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REVIEWS

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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).

RISTO VILKKO. ***A hundred years of logical investigations. Reform efforts of logic in Germany 1781–1879.*** Mentis, Paderborn, 2002, 208 pp.

This book is a contribution to the new general trend in historiography of symbolic logic aiming at a revision of the standard and prevailing evaluation of 19th century logic. In the last twenty years historical evidence has shown that "classical" modern logic, which ranged from the 16th century to the 19th century, was also a creative period. This standpoint has been supported by many papers and books written recently by Christian Thiel, Ivor Grattan Guiness, Peter Simons and Volker Peckhaus, among others. Thus, the reader should be advised that the aim of the author is rather historical than philosophical: to correct the standard assessment of 19th century logic by considering the conceptions of logic sketched by German philosophers and mathematician during the 19th century. Despite its subtitle, the book focuses on the period between Hegel's death (1831) and the early 1880's, when mathematicians began to work intensively on logical matters, and foundational problems in mathematics came to the forefront.

The book is organized in six chapters and three appendices. The first chapter is a nine-page introduction and the last chapter a four-page conclusion. Chapters 2 to 4 are devoted to the 19th century philosophical discussion about the nature of logic and its relations to epistemology, methodology and metaphysics. So chapter 2 deals with the philosophical discussion in Germany about the reform of logic, that is, the so-called *logische Frage* (logic

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question). This discussion took place in post-Kantian and post-Hegelian philosophy, when the possibility of logic had to be justified as a philosophical discipline and as theory of science. Chapter 3 provides an exposition of the background of the logic question: the place of formal logic in Kant's philosophy, the confrontation between Herbart's school and Hegel, and the logical work of Moritz W. Drobisch, who in his early book ***Neue Darstellung der Logik*** (1836) included an appendix with an algebraic calculus of syllogism. The analysis made in this context of Hegel's criticism to formal logic is very illuminating. Chapter 4 is devoted to the 19th century influential philosopher Adolf Trendelenburg, whose criticism of both, Herbart and Hegel, led him to pose his *logische Frage* in 1842. Vilkko stresses that this question is not related to logic, but to metaphysics and epistemology. However, he does not mention here the relevance of Trendelenburg in transmitting many ideas of Leibniz that influenced later the conception of logic of Frege and Schröder.

Chapters 5 and 6 constitute a second part of the book dealing with the logical work of mathematicians during the second half of the 19th century. From these chapters the author intends to get a re-evaluation of Schröder's and Frege's historical context. Chapter 6 gives an account of the development of the algebra of logic in Germany during the second half of the 19th century, focusing on the logical work of Robert Graßmann and Ernst Schröder. In the case of the former (brother of the famous mathematician Hermann Graßmann), the author emphasizes the independence of his *Formenlehre*, or doctrine of forms, from the British algebra of logic, and the influence of the Leibnizian tradition (exemplified by Johann Heinrich Lambert and August D. Twesten). In the case of Schröder, the purpose of the author is to show the connection of his logic work to the discussions about the reform of logic (p. 101). The sections dedicated to Schröder owe a big deal to previous research due to Volker Peckhaus on this subject. Rilkko points out accurately that Schröder investigations are concerned with the algebraic structure of logic and not with some algebraic method of logical notation. Thus, Schröder carried out a "structural theory of logic" (section 5.2.2), according to which logic should be seen as a model of formal or abstract algebra. The chapter also outlines Schröder's project of a scientific universal language on the basis of his algebra of logic, and more generally, section 5.3 defends the idea of an universal-algebraic programme both in Graßmann and Frege.

Chapter 6 seeks for "a hermeneutic historiographical dialog" with Frege, discussing his conceptual notation. As he states in the introduction, (p. 19) in this chapter the author challenges the "Dummettian" approach according to which Frege's results should not be considered in relation to their historical background. Thus, Frege's motivation for writing ***Begriffsschrift*** is clarified. The roots of Frege's conceptual notation in the Leibnizian tradition of *lingua rationalis* are stressed, and the influence of the Kantian tradition is also mentioned (p. 141), but without going deeply into the subject. Now, Vilkko analyzes in detail the reception of ***Begriffsschrift***, questioning the widespread assumption that it was poor and entirely hostile. He takes into account six reviews of Frege's book (in fact he mentions eight reviews, but the remaining two are not made explicit), remarking their positive assessments (even in Schröder's rather negative review). Thereafter, Frege's reaction to criticism is analyzed.

In the concluding remarks of chapter 7, Vilkko observes that the proper understanding of the "revolution in logic" of late 19th century calls for an acquaintance with the philosophically oriented discussion on the reform of logic in the first half of that century (p. 148). For this reason, the most of the new material is clearly to be found in chapters 2 to 4. In this respect, both the description of Drobisch's algebraic calculus and appendix 1 with a comparison of the first three editions of Drobisch's logic book are very illuminating. Appendix 2 gives a detailed description of the Herbart's school and its members. Notwithstanding, the discussion about the reception of the ***Begriffsschrift*** throws as well some new light on this first stage of symbolic logic, revising the context from which the conceptual notation and quantificational logic emerged. It must be noticed that appendix three reproduces the original German texts of the quotations made throughout the book.

In summary, with his book Vilkko succeeds in offering a clear and interesting account of the historical background of the rise of symbolic knowledge in 19th century Germany. In doing so he discusses the work of not well-known figures of the German philosophical and logical landscape at that time. However, not all of the main logicians in the German speaking world are taken into account. The case of Bernard Bolzano could be mentioned as an example. But it can be argued that his logical work lies beyond the scope of this book since it belongs to a different branch in the rich and complex history of modern logic. A discussion of the influence of Hermann Lotze in Frege's logic is also missed in the book. Though this is a controversial point, one should expect that the author expressed at least his own position about it. A misprint could be found in a reference of p. 123: 'Frege 1874' should be read as 'Frege 1874b'. Despite this minor remarks, it is beyond doubt that this book gives an useful and lucid account of the idea of logic prevailing in the 19th century Germany, being a highly valuable contribution to the understanding of the rise of symbolic logic.

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V. A. Bazhanov. ***Ocerki sotsialnoj istorii logiki v Rossii*** [***Sketches of the social history of logic in Russia***]. Izdatel'stvo Srednevolzhskogo nauchnogo tsentra, Simbirsk-Ul'yanovsk, 2002, 124 pp.

V. A. Bazhanov is well-known for a variety of works on the social history of logic in Russia. In the book under review he describes the circumstances of life of Russian logicians in the transition period from the czarist Russia until the thirties of 20th century in the Soviet Union.

Bazhanov starts his work with a survey of the development of logical thought in Russia. In the following chapter he describes the scientific and social political work of Alexander Vasilevitch Vasil'ev (1853–1929), father of the founder of imaginary logic, N. A. Vasil'ev. A. V. Vasil'ev was one of the most important Russian mathematicians of his time. He was acquainted to B. Russell, encouraged P. S. Poretskij to work in logic, and contributed to the philosophy of mathematics especially by discussing the work of Lobatschewski. He wrote a biography of Lobatschewski, which was printed in the Soviet Union, but not delivered, and eventually destroyed in the end of 1929. A new edition appeared only in 1992.

Already in 1920 A. V. Vasil'ev described the dilemma of the Russian intelligentsia at that time: "There exists a hard alternative: Either the rejection of the best traditions, which were developed by mankind on its hard way, especially the defense of the freedom of thought and speech, the defense of the value of man and the value of science—or the escape from the native country and the refusal of direct work for the benefit of the people. Is there really only a tertium non datur?" (Vasil'ev, A. V., *P. L. Lavrov—istorik i filosof matematiki*. In: ***Sbornik pamyati P. L. Lavrova***, Petrograd: Kolos, 1922, pp. 373–384).

The second chapter deals with Nikolai Alexandrovitch Vasil'ev (1880–1940), the son of A. V. Vasil'ev. Bazhanov sketches the story of his life. Doing this, he touches on the varied interests of N. A. Valsil'ev, reaching from poetry to medicine, psychology, philosophy, logic. His work in logic became most important.

His life almost entirely takes place in Kazan, where he finishes his studies in medicine with distinction in 1904. In 1906 he passes the exam of the historical-philological faculty, then entering a program for the preparation of a professorship. In 1917 he is appointed a docent and in 1918 he became a professor. N. A. Vasil'ev was very well acquainted with international literature. In 1911 he spent on a study visit in Germany, France and England, where he began to prepare his extensive work on imaginary logic.

The completion of this work was prevented by the First World War and a nervous disease, which first occurred at the front and eventually caused his committal to a mental hospital, where he died in 1940.

The author knows to describe the many-sided interests of N. A. Vasil'ev. Concerning Vasil'ev's logic, the author can rely on secondary literature. At some places the appreciation seems to be too wholesale and formalistic. So when N. A. Vasil'ev is recognized on p. 67 not only as the founder of paraconsistent logic, but also of intuitionistic and many-valued logic. Here the notion "forerunner" would be appropriate and in respect to intuitionistic logic even this seems to be too strong. In fact, already in his first work about non-Aristotelian logic N. A. Vasil'ev declares the law of excluded third as unsound and replaces it by the law of excluded fourth. However, the justification for this replacement is entirely independent of the intuitionistic critique of the law of excluded third.

Chapter 3 is devoted to I. E. Orlov (1886–1936). Here the author presents interesting facts about the scientific work of Orlov and his life in the Soviet Union. Like N. A. Vasil'ev, Orlov is a scientist of the generation of the eighties of the 19th century, receiving education in czarist Russia. These scientists had the alternative either to submit to the dogmas of Marxist-Leninist philosophy or to emigrate and to continue their work outside Russia (cf. p. 73).

Orlov became world-renowned for his 1928 article "Calculus of the compatibility of propositions" (*Ischislenie sovmestimosti predlozhenij*, ***Matematicheskij sbornik***, vol. 35 (1928), pp. 263–286). In this paper he presented the first axiomatization of a system of relevant logic (precisely of the implication-negation-fragment of system R).

Orlov pursued a manifold of scientific interests. So he worked not only in the field of logic, but also in psychology, logic of science, philosophy of mathematics, philosophy of physics, theory of music and chemistry, the last entirely occupying his attention during the thirties.

Orlov began publishing already in 1914. His publications during the Soviet time were sometimes ideologically determined. However, in his book "Logic of science", published in 1925, neither Marx nor Lenin were mentioned. Only Engels occured at one place.

In the philosophy of mathematics Orlov contributed a critique of Cantor's theory of sets. The emphasis of this critique was directed against Cantor's acceptance of the actual infinity: "In Cantor's theory there is no dialectics. His contradictions have plain, formal character, which is based on simple ignorance" (Orlov, I. E., *Suzhchestvuet li aktual'naya beskonechnost'*, ***Pod znamenem marksizma***, vol. 1 (1924), pp. 136–147, p. 147).

During the years 1928–1929 Orlov changes his main fields of work. He abandons philosophy and logic and turns entirely to chemistry. Orlov seems to be no victim of reprisals, because three years after his dead his book on the production of iodine and bromine was published.

Chapter 4 is dedicated to the work of Viktor Ivanovitch Shestakov (1907–1987). The author opposes B. V. Biryukov's thesis that C. E. Shannon took his idea of logical circuits through G. Birkhoff from Shestakov. It is undisputed, however, that Shestakov developed his basic idea before Shannon, even if Shestakov did not gain the international recognition, Shannon found. In the thirties of the 20th century Shestakov invented applications of logic in different fields of engineering.

In his chapter on S. A. Yanovskaya (Janovskaja) (1896–1966) the author concentrates on her biography in the thirties of the 20th century. The author asks, how a person, strongly committed to Marxist-Leninist ideology in its most militant variant, who severely attacked her ideological opponents as enemies, could become a person, who deliberately and forcefully rebuilt, what she had destroyed earlier, while circumspectly separating the seeds of new tendencies from the old hostile relations.

The author asks, what were motives and the basis of this ideological overthrow. He asks, to what extent Yanovskaya's fate was typical, to what extent does it mirror the fate of science, philosophy and logic in the USSR, of Soviet philosophers and logicians.

In 1931 Yanovskaya became a so-called red professor. Following A. F. Losev, she received this title for an ideological campaign in the mathematics. In 1930 she had established that only few Marxists were among the university mathematicians of the Moscow School. Even the word "comrade" was not used in the Mathematical-Mechanical Institute and in the Mathematical Society (cf. Yanovskaya, S. A., *Ocherednye zadachi matematikov-marksiztov*, ***Pod znamenem marksizma***, vol. 5 (1930), pp. 88–94, p. 88). For Yanovskaya, the most important problem in the development of mathematics is the problem of the cadres, viz. the party cadres among mathematicians. With the persecution of professors of the Moscow School "the year of the big upheaval at the front of mathematics" began (Yanovskaya, S. A., *Ocherednye zadachi matematikov-markcsiztov*, ***Pod znamenem marksizma***, vol. 5 (1930), pp. 88–94, p. 91) providing the solution of the problem of the cadres (cf. p. 111).

During the thirties Yanovskaya writes about idealism in mathematics unmasking "the bourgeois philosophy of mathematics" (cf. p. 113). Her positive contribution to the development of logic in the USSR was the organization of the seminar of mathematical logic in 1943, which she headed together with I. I. Zhegalkin and P. S. Novikov. Yanovskaya supported the Russian editions of many western publications about formal logic, starting in the year 1948 with Hilbert's and Ackermann's "Outlines of theoretical logic".

The author has gathered in his book contributions to the circumstances of life and work of some key figures in Russian logic. Almost complete bibliographical data for each chapter are especially helpful for further work on these logicians. The book deserves readers outside the Russian speaking community. Therefore, an English edition of the book would be welcome.

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DENNIS E. HESSELING. ***Gnomes in the fog. The reception of Brouwer's intuitionism in the 1920s.*** Science Networks. Historical Studies, vol. 28. Birkhäuser, Boston, 2003, xxiii + 447 pp.

The title of Hesseling's work has a beautiful explanation that I have no wish to spoil by attempting to summarize it here; the curious are referred to the very beginning and the very end of this most useful book. The titles of its six chapters are: 1. Kronecker, the semi-intuitionists, Poincaré; 2. The genesis of Brouwer's intuitionism; 3. Overview of the foundational debate; 4. Reactions: existence and constructivity; 5. Reactions: logic and the excluded middle; 6. The foundational debate and its context. Of the appendices, I mention A. Chronology of the debate and B. Public reactions to Brouwer's intuitionism.

The main contribution of ***Gnomes in the fog***—a somewhat revised version of Hesseling's dissertation of the same title defended at Utrecht University in 1999—is twofold.

First, it maps in detail, and even quantifies, an episode in the search for mathematical roots between 1870 and 1940 worthy of treatment at length, namely, Brouwer's introduction of his full-blown intuitionism just before 1920 and its reception in the decade that followed. Thus, from Hesseling's detailed investigations we obtain quantitative confirmation of Van Dalen's view that the 'Grundlagenstreit' did not begin until 1921 and in effect came to an end as early as 1928; we learn that during those years no fewer than 120 public reactions to intuitionism were published; that the debate was almost exclusively a German affair; that there is no clear relation between it and the turbulence in the Weimar Republic; that until and including 1928 only one woman participated in the debate; that the first high school text that mentions intuitionism was published in 1927.

Second, the book serves as a (salutary) reminder of the difference between a debate considered as a social phenomenon and, more abstractly, as a confrontation of different systems of thought.

In this difference lies the explanation of the following three facts among those brought to the fore in the central part of the book, chapters 3 through 5:

(1) The debate did not begin until three years after Brouwer had published the paper in which he introduced his full-blown intuitionism. Moreover, the paper that sparked off the debate was neither written by Brouwer nor a particularly faithful presentation of his ideas.

(2) Half of intuitionism was—not completely, but almost—left out. As a result, when the dust settled, some of the most fundamental of Brouwer's principles had hardly at all been discussed. Moreover, the half that was discussed often was discussed in a such a way that the coherence it had in Brouwer's setup was lost.

(3) A paper that appeared while the debate was in full swing and that nowadays is considered to have been among the most valuable ones written in that period, played almost no role at the time.

Ad (1). Brouwer first presented his full-blown intuitionism in print in 1918, in his paper *Foundation of set theory independently of the logical principle of the excluded middle. First part: general set theory*. It has been noticed before that, given the technical and undramatic style of its title and contents, this paper was not likely to cause a stir; and indeed, as Hesseling's quantitative findings corroborate, it did not. The paper that did spawn the debate in the social sense was Weyl's *On the new foundational crisis of mathematics*, three years later; but that one did little to achieve its explicit aim of furthering the intuitionistic cause. The technical reasons for this failure lie in the particular way in which Weyl adapts some of the intuitionistic notions; for the details, see Brouwer's *Comments on Weyl 1921* in P. Mancosu, ***From Brouwer to Hilbert***, Oxford: Oxford University Press 1988, pp. 119–122, as well as D. van Dalen, *Hermann Weyl's intuitionistic mathematics*, this BULLETIN 1(2), 1995, pp. 145–169, and M. van Atten, D. van Dalen and R. Tieszen, *Brouwer and Weyl: the phenomenology and mathematics of the intuitive continuum*, ***Philosophia Mathematica*** 10(3), 2002, pp. 203–226. Of particular historical interest in this context is the following note by Brouwer, presumably overlooked by Hesseling (and certainly by me when writing the paper just referred to). It is probably from the late 1920s:

> As creative Neo-Intuitionist besides Brouwer one should consider only his pupil Heyting, who has reformed geometric axiomatics on the basis of the new intuitionism. Weyl can to this day hardly be mentioned in this context: it is true that he has published, as the first, half-understanding follower of Brouwer, some attempts at popularisation, but the ingredients of his own contained therein are all mistaken and mislead the audience.
>
> [Brouwer Archive, Department of Philosophy, Utrecht University. I am grateful to the director of that archive, Dirk van Dalen, for granting permission to quote it here. Translation mine]

A well-known English saying perfectly summarizes Brouwer's opinion of his friend Weyl here.

Ad (2). Intuitionistic mathematics, for Brouwer, was a consequence of a certain philosophy of mind; (part of) this philosophy of mind gave Brouwer, to borrow someone else's phrase, his fists to box with. In particular, both his view on the issue of mathematical existence and on that of the principle of the excluded middle (PEM) are immediate and closely related consequences of it. It is not surprising, therefore, to find that in discussions of intuitionism in which this foundational philosophy is neglected, the one issue is dealt with separately from, and perhaps even in absence of, the other; with the expected consequences for the result, if meant as an evaluation of Brouwer's project. Most of the actual discussion nevertheless having been of this type, Hesseling devotes chapter 4 to constructivity and chapter 5 to logic. In a review of the original dissertation in a Dutch newspaper, Fred Muller criticized this approach as follows:

> I had the feeling of being shown around a town in a bus of which the guide had blinded all windows on the left. Then I made the same tour again, but this time all windows on the right were blinded. This way of going about the tour would be justified if mathematical existence and the principle of the excluded middle had nothing at all to do with each other. But this is not the case, as Hesseling knows. In this light, a double tour is too much.
> [F.A. Muller, *Muizen tegen kikkers* [*Mice against frogs*], ***NRC Handelsblad***, May 22, 1999. Translation mine]

Muller's simile is accurate; but this points to a problem with the book only if one has a strong preference for the abstract perspective—which always welcomes the wisdom of hindsight—over the social one. Hesseling's preference is different, and this explains why he decided faithfully to mirror the structure of the historical debate in the structure of his book (see middle of p. xxi).

As firmly rooted in Brouwer's analysis of the mind as his views on existence and PEM is his (non-classical) notion of choice sequence. Interestingly, in lectures from 1905 titled 'Logical principles of mathematical thought', Hilbert too started his development of mathematics from an 'axiom of the existence of mind':

> I have the ability to think things, and to designate them by simple signs ($a, b, \ldots$ $X, Y, \ldots$) in such a completely characteristic way that I can always recognize them again without doubt. My thinking operates with these designated things in certain ways, according to certain laws, and I am able to recognize these laws through self-observation, and to describe them perfectly.
> [As quoted in V. Peckhaus, *The pragmatism of Hilbert's Programme*, ***Synthese*** 137(1–2), 2003, p. 150]

One wonders what would have happened if, through self-observation, Hilbert would have come to see the possibility of choice sequences. The question has been raised before, and in all likelihood even without knowledge of the passage just quoted: 'Hilbert, I presume, didn't want to permit choice sequences? To me they seem to be quite concrete [anschaulich],' Gödel wrote to Bernays in 1969; Bernays replied that 'As far as I know, Hilbert never took a position on choice sequences at all' (quoted from S. Feferman et al., eds., ***Kurt Gödel. Collected works***, Oxford: Oxford University Press 2003, vol. IV, p. 271 and p. 279). In an unsent draft of his letter, Gödel had written that 'it now seems to me, after more careful consideration, that choice sequences are something concretely evident and therefore are finitary in Hilbert's sense, even if Hilbert himself was perhaps of another opinion' (p. 269); the possible discrepancy that Gödel mentions here may be seen as the analogue for one person of the difference between the two senses of debate mentioned above.

For a (full or partial) evaluation of the debate in the abstract sense—a task that Hesseling deliberately did not make central to his effort—it is of course essential to obtain answers to questions like this; and that in the 1920s they were not sufficiently addressed illustrates the claim (implicit on p. xix) that the discussion in the 'Grundlagenstreit' was, in general, not of a high level. Hesseling remarks that in particular the classical side was simply not well-informed. As this situation essentially did not change in the 1930s and 1940s, and moreover the debate in the social sense had long since come to an end by then, it is not surprising that when Brouwer in 1954 finally did publish (in English) a paper with a polemical title—*An example of contradictority in classical theory of functions*—, in which he relied on one of the furthest developments of the intuitionistic conception of the role of subjectivity in mathematics, this did not at all lead to a reopening of the debate.

Ad (3). In the middle of the debate, 1925, Kolmogorov's paper *On the principle of the excluded middle* appeared. In the abstract sense, it made an enormous contribution by giving the first (partial) formalisation of intuitionistic logic and by giving a translation of classical into intuitionistic logic; in the social sense, its contribution was nil. Hesseling writes, 'Since the paper was written in Russian, it remained almost completely unnoticed. The only reference I found to it in the whole foundational debate was a letter from Glivenko to Heyting in October 1928. There is not a single reference to it in any of the public contributions until 1933' (p. 241). In turn, another Russian paper from that time escaped Hesseling's attention: Khinchin's *Ideas of intuitionism and the struggle for content in contemporary mathematics* from 1926; interestingly, in this paper it is mentioned that a seminar on intuitionism was held in Moscow then. I obtain this information from p. 202 of J. von Plato, ***Creating modern probability***, Cambridge: Cambridge University Press 1994.

I should like to mention a suggestion that von Plato made to me in email correspondence: the suggestion that Kolmogorov 1925, before it became widely known, did exert indirect influence, via Glivenko's *On some points of the logic of Mr. Brouwer* from 1929. The fact that Glivenko does not explicitly refer to Kolmogorov need not be decisive here. Indeed, upon re-reading Glivenko's letters to Heyting (published in A.S. Troelstra, *On the early history of intuitionistic logic*, in P.P. Petkov, ed., ***Mathematical logic***, New York: Plenum Press 1990, pp. 3–17.), I think it is clear that Glivenko had come to know Kolmogorov's paper somewhere between July 4 and October 13, 1928, and arrived at the result that was published in 1929 only after having received, on October 17, 1928, a letter from Heyting that convinced him of the acceptability in formalized intuitionistic logic of the Ex Falso principle (this is evidence against Mancosu's claim in ***From Brouwer to Hilbert***, p. 281, lines 3–4). Partly because of this principle, not accepted by Kolmogorov, Glivenko obtained a stronger result than Kolmogorov had; and although Glivenko in the correspondence does not explicitly say so, it is not unlikely that in working towards his result he had been motivated by his reading of Kolmogorov. On the other hand, the scope of Kolmogorov 1925 was much wider than that of Glivenko 1929 and included interesting philosophical points as well. (I express my gratitude to Jan von Plato and Paolo Mancosu for discussion of the issues addressed in this paragraph.)

The final chapter of Hesseling's book addresses some questions that are natural to ask but perhaps difficult to answer, questions that inquire into connections between the foundational debate and its cultural (evidently understood by Hesseling in a sense that implies 'non-mathematical') context. In his discussion of possible relations between the debate in mathematics on the one hand and contemporary developments in art and physics on the other, Hesseling is commendably cautious, and, given the relative autonomy of their subject matters, it is probably not surprising that he could not find any specific influences from the latter on the former. The same seems to go for politics at the level of individual political preferences. On a larger scale of politics, many of the participants in the debate were affected, in well-known ways, by Hitler's coming to power in 1933; but as Hesseling shows elsewhere in the book, the debate was already over by then. In philosophy, a popular current in the 1920s was the 'Intuitionismus' of 'Lebensphilosophie'. As was to be expected, a comparison of this (particular kind of) philosophical intuitionism with mathematical intuitionism leads to a number of interesting remarks on their similarities and differences; as far as the question of actual influence on the foundational debate is concerned, Hesseling makes it plausible that this did indeed exist, mainly in the form of making mathematical intuitionism guilty by association. He also shows that the cultural factor that had the greatest effect on the debate was the use of metaphors inspired by (mainly) political events ('crisis', 'revolution', 'Putsch', 'bolshevism', 'paper money'). But the effect of this cultural factor must ultimately have depended on personal factors, I should think; for it is the individual with its particular sensibilities that chooses to use (or not to use) a particular metaphor in a particular context. Throughout the book, Hesseling shows a keen eye for personal factors. He devotes a whole section (2.9) to a discussion of Brouwer's personality (for those familiar with the Myers-Briggs Type Indicator: 'INTP bordering on INFP', Hesseling concludes) and its effect on others, notably Hilbert and Weyl; to me it seems that the addition of similar sections analysing Hilbert, Weyl, and Fraenkel (who, as Hesseling shows, was the most important contemporary commentator on the foundational debate) would have done more to deepen this account of the debate than the discussion of possible cultural influences in chapter 6 (even though, as mentioned above, such questions are natural and need to be addressed).

In closing, two practical notes.

First, although both the original dissertation and this commercial edition have been typeset in LaTeX, in the transformation of the former into the latter the publisher sometimes seems to have had difficulties with mathematical symbols (see for example the glossary entry 'cardinal number' on p. 373) and with Greek, all of which has been turned into a kind of dashed lines; according to the dissertation, the latter should read:

p. 218, footnote 4: 'Περὶ Ἑρμενείας'

p. 365, line 10: 'Οικονομου Αἱ αντινομιαι εις τα νεωτερα μαθηματικα'

p. 416, line 12: as on p. 365.

Second, an update of p. 378 for those stimulated by this book to do further research. Recently (but, given the date on p. xiv, still before the manuscript of this book had been handed to the press) the Brouwer Archive moved to the Department of Philosophy at Utrecht University, and that of Heyting to the Rijksarchief Noord-Holland in Haarlem (where, in the long run, the Brouwer Archive will be deposited too).

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JOSEPH Y. HALPERN. ***Reasoning about Uncertainty.*** MIT Press Cambridge, MA, 2003, xiv + 483 pp.

Reasoning about Uncertainty is an introduction to the mathematics of uncertainty written for researchers in various disciplines, and forms a natural sequel to Fagin *et al.* ***Reasoning about Knowledge***, MIT Press, 1995. The preposition in the title—*about*, rather than *under* or *with*—is significant: Halpern's volume is primarily conceived as a study of formalisms for representing uncertainty and their mathematical interrelationships, rather than an endorsement of any particular philosophical stance on uncertain reasoning. The choice of topics is broad, and the exposition matter-of-fact and unopinionated. Few students of uncertain reasoning will fail to find technical material of direct relevance to their own preferred approach here.

Following a brief introduction in Chapter 1, Chapter 2 surveys the gamut of approaches to representing uncertainty. The survey begins with an orthodox subjectivist presentation of probability theory; but most of the chapter is taken up with alternative approaches, including upper and lower limits of sets of probability distributions, outer and inner measures, Dempster-Shafer belief functions, preference structures, and plausibility structures. The last two of these will be unfamiliar to many readers, but they play a central role in the book. Very roughly, a preference structure is a sample space equipped with a pre-order $\succeq$, where $w \succeq w'$ is to be read as: "outcome w is more 'normal' than outcome w'." (It is standard in the context of this book and the sources it cites to refer to outcomes as *possible worlds.*) This pre-order may then be lifted to a relation on events, resulting in what Halpern calls a *plausibility measure*. The reader has to wait until later chapters to appreciate Halpern's motivation for studying these representations of uncertainty.

Chapter 3 discusses, in great—indeed somewhat laborious—detail, strategies for revising uncertain beliefs in response to evidence, using each of the approaches set out in Chapter 2. For the majority of readers, the most useful material here is probably the discussion of probabilistic updating, which covers not only standard conditioning, but also Jeffrey conditioning and minimum cross-entropy updating. One oddity of the discussion here is the type of Dutch book arguments employed in defence of conditioning. The text explains in detail how the standard policy of pricing conditional bets according to conditional probabilities is forced by the need to avoid Dutch books. Yet, as the author is aware, static pricing policies for conditional bets are not in themselves revision policies for the prices of unconditional bets in response to accumulating evidence. The rather slippery *diachronic* Dutch book arguments required to connect the two are relegated to a few brief references in the notes.

The appropriateness of conditioning as a revision strategy is nevertheless pursued further in Chapter 6, in the context of probability structures for multi-agent systems. (The technical material introduced here also forms the basis for the epistemic and probability logics discussed in later chapters.) Familiar puzzles, such as the Monty Hall paradox and the Three Prisoners problem, which require conditioning over a 'sophisticated' outcome space, are presented here; the main result characterizes those situations in which a 'naive' space suffices. By this stage of the book, the formalisms have begun to get rather unwieldy for the amount of insight they generate. Having spent some time unpeeling the various definitions and working through the results, I must say I did not feel much more enlightened about conditioning than when I started: the main result essentially just recapitulates observations that are elsewhere presented with much less notational fuss. In particular, this result fails to answer the most obvious question concerning 'naive' versus 'sophisticated' probability spaces: how do I know when my sophisticated space is sophisticated *enough*?

One very rewarding aspect of this book is its extended treatment of conditionals, understood as statements of defeasible inferential commitment. On this view, to say "If Tweety is a bird, then Tweety flies" is to express the commitment to infer that Tweety flies if one learns that Tweety is a bird. The commitment is defeasible because it is possible simultaneously to be committed to inferring that Tweety does not fly if one learns that Tweety is in fact a penguin (and hence also a bird). Chapter 8 discusses the logic of such conditionals, taking, as its point of departure, a familiar collection of axioms. In providing semantics to match these axioms, Halpern returns to various of the representations of uncertainty introduced in Chapter 2, and in particular to preference structures and plausibility spaces. The use of preference structures was, of course, pioneered by David Lewis in his analysis of counterfactual conditionals: suppose we are given a preference structure, equipped with a function assigning to each possible world a propositional logic valuation; then we can say that the (defeasible) conditional $\phi \rightarrow \psi$ is true at a world w just in case ψ is true in all of the maximally normal worlds accessible from w in which ϕ is true. Halpern generalizes this semantic framework to (a certain class of) plausibility structures: on this generalization, $\phi \rightarrow \psi$ is true at a world w just in case (roughly) the plausibility of $\phi \wedge \psi$ at w exceeds the plausibility of $\phi \wedge \neg\psi$ at w. The chapter discusses the correspondence between various semantic approaches to conditionals and the axioms of conditional logic which characterize them. The discussion ties in neatly with the account of probability logics explained in Chapter 7, and is extended in Chapter 9 to discussions of belief revision in the tradition of the so-called AGM-postulates.

Mathematically interesting as these connections are, their broader philosophical significance is less obvious. It is good to know that there is a probabilistic semantics for defeasible conditionals which is axiomatized by familiar principles of conditional logic. But closer inspection indicates that the conditionals typically do not mean what we thought they meant—in particular, $\phi \rightarrow \psi$ typically translates to something like $p(\psi \mid \phi) \approx 1$. Likewise, it is good to know that there is a semantics for defeasible conditionals in terms of preference structures and plausibility structures, which avoids any such unit conditional probabilities. But since no one really understands what it really means to say that one world is more 'normal' than another, no one in truth can give any compelling reason to obey the inferential prescriptions which ensue. In short, despite the impressive unifying analysis of the semantics of conditional logic, there is little here that underwrites any useful principles of reasoning.

The final chapter of the book concerns the problem of direct inference. Suppose you are sure that 99% of birds fly, and that Tweety is a bird. If you know nothing else, what credence should you give to the proposition that Tweety flies? Seemingly, 0.99; but why? What principles govern the relationship between sure knowledge about populations and unsure beliefs about individuals? The Carnapian approach favoured by Halpern, the so-called random-worlds approach, can be described in simple terms as follows. Suppose there are n birds in total, and we know that a proportion r/n of them fly. Consider all possible worlds consistent with these facts—that is, all $n!/(r!(n-r)!)$ ways of choosing the r flying birds from the n birds. Focusing now on a particular bird, say Tweety, it is easy to see that the number of worlds in which Tweety flies is $(n-1)!/((r-1)!(n-r)!)$, and hence that the proportion of worlds in which Tweety flies is r/n—exactly the same as the proportion of birds which fly in every world. Taking all worlds to be equally probable then justifies the assignment of probability r/n to the proposition that Tweety flies. Of course, the system that Halpern describes is far more sophisticated that this simple sketch. First, it is couched in the full language of first-order logic (augmented with statistical quantifiers); second, it considers not a fixed domain size, but rather limiting probabilities as the domain size tends to infinity; third, it goes to some lengths to deal with the need to interpret reported statistical ratios as approximations. Halpern provides a clear account of this approach and of the inferences it licences. Usefully, connections are made with Dempster's rule of combination, with the account of conditionals and belief-revision discussed above, and with maximum entropy belief-revision. Again, many readers will rightly be skeptical about the philosophical significance of the mathematical relationships that Halpern charts. After all, it is hard to see how such fundamentally simple combinatorial observations as that made above can possibly underwrite any substantive philosophical position on statistical inference. But, here as elsewhere, such philosophical concerns are not to the fore; and certainly, no student of direct inference can afford to ignore the technical material discussed in this chapter.

If I have one complaint after working through this book, it is that its often very general approach does not always yield sufficient enlightenment to repay the effort of coping with the elaborate notation and innumerable, lengthy definitions. The generalization of preference structures to plausibility measures, and the hugely involved discussion of belief-revision postulates in the context of 'belief-change systems' are good examples of this sometimes dubious tradeoff. The problem of checking multiple definitions spread over various chapters is sometimes compounded by the presence of typographical errors in formulas. For instance, it would take quite a confident graduate student to work out what is really going on in Example 9.3.6. (I note in passing one—uncharacteristic—substantive technical error: on p. 283, Halpern incorrectly claims that a certain axiomatic system is complete for the propositional calculus, citing Popkorn ***First steps in modal logic***, Cambridge, 1994, as an authority. I pointed this out to Sally Popkorn in person, who said that she(?) had been 'misquoted'.) On the other hand, since the aim of the book is to chart the relationships between a number of very different and complicated mathematical theories, many of these difficulties are probably unavoidable; and we should not lose sight of the very great service which the author has rendered to the field. Indeed, there can be very few researchers interested in uncertain reading who would not find much of value in this work.

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GABRIEL DEBS AND JEAN SAINT RAYMOND. *Compact covering mappings and cofinal families of compact subsets of a Borel set*. ***Fundamenta Mathematicae***, vol. 167, no. 3 (2001), pp. 213–249.

GABRIEL DEBS AND JEAN SAINT RAYMOND. *Compact covering mappings between Borel spaces*. ***Acta Universitatis Carolinae. Mathematica et Physica***, vol. 40, no. 2 (1999), pp. 53–64.

GABRIEL DEBS AND JEAN SAINT RAYMOND. *Cofinal Σ^1_1 and Π^1_1 subsets of ω^ω*. ***Fundamenta Mathematicae***, vol. 159, no. 2 (1999), pp. 161–193.

GABRIEL DEBS AND JEAN SAINT RAYMOND. *Compact-covering-properties of finite-to-one mappings*. ***Topology and its Applications***, vol. 81, no. 1 (1997), pp. 55–84.

GABRIEL DEBS AND JEAN SAINT RAYMOND. *Some applications of game determinacy*. ***Acta Universitatis Carolinae. Mathematica et Physica***, vol. 37, no. 2 (1996), pp. 7–23.

GABRIEL DEBS AND JEAN SAINT RAYMOND. *Compact covering and game determinacy*. ***Topology and its Applications***, vol. 68, no. 2 (1996), pp. 153–185.

GABRIEL DEBS AND JEAN SAINT RAYMOND. *Compact covering mappings between Borel sets and the size of constructible reals*. ***Transactions of the American Mathematical Society***, vol. 356, no. 1 (2004), pp. 73–117.

All spaces in this review are separable and metric. Consider three properties of continuous surjections $f : X \to Y$.

(IP) f is *inductively perfect* if there is $X' \subseteq X$ such that $f[X'] = Y$ and $f^{-1}(L) \cap X'$ is compact for every compact $L \subseteq Y$.

(CC) f is *compact covering* if for every compact $L \subseteq Y$ there is a compact $K \subseteq X$ such that $f[K] = L$.

(CCC) f is *countable compact covering* if for every countable compact $L \subseteq Y$ there is a compact $K \subseteq X$ such that $f[K] = L$.

Every inductively perfect map is compact covering, and every compact covering map is countable compact covering. In E. Michael, *Some problems*, in ***Open problems in topology***, North-Holland, 1990 pp. 271–278 Michael raised the question whether the converses hold for maps between separable metrizable spaces, after he proved related results for a related class of the so-called 'tri-quotient maps.' Let us write

$$\mathbb{A}(\boldsymbol{F}, \boldsymbol{G})$$

if for every $X \in \boldsymbol{F}$ and $Y \in \boldsymbol{G}$, every compact covering map $f : X \to Y$ is inductively perfect. It was previously known that $\mathbb{A}(\text{Polish}, Y)$ holds for all Y (J. P. R. Christensen, *Necessary and sufficient conditons for the measurability of certain sets of closed subsets*, ***Mathematische Annalen***, vol. 200 (1973), pp. 189–193, Jean Saint-Raymond, *Caractérisation d'espaces polonais. D'après des travaux récents de J. P. R. Christensen et D. Preiss*, in ***Séminaire choquet, 11e–12e années (1971–1973), initiation à l'analyse, exp. no. 5***, Secrétariat Mathématique, 1973 p. 10) and that $\mathbb{A}(X, K_\sigma)$ holds for all X (A. V. Ostrovskiĭ, *On new classes of mappings associated with k-covering mappings*, ***Vestnik Moskovskogo Universiteta. Seriya I. Matematika, Mekhanika*** (1994), 4, pp. 24–28, 75, W. Just and H. Wicke, *Some conditions under which tri-quotient or compact-covering maps are inductively perfect*, ***Topology and its Applications***, vol. 55 (1994), 3, pp. 289–305 for countable instead of K_σ), while a counterexample to $\mathbb{A}(X, Y)$ can be constructed using the Axiom of Choice. These partial results suggest that Michael's questions may have (originally apparently unintended) descriptive set-theoretic flavor. Papers under the review pursue this line of research. To quote the authors, 'The spirit of the proofs and constructions we make mixes intuition from topology and descriptive set theory.' The main thread follows the question when $\mathbb{A}(\boldsymbol{F}, \boldsymbol{G})$ holds for $\boldsymbol{F}$ and $\boldsymbol{G}$ among the standard pointclasses such as Π^0_α and Σ^0_β.

In G. Debs and J. Saint Raymond, *Compact covering and game determinacy*, ***Topology and its Applications***, vol. 68 (1996), 2, pp. 153–185 the authors prove that in this context it suffices to consider maps that are projections between zero-dimensional spaces. Namely, for $f: X \to Y$ they define $Z \subseteq 2^\omega \times 2^\omega$ that is a perfect preimage of X such that its projection $\pi[Z]$ is a perfect preimage of Y and π is compact covering (inductively perfect) if and only if f is compact covering (inductively perfect).

The main positive result of the paper, Corollary 6.5, is that the Analytic Determinacy $(\mathrm{Det}(\Sigma^1_1))$ implies $\mathbb{A}(\Pi^1_1, \Pi^1_1)$. The other side of the coin is also investigated: $\mathbb{A}(\Pi^1_1, \Pi^1_1)$ implies that ω_1 is inaccessible in L. Four more ZFC-counterexamples to $\mathbb{A}(X, Y)$ for separable metric X and Y are given. The obtained spaces are not projective, but the connecting maps have simple fibres. These constructions answer a question of Michael.

Inductively perfect images of Π^1_1, Π^0_ξ and Σ^0_ξ spaces are Π^1_1, Π^0_ξ and Σ^0_ξ, respectively (for $\xi \geq 2$). What can be said about compact covering images of Borel sets? In 1984 Ostrovsky conjectured that they are always Borel. The authors prove that compact covering projections of Π^0_2 sets are Π^0_2, and if a compact covering projection of Π^0_ξ (Σ^0_ξ, respectively) set is Borel, then it is Π^0_ξ (Σ^0_ξ, respectively) for $\xi \geq 2$. It is also proved that Ostrovsky's conjecture follows from the assumption that $(\aleph_1)^{L(\alpha)}$ is countable for all reals α. These are applications of the result that if X and Y are Borel, then every inductively perfect map $f: X \to Y$ has a Baire class-1 selection, again proved using games. A map $g: Y \to X$ is a *selection* for f if $f(g(y)) = y$ for all $y \in Y$.

All applications of games in G. Debs and J. Saint Raymond, *Compact covering and game determinacy*, ***Topology and its Applications***, vol. 68 (1996), 2, pp. 153–185 follow a common scheme. To prove $A \Rightarrow B$, devise a two-player game G such that if player I wins G then A fails, and if player II wins then B holds. Then $A \Rightarrow B$ follows from the determinacy of G. Hence if it is possible to construct closed (or Borel) G, the implication follows.

Exploiting this idea is the main theme of the survey paper G. Debs and J. Saint Raymond, *Some applications of game determinacy*, ***Acta Universitatis Carolinae. Mathematica et Physica***, vol. 37 (1996), 2, pp. 7–23. Illustrations of uses of games include results of classical descriptive set theory, such as perfect set property of Borel and analytic sets and Hurewicz separation theorem. It is also proved that every Σ^1_1 subset of the plane all of whose sections are countable can be covered by a countable family of graphs of Borel functions, and furthermore that if all sections are only assumed to be Σ^0_2 then the set can be covered by countably many Borel sets all of whose sections are closed. This is an application of a result of Martin, that a winning strategy for a Borel game can be chosen in a Borel way; unfortunately this result is given without a proof or a reference. The paper ends with an outline of the above mentioned application of $\mathrm{Det}(\Sigma^1_1)$ from G. Debs and J. Saint Raymond, *Compact covering and game determinacy*, ***Topology and its Applications***, vol. 68 (1996), 2, pp. 153–185. The authors also give the proof of the Gale–Stewart theorem on determinacy of closed games, thus producing a self-contained survey paper.

Unlike other papers reviewed here, G. Debs and J. Saint Raymond, *Compact-covering-properties of finite-to-one mappings*, ***Topology and its Applications***, vol. 81 (1997), 1, pp. 55–84 makes no assumptions on the descriptive complexity of spaces X and Y. It was shown (Theorem 7.9 of G. Debs and J. Saint Raymond, *Compact covering and game determinacy*, ***Topology and its Applications***, vol. 68 (1996), 2, pp. 153–185) that countable compact covering does not imply compact covering (thus answering Michael's question) even for $f: X \to Y$ when X is K_σ and each fiber has at most one accumulation point. [A negation of this statement was previously published in the literature.] On the other hand, if all fibers of a countably compact covering map have size at most two, then the map is compact covering. This is a nontrivial result of Ostrovsky. The gap between these two results is filled in G. Debs and J. Saint Raymond, *Compact-covering-properties of finite-to-one mappings*, ***Topology and its Applications***, vol. 81 (1997), 1, pp. 55–84. It is proved that there is a countably compact covering map between separable metric spaces all of whose fibers are finite that is not compact covering. However, by the main result of this paper, if f is countably compact covering and the size of fibers is bounded by some $n \in \mathbb{N}$, then f is compact covering.

The proof makes use of $\mathcal{K}(X)$, the hyperspace of all compact subsets of X, considered with the Hausdorff metric. The following characterization plays a key role in the proof: a map is countably compact covering if and only if it has a 'quasi-upper semicontinuous' lifting $\phi\colon Y \to \mathcal{P}(\mathcal{K}(X))$. A large part of G. Debs and J. Saint Raymond, *Compact-covering-properties of finite-to-one mappings*, ***Topology and its Applications***, vol. 81 (1997), 1, pp. 55–84 deals with properties of quasi-usc maps.

The gap between 'det(Σ^1_1) holds' and 'ω_1 is inaccessible in L' is considerable, and in G. Debs and J. Saint Raymond, *Cofinal Σ^1_1 and Π^1_1 subsets of ω^ω*, ***Fundamenta Mathematicae***, vol. 159 (1999), 2, pp. 161–193 the authors revisit the question when $\mathbb{A}(X, Y)$ holds to search for more optimal assumptions. The class $D_2(\Delta^0_2)$ of all differences between two Δ^0_2 sets plays an important role, as well as the ordering $\leq^*$ on ω^ω, defined by $f \leq^* g$ if and only if $(\forall^\infty n) f(n) \leq g(n)$. The following statements are shown to be equivalent to each other (and this is a short list):

1. $\mathbb{A}(\Pi^1_1, D_2(\Delta^0_2))$,
2. $\mathbb{A}(\Delta^1_1$,Polish),
3. $\mathbb{A}(D_2(\Delta^0_2)$,Polish),
4. every $\leq^*$-cofinal Π^1_1 subset of ω^ω has a $\leq^*$-cofinal Σ^1_1 subset,
5. every $\subseteq$-cofinal Π^1_1 subset of $\mathcal{K}(\omega^\omega)$ has a $\subseteq$-cofinal Σ^1_1 subset,
6. for every $\alpha \in \omega^\omega$, $\omega^\omega \cap L(\alpha)$ is $\leq^*$-bounded.

It is remarkable that no direct proofs of some of the implications are known. For instance, the only known proof that (4) implies (5) proceeds via (6).

This paper contains a number of results on the structure of cofinal subsets of $(\omega^\omega, \leq^*)$. For example, (6) implies that every cofinal Σ^1_2 set includes a cofinal Σ^1_1 subset. The same assumption implies that every cofinal Π^1_1 set is *continuously cofinal*: it includes the image of a continuous $\phi\colon \omega^\omega \to \omega^\omega$ such that $f \leq^* \phi(f)$ for all f. There is a slight overlap with Brendle, Hjorth and Spinas (J. Brendle, G. Hjorth, and O. Spinas, *Regularity properties for dominating projective sets*, ***Annals of Pure and Applied Logic***, vol. 72 (1995), 3, pp. 291–307), but the results are formally incomparable.

Paper G. Debs and J. Saint Raymond, *Compact covering mappings between Borel spaces*, ***Acta Universitatis Carolinae. Mathematica et Physica***, vol. 40 (1999), 2, pp. 53–64 surveys the earlier results, announces some new results, and presents all of them in a unified way. It is noted that (a) Det(Σ^1_1) implies $\mathbb{A}(\Pi^1_1, \Pi^1_1)$, (b) $(\forall\alpha)\omega^\omega \cap L(\alpha)$ is $\leq^*$-bounded implies $\mathbb{A}(\Pi^1_1, \Pi^0_2)$, and (c) $(\forall\alpha)\aleph_1^{L(\alpha)}$ is countable implies $\mathbb{A}(\Pi^1_1, \Pi^0_3)$. Each of three implications is proved via constructing a determined game G. Recall that it suffices to consider maps that are projections. If $\pi\colon X \to Y$ is a projection, then G is designed so that I constructs $y \in 2^\omega$ and II constructs $H_x \in \mathcal{K}(X)$. Player II wins if $y \notin Y$ or both $H_z \subseteq X$ and $y \in \pi[H_x]$. A winning strategy for II gives a continuous $\phi\colon Y \to \mathcal{K}(X)$ such that $X' = \phi[Y]$ witnesses that π is inductively perfect. If I wins, then the map cannot be compact covering.

In this survey paper proofs of the results are sketched, thus giving a rough idea of the flavor of proofs. This paper (as well as G. Debs and J. Saint Raymond, *Some applications of game determinacy*, ***Acta Universitatis Carolinae. Mathematica et Physica***, vol. 37 (1996), 2, pp. 7–23) contains a fair number of typographical errors, but they are rather obvious and therefore do not tend to disturb the reader. (For example: It is not clear what is the relation between Y and $\mathcal{Y}$ in the statement of Theorem 5.2, and symbols for pointclasses in the statement of Hurewicz's theorem in G. Debs and J. Saint Raymond, *Some applications of game determinacy*, ***Acta Universitatis Carolinae. Mathematica et Physica***, vol. 37 (1996), 2, pp. 7–23, page 16, are mixed up.)

In G. Debs and J. Saint Raymond, *Compact covering mappings and cofinal families of compact subsets of a Borel set*, ***Fundamenta Mathematicae***, vol. 167 (2001), 3, pp. 213–249 the problem of finding sufficient and necessary set-theoretic assumption for $\mathbb{A}$(Borel,Borel) is related to

Is every cofinal Π^1_1 subset of $\mathcal{K}(X)$ (for a Borel X) continuously cofinal?

This paper contains proofs of the optimality of the set-theoretic assumptions used previously, as well as another step in the climb up the Borel hierarchy. The following are equivalent:

1. $(\forall\alpha \in \omega^\omega)\omega^\omega \cap L(\alpha)$ is $\leq^*$-bounded.
2. $\mathbb{A}(\Pi^1_1, \Pi^0_2)$.
3. every cofinal Π^1_1 subset of ω^ω is continuously cofinal.
4. every cofinal Π^1_1 subset of $\mathcal{K}(\omega^\omega)$ admits a Borel dominating function.

The following are equivalent (recall that $\mathbb{A}(\Pi^0_2, Y)$ for every Y is a theorem of Christensen and Saint Raymond):

5. $(\forall\alpha \in \omega^\omega)\aleph_1^{L(\alpha)}$ is countable.
6. $\mathbb{A}(\Pi^1_1, \Pi^0_3)$.
7. $\mathbb{A}(\Pi^0_3, \Pi^0_3)$.

The following are equivalent:

8. $\mathrm{Det}(\Sigma^1_1)$.
9. $\mathbb{A}(\Pi^1_1, \Pi^1_1)$.
10. every cofinal Π^1_1 subset of $\mathcal{K}(\omega^\omega)$ is continuously cofinal.
11. every cofinal Π^1_1 subset of $\mathcal{K}(\mathbb{Q})$ admits a Borel dominating function.

The first and third sets of equivalences are partially extracted from previous work, while the second one is completely new. The main technical innovation required in the implication from (5) to (6) is in devising a Borel game G associated to a projection $\pi\colon X \to Y$, where Y is Σ^0_2 and $X = R \cap (Y \times Z)$ for a Borel R and a Σ^1_2 set Z. Write Y as an increasing union $\bigcup_n F_n$ of closed sets, and Z as a projection of a tree T on $2 \times \omega_1$. Player I constructs a real $y \in Y$, while II constructs a branch (x, θ) through T. Player II is allowed to 'reset' θ each time I's move misses some F_n. Hence if I plays a real in Y, II is forced to play a real in Z. II wins if $y \notin Y$ or $(y, z) \in X$, and the determinacy of this game implies that π is inductively perfect or it is not compact covering.

Using the assumption that $\aleph_1^{L(\alpha)}$ is countable, such Borel game G is devised when Y is only assumed to be Π^0_3. In G. Debs and J. Saint Raymond, *Compact covering mappings between Borel sets and the size of constructible reals*, ***Transactions of the American Mathematical Society***, vol. 356 (2004), 1, pp. 73–117 this construction is propagated all the way through the Borel hierarchy. In is shown that $\mathbb{A}(\Delta^1_1, \Delta^1_1)$ is equivalent to '$\aleph_1^{L(\alpha)}$ is countable for all $\alpha \in \omega^\omega$.' An effective variant of this result is also given. In the earlier papers, the effectivization of the result was obtained by analogous proof and straightforward switching of quantifiers. The situation here is more complex. In G. Debs and J. Saint Raymond, *Applications semi-propres sur un espace borélien*, ***Comptes Rendus de l'Académie des Sciences. Série I. Mathématique***, vol. 332 (2001), 5, pp. 423–426 the authors showed that for $\alpha \in \omega^\omega$, if $\aleph_2^{L(\alpha)} < \aleph_1$, then $\mathbb{A}(\Pi^1_1, \Sigma^0_4)$ holds for maps that are $\Delta^1_1(\alpha)$. In a similar spirit, in G. Debs and J. Saint Raymond, *Compact covering mappings between Borel sets and the size of constructible reals*, ***Transactions of the American Mathematical Society***, vol. 356 (2004), 1, pp. 73–117 it is shown that for every $\alpha \in \omega^\omega$ and $\xi < \omega_1^{\mathrm{CK}}$, if $(\aleph_{\omega^\xi+1})^{L(\alpha)} < \aleph_1$ then $\mathbb{A}(\Pi^1_1(\alpha), \Sigma^0_\xi(\alpha))$ holds for $\Delta^1_1(\alpha)$ maps f. This assumption suffices to associate a Borel game G like above to every compact covering $\Delta^1_1(\alpha)$ projection. As the authors point out, by G. Debs and J. Saint Raymond, *Applications semi-propres sur un espace borélien*, ***Comptes Rendus de l'Académie des Sciences. Série I. Mathématique***, vol. 332 (2001), 5, pp. 423–426 the assumption is not optimal. But in G. Debs and J. Saint Raymond, *Arbres distingués, bi-arbres et théorèmes de* *relèvement*, ***Comptes Rendus Mathématique. Académie des Sciences. Paris***, vol. 336 (2003), 8, pp. 625–628 the authors announced that $\aleph_{\xi+1}$ is countable in L if and only if $\mathbb{A}(\Delta^1_1, \Sigma^0_{1+\xi+2})$, if and only if $\mathbb{A}(\Delta^1_1, \Pi^0_{1+\xi+2})$. In the same paper, they announce the confirmation of Ostrovsky's conjecture that compact covering images of Borel sets are Borel.

The proof of the main result of G. Debs and J. Saint Raymond, *Applications semi-propres sur un espace borélien*, ***Comptes Rendus de l'Académie des Sciences. Série I. Mathématique***, vol. 332 (2001), 5, pp. 423–426 occupies almost all of the paper and involves a remarkable technical innovation. A *double tree* is a pair $(R^+, R^\bullet)$ of tree relations on $2^{<\omega}$ such that any two R^+-comparable nonzero elements have the same $R^\bullet$ predecessor. In Theorem 2.8 it is proved that for every Borel $Y \subseteq 2^\omega$ there is an 'interpolable' double tree $(R^+, R^\bullet)$ such that $y \in Y$ if and only if $\{s \in 2^{<\omega} | s \prec y\}$ contains an infinite R^+-chain and $y \notin Y$ if and only if $\{s \in 2^{<\omega} | s \prec y\}$ contains an infinite $R^\bullet$-chain. It seems plausible to expect other applications of this new representation of Borel sets.

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