its central figure), Waclaw Sierpinski (the senior member), and later Kazimierz Kuratowski. Tarski's professors in logic were Jan Łukasiewicz, who moved to Warsaw in 1915 and was the first minister of education in the new Poland, Stanisław Leśniewski, and Tadeusz Kotarbinski. All three were students of Kazimierz Twardowski in Lvov, who was in turn a student of Franz Brentano in Vienna. (Brentano's other students include Sigmund Freud and Edmund Husserl.) Tarski took analytic geometry, calculus, and analysis from Mazurkiewicz, higher algebra, set theory, and measure theory from Sierpinski, logic and philosophy from Kotarbinski, set theory and the foundations of arithmetic, geometry, and logic from Leśniewski, the theory of relations and "on freedom and necessity" from Łukasiewicz, and topology from Kuratowski. Tarski's first published paper (1921, in Polish) solved a problem in set theory presented in one of Leśniewski's courses. In one of Leśniewski's seminars, the 1918 book by Louis Couturat, ***The Algebra of Logic***, was subjected to intense criticism. Tarski asked, "is it worthwhile?" But he eventually devoted much of his life to the subject.

Leśniewski was Tarski's dissertation advisor. For his Ph.D., Tarski solved a problem concerning Leśniewski's system of logic. His results appeared in a Polish philosophical review in 1923 and then, translated into French, were published in two parts in Fundamenta Mathematicæ. The first part appeared in 1923 under the name Alfred Tajtelbaum, the second in 1924 under the name Tajtelbaum-Tarski. Thereafter all his papers were by "Alfred Tarski," for he changed his name just in time (19 March 1924) for "Tarski" to appear in the announcement of his doctoral examination (24 March 1924). Tarski also converted to Catholicism in spite of being a professed atheist. Such changes were not uncommon at the time. Łukasiewicz and Leśniewski had encouraged him to change his name for professional advancement. Many socialists (Tarski was one) favored assimilation as a solution to the "Jewish question". Tarski probably felt more Polish than Jewish, and most Poles were Catholic.

So ends the first two chapters. Next is the first interlude, on the Banach–Tarski Paradox, set theory, and the Axiom of Choice. In 1924 Banach and Tarski published a paper containing what is now known as the "Banach–Tarski Paradox," that "a pea can be cut up to make the sun." There is a brief review of infinity, including Aristotle's account of Zeno's Paradox, Galileo's discussion of paradoxical properties of infinite sets, and Cantor's introduction of cardinal numbers. Cantor believed but could not prove his Trichotomy Principle (that any two cardinals are comparable) and he introduced the Well-Ordering Hypothesis (that every set can be well-ordered), which he regarded as intuitively true, although he also tried unsuccessfully to prove it. Zermelo first proved the Well-Ordering Hypothesis in a paper of 1904, and, in a second publication in 1908, formulated as axioms all the set-theoretical principles used in his proof, including the crucial and essential Axiom of Choice. This axiom was attacked by some but accepted by others, such as Hausdorff, whose book on the foundations of set theory, published in 1914, was studied by Sierpinski and Tarski. In 1918 Sierpinski assembled several statements equivalent to the Axiom of Choice (including Cantor's Trichotomy Principle), and Tarski extended the list in one of his three papers on set theory published in 1924. Tarski proved, for example, that the Axiom of Choice is equivalent to the statement that $\mathfrak{m}^2 = \mathfrak{m}$

for every infinite cardinal $\mathfrak{m}$. Using the Axiom of Choice, Hausdorff had shown that there is no good notion of measure for sets in dimension 3 or more, while Banach had shown in 1923 that there *is* such a notion for dimension 2. Hausdorff worked with the surface of a sphere, but Banach and Tarski independently discovered paradoxical decompositions using solid balls. They collaborated on a joint paper, combining their results and jointly obtaining new ones. (Tarski told me once that he regretted their decision to share credit for all the results in that paper, since Tarski's original version of the paradox was stronger than Banach's.) The interlude mentions a few subsequent developments, including Gödel's proof of the consistency of the Axiom of Choice relative to Zermelo-Fraenkel set theory and a theorem of Solovay which implies that the Banach–Tarski Paradox requires the Axiom of Choice. The interlude concludes with remarks on Tarski's philosophical attitude toward set theory (which was anti-Platonist), for which the sources are tape recordings and memories.

In 1924 Tarski became the youngest docent at Warsaw University, only 23 years old. Samuel Eilenberg was one of the students who attended his logic course. Tarski's salary as docent was small, so he also worked at the Polish Pedagogical Institute of Warsaw, which trained young women for teaching. He lost that job when there were complaints about the mathematics teacher being Jewish. His next job was as teacher at the Żeromski Lycée, a private school in his neighborhood. Besides his two jobs, Tarski attended seminars at the university, published 50 papers in 1924–1939, wrote the first version of his introductory logic textbook, and co-authored a high-school geometry textbook. How did he do it? "He was limitlessly energetic, enthusiastic, organized, aggressive, and competitive, and he had an iron constitution. Tarski may have been small, but he was sturdy and healthy. Almost until his death he could outtalk, outargue, outdrink, and outlast anyone who tried to stay awake with him into the wee hours." He "became a dedicated hiker and even did some mountain climbing." From 1923 onward he took his vacations in Zakopane and kept a journal chronicling his hikes to various peaks in the Tatra mountains. Evidence of some of Tarski's affairs comes from that journal.

In the mid-1920's Tarski was engaged to Irena Grosz, a writer and political activist. They went their separate ways as did their politics, his to the right, hers to the left, but they remained lifelong friends. Tarski married Maria Witkowska in 1929. She had moved to Warsaw from Minsk. In Warsaw she lived with her sister Józefa and taught young children in the elementary wing of the Żeromski school, where she met Tarski. After their marriage they moved to a flat in a new housing estate in the northern part of Warsaw. Until then, Tarski had lived at home with his parents.

In 1928 a new professorship in logic was created at the University of Lvov. Tarski and Leon Chwistek were the candidates. The Polish mathematicians and logicians supported one or the other; even Bertrand Russell was consulted. The position went to Chwistek in 1930. Many people, then and now, thought Tarski should have gotten it, so "the Lvov affair" has long been the subject of much discussion and speculation, especially since Tarski might well have perished in World War II if he had had a Polish professorship.

The focus on first-order logic and the method of eliminating quantifiers were initiated by Leopold Löwenheim in his paper of 1915, and were extended and applied by Thoralf Skolem and C. H. Langford in the 1920's. Tarski was charged with running the "exercise sessions" for Łukasiewicz's seminar in the years 1927–1929. He suggested to Mojżesz Presburger the "exercise" of applying the elimination of quantifiers to the first-order additive theory of natural numbers. Presburger's positive solution in 1928 has subsequently caused the theory to be called "Presburger arithmetic." Tarski himself applied the method to elementary algebra and geometry. By generalizing an algorithm due to C.-F. Sturm, Tarski proved by 1930 that elementary algebra and geometry are complete and decidable, a result that he regarded as one of his two most important contributions, the other being the theory of truth.

Karl Menger, a year younger than Tarski but already professor of geometry at the University of Vienna and member of the Vienna Circle, visited Warsaw in the autumn of 1929. At Menger's invitation, Tarski went to Vienna in February 1930 and gave three talks to the Vienna Circle. There he met Rudolf Carnap, who had studied with Gottlob Frege in Jena, and Kurt Gödel, who had just completed his Ph.D. under Hans Hahn. There is a photograph of Tarski and Gödel together in Vienna. At Tarski's invitation Carnap visited Warsaw in November 1930. In January 1931, Gödel wrote to Tarski, announcing his incompleteness results. This was a second great disappointment to add to his loss of the Lvov professorship, since Tarski had been close to achieving such results himself. Tarski used Gödel's method to prove his famous theorem on the undefinability of truth. Willard Van Orman Quine, a new Ph.D. from Harvard, visited Warsaw in May 1933, and six years later, played a crucial role in saving Tarski's life.

Tarski began an affair with Maria Kokoszyńska-Lutmanova around that time. She accompanied him to Vienna in 1935, where she took a famous photograph of Tarski and Gödel standing together in the street, and to Paris later that year for the Unity of Science conference. During the planning for that conference in Prague in 1935, Tarski first met Karl Popper. Popper showed Tarski the page proofs of his ***Logik der Forschung*** [Logic of Scientific Discovery], and when they met again in Vienna, Tarski showed Popper the page proofs of his great paper on the concept of truth. Popper later wrote, "Although Tarski was only a little older than I, . . . I looked upon him as the one man whom I could truly regard as my teacher in philosophy. I have never learned so much from anybody else." Kokoszyńska-Lutmanova wrote letters to Tarski decades later, "quite directly saying she did not realize how much he had taught her."

At Carnap's urging, Tarski gave two talks at the Unity of Science conference in Paris in September 1935, one on his theory of truth, the other on the concept of logical consequence. These talks created such a sensation and vehement opposition that an additional session was scheduled for a discussion of the controversy.

Tarski traveled, lectured, and met with colleagues all over Europe in 1935–1938. For example, as he told Herb Enderton in 1978, while on a walk with Kurt Grelling he witnessed a historic speech by Hitler in Berlin in September 1938. Later during that same visit Grelling arranged a party at which Tarski first met Leopold Löwenheim. Tarski recalled, "Löwenheim told me I was the first logician of academic status he ever met."

Back in Warsaw, Tarski supervised the Ph.D. work of Andrejz Mostowski, completed in 1938. Although Tarski had been promoted to adjunct professor in 1935, he was not a *full* professor, and therefore could not sign the dissertation. Shortly after that he met Wanda Szmielew. His journal of hikes in the Tatra mountains thereafter has numerous entries for Szmielew and no more entries for Kokoszyńska-Lutmanova.

In 1937 Tarski was the obvious choice for a new faculty position at the University of Poznan but did not get it. Rather than make public the reason for the denial (anti-semitism) the post was dissolved. In September 1939 Leśniewski died of thyroid cancer. His natural successor was Tarski, but the problem of anti-semitism was even worse by then. Although Tarski had been invited by Quine and others to attend the Fifth International Conference on the Unity of Science, scheduled to occur at Harvard in the late summer of 1939, Tarski hesitated to accept, wanting to be in Warsaw when the decision about Leśniewski's replacement was made. Quine was puzzled by Tarski's willingness to remain in what seemed from abroad to be a very dangerous situation, and wrote again suggesting that Tarski test the waters for a position in the USA. Quine offered to arrange a series of lectures at various universities after the conference. At that, Tarski finally agreed to go. He applied for a visitor's visa, received it on 7 August 1939, and sailed four days later on 11 August 1939. Adolf Lindenbaum's wife applied for her visa only a few days after Tarski, but it was denied, so she and Adolf had to remain in Poland, where they died in the holocaust. Coincidentally, Stanislaus Ulam and his brother Adam were on the same boat. A photograph, obtained from Ulam's widow Francoise, shows Tarski, the Ulams, and an unidentifiable female companion whose image has been scratched out. The boat arrived in New York on 22 August 1939, greeted by von Neumann for the Ulams, and Carl Hempel for Tarski. Germany invaded Poland on 1 September 1939 and Tarski presented his talk to the conference eight days later. The invasion of Poland by Germany had occurred only three weeks after Tarski left for the United States with only a small suitcase of summer clothes. He was trapped outside Poland, his wife and two children were still there, and he did not see them again for another seven years.

The next interlude is a detailed explanation of Tarski's theories of truth. The story picks up again in the United States. Funds were cobbled together at Harvard to appoint Tarski as research associate, but he needed a permanent job and had the wrong kind of visa. He applied for a permanant visa from Havana. It was granted on 29 December 1939, and Tarski spent New Years Day of 1940 in Miami. As part of his job search Tarski lectured to Ernest Nagel's undergraduate philosophy class at Columbia. One of the students in the class, who had never heard of Tarski but eventually became his right-hand man in Berkeley, was Leon Henkin. Tarski was offered a visiting professorship at the City College of New York for the spring of 1940. There he gave his first full-semester university course in the USA. He lectured on the calculus of relations, the same topic as his last course in 1973. In his class were J. C. C. McKinsey, who became "the only real friend he had had in the European sense" in the USA, and an undergraduate who later won a Nobel Prize in economics, Kenneth Arrow, who wrote, "In fact what I learned from him played a role in my own later work – not so much the particular theorems but the language of relations was immediately applicable to economics". Tarski's appointment to CCNY produced a small article in the New York Times on 21 November 1939, in which Bertrand Russell is quoted, calling Tarski "the ablest man of our generation in logic and semantics." Tarski and McKinsey both wrote (and assisted each other with) separate papers on the calculus of relations, but later collaborated on three other papers. In the same period Tarski colloborated with Erdös on the first of two joint papers. Tarski spent the spring semester of 1942 as a Guggenheim fellow at the Institute for Advanced Study, where he discussed his results on the calculus of relations with Gödel.

In the summer of 1942 Tarski started a book-length manuscript on the calculus of relations, and at the end of that summer he made his great journey across the continent to become assistant professor at the University of California at Berkeley. Thus began the second half of his life, an extraordinary whirlwind of publications, students, colleagues, visitors, courses, seminars, travels, conferences, hirings, empire building, controversies, conflicts, triumphs, tragedies, deaths, family affairs, extramarital affairs, separations, reunions, estrangements, reconciliations, parties, honors, and honorary degrees. Almost thirty years after his arrival at Berkeley, a conference in honor of his 70th birthday commemorated a monumental career. As his contribution to the proceedings, Tarski intended to revise his 1942–3 manuscript, but instead he spent the last 12 years of his life completing it as his last major work. The resulting book, ***A Formalization of Set Theory without Variables***, was finished just before he died on 27 October 1983. At a memorial service, Henkin described Tarski as "proud, penetrating, persistent, powerful, passionate" and characterized by "that supreme absence of self-doubt".

The authors have written a delightful, fascinating, and vivid portrait of an extraordinarily dynamic, dramatic, demanding, and influential figure in 20th century logic, mathematics, and philosophy. Many times while reading this book I thought, "Yes! That's what he was like!"

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The essential Turing, edited by B. Jack Copeland, Oxford University Press, 2004, vii + 613 pp.

Do you know who is Alan Turing? Have you read any of his papers? It is often the case that an original paper introducing a new concept contains more than what you can find in later textbooks, and in the case of Turing this is doubly so. Although everyone knows the notion of the Turing Machine, reading the original paper about it feels like a revelation.

So what can you find in this book? To start with, this book is not a biography of Turing, at least not in the traditional sense. It is a collection of brilliant papers, thoughtful lectures and some correspondence, including a letter to Winston Churchill. Not everybody knows the full extent of Turing's contribution to science. The breadth of the topics in this book is simply awe-inspiring and hard to believe.

The book itself follows Turing's work as it happened—the papers are presented more or less in chronological order. The editor decided to split the book into four parts, roughly corresponding to four main areas of Turing's work. These are: Computable numbers, Enigma, Artificial Intelligence and Artificial Life. Each of these parts is preceded by introductory material, giving historical background and relevant information needed to understand the papers which follow. Two of the introductory sections are quite substantial.

The first one, called *On Computable Numbers: A Guide*, is a primer into basic theory of machine computation, computability and logic. This allows even readers without computer science background to understand the material in this book. However, even seasoned computer scientists may find there something new, especially in the historical notes. The second longer introductory material is on Enigma. Most of the papers are also put in the context separately, with an introduction at the beginning of each chapter. As a big plus, all introductory material is heavily supplemented by references to the original sources.

Computable numbers. The first work of Turing in this collection is his most famous and arguably the most important one: *On Computable Numbers with an Application to the Entscheidungsproblem* (1936). This is THE paper where Turing Machines were introduced. It makes for a very interesting reading and is a must read even if you know the theory of Turing machines. The editor made a very good decision to include the chapter *On Computable Numbers—Corrections and Critiques*. You can find there Turing's own corrections from 1937, Post's critique and a thorough contribution by Donald Davies, containing a redesign of the universal machine **U**.

The following chapter *Systems of Logic Based on Ordinals* (1938) contains Turing's PhD thesis (supervised by Church), later published in the ***Proceedings of the London Mathematical Society***. Again a very interesting work, even though the use of λ-calculus notation makes it harder to read than necessary. And as Copeland points out, this work also introduces machines with oracles, still on curriculum today.

Enigma. It is a well known fact that Turing contributed immensely to breaking the Enigma code during World War II while working in the famous Bletchley Park. This part of his life is well covered by several books about Enigma, but usually without any details of the problems worked on. This is partly because most documents from that time have been secret and only recently released to the public. The other reason is that the technical side is not that interesting to a broad audience. Again Copeland provides quite a detailed introduction containing historical context and references.

Chapter 5 *History of Hut 8 to December 1941* is not by Turing, but by his (then) colleague Patrick Mahon. It was written shortly after the war and provides us with the description of the work done on Enigma and Turing's contribution to that. What follows is the chapter *Bombe and Spider* from Turing's famous 'Prof's book' (***Treatise on the Enigma***)—a write-up of Turing's work on Enigma, a bible of Bletchley Park cryptographers. There is also a letter of Turing to American cryptographers dealing with some aspects of breaking Enigma. Finally you can find here the letter to W. Churchill (written together with three of Turing's colleagues), which secured the necessary resources needed to crack ciphers in a timely manner.

Artificial Intelligence. The next big area Turing had a tremendous impact on is Artificial Intelligence. The first chapter in this part of the book is his *Lecture on the Automatic Computing Engine*, given in 1947 to the London Mathematical Society. It is the first work mentioning computer intelligence, but gives mainly the technical information behind the design of ACE. (Do you know how the mercury delay lines worked?) Next is the report called *Intelligent Machinery*, the first manifesto of Artificial Intelligence. It may be a surprise to some that Turing deals in this paper with neural networks.

What follows is the other Turing's masterpiece: *Computing Machinery and Intelligence*. This paper is of more philosophical nature and introduces the 'ultimate' intelligence criterion—the imitation game today known as Turing's test. In the rest of this part of the book are three Turing's radio broadcasts on AI: lectures *Intelligent Machinery, A Heretical Theory*, *Can Digital Computers Think?*, and *Can Automatic Calculating Machines Be Said To Think?*, a panel discussion. All three are well worth reading.

Artificial Life. The only Turing's paper in the last part of the book is *The Chemical Basis of Morphogenesis*. This paper is very different to Turing's work on computability. It deals with morphogenesis, i.e., the way by which genes determine the anatomical structure of a living organism. To solve this problem Turing invented an idealised chemical model, nowadays called 'the reaction diffusion model'. Knowledge of calculus, and some elementary biology and chemistry, comes handy in this chapter, but not much more is required from the reader. The chapter is fascinating and well deserves reader's attention.

The last two papers do not really belong to the last part of the book. *Chess* is a short essay on the design of a chess program, more suited to the AI part. The final paper is called *Solvable and Unsolvable Problems*. In this paper we get round the full circle back to the Turing's results presented in the first chapter. However this time we find a paper for a lay audience, the readers of 'Science News'.

So to whom is this book intended? It may be surprising, but for example this is the first time the most important Turing papers appeared in an affordable book. (***The Collected Works of A. M. Turing*** being very expensive.) Secondly, it is inspiring reading for both students and seasoned academics—especially in its breadth. It is also accessible to anybody with a good knowledge of mathematics who wants to broaden his/her horizons. However if you are looking for a biography or the less known works of Allan Turing, you should get a different book, or check the internet archive of his works.

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Itay Neeman. *Games of countable length*. ***Sets and Proofs (Leeds, 1997),*** edited by S. Barry Cooper and John K. Truss, London Mathematical Society Lecture Note Series, vol. 258. Cambridge University Press, Cambridge, 1999, pp. 159–196.

Itay Neeman. *Unraveling Π^1_1 sets*. ***Annals of Pure and Applied Logic***, vol. 106, no. 1–3 (2000), pp. 151–205.

Itay Neeman. *Unraveling Π^1_1 sets, revisited*. ***Israel Journal of Mathematics***, to appear.

These three papers are concerned with the determinacy of games of perfect information between two players, where the players take turns playing either real or natural numbers. The first paper contains a very readable expository introduction to the study of determinacy for games which last for countably many rounds. Such a game is determined by the length of the game, whether the players are to play real numbers or natural numbers, and the payoff set, the set of sequences which are winning plays for the first player. One important issue is finding the minimal large cardinal concept implying the determinacy of a given game. While some results on games of fixed countable length are mentioned, variable length games are the focus of the paper. The author defines a *length condition* to be a set $L \subset \mathbb{R}^{<\omega_1}$ with the property that every member of $\mathbb{R}^{\omega_1}$ has an initial segment in L. A variable length game is then determined by a length condition L and a payoff set. A run of the game then consists (modulo some simple coding) of a countable sequence of real numbers, where the game ends at the first point that the play so far is in L, with the winner determined by the payoff set. For example, in the *continuously coded game* G_{cont}, Player I is required to play a natural number along with her (real number) move in each round, and the game ends either when she runs out of natural numbers or plays some number twice. The *f-continuously coded game* G_{f-cont} is defined relative to a function $f: \mathbb{R}^2 \to \mathbb{N}$. In round ι, the players collaborate to build the real x_ι as in the usual ω-length game on natural numbers. Then, using the fact that the association of a distinct natural number to each round induces a canonical coding of the play so far by a real z_ι, n_ι is defined to be $f(z_\iota, x_\iota)$, and again play continues until the n_ι's exhaust the natural numbers or include the same one twice. The f-continuously coded game is stronger than the continuously coded game, in that each continuously coded game can be simulated by an f-continuously coded game with the right payoff set and the right choice of f. For each game, the payoff set is said to be in a given pointclass $\Gamma \subset \mathcal{P}(\mathbb{R})$ if the set of codes for winning positions for the first player is in this class. The author showed in his dissertation that if there exists a so-called *strong past a Woodin* cardinal, then all continuously coded games with Π^1_1 payoff are determined. The main result in the paper, due to the author in collaboration with John Steel, indicates that this result is close to optimal.

Stating the result requires fixing some terminology, however. First, to each game g there is associated a *game quantifier* $\Game_g$. For a game g whose payoff set is coded by a set of reals, if $A \subset \mathbb{R}^2$, then $\Game_g A$ is the set of reals a for which Player I has a winning strategy in the game g with payoff $A_a = \{b \in \mathbb{R} \mid (a,b) \in A\}$. The other elements of the result use terminology from inner model theory (mice, iteration trees, comparison) which are defined in the paper but which we will not try to summarize here. Skipping several steps, a mouse is said to be *tame* if its extender sequence contains no extenders which appear to be strong past a Woodin cardinal when listed. Then $\mathbb{R}^{tame}$ is defined to be the set of real numbers appearing in tame mice, and $\trianglelefteq^{tame}$ is the canonical wellordering of $\mathbb{R}^{tame}$ induced by the comparison process on (tame) mice. The main result of the paper then is that G_{f-cont}-determinacy for all $f: \mathbb{R}^2 \to \mathbb{N}$ implies that there is a function $f: \mathbb{R}^2 \to \mathbb{N}$ such that the wellordering $\trianglelefteq^{tame}$ is $\Game_{f-cont}\Delta^1_2$. The upshot is that if G_{g-cont}-determinacy holds for all $g: \mathbb{R}^2 \to \mathbb{N}$, then there is no uncountable wellordered $\Game_{f-cont}\Delta^1_2$ sequence of reals, and so, by the main result of the paper, the set of reals appearing in tame mice is countable. Further, no large cardinal appearing in a tame mouse can prove G_{f-cont}-determinacy for all $f: \mathbb{R}^2 \to \mathbb{N}$.

The proof proceeds by repackaging the iteration games used in the comparison of two tame mice as an f-continuously coded game for a suitable function f. What makes this reasonable is that the comparision processes consists of iteratively identifying the least point of disagreement between the two mice in question, and applying an elementary embedding which removes the disagreement. Since the models are countable and each disagreement is dealt with just once, one can associate an integer to each round. The most serious difficulty here is that to directly model comparison the two players would have to play the infinite branches through their trees simultaneously, which they can't do in a game of perfect information. This difficultly greatly increases the complexity of the argument, requiring each player to guess moves (cofinal branches through the iteration trees played so far) for her opponent; if these guesses are wrong, play then continues in separate runs of the game corresponding to each guess. Tameness of the mice is used repeatedly to get around the difficulties, using the key theorem of Steel (stated in the paper) that iteration trees with two cofinal branches with wellfounded limit models give rise to (partial) Woodin cardinals.

The second paper concerns integer games of length ω. In 1975, D. A. Martin (*Borel determinacy*, JSL XLIX 1425) proved that all such games with Borel payoff set are determined. He later produced a simplified, inductive version of this proof which introduced the notion of a *covering*, roughly, a function π from the set of runs of a suitably defined game T to the set of runs in another game S, along with a mapping Ψ sending strategies in T to strategies in S. The key property of the mapping Ψ is the following lifting condition: if Σ is a strategy in T and y is a play in S according to $\Psi(\Sigma)$, then there is a play x of T according to Σ such that $\pi(x) = y$. If A is a set of runs of S, the covering is said to *unravel A* if $\pi^{-1}(A)$ is clopen. Since clopen games are determined, finding a covering which unravels a set of reals shows that the corresponding game is determined. Martin's simplified proof of Borel determinacy then proceeds by showing that all Borel sets can be unraveled. The question of whether Π^1_1 sets could be unraveled was left open, and answered positively in the second paper under review. Since Π^1_1-determinacy implies that $0^\#$ exists, one cannot expect to produce an unraveling for Π^1_1 sets in ZFC, and in fact Steel had previously shown that the existence of an ordinal κ with $o(\kappa) = \kappa^{++}$, where o represents the Mitchell order on measures, is a consistency-strength lower bound for Π^1_1 unraveling. The construction in the second paper unravels Π^1_1 sets from a hypothesis with exactly this consistency strength.

Unraveling Π^1_1 sets does more than prove the Π^1_1-determinacy, for which the optimal proof is due to Martin. Since Borel subsets of the set of runs of T are known by Martin's Borel determinacy result to be determined, one gets the determinacy of all sets in the σ-algebra generated by the Π^1_1 sets if one can produce a covering which simultaneously unravels any given countable collection of Π^1_1 sets. In Section 6 of the second paper the author does just that. If $T^\#$ exists one has furthermore that Σ^1_1 subsets of the set of plays of T are determined, which gives the determinacy of a still wider class of subsets of ω^ω, though still a subclass of Δ^1_2.

The argument in the second paper, however, is long and very difficult, and the third paper presents a greatly simplified construction which unravels a given Π^1_1 set. It suffices here to unravel the complete Π^1_1 set, the set of reals coding wellorderings in some fixed association $s \mapsto \preceq_s$ of members of $\omega^{<\omega}$ to finite linear orders. To do this, the author defines a game T which is some sense the disjoint sum of two games, called the *repeated rank game* and the *inverted rank game*. In the first round of T the first player proposes a set on which to play and the second player chooses which of the two games to play with that set. Each of the two games continuously produces a real (among other things), and in the repeated rank game this real codes a wellfounded linear order, while in the inverted rank game it produces an illfounded one. The preimage of the set of reals coding wellorderings under this association is then a clopen subset of the runs of T. Most of the work in the paper involves defining the function Ψ. The author first shows that to establish the lifting condition it suffices to construct for each run of the ordinary game a certain system of elementary embeddings and partial runs of T (from both subgames) which agree about the associated plays in the ordinary game. Then, given a strategy Σ in T, the associated strategy $\Psi(T)$ in the ordinary game involves (on the side) the continuous construction of such a system. The strategy $\Psi(\Sigma)$ is then guided by the plays of the games in this system, half of which are given by Σ itself. For the other half one uses the fact that the two games are, as their names suggest, roughly inverses of one another, in that the role of I in one game is approximated by the role of II in the other and vice versa. The strategy Σ then is used to fill in moves for the opposite player in the plays of the game of the opposite type. Though measurable cardinals are used throughout the construction, the full strength of the hypothesis is used at just one point, to ensure the coherence of these plays on both sides of T.

These three papers present important developments in the recent study of determinacy. They are technically difficult, yet very carefully and clearly written.

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