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ON KIT FINE'S *THE LIMITS OF ABSTRACTION* – DISCUSSION

1. INTRODUCTION

The best place to begin these comments is to say that I found this to be a wonderful book – genuinely thrilling to read. It is a challenge for me to find a contribution that hasn't been already advanced many times, since the core topics – reference to abstract objects, say, or impredicativity, to choose two among many – have received so much intense attention in recent decades. I'll try not to just repeat arguments that I already know are in the literature. Also, since many other people are in a better position than I am to comment on the technical side of the general theory of abstraction, I'll refrain from comment, except to indicate my enthusiasm. Looking to a general account rather than remaining content with *ad hoc* justifications of this or that abstraction principle is clearly a crucial step to understanding the issues. Finally, I'll have little that is critical to say, because I'm in broad agreement with those of the central reflections in *The Limits of Abstraction* on which I may have something novel to contribute.[1]

I am most interested in learning how the work in the book can be pushed forward to address further questions. I'll put those questions on the table and inquire about the relations of these questions to the doctrines of *The Limits of Abstraction*, and the promised extension via "procedural postulationism", supplemented perhaps by the author's other research into definition and essence. In particular, I hope to learn whether and how these doctrines might shed light on striking and puzzling cases in non-foundational mathematical practice where finding the right definition is a significant theoretical objective.

(To facilitate the connection between the mathematical cases and the issues at hand, I'll consider in section II some cases salient to Frege in the nineteenth century, but the point holds for present mathematical practice as well.) To this end I'm asking for clarification of various tantalizing doctrines whose explanation is sufficiently compressed that many interesting directions lie open, depending on how the terse exposition is elaborated. Depending on how the condensed remarks are spelled out, the doctrines implicit in the book (and in the "procedural postulationism" still to come) may be illuminating, neutral or even in conflict with natural ways of reading some aspects of mathematical practice that puzzle me. So I'm eager to hear more. To put it colloquially, given my admiration for the book and its author, my best strategy is to imitate the coach of an exceptional athlete: point to the field, hand him the ball and get out of the way.

I'm especially interested in development of the author's suggestion that in foundations of this kind it is typically not enough that a definition be laid down; there also must be reason to regard it as in some way or other distinguished. Even if we can produce demonstrably equivalent definitions, we must be able to make sense of the suggestion that *this* definition – and not *that* one even if it is equivalent – really gets things right, or captures the essence of the thing defined, or is, to use the old terminology, a real rather than a nominal definition. I'm sympathetic to the idea that some definitions are better than others in broadly this way, though I'm also sensitive to the difficulty in articulating an acceptable basis for distinguishing definitions into real and nominal. One place in *The Limits of Abstraction* where the idea shows up is in a preliminary discussion (pp. 29–32) of the problem of arriving at a unique numerical operator among the many that are available. Though the proposal itself is set aside as a suggestion for defining the numbers, this is not because of doubts about the idea that one definition may better enshrine "the essential properties of numbering and the numbers" (p. 29).

In the second section I'll look to one place where we need to make sense of people who speak of the "proper" definition a mathematical concept or object, or a definition that corresponds to the essential features of the concept or object being studied. Quite simply, as a simple descriptive observation about ongoing mathematical investigation, one objective is to arrive at a definition that – speaking loosely – "gets it right". I have in mind remarks like these; the historical ideas mentioned will be touched on again in section 2:

> People who know only the happy ending of the story can hardly imagine the state of affairs in complex analysis around 1850. The field of elliptic functions had grown rapidly for a quarter of a century, although *their most fundamental property, double periodicity, had not been properly understood;* it had been discovered by Abel and Jacobi as *an algebraic curiosity rather than a topological necessity*. The more the field expanded, the more was algorithmic skill required to compensate for the lack of *fundamental understanding* ... [Cauchy even came] to understand the periods of elliptic and hyperelliptic integrals, *although not the reason for their existence*. There was one thing he lacked: Riemann surfaces. (Freudenthal, (1975) p. 447 emphasis mine)

The fact that mathematicians speak this way and motivate research accordingly needs philosophical clarification. Of course, we can grant that in some circumstances mathematicians set finding the "right" definition or fundamental property as one among many research objectives, without adopting any stance as to whether or not that way of talking is philosophically tenable. All we need say to begin is that this is a way mathematicians talk, and it is embedded in the practice in systematic ways. The philosophical question is how seriously we can take this way of talking and whether there is a philosophically interesting connection to our studies of definition both in the foundations of arithmetic and in general.

I'm hopeful that some clarity about this kind of methodological question can be attained by fleshing out ideas like that of a definition encapsulating the essence of a concept rather than turning on accidental features. But there is a complication: in a range of interesting cases in mathematical practice, the discovery of the "right" definition is a real *discovery*. The question of what the right definition is might require profound investigation to answer. A concept or object can be introduced and discussed for some time before the right way to get at it is properly understood. Indeed, it might be that the definition or devices by which a concept or object is introduced is set aside as accidental, in favor of an equivalent definition in terms that only were worked out later. This is exemplified by the case mentioned in the above quote: "elliptic functions" were originally introduced as the inverse of certain kinds of integral. Later it was discovered that elliptic functions have two periods. That is, if Φ is an elliptic function, there will be complex numbers ω_1 and ω_2 such that for any complex z, and any integers m and n, $\Phi(z) = \Phi(z + m\omega_1 + n\omega_2)$. Later still, it was recognized that this is a reasonable choice as a defining feature of such functions, while the original definition in terms of the inverse of an integral, though equivalent, is better seen as accidental. Reflecting this, most textbooks today *define* "elliptic function" as a (meromorphic) complex function with this characteristic property. Individual functions are taken to be determined by their periods, rather than by the integrals that they are inverse to.

In many cases there is a principle of evidence for "getting it right": if the new definition really is the correct one, then adopting it should make investigation easier. It will facilitate discovery and shed light on topics that were previously obscure. (I'll suggest in section 2 that this point is in fact implicit in Frege's discussion of the Caesar problem.) This raises a question I'll return to about the role of "real" definitions – or "capturing the essence" of a concept – as the author of *The Limits of Abstraction* conceives these things: what do we *gain* if we hit on the real definition rather than some accidentally equivalent one? Do we gain some cognitive advantage for subsequent investigation if we get things right?

Does the conception of real definition implicit in *The Limits of Abstraction* have any room for such phenomena? Or is it impossible – given the author's understanding of definition – for an object to be introduced, used and studied via one definition or tacit specification, while its real definition remains unknown and even – until more is learned – not practically knowable? (If so, then someone like me seeking a conceptual framework to use for this aspect of mathematical practice should look elsewhere.) It would seem on the surface as if the author must have room for something like this, if he is to maintain that some abstraction principle can capture "the essential properties of numbering and the numbers". Numbers and numbering were both used in daily life and studied in sophisticated mathematics long before abstraction principles came explicitly on the scene. But this surface impression could be misleading. Perhaps the concept of number introduced by the abstraction principle is seen as some kind of replacement for or explication of the naïve conception of number, rather than an analytical regimentation of it. Perhaps something else is going on. So here it would be useful to have further clarification of the relationship the author envisions between the regimented presentation of "the essential properties of numbering and the numbers" and both the prior practices of counting and the ongoing mathematical studies that regimented presentation is meant to undergird.

Some of the programmatic remarks about definitions in *The Limits of Abstraction* are especially tantalizing because of a further feature of the practice of seeking good definitions that the definition of elliptic function exhibits. The appreciation of what is essential about a concept or object can require a shift in the domain of discourse. The original definition of elliptic function makes sense if the functions are defined only on the real numbers. But one of the periods will always be a non-real, complex number. So to bring out the "fundamental property" the domain of definition has to be *extended* beyond the one originally envisioned. Often we can't make out the essential features of a mathematical concept or object unless we define it over the "proper" domain where such "proper domains" may be extensions of the ones with reference to the concept or object was originally introduced.[2] As I'll note in III, some of the remarks the author makes in his preview of his method of "procedural postulationalism" are especially intriguing in this connection.

It will help for orientation to have a simpler example, though it has the disadvantage of being imagined rather than real. It will help avoid thorny issues if we make it a simple explicit definition. Say we have managed to define number somehow, and we're interested in prime numbers. We have, of course, a canonical definition: a natural number n is prime iff its only positive integer factors are 1 and *n*. Now say that someone solves the Riemann hypothesis, and other breakthroughs occur in our understanding of the Riemann zeta-function, so that we can arrive at a complete understanding of the distribution of prime numbers in the natural numbers. This requires us to *extend* our structural understanding of the natural numbers: to exploit the zeta-function we have to see the natural numbers as embedded in the complex numbers, a structure which (let's say for the sake of argument) is itself constructed out of the integers. Now furthermore, let's say that once the underlying factors are laid bare, the prime numbers turn out to have an astonishingly lawlike and regular distribution, once the underlying mathematics is laid bare. Also, it turns out that the prime numbers are the simplest example of a structure exhibiting this sort of distribution, and the general theory of these distributions becomes a core mathematical discipline, with the result that from the point of view of the best available mathematical theories, the sense of what should be taken as a defining characteristic of a prime number changes: rather than being essentially divisible only by itself and one, prime numbers are seen as essentially having the newly discovered properties. The traditional definition, while still useful for teaching elementary students, comes to be seen as accidental and even a curiosity.

Obviously this story is fanciful in the extreme as a forecast of the future of mathematics, but the question here is whether it is coherent, relative to the understanding of definitions that is implicit in *The Limits of Abstraction*. Can it be that we use a concept, with both an accepted definition and a role in a range of ongoing scientific investigations, in such a way that we can discover later that the definition capturing the essence of the concept is not the original definition but rather an equivalent one that has virtues especially prized by the practice? Is it possible that the rationale for this reformed understanding of the concept can coherently appeal to investigations that themselves appear to presuppose the concept?

One principle that seems at first sight to cut against this possibility is what *Limits of Abstraction* calls "Limited Access" (pp. 72–77). But this depends on how various qualifiers are unpacked. The principle holds that "the means by which a contextually defined object is introduced into the discourse provides essentially the only means by which it may be identified. Thus if numbers are introduced by Hume's law, then any particular number must essentially be identified as the number of a given concept...".(p. 76) This appears restrictive, but the next few paragraphs contain a condensed discussion of possible tenable variations that can arise, depending on how "means" and "essentially" are to be cashed out. The refinements include "in identifying a given contextually defined object, we should allow ourselves to use whatever structural relations might be directly or indirectly involved in formulating the definition" (p. 77) and the general observation that "our access to abstract objects such as numbers or sets or directions appears to be limited by the structural relations with which they are naturally associated." (p. 77) Obviously a great deal will depend on the further elaboration of "naturally associated", "structural relations" "directly or indirectly involved" and other charged expressions in this compressed discussion. The treatment is so terse that I'm not sure how the story will go, so some clarification and development would be welcome.

Especially exciting in this regard is the gesture in the direction of the positive account to come ("procedural postulationism"): "The basic idea behind this alternative approach is that, instead of stipulating that certain statements are to be true, one specifies certain procedures for extending the domain to one in which the statements will in fact be true...[the legitimacy of these procedures] does not depend on the prior knowledge that the objects which are to be introduced into the domain already exist." (p. 100) Once again, these words hold out hope for an account of definition that will be subtle enough to incorporate a wide range of phenomena displayed by the practice of seeking good definitions in ongoing mathematical practice. So here too, we may hope that further development will illuminate over a wider range than philosophical treatments of definition tend to travel. I'll return to this topic after putting some orienting background in place.

2. HISTORICAL BACKGROUND

To flesh out the kind of question I hope clarification of the principles of definition underwriting *The Limits of Abstraction* will illuminate, it will help to give some thumbnail history. (This section won't refer directly to the *The Limits of Abstraction*, but it is a necessary detour, as otherwise it will be obscure why I think ideas like ''essence'' and ''real definition'' might come in handy for understanding mathematical practice if the ideas are articulated thoroughly.) To short-circuit a lengthy discussion of mathematical practice, I'll consider the way that some of Frege's remarks in the core sections of *Grundlagen* would have resonated with the mathematicians around him. The practice of defining objects in ongoing mathematics was an issue around Frege, and we can see it show itself in the discussion of definition in *Grundlagen*. (It is thematically cleaner, that Frege's discussion is colored by the issues about mathematical definition I've been discussing, but it isn't absolutely essential. The central concern is the issues about definition themselves.) Frege and his core mathematical audience would have seen several complications and ramifications that we overlook today. (My point isn't to give a scholarly defense of these claims but rather to use them to illustrate some issues that can arise for anyone worried about the role of definitions in mathematics.)

Here are a few framing details that I defend elsewhere – I'll presuppose them here.[3]

(a) Frege's work in ''ordinary'' (non-foundational) mathematics is concentrated in two areas: geometry and complex analysis (with a special emphasis on the theory of elliptic functions and integrals).

(b) German complex analysis at the time exhibited a sharp divide between the "computational" approach of Weierstrass, and the "conceptual" Gottingen approach of Riemann.
(c) The adherents of the Riemann tradition included Frege's teachers and Dedekind. More generally, the evidence from Frege's teaching, research and context indicates that he was immersed in the Riemann tradition and opposed to the principles driving the Weierstrass approach.

Even with all the attention focused on sections 55–83 of *Grundlagen*, I think we've missed at least one issue that would have been salient for Frege's readers and that Frege would have expected his readers to recognize as lying behind his words. A basic theme is one we've mentioned. It can be of the greatest scientific importance, for the ongoing practice of mathematics (and not just the rigorous foundation), to find the "right" definition of something. A subtheme is that there is generally a payoff for doing this successfully: the definition will aid in discovering new results, finding proofs, understanding what is going on, etc. In a word, a successful definition is fruitful.

In this light, consider *Grundlagen* §67. Frege is responding to a natural question: why not say that the things whose introductions are forced by the definition count as numbers (or in this case directions), and nothing else does? Frege's response ties the presentation – independence of objects to the potential for increasing knowledge through deductive reasoning.

§67 If we were to try saying: q is a direction if it is introduced by means of the definition set out above, then we should be *treating the way in which the object q is introduced as a property of q, which it is not*...[and a further unacceptable consequence would be that:] All identities would then amount simply to this, that whatever is given to us in the same way is to be reckoned as the same. *This, however, is a principle so obvious and so unfruitful as not to be worth stating. We could not, in fact, draw from it any conclusion which was not the same as one of our premises*. Why is it, after all, that we are able to make use of identities with such significant results in such diverse fields? Surely it is rather because we are able to recognize something as the same again even though it is given in a different way. (Frege, 1884/1953, p. 78–79 emphasis mine)

Here Frege appears to be rejecting a weak version of Limited Access: we are not allowed to treat the fact that an object was in fact introduced in a certain way as a property of the object. We should recall that although today mathematics has advanced to the point that the introduction of objects with reference to some equivalence relation is old hat, to Frege it was a relatively novel device for defining direction (orientation, etc.); Frege would have seen himself as exploring a new technique whose potential wasn't fully charted. Outside of geometry a high profile use of this manoevre was in Dedekind's reconstruction of Kummer's theory of ideal numbers. We'll consider that, and the motivations for it, in a moment. First let's remind ourselves of how Frege regards the connection between his conception of fruitful mathematical innovation and the *Grundlagen* conception of "extending knowledge". Frege remarks in *Grundlagen* #64 that in the Hume principle "we carve up the content in a new way and this yields us a new concept (p. 75)." These metaphors of carving are marshaled (elsewhere in *Grundlagen* as well as in other Fregean writings) to support the suggestion that logical inferences exploiting the truly "fruitful definitions" (sometimes "fruitful concepts") actually extend knowledge. In Frege's most vivid expression of the point, he writes:

> [Kant] seems to think of concepts as defined by giving a simple list of characteristics in no special order; but of all ways of forming concepts, that is one of the least fruitful. If we look through the definitions given in the course of this book, we shall scarcely find one that is of this description. The same is true of the really fruitful definitions in mathematics, such as that of the continuity of a function. What we find in these is not a simple list of characteristics; every element is intimately, I might almost say organically, connected with the others... the more fruitful type of definition is a matter of drawing boundary lines that were not previously given at all... The conclusions we draw from it extend our knowledge, and ought therefore, on Kant's view, to be regarded as synthetic; and yet they can be proved by purely logical means, and are thus analytic. (Frege, 1884/1953, pp. 100–101)

The logical structure of "recarving" is meant to fit with and underwrite a methodological story about the concepts and definitions that are especially fruitful in practice. Among other things, the quantificational analysis is meant to explain how such definitions are especially natural, or as Frege puts it figuratively in the above remarks: "every element is intimately, I might almost say organically, connected with the others."

Frege stresses the point that he regards his foundational effort as bound to address the concepts that are "most fruitful" as revealed in ongoing *scientific* practice. That is, an especially salient target for a logical system is that it must be able to represent the fruitful concepts, as discovered in "scientific workshops: logic's true field of observation":

All these concepts have been developed in science and have proved their fruitfulness. For this reason what we may discover in them has a far higher claim on our attention than anything that our everyday trains of thought might offer. For fruitfulness is the acid test of concepts, and the scientific workshop is logic's genuine field of observation. (Frege, 1882/1979 p. 33)

To understand how Frege would have expected these words to resonate with his audience – at least the mathematically informed readers – we need to look to the work that dominated his environment. Around Frege, by those who worked in the stream of research Frege did, it was believed (rightly as we can see in retrospect) that a revolution in mathematical method was implicit in Riemann's methods for complex analysis. The particular feature of Riemann's approach to complex function theory that is relevant here is the specific way it sought the "right" definition of key functions and objects. Dedekind sums up the attitude vividly in the following remarks. He represents his approach – inspired by his teacher Riemann – as pushed forward by an emphasis on "the internal rather than the external":

[Gauss remarks in the *Disquisitiones Arithmeticae*]: "But neither [Waring nor Wilson] was able to prove the theorem, and Waring confessed that the demonstration was made more difficult by the fact that no notation can be devised to express a prime number. But in our opinion truths of this kind ought to be drawn out of notions not out of notations." In these last words lies, if they are taken in the most general sense, the statement of a great scientific thought: the decision for the internal in contrast to the external. This contrast also recurs in mathematics in almost all areas; [For example] (complex) function theory, and Riemann's definition of functions through internal characteristic qualities, from which the external forms of representation flow with necessity. [Dedekind continues, in paraphrase: The contrast also comes up in ideal theory, and so I am trying here to put down a definitive formulation.] (Dedekind, 1895, pp. 54–55)

Of course, the *philosophical* problem of articulating the difference between "fundamental characteristics" that allow you to "predict the results of calculation" and "forms of representation which should be results, not tools, of the theory" is thorny. But the remark was nonetheless fairly transparent at the time, since the specific examples and methods Dedekind is alluding to were well-recognized. Indeed, Dedekind is deploying catchphrases that were so common among those in the Riemann stream as to be clichéd. Though the contrast of "internal" and "external" was not explicitly philosophically articulated, every mathematically literate reader knew exactly what Dedekind was getting at, and could name a long list of canonical examples. One example is especially interesting here. Dedekind explicitly links the Riemann stance with *his* version of an answer to the "Caesar problem": his reformulation of Kummer's ideal theory in a representation – independent way. Dedekind has just presented a Kummer-style theory of divisibility of ideals he regards as not wholly inadequate, but which he nonetheless rejects.

One notices, in fact, that the proofs of the most important propositions depend on the representation of an ideal by the *expression* [ma, $m(b + \theta)$] and on the effective realization of multiplication, that is on a *calculus* . . .If we want to treat fields of arbitrary degree in the same way, then we shall run into great difficulties, perhaps insurmountable ones. Even if there were such a theory, based on calculation, it still would not be of the highest degree of perfection, in my opinion. It is preferable, as in the modern theory of [complex] functions to seek proofs based immediately on fundamental characteristics, rather than on calculation, and indeed to construct the theory in such a way that it is able to predict the results of calculation. . .Such is the goal I shall pursue in the chapters of this memoir that follow. (Dedekind, 1877/1996, p. 102 italics in original, underscoring mine)

What sorts of reasons were given to justify claims that some definition or other was based "on fundamental characteristics" instead of "externals"? It would be disappointing if the only justification Riemann, Dedekind et al could provide took the form of some brute, inarticulate and unarticulatable aesthetic response, as if "being the right definition" were grounded in nothing more than "powerfully *seeming* to be the right definition". In fact, the reasons were subtle and various. I'll note just one here: it was seen as evidence that a definition got things right if it was fruitful.

Riemann used a particular turn of phrase to make this point: he spoke of his most significant definitions as making it possible to "see practically without computing" results which had required "tiresome computations". These improvements in evidentness brought out by a proper definition were expected to be systematic: the proper definition should make a range of interesting problems easier, in the long haul. Dedekind is echoing this rhetoric in the above quotes. Any mathematician of his time would have recognized this, as these phrases too had become clichés. As one illustration, even as late as 1899 a textbook writer characterizes some strengths of the Riemann approach with a string of buzzwords and catch – phrases, some of which we've just seen:

> ...the synthetic treatment of analytic problems which builds up the expression for the functions and integrals *solely on the basis of their characteristic properties* and *nearly without computing* from the given element and *thereby guarantees a multifaceted view into the nature of the problem and the variety of its solutions*. (Stahl, 1899, p. III emphasis mine)

For the issues we're discussing, it's especially interesting where Dedekind takes this. We began the section with a quote in which Dedekind relates his assessment of Riemann's methodology to his effort to recast Kummer's theory of ideal numbers extending a given structure. The main point of the ideal numbers is to provide extra numbers that will *divide* the numbers in the original structure (with this division having desired properties). Kummer, though, doesn't tell you how to decide whether a given object is an ideal number; he only says how things that are given as ideal numbers behave. (For example: his rules tell you – if you are handed two ideal numbers, identified as such – what the sum of those numbers will be.) Dedekind, in his recasting, introduces an *equivalence class* (an "ideal") of things in the original domain that are divided by a given ideal number, with the rules for manipulating ideal numbers derived from the properties of the explicitly presented ideals:

> ....Since a characteristic property serves to define, not an ideal number, itself, but only the divisibility of the numbers in **o** by the ideal number, one is naturally led to consider the set **a** of *all* numbers α of the domain **o** which are divisible by a particular ideal number. I call such a system an ideal for short, so that to each particular ideal number there corresponds a particular ideal **a**. (Dedekind, 1877/1996, p. 58 italics in original)

This brings us full circle: implicitly in the case of Frege (or so I'm suggesting) and explicitly for Dedekind, the Caesar problem is bound up with a family of subtle questions about the role of definition in non-foundational mathematical research as well as philosophical foundations.

3. "PROCEDURAL POSTULATIONISM" AND ESSENCE AGAIN

It's smoother for exposition to present these 19 century examples, but the point holds for contemporary mathematics as well. (Of course, there is a lot of context-sensitivity, and variation along subfields, and so forth, and mathematicians today are typically less explicit than Riemann and Dedekind about the implicit methodology, but these features of ongoing mathematical practice are no less real and in need of philosophical attention and clarity for all that.) In these closing remarks I'll be more specific about some of the things I hope/believe can be illuminated as some of the more compressed parts of *Limits of Abstraction* are unfolded.

So let's return to the brief characterization of "procedural postulationism". "The basic idea behind this alternative approach is that, instead of stipulating that certain statements are to be true, one specifies certain procedures for extending the domain to one in which the statements will in fact be true... [the legitimacy of these procedures] does not depend on the prior knowledge that the objects which are to be introduced into the domain already exist." (p. 100) Unless further elaboration corrects my impression, it seems to me that this will lay the groundwork for philosophically framing a technique that is ubiquitous in mathematical practice, though it is not philosophically well understood: making desired statements true by extending the domain so that the statements do come true. You want equations to split into linear factors? Extend the reals with *i*. You want every pair of lines to intersect, and equations of degree m and n to intersect in exactly mn places? Extend the underlying plane with points at infinity (and be careful to count intersections properly). This shows up when global features of a domain are at issue too: you want the surface to be compact? Extend it with a point (one-point compactification) or if you want to preserve more nice properties extend it in a more involved way (Stone-čech compactification). Can we go further and give a philosophical scaffold for the idea that in many cases these extensions provide the "right context" for the study of the given theory? Can we give a philosophical account of what could be meant when the essential properties of these objects, or the proper definitions of them, can be arrived at by exploiting such extensions? (Both in cases where finding the essential properties or proper definitions is taken to require the expansion of a domain, and in other cases where the domain remains fixed.) As mentioned, much will depend on how the compressed remarks cited in I are developed.

The author's discussions of his preference for a predicative account do leave it unclear how much room there could be for the possibility that a real definition could be a later introduction, superseding the definition by which the concept or object was originally introduced. Is it possible, given how the author understands these matters, for a concept or object to be introduced or defined in a context where there is also an implicit methodology that determines some other, *subsequently discovered* definition as the one that really captures the essence of what is introduced or defined? How important is it to our specification of the essential properties of a concept or object that we in fact introduce the concept a certain way, or even that we *must* introduce it a certain way? (The essence-capturing definition may be implicitly determined by something like a Ramsey sentence, with further investigation required to ascertain what best satisfies it.) Does the author's vision have room for definitions that introduce a concept or object, but which ultimately serve only as ladders to be kicked away (required by the human condition or the logocentric predicament, perhaps, but not by the nature of what is defined)?

Another question that naturally arises is just what we are to take as *evidence* that we have found the essence, or the real definition, or what have you. As I've noted, a core piece of the background methodology for Riemann and followers was that finding the right definition should make things easier: it will bring new things to light, and render relatively evident matters that had previously been obscure or drowning in computations. Can we say anything analogous on the conception of definition setting the backdrop for *The Limits of Abstraction?* Are there any characteristic marks by which we can recognize the real definition when we happen upon it? Can we say anything general, or will we have to rest with a different *ad hoc* rationale for each separate case? Is there anything we *gain* by working with the real definition rather than a nominal one, or is possessing and using the real definition something which may well be inert as far as fruitfulness for further investigation is concerned? Will arriving at the real definition have any effect at all on the progress of the practice of (say) number theory?

This may give a sense of a few of the reasons why I find *The Limits of Abstraction* exciting. The crafting, choice and use of definitions in ordinary mathematical practice is far more involved and complicated than philosophical theories of definition have had room for, except perhaps medieval theories that today strike us as begging more questions than they answer. The views articulated in the book, and even more those that are presupposed or sketched out for future development have the potential to give a great step forward in our understanding of a neglected range of questions arising from ordinary mathematical practice. Depending on how the compressed discussions noted in I are unpacked, we have the potential for an account that will shed light on these phenomena or (almost as good) will conflict with the naïve interpretation of the phenomena in a way that can promote deeper understanding.

Having indicated a few places where the compressed discussion calls out for further development, and having indicated some questions that I hope such further development might engage, I'll get out of the way.

NOTES

[1] This is not to say I agree across the board, only that those disagreements I have – on impredicativity, for example – are for reasons that are completely unoriginal.

[2] A further example – especially relevant here since it was both salient to Frege and involves the definition of "direction of a line" (in the guise of "point at infinity") – is "conic section". The traditional definition in terms of slices of cones, and even the definition in terms of the degree of the equation (second) in ordinary Cartesian coordinates fail to bring out the underlying unity of the concept, and complicate many of the central results. Currently it's accepted that the natural context for the study of conic sections is the projective plane (the ordinary plane augmented with a point at infinity for every class of parallel lines) and the natural analytic definition exploits the corresponding homogeneous, rather than Cartesian coordinates.

[3] See my (200x) and (200y).

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