Kreisel and Wittgenstein

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September 17, 2018

Georg Kreisel (15 September 1923 – 1 March 2015) was a formidable mathematical logician during a formative period when the subject was becoming a sophisticated field at the crossing of mathematics and logic. Both with his technical sophistication for his time and his dialectical engagement with mandates, aspirations and goals, he inspired wide-ranging investigation in the metamathematics of constructivity, proof theory and generalized recursion theory. Kreisel's mathematics and interactions with colleagues and students have been memorably described in *Kreiseliana* ([Odifreddi, 1996]). At a different level of interpersonal conceptual interaction, Kreisel during his life time had extended engagement with two celebrated logicians, the mathematical Kurt Gödel and the philosophical Ludwig Wittgenstein. About Gödel, with modern mathematical logic palpably emanating from his work, Kreisel has reflected and written over a wide mathematical landscape. About Wittgenstein on the other hand, with an early personal connection established Kreisel would return as if with an anxiety of influence to their ways of thinking about logic and mathematics, ever in a sort of dialectic interplay. In what follows we draw this out through his published essays—and one letter—both to elicit aspects of influence in his own terms and to set out a picture of Kreisel's evolving thinking about logic and mathematics in comparative relief.[1]

As a conceit, we divide Kreisel's engagements with Wittgenstein into the "early", "middle", and "later" Kreisel, and account for each in successive sections. §1 has the "early" Kreisel directly interacting with Wittgenstein in the 1940s and initial work on constructive content of proofs. §2 has the "middle" Kreisel reviewing Wittgenstein's writings appearing in the 1950s. And §3 has the "later" Kreisel, returning in the 1970s and 1980s to Wittgenstein again, in the fullness of time and logical experience.

Throughout, we detected—or conceptualized—subtle forth-and-back phenomena, for which we adapt the Greek term "chiasmus", a figure of speech for a reverse return, as in the trivial "never let a kiss fool you or a fool kiss you".[2] The meaning of this term will accrue to new depth through its use in this account to refer to broader and broader reversals.

[1] Most of the essays appear, varyingly updated and in translation, in the helpful collection [Kreisel, 1990a]. Our quotations, when in translation, draw on this collection.

[2] I owe the use of this term to my colleague Jeffrey Mehlman's in his remarkable [2010, §7].

1 Early Kreisel

At the intersection of generations, Kreisel as a young man had direct interactions with Wittgenstein in his last decade of life. Kreisel matriculated at Trinity College, Cambridge, where he received a B.A. in 1944 and an M.A. in 1947. In between, he was in war service as Experimental Officer for the British Admiralty 1943-46, and afterwards, he held an academic position at the University of Reading starting in 1949. According to Kreisel [1958b, p.157], "I knew Wittgenstein from 1942 to his death. We spent a lot of time together talking about the foundations of mathematics, at a stage when I had read nothing on it other than the usual *Schundliteratur*." Indeed, they again had regular conversations in 1942, when we can fairly surmise that the 18-year old Kreisel would have been impressionable and receptive about the foundations of mathematics. For 1943-45, however, their generally separate whereabouts would have precluded much engagement. During 1946-1947, after the war, they had regular discussions on the philosophy of mathematics, although Wittgenstein had not written very much on the subject for two years.[3] At that time, Kreisel wrote his first paper in mathematical logic, [Kreisel, 1950]. From 1948 on, they would only have had intermittent contact, as Wittgenstein had resigned his professorship in 1947 and Kreisel took up his academic position at Reading in 1949. By the end of 1949, Kreisel had submitted for publication his [1951] and [1952a], the first papers on his "unwinding" of proofs. Wittgenstein was diagnosed with prostate cancer in 1949 and died in 1951. In what follows, we make what we can of the "early" Kreisel of the 1942 and 1946-7 conversations, our perception refracted through his published reminiscences.

Nearly half a century afterwards, Kreisel [1989a] provided "recollections and thoughts" about his 1942 conversations with Wittgenstein.[4] Early paragraphs typify the tone (p.131):

> I was eighteen when I got to know Wittgenstein in early 1942. Since my school days I had had those interests in foundations that force themselves on beginners when they read Euclid's *Elements* (which was then still done at school in England), or later when they are introduced to the differential calculus. I spoke with my 'supervisor', the mathematician Besicovitch. He sent me to a philosophy tutor in our College (Trinity), John Wisdom, at the time one of the few disciples of Wittgenstein. Wittgenstein was just then giving a seminar on the foundations of mathematics. I attended the meetings, but found the (often described and, for my taste, bad) theatre rather comic.
>
> Quite soon Wittgenstein invited me for walks and conversations. This was not entirely odd, since in his (and my) eyes I had at least one advantage over the other participants in the seminar: I did not study philosophy. Be that as it may, in his company (*à deux*) I had what in current jargon is called an especially positive *Lebensgefühl.*

Kreisel soon went on (p.133): "One day Wittgenstein suggested that we take a look at Hardy's [*A Course of*] *Pure Mathematics* together. This introduction

[3]See [Monk, 1990, p.499].

[4]What we quote from [Kreisel, 1989a] is taken from the English translation in [Kreisel, 1990a, chap.9]. [Kreisel, 1978c] provides a shorter account of the 1942 engagements with Wittgenstein.

to differential and integral calculus was a classic at the time, and, at least in England, very highly regarded." Kreisel thence put the book in a mathematical and historical context, mentioning that Wittgenstein "had only distaste" for it—"something in the style, and perhaps also in the content, was liable to have got in the way"—and opining that the "foundational ideal" in Hardy was passé and to be supplanted by Bourbaki. Kreisel then recalled (p.136):

> In the first few conversations about Hardy's book, Wittgenstein discussed everything thoroughly and memorably. The conversations were brisk and relaxed; never more than two proofs per conversation, never more than half an hour. Then one switched to another topic. After a few conversations the joint readings came to an end, even more informally than they had begun. It was, by then, clear that one could muddle through in the same manner.

As a matter of fact, Wittgenstein in his 1932-3 "Philosophy for Mathematicians" course had already read out passages from Hardy's book and worked through many examples.[5] What Kreisel writes coheres with Wittgenstein having made annotations in 1942 to his copy of the eighth, 1941 edition of Hardy's book.[6]

Just before these remarks, Kreisel had given a telling example from the conversations (p.135): "If $y = f(x)$ is (the equation of) a curve continuous in the interval $0 \leq x \leq 1$ and such that $f(0) < 0$ and $f(1) > 0$, then f intersects the x-axis. The job was to compute, from the proof (in Hardy) a point of intersection." This of course is the Intermediate Value Theorem, the classical example of a "pure existence" assertion. In a footnote, Kreisel elaborates: "The proof runs as follows. If $f(\frac{1}{2}) = 0$, let $x_0 = \frac{1}{2}$. Otherwise, consider the interval $\frac{1}{2} \leq x \leq 1$ if $f(\frac{1}{2}) < 0$, and the interval $0 \leq x \leq \frac{1}{2}$ if $f(\frac{1}{2}) > 0$, and start again. This so-called bisection procedure determines an x_0 such that $f(x_0) = 0$."[7]

Kreisel mentioned "constructive content" and how "...in the conversations one looked for suitable additional data". He elaborated elsewhere ([Kreisel, 1978c, p.79]):

> Wittgenstein wanted to regard this proof as a *first step*, and restrict it by saying: the proof only gives an applicable method when the relevant decision (whether $f(\frac{1}{2})$ is equal to, greater than, or less than 0) can be done effectively (e.g. if f is a polynomial with algebraic coefficients).
>
> I still find Wittgenstein's suggestion (of a certain restriction) agreeable: *satisfaisant pour l'esprit*. But it is certainly not useful (since the restriction is hardly ever satisfied). A variant ([Kreisel, 1952b]) is *much* more useful: it applies when the restriction is only approximately satisfied, i.e. when one is able to decide not necessarily at $x = \frac{1}{2}$ itself, but sufficiently close to it (e.g. in the case of recursive analytic functions on $[0, 1]$).

[5] cf. [Wittgenstein, 1979].

[6] See [Floyd and Mühlhölzer, 2019] for accounts and interpretations of these annotations.

[7] This proof is a binary version of the original [Cauchy, 1821, note III] proof of the Intermediate Value Theorem, and there is a historical resonance here. Being a pure existence assertion, the formulation and proof of the Intermediate Value Theorem by Cauchy and [Bolzano, 1817] was a significant juncture in the development of mathematical analysis. Their arguments would not be rigorous without a background theory of real numbers as later provided e.g. by [Dedekind, 1872]. The glossy Dedekind-cut proof found in Hardy (§101) is embedded in that theory, and Wittgenstein raised issues about the extensionalist point of view generally — cf. [Floyd and Mühlhölzer, 2019].

Wittgenstein's suggestion here—what Kreisel finds "agreeable"—is quite astute, resonant with the Intermediate Value Theorem not being intuitionistically admissible. There are continuous functions for which it is not intuitionistically possible to decide for their values whether they are equal to, greater than, or less than 0.

The forth-and-back in the quotation about Wittgenstein's agreeable suggestion and then its lack of usefulness is a local *chiasmus* of some significance. Kreisel is best known today, of course, for pioneering the study of the constructive content of proofs and the metamathematics of constructivity. In recollections ([Kreisel, 1989a, p.131]) "still exceptionally vivid, though perhaps rose-colored", he is emphasizing in self-presentation the constructive content. The 18-year old Kreisel may fairly be said to have been launched into his lifelong work by these early conversations with Wittgenstein. Kreisel subsequently wrote (p.136): "After the war I had a chance to go into mathematical logic in more detail; in particular, into consistency [WF] proofs. Instead of pursuing Hilbert's aim of eliminating dubious doubts about the usual methods of mathematics a more compelling application (better: interpretation) of those proofs occurred to me. Once again, the issue was a kind of constructive content; not, however, for items in some mathematical textbook, but for all derivations in some current formal systems." This was the direction of Kreisel's initial, and incisive, work in mathematical logic published in [Kreisel, 1950], of which more below.

On Wittgenstein's side, through 1942 he was actually working as a hospital dispensary porter in London toward the end of the Blitz, coming up to Cambridge on alternate weekends to deliver lectures on the foundations of mathematics (and presumably meeting with Kreisel then).[8] During this period, he penned remarks that would be compiled into Parts IV-VII of the *Remarks on the Foundations of Mathematics*.[9] Part V of the *Remarks* has an extensive discussion of non-constructive existence proofs and Dedekind cuts—Hardy's approach to the reals.

Kreisel (p.137) went on to write that Wittgenstein lent him a copy of *The Blue Book* at the beginning of summer 1942 and that he returned it by its end. *The Blue Book* was a text that Wittgenstein had dictated for his 1933-4 "Philosophy for Mathematicians" course and of which only a few copies were maintained. In *The Blue Book* Wittgenstein first brought forth the textures of meaning and language that would be elaborated in the *Philosophical Investigations*, like "language games" and their understanding through "training" toward the beginning and what to make of "I am in pain" with respect to the "I" at the end. Notably, in the face of this Kreisel only mentioned raising a "malaise" with Wittgenstein about his notion of "family resemblances of concepts". Invested in mathematics, Kreisel gave as an example the concept of group with its subcategories, mentioning a latter-day motto of his, "*relatively* few distinctions for *relatively broad* domains of experience". He could be said to have sidestepped Wittgenstein's main thrust, as exemplified by his example of

[8] cf. [Monk, 1990, chap.21,esp. p.443], [Wittgenstein, 1993].

[9] cf. [Monk, 1990, p.438]. The part numbers given for the *Remarks* are evidently for its second edition [Wittgenstein, 1978].

"game", where various games have family resemblances but there is no property joining all instances, and the generality may be open-ended and evolving.

At this point, we record a passage from the [Monk, 1990] biography, a part of which has been passed along several times about Kreisel vis-à-vis Wittgenstein:

> In 1944—when Kreisel was still only twenty-one—Wittgenstein shocked Rhees by declaring Kreisel to be the most able philosopher he had ever met who was also a mathematician. 'More able than Ramsey?' Rhees asked. 'Ramsey?!' replied Wittgenstein. 'Ramsey was a mathematician!'

Wittgenstein was steadily drawn to mathematicians for conversation and intellectual stimulation. In the early 1940s, he would have found interaction with Kreisel in the next generation newly stimulating.

The post-war, 1946-7 conversations may have been extensive and far-ranging, but we can only make something of two published recollections of Kreisel. The first is about style, from [Kreisel, 1978b, n.2]:

> The matter of jargon, or style, came up often in my conversations with W (from 1942 to his death in 1951). For example, once after W had invited F.J. Dyson, who at the time [1946–] had rooms in College next to W's, to discuss foundations, Dyson had said he did not wish to 'discuss' anything because *what* W had to say was not different from anything everybody was saying anyway, but he wanted to hear *how* W put it. W spoke to me of the occasion, agreeing very much with what Dyson had said, but finding Dyson's jargon a bit 'odd'. On another occasion, W said: Science is O.K.; if only it weren't so grey.

This resonates with what Kreisel wrote at the end of [1989a], that "The expository style (of Wittgenstein's conversations, where 'expository' would not apply to discussions) was at any rate for me much more successful", and "Wittgenstein's favorite quotation: *Le style, c'est l'homme*". Beginning with Wittgenstein's "distaste" for the style of Hardy's book, one can venture that the young Kreisel imbibed a sensibility to "style" so construed, this later seeping into his mathematical approach and writing.

The other recollection involves consistency proofs and the unprovability of consistency. As mentioned above, from [Kreisel, 1989a, p.136] one has "After the war I had a chance to go into mathematical logic in more detail; in particular, into consistency proofs", and at that time he had done the work to be published in [Kreisel, 1950]. From that publication, we can gather that he had began by assimilating the 1939 *Grundlagen der Mathematik II* of Hilbert and Bernays.[10] Kreisel wrote in [1983a, pp.300f] about what would have been from 1946-7:

> A few days after receiving several short, reasonable explanations of Gödel's incompleteness proofs Wittgenstein opined full of enthusiasm that Gödel must be an exceptionally original mathematician, since he deduced arithmetical theorems from such banal—meaning: metamathematical—properties like *WF* [consistency]. In Wittgenstein's opinion Gödel had discovered an absolutely new method of proof.

[10] Kreisel elsewhere in [1987, p.395] wrote of "consistency proofs (which I had learnt in 1942 from Hilbert-Bernays Vol. 2)". This may have been, especially in the sense of first acquaintance, but the tenor of various other recollections would suggest first full assimilation later.

> ...
> What he meant was that the metamathematical interpretation (made possible by the arithmetization of metamathematical concepts) makes the relevant arithmetical theorems immediately evident. This can be compared to the geometric interpretation of algebraic formulas, such as $ax^2 + ay^2 + bx + cy + d = 0$, from which it becomes obvious that two such equations cannot have more than two common roots (x, y), since two circles can intersect in at most two points.

There is ample evidence that Wittgenstein had already become aware of some of the ins and outs of Gödel's incompleteness theorem a decade earlier in 1937, when Turing's work came out.[11] What Kreisel is drawing attention to is Wittgenstein's apprehension of a "new method of proof", the metamathematical interpretation making the relevant arithmetical theorems "immediately evident".

Kreisel is known to have lectured on "Mathematical Logic" at the Moral Sciences Club on 27 February 1947, with Wittgenstein chairing.[12] The subject was presumably on the work to be published in [Kreisel, 1950].

Kreisel in that [1950] deftly provided "constructive content" to the Gödel incompleteness theorem, first exhibiting the sensitivity to recursiveness that would be a hallmark of his subsequent work. Drawing out recursive aspects of the Hilbert-Bernays 1939 *Grundlagen der Mathematik II* proof of Gödel's second incompleteness theorem, Kreisel established, in modern terms, that the Skolemized form of Gödel-Bernays set theory has no recursive model, exhibiting as a corollary a formula of first-order logic which has a model but no recursive model. Discussing at the end the definability of predicates through diagonalization, Kreisel provided the following telling, footnote 4:

> A great deal has been written since Poincaré on diagonal definitions occurring in a system of definitions. A very neat way of putting the point is due to Prof. Wittgenstein:
>
> Suppose we have a sequence of rules for writing down rows of 0 and 1, suppose the pth rule, the diagonal definition, say: write 0 at the nth place (of the pth row) if and only if the nth rule tells you to write 1 (at the nth place of the nth row); and write 1 if and only if the nth rule tells you to write 0. Then, for the pth place, the pth rule says: write nothing!
>
> Similarly, suppose the qth rule says: write at the nth place what the nth rule tells you to write at the nth place of the nth row. Then for the qth place, the qth rules says: write what you write!

Kreisel is acknowledging the rule-following versions of the Gödelian contrary, as well as the Turing direct, diagonalization arguments as given by Wittgenstein in conversation. As Kreisel moved forward with his "unwinding" [1951, 1952a] for constructive content of known proofs, this marks a closure point for the formative time of his direct engagement with Wittgenstein. Significantly, Kreisel will return to this footnote in the fullness of time and with a altered perspective, as will be discussed in §3.

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[11] cf. [Floyd, 2001]. *Remarks on the Foundations of Mathematics* [1956], Part I, drawn from 1937 manuscripts, has Wittgenstein ruminating over Gödel's proof of the the incompleteness theorem.

[12] cf. [Wittgenstein, 1993, p.355].

On Wittgenstein's side, he with a change of aspect wrote in 1947 about Turing and rules ([Wittgenstein, 1980, §1096]):

> Turing's 'machines'. These machines are humans who calculate. And one might express what he says also in the form of games. And the interesting games would be such as brought one via certain rules to nonsensical instructions. I am thinking of games like the "racing game". One has received the order "Go on the same way" when this makes no sense, say because one has got into a circle. For that order makes sense only in certain positions. (Watson.)
>
> A variant of Cantor's diagonal proof:
>
> Let $N = F(k, n)$ be the form of the law for the development of decimal fractions. N is the nth decimal place of the kth development. The diagonal law then is $N = F(n, n) =$ Def $F'(n)$. To prove that $F'(n)$ cannot be one of the rules $F(k, n)$.
>
> Assume it is the 100th. Then the formation rule of $F'(1)$ runs $F(1, 1)$, of $F'(2)$ $F(2, 2)$ etc. But the rule for the formation of the 100th place of $F'(n)$ will run $F(100, 100)$; that is, it tells us only that the hundredth place is supposed to be equal to itself, and so for $n = 100$ it is *not* a rule.
>
> [I have namely always had the feeling that the Cantor proof did two things, while appearing to do only one.]
>
> The rule of the game runs "Do the same as . . ."—and in the special case it becomes "Do the same as you are doing".

This intensional, "rule" version of Turing's undecidability argument showing that the diagonal rule cannot be among the listed rules[13] corroborates Kreisel's footnote.

2 Middle Kreisel

In 1953, Wittgenstein's literary executors Elizabeth Anscombe and Rush Rhees published *Philosophical Investigations* [1953], what would become Wittgenstein's main legacy, out of manuscripts intended for publication. In 1956, the executors and G.H. von Wright published *Remarks on the Foundations of Mathematics* [1956], out of sporadic, working manuscripts from 1937-1944. And in 1958, Rush Rhees published *The Blue and Brown Books* [1958], two crafted texts from 1933-1935 sparsely circulated but never intended for publication. Kreisel, well into his career publishing five papers a year in mathematical logic and having met Gödel in Princeton, took it upon himself to provide extensive reviews of both the 1956 and 1958 publications. Let us proceed to this "middle" Kreisel with respect to Wittgenstein. Beyond our focus on Kreisel, it is of interest to take account of these reviews as part of the initial reception of Wittgenstein's works, especially in light of the considerable scholarship now attendant to this corpus.

Kreisel in his review [1958b] of the *Remarks on the Foundations of Mathematics* (RFM) took the compilation as presenting Wittgenstein's philosophy of mathematics, and contributed to setting a negative tone for its interpretation for quite some time. It is to be remembered, first of all, that RFM consists of unpolished, ruminating remarks never intended for publication and exhibit an

[13] [Floyd, 2012] calls this "Wittgenstein's diagonal argument" and analyzes it in great detail with respect to Turing's 1936 paper.

evolution of thought and focus. Something of this as well as residual positivities for Kreisel were conveyed by him at the end of his review in a "Personal Note", which reveals an anxiety of influence:

> I knew Wittgenstein from 1942 to his death. We spent a lot of time together talking about the foundations of mathematics, at a stage when I had read nothing on it other than the usual *Schundliteratur*. I realise now from this book that the topics raised were far from the center of his interest though he never let me suspect it.
>
> What remains to me of the agreeable illusions produced by the discussions of this period is, perhaps, this: every significant piece of mathematics has a solid mathematical core (p.142, 16), and if we look honestly we shall see it. That is why Hilbert-Bernays vol. II, and particularly Herbrand's theorem satisfied me: it separates out the combinatorial (quantifier-free) part of a proof (in predicate logic) which is specific to the particular case, from the 'logical' steps at the end. Certain interpretations of arithmetic and analysis have a similar appeal for me. I realise that there are other points of view, but for the branches of mathematics just mentioned, I still see the mathematical core in the combinatorial or constructive aspect of the proof.
>
> I did not enjoy reading the present book. Of course I do not know what I should have thought of it fifteen years ago; now it seems to be a surprisingly insignificant product of a sparkling mind.

Whether Kreisel was personally miffed or not, Wittgenstein scholarship has shown that Wittgenstein often did not discuss directly with students and others at the time what was at "the center of his interest". The "agreeable illusions" is chiasmatic, as Kreisel by this time had incisively pursued "the combinatorial (quantifier free) part of a proof" in [1951, 1952a] and moreover had shifted the focus of consistency proofs onto such parts in [1958a].

As to the concluding "insignificant product of a sparkling mind", this would become quoted, but evidently the "product" is the literary executors', concocted out of varying working manuscripts.

Kreisel begins his review by discussing Wittgenstein's "general philosophy" as a sophisticated empiricism sensitive to the ways of language. Kreisel considers Wittgenstein's starting point to be (p.138): "he is not prepared to use the notions of mathematical object and mathematical truth as tools in philosophy." But Kreisel does not consider as convincing Wittgenstein's arguments against them, and (p.137) "his reduction to rules of language". For Kreisel, "the real objection to these notions is that, at any rate as far as I know, there does not exist a single significant development in philosophy based on them." With this pragmatic pronouncement, he simply skirts the depths of Wittgenstein's grapplings in RFM with the objectivity of rule-following. Kreisel's only allusion to this is in a footnote (p.138):

> ...it should be noted that Wittgenstein argues against a notion of mathematical object (presumably: substance), ...but, at least in places ...not against the objectivity of mathematics, through his recognition of formal facts.

Having ferreted this out of Wittgenstein, Kreisel himself would later become known for the dictum, "the objectivity of mathematics over the existence of mathematical objects".[14]

Kreisel next gets to Wittgenstein on proof. While a large part of RFM is devoted to aspects of proof, Kreisel here focuses on proof as related to theorem and, later in the review, on the equivalence of proofs (see below). Kreisel takes up as two themes that "A theorem is a rule of language and the proof tells us how to use the rule", and "The meaning of a theorem is determined only after the proof".[15] Kreisel discusses the various ways Wittgenstein approaches these themes at some length, but then deliberately reverses proof and theorem (p.140):

> Quite generally, it is simply not true that proof is primary and theorem derived, that only the proof determines the content of a theorem. In fact, Wittgenstein is wrong in saying that generally we change our way of looking at a theorem during the proof (p.122, 30), but equally often we change our way of looking at the proof as a result of restating the theorem; ...

Kreisel will maintain this in his thinking as a *chiasmus*, elaborated with examples, but one can see it as a sort of surface reversal which can be subsumed into the greater depths of Wittgenstein's thinking.

First and foremost, Wittgenstein in RFM is seeing mathematics as a multifarious edifice of procedures and conceptual constructions, one for which proofs and methods of deduction as embedded in practice are crucial. Kreisel, in flattening the situation to a dichotomy between proof and theorem, and then shifting the weight back to theorem, eschews the complexity of interplay and moreover actually reinforces the importance of argument and construction. While Wittgenstein emphasizes how a proof accrues to the meaning of a theorem both by newly delineating its interplay of concepts and by providing procedural means for its further application, Kreisel emphasizes that (p.141) "*a theorem becomes an assertion about the actual structure of its own proof*"—which while focusing on theorem is in line with Wittgenstein's thinking. Kreisel's other way of shifting from proof to theorem is to emphasize that a proof yields new theorems, e.g. about structures.[16] Again, this is in accord with Wittgenstein's thinking, according to which a proof as procedure and becoming method is autonomous and would prove perforce various theorems.

Second, Kreisel continues from the above displayed passage with (p.140):

> e.g. if we are accustomed to the principle of proof that the totality of all subsets of a set is itself a set, we may reject it when it is pointed out to us that it is

[14]For example, [Putnam, 1975, p.70]: "The question of realism, as Kreisel long ago put it, is the question of the objectivity of mathematics and not the question of the existence of mathematical objects."

[15]Kreisel (p.136) refers to RFM II §39 for the first and RFM II §31 and III §30 for the second.

[16] Even much later, [Kreisel, 1983a, p.297] supports this versus Wittgenstein though with an oddly drawn example: "A caveman conjectures that $a^2 - b^2 = (a + b)(a - b)$ is valid for all even integers. Of course he is right. But the proof shows that the theorem has nothing to do with the distinctions of even and odd, integer or fraction. Therefore one formulates (the more general theorem for *arbitrary commutative rings*. This notion is determined by those few properties of the even integers which enter in the proof of $a^2 = b^2 = (a + b)(a - b)$. The more general theorem is more appropriate to the proof; in short: it is more meaningful."

> only valid for the notion of a combinatorial set and not, e.g. for the notion of a set as a rule of construction.

Pursuant of this—or with it as an anticipation—Kreisel in a later, critical part of the review, on "Higher Mathematics", writes (p.153): "Wittgenstein says (p.58, 6) that it was the diagonal argument which gave sense to the assertion that the set of all sequences (of natural numbers) is not enumerable." After describing the diagonal argument and posing it as a "definition" of non-enumerability, Kreisel then wrote (p.153): "What is wrong here? Well, after all there was a paradox, Skolem's paradox, which puzzled people. The mistake is to think that the diagonal argument applies *only* to the set of all sequences ..." Kreisel's allusion to Skolem's paradox, in purported line with the above displayed quote, is a local *chiasmus* in itself—about proof, theorem, and now the set of all sequences. Contrary to what Kreisel said about the diagonal argument being applicable in only one situation, Wittgenstein on the cited page had ruminated about "the diagonal procedure" in its various aspects, and wrote, rather, that "it gives sense to the mathematical proposition that the number so-and-so is different from all those of the system". A few pages earlier (pp.55f), he had discussed the diagonal procedure as a method, e.g. of transcending the algebraic numbers, and had expressed skepticism about the "idea" that the real numbers are not enumerable. Kreisel's simple gloss is seen to be overshadowed by Wittgenstein's wide-ranging remarks on the diagonal procedure as proof.[17]

The rest, and most, of the review concerns the "philosophy of mathematics". Kreisel had taken as Wittgenstein's conclusion in "general philosophy" (p.137):

> He regarded the traditional aims of philosophy, in particular of crude empiricism, as unattainable. He objected to a mathematical foundation of mathematics because the concepts used in the foundation are not sufficiently different from the [mathematical] concepts described (p. 171, 13) and, he thought (p. 177) that there are no mathematical solutions to his problems. He said the aim of a philosophy of mathematics should consist in a clarification of its grammar ...

For Kreisel, (p.143) "I do not accept his conclusions since I do no think that they are fruitful for further research." Again a pragmatic pronouncement, and after rejecting on these grounds Wittgenstein's main thrust, the "clarification of its grammar" as a matter of mathematical activity, Kreisel proceeds, over several pages, to counter Wittgenstein's negativity about foundations with the fruitfulness of contemporary investigations of set theory and of constructivity. On the latter, Kreisel is discerning about the differences between intuitionism and finitism, and here he does take Wittgenstein as making contributions to finitist investigations.

Having cast light from his direction on foundations, Kreisel in the concluding pages of the review returns to proof—the focus of Wittgenstein's "foundational"

[17]Notably, Kreisel in his next review [1960], to be discussed below, went to the extent of providing a "Correction" to the present review, allowing that Wittgenstein's (p.251) "remarks can be given a little more sense if an intensional notion of function (*rule* of calculation) is considered", and then giving three viable meanings of "enumeration". This resonates with how Wittgenstein was exploring the use of the diagonal argument and [Kreisel, 1950, n.4] as discussed at the end of §1; we will return to this at the end of §3, about a *mea culpa*.

concerns—as newly to be considered in the wider context. While Kreisel had earlier chiasmatically shifted the weight to the range of theorems that a proof can prove, he gets here to the range of proofs and Wittgenstein's interest in characterizing the equivalence of proofs and how they might be compared. Kreisel writes that Wittgenstein (p.151) "does attempt to find a characterization of a very general sort by basing a comparison of proofs on the application, or, as he puts it (p. 155, 46) on what I can do with it." Though he finds limitations to this, Kreisel in support raises non-constructive existence proofs and "what we can do" with them—which is allusive to his own researches along these lines and their inspiration in his early conversations with Wittgenstein. Wittgenstein in RFM had ruminated over Gödel's proof of the incompleteness theorem, mainly about its ostensible play with truth, provability, and consistency. Taking his arguments as "wild", Kreisel strikes a positive path through Gödel's arithmetization-of-syntax argument, delineating that (p.154) "all that one needs of the concept of truth is $\mathcal{R}$ or $\neg\mathcal{R}$." Wittgenstein, generally, raised issues around consistency as a formal concept, with respect to proofs and contradictions. Kreisel insisted on the fruitfulness, writing (p.156): "proofs of consistency and, more generally, of independence yield, perhaps, a better control over a calculus than anything else."

In his review [1958b] of RFM, aside from taking up objectivity vs. objects Kreisel mainly addressed what he regarded as challenges posed by the text concerning proof and foundations of mathematics as per meaning and knowledge. Variously flattening aspects, he set out contrasting viewpoints of proof vs. theorem, of the fruitfulness of foundational investigations, and even of specifics of the diagonal argument, the incompleteness theorem, and consistency. In this, he elaborated and promoted constructive aspects of proof.

Kreisel's review [1960] of *The Blue and Brown Books* can be seen as complementary, in that the text deals more centrally with language, and so what should be addressed is set in the seas of language rather than the precisification of mathematics. It will be remembered (cf. §1) that Wittgenstein lent Kreisel a copy of *The Blue Book* in the summer of 1942. The books first advance the method that would serve to buttress the mature *Philosophical Investigations*. In brief, Wittgenstein heralds the notion of a "language game" to shed light on the foundations of logic: the method utilizes simplified snapshots of portions of human language use to clarify meaning, understanding, and thinking. For concepts and categories, there is an exploration of the limits of reductive possibility, to be seen in the plasticities of language. For Kreisel, (p.240) "... quite natural developments of Wittgenstein's considerations may be formulated as a *reduction to the concrete*; for want of a better term I shall call it semi-behaviourism (with respect to mental acts) or semi-nomimalism (with respect to abstract objects)." This encapsulated interpretation is what Kreisel will discursively discuss in the review, and at the end of the review is a telling summary (p.251):

> As to content, the ideas of the book seem to be most relevant to the discipline which studies what is concrete (and whose exact delineation is yet to be evolved). On the positive side there are descriptions of little noticed phenomena

> (phenomenology) and reductions to concrete terms of many situations that are in the first place viewed abstractly. As described above a wider sense of 'reduction' is appropriate than is used in crude positivism or nominalism. This work shows convincingly a natural tendency of being unnecessarily abstract. On the negative side, we have Wittgenstein's theoretical positions; on analysis, there are seen to be cogent consequences of philosophical doctrines, which, roughly speaking, overestimate what can be done in concrete terms. Since the former seem to be easily refuted they are used in *reductio ad absurdum* arguments applied to the latter.
>
> As an introduction to the significant problems [of] traditional philosophy the books are deplorable.[2]
>
> [2]This is largely based on a personal reaction. I believe that early contact with Wittgenstein's outlook has hindered rather than helped me to establish a fruitful perspective on philosophy as a discipline in its own right, and not merely for example as methodology of highly developed sciences. . . .

The last sentence and its footnote are darker still than what Kreisel had written in that "Personal Note" at the end of the RFM review [1958b], quoted at the beginning of this section.

In the body of the review, Kreisel rounds out his contentions about Wittgenstein's "reduction to the concrete" with (p.240) "some illustrations taken from the philosophy of mathematics." At first, Kreisel is broadly affirmative about how Wittgenstein describes (p.241) "often surprisingly successfully, situations which are normally considered to involve just those mental acts and abstract objects which he eliminates." Kreisel relates this to how (p.242) "detailed investigations in the foundations of mathematics"—of which he writes tellingly in a footnote "My own in this direction have certainly been influenced by the view of Wittgenstein's work here described"—"have revealed a similar situation with respect to a nominalist (finitist, or, more generally, predicative) elimination of such abstract objects as the totality of natural numbers or of functions." Kreisel points out how for a wide class of proofs Herbrand's theorem provides "an elimination in a quite precise and natural sense" and similarly, "in a large part of analysis, quantification over all real numbers can be eliminated". Concluding about Wittgenstein's "practice of philosophy", (p.242) "Both his examples and the studies in the foundations of mathematics show clearly that *we have a general tendency to describe language* and, in particular, mathematical practice, *by means of concepts whose level of abstraction is higher than the minimum actually needed.*"

In the extended Remark following, Kreisel significantly pulls back by suggesting that what he had earlier written (p.243) "may be too logically biased and even altogether pragmatic." Instead, "we may look at these books, particularly *The Brown Book*, as a contribution to the study of *what is concrete, of what is (immediately)* given." On this he brings in (p.243) "the theoretical question of the existence of sense-data" and Wittgenstein's "seeing X as Y". With the latter, Kreisel is astute enough to bring out something that would be central to Wittgenstein's later thinking, though by calling them "phenomenological studies" he diminishes their logical import. Kreisel pronounces (pp.243f): "though even in his later book *Philosophical Investigations* these phenomenological studies have not gone far enough to establish a discipline, the later work is incomparably better in this respect than the books under review."

Proceeding, Kreisel next considers Wittgenstein's "theoretical positions", which he takes to be (p.244):

> ... (i) negative assertions on what cannot be said (or: is not), such as what is common or essential to those cases which he describes as families of concepts, (ii) assertions on what should be accepted as a decisive criterion (equality or difference in) meaning, such as the actual use of a term, (iii) the identification of metaphysical distinctions with grammatical ones.

Addressing (i), Kreisel takes Wittgenstein as objecting (p.244) "(a) generally, to the introduction of an (abstract) object common to all instances of a general term, (b) to the assumption that a general term always corresponds to a (single?—presumably: well-defined) property." Addressing these, Kreisel again resorts to mathematical illustrations. For (a), he points out that properties of rotations in the plane and multiplication of complex numbers can be commonly derived from the group axioms, and while there is a distinction in the two applications (p.245) "it's a distinction without a difference" and "the distinction is not vivid". For (b), Kreisel alludes to "mechanical procedure" à la Turing, and notes that (p.246) "It seems very natural that one is not instantaneously convinced of correct characterisations even if the arguments are good on reflection." Finally, as to what is essential to a concept, Kreisel points to the great deal of clarity gained "by the rather surprising discovery that relatively few abstract structures were essential to the proofs in the greater part of current mathematics."

By remaining in the concrete and curtailed formulations of mathematics, Kreisel is reducing away from Wittgenstein's main thrust in *The Blue Book* about the contexts and ostensible workings of language and meaning. Wittgenstein [1958, p.17], discussing "our craving for generality", pointed out "We are inclined to think that there must be something in common to all games, say, and this common property is the justification for applying the general term 'game' to the various games; whereas games form a *family* the members of which have family likenesses", these overlapping in various ways. In a different direction (p.18), of "the man who has learnt to understand a general term, say, the term 'leaf'," "We say that he sees what is in common to all these leaves; and this is true if we mean what he can on being asked tell us certain features or properties which they have in common. But we are inclined to think that the general idea of a leaf is something like a visual image, but one which only contains what is common to all leaves", there being no such visual image. Finally, Wittgenstein somewhat anticipates the analytical and reductive approach that Kreisel is taking, with (p.18):

> Philosophers constantly see the method of science before their eyes, and are irresistibly tempted to ask and answer questions in the way science does. This tendency is the real source of metaphysics, and leads the philosopher into complete darkness. I want to say here that it can never be our job to reduce anything to anything, or to explain anything. Philosophy really *is* 'purely descriptive'.

Addressing (ii) of the penultimate displayed quote, Kreisel continues to take a reductive, scientific approach (p.247): "As far as *actual use* of words is concerned," it "may refer to the words spoken" or "it may also mean the *real* role of the word (as Wittgenstein puts it) undistorted by the vagaries of linguistic expression." It will become increasingly understood that Wittgenstein generally meant, rather, the use in a broad sense in our ordinary language. More attendant to the "real role", Kreisel opines "... in the cases of the eliminations of abstract terms ... there seems no doubt about the actual use ... "But in other cases the whole problem is thrown back to what is conceived as the real role"—on this referring to his discussion of "non-constructive" in the foundations of mathematics in his RFM review.

Addressing (iii), Kreisel first recalls that in his RFM review, he (p.247) "also questioned the value of the 'reduction' of metaphysics to grammar." Here, he refers to "syntactic" and "truth under the given interpretation" in mathematical logic, and opines, "I see no evidence that the grammatical distinctions which are to replace (problematic) metaphysical ones, are going to be described by means of less problematic concepts." This *is* a valid point, especially in answer to the temptation to take schematic formalization as elucidation of the large domains of truth and language. For Kreisel: "... the reference to grammar is deceptive for two additional reasons: First, ... one does not usually consider such questions as 'what is a noun' in a theoretical way .... Second, while it is apt to speak of a grammatical role of a word in a language, the difficulty of formulating this seems to be of an entirely different order from school grammar ..." Wittgenstein's use of the term "grammar" may indeed be deceptive at first, but it will become increasingly understood that he was taking it not as some sort of syntactic classification, but rather a tying of meaning to rules, of uses of general semantic types as these are correlated with syntactic categories in utterance and use.

Stepping back, one can fairly get the feeling that Kreisel in his review did not come to terms with Wittgenstein's frontal engagement with language and meaning. The *Philosophical Investigations* had come out in 1953, and the editor of *The Blue and Brown Books* had subtitled it *Preliminary Studies for the 'Philosophical Investigations'*. Nonetheless, Kreisel insisted on pursuing a path akin to the one taken in his RFM review, of making reductive logical pronouncements and alluding to logical-mathematical examples—managing, along the way, to make positive remarks about the elimination of abstract objects e.g. through Herbrand's theorem. In the large, Wittgenstein had begun to explore the seas of language, its waves to and fro, when reduction does not work to get at meaning.

3 Later Kreisel

In the fullness of time, after having pursued and stimulated avenues of research in constructive mathematics and proof theory and having had a substantive engagement with Gödel, Kreisel in several publications came again to engage with the words and ways of Wittgenstein. Latterly meditating on these in dialectical interplay with his own work and experience, Kreisel exhibited in style and tone a new, if commemorative, acknowledgement. This "later" Kreisel we pursue through his publications in chronological order, now to the further purpose of setting out his latter-day evolving thinking about logic and mathematics.

[Kreisel, 1976a], "Der unheilvolle Einbruch der Logik in die Mathematik", appeared among a collection of essays on Wittgenstein in honor of G.H. von Wright. The title is from *Remarks on the Foundations of Mathematics* IV 24, "The disastrous invasion of logic into mathematics". Kreisel takes this up as a theme of RFM—this in itself evincing a new positivity about that work—and proceeds to articulate his own thinking along these lines in light of contemporary developments.

Kreisel at the beginning cogently summarizes his line of thought (p.166):

> The aspects (of proofs and rules) which are regarded as basic in (1) current—somewhat pretentious—*logic*, are not only *different* from those which are essential in (2) current *mathematical practice* (which almost goes without saying), but actually *harmful* for a study of (2). The reason is that those basic questions of 'principle', concerning the validity of principles of proof and definition, appear more glamorous than the genuinely useful problems concerning current mathematical practice, and thereby divert attention from the latter. The 'practice' referred to in (2) includes not only applications inside or outside mathematics, but also facts of experience concerning mathematical reasoning: which (combinatorial) configurations and (abstract) ideas we handle easily.

Then setting out toward elaboration, Kreisel instinctively retrenches (p.168): "...at least in my own case, the quotation has not been of direct, not even of heuristic use. I have known it for nearly 20 years, and stressed its plausibility in my—otherwise rather negative—review of RFM. The brutal fact is that the quotation does not contain the remotest hint of how (the pretentious) logical analysis is to be replaced, that is *which concepts should be used in the analysis of proofs in the place of the 'basic' concepts of proof theory* and which questions should be asked in place of the 'principal' problems of proof theory ..." But later, "...the *value of Wittgenstein's quotation* (for me) can perhaps be summarized as follows: It is incisive and memorable, and so makes the reader familiar with a certain aim. If sometime later this aim is approximated, the reader is likely to take a closer look instead of moving on, breathlessly, to the next 'interesting' possibility."

Focusing on proofs and rules, Kreisel begins with the stark (p.169): "Proof theory is, in my opinion, a particularly crass example of that pretentious logic which was mentioned in the summary of this article ... The claims of proof theory to have uncovered the true, in particular, formal nature of mathematical reasoning surpass in pretentiousness the claims of most traditional philosophers." This is a bit of *chiasmus*, a reversal toward Wittgenstein, in that Kreisel had himself proceeded in collaborative work in proof theory during this period with something of such "claims" as incentive.

Be that as it may, according to Kreisel, "Wittgenstein's critique of proof theory and its principal problems (for example in the *Remarks*) is wildly exaggerated, and therefore quite unconvincing." (p.170) "Worse still, Wittgenstein's own attempts to characterize what is essential in proofs aren't much better (than Hilbert's)." First, he "stresses that proofs *create*—or at least use!—*new concepts*." Yet "the brutal fact remains that, somewhere or other, *propositions concerning these new concepts have to be proved too*." And second, he stressed that "proofs must be graspable and memorable ... and visualizable if we mean literal seeing of some spatio-temporal configuration ... " "But all this is clearly secondary, as long as there are (genuine) doubts about the *principles of proof* that are used."

On this last, Kreisel makes an autobiographical remark revealing something of influence. He had a "long hesitation before studying the idea of simplicity or 'graspability' (*Übersehrbarkeit*) of proofs" (pp.173f):

> I just wasn't confident about finding a sensible measure in any direct way. First, I tried my hand at analyzing *simplicity* of principles of proof ..., by means of socalled autonomous progressions. Granted that these attempts were pretty faithful to the intended meaning, I soon came to this conviction: if the analyses are (even only) approximately right then those intended principles are just of little intrinsic interest ... So instead I went back to more traditional questions about proofs, in particular, infinite proofs in intelligently chosen languages with infinitely long expressions, and, above all, intuitionistic logic. ... What I overlooked was the witless way in which proofs entered! No recondite properties of proofs were involved, no relations between proofs or between proofs and other objects, nothing except their 'logical' aspects which occurs to us without any experience in mathematics at all! In short, nothing but the hackneyed business: The proofs establishes its conclusion (in particular a logic-free conclusion in the intuitionistic case).

Kreisel continued (p.174): "But, at last, I had become ... convinced that questions of validity are by no means theoretically senseless ... but that they are unrewarding at the present time." "At this stage it was natural to move so to speak to an opposite extreme, in particular, opposite to Hilbert's proof theory: I went about looking for methods of proof and properties of proof which are *trivial for proof theory*, but *essential for mathematical practice* ... to be analyzed by appropriate mathematical measures of *complexity*."

On this, Kreisel gives two extended examples, the first being *explicit definitions* (p.174):[18]

> ... we think of explicit definitions as *introducing* new concepts, the definition being usually supplemented by a list of properties (of the new concept), which are proved by the use of the explicit definition. As is well-known, this way of introducing a new concept is trivial for Hilbert's proof theory, because such concepts are in an obvious way *eliminable*. On the other hand, for mathematical practice they are not only useful, but as it were typical—at least for modern mathematics, which is dominated by the *axiomatic method*. This proceeds as follows. A structure is defined explicitly in set theoretic or number theoretic terms, and then is shown to be, say, a unitary group: the axioms for unitary groups then constitute the supplementary 'list of properties' (of the structure or concept) mentioned above. The choice of such properties—or, as one says—of the proper *cadre*—is often the key to solve mathematical problems.

[18] [Kreisel, 1977] elaborates along some lines, and in particular has a longer subsuming account (pp.120ff) of explicit definitions.

For Kreisel, his student Richard Statman in his 1974 dissertation made (p.175) "impressive progress by means of a suitable measure of complexity which is *relevant in a large number of cases*, in particular, for analyzing the role of explicit definitions."

The second example (p.177) "concerns a more subtle 'invasion' by logic, namely a somewhat exaggerated idea of the role of socalled logical languages, for example, of predicate logic of first order", the exaggeration to be considered concerning "the ideal form of a (mathematical) proposition". On this, Kreisel focuses on real closed fields. After mentioning Sturm's work on determining the number of zeros of a polynomial in an interval and noting that *effective* decisions can be made when the coefficients are algebraic,[19] Kreisel thence brought in, of course, Tarski and the decidability of the first-order theory of real closed fields as a generalization. On this though, Kreisel opined (p.178) "The trouble began when people started to get interested in the efficiency of decision procedures ...", and "... assumed that the 'ideal form' of 'the' decision problem for real closed ordered fields should deal with all formulas of the first-order language (of fields). They found so-called *upper and lower* bounds, namely $2^{2^{cn}}$ and 2^{cn} respectively, where n is the length of the formula." (p.179) "... the most obvious conclusion from the lower bounds is simply this: the full first-order language is *not* appropriate! And one would look for a *subclass* of that language [that has] a truly efficient procedure ...". In the contemporaneous [1976b], Kreisel made proposals along these lines, and in [1982] worked out details for an application of Herbrand's Theorem for Σ_2 formulas.

After discussing related issues in budding computer science, Kreisel wraps up with "questions of 'principle' " and (p.186):

> I find it hard to have confidence in our whole 'critical' philosophical tradition, with it paradoxes, its dramatic claims either to see profound errors in our ordinary views or profound misconceptions in 2000 year old questions. It all sounds like a paranoid's paradise, and forgets the most striking fact of intellectual experience: how our thoughts seem to adapt themselves to the objects concerned, as we study them and get familiar with them (in a detached way) and how, with this familiarity comes the judgment need to distinguish between plausible and implausible theories, substantial and superficial contributions.

After having elaborated in his own way about "the disastrous invasion of logic into mathematics", Kreisel here seems to come around to Wittgenstein's grounding faith in familiarity and the importance of our adaptability in coming to judgment.

[Kreisel, 1978b], "Lectures on the Foundations of Mathematics", is ostensibly a review of the compilation [Wittgenstein, 1976] of lectures notes for 1939 lectures put together by Cora Diamond. As if setting the stage, Kreisel quickly sketched Wittgenstein's progress *ab initio*, mentioning that (pp.98f) "W found that quite elementary mathematics provided excellent illustrations of weaknesses

[19]Notably, this harkens back to that 1942 conversation with Wittgenstein (cf. §1) on "constructive content" of the Intermediate Value Theorem and Kreisel's effectivization in [Kreisel, 1952a].

of traditional foundations, t.f. for short"—this incidentally setting a contrastive, positive tone from his RFM review.[20] But then, Kreisel shifts the purpose (p.99): "The main aim of this review is to restate the complaints of W and Bourbaki about t.f., with due regard for the discoveries of mathematical logic . . . By and large, at least in the reviewer's view, the discoveries of logic support the principal complaints." With this Kreisel virtually ignores [Wittgenstein, 1976], writing of it dismissively that (n.[2]) it "does not even record what W said in the lectures, but what a bunch of students thought he had said", and referring to it only on one page (p.107).

Kreisel takes the principal target of W and Bourbaki to be (p.99) "the *formal-deductive presentation* of mathematics in a *universal system*". But while "Bourbaki simply record their impression (of set-theoretic foundations)", Kreisel writes of Wittgenstein that (pp.99f) ". . . W attempts to convert fundamentalists by 'deflating' the notions and thus the so-called fundamental problems of t.f., stated in terms of those notions. In W's words, he wants to *show the fly the way out of the fly bottle*. He does this with much ingenuity and patience, and some overkill."

Proceeding to "complaints", Kreisel gets to (p.101): ". . . the general complaint (of W and Bourbaki) is that t.f. may be *poor philosophy*, in the broader popular sense of 'philosophy', specifically, if in practice the general aims of foundations are better served by alternatives, for example, by ordinary careful scientific research and exposition." Taking as "principal complaint: better current ideas than t.f.", Kreisel discusses how both W and Bourbaki emphasize that "the *choice of explicit definitions* is incomparably more significant than the glamorous preoccupations of t.f., not only for discovery, a 'mathematical' affair, but also for intelligibility, a principal factor in reliability."[21] Finally, Kreisel addresses (p.102) "specific complaints about some glamor issues of t.f.". The first is "the matter of *contradictions* as in the paradoxes, or their absence, consistency, as in Hilbert's program." "W had a particularly strong aversion", whereas "at least by implication, Bourbaki was unimpressed". The second example is "higher (infinite) cardinals". As in his RFM review, Kreisel connects this to the diagonal construction, but now mentions favorably how Wittgenstein "preferred to use the construction in the context of rules", recalling Wittgenstein's formulation as given in [Kreisel, 1950, n.4].[22]

Throughout Kreisel's discussion of "complaints", there is in contrast to his RFM review a softer attitude toward Wittgenstein. This continues into Kreisel's acknowlegement (p.103) of "W's advice"—what mainly he draws from the book ostensibly under review—that "when confronted . . . by a philosophical problem about (mathematical) notions or proofs, we should see what we *do* with them,

[20]cf. §1.

[21][Kreisel, 1976a] elaborated on explicit definitions, as described in our account of it above. Significantly, Kreisel there wrote (p.175): "As far as I know, Wittgenstein himself never stressed the role of explicit definitions particularly." Now, he is accrediting to Wittgenstein how he stressed the choice of explicit definitions.

[22]cf. end of §1.

how we *use* them."[23] Kreisel concludes his "review" by "balancing the account on the positive side of t.f." He opined that to Wittgenstein the weaknesses of t.f. mattered less than the (pretentious) style, but proceeded to set out several examples—two from Gödel—for how such stylistic urgings may signal possibilities for progress.

[Kreisel, 1978a], "The motto of 'Philosophical Investigations' and the philosophy of proofs and rules," ostensibly takes up that motto, "All progress looks bigger than it is" interpreted as (p.13) "the ratio of *actual progress* (as judged by mature reflection) to *apparent progress* (measured by expectations after a few initial successes) is generally poor." With this as underlying thrust, Kreisel proceeds to elaborate on Wittgenstein's "family resemblances of concepts" and "principal pedagogic aim for philosophy", and discusses, in an extended appendix, "proofs and rules" to draw in recent logical experience. With this, Kreisel hovers closest to Wittgenstein's major work, *Philosophical Investigations*.

Kreisel starts by laying some groundwork about (p.15) "General features of traditional philosophy, and some of their implications": (a) "...traditional notions occur to us when we know very little." "...when we know very little, we tend to see superficial, abstract features of objects. And when we do see specific features we often cannot say very well—cannot 'define' in familiar terms—what we see." (b) "When we know very little, the main intellectual tools available are a sense of coherence and, more generally, introspection." (c) "When we know very little compared to the scope of a question, we are often bad at guessing even remotely the methods needed for a satisfactory answer though often we recognize such an answer immediately when we see one." Kreisel peppers (b) and (c) with historical examples involving Galileo, Plato, Aristotle, Newton, and Cauchy.

Kreisel then focuses on (p.17) "Family resemblances of concepts", and, in connection, "the discovery of definitions". It will be remembered[24] that in 1942 Wittgenstein lent young Kreisel a copy of *The Blue Book*, and that, remarkably, Kreisel only reacted about family resemblances, and that with reference to the group concept. Here too, of the many themes of *Philosophical Investigations*, Kreisel concentrates on family resemblances, now with a remarkably literal twist (p.19): "As I see it (now), Wittgenstein's slogan of 'family resemblances' reminds one of a class of phenomena where the limitations of the traditional style are exceptionally vivid, and hence instructive. I mean the phenomena of *literal* family resemblances, say of the Hapsburgs or the Bourbons [sic]. What can we realistically expect from any definition of such a family resemblance, say, in the style of analytical philosophy?" This focus on literal family resemblances amounts to a local *chiasmus* moving in reverse to Wittgenstein's conceptualization of aspectual similarities and analogues. The tenor of *Philosophical Investigations* is to pursue aspects and work against definitions

[23] This recalls Wittgenstein's attempt to compare proofs according to "what we can do with them" in RFM, as already discussed by Kreisel in his RFM review (cf. §1).
[24] cf. §1.

of family resemblances in terms of biological causes or necessary and sufficient conditions. Be that as it may, Kreisel proceeds to several points (p.19):

(a) "The first thing to expect is, probably, a *genuine theory* of literal family resemblances or some kind of practical mastery. As appears almost certain now, molecular biology is the appropriate tool here." With this scientific coordination of a question emerging "when we know very little", and especially with Kreisel soon following up with "We cannot expect to find a common element in ordinary experience", one can see Kreisel as proceeding orthogonally to Wittgenstein by looking for a genetic reduction. (b) "A second use to expect from a definition would be for the study of our actual *process of recognizing a family resemblance.* At least here, Kreisel is sensing the importance of what Wittgenstein wrote of as "seeing as" and "the dawning of an aspect". (c) (p.21) "An imaginative (clever) definition in this style, in terms of familiar things, may well be useful for *stimulating*—not the actual process of recognition of family of resemblances, but some of its useful results." Again taking a scientific approach, Kreisel mentions as an example of this kind of stimulation "*logical validity* in terms of *derivability*, say by Frege's rules".

Lastly, Kreisel attends to what he terms (p.22) "intimate pedagogy", what he took to be (p.14) "Wittgenstein's principal pedagogic aim for philosophy". Kreisel takes a particular tack (p.22): "Suppose we have come to the conclusion that some given notion, for example, one of those grand traditional notions, has to do with a family resemblance .... Of course, we do not assume that such a conclusion, even if sound, can be conveyed convincingly, especially to individuals with very limited experience. We ask the pedagogic question: What can be done?" Emphasizing the need for discretion—not to make "grand" claims—and that "precise formalization" can be instructive, Kreisel proceeds to two examples, Tarski's truth definitions and Gödel's incompleteness theorem. Of the latter (p.24) "It unquestionably refutes the idea that, in mathematics, abstract notions are merely used as a *façon de parler.* Hilbert expressed this idea explicitly and precisely in his consistency programme. A more direct formulation of the idea, which is equally easy to make precise, is that a proof by use of abstract notions of a theorem stated in elementary form, can be straightforwardly converted into an elementary proof." Incidentally, Kreisel soon wrote revealingly (p.25): "*Digression* for readers who have seen my (constipated and fumbling) review in [[Kreisel, 1958b]] of *Remarks on the Foundations of Mathematics.* To me the single most disturbing (and most surprising) defect of those *Remarks* was and remains Wittgenstein's own fumbling."

As in his other articles concerning Wittgenstein, Kreisel insists on taking a scientific approach, and here, in an extended appendix, he further focuses on logic and mathematics to draw subtle distinctions about proofs and rules that round out his contentions. In particular, he has (p.27) "the novel twist of using notions from Brouwer's intuitionist foundations to examine a natural analogue of Church's thesis." While having taken on the motto from the *Philosophical Investigations*, Kreisel interestingly and chiasmatically proceeds away from its broad concerns towards the nuances of progress in logic and mathematics.

[Kreisel, 1983a], "Einige Erläuterungen zu Wittgensteins Kummer mit Hilbert und Gödel", starts out "I was very astonished by the *Remarks on the Foundations of Mathematics* when they came out, especially by those on Gödel's incompleteness theorems, for reasons that I can state precisely only now ... " The article, in a recapitulative way, engages Wittgenstein's views on consistency and incompleteness with a palatably seasoned appreciation.

Initially, Kreisel adopts and adapts Wittgenstein's (p.296) "proofs easy to take in and remember". In RFM, Wittgenstein had importantly discussed how mathematical proofs are to be "easy to take in and remember [*überschaubar und einprägsam*]" and "perspicuous [*übersichtlich*]". Kreisel declares that "... one of the main concerns of mathematics is to provide general guidelines for proofs to be easy to take in and remember." The guidelines are for what he analyzes into two parts as follows: "For usually one starts from a long, opaque proof and dissects it—with intuition—into a few lemmas, that is to say into a structure *easy to take in.* In this process one tries to formulate (or, if necessary to reformulate) the lemmas in such a way that the properties used in their proofs are easily assimilated by the memory, so that they are *easy to remember.*" Kreisel frames this with elements from [Kreisel, 1976a] (discussed above), especially the appeal to properties that occur frequently and their axiomatic analysis for perspicuity. He then sets out (p.297): "Now we are ready to apply some of Wittgenstein's favorite slogans to the axiomatic analysis of proofs, e.g. the relatively original: the proof constructs (i.e., in the proof one discovers) new concepts, or the very popular one around 1930: only the proof gives meaning to the theorem that it proves." (It will be remembered that Kreisel in his [1958b, pp.140f] review of RFM had worked chiasmatically against these slogans.) Kreisel proceeds to give "two (entirely elementary) examples",[25] these evidently in the spirit of the elementary RFM examples.

Proceeding to Hilbert's program and consistency, Kreisel declaims (p.298): "Like many others around 1930, Wittgenstein was decidedly enthusiastic about the main component of Hilbert's program: formalization." Yet on two points Wittgenstein was critical: "Firstly, ... he thought it not fruitful to consider *all* calculations of a 'calculus. Put differently: formal provability (even by limited means) without regard to ease to take in and remember seemed to him a bad idealization." "Secondly, he was disturbed by Hilbert's exaggerated claims for the importance of consistency." On this last, Kreisel puts Wittgenstein in company with Brouwer and Russell as also "very critical", and mentions Gödel and Gentzen's criticism that "consistency at best guarantees the validity of universal theorems ..., whereas in practice one is rather more interested in existence theorems."

Considering next the shift from provability to proofs, Kreisel writes (p.300): "Since completeness and incompleteness only relate to provability, and have nothing to do with the structure of proofs, they lose their central role." But then, "What happens to incompleteness *proofs* when incompleteness itself loses its 'fundamental' significance? A normal person remembers the good advice:

[25] The first was given in footnote 16.

we have nothing to fear but fear itself. In other words, such proofs have more meaningful consequences ....” On this Kreisel relates an anecdote from the 1940s, the last of his quoted in §1, with how, with the incompleteness proofs, “In Wittgenstein’s opinion, Gödel had discovered an absolutely new method of proof.”

Kreisel ends with “Wittgenstein’s expectations” (pp.301f):

> Above all the *Remarks* were meant to stimulate the reader to have his own thoughts; especially those readers who had already come close to Wittgenstein’s thoughts. ...
>
> This expectation was confirmed by my own experience. When they came out, the *Remarks* did not help me at all. Since the end of the sixties I myself had started to consider structural properties of proofs. After a lecture in 1973 in which I presented these ideas and their development (also by Statman), Nagel drew my attention to the fact that these tendencies (certainly not the details) reminded him of Wittgenstein’s *Remarks*. I was absolutely unaware of this connection before then. But I am entirely aware of the additional confidence in my own thoughts that I derived afterwards from leafing through, e.g., Wittgenstein’s *Zettel*. Added to this was a certain pleasure, at his skillful formulations and my reformulations of his less skillful ones.

That said, Kreisel retrenches with softer versions of criticisms from his RFM review: how Wittgenstein’s specific examples were not fruitful for Kreisel; how he has no use for Wittgenstein’s “fussing with *clarity* and *clarification*”; and Wittgenstein’s “often erroneous contraposition of *clarification of existing knowledge* and *new constructions*”. Nonetheless, throughout [Kreisel, 1983a] there is steady, serious engagement with Wittgensteinian incentives in RFM.

Kreisel’s last articles concerning Wittgenstein are variously elliptical, reminiscent, or outright expressionistic. [Kreisel, 1983b] is a quick review of Kripke’s 1982 book, *Wittgenstein: On Rules and Private Language*, a review that amounts to a series of chiasmatic remarks putting things in a series of different nutshells. [Kreisel, 1989a] is a collection of “recollections and thoughts” about conversations with Wittgenstein, from which we have already drawn in §1. And [Kreisel, 1989b], *Zu Wittgensteins Sensibilität*, written for a festschrift, is a remarkably expressionistic series of wide-ranging aphorisms, quips, repartees, and things that came to mind—but nevertheless an article that fully affirms Kreisel’s deep engagement with Wittgenstein.

As a way of affirming and accentuating an overall *chiasmus* for Kreisel, his eventual reversal in attitude about RFM after his negative review [Kreisel, 1958b], we consider passages from a appendix to a long letter [1990b] that Kreisel wrote to Grigori Mints in 1990. First, from p.24:

> In a sense I might be said to have made fun of Wittgenstein in a review I wrote in the 50s of his *Remarks on the foundations of mathematics* (although this description certainly does not fit the way I felt about that volume nor about the review). I had made a mistake, which I noticed some 20 years later,* and have referred to it many times. But let me repeat it here, since you may not have taken in these references.

> *Main Mistake.* I did not look at the preface, where the editors say in the clearest possible terms that they had found a box full of notes by Wittgenstein, and that *they* had selected what, to *them, seemed most extraordinary.* N.B. I knew those editors! So, if I had looked at the preface this passage would have been an *immediate warning*: what is most extraordinary (=remarkable) to *them* was almost bound to be either wrong [?] or even an aberration.

With the * he references [Kreisel, 1979], a brief review of the second edition [Wittgenstein, 1978] of RFM that does not mention any mistake. In the preface to RFM [Wittgenstein, 1956], the editors nowhere state that they "had found a box full of notes", but do state (p.viie) "...what is here published is a *selection* from more extensive manuscripts".

Later on, Kreisel wrote (pp.25f):

> *Consequences of the main mistake.* Actually, in the last paragraph of the review (in the 50s) I said explicitly that I simply did not recognize in the *Remarks on the foundations of mathematics* what I had remembered minimally from my conversations with Wittgenstein. Fittingly (at least from my view of the world), I ignored what I remembered of Wittgenstein, and read the volume as a foil to my then current interests, mentioned above: What, if anything, does it say that is in conflict with—tacitly, the mere coherence of—the foundational tradition?
>
> *STAGGERING OVERSIGHT* on my part. I myself had put on record (in [[Kreisel, 1950]], somewhere in a highly visible footnote)—published during Wittgenstein's lifetime!—Wittgenstein's perfectly good understanding of Gödel's incompleteness theorem; tacitly, in the mid 40s, after I had explained it to him in $< \frac{1}{2}$ hour in WORDS CONGENIAL TO HIM. In accordance with his habit he recorded the explanation in his own words, incidentally stumbling thereby on—what later came to be called—Henkin's problem.

Henkin's problem is [Henkin, 1952]: "If Σ is any formal system adequate for recursive number theory, a formula (having a certain integer q as its Gödel number) can be constructed which expresses the proposition that the formula with Gödel number q is provable in Σ. Is this formula provable or independent of Σ?" [Kreisel, 1953] discussed an approach to this problem, and then [Löb, 1955] established provability for more general formulas and under minimal conditions on Σ, the result now known, of course, as Löb's Theorem.[26]

The overall *chiasmus* working its way through the previous and the current sections is *first*, the critical attitude Kreisel took to the *Remarks on the Foundations of Mathematics* in his review [Kreisel, 1958b] as particularly seen in his negative remarks about Wittgenstein's purported construals of the diagonal argument and the first incompleteness theorem, and *second*, a gradual working back, as traced in this section, to a nuanced assessment and appreciation in his articles in the 1970s and 1980s, particularly in [Kreisel, 1983a], ostensibly in tandem with the evolution of his own thinking and experience. The letter cements the *chiasmus* further, working various angles of mistakes and oversights. In particular, Kreisel not only records the *mea culpa* of his not recalling having mentioned Wittgenstein's rule-following version of the diagonal argument in first

[26] With *Bew* the provability predicate and $\ulcorner\varphi\urcorner$ the Gödel number of φ, Löb's Theorem asserts that for adequate Σ, if $\Sigma \vdash Bew(\ulcorner\varphi\urcorner) \longrightarrow \varphi$, then $\Sigma \vdash \varphi$. Henkin had asked whether $\Sigma \vdash \varphi$ or not in the special, fixed-point case when *is* φ.

logic paper [Kreisel, 1950],[27] but credits Wittgenstein for actually formulating Henkin's problem, a problem he himself later worked on.

Stepping back and taking it all in from the beginning, one sees the "early" Kreisel as stirred to his lifelong engagement with constructivity and proof by conversations with Wittgenstein, particularly with the "combinatorial core" of consistency proofs. One sees the "middle" Kreisel with an anxiety of influence reacting negatively in his reviews of Wittgenstein publications, flattening his work on language and insisting on the fruitfulness of research into constructivity and even set theory. Finally, one sees the "later" Kreisel in published essays interestingly integrating his latter-day, seasoned outlook on logic and mathematics with remembrances of the words and ways of Wittgenstein. Proceeding in dialectical engagement, Kreisel growingly acknowledges Wittgenstein as at least providing a conceptual context. But while aspiring to encompass Wittgenstein's broad ways of thinking about language, Kreisel would ultimately remain within the compass of logic and mathematics as set out by RFM. There, the engagement was enlivened by an appreciation of mathematical practice as the place to look for the important structural properties of proof; the role of explicit definitions in that regard; and the importance of proofs "easy to take in and remember". This rounds an arrow of influence, those early conversations with Wittgenstein having stimulated Kreisel to constructivity, logic, and proof.

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[27] cf. end of §1.

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