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BROUWER AND FRAENKEL ON INTUITIONISM

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In the present paper the story is told of the brief and far from tranquil encounter of L.E.J. Brouwer and A. Fraenkel. The relationship which started in perfect harmony, ended in irritation and reproaches.[1]

The mutual appreciation at the outset is beyond question. All the more deplorable is the sudden outbreak of an emotional disagreement in 1927. Looking at the Brouwer–Fraenkel episode, one should keep in mind that at that time the so-called *Grundlagenstreit*[2] was in full swing. An emotional man like Brouwer, who easily suffered under stress, was already on edge when Fraenkel's book *Zehn Vorlesungen über die Grundlegung der Mengenlehre*, [Fraenkel 1927] was about to appear.

With the *Grundlagenstreit* reaching (in print!) a level of personal abuse unusual in the quiet circles of pure mathematics, Brouwer was rather sensitive, where the expositions of his ideas were concerned. So when he thought that he detected instances of misconception and misrepresentation in the case of his intuitionism, he felt slighted. We will mainly look at Brouwer's reactions. since the Fraenkel letters have not been preserved.

The late Mrs. Fraenkel kindly put the Brouwer letters that were in her possession at my disposal. I am grateful to the Fraenkel family for the permission to use the material.

I am indebted to Andreas Blass for his valuable suggestions and corrections.

Abraham Fraenkel (then still called Adolf) was one of the first nonpartisan mathematicians, if not `the` first, who developed a genuine interest in intuitionism. He was already well-known as the author of a successful

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[1]The events have to be seen in the perspective of Brouwer's personality and his foundational program, in conjunction with the general situation in the foundations of mathematics in the twenties. An extensive treatment of the required background material can be found in the biography, *Mystic, Geometer, and Intuitionist: L.E.J. Brouwer. Volume 1 The Dawning Revolution*, [van Dalen 1999].

The German text of most of the letters and manuscripts can be obtained from the author.

[2]See [van Dalen 1999], §8.8, and also the forthcoming second volume, chapter 3.

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book on set theory, written while serving in the German army during the first world war.[3]

In the twenties he did his finest work in set theory, which established his reputation for good; he showed that the axiom of choice is independent of the axioms of set theory, using a method later generalized by Mostowski, and now known as the method of the Fraenkel-Mostowski permutation models; furthermore he improved Zermelo's axiom system by providing a precise formulation for the so-called "definite properties" which occur in Zermelo's separation axiom, and by adding the "replacement axiom". The resulting system goes by the name of 'Zermelo-Fraenkel set theory'. The fact that we nowadays use Skolem's independently proposed formulation, does in no way detract from Fraenkel's achievement.

At the time that Fraenkel enters the history of intuitionism he had already made a reputation in mathematical circles. His text on set theory was a success; it discussed the foundational issues and the underlying ideas, instead of just presenting a technical exposition. The competing texts in set theory at that time were Schoenflies' *Entwickelung der Mengenlehre und ihrer Anwendungen* (second edition, 1913) and Hausdorff's *Mengenlehre* (1914). Both of these were much more technical in nature, and more topologically inclined.

Through his marriage with a Dutch wife, Fraenkel became sufficiently familiar with the Dutch language to read Brouwer's early foundational work, so—given his interest in the philosophy and foundations of mathematics—he was eminently suited to look into the somewhat exotic issues of intuitionism.

In 1920 or 1921 contact was established between Brouwer and Fraenkel, and in March 1921, in the period between the German winter and summer semester. Fraenkel, during a stay with his in-laws in Amsterdam, made an appointment to see Brouwer at home in the town of Laren.[4] The two got along so well that more meetings were planned. And when the second edition of Fraenkel's *Einleitung in die Mengenlehre*, [Fraenkel 1923], was about to be printed, Brouwer took part in the proof-reading. Fraenkel thanked Brouwer for his assistance in a letter of April 18, 1923. In the same letter he gratefully acknowleged the permission to use the mathematics library at Amsterdam. This sounds curious, but at the time there was no mathematical institute, and the mathematics library was only open to staff members; outsiders had to ask Brouwer's permission. Furthermore he had been attending Brouwer's lectures – probably supplemented by talks during his visits to Brouwer's house. We can infer from the letter that in mathematical circles (in Germany), intuitionism was not considered a really viable enterprise: "It was, among other things, most interesting for me to

[3] *Einleitung in die Mengenlehre*, [Fraenkel 1919].

[4] Brouwer lived not far from Amsterdam in an area called *het Gooi*, where he moved back and forth between the twin towns of Laren and Blaricum.

observe the active life of intuitionism which had already been pronounced dead in many quarters: these questions are still fermenting inside me."

The *Einleitung* shows in fact that Fraenkel had made himself as far as possible familiar with the intuitionistic literature. Since the first salvos in the so-called *Grundlagenstreit* had already been fired, in particular with Hilbert's attacks on Weyl and Brouwer, it took some courage to devote a complete section to a generally sympathetic review of intuitionism. In *Die Intuitionisten, namentlich Brouwer* ([Fraenkel 1923] p. 164–176) Fraenkel gave an up to date exposition of the philosophical and foundational background of intuitionism.

The reader who knows Weyl's spectacular paper *Die neue Grundlagenkrise in der Mathematik*,[5] will no doubt recognize the influence of Weyl's rhetoric. The opening sentences of the section *Die Intuitionisten, namentlich Brouwer*, reflect Weyl's colourful expressivity:

> After this excursion to the domain of philosophy, we must go on to another group of dangerous revolutionaries, which is exclusively made up of mathematicians. This group is more dangerous, in the first place because the attack is carried out with much sharper, partly mathematically finely whetted arms, but also insofar as here it concerns not just a small border correction, aimed at the exposed province of set theory, at the cost of the mathematical empire, but the attack is carried into domains of this empire that are most flourishing and imagined as most safe. If it definitively succeeds, then there will remain of present-day mathematics, apart from tightly bordered impregnable areas (in particular arithmetic in the narrow sense), only immense ruins, from which indeed through the labour of generations new, somewhat inhabitable (and not comparable in comfort to the old one) housing can be constructed.

The above rather faithfully reflects the general view of intuitionism; it is a bleak apocalyptic vision of 'mathematics under intuitionism'. In a sense Fraenkel was right; in spite of Brouwer's high expectations, there was in 1923 little to show for his approach.

Basically intuitionism was at this time still a land full of promises, but with little fruit. Brouwer's positive results – in particular centered around the continuity theorem – were yet to come.[6]

On the whole, Fraenkel's report on intuitionism in this book is objective and matter-of-fact, without interpretations of his own.

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[5]Weyl's intuitionistic views are discussed in [van Dalen 1995].

[6]Cf. [van Dalen 1999] §8.7, where the new ideas on choice sequences are presented, and §10.2, where the spectacular breakthrough of the fan theorem, the bar theorem, the continuity theorem etc. is documented.

In the same year Brouwer visited the Fraenkels in Marburg, during the annual meeting of the German Mathematics Society.[7] In a message of July 11 1923 Brouwer had announced his plans to give a talk in Marburg, sending at the same time the (final?) corrected proofs of the "Einleitung". The book was duly produced and in December 1923 Mrs. Fraenkel, while visiting Amsterdam, presented Brouwer with a copy of the new edition, for which Brouwer thanked in a amiable letter (December 5 1923), praising the book – "The book will exert (...) in very wide circles, both of mathematically and of epistemologically interested readers, an intensive and beneficial effect, while nowhere you make it hard for the reader, and nonetheless you usually take him to the heart of the matter."

After this letter no more exchanges are documented, until suddenly there is an upset and anxious letter from Brouwer on December 21, 1926.

Fraenkel had given a series of lectures on set theory in Kiel; the lectures were intended for a broad audience and dealt with a wide range of topics in the foundations of mathematics. The publisher, Teubner, had invited Fraenkel to publish the lectures in the well-known series *"Wissenschaft und Hypothese"*. As in the case of the *Einleitung*, Fraenkel had asked Brouwer to go through the proofs, and Brouwer had agreed to do so. Fraenkel had again devoted ample space to a treatment of intuitionism, but this time the situation had changed drastically. In the first place the hostilities in the Grundlagenstreit had gone beyond the traditional limits of precise but courteous exchange. Hilbert's attacks got more personal than ever, and the debate had turned from a scholarly exchange into a conflict that threatened to split the German mathematical community. Brouwer had scored a number of successes; he had achieved the (generally thought) impossible, that is, he had transcended the purely combinatorial part of constructivism in his fan theorem, continuity theorem and related facts; moreover he had gained access to the prestigious platform of the *Mathematische Annalen* with his exposition of the intuitionistic program. And finally, the mathematical world had started to take him seriously. As a token of the latter, one may consider his invitation to give a lecture course on intuitionism in "the other mathematical city", Berlin.[8]

Given the appreciable progress in intuitionistic mathematics, and the issues at stake in the mathematical power game, a fair and faithful report became of the greatest importance for Brouwer. This may help to explain his tense reaction to Fraenkel's new book.

One can imagine that Brouwer was incensed when almost three weeks after he got the proof sheets, Fraenkel asked him to return them because the publisher had set a tight schedule. He wrote a sharp letter to Fraenkel:

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[7] 20-25 September 1925. Brouwer gave a talk with the title "The role of principle of the excluded third in mathematics".

[8] His lectures have been published posthumously, [Brouwer 1992].

Dear Mr. Fraenkel, December 21 1926
I cannot tell you how dumbfounded I was when you informed me, only three weeks after I had received your first proof sheets, that the time for adopting possible suggestions for changes had run out. What kind of a wizard you must have taken me for, if you deemed me capable of studying in three weeks time a book of more than 100 pages so thoroughly, in the middle of term, when my time was almost completely claimed by other matters, that I could bear the responsibility for suggestions of changes! Even now I have not finished my appraisal in detail; I will certainly need the week of Christmas for that. With the unexplainable and in my opinion for all parties (author, publisher and public) harmful haste that is made with this book, the only way out for me is to turn my comments into a review of your book, where I will have to rectify a good deal (in particular where intuitionism is concerned). But it is very good that I have for once a reason to have done with the false information on intuitionism which is given to the public nowadays.
In order to make my review, as far as possible, free from personal remarks I would like to suggest three small changes, which certainly can be inserted in the galley proofs:

1. to strike out the (indeed totally unfounded) insinuation in footnote 12.
2. to speak in page proof 20, in line 21 from the bottom, line 15 from the bottom and line 7 from the bottom, only of Brouwer instead of the intuitionist in general.
3. to include in the bibliography my collective intuitionistic papers (among which are the, so far, only existing "not just verbalizing, but also creating" publications in the area of intuitionism – not counting the dissertation of Heyting).

With best greetings and wishes for the holiday,
Your Brouwer
P.S. A batch of reprints has to-day been dispatched to you. It is clear that under the present circumstances I cannot allow, that my perusal of your proofs is mentioned in any way in your book or its foreword.

Clearly, Brouwer wanted more time, as he had a great deal to say about Fraenkel's representation of intuitionism. In the absence of the original proofs, it is hard to say, what the specific comments of Brouwer were. In particular – what insinuation was hidden in the mentioned footnote? Brouwer's objections will be sketched in the remainder of the present paper. Let us recall that in 1926, most of the initial foundational problems of intuitionism had been overcome, and in particular the local uniform continuity theorem had been proved.[9] So Brouwer was not kindly disposed towards a description of his intuitionism as (i) a polished version of earlier constructivisms, (ii) only a tiny fragment of traditional mathematics.

In Fraenkel's œuvre, the *Zehn Vorlesungen* stands out as a provocative, inspiring booklet. It evidently is the result of lectures, intended to keep the audience interested. As in 1923, Fraenkel was under the influence of the style of Weyl's "New Crisis" paper, [Weyl 1921]. Any reader will agree that Fraenkel managed to evoke a dramatic atmosphere, well suited to keep the reader spellbound. Some examples of his graphic expressions are " ... , which in the otherwise essentially finitary processes of these domains introduces the infinity bacillus and thus contaminates them in the eyes of the intuitionists" (p. 49); and after observing that the intuitionists amputate a large part of analysis from the body of mathematics, Fraenkel goes on "however, just as after the amputation of a limb, after the already completed recovery of the physiological process in the organism of the body, pains are still subjectively experienced in the amputated limb, the proofs and statements of the intuitionistic torso of mathematics take almost painfully complicated forms" (p. 58).

On the whole, the *Zehn Vorlesungen* monograph is a balanced presentation of the state of foundational research, without going into technicalities. But no matter how objective Fraenkel tried to be, below the surface his heart was rather with Hilbert than with Brouwer. The '*ignorabimus*'[10] did not appeal to his taste, but he conscientiously tried to give Brouwer credit where it was due.

Why then, was Brouwer so upset? The answer can be found in the following letters.

> January 12 1927
>
> Dear Mr. Fraenkel,
> That Cantor's fundamental theorem is "obvious" for completely deconstructible point sets,[11] but false for general point sets, has nothing to do with a "gradual refining" of the basic notions, but only with the fact that the intuitive initial construction of mathematics (which, where it occurs with my predecessors, never goes beyond the denumerable), was at first (1907) explained as a completely deconstructible, finitary spread, next as completely deconstructible (not necessarily finitary) spread, and finally as simply a spread, but which was always in the phase of its introduction called "spread", for short. One cannot keep introducing new terminology all the time; therefore I have always

[9] See [van Dalen 1999] chapter 10.

[10] A term repeatedly used by Hilbert, who violently opposed its message: ' we shall not know'.

[11] *vollständig abbrechbare Punktmengen*

called my basic construction of mathematics, any time it required a generalization, "spread", as before. Only a few months ago such a generalization became again necessary, as you can read in my note "Intuitionistic introduction of the notion of dimension". Also after this generalization certain, so far "obvious", theorems will turn out to be "false". Nonetheless, admonishments from your side, as in the mentioned footnote, do not have the least justification.

Should you want to stick to this humiliating and hollow insinuation, even after my urgent request and my urgent advice to strike it out, then the competent reader (I too, claim to qualify as such) can only view that as a declaration of war to me; I am asking myself in vain what grounds I could have given you.

Excuse me that I write sharply and clearly; but I will have to do so in public afterwards, and then it should not be said that I have not drawn your attention to the implications of your statements involved here, and warned you for them.

With best greetings
Your Brouwer

January 28 1927
(Address until the middle of March)
Berlin-Halensee, Joachim Friedrichstr. 25 II

1 Enclosure.

Dear Mr. Fraenkel,

You are really mistaken, and you really injure me again, if you attribute to my latest card an "irritation which is independent of you"; please consider that you quote my position of 1913 and 1919 with respect to Cantor's fundamental theorem in your letter of December 30 1926, as an example of the fact that in connection with the gradual refining of the fundamental notions the notion "obviously" leads easily to mistakes, a statement which must have seemed also to you, after the discussion on my latest card, just as unfounded as it is insulting.

How little one can speak of a "declaration of war" from my side, and how much I muster all my efforts to avoid a public fight between us both, you can conclude from the fact that I have succeeded in getting a statement from Teubner that he is even now prepared to insert larger changes before the printing of your book. And so I would like to beg you urgently not to continue the expropriation which the German mathematical review literature has practised on me, by making me share what is my exclusive personal intellectual property with Poincaré, Kronecker

and Weyl (I am to blame to a certain extent myself for this injustice, due to the fact that I have associated myself a couple of times with my predecessors in a manner which could easily mislead the superficial reader, calling them intuitionists).
For your orientation I send you (with the request to return it someday, because it is my last copy) the German translation of a paragraph of an essay, that will be published in the *Revue de Métaphysique et de Morale*, and I propose for your book the following changes, which are minimally required by justice:

: α To edit the second paragraph of §6 of the 3/4-th lecture:[12]
"[in] this intuitionism two phases can be distinguished, of which the first one is only a phenomenon of reaction [. . . old text . . .] of the last quarter [. . . old text . . .] by Cantor; at the beginning of this century [. . . old text . . .] adopted a much milder position.
The second, much more radical phase, which does not just concern the foundation of mathematics, but which reshapes the complete doctrine of mathematics, was inaugurated by Brouwer, who was joined by Weyl as an adherent. According to a formulation of Brouwer this neo-intuitionism is based on the two following principles:

1. The independence of mathematics [. . . old text . . .] will be capable.
2. The constructive definition of set [spread] [. . . old text . . .] without using the Bolzano-Weierstrass theorem".

These two principles are on the one hand exclusively mine,[13] on the other hand they implicitly embody in a completely rigorous way the whole future rebuilding of mathematics.

: β Lines 15-21 of section §9 of the 3/4-th lectures[14] are to be revised for example as:

"[. . . old text . . .] of a real function which is continuous in a closed interval; the deficiency of this proof is matched in intuitionism (cf. Brouwer 5) by the curious (in fact in no way obvious, but rather deep) fact, that each function which is defined everywhere on a continuum,[15] is uniformly continuous".

[12] cf. [Fraenkel 1927] p. 34, 35.

[13] [Brouwer's note] so that it is a crude injustice with respect to me to claim that "these considerations of the new adherents to intuitionism have emerged, at totally different places, independent of each other, in a remarkable agreement".

[14] cf. [Fraenkel 1927] p. 48.

[15] [i.e. a connected compact set]

(In the summer of 1919 I have in personal conversations with Weyl in the Engadin, as a result of which he was converted to my views, once, in connection with the definition of the continuous function in §1 of my *Begründung der Mengenlehre unabhängig vom logischen Satz vom ausgeschlossenen Dritten* stated and motivated the conjecture that these functions are the only ones existing on the full continuum (cf. in this connection p. 62 of my paper *Über Definitionsbereiche von Funktionen*, which has just appeared in the Riemann issue of the *Annalen*). The legend which has since then been circulated about Weyl, "that it is obvious in Brouwerian analysis that there cannot exist on the continuum any but uniformly continuous functions", can only be based on this (... and other ones, half understood by Weyl) stated conjecture).

: γ Extend line 16 of the first paragraph of §10 of 3/4-th lecture[16] as follows:
"in an inductive (or recurrent) way. Over and above this, Brouwer has subsequently made the step (already mentioned in §6), that he unfolds the ur-intuition further to the general spread construction, and in this manner extends the intuitionistic founding of (discrete and denumerable) arithmetic to (continuous and non-denumerable) analysis. From this ur-intuition, stressed with special emphasis ... ."

: δ To complete the part of the References which concerns me, at least as follows:

: " 1. Begründung der Mengenlehre unabhängig vom logischen Satz vom ausgeschlossenen Dritten I–II. Begründung der Funktionenlehre unabhängig vom logischen Satz vom ausgeschlossenen Dritten. I. *Amsterdamer Verhandelingen*, **12** no. 5, 7, 13, no. 2 (1918–1923).

: 2. Intuitionistische Mengenlehre. *Jahresbericht der Deutschen Mathematiker Vereinigung*, **28** (1919), p. 203–208.

: 3. Über die Bedeutung des Satzes vom ausgeschlossenen Dritten in der Mathematik, insbesondere in der Funktionentheorie. *Journal f.d. reine u. angewandte Mathematik*, **154** (1925), p. 1–7.

: 4. Zur Begründung der intuitionistischen Mathematik I–III. *Mathematische Annalen*, **93** (1925), S. 244–257; **95** (1926) S.453–472; **96** (1926),p. 451–488.

[16] cf. [Fraenkel 1927] p. 50.

: 5. Über Definitionsbereiche von Funktionen, *Mathematische Annalen*, **97** (1926), p. 60–75."

(The citing of the three Amsterdam essays would in any case be more necessary than that of the three *Annalen* papers, which altogether only bring a technical elaboration – without any philosophical addition whatsoever – of the first (least important one) of the three mentioned *Verhandelingen*. And the citation of my paper which appeared in the Riemann volume, which is of central importance for the continuity question for full functions (cf. above under β) and in general for the continuum problem, seems to me of the utmost urgency, where for the rest you mention anyway indeed every philosophy of the textbooks on set theory).

: In the last paragraph of §8 of the 9/10-th lectures, line 17 from the bottom, mention instead of "the opinion of the radical intuitionist", "the opinion of Brouwer" (this opinion is, even if it has since then been repeated after me by others, nonetheless to no lesser degree my intellectual property).

According to a statement of Schopenhauer, there will be practised against each innovator, by the automatically appearing opposition, at first the strategy of (factual) ignoring (*totschweigen*), and after the failure of this strategy, that of priority theft. Should this also apply in my case, I am convinced that you do not belong to my enemies, that on the contrary you harbour the wish— and after learning the above—will help to make the above-mentioned strategy against me as little successful as possible.

Finally I beg you to believe that the purely factual content of this letter is accompanied only by benevolent and friendly feelings towards you.

With best greetings, your Brouwer

Vienna February 4 1927

Dear Mr. Fraenkel,

Many thanks for your latest letter. Perhaps you will have the kindness, to send me (at my Berlin address) a copy of the formulation of the insertion, contrary to my wishes, of information on our strictly private correspondence, so that I can, if somehow possible, state before the printing, that I agree.

The preface should as a matter of fact by no means conceal the fact that, if the process of the printing had answered my expectations, I would had made roughly twenty times as many suggestions for modifications, as had been possible for me in the

> actual, deplorable course. Also, for example, the *Revue de Métaphysique et de Morale* should under no circumstance be mentioned in the preface. And many more possibilities occur to me, that would not be acceptable to me, so that I must urgently beg you, to send me, if possible by return mail, the mentioned text.
> With best greetings,
> Your Brouwer

It appears that Brouwer was dissatisfied with Fraenkel's lumping together of a large number of constructivists of varied foundational views. Indeed, one gets the impression that Fraenkel had decided to apply the name to everybody with doubts about the excluded third or pure existence proofs. On page 34 Kronecker is classified as a intuitionist, together with Borel, Lebesgue and Poincaré and on page 49 Skolem and Weyl are added to the camp of the intuitionists.

As a matter of fact, Fraenkel did make distinctions between the various intuitionists, e.g. the 'radical intuitionists' (Brouwer) and the 'moderate intuitionist' (like Poincaré, page 117 ff.).That Brouwer was nonetheless offended can easily be understood. Apart from Kronecker, who carried out parts of the arithmetisation of analysis and algebra, only Brouwer had consistently developed a program for a practical constructivistic revision of mathematics. Moreover, his ideas and methods were incomparably more consistent and imaginative than those of his fellow travellers. Weyl should probably be viewed as the most radical follower, but Weyl's views, those of the "New Crisis"-paper, were clearly grafted on Brouwer's ideas.

In short, Brouwer not only saw an attempt at a wholesale identification of all kinds of constructivism with his intuitionism, but also signs of misattribution of ideas and results.

The card of January 12 1927 seems to elaborate the content of the mentioned objectionable footnote.

Brouwer's pre-occupation with *Cantor's Haupttheorem* is not surprising. For a long time the theorem, nowadays called the Cantor–Bendixson theorem, was one of the highlights of set theoretic topology, it was one of the first structure theorems about the continuum (and similar spaces). It states that each closed set can be split into a denumerable and a perfect part. Brouwer had in his topological period generalized the theorem, [Brouwer 1910], so he was in all respects entitled to consider himself an expert on the topic. The references in the letter are instructive as they give a brief genealogy of the notion of spread. The notion that appears in the letter is that of *völlständig abbrechbare Menge*, translated here as "completely deconstructible spread". The notion occurs in [Brouwer 1919A], it is best viewed as a constructive analogue of the proof technique of the Cantor–Bendixson theorem: consider a closed set A and remove the subset A' of all isolated points; by iterating this process transfinitely one eventually obtains a perfect (or an empty) set. In [Brouwer 1919A] Brouwer tried to follow this method, but apparently was not satisfied with the details. In his private reprint copy he indicated a number of problems, unforeseen at the time of writing. The upshot of the paper is that for deconstructible sets suitably formulated versions of the Cantor-Bendixson theorem hold. In an earlier paper (the review of Schoenflies' second edition of the set theory *Bericht*), Brouwer had launched a new characterization of sets allowed in the Intuitionistic framework, the so-called "well-constructed sets", which take an intermediate position between the sets of the dissertation, [Brouwer 1907], and the spreads of 1918. These well-constructed sets were by definition unions of a denumerable set and a spread-like part, avant la lettre. He claimed therefore that the Cantor–Bendixson theorem was obvious, – a claim that was withdrawn in [Brouwer 1919B]. Even worse, the theorem was pronounced false, and Brouwer revised his position by remarking that henceforth it was the task of intuitionism to formulate suitable conditions for the theorem to hold.

The important historical fact is that Brouwer slowly obtained his ultimate notion of spread; the roots are visible in the dissertation (literally: there is a picture of a subtree of the binary tree on page 65!),[17] but it took him years to think out the legitimacy of choice sequences, and to find the proper formulation and principles. And, indeed, in 1926 he again saw himself forced to further generalize his spread definition (cf. [Brouwer 1926], p. 1).

Apparently Fraenkel had complained that Brouwer's use of ever new notions was confusing (or worse). He undeniably had a point; Brouwer's search for the best possible notions often resulted in revisions that looked mysterious to the outsider. He was quite frank about this fact; in [Brouwer 1919B], footnote 9, he admitted that in earlier papers the consequences of the Intuitionistic position were not yet fully clear to him. But revisions, one would say, are part of the normal mathematical practice. Perhaps the formulation of the footnote was unfortunately chosen, so that Brouwer felt he had to take a stand. He must have felt that any well-informed reader would interpret Fraenkel's comments as a 'declaration of war'.

The next letter is a reaction to a letter of Fraenkel which has not been preserved. Here Brouwer had calmed down somewhat, although he explicitly complains that the comments in the German literature do him an injustice by expropriating that what is his 'exclusive personal intellectual property', by making him share it with Poincaré, Kronecker and Weyl. He is, however, fair enough to take part of the blame, as he had himself admitted his predecessors in the company of intuitionists.[18]

[17]The reading of the relevant passage is not all that obvious, so it is helpful indeed to see here that Brouwer in retrospect confirmed that spreads occur *avant la lettre* in his dissertation.

[18]Cf. [Brouwer 1907], p. 83; his own neo-intuitionism is mentioned on p. 85.

The letter continues with a number of suggested corrections/improvements. All of them were adopted in one form or another by Fraenkel, correctly so since he got the authorised version 'straight from the horse's mouth'. Brouwer used the term 'neo-intuitionism', which he had already introduced into his inaugural address in 1912. The term is unfortunate, as it suggests the existence of an earlier 'intuitionism'. However, all the constructive and semi-constructive movements and individuals before Brouwer had too little in common to designate them by a common name. Brouwer later did not use the term again; on the contrary – in his Berlin Lectures in 1928, he referred to Borel, Kronecker, Lebesgue, Poincaré as "pre-intuitionists". Bockstaele in his monograph *Het intuïtionisme bij de Franse wiskundigen*, [Bockstaele 1949], speaks of 'semi-intuitionists', which seems a better choice.

Brouwer's letter is unusually candid, an indication that he was trying to avoid another conflict, of which his career was so rich. All evidence points to the fact that Brouwer appreciated Fraenkel's interest and company, as the end of the letter convincingly shows.

The correspondence (of which Fraenkel's letters are missing) shows that the two reached an agreement. Brouwer even retracted his refusal to be mentioned in the foreword, but he was careful enough to ask Fraenkel for the literal text of the passage in which he was to be mentioned. He made it, however, perfectly clear that, had there been time enough, "he would have made twenty times as many proposals for changes".

The episode ended with Brouwer's review of the *Zehn Vorlesungen*, in the *Jahresberichte der Deutschen Mathematiker Vereinigung*, ([Brouwer 1930]). The review faintly echoed the demand for "twenty times as many changes", it closes listing a number of misconceptions of Fraenkel with respect to intuitionism. The most glaring one is Fraenkel's assertion (p. 44) that intuitionists only allow decision-definite (*Entscheidungsdefinite*) sets, a claim that Fraenkel had borrowed from Becker.

Another bone of contention is Fraenkel's claim that a consistency proof of the axiom of choice shows that there is no hope for the intuitionists to refute it (p. 58), see below.

The review itself is on the whole critical in tone. The obliteration of the distinctions between the various kinds of intuitionists was a conspicuous topic of the review. After acknowledging the correct description of the position of new intuitionism on pages 34, 35 (where Fraenkel had adopted Brouwer's proposals almost verbatim), Brouwer found the treatment lacking in precision in the later parts, where under the name 'intuitionism' now the old, then the new variety was discussed.

We note in passing that Brouwer here explicitly disagreed with Fraenkel's conjecture that even the intuitionists would attach an important epistemological value to consistency proofs.

Brouwer kept his drafts and notes for the review. The collection is unfortunately not complete; in particular Fraenkel's letters and the manuscript intended for the *Revue de Métaphysique et de Morale* are missing. Unfortunate as this may be, the remaining material allows us a glimpse of Brouwer's ideas at the time. It is likely that the missing manuscript was a pre-version of the Vienna Lectures. After all, Brouwer was paying attention again to the philosophical side of his enterprise, and between the Berlin Lectures and the Vienna Lectures the missing manuscript can probably be interpolated. There are, however, some drafts that are of a philosophical – foundational nature.

A fairly complete draft of a review of the *Zehn Vorlesungen* is reproduced below. The document is not dated, and as far as I know it was not published. The draft bears no resemblance to the published review. Curiously enough, no criticism of Fraenkel presentation of intuitionism is mentioned.

Brouwer mentions two shortcomings of the monograph: the absence of an analysis (or even definition) of his notion of spread, and the complete exclusion of topology, or 'set theoretic geometry' as he called it. Apparently this version was written in a more benevolent mood, it shows a Brouwer who had decided to ignore the friction caused by the proof reading, but it also shows that the topologist was still alive inside him.

Unpublished review.

> The book contains ten lectures, given in the summer of 1925 in Kiel, supplemented by numerous references to the literature. The first double lecture is devoted to a short, very clear sketch of Cantor's theory of abstract sets and an exposition of the set theoretic antinomies. After the introduction of the notion of equivalence and the notion of cardinal number, the non-denumerability of the continuum is proved. Next the notions required for the arithmetic of cardinal numbers, together with order types and ordinal numbers, are introduced, after which the well-ordering theorem and the continuum problem are mentioned. A discussion of the most important set theoretic antinomies follows.
>
> The second double lecture begins with an exposition of Poincaré's position, which sees, as everybody knows, in the so-called impredicative procedures the source of the antinomies. Fraenkel is of the opinion, as he has stressed at earlier occasions, that "between the radicalism of the modern intuitionists on the one hand and the successes of the axiomaticians on the other hand, the problems connected with the non-predicative have been relegated to the background", without a material justification. For, Fraenkel argues, intuitionism on the one hand, which goes far beyond the

rejection of the impredicative procedures through its consistent rejection of all pure existence judgements, buys this transcendence of Poincaré's position with many sacrifices of results and with complications in proofs—formalism on the other hand, which also justifies (or tries to justify) certain impredicative procedures as non-contradictory, is denied the advantage of constructiveness for these theorems. –

The discussions on impredicative procedures are followed by an extensive exposition of the intuitionistic criticism. The author describes the grand discussion, called forth by intuitionism, without taking up a resolute private position; he rather has the evident intention, to make the reader acquainted with, and to make clear to him, each single position. And Fraenkel could indeed attain this goal for everyone who allows himself to be introduced by the book to the collection of problems. That the constructive part of intuitionism, Brouwerian set theory, finds no exposition in the book, will be experienced as a lacuna, the more so since comprehensible expositions and explanations, even of Brouwer's definition of set,[19] are for the time being available only in small numbers.

The third double lecture treats, essentially following Zermelo, the axioms of set theory. "The formulation of the axioms, leads", as Fraenkel observes on p. 60 "to a logical analysis of the notions, methods and proofs, which are present in the historical set theory of Cantor .... Furthermore,[20] if anything were to be gained by the axiomatic method, the axioms must obviously have a so narrow and well-delimited character, that the antinomies of set theory cannot be derived from them by means of deductions", where of course, as long as the consistency of the axiom system is still lacking, the question remains open wether perhaps the axiom system carries the germ of other, as yet unknown, contradictions, "A refutation of the intuitionistic reflections and intuitions is, to be sure, for the time being neither attempted nor made possible". – The axiom of choice is, in accordance with its historical role, treated especially thoroughly. The fourth double lecture begins with a reference to the most essential formal gap of Zermelo's system, which is caused by the use of a notion of "meaningful property" which is not made sufficiently precise. Then an exposition of Fraenkel's own construction follows, in which the notion of a meaningful property is replaced by a strictly specified notion of function. If x is some variable set, then as a function counts: each fixed set, furthermore the set x itself, and in addition the union

[19] i.e. spread.

[20] "Weiter" in Fraenkel's book; Brouwer writes "Dabei".

and power set of x, which exist on the grounds of the axioms; finally, if $\varphi(x)$ and $\psi(x)$ are functions of x, the pair $\{\varphi(x), \psi(x)\}$ and the function of a function $\varphi[\psi(x)]$.

Where now, with Zermelo, in each set all elements which are characterized by a meaningful property can be collected (separated), with Fraenkel it is postulated that: if m is a set and $\varphi(x)$ and $\psi(x)$ are given functions in the adduced sense, then there exists a set consisting of all those elements x of m, for which the set $\varphi(x)$ is an element of the set $\psi(x)$ (resp. is not an element of the set $\psi(x)$). Taking into account the above mentioned improvement, the axiom system of Fraenkel is decisive step forward with respect to that of Zermelo. The formulation of the axiom system is followed by a sketch of the construction of set theory on the basis of the axiom system.

The axiomatic theory of equivalence and, in the beginning of the fifth and last double lecture, the axiomatic theory of order, plus that of finite sets is sketched. Then considerations on completeness, consistency and independence of axiom systems in general and specifically of the set theoretic axioms follow, where in particular Hilbert's metamathematical methods are discussed.

A sketch of the reflections, which had led Fraenkel to a most remarkable contribution to the independence problem, namely to a proof of the independence of the axiom of choice in the axiom system of Zermelo, provided the axiom of separation is replaced by Fraenkel's precise formulation, forms the end of the book.

This, briefly, is the contents of Fraenkel's book on abstract set theory, which is clear, and which is written in a stimulating manner. It would seem desirable to me that Fraenkel had stressed and further elaborated in his book that Cantor's achievement is by no means exhausted by the founding of abstract set theory. The fact that Fraenkel designates abstract set theory simply as Cantor's set theory, could easily divert the attention of someone who reads Fraenkel's book by way of introduction from Cantor's gigantic creation of set theoretic geometry (the theory of point sets), and it may encourage many a mathematician who feels unfavourably disposed towards abstract set theory to throw away the baby with the bath water and to renounce a manner of view which has produced one of the greatest revolutions in science. The basic idea of set theoretic geometry, which is completely independent of the controversies about abstract set theory, consists of the fact that arbitrary subsets of Euclidean (and more general) spaces (that is, subsets of given domains of elements) are considered and investigated with respect to their geometric (topological and metrical) properties, so that, even if one restricts oneself to subsets which can be built constructively in the strictest sense, still such an immeasurable extension of the domain of geometric objects of research results, that it is at best comparable to the one that in its day was accomplished by the introduction of the notion of 'coordinate'. Here a realm of the most marvellous insights, which go back to Cantor, has been opened up, and indeed with methods which are epistemologically at the level of the methods of analysis and the calculus of variations. Apart from the just mentioned possible side-effect, indeed unintended by Fraenkel, the book can be heartily recommended to anyone who looks for an introduction into the foundational problems of abstract set theory, and who has some training in mathematical thinking. In particular it would be desirable that it would find a reception and careful observation in philosophical circles. But also professional mathematicians will welcome its publication.

Among the remaining drafts in the Brouwer Archive there are two sheets with notes made by Brouwer while preparing his review. The notes are often a bit criptic for the reader, although they must have been clear enough for Brouwer. The numbering is not Brouwer's. The numbers in brackets refer to the pages of the *Zehn Vorlesungen*. Comments are added in small print.

Brouwer's notes re review Fraenkel 1927.

1. – the material for a-logical mathematics can only be found with me. With me only Heyting and (a little) Weyl have ...
   See the letter of December 21 1926.
2. – (51) Kronecker counts himself as an intuitionist because at that time no antinomy was required to argue about.
3. – (86) a thoughtless proof never guarantees anything (all numbers rational or irrational).
   This is a comment on Fraenkel's claim that even the intuitionists would acknowledge that one can never show (presumably from ZF) the opposite of a statement that has been derived from $ZF + AC$. Brouwer's objection is of foundational nature: a formal (i.e. thoughtless) proof from certain principles has no prima facie power over the universe of mathematics.
   Fraenkel's claim is curious, it requires quite a bit: consistency of $ZF + AC$. This criticized statement, "in accordance with Hilbert's modern views" as Fraenkel puts it, may just have been an informal conviction dropped in a popular lecture; nonetheless it illustrates that in the discussions around the Grundlagenstreit precision was often lacking.
4. – (152) the old intuitionists (Poincaré) still work with consistency

A comment on Fraenkel's observation that intuitionists do not need a consistency proof (p. 152, line -7). Also a reference to Poincaré's 'consistency = existence'.

5. – (73) this is true neither for the old, nor for the new intuitionism.

   Refers probably to the lines 13 ff., where Fraenkel states that giving up non-denumerable cardinalities, is a "suicidal discarding of the science of Cantor in favour of intuitionism". Indeed, the old intuitionists Kronecker, Weyl and others had shown how to salvage large parts of analysis without recourse to higher cardinalities, and the new intuitionists (Brouwer) had accepted non-denumerable sets.

6. – 63) "genetic" = "confused", are there followers ? or else "genetic" = "neo-intuitionistic". Then incorrect.

   Here Brouwer critizes Fraenkel's notion of 'genetic'. In the first chapter set theory is explained along Cantorian lines, what since Halmos has become known as 'naive set theory'. Fraenkel uses the predicate 'genetic' in this sense. It would be difficult to see this, however, as a construction method for the set theoretic universe, hence Brouwer's qualification as 'vague'. For intuitionists the genetic method is a precise one, embodied in Brouwer's conception of the 'ur-intuition'. The intuitionists would, however, not recognize Fraenkel's justification along axiomatic lines.

7. – before the antinomies nobody had doubted that mathematics (outside set theory) was correct.
8. – intuitionism not popular à la Einstein.
9. – that sentence of Kronecker can have any meaning.
10. – (41) not if one has proved the consistency of p.t.e.

    This comment probably refers to the footnote on p. 41, where Fraenkel discusses the possibility of a provably undecidable problem (decidable in the logical sense $\vdash A$ or $\vdash \neg A$, or maybe $\vdash A \vee \neg A$).

11. – (58) spreads — species

    Brouwer wants to contrast his constructive 'sets' (i.e. spreads) with species, which are the analogues of Cantorian sets.

12. – (47) contra Weyl's priority

    The historic description of Fraenkel is here correct, it confirms Brouwer's priority over Weyl. Brouwer apparently attached a considerable importance to this point. Weyl's superior presentation had convinced many readers that (at least) part of the basic notions were created by Weyl.

13. – (152) as if intuitionists would allow axioms at all! Here to point at the role of "language".
14. – (154) "every number is either rational or irrational".

    On this page Fraenkel offers an extensive justification of the axiomatic (or better, formal) approach. His thesis is: the worst intuitionists can do is declare a notion or statement meaningless. He inquires why (e.g.) the principle of the excluded third does not yield contradictions, as is the case in all other known mathematical errors? Brouwer's note must refer to the intuitionistic theorem that not every real number is rational or irrational ($\neg\forall x(x \in Q \vee x \notin Q)$.) a fact that he had established in his Berlin Lectures (1927). Apart from this intuitionistic objection, it is remarkable that Fraenkel without reservations follows Hilbert in the conviction that "all conclusions, even when not correct, are in harmony" (*Schlussfolgerungen, die, wenn nicht richtig, so doch mindestens übereinstimmend sind)*. Probably the belief in one underlying mathematical reality was so strong that hardly anyone even considered the possibility of inherent incompleteness.

15. – At the beginning of these objections refer in a note to the preface.

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16. – distinction between Zermelo and Fraenkel (104, 75, 126, 102) replacement of the meaningful property by using alternatingly def. 4 (p. 110) and axiom V (p. 106) (p.t.e., however, also to be used with Fraenkel in ax. V' (cf. ax VIII, p. 115 (114, 153)).1
17. – Application: Equivalence – Order – Finiteness (143 ....) (147).
18. – 1) Completeness of axiom system
19. – 2) Hilbert (54). here plagiarism cited, (55) "Metamathematics"= "mathematics of 2^{nd} order."

    The remark on Hilbert may appear cryptic to the reader who is not familiar with the finer details of the history of the foundations in the twentieth century. Brouwer at various occasions claimed that during discussions in the dunes at Scheveningen, in the vacation of 1909,[21] he had informed Hilbert of his detailed analysis of the various levels of mathematics, logic and language, as laid down in his dissertation. Brouwer had analyzed Hilbert's Heidelberg lecture, [Hilbert 1905], in terms of these levels. In "Intuitionistic reflections on formalism" [Brouwer 1928], Brouwer elaborates his views again, this time in the light of the new intuitionistic insights. It is known that Brouwer resented Hilbert's ignoring of intuitionistic ideas and contributions. He considered the absence of any reference or credit to his influence just ill-mannered. Hence the term 'plagiarism'.
20. – (161) Hilbert is not justified by the denumerable; his metamathematics is at most a tool to escape the set definition.
21. – (3) Independence of the axiom of choice (164, 165, 166) following Fraenkel.

The following document consists of a more precise discription of the various intuitionistic views; it is a handwritten draft and it clearly is incomplete. At the time of the Fraenkel episode Brouwer was preparing a number of philosophical expositions, to wit the introduction to the Berlin Lectures, the Vienna Lectures and the "Intuitionistic Reflections". This little discussion

[21] See [van Dalen 1999] p. 128.

clearly takes place against the background of the Fraenkel review, after a general discussion a reference to Fraenkel creeps in again.

The striking feature is the radical way in which Brouwer dissociates himself from the earlier varieties of intuitionism. The name 'alogicist' can be seen in that light.

In view of the ample reference to Fraenkel's *Zehn Vorlesungen* the draft could very well have been another draft of a review. The page numbers refer again to the *Zehn Vorlesungen*.

> The old-intuitionist assumes intuitively only the natural numbers, but arrives, however, at analysis and geometry by means of classical logic, usually under the application of axioms (e.g. for projective geometry: any two lines either coincide, or have one point in common). Although they usually let a vague intuition suggest these axioms, this intuition then should on the basis of beliefs in *a priority* give dispensation from the consistency proof, and it has also completely paralyzed the alertness for the absence of constructivity in the rules.
>
> Therefore the quotation of Kronecker can very well be applied to old-intuitionists. But in the end they are much closer to the formalists than to neo-intuitionism, in particular since the formalists, too, presuppose the natural numbers. The three categories are better characterized as Leibnizians, Kantians and Alogicism. Compared to the "alogical" or "correct" mathematics the "logical" (Kantian or Leibnizian) mathematics appears absurd, chaotic, or as a vague, respectively distorted, image of a (usually also finer and more complicated) reality. The recording of reality by means of axioms and logic has in mathematics, exactly as [... ?] only a coarsening and smothering effect.
>
> I have myself in the past (in particular in my inaugural lecture) introduced the name "intuitionism" as a collective name for Kantians and alogicists. Soon after that I have restricted it exclusively to alogicists, definitely from 1918 on. A title *Intuitionistic introduction of the notion of dimension*, therefore means with me "Correct introduction of the notion of dimension", and tells implicitly that all earlier introductions of this notion (in the first line my own one from the year 1913) are false.
>
> The mixing of both groups is completely mistaken when on p. 38 it is claimed that the "pure existence statements are in general declared to be meaningless and rejected by the intuitionists". For, the Kantians have cast in doubt only the very worst existential statements (most of all when they, in conformity with their conceptual basis, miss whatever guidance of a vague intuition), such as the well-ordering theorem, but never the existence of at least one

> point of intersection of two arbitrary lines in projective geometry, or the decimal expansion of an arbitrary real number; just so they have believed the fundamental theorem of algebra on the ground of the, until a few years ago, exclusively available Kantian proofs.
>
> Similarly it is (since Fraenkel indeed also combines the Kantians and alogicists under the name "intuitionist") misleading, if Fraenkel speaks on p. 38/39 of a revolutionizing of "classical " mathematics of the 19th century, and its replacement by a new, far more restricted "intuitionistic" mathematics, and says that "the intuitionist" construes the answer: 'either yes, or no' to an arbitrary mathematical question as an unfounded prejudice. Because here too, what is claimed about intuitionists holds exclusively for alogicists. The same holds for the "fundamentally pronounced doubt" of the conviction of the solvability of any mathematical problem, which is credited on p. 42 to the intuitionists *tout court*; whereas the Kantians (in particular the especially introduced Pasch in the pre-alogical period) never called attention to the difference between existential and constructive solutions, and on the contrary only questioned the meaningful character of the first one (in any case, not before he had learned that from me). Similarly when he speaks about the superfluousness of the consistency proof of the axioms for "the intuitionists" (p. 152); this could only refer to the (mainly occurring with laymen, but seldomly with mathematicians) "dogmatic", "descriptive" Kantians, that is to those who consider mathematics as something which rests on a collection of laws of thought, and all mathematical theorems, in particular the axioms, as synthetic judgments a priori. Not, however, to "regulating", "correcting", "canalizing", "active", "ordering", "creative", "stylizing" [persons, vD] like Klein, who consider the axiomatization as a totality of more or less efficient acts of will (specifications of vague intuitions), and just as little to Poincaré and Weyl, as Fraenkel underlines at p. 155,156.
>
> For the alogicians, however, the statement is anyway unfortunate; the alogicians do indeed not reject the "axiomatic method", but the method is not considered at all, because it concerns the mathematical language, not mathematics. The axiomatic method is thus inadmissible here, and cannot even be rejected.

The main purpose of the above lines is to clarify the relation between the old and new intuitionists. The formalists are mentioned in passing, but only to note that they, like the old intuitionists, accept the (conceptual) natural numbers. Brouwer, in this draft, seems to draw the line between old and new intuitionists in logic. He, indeed, rather stresses logical and language-like features. In particular the position of the old intuitionists on geometry is at variance with new intuitionism.

In spite of Brouwer's sincere rejection of the axiomatic method, it is good to recall that in 1925 Heyting wrote a dissertation in exactly that area. What Brouwer actually objected to, I guess, was the axiomatic method in its 'creative role', and in the framework of classical logic.[22] The objections to logic are, by the way, not a new invention of Brouwer. Jules Molk, a Kronecker follower expressed his views clearly in [Molk 1885]: "Definitions should be algebraic and not just logical", "It is not sufficient to say 'something is or is not the case"; one should not read too much, however, in statements of this sort. They must be interpreted in their historical context, and in the nineteenth century one could simply not expect a coherent critique of traditional logic.

The fact remains that even Kronecker acknowledged geometry as an independent discipline, beyond the domain of arithmetization. He would presumable be quite happy with Euclid's, or Hilbert's geometry, including its logic.

Since both the semi-intuitionists and formalists (Kantian or Leibnizian) practiced mathematics by the rules of logic, Brouwer's choice of "alogicist" is not unreasonable. Fortunately the name 'a-logical' has not been adopted (or even seriously been considered), its negative character does not do justice to the conceptual basis of Brouwer's intuitionism. Brouwer must have put forward the name just in reaction to the confusion which existed at the time, and which was not cleared up by Fraenkel's exposition.

The finer distinctions between, let us say, the semi-intuitionists and the intuitionists were blurred by Fraenkel, rather than made visible. Brouwer correctly points out that in the obvious cases, e.g. the existence of a well-ordering of the continuum, or Hilbert's basis theorem, the semi-intuitionists recognized the illusory character of 'pure existence proofs', but in more pedestrian contexts, such as the basic axioms of projective geometry (e.g. for any two distinct lines there is one point of intersection), they would happily go along. Indeed, part of the discussion of Zermelo's well-ordering theorem exhibited the marks of lofty principles to be practiced on Sundays. One wonders if Brouwer, irritated by the general confusion, did not read too much into the statements of the semi- (and quarter-) intuitionists. After all, in the twenties the foundations of mathematics were a kind of gentleman's pastime. Most participants in the foundational debate practised logic and foundations on the side, e.g. Hilbert, Weyl, Herbrand, Skolem, Fraenkel, Zermelo, von Neumann were by no means solely occupied by foundations. Brouwer's full-time occupation with foundations (from the twenties onward) was rather exceptional, and professional logicians, such as Gödel and Gentzen, had not yet entered the field. So the rather unsystematic activities of various

[22] Cf. Brouwer's dissertation (1907), p. 134 on Euclid, and p. 140 on existence.

mathematicians (e.g. Poincaré, Borel) could, from one point of view, hardly be a justification for calling them professional constructivists or intuitionists. Be that is it may, there was the pronounced tendency to lump all mathematicians with (part-time) constructive tendencies together and put them in the camp of the intuitionists.

One can well imagine Brouwer's dismay for being equated to amateurs in constructivism (no matter their status in mathematics), and to see that viewpoints were attributed in a somewhat careless way.

Brouwer's diatribe speaks for itself, most of it cannot be misunderstood. An exception must perhaps be made for the last paragraph. Brouwer's harsh words on the axiomatic method seems difficult to reconcile with the fact that Heyting had two years before defended his Ph. D. thesis, *Intuitionistic Axiomatics of Projective Geometry* (not in Fraenkel's bibliography of the *Zehn Vorlesungen*).

The fact is that Brouwer was not wholly opposed to logic and axiomatic, but rather to their roles as autonomous disciplines, preceding mathematics. In his dissertation he clearly stated that logic comes after mathematics ("Mathematics is independent of logic; practical and theoretical logic are applications of different parts of mathematics") and already in his *Unreliability of the logical principles* (1908), he formulated the positive role of logic as follows:

> Is it allowed, in purely mathematical constructions and transformations, to neglect for some time the idea of the mathematical system under construction and to operate in the corresponding linguistic structure, following the principles of *syllogism*, of *contradiction*, and of *tertium exclusum*. And can we then have confidence that each part of the argument can be justified by recalling to the mind the corresponding mathematical construction? Here it will be shown that this confidence is well-founded for the first two principles, but not for the third one.[23]

In modern language, one would formulate Brouwer's view as "intuitionistic logic preserves constructibility", a statement that finds its justification in the Brouwer-Heyting-Kolmogorov interpretation of the logical operations.

So the proper way to read the last paragraph is: the axiomatic method, understood in its creative role, makes no sense for intuitionists. By 'creative role' we mean the practice codified by Gödel's completeness theorem (or rather, the model existence lemma): a consistent axiomatic system has a model. Fraenkel, by the way, puts forward a more modest version (after Weyl): the axiomatic method consists simply of the complete collecting of the basic notions and facts, from which the collective notions and theorems of a discipline can be deduced definitionally or deductively." But without

[23] For a similar formulation cf. [Brouwer 1952], p. 141.

the more lofty aims, the axiomatic method is indeed prone to become "an empty formula game".

Brouwer, in his dissertation, directed his criticism against the modest axiomatic method and the daring version alike: suppose one shows the consistency of an axiomatic system, "does it follow from the consistency of the logical system that such a mathematical system [i.e. a model] exists? Such a conclusion has never been proved by the axiomaticians." Even Gödel's completeness theorem would not have satisfied Brouwer, because of its essentially classical arguments. Unfortunately, no comments of Brouwer are known in this matter. One would guess that the modern arguments of Veldman [Veldman 1976] would be acceptable to Brouwer.

After the above described episode there does not seem to have been any contact between Brouwer and Fraenkel (although they probably met at one conference or another). Fraenkel became highly critical of Brouwer, mainly on political grounds. In his autobiographical book *Lebenskreise*,[24] he claimed that Brouwer fostered strong German, if not National Socialistic, sympathies. One guesses that Fraenkel was not really well-informed on the matter. His claim that Brouwer was ejected from the board of the Mathematische Annalen by Hilbert, on account of Brouwer's alleged protest against the publications of too many Eastern European Jewish (*ostjüdische*) authors, is not borne out by the available evidence, cf. [van Dalen 1990].

Nor has any evidence been found that Brouwer was offered a chair in Berlin in 1933, as claimed by Fraenkel. There were, however, certain negotiations with Göttingen in 1934; possibly Fraenkel was misinformed. Apart from the brief contact with Göttingen, Brouwer kept clear of the Nazis or their Dutch followers. Fraenkel must have been the victim of ill-founded rumours; in answer to a question of Fraenkel, Courant wrote him, after declaring that Bieberbach was more crazy than dangerous, that "Much more dangerous are people like Brouwer, who has been an ardent collaborationist and has been deposed."[25] Courant was wrong on both counts, he also relied on dubious gossip that started to circulate after the war.

Later in life, Fraenkel suspected Brouwer of anti-semitic sympathies, and Brouwer never forgave Fraenkel for his real or imagined lack of cooperation in the matter of revising the representation of intuitionism. In both cases one might use the term 'sulking'. As late as 1946, in a lecture at the university of Leuven in Belgium, Brouwer mentioned Fraenkel's refusal to adopt all of Brouwer's corrections, remarking that nonetheless a public reprimand would have gone too far.[26] Fraenkel raised more serious allegations against Brouwer in his auto-biography, *Lebenskreise. Aus den Erinnerungen eines Jüdischen Mathematikers*, [Fraenkel 1967]. According to him Brouwer tried

[24][Fraenkel 1967], cf. page 160, ff.

[25]Courant to Fraenkel October 19 1945. Courant Archive

[26]Communication A. Borgers.

to block Fraenkel's appointment in Kiel in 1928. Whether this is true I have not been able to ascertain; there is no evidence in the Brouwer Archive, but there might be (or have been) a letter of reference in one of the German archives. In itself Brouwer's interference seems improbable, scientifically speaking, there was no reason to oppose Fraenkel's appointment, and political or racial arguments were not in Brouwer's line.

Summing up the history of the Brouwer-Fraenkel relationship, one can see that the two actors were not in the same class. Brouwer evidently was superior in scientific respect, and Fraenkel was the more gifted expositor. Fraenkel was by nature or training a set theorist, and the appreciation for alternative approaches was sincere, but he lacked the penetrating intellectual power to unravel the intricacies of Brouwer's admittedly complicated notions. He also missed the subtle distinctions between the various brands of constructivism. Since he did not analyze the contributions of Brouwer's predecessors he suggested a far greater analogy than actually was the case. Indeed the influence of the beautifully readable book of Fraenkel was so great that it dominated the ideas on the foundations of mathematics for a considerable period. On the technical side one could saythat Fraenkel had missed the significance (and perhaps the idea) of Brouwer's notion of spread. Thus he did no justice to the positive aspects of intuitionism and to its promises for a viable and interesting mathematics. In fact, in 1927 Brouwer had firmly established his revolutionary results in analysis, and it could no longer be maintained that intuitionism was an impoverished fragment of classical mathematics. From the "Intuitionistic Reflections" of 1927, it can be seen that Brouwer was keenly aware that these new results gave him an edge over classical mathematics: "the formalist school should ponder the fact that in the framework of formalism *nothing* of mathematics proper has been secured up to now (since, after all, the metamathematical proof of the consistency of the axiom system is lacking, now as before), whereas i basis of its constructive definition of spread and the fundamental property it has exhibited for fans has already erected anew several of the theories of mathematics proper in unshakeable certainty", [Brouwer 1928]. Fraenkel had in his presentation of the phenomenon of choice sequence heavily relied on Weyl's "New Crisis" paper, hence the flavour of the sections on intuitionism is here and there strongly influenced by Weyl's preferences. Even the corrections which Brouwer suggested—and most of them were inserted by Fraenkel—did not save the presentation of intuitionism proper. Most of the corrections were of a historic-philosophical nature.

In defence of Fraenkel it may be pointed out that the lectures of the *Zehn Vorlesungen* were presented before a large audience, and aimed at a large readership, which would have had great difficulty in grasping spreads in Brouwer's formulation. The virtue of Fraenkel's book (and all his later books) is that he introduced a broad section of the scientific community to the fascinating new developments of a mathematics in turmoil. Fraenkel also should be credited for pointing out a curious psychological hypocrisy of Hilbert, who to a large extent adopted the methodical position of his adversary – "one could even call him an intuitionist" (*Zehn Vorlesungen* p. 154). Although the inner circle of experts in the area (e.g. Bernays, Weyl, von Neumann, Brouwer) had reached the same conclusion some time before, it was Fraenkel who put it on record.

Brouwer's role in the above affair is legitimate; he correctly insisted at a fair representation of his intuitionism. But as at so many occasions, he handled the matter without humour and without concern for the feelings of the other party. A bit more consideration might have prevented a good deal of hard feelings.

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