Infinitesimals, Magnitudes and Definition in Frege

Jamie Tappenden

July 22, 2015

Contents

1 Introduction: Infinitesimals, Objects and Contextual Definition

The publication of a complete *Grundgesetze* translation will support a wider engagement with topics previously closed to many in the English speaking world. The account of real numbers/magnitudes, which occurs nowhere but in previously untranslated parts of *Grundgesetze* II is among the most exciting. My goal here is a mathematical/historical prolegomenon to *Grundgesetze* II on magnitude. I'll address some contextual and textual issues, both as interesting in their own right and as delineating connections to mathematical work in Frege's environment.

Frege's approach to the real numbers in *Grundgesetze* II is so different from what is widely taken as "late nineteenth century foundations of analysis" that it appears to be a solution to different problems. The currently best-known accounts of $\mathbb{R}$ - Dedekind's and Cantor's - address the structure of the real numbers to secure properties (every Cauchy sequence converges, every set bounded above has a least upper bound,...) recognized as indispensible in real analysis. Any role for $\mathbb{R}$ in measurement is an afterthought. By contrast, Frege's account is intrinsically structured by connections to applications and measurement. It sits within a distinct tradition tracing back to Eudoxus, based on ratios of quantities.[1] Antecedents closer to Frege included Riemann (1854) on space, Hankel (1867)'s foundation of number systems for Riemannian complex analysis, and the work of Helmholtz (though Frege differed with these writers on many points). The history of logical foundations hasn't yet engaged in detail with these currents, so the mathematical ramifications and tacit objectives shaping Frege's account of $\mathbb{R}$ are correspondingly obscured.

There is too much to cover in a short paper, so I'll restrict the focus. To engage the discussion with ongoing Frege scholarship, I'll take up some recognized textual questions. For the first, we can look to the last paragraph of *Grundgesetze*:

> This question may be viewed as the fundamental problem of arithmetic: how are we to apprehend logical objects, in particular the numbers? What justifies us to acknowledge numbers as objects? (Frege (2013) Vol II Afterword p. 265)

[1] Stein (1990) contrasts these two approaches.

What is the methodological significance of the "as objects"? It's complicated. In previous work (Tappenden (2005)) I explored some ways this objective reflected methodological arguments among mathematicians of the time.[2] Here I'll fit in more pieces of the puzzle, partly to further illustrate how, in Frege's eyes, these issues would have had methodological reverberations with the mathematics around him. That is, in addition to the logical, semantical and metaphysical dimensions of the definitions of number and magnitude, there were issues arising directly from mathematical practice.

A second question: What explains Frege's shifting evaluation of contextual definitions? In particular: what is behind the jarring switch from a contextual to an explicit definition of number in the heart of *Grundlagen*? Fortuitously, a narrower textual question can focus the discussion: how can we reconcile the apparent contradictions that emerge when we assemble the remarks Frege makes about infinitesimals and add the rejection of infinitesimals that appears to follow from a main result of *Grundgesetze* II?

A background question that I won't directly confront here (though I'll occasionally allude to it) concerns the most obvious change in Frege's framework between *Grundlagen* and *Grundgesetze*: the division of the content (*Inhalt*) of an expression into its sense (*Sinn*) and meaning (*Bedeutung*). Frege himself is unforthcoming about what subsidiary changes may have been induced by this one, so we need to look for footholds where we can find them. The cases we discuss here seem to provide one patch of rough ground, as they present examples of Frege appearing to countenance expressions introduced via contextual definitions that (by the lights of the later framework) provide a *Sinn* but not a *Bedeutung*.

Here is the paper in outline. In 2 I'll prepare the ground with an illustration of how much we can miss if we don't make the effort to put Frege's writings in their broader context. In 3 I'll review a puzzle: Make sense of Frege on contextual definitions, especially with reference to the transition in *Grundlagen* from a contextual to an explicit definition of number. In 4 I'll consider Frege's early contextual definition of infinitesimals, which clashes with his commitments in the 1890s and later. In 5 I'll look at one of the rare

[2]I'm not suggesting that connections to mathematical practice *exhaust* the ramifications of "acknowledging numbers as objects", only that they are important dimensions.

cases where Frege explicitly endorses a mathematican's defining practice — Riemann on the definite integral — in connection with Frege's constraints on definitions and introducting objects. In 6 I'll reconsider some of Frege's remarks in §60 - §68 with specific attention to the idea of "extent of validity". This will present a picture of at least some contextually defined expressions as functioning in a limited way, which might raise the question: Why would Frege care if such expressions could function in that way? In 7 I'll consider one specific case — Clebsch-Aronhold symbolic notation — where contextual definition through equations was used successfully around Frege in precisely that way, including acknowledged *Bedeutungslosigkeit* and *in principle* limited generality.

2 Hankel, Gauss and Negatives: A Cautionary Vignette

I'll begin with an orienting illustration of the degree to which Frege's writings by themselves give poor guidance about his environment. (Nothing in the rest of the paper depends on this section.) Frege is so stingy with credit, so careless with (mis)quotes, so cavalier about accurately representing opponents' views, and so unforthcoming about the mathematics around him, that sticking just to what he writes can mislead. Recall, as Dummett observes, that in *Grundgesetze* II §164, after criticising Cantor relentlessly, Frege appeals to Cantor's theorem without acknowledging Cantor.[3] In that case the result is so renowned that readers today need not be told. But many, or even most, such cases now skip by unnoticed, though Frege's readers didn't need to be filled in. Part of our job as scholars is to retrieve this web of connections that once went without saying.

A foundation for Frege's treatment of magnitudes in *Grundgesetze* is his account of negative magnitudes. The anchor is a long quotation from Gauss, stating that negative numbers should be understood in terms of the converse of a Relation.[4] As Dummett (1991) (pp. 278 - 279) notes, the Gauss view structures Frege's whole treatment of magnitudes, not just negatives. How distinctive was this view of negative numbers? Frege mentions only Gauss's

[3]Dummett (1991) p.284 The forceful comments in Tait (1997) are a valuable counterweight to a tendency to rely uncritically on Frege's assessment's of Dedekind and Cantor in particular.

[4]Frege (2013) II §162; the quote comes from Gauss (1831) pp. 175 - 76.

view in *Grundgesetze*, so if we are taking Frege as our guide to the environment, we need to go back to *Grundlagen* and Frege's critical discussion of Hankel.

In *Grundlagen* Frege mentions Hankel more than any other figure — once even favorably — yet writing on Frege rarely says more about Hankel's work than what Frege conveys.[5] Let's try to piece together Hankel (1867) from what Frege reports there. Apparently i) Hankel believes that the concept of number must be grounded in a primitive operation of "putting" (§20, §42) ii) and also in a "pure intuition of magnitude" which grounds definitions of operations that may then be extended (§12). iii) He also believes that numbers are objects of thought dependent on a thinking subject (§92) iv) for which the question of existence is simply the question of whether or not the theory describing them is consistent (§92). And finally, v) Hankel believes that mathematical objects are linguistic signs (§95).

The view represented by i) - v) is incoherent. Did Hankel really believe all these things at once? It can be a challenge to refine the raw materials of Hankel (1867), but not so difficult that one cannot say with confidence: No, Hankel did not believe all those things at once, not as Frege presents them. Did Frege believe Hankel believed all these things? We can't peek into Frege's mind, but we can say with confidence that Frege makes no effort to accurately represent Hankel. He doesn't even trouble himself to quote Hankel accurately.[6] Hankel (1867) - a work that was genuinely important to nineteenth-century mathematics - is treated as just a rhetorically useful attack-target-quote generating machine.[7]

[5]References to Hankel in *Grundlagen*: §5 (the approving mention), §6, §12, §20, §42, and repeatedly throughout §§92 - 99. Frege does refer more often to Baumann (1868) and Baumann (1869) (a two-volume compilation of material from other writers with Baumann's commentary), but only to cite someone else excerpted there (Spinoza, Hume, Hobbes, . . .).

[6]Frege misquotes Hankel on "putting" by omitting an parenthetical clarification in the sentence. cf. Tappenden (1997) p. 216 - 7

[7]Hankel is of considerable interest not only in his own right but in connection with Frege, though for the specific purpose of this paper I'll defer the bulk of the material for other work in preparation. (I have some remarks on Hankel in connection with Frege in Tappenden (995a), though I've learned more in the intervening years. Some information on Hankel's connection to Frege's mentor Abbe is in Tappenden (2011).) But a few remarks can help to explain what produced the seemingly conflicting quotes Frege catalogues. To be sure, Hankel bears some of the blame for being misunderstood, as the writing in Hankel

Consider, in this connection, Frege's criticism of Hankel on negative numbers in *Grundlagen* §95, which contains point v) above. To judge from it, Hankel advances a quite crude formalism:

> [quoting Hankel; ellipses added by JT:] "It is obvious that, for $b > c$, there is no number x in the series $1, 2, 3, \ldots$ which solves our problem;...There is nothing, however, to prevent us from regarding the difference $(c - b)$ in this case as a *symbol* which

(1867) is often obscure and he falls well short of Frege's logical precision. Frege makes this fair complaint in *Grundgesetze II* §145 (footnote), as does Husserl in an 1891 letter to Frege (Frege (1980) p.66). (The project might have been clarified in the projected second volume and other subsequent writings, but for Hankel's early death (at age 34, in 1873).) But the legitimate objection that Hankel is unclear is different from the charge (implicit in *Grundlagen*) that Hankel clearly states a collection of positions that are collectively incoherent.

Some of the obstacles to a simple interpretation arise just from the fact that Hankel is trying to do justice to a diverse variety of insights and areas of mathematics, and to satisfy many desiderata — perhaps unrealistically many — at once. They are not unreasonable objectives, though not all of them are on Frege's agenda. So for example, like Grassmann, Riemann and Frege's mentor Abbe among others, Hankel distinguishes between magnitude (*Grosse*) and number (*Zahl*). (Frege makes this distinction explicitly in *Grundgesetze*, but not in *Grundlagen*; in the earlier Frege (1874) natural numbers and continuous magnitudes are treated uniformly.) In some cases, the Hankel remarks quoted in *Grundlagen* address magnitudes and in others they pertain to natural numbers. The appearance of incoherence is in part due to Frege not giving the least hint of this. Hankel is also setting out both to explain specific number systems like the natural, real and complex numbers, and also to explain algebraic reasoning that extends such systems, both in the older fashioned "computing with variables" sense of algebraic that *is* quite naturally described as "manipulating signs" and an inchoate recognition of the contemporary "describing structures" sense of "algebraic" that subsequently emerged clearly in Dedekind's writing. He acknowledges that proofs in mathematical analysis can go astray if spatial intuition is invoked uncritically in connection with continuity (Hankel (1882) p. 51) so he aims at a theory based on concepts. But he also maintains (not at all unreasonably) that it is a mistake to avoid the spatial presentation of quantity altogether when doing real analysis (Hankel (1867) p. 46 - 47) so he incorporates a spatial "intuition of magnitude" as a reference point and strives to find "substrates in intuition" (*Substrate in der Anschauung*). The word "*Substrate*" and the objective of finding one for negative and other numbers comes from a different remark in Gauss (1831) (p.175) which Hankel also quotes (p.71). In this remark, which was also well-known and widely cited at the time, and which sets up the subsequent discussion of negative numbers on the following page, Gauss explicitly links providing such a substrate and avoiding the situation where reasoning with imaginary quantities appears to be just a "contentless game with signs" (*inhaltleeres Zeichenspiel*).

> solves the problem and which is to be operated with exactly as if it were a figure number in the series $1, 2, 3 \ldots$"
>
> [Frege comments:] Nevertheless, there is something to prevent us from regarding the difference $(2-3)$ without more ado as a symbol which solves our problem; for an empty symbol is precisely no solution; without some content it is merely ink or print on paper, as which it possesses physical properties but not that of making 2 when increased by 3. Really, it would not be a symbol at all, and to use it as one would be a mistake in logic. Even for $c > b$ it is not the symbol "$(c-b)$" that solves the problem, but its content. (Frege (1884) §95)

Is Hankel really such a simple-minded formalist? Later on even Frege doesn't claim this. In *Grundgesetze* II §145 (footnote) Frege says that it isn't clear what Hankel is, since his mode of expression suggests formalism sometimes and other times not. A few pages later, (§159), Frege says that Hankel's conception of magnitude is a geometric conception, which is hard to square with the suggestion Hankel could be merely a formalist, as, of course, are points i) - iii) above.

In fact, Hankel is not suggesting in the remarks Frege quotes that we should *rest content* with setting out rules for the sign. In the rest of the paragraph, he indicates some algebraic operations involving the sign that he wants to secure: existence of a zero, a + (-a) = 0, etc. What should the signs be understood to mean? In the paragraph *immediately after* the one Frege quotes, Hankel passes the burden on to Gauss's account of negative numbers, via *exactly the same passage* that Frege presents in *Grundgesetze* II §162.[8] That is, Frege criticizes Hankel for maintaining that negative numbers are just signs, when the same page of Hankel (1867) reveals Hankel's view to be in a fundamental respect the same as Frege's: negatives are secured through Relations that can have converses. And though Hankel can be obscure, and he says things that could look crudely formalist if quoted in isolation, he is not saying that the objects of mathematical thought just are the mathematical symbols and nothing more.[9]

[8] Well, almost exactly the same. As the editors of Frege (2013) observe, Frege misquotes Gauss by leaving out the word "whole" (*ganze*) in the final sentence.

[9] Hankel continues, indicating the value of an intuitive interpretation — a "substrate

I am not suggesting that Frege learned about Gauss's position from Hankel's book, or that Hankel's book convinced him of the value of this account of negative numbers. I would be surprized if this were true. Rather, Frege and Hankel shared an environment in which that Gauss passage was the foundation of the dominant view. As Frege notes in *Grundlagen* §1 and elsewhere, there was an extensive debate about negative numbers in the early nineteenth century: many different intellectual niches were occupied.[10] Gauss's account of negatives was salient to those in the Göttingen tradition. For example, the core idea was incorporated into Riemann's lectures on complex analysis and it was likely taught to Frege in the complex analysis courses he took from Abbe and Schering.[11] A particularly striking illustration comes from a Jena physiologist William Preyer, who was part of a salon that Frege attended.[12] When Preyer gives an account of negatives in his mathematical theory of the structure of sensation (Preyer (1877) p. 43) he quotes precisely the same Gauss passage and describes it as "well-known" (*bekanntlich*). By fastening on a poorly crafted remark, omitting its context and interpreting it uncharitably in a way that in fact clashes with other things Frege quotes, Frege not only distorts Hankel's view but does so in a way that obscures that Frege shares with Hankel and many others in his environment the fundamental Gaussian grounding principle.

3 A Perennial Scholarly Puzzle: Frege on Contextual Definitions in *Grundlagen* and Later

It is a truth universally acknowledged that the dialectical structure of *Grundlagen* §64 - §68 is perplexing. Prior to these sections, Frege engages a series of proposals for defining numbers, and finds them wanting. In §64 - §67 he appears to provide his own account, via what is now called "Hume's principle" relating the statements "F is like-numbered with G" and "The number of F's = the number of G's". The definition is underwritten by one of the

in intuition" (p.7). Though the numbers as objects of thought are not identified with the intuitive substrates, Hankel shows a vague awareness of a point that would only be worked out in detail by others (beginning more or less at the time of Hankel's writing with Houël, Beltrami and Klein): An interpretation of a set of formal rules indicates its consistency.

[10] Schubring (2005) VII ch. 3 is a useful guide to these disputes.

[11] For more on this Gauss passage as an aspect of Frege's environment, see Tappenden (2011) pp. 88 - 89

[12] On Preyer and Jena cf. Tappenden (2011) section 2.3.2

core principles Frege announces at the outset of the book: "never to ask for the meaning of a word in isolation, but only in the context of a proposition". (Frege (1884) p. X) He indicates that this suffices to legitimate contextual definitions: "It is enough that the proposition as a whole has sense; it is this that confers on its parts also its content." (Frege (1884) §60) It appears to be a done deal. And then the definition is ... *rejected* in favor of an explicit definition: "The number belonging to F is the extension of the concept 'equinumerous with F' " (Frege (1884) §68). The "official" definition is used only to prove Hume's principle, which then does all the logical work.

The shift is so rhetorically abrupt that it has been suggested that Frege had *Grundlagen* nearly completed with the Hume principle defining number but discovered problems and tacked on the new definition at the last minute.[13] Frege *eventually* rejects contextual definitions (for instance, at *Grundgesetze II* §66 and other passages considered below) but not in *Grundlagen*.[14] There he writes of contextual definition as a legitimate technique, with the qualification that limits to their generality make them inappropriate for the canonical definition of numbers. Furthermore, it doesn't appear that Frege changed his mind about contextual definition *as such* during the writing of *Grundlagen* since as we'll see in 4, Frege endorses contextual definitions again in Frege (1885), in language that seems unchanged.

Frege retains some aspects of the Hume's principle definition in *Grundgesetze* but in a modified form. He continues to define number by producing an identity between objects out of an equivalence relation between concepts (in *Grundgesetze*: functions). But instead of a general principle of contextual definition, he grounds the transition in a prior "logical law", for example in

[13]For example, Wilson (1999), who writes of "pull[ing] a switcheroo in the middle of *Grundlagen* without adequately alerting his readers to the shift." (p. 256)

[14]Whatever may have been the function of Hume's principle in *Grundlagen*'s overall dialectic, Frege does appear to have dropped it as a part of his basic foundational account, even as an expository device. Among the evidence for this is a detailed outline of a course on the concept of number for *Gymnasium* teachers that Frege taught in 1891. The course was part of a renowned teacher training seminar offered regularly by the Jena University Pedagogical Faculty. The account of the definition of number adheres closely to *Grundlagen*, but it goes directly from the account of equinumerosity as 1-1 correspondence to the explicit definition of number, with no mention of Hume's principle. (cf. Detmer (1891) p. 256 - 257) I'm grateful to David Sullivan for drawing my attention to this article, which he discusses in (Sullivan (201)).

these remarks just after dismissing contextual definitions as creative:

> If there are logical objects at all — and the objects of arithmetic are such — then there must also be a means to grasp them, to recognize them. The basic law of logic which permits the transformation of the generality of an equality into an equality serves for this purpose. Without such a means, a scientific foundation of arithmetic is impossible. For us it serves the purposes that other mathematicians intend to achieve by the creation of new numbers. (Frege (2013) Vol II, §147)

Another change from *Grundlagen* to *Grundgesetze*: in the former there is some slippage between his use of meaning/reference/*Bedeutung* ("Never ask after the meaning of a word...") and sense/*Sinn* ("It is enough that the proposition as a whole has sense ..."). Once Frege distinguishes the two sharply, he frames the semantic version of his "logical law" in terms of the two sides of the equation having the same *Bedeutung*, and he makes heroic (though ironic) exertions to ensure all expressions have *Bedeutung*. (Frege (2013) Vol I, §10, §§29-32). In connection with the "logical law", sense is wheeled in as something of an afterthought (Frege (2013) Vol I, §32).

4 The Narrow Problem: What, Precisely, is Frege's Attitude to Infinitesimals?

In his middle writings Frege twice endorses the use of infinitesimals, in both instances via a contextual definition of the differential:

> ... Only in a proposition have the words really a meaning...It is enough if the proposition taken as a whole has a sense; it is this that confers on its parts also its content.
>
> This observation is destined, I believe, to throw light on a whole series of difficult concepts, among them the infinitesimal. [footnote:] The problem here is not, as might be thought, to produce a segment bounded by two distinct points whose length is dx but rather to define the sense of an identity of the type
>
> $$df(x) = g(x)dx$$

(Frege (1884) §60)

> As far as the foundations of the differential calculus are concerned, we shall, I believe, have to go back for this purpose to the concept of a limit in the sense of algebraic analysis, and though the author belittles this as 'negative' this would seem to be due only to a misunderstanding. In my *Foundations of Arithmetic* (p. 72 [= §60] , note 1) I recently indicated how the differential can preserve a certain independence also on the kind of foundations I have chosen. (Frege (1885) 329)

In his non-foundational practice Frege uses infinitesimals in the form of differentials in both his PhD dissertation and *venia docendi* essay, (Frege (1873) p. 28 - 29, p. 38 - 40, and *passim*; Frege (1874) p. 64, 65, p. 67 and *passim*). In the former he says the differential signifies an "infinitely small motion". The uses are not superficial, but rather enter into fundamental definitions, as in section 3 of Frege (1874) where he introduces (what we now call) the infinitesimal generator of a function f and calculates with infinitesimals to ascertain f's properties.[15]

But Frege apparently has no room for infinitesimals in *Grundgezetze*. A main result in volume II is the Archimedian axiom, which excludes infinitesimals (on some understandings of them) from the structure of $\mathbb{R}$. (This fits with a familiar just-so story: development of logical foundations "banished infinitesimals", didn't it?) Furthermore, in Frege (1892) (reviewing Cantor (1890)) he writes of the Archimedian axiom as an *axiom*. As Frege uses the word (and he can get cranky about it) axioms are *evident*. Moreover, he endorses Cantor's destructive criticism (though with a qualification):

> In chapter VI Mr. Cantor deals with the question whether there are actually infinitely small numerical quantities and gives a negative answer to the question for those infinitesimals that can be mapped as limited rectilinear continuous distances. The main part of the proof, which is not carried out, I accept as valid. (Frege (1892) 271)

Interpretation problems don't come much starker: Frege appears to affirm two contradictory claims. It is possible, of course, and even likely that

[15]The indispensible guide to this and other aspects of Frege (1874) is Gronau (1997).

sometime between 1885 and 1892 Frege changed his mind, and decided that his contextual definition didn't secure even "a certain independence" for infinitesimals. Frege's views underwent significant changes as he worked out the core proofs of *Grundgesetze*. But even if we posit a change of mind, puzzles remain. Since Frege counts the Archimedian axiom as an axiom, it would be surprizing if he didn't regard it as evident from the beginning. So the clash between the axiom and his embrace of infinitesimals remains mysterious even if we restrict attention to early writings. Moreover, it is unsatisfying to resolve the conflict by positing a change of mind, if we have no idea why the change of mind occurred, or what informed the original remarks. Another reason for not resting at "changed his mind" is that the remarks in both the review of Cohen and of Cantor are qualified in ways that demand elucidation. In the earlier review, Frege writes that the foundations of calculus should use limits, which suggests a rejection of infinitesimals, before adding that nonetheless his definition preserves "a certain independence". Whatever "a certain independence" may be, it presumably falls short of "defined as an object". But what status does it indicate?

In the Cantor review, Frege restricts his discussion to Cantor's understanding of what infinitesimals might be — they "can be mapped as limited rectilinear continuous distances" — while other ways of appealing to infinitesmals are not touched.[16] Indeed, as Philip Ehrlich has pointed out, Otto Stolz replied to Cantor's argument (I paraphrase): "Very interesting. But it's irrelevant to both conceptions of infinitesimal that *I* articulated."(Ehrlich (2006) p.52-53) This observation is reinforced by the remark from *Grundlagen*: "The problem here is not... to produce a segment bounded by two distinct points whose length is dx but rather to define the sense of an identity... $df(x) = g(x)dx$". There are two (at least) different ideas whose relationship is unclear: one given by infinitesimal lengths, and the other by a class of identities.

It is one thing to say that certain concepts — "derivative", "integral" etc. — are properly defined in terms of limit rather than infinitesimal. It is quite another to say that the concept of infinitesimal is to be rejected as lacking legitimate mathematical use. Frege endorsed the former and —

[16]For example, cases where functions take infinitesimal differences in argument in Lie's "infinitesimal transformations", or indeed the applications in Frege's own non-foundational work as cited above, to consider just two instances.

at least up to 1885 — refrained from the latter. This is not odd: At the time, on this issue, many places on the map were occupied. For example, Frege's Jena colleague (as of 1879) Johannes Thomae was among those who devised and exploited infinitesimals in real analysis. (He was one of Cantor's favored targets.[17]) Thomae's contributions (in for example Thomae (1870) and Thomae (1880)) to the study of order in fields with infinitesimals (what we now call non-Archimedean fields) were often astute.[18] Thomae showed it was perfectly possible to exploit $\delta - \epsilon$ definitions of limit, derivative, integral, etc. and still deploy infinitesimals to productive ends. (In particular, in Thomae's case, to the comparison of the rates at which functions approach zero.) Frege reasonably takes for granted that most contemporaneous readers won't need this spelled out.

There were accounts presenting infinitesimals/differentials through equations similar to Frege's.[19] One came from Thomae, whose specification of differentials in a textbook on the definite integral is strikingly close to Frege's (or rather, strikingly close to what Frege's would be if Frege's addressed the two-variable case):[20]

> [If a given limit condition holds] then we say that a function $\omega(x, y)$ possesses a (first) total differential and we write:
>
> $d\omega(x, y) = \frac{\partial\omega(x,y)}{\partial x}dx + \frac{\partial\omega(x,y)}{\partial y}dy = \omega_1 dx + \omega_2 dy$
>
> (Thomae (1875) p. 37)[21]

[17]Cantor's polemics against Thomae are discussed in Dauben (1990) (p.131, 233). See also Laugwitz (2002).

[18]See Ehrlich (1995) p.199 fn.18 and at greater length in Ehrlich (2006). See also Laugwitz (2002)

[19]One example particularly close to Frege is the textbook Snell (1846) (p. 180, p. 207, p. 284 and *passim*) of his beloved teacher, though this was not a research monograph and many years had passed since it appeared.

[20]One note to avoid misunderstandings: Thomae's use of the differential to extend the Riemann integral from one to many variables (which is what Thomae (1875) (p. 37) does) and his use of infinitesimals in Thomae (1870) and Thomae (1880) are different, unconnected projects.

[21]In the far right term, Thomae merely makes explicit that the middle term may be rewritten in terms of two abbreviations ω_1, ω_2 he defined earlier.

Otto Stolz's definition of a "moment" in Stolz (1884), elaborated in Stolz (1885), is another close match.[22] (Though he adopts Newtonian terminology, there is no difference with differentials that matters here.) Stolz's goal is modest. He merely wants to show that it is possible to consistently extend $\mathbb{R}$ to include infinitely small elements. He regards the extension as not absolutely necessary, and he is open to the possibility that it may not prove to be of much value.[23]

Stolz introduces moments of functions, then specifies an ordering relation and conditions for adding and multiplying them. Division turns out to be tricky. He needs it, since he wants ordinary derivatives to be ratios of moments, so he lays down this contextual definition:

> 6. *Definition.* When $lim(f : g)$ is a positive number or $+\infty$, a thing distinct from the moments is to exist, designated by $\mathfrak{u}(f) : \mathfrak{u}(g)$, which satisfies the equation:
>
> $$\mathfrak{u}(g) \,.\, \{\mathfrak{u}(f) : \mathfrak{u}(g)\} = \mathfrak{u}(f).$$ [24]

Two points are noteworthy here. First, there is little difference in form separating Frege's 1884/1885 contextual definition of differential, and Stolz's of ratio of moments/differentials. Stolz's explorations have so many affinities to Frege's terse remarks that they could be among the resources Frege drew on when thinking through the "certain independence" available for infinitesimals/differentials.[25] Second, Frege explicitly considers and *rejects* precisely

[22]On Stolz, I'm indebted to correspondence with Philip Ehrlich as well as Ehrlich (2006) especially (pp. 14 - 20).

[23]"...the infinitely small is not at all required for the Differential- and Integral-calculus. Already for Cauchy the term infinitely small magnitude serves only to indicate for short a variable magnitude which approaches the limit zero and could be fully suppressed without leaving a gap.... one cannot expect any more from any kind of infinitely small magnitudes. Whether or not the theories developed above will have any significance in mathematics cannot be decided without doubt." (Stolz (1884) (p. 36), translation from Ehrlich (2006) (p. 17))

[24]Stolz (1885) p. 211. Stolz (1884) (p. 31) contains an essentially identical definition.

[25]The main reservation I have concerning a possible link between Frege and Stolz on the contextual definition of infinitesimals/moments concerns the timing. Stolz (1885) appeared too late to be a resource for *Grundlagen*, though Frege read it later. Stolz (1884) appeared in time to prompt Frege's quick aside and footnote, but in the low-profile proceedings of the Innsbruck natural science and medicine society. Two earlier papers in

this contextual definition in *Grundgesetze* II §143, on the grounds that it is "creative". This reinforces for the case of contextual definition of the infinitesimal/differential what ~~was~~ implied by Frege's general conditions on definitions in *Grundgesetze* II: Whatever niche Frege may have reserved for the infinitesimal/differential in in 1884/1885 has evidently vanished.

What else might infinitesimals be, if not *reeeeally* small distances? I think we should consider the possibility that for Frege, at least in 1885 and before, they don't need to be anything at all: that fixing the sense of identities of the form $df(x) = g(x)dx$ gives a sense to and a use for the expression dx but doesn't present the infinitesimal in a way that would allow one to infer equalities $dx = a$ with objects that didn't come presented in the form dx or $df(x)$. That is, the contextual definition of infinitesimal doesn't allow one to "recognize [it] as the same again [if] it is given in a different way" (modified from Frege (1884) §67). This would mean that, for Frege, the "certain independence" would not include being counted as an *object*, since "If the symbol a is to denote an object for us, we must have a criterion that will in every case decide whether b is the same as a, even if it is not always within our power to apply this criterion." (Frege (1884) §65).[26]

---

the higher-profile *Mathematische Annalen* prefiguring some of the themes in Stolz's 1884 paper would likely have caught Frege's attention: Stolz (1881) re-introducing German mathematics to Bolzano's foundations of analysis and Stolz (1883), which discusses a variety of foundational points in light of ancient mathematics, including Dedekind on continuity and Thomae on infinitesimals. But neither has Stolz's definition of ratio of moments. Of course, Frege could have learned of Stolz's work on moments from Thomae, or through correspondence.

Though of course it can't be assumed without tangible evidence, such communication could easily have taken place, perhaps even via a personal connection tracing back to Frege's student days. Stolz visited Göttingen during Frege's time there, and both attended Clebsch's lectures on geometry.(Robinson (2008)) Stolz was an active participant in the circle around Clebsch, as indicated, for example, in his correspondence with Felix Klein. (Binder (1989)) During the period both were at Göttingen Stolz published Stolz (1871) in the Göttingen journal *Mathematische Annalen* on Frege's ultimate thesis topic, the representation of extension elements in geometry. So there would have been occasion and reason for them to have established lines of communication. Telling against the suggestion that Stolz and Frege communicated more than cursorily is Frege's complaint in the forward to *Grundgesetze* I that Stolz "seem[s] not to be aquainted with my works". (p. XI)

[26]Indeed, when Stolz (Stolz (1888) p.603) struggled to explain precisely what role his contextual definition secured for moments, he appealed to different ways of saying essentially that the signs need not signify objects.

Despite the Caesar problem, Frege still endorses contextual definition in 1885. This is not inexplicable. Frege's model is a recognized pattern of argument in geometry, and he appears to have no reservations about its informal use. It would be a significant step to reject the construction out of hand. In later years Frege might be willing to simply jettison flourishing branches of mathematics for failing to meet his foundational standards, but up to and during the *Grundlagen* period he was more tentative.

But whatever Frege may have been drawing on in 1884/1885, his verdict in 1903 is unambiguous. In the continuation of *Grundgesetze* II §143 Frege asks how one could know that such stipulations could be consistent unless one *produced the object* they are supposed to be stipulations about. In the next section we'll find Frege discussing a definition he regards as doing better.

5 Presenting Objects and "Extent of Validity": The Riemann Integral

At the beginning of the unpublished "Logical Defects in Mathematics" — apparently originally intended for *Grundgesetze II* — Frege makes illuminating remarks about the definition of integral in Riemann's *Ueber die Darstellbarkeit einer Function durch eine Trigonometrische Reihe* (Riemann (868a)).[27] First Frege quotes Ludwig Scheeffer: "... according to the Riemannian definition of the definite integral, $\int_{x_0}^{x_1} \sqrt{1+f'(x)^2}\,dx$ acquires no meaning."[28] Frege complains, as a regular Frege reader would expect, that this is confused: *linguistic signs* have meaning, not integrals. But he adds the unexpected step of making sure Riemann himself is not convicted of this confusion! The regular Frege reader looks on with astonishment as Frege goes out of his way to read Riemann charitably. He quotes Riemann's definition and notes that

[27] I follow the editors of *Posthumous Writings* (see p. 157 fn. 1) in viewing Frege (1898) as originally intended for *Grundgesetze II*, in particular as it uses "the first volume" to refer to *Grundgesetze* I (Frege (1898) p. 163).

[28] Scheeffer (1884) p.49 quoted in Frege (1898) p. 157 - 8. It is worth a passing note, as a corrective to the tendency to take Frege's disdain for contemporaries at face value, that the Scheeffer article was historically significant in the development of the concept of integral, and indeed the phrase Frege carps at conveys a deep observation. $\sqrt{1+f'(x)^2}$ yields a curve that intuitively has a length, but the Riemann integral that should be providing that length is undefined. This was a spur to the development of definitions of the integral, such as Lebesgue's, applying to a wider range of curves. cf. Hawkins (1975) ch. 3.4

Riemann's words do indeed, strictly speaking, run together sign and object signified, but Frege excuses Riemann because "his sense is made clear enough" (p. 158) and "This sickness [of confusing sign and signified] may well not have been so prevalent when Riemann wrote his article".

Frege puts Riemann's definition of definite integral forward as a model correct definition[29] while the article as a whole is an extended criticism of mathematicians for inadequate definitions, notably the concepts of function, variable, and in a closing cutlass slash, "power series":

> How often is the phrase 'power series' used! But what do we understand by it? What is a power? What is a series? People don't question whether these things are configurations which men produce with writing implements, possessing physical properties, or whether powers, series and power series are only designated by such configurations, but are themselves non-spatial and invisible . . . And yet this indifference! . . . [A]nyone using a word such as 'number', 'function' or 'power series' should by rights state what he understands by it. (Frege (1898) p. 165)

This contains an implicit contrast and declaration of allegiance that Frege's readers would have recognized (especially if "Logical Defects . . . " were packaged with the interminable heavy-handed criticism of Weierstrass in *Grundgesetze II*).[30] Power series are the building blocks of Weierstrass's complex analysis: he defines the fundamental concept of "analytic function" as precisely those functions of a complex variable with power series representations. This stood in well-known contrast with Riemann, who defined the equivalent concept of complex differentiable function in terms of conditions (now called the Cauchy-Riemann equations) on the functions themselves.[31] This difference

[29]Of course, Frege expects that the concepts of real number, number, etc. that contribute to the definition should also be defined.

[30]It is possible that Frege had further thoughts along these lines that have been lost, as the *Nachlass* catalogue lists one page on power series and a forty page notebook including "Bedeutung des Integrals: Scheefer [sic], Riemann". The catalogue also lists an undated 53 page notebook on "*Bestimmte Integrale*" (Definite Integrals) plus 6 pages on "*Bemerkungen über die Vertauschbarkeit der Integrale*" (Remarks on the commutativity of integrals) from October 1896. Veraart (1976) p.102 - 103

[31]The Riemann definition goes this way. A complex function f may be decomposed into two real-valued functions u and v with $u, v : \mathbb{R}\times\mathbb{R} \to \mathbb{R}$, with $f(x+iy) = u(x, y)+iv(x, y)$.

was not incidental, rather it reflected a fundamental divergence in method.[32]

Frege's concern for rigor is often viewed narrowly as a concern for certainty, to avoid "encounter[ing] a contradiction that brings the whole edifice down in ruins." (Frege (1884) p. IX) But that's far from his sole concern. He also wants to get definitions right to delineate "limits to the validity" (*Gültigkeitsgrenzen*) of propositions:

> [Rigorous proofs often reveal] more precisely the conditions restricting the validity (*Bedingungen der G[ü]ltigkeit*) of the original proposition. (Frege (1884) p.3)
>
> Proof is now demanded of many things that formerly passed as self-evident. Again and again the limits to the validity (*die Grenzen der G[ü]ltigkeit*) of a proposition have been in this way established for the first time. (Frege (1884) §1)
>
> In all directions the same ideals can be seen at work rigour of proof, precise delimitation of limits of validity (*G[ü]ltigkeitsgrenzen*), and *as a means to this*, sharp definition of concepts. (Frege (1884) §1) (my emphasis)[33]

Anyone familiar with Riemann (868a) (and that included virtually every German mathematician) would recognize this language.[34] In that article Riemann repeatedly says that one of his objectives is to establish "the extent of validity" (*den Umfang seiner Gültigkeit*) of the concept of definite integral, including in the title and the first sentence of the chapter in which he gives

---

f is counted as complex-differentiable if $\frac{\partial u}{\partial x} = \frac{\partial u}{\partial y}$ and $\frac{\partial v}{\partial x} = -\frac{\partial v}{\partial y}$ (partially differentiating u and v as real functions). Only the intrinsic behavior of the function, and not any features of the representation are used.

[32] I discuss the contrast in the Weierstrass and Riemann conception of mathematics in connection with Frege in Tappenden (2006). Note that another Göttingen/Berlin contrast is implicit in Frege's ms: Scheeffer was a Berlin PhD claiming (not unreasonably) to have found a weakness in Riemann's definition of integral. The article appeared in *Acta Mathematica*, edited (and newly founded) by Weierstrass acolyte Gösta Mittag-Leffler.

[33] I discuss these turns of phrase and further overtones in connection with Riemann in Tappenden (2006) pp.118 - 120

[34] The Riemann article would have been especially salient to Frege because he taught "Definite Integrals and Fourier Series" in Winter Semester 1878/79 and 1880/81. (Kreiser (2001) p. 280). Riemann's article was the definitive treatment of those two connected topics.

his definition.[35] On the next page Riemann ties the extent of validity of the integral to sharp definition: "[W]e'll examine now the extent of validity of the concept [of definite integral] or the question: In which cases is a function integrable and in which not?"[36] In the introductory paragraph, he states that the paper will study the representation of functions via trigonometric series, after first completing a preliminary examination of the concept of definite integral and the *Umfang seiner Gültigkeit* (p. 227).[37] Frege cites "extension of the validity of the concept 'a function is integrable' " when (approvingly) listing examples of "expressions found in Riemann's writings". (Frege (1898) p. 158)[38]

A closer look at the Riemann definition itself reveals a further connection. The first few sections of Riemann's paper concern the history of the concept of *function*. He recounts the transition from functions as analytic expressions (Euler, Cauchy) to his and Dirichlet's concept of arbitrary correspondence.[39] This is, of course, important background for Frege's treatment of functions, but I'll leave that for elsewhere. The broader conception of function requires a new definition of integral. If functions are given by analytic expressions, their integrals can be given by rules stated in terms of those expressions.[40]

[35] The chapter is titled "*Ueber den Begriff eines bestimmten Integrals und den Umfang seiner Gültigkeit*". (Riemann (868a) p. 239) Note also p. 265

[36] p. 240 "*Untersuchen wir jetzt zweitens den Umfang der Gültigkeit dieses Begriffs oder die Frage: in welchen Fällen lässt eine Function eine Integration zu und in welchen nicht?*"

[37] For a variety of reasons grounded in Riemann's methods (for example the systematic exploitation of interactions between local and global properties and his concern for applications), such turns of phrase often cropped up in writings by or about Riemann. For one example, in a work on potential theory (Betti (1885) p.VII), the author (+ translator) states that he omits the Dirichlet principle because the limits to its validity (*die Grenzen seiner Gültigkeit*) have not been established. The phrase is repeated verbatim in the *Jahrbuch über die Fortschritte der Mathematik* review of the book in 1886: "*Das Dirichlet'sche Princip ist, da die Grenzen seiner Gültigkeit noch nicht festgestellt sind, nicht benutzt.*" Another example: In his renowned article on the distribution of prime numbers, Riemann places stress on replacing a function defined only on the upper half of the complex plane with "an expression of the function which is everywhere valid (*immer gültig*)." (Riemann (1974) p.299)

[38] "*Umfang der Gültigkeit dieses Begriffs 'eine Funktion lässt eine Integration zu'* "

[39] This fits with yet another tacit Riemann/Weierstrass contrast in "Logical Defects ...", since the ms contains critical remarks on Berlin-affiliated Eduard Heine's treatment of functions as expressions (p. 164).

[40] At times Cauchy would define function and integral more broadly, without reference to

For example, this template for one-variable polynomials:

$$\int(a_nx^n+a_{n-1}x^{n-1}+\ldots+a_1x+a_0)dx = \frac{a_nx^{n+1}}{n+1}+\frac{a_{n-1}x^n}{n}+\ldots+\frac{a_1x^2}{2}+a_0x+C$$

This says nothing about coextensive functions not presented exactly this way.

Riemann gives the representation-independent definition familiar today from school calculus. Slightly modernized: A function f is (Riemann) integrable and $\int_a^b f(x)\,dx = s$ iff:

$\forall\epsilon > 0\,\exists\delta > 0$ s. t. for any partition $a = x_0 < x_1 < x_2 < \cdots < x_n = b$ and arbitrary $t_i \in (x_i, x_{i+1})$:
if $max|x_{i+1} - x_i| < \delta$ then $\left|s - \sum_{i=0}^{n-1} f(t_i)(x_{i+1} - x_i)\right| < \varepsilon$.

That is: if a limit s exists as the partitions are arbitrarily refined, then $\int_a^b f(x)\,dx = s$.

Riemann gives a sharp definition of definite integral, not in terms of properties of signs but rather (as Frege would put it) as an object. By doing things this way, Riemann secures the generality/"extent of validity" of the construction.

Frege's discussion of Riemann's definition would have been read against the background of a recognized methodological debate over mathematical definition. Mathematicians in the broadly Riemann stream of research viewed defining mathematical objects as objects, rather than in terms of particular symbolic representations (such as power series) as advantageous for, among other reasons, mathematical fruitfulness.[41] (Dedekind was particularly insistent on this.) On this view, presenting/defining objects in a representation-independent way is better *mathematically*. Among the reasons cited was

expressions, but the broad definitions were idle. As Dirichlet observed, Cauchy generalized functions from real to complex arguments in a way that only makes sense if the "function as analytical expression" concept is presupposed. cf. Hawkins (1975) p. 10 - 11

[41]I discuss this aspect of Riemannian methodology in connection with the Caesar problem in Tappenden (2005), comparing Dedekind's approach to ideal numbers, and (again with reference to Dedekind) in Tappenden (2008). Another resource on Dedekind is Avigad (2006).

that this approach allows multiple perspectives on a problem. See for example these framing remarks by an editor of Riemann's lectures on elliptic functions:[42]

> ...[Riemann's methods] bring out in a flexible way the essential properties of the elliptic functions ... build[ing] 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 guarantee[ing] a multifaceted view into the nature of the problem and the variety of its solutions. (Stahl (1899) p.III)

6 *Grundlagen*'s Discussion Revisited: Restricted Definitions and Introducing Objects

The Julius Caesar problem has been viewed as mathematically inert - just a philosopher's problem. It is true that by striving for such complete generality Frege pushed farther than most mathematicians would think worthwhile. But as we've seen, Frege's words would have been read against the background of a methodological debate for which the Caesar problem represented an unusually pure extreme. With this in mind, let's revisit *Grundlagen* just before Frege wheels the Caesar problem in, to review his reservations about the Hume's principle definition.

Frege takes the abstractive definition to have several related flaws that he details in §§65, 66 and 67.[43] Here's the first point:

> In order, therefore, to justify our proposed definition of the direction of a line, we should have to show that it is possible, if line a is parallel to line b, to substitute
>
> "the direction of b"
>
> everywhere for

[42]In this passage, Stahl is addressing Riemann's work in complex analysis, rather than real analysis. However, the methodological principles Stahl is describing were taken to be characteristic of Riemann's approach to mathematics *tout court*.

[43]The most thorough exploration of the geometric background of such abstraction principles is in Mancosu (201) ch. II

"the direction of a"

> ... [W]e are being taken initially to know of nothing that can be asserted about the direction of a line except ... that it coincides with the direction of some other line. We should thus have to show only that substitution was possible in an identity of this one type, or in judgement-contents containing such identities as constituent elements. The meaning of any other type of assertion about direction would have first of all to be defined, and in defining [we must ensure] that it must remain possible to substitute for the direction of any line the direction of any line parallel to it. (Frege (1884) §65)

That is: the definition licenses substitutions of equals, but the occasions to use this license will be limited since the only statements known about directions are identities of the type just defined (either free-standing or as components of larger sentences). To extend the use of "direction of ( )", the correctness of the substitutions in the *extended* context must be ensured. In §66 Frege complains that the definition doesn't resolve "The direction of $a = q$" for arbitrary q. In §67 he considers allowing the equations to be true *only* for q introduced by means of this definition. This will fail to introduce an object because it makes the candidate "object" too dependent on the specific representation:

> 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 it, which it is not...[and furthermore] we should be presupposing that an object can only be given in one single way; for otherwise it would not follow, from the fact that q *was* not introduced by means of our definition that it could not have been. (Frege (1884) §67)

So this representation-dependence is incompatible with being an object. Frege reaffirms that the way an object is given must not be regarded as its immutable property in *Grundgesetze* I (§10 second footnote), in connection with the introduction of value-ranges. "OK", asks an interlocutor: "So it's not an object. So what?" Frege's reply appeals to the mathematical fruitfulness of introducing something as an object, thereby widening the range of validity:

> All equations would amount to this, that whatever is given to us in the same way is to be acknowledged as the same. But this is a principle so obvious and so unfruitful as not to be worth stating. In fact, no conclusion could be drawn that would be different from one of the premises. The versatile and meaningful usability of equations is based rather on the fact that you can re-identify something though it is given in a variety of ways. (Frege (1884) §67, I've modified Austin's translation)

The drastic hack of only considering directions as introduced by one specific definition could underwrite a consistent use, but it would be mathematically sterile, useless for real extensions of knowledge in which conclusions have genuinely different content from the premises. In evaluating Frege's remarks about the unfruitfulness of such restricted definitions, bear in mind that as Frege sees it, inferences can be worthwhile for psychological reasons even if they don't "extend knowledge". This is most clearly indicated in Frege (1880), a discussion that is reflected in a cryptic passage at §88 of *Grundlagen*.[44] Frege contrasts Boolean algebra and the *Begriffsschrift*: the former does not support inferences that extend knowledge, which "is surely responsible for the impression one gets in logic that for all our to-ing and fro-ing we never really leave the same spot."(Frege (1880) p. 34) Even granting the contrast, with *Begriffsschrift* on one side and Boolean algebra on the other, this leaves a significant role for the "to-ing and fro-ing". Frege isn't claiming that useful clarification can't be achieved by rearranging Boolean combinations of properties!

The hedged claim to "preserve a certain independence" for the differential might signal just a useful field-specific device providing abbreviations and simplifications, and nothing more. That could solve the textual problem of sorting out the apparently conflicting remarks on infinitesimals. By giving a sense to the equation $df(x) = g(x)dx$ we make a limited range of potentially useful substitutions available, but we would not "introduce an object". This helps explain why the definition of number modeled on the definition of direction has to be set aside in favor of the definition providing sharp boundaries, even though the definition seemed to have satisfied the requirement of "giving sense to an identity". For a concept with the general applicability

[44] I discuss this idea of "extending knowledge" and its interaction with Frege's idea of "fruitful concepts" in Tappenden (1995)

and importance of the concept of number, such restrictions on the extent of validity of equations are unacceptable. And finally, we can better locate what has changed between the hedged and ambivalent endorsement of contextual definitions up to 1885 and the rejection of Stolz's contextual definition of ratio of moments in 1903. For whatever cluster of reasons, Frege has ceased to regard this niche of "a certain independence/but not picking out an object" as tenable.

Whatever the motive, the contextual definition is indeed set aside, as announced in the first sentence of §68 preparing to introduce the Caesar problem. Studies of the Caesar problem in the scholarly literature have revealed suprising intricacy, with both semantic and epistemological issues in play.[45] The objection from the unfruitfulness of restricted definitions is of yet a further type: an objection from *mathematical method*, which reverberates with the discussion in 5 of the mathematical advantages of defining the Riemann integral in a representation-independent way. Securing the contextual definition of number/direction through limiting equalities would come at the cost of restricting the mathematical insights that those definitions could support. The price, reckoned in terms of the "extent of validity" of the definition, would be stultifyingly high.

It wasn't unreasonable for Frege to strive to secure generality for equations just from a mathematical point of view, independent of philosophical analysis. It's easy to find instances in Frege's environment where profound insights depended on identifying objects presented in drastically different ways, even crossing disciplinary boundaries to do so. To consider just one: In Riemann (1857), Riemann defined a number that we now call the *genus* of a surface in topological terms (connectivity on a surface). In the 1860's Clebsch gave a definition in algebraic-geometric terms, appealing to the degree of the curve and the number of certain singularities (cusps and double points). The discovery that Riemann's number and Clebsch's were identical was an enormously illuminating breakthrough that forged a bond between profoundly different approaches to surfaces. Frege regards introducing directions, numbers, etc. as objects as part of what is needed to secure such

[45] I've learned from more writers than I could list here about these dimensions of the Caesar problem, but I should mention that for this essay I've found Heck (2011) ch. 4, 5, 6 especially helpful.

subfield-transcending discoveries.

7 Identities Fixing Sense Without Providing Reference: An Example from Frege's Environment

I've noted that at least up to 1885 Frege seems to accept a limited role for definitions that fail to "present an object", for example the contextual definition securing "a certain independence" for the infinitesimal. But won't the role be so limited as to have the practical effect of banning such definitions altogether? Why would Frege think that equations could in principle work that way? One reason is detailed in this section: in an area of mathematics Frege was undoubtably familiar with, families of equations worked exactly that way. Though the applicability was limited, the equations were useful and even practically indispensible in that restricted domain. We have at least that much reason to think Frege would have regarded such a pattern as potentially useful: it was already being put to productive use.

Clebsch-Aronhold symbolic notation perfected by Frege's Göttingen teacher Alfred Clebsch was a set of "identities" to simplify intricate expressions arising in invariant theory. A simple illustration will suffice here. *Binary forms* are two-variable polynomials that are *homogeneous*, meaning that for each term, the exponents of x and y sum to a constant, as in the general binary form of degree n:

$$f_n(x) = \bar{a}_0 x^n \, + \, \bar{a}_1 x^{n-1} y \, + \, \bar{a}_2 x^{n-2} y^2 \, + \ldots + \, \bar{a}_n y^n$$
$$(a_i \neq 0 \, \forall i \in \{0, \ldots n\})$$

If the form is well-behaved, it will be the nth power of a linear factor:

$$f_n(x) = \bar{a}_0 x^n \, + \, \bar{a}_1 x^{n-1} y \, + \, \bar{a}_2 x^{n-2} y^2 \, + \, \ldots + \, \bar{a}_n y^n = (a_1^* x + a_2^* y)^n$$

Things will be that simple only when the algebra gods are feeling *very* benevolent. The idea of symbolic notation is to *create* the linear factor by brute force, through a set of equations. First a preparatory rewriting. To get the form to more closely resemble a power of a linear factor, instead of $\bar{a}_i$, write (with no loss of generality) $n_i \bar{a}_i$, where $n_k = \frac{n!}{k!(n-k)!}$, the binomial coefficient:

$$f_n(x) = \bar{a}_0 x^n + n_1 \bar{a}_1 x^{n-1} y + n_2 \bar{a}_2 x^{n-2} y^2 + \ldots + n_{n-1} \bar{a}_n x y^{n-1} + \bar{a}_n y^n$$

Then, as Osgood (1892) (p.252) puts it: "we may write *symbolically*" (his emphasis):

$$\bar{a}_0 = a_1^{*n},\ \bar{a}_1 = a_1^{*n-1} a_2^*,\ \bar{a}_2 = a_1^{*n-2} a_2^{*2}, \ldots, \bar{a}_{n-1} = a_1^* a_2^{*n-1},\ \bar{a}_n = a_2^{*n}$$

These equations yield the desired simplification: $f_n(x) = (a_1^* x_1 + a_2^* x_2)^n$ The qualification "symbolically" reflects that *only in a very restricted class of contexts do these substitutions make sense*:

> [I]n any expression in which the $\bar{a}$'s enter linearly, they may be replaced by their symbolic representatives . . . The symbols a_1^*, a_2^* taken by themselves, have no meaning in terms of the coefficients $\bar{a}$; only when combined in expressions of degree n in a_1^*, a_2^*, are they capable of interpretation in terms of the $\bar{a}$'s. (Osgood (1892) pp. 251 - 2)[46]

The coefficients are, in Russell's phrase, incomplete symbols. They have no meaning except as contributing to formulae in which they appear.

Osgood's words are not idiosyncratic. This is how symbolic variables were understood and discussed. Clebsch himself, in his 1872 textbook *Theorie der Binären Algebraischen Formen* says that symbolic variables not occurring in the circumscribed contexts "have no meaning" ("*haben gar keine Bedeutung*") (Clebsch (1872) p. 30; p. 31) and "no definite real interpretation" ("*keine bestimmte reale Deutung*") (Clebsch (1872) p. 31); In another standard textbook (Gordan (1887) p. 10) we read that the technique is a "pure symbolic process" (*rein symbolischer Process*) that yields "*keine Bedeutung*".[47]

[46] I have changed the names of the symbolic variables in some quotes to give uniformity to the presentation.

[47] This way of speaking was preserved. In a 1931 textbook we read: "where the symbols a^* are meaningless except in products of n together, in which case they mean the actual coefficients with corresponding subscripts." (Coolidge (1931) p. 79) More recently, in a textbook presentation meeting contemporary standards of rigor: "Symbolic variables by themselves have no real meaning; it is only when they occur in the particular power products of degree n that they acquire an actual value in terms of the coefficients of our binary form." (Olver (1999) p. 112)

The restrictions on the use of the equations produces the situation Frege describes in *Grundlagen* §65: All we are taken to know at the outset about the direction of a line [symbolic coefficient] is that it coincides [coincides in certain products] with the direction of some other line [with some other coefficient]. Only substitition "in an identity of this one type, or in judgement-contents containing such identities as constituent elements" is allowed. For symbolic notation, the restriction was explicitly recognized:

> Although [degree m form f] is not an exact power, we assume the privilege of placing it equal to the mth power of a purely symbolical linear form $f = (a_1^* x_1 + a_2^* x_2)^m$...
> This may be done provided we assume that the only defined combinations of the symbols a_1^*, a_2^*, that is, the only combinations which have any definite meaning, are the monomials of degree m in a_1^*, a_2^*:
>
> $\bar{a}_0 = a_1^{*n},\ \bar{a}_1 = a_1^{*n-1} a_2^*,\ \ldots, \bar{a}_n = a_2^{*n}$
>
> and linear combinations of these. Thus $a_1^{*m} + 2a_1^{*m-1} a_2^{*2}$ means $\bar{a}_0 + 2\bar{a}_2$. But $a_1^{*m-2} a_2^*$ is meaningless; (Glenn (1915) p.54)

Despite the restrictions symbolic notation proved valuable for dealing with properties left unchanged by classes of substitutions (i.e. the *invariants* of the forms).[48] Using the symbolic techniques Gordon obtained a finite basis generating all invariants and covariants of binary forms. Despite the logical oddity and the limited extent of validity, the technique would have appeared practically indispensible.[49] Giving the method up would be to abandon nearly the entire field of algebraic invariant theory as it then stood in Germany.

Frege would have had sufficient familiarity with the Clebsch-Aronhold notation to recognize the match with the discussion in *Grundlagen* §§64 - 67. This was a central research area, pioneered by one of his Göttingen teachers. Clebsch (1872) and a shorter presentation Clebsch (1871) in the Göttingen journal appeared while Frege attended two courses of Clebsch's

[48] For the curious, there is a clear illustration of the use of symbolic notation to present a simple invariant (the discriminant of a binary form) in Osgood (1892) (p. 253 - 4), with more intricate examples following.

[49] "This notation must be mastered by those who would go deeply into the theory of invariants and its applications." Dickson (1914) p. vi

geometry lectures. Behind the algebraic clothing, the theory of n-ary forms was a theory of curves and surfaces. It was consistently deployed in Clebsch's geometry lectures, to judge from Clebsch (1876), a textbook based on three Clebsch lecture courses held during Frege's time at Jena.[50] (Frege attended at least one).[51] Frege retained the notes he took in Clebsch's courses and borrowed the published lectures from the library on two occasions before 1884.[52]

Frege's research and teaching history reinforces the last paragraph. In 1877 he gave a lecture "*Über Invarianten*" to the Jena mathematical society. (Frege (1983) p. 378 - 380) The talk didn't require Clebsch-Aronhold notation, but this shows invariants to be in his early research sights. In the descriptions of his research seminars he occasionally abbreviates with symbolic notation.[53] In the seminars Frege regularly treats simple invariants: the discriminant of a binary form, the determinant, the cross-ratio,... His seminar covered determinants in 1883 - 84 just before the appearance of *Grundlagen*, and in 1895 - 96 it addressed *inter alia* the discriminant of a second degree equation in the context of studying the invariants and covariants of conics.[54] Preparing these topics he would have consulted the notes from, and textbook based on, Clebsch's classes where he would have been immersed in these techniques.[55]

More external evidence could be stacked up, but there's enough to secure the point: Frege had a practicing mathematician's familiarity with a technique that put forward equations to contextually define new expressions. The

[50]For one of many uses of the symbolic method in Clebsch (1876) see pp. 183 - 95.)

[51](Bynum (1972) p.3 fn. 4, Clebsch (1876) p. V)

[52]On retaining the lecture notes see Veraart (1976), (p.102). On this and other mentioned library borrowings see Kreiser (1984) p. 21. Frege also borrowed Clebsch and Gordan (1866) on Abelian functions, which further suggests an immersion in Clebsch's work; Frege presumably read it attentively since he gave lecture courses and led seminars on Abelian functions and the special case of elliptic functions on many occasions.

[53]For these descriptions, see Kreiser (2001) pp. 302 - 20. Among the Frege's uses of symbolic abbreviation in these seminar descriptions a particularly clear example is the seminar description for 1890 - 91 where he uses abridged notation throughout, and writes of "symbolic relations for linear equations" (*symbolische Bezeichnung für lineare Gleichungen*). cf. Kreiser (2001) p.306

[54]Frege (1983) p.345, Kreiser (2001) p. 302 p.312

[55]Consider, as an illustration, the discussions of determinants woven through the treatment of invariants in Clebsch (1876) pp. 167 - 195

equations and new expressions were practically important but fell short of "introducing an object". Because the symbols don't designate objects, there are limits to what one can do with them. You can't consider the properties of an object denoted by the symbol from a variety of different point of view. But in their limited domain, they do what they are supposed to do, and do it well.

Perhaps this is the sort of practice Frege was alluding to, when he wrote cautiously of conferring "a certain independence" via a contextual definition of infinitesimal/differential. If so, there's an intelligible pattern to Frege's shift from qualified to outright rejection: in 1884 he regarded defining objects with sharp-boundaried definitions as optimal, but he recognized that many successful mathematical practices fell short of that ideal, without any obvious way of reframing the practices to match the ideal. In 1884/1885, he wasn't prepared to reject these practices out of hand, hence §60 - §68 has its bifurcated character: he concedes that contexual definitions can serve some purposes and gestures at what the purposes are, but he also indicates that they are sub-optimal for reasons of both logic and mathematical fruitfulness. Then he provides a definition that does have the virtues he demands. Hence the equivocal rejection of Hume's principle, hence the hedged endorsement of contextual definition for infinitesimals. Subsequently, the story could run, Frege's attitude to practices he judged to be sub-optimal hardened. Once he sharply frames the issue in terms of *Sinn* and *Bedeutung* he rejects using terms with no *Bedeutung*, noting the confusions that arose from divergent series.[56] (Frege (892b) p.169, Frege (891a) p.148) Hence the outright rejection of Stolz's contexual definition. I think this account is plausible as an outline, but the main conclusion I draw is that we need to know more about what shared knowledge Frege took for granted among his readers before we can feel confident we understand what is going on.

[56]It is worth noting that in Frege's environment the rejection of divergent series was also a rejection of certain equations that were not identities. In the early nineteenth century, the field of "algebraic analysis" developed some ideas implicit in Euler's treatment of divergent series by distinguishing equality (holding between numbers) and a formal equality which could relate expressions for divergent series and their generating functions. (For example, a textbook whose auther was a full professor of mathematics at Göttingen during Frege's time there used distinct symbols for ordinary equality and formal equality. cf. **?** p. 29 and *passim*) cf. **?**, or for more detail Jahnke (1990).

8 In Short

The best-known nineteenth century research on foundations of analysis — that of Dedekind and Cantor — was bound up with non-foundational research, which suggests that we can expect to find Frege's foundational treatment of $\mathbb{R}$ to engage with active non-foundational research as well. And indeed, in two definitions pertaining to analysis - the definition of infinitesimal and that of definite integral - Frege engages with the mathematical environment in a way that blurs the boundary between philosophical and mathematical motives. He accepts contextual definitions with reservations in 1884-85, perhaps because through them some mathematical devices of proven usefulness could be retained. (I've suggested that symbolic notation was one of these, but even setting it aside, Frege's tone indicates that he regards "a certain independence" for the infinitesimal as worth preserving, not merely achievable in principle.) A reason he cites in §67 for eschewing the option in the case of numbers is the mathematical fruitfulness of defining something as an object, combined with the centrality of the concept of number.

Even an aspect of Frege's work that as much as any has been taken to be a paradigm of mathematically inert philosophizing - the Julius Caesar problem - turns out to have ramifications for known methodological debates in Frege's environment. Though the specific case of Julius Caesar no doubt seemed inconsequential for mathematical method, the contrast between representation-dependent and representation-independent definitions did not. We should sustain this perspective as we survey the largely uncharted territory of the context and philosophical motivations for Frege's account of magnitude: the upshot will be a more satisfying, multi-dimensional account of Frege as a foundational thinker, engaged not just with his conception of foundations of mathematics, but with mathematics itself and its methodology.

References

Avigad, J. (2006). Methodology and metaphysics in the development of Dedekind's theory of ideals. In Ferreiròs, J. and Gray, J., editors, *The Architecture of Modern Mathematics*, pages 159 – 186. Oxford University Press.

Baumann, J., editor (1868). *Die Lehren von Raum, Zeit und Mathematik in der neueren Philosophie nach ihrem ganzen Einfluss dargestellt und beurtheilt*, volume 1. Berlin: Georg Reimer.

Baumann, J., editor (1869). *Die Lehren von Raum, Zeit und Mathematik in der neueren Philosophie nach ihrem ganzen Einfluss dargestellt und beurtheilt*, volume 2. Berlin: Georg Reimer.

Betti, E. (1885). *Lehrbuch der Potentialtheorie und ihrer Anwendungen auf Elektrostatik und Magnetismus*. Kohlhammer, Stuttgart. German translation by W.F. Meyer.

Binder, C. (1989). Über den Briefwechsel Felix Klein - Otto Stolz. *Wissenschaftliche Zeitschrift. Mathematisch-naturwissenschaftliche Reihe. Ernst-Moritz-Arndt-Universität Greifswald*, 38(4):3–7.

Bynum, T. W. (1972). On the life and work of Gottlob Frege. In Bynum, T. W., editor, *Conceptual Notation and Related Articles*. Oxford: Oxford University Press.

Cantor, G. (1890). *Zur Lehre vom Transfiniten*. Halle: C. E. M. Pfeffer.

Clebsch, A. (1871). Zur Theorie der binären algebraischen Formen. *Mathematische Annalen*, 3:265 – 267.

Clebsch, A. (1872). *Theorie der Binären Algebraischen Formen*. Leipzig: Teubner.

Clebsch, A. (1876). *Clebsch A. and Lindemann F. [1876] Vorlesungen Über Geometrie*, volume 1. Leipzig: Teubner. (Lectures by Clebsch transcribed and edited by Ferdinand Lindemann).

Clebsch, A. and Gordan, P. (1866). *Theorie der Abelschen Funktionen*. Leipzig: Teubner.

Coolidge, J. L. (1931). *A Treatise on Algebraic Plane Curves*. Oxford Univ Press.

Dauben, J. (1979/1990). *Georg Cantor: His Mathematics and Philosophy of the Infinite*. Princeton University Press, Princeton. 1990 Reprint of 1979 Harvard University Press publication.

Detmer, W. (1891). Die fortbildungskurse an der universität jena für lehrer Deutschlands, österreichs und der Schweiz. *Pädagogisches Archiv*, 33(4):252 – 61.

Dickson, L. (1914). *Algebraic Invariants.* New York: John Wiley and Sons.

Dummett, M. (1991). *Frege: Philosophy of Mathematics.* Harvard University Press, Cambridge, Mass.

Ehrlich, P. (1995). Hahn's "*Ueber die Nichtarchimedischen Größensysteme*" and the development of the modern theory of magnitudes and numbers to measure them. In Hintikka, J., editor, *From Dedekind to Gödel: Essays on the Development of the Foundations of Mathematics*, pages 165 – 213. Dordrecht: Kluwer Academic Publishers.

Ehrlich, P. (2006). The rise of non-Archimedean mathematics and the roots of a misconception I: The emergence of non-archimedean systems of magnitudes. *Archive for History of Exact Sciences*, 60:1 – 121.

Frege, G. (1873). *On a Geometrical Representation of Imaginary forms in the Plane.* PhD thesis, Göttingen. Translated by Hans Kaal in Gottlob Frege *Collected Papers* Brian McGuinness (ed.) Oxford: Basil Blackwell 1984 pp. 1 - 551 second page references to this translation.

Frege, G. (1885). Review of H. Cohen *Das Prinzip der Infinitesimal-Methode und seine Geschichte. Zeitschrift für Philosophie und philosophische Kritic*, 87:324 – 9. Translated by Hans Kaal in Gottlob Frege *Collected Papers* Brian McGuinness (ed.) Oxford: Basil Blackwell 1984 pp. 108 - 111 second page references to this translation.

Frege, G. (1891a). *Funktion und Begriff.* Jena: Hermann Pohle. Translated as "On Sense and Meaning" by Peter Geach in Gottlob Frege *Collected Papers* Brian McGuinness (ed.) Oxford: Basil Blackwell 1984 pp. 137 - 56 page references to this translation.

Frege, G. (1892). Review of Georg Cantor, *Zur Lehre vom Transfiniten: Gesammelte Abhandlungen aus der Zeitschrift für Philosophie und philosophische Kritik. Zeitschrift für Philosophie und philosophische Kritik*, 100:269 – 72. Translated by Hans Kaal in Gottlob Frege *Collected Papers* Brian McGuinness (ed.) Oxford: Basil Blackwell 1984 pp. 178 - 181 page references to this translation.

Frege, G. (1892b). Über Sinn und Bedeutung. *Zeitschrift für Philosophie und philosophische Kritik*, 100:25 – 50. Translated as “On Sense and Meaning” by Max Black in Gottlob Frege *Collected Papers* Brian McGuinness (ed.) Oxford: Basil Blackwell 1984 pp. 157 - 77 page references to this translation.

Frege, G. (1898?). Logical defects in mathematics. In Hermes, H., Kambartel, F., and Kaulbach, F., editors, *Posthumous Writings*. Basil Blackwell.

Frege, G. (1953/1884). *The Foundations of Arithmetic*. Evanston: Northwestern University Press, 2nd edition. J. L. Austin, Trans. original published 1884, first edition 1950.

Frege, G. (1979/1880). Boole’s logical calculus and the concept - script. In Hermes, H., Kambartel, F., and Kaulbach, F., editors, *Posthumous Writings*, pages 9 – 46. Chicago: University of Chicago Press.

Frege, G. (1980). *Philosophical and Mathematical Correspondence*. Chicago: University of Chicago Press. Gabriel, G, Hermes, H. Kambartel, F. Thiel, C and Veraart, A. Editors, Abridged from the German edition by B. McGuinness, Translated by H. Kaal.

Frege, G. (1983). *Nachgelassene Schriften*. Hamburg: Felix Meiner Verlag, second, expanded edition. Hermes H., Kambartel F. and Kaulbach F. (eds.).

Frege, G. (1984/1874). Methods of calculation based on an extension of the concept of quantity. In *Collected Papers*, pages 56 – 92. Blackwell.

Frege, G. (2013). *Basic Laws of Arithmetic. Derived using concept-script*, volume I and II. Oxford University Press. Philip Ebert and Marcus Rossberg, (trans.).

Gauss, C. (1831). Theoria residuorum biquadraticorum: Commentatio seconda. *Göttingensche gelehrte Anseigen*, 64(23). Page reference to reprinting in *Werke II* p. 169 - 178.

Glenn, O. (1915). *A Treatise on the Theory of Invariants*. Boston: Ginn and Co.

Gordan, P. (1887). *Vorlesungen über Invariantentheorie*, volume II. Leipzig: Teubner. Edited by Georg Kerschensteiner.

Gronau, D. (1997). Gottlob Frege, a pioneer in iteration theory. *Grazer mathematische Berichte*, 334:105–119. volume title: Iteration Theory (ECIT 94) Proceedings of the European Conference on Iteration Theory, Opava.

Hankel, H. (1867). *Vorlesungen über die Complexen Zahlen und Ihren Functionen: Theorie der Complexen Zahlensysteme*, volume I. Teubner, Leipzig.

Hankel, H. (1870/1882). *Untersuchungen über die unendlich of oscillierenden und unstetigen Functionen.* Tübingen: Ludwig Fues. Reprinted in *Mathematische Annalen* **22** (1882) pp. 63 - 112 and in the Ostwalds Klassiker series 1905.

Hawkins, T. (1975). *Lebesgue's theory of integration. Its origins and development.* Chelsea, New York.

Heck, R. (2011). *Frege's Theorem.* Oxford University Press.

Jahnke, H. N. (1990). *Mathematik und Bildung in der Humboldtschen Reform.* Göttingen: Vandenhoeck & Ruprecht.

Kreiser, L. (1984). G. Frege: Die Grundlagen der Arithmetik - Werk und Geschichte. In Wechsung, G., editor, *Frege Conference 1984.* Academie-Verlag, Berlin.

Kreiser, L. (2001). *Gottlob Frege: Leben, Werk, Zeit.* Felix Meiner Verlag, Hamburg.

Laugwitz, D. (2002). Debates about infinity in mathematics around 1890: The Cantor-Veronese controversy, its origins and its outcome. *N.T.M.*, 10:102–26.

Mancosu, P. (201?). *Abstraction and Infinity.* Oxford University Press. to appear.

Olver, P. (1999). *Classical Invariant Theory.* Cambridge: Cambridge University Press.

Osgood, W. (1892). The symbolic notation of Aronhold and Clebsch. *The American Journal of Mathematics*, 14(3):251–61.

Preyer, W. (1877). *Elemente der reinen Empfindungslehre*. Hermann Dufft.

Riemann, B. (1854/1868a). Ueber die Darstellbarkeit einer Function durch eine Trigonometrische Reihe. *Abhandlungen der Koenigliche Gessellschaft d. Wissenschaft zu Göttingen*, XIII. Habilitation paper, published posthumously Reprinted in Bernhard Riemann's *Gesammelte Mathematicsche Werke und Wissenschaftlich Nachlass* R. Dedekind and H. Weber with Nachträge ed. M. Noether and W. Wirtinger, 3rd edn R Narasimhan (ed) Springer, New York. 1990 1st ed. R. Dedekind, H Weber (eds.) Leipzig: Teubner 1876 p.272 - 87.

Riemann, B. (1857). Theorie der Abel'schen Functionen. *Journal für die reine und angewandte Mathematik*, 54. reprinted in Bernhard Riemann's *Gesammelte Mathematicsche Werke und Wissenschaftlich Nachlass* R. Dedekind and H. Weber with Nachträge ed. M. Noether and W. Wirtinger, 3rd edn R Narasimhan (ed) Springer, New York. 1990 1st ed. R. Dedekind, H Weber (eds.) Leipzig: Teubner 1876 pp.88-142.

Riemann, B. (1859/1974). On the number of primes less than a given magnitude. In Edwards, H., editor, *Riemann's Zeta-function*, pages 299 – 310. New York: Academic Press. H. Edwards (trans.) published as an appendix.

Riemann, B. (1868/1854). Ueber die Hypothesen, welche der Geometrie zu Grunde liegen. *Abh. der Kngl. Gessellschaft d. Wissenschaft zu Göttingen*, XIII. (= Bernhard Riemann's *Gesammelte Mathematicsche Werke und Wissenschaftlich Nachlass* R. Dedekind and H. Weber with Nachträge ed. M. Noether and W. Wirtinger, 3rd edn R Narasimhan (ed) Springer, New York. 1990 1st ed. R. Dedekind, H Weber (eds.) Leipzig: Teubner 1876 p.272-87) English trans. by W.Clifford (rev. by Ewald) in Ewald [1996] p.652-61 page references to translation.

Robinson, A. (2008). Stolz, Otto. In Gillespie, C., editor, *Complete Dictionary of Scientific Biography*, volume 13, page 81. Detroit: Charles Scribner's Sons.

Scheeffer, L. (1884). Allgemeine Untersuchungen über Rectification der Curven. *Acta Mathematica*, 5:49 – 82.

Schubring, G. (2005). *Conflicts between Generalization, Rigor and Intuition.* Springer.

Snell, K. (1846). *Einleitung in die differential- und integralrechnung.* Brockhaus.

Stahl, H. (1899). *Elliptische Functionen: Vorlesungen von Bernhard Riemann.* Teubner, Leipzig.

Stein, H. (1990). Eudoxus and Dedekind: On the ancient theory of Greek ratios and its relation to modern mathematics. *Synthèse*, 80:163–211.

Stolz, O. (1871). Die geometrische Bedeutung der complexen Elemente in der analytischen Geometrie. *Mathematische Annalen*, 4:416 – 41.

Stolz, O. (1881). B. Bolzano's Bedeutung in der Geschichte der Infinitesimalrechnung. *Mathematische Annalen*, 18:255 – 279.

Stolz, O. (1883). Zur Geometrie der Alten, insbesondere über ein Axiom des Archimedes. *Mathematische Annalen*, 22:504 – 519.

Stolz, O. (1884). Die unendlich kleinen Grössen. *Berichte des Naturwissenschaftlich-Medizinischen Vereines in Innsbruck*, 14:21–43.

Stolz, O. (1885). *Vorlesungen über Allgemeine Arithmetik, Erster Theil: Allgemeines und Arithmetik der Reelen Zahlen.* Leipzig: Teubner.

Stolz, O. (1888). Über zwei Arten von unendlich kleinen und unendlich großen größen. *Mathematische Annalen*, 31:601–604.

Sullivan, D. (201?). An addition to the frege literature. ms to appear.

Tait, W. (1997). Frege versus Cantor and Dedekind: On the concept of number. In Tait, W., editor, *Early Analytic Philosophy*, pages 213 – 48. Open Court, Peru, Ill.

Tappenden, J. (1995). Extending knowledge and 'fruitful concepts': Fregean themes in the foundations of mathematics. *Noûs*, 29:427–67.

Tappenden, J. (1995a). Geometry and generality in Frege's philosophy of arithmetic. *Synthèse*, 102:319–361.

Tappenden, J. (1997). Metatheory and mathematical practice in frege. *Philosophical Topics*, 25(2):213–264.

Tappenden, J. (2005). The Caesar problem in its historical context: Mathematical background. *Dialectica*, 59(fasc. 2):237–264.

Tappenden, J. (2006). The Riemannian background to Frege's philosophy. In Ferreirós, J. and Gray, J., editors, *The Architecture of Modern Mathematics: Essays in History and Philosophy*, pages 97 – 132. Oxford University Press.

Tappenden, J. (2008). Mathematical concepts: Fruitfulness and naturalness. In Mancosu, P., editor, *The Philosophy of Mathematical Practice*, pages 276 – 301. Oxford University Press.

Tappenden, J. (2011). A primer on Ernst Abbe for Frege readers. *Canadian Journal of Philosophy*, Supplementary Volume: *Truth And Values: Essays for Hans Herzberger*: 31 – 118.

Thomae, J. (1870). *Abriss einer Theorie der Complexen Functionen und der Thetafunctionen einer Veränderlichen.* Halle: Louis Nebert.

Thomae, J. (1875). *Einleitung in Die Theorie der Bestimmten Integrale.* Halle: Louis Nebert.

Thomae, J. (1880). *Elementare Theorie der Analytischen Functionen einer Complexen Veränderlichen.* Halle: Louis Nebert.

Veraart, A. (1976). Geschichte des wissenschaftlichen Nachlassen Gottlob Freges und seiner Edition: Mit einem Katalog des ursprünglichen Bestands der nachgelassenen Schriften Freges. In Schirn, M., editor, *Studien zu Frege / Studies in Frege*, volume I. Fromann-Holzboog, Stuttgart-Bad Cannstatt:.

Wilson, M. (1999). To err is Humean. *Philosophia Mathematica*, 7(3):247 – 57.