The Riemannian Background to Frege's Philosophy[1]

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There was a methodological revolution in the mathematics of the nineteenth century, and philosophers have, for the most part, failed to notice.[2] My objective in this chapter is to convince you of this, and further to convince you of the following points. The philosophy of mathematics has been informed by an inaccurately narrow picture of the emergence of rigour and logical foundations in the nineteenth century. This blinkered vision encourages a picture of philosophical and logical foundations as essentially disengaged from ongoing mathematical practice. Frege is a telling example: we have misunderstood much of what Frege was trying to do, and missed the intended significance of much of what he wrote, because our received stories underestimate the complexity of nineteenth-century mathematics and mislocate Frege's work within that context. Given Frege's perceived status as a paradigmatic analytic philosopher, this mislocation translates into an unduly narrow vision of the relation between mathematics and philosophy.

This chapter surveys one part of a larger project that takes Frege as a benchmark to fix some of the broader interest and philosophical significance of nineteenth-century developments. To keep this contribution to a manageable

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[2] One exception is Howard Stein [1988], who discusses the importance of Dirichlet, rather than Riemann, to this revolution, but both exemplify the reorientation in method of which he writes: 'Mathematics underwent, in the nineteenth century, a transformation so profound that it is not too much to call it a second birth of the subject – its first having occurred among the ancient Greeks...' (Stein [1988] p. 238)

length, the chapter will emphasize the philosophical interest and methodological underpinnings of the mathematical history, with the connection to Frege given in outline, to be fleshed out elsewhere.[3]

I'll concentrate on issues intersecting one specific theme: the importance of fruitfulness in applications as a criterion for the importance and 'centralness' of a foundational concept, with special attention to the concept of function. We'll find a connected family of Fregean passages mirrored in methodological views and research strategies explicitly enunciated by Riemann and his successors. In the abstract, of course, this sort of parallel could be just a coincidence: perhaps Frege was unaware of these developments or out of sympathy. The role of the biographical detail about Frege's mundane life as a mathematics professor will make it clear that Frege was both aware and in crucial respects sympathetic.

I will recall some points about Frege; I can be brief since I've explored them extensively elsewhere. (Tappenden (1995)) It is well known that a major Fregean innovation was the choice of function/argument rather than (say) subject/predicate as the basis of his logic. Frege is also explicit about why he chooses the framework he does:

> 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. ([BLC] p. 33)

This stance resonates with other facets of Frege's epistemology, notably his view that it is because of 'fruitful concepts' that logic can extend knowledge, and his view that only through applications to physical reality is mathematics elevated above the status of a game. Moreover, it fits with a Fregean aspiration I have called (in Tappenden [1995]) the 'further hope'. In answering the rhetorical question of why one should prove the obvious, Frege cites several reasons, among them:

> ... there may be justification for a further hope: if, by examining the simplest cases, we can bring to light what mankind has there done by instinct, and can extract from such procedures what is universally valid in them, may we not thus arrive at general methods for forming concepts and establishing principles which will be applicable also in more complicated cases? ([FA] p. 2)

How did Frege expect that his many interconnected remarks on such themes would be received? Would he expect his audience to hop through them as *obiter dicta* or to see in them charged declarations of methodological principle? The rest of this chapter will bring out that Frege could not have failed to realize that these

[3] I develop the story in detail in Tappenden [----].

remarks would be read as a kind of declaration of allegiance, or at least affinity with one of two competing schools.

One rhetorical obstacle to the recognition of the facts about Frege's mathematical context and the interest they bear should be noted at the outset. There is a story about Frege's background that is so generally accepted as to deserve descriptions such as 'universally acknowledged dogma'. The story has it that the mathematics of the nineteenth century, so far as it is relevant to Frege's career, can be summed up as the process of the 'arithmetization of analysis' exemplified by Weierstrass. I will argue that this folk legend is not just wrong, but *wildly* wrong. It is not just wrong in that it excludes a wide range of facts and events but wrong about the *spirit* of what was happening at the most revolutionary turns, and wrong about what philosophical issues that events rendered salient. First, there was much more going on than the arithmetization of analysis. Second, the other things going on were much more relevant to Frege's training and research. Third, to the extent that Frege had any stance concerning the Weierstrass 'arithmetization of analysis' program it was as a *critic*.[4]

The story that will unfold here will run as follows:

a) In Germany from the 1850s on, there was a clash of mathematical styles in complex analysis and neighbouring fields in which the issue of the fruitfulness of concepts was of paramount importance; this was bound up in intricate and sometimes surprising ways with the development of geometry and geometric interpretations of analysis.
b) The coarsest division separates 'followers of Riemann' and 'followers of Weierstrass'. Riemann's mathematics was revolutionary, exhibiting for the first time a variety of the styles of reasoning that we associate with contemporary mathematics, while Weierstrass' was a continuation of a broadly computational mathematics that continued what had gone on before. The Riemann material was foundationally in flux, and it gave rise to several different ways of rendering the basic results tractable. In addition, the work presented certain methodological issues – clarity and fruitfulness of concepts, the role of geometry and intuition, connection to applications – in an especially urgent way.

4 The idea of 'the development of rigour in the nineteenth century' has been so thoroughly identified with 'the arithmetization of analysis' in the style of Weierstrass that I should take a moment to make explicit that by describing Frege as a critic of the programme I am in no way making the preposterous suggestion that he opposed rigour. The arithmetization of analysis (to the extent that this means specifically Weierstrass' broadly computational program of extending the techniques of algebraic analysis by exploiting power series) was one among many programs for imparting rigour. Dirichlet and Dedekind, to name just two of the nineteenth-century mathematicians committed to rigour, had no part of the procrustean restrictions that were bound up with the Weierstrass techniques.

Also, the issues I will be discussing have little to do with 'δ-ε' definitions of continuity, differentiability, etc. In the mid-nineteenth century this style of definition was common coin. Riemann's definition of (what we now call) the Riemann integral was also in the 'δ-ε' mould.

c) Frege's non-foundational work and intellectual context locate him in reference to these live issues of mathematical method. He is securely in the *Riemann* stream.[5]
d) These methodological issues were reflected in Frege's foundations – notably in his assessment of the importance of the concept of function, in his emphasis on applications, the theory of negative and real numbers in *Grundgesetze II*, his principle that objects should be presented independently of particular modes of representation, his opposition to piecemeal definition, his view of the relation of arithmetic to geometry, his stance on 'fruitful concepts' and its connection to extending knowledge, and his consistently critical stand on Weierstrass and his acolytes.

I Myth and Countermyth: Frege's position in nineteenth-century mathematics or: the 'arithmetization of analysis' in the style of Weierstrass is a red herring

Frege's foundational work was developed in a context in which the issues it addressed were still volatile and disputed. Before spelling out this history, it will be useful to indicate two commonly accepted positions that I'll be arguing against. (I'll call them 'the myth' and 'the countermyth' for reasons that will become apparent.)

a) The myth (Russell, folklore): Weierstrass to Frege – a natural succession?

The thesis part of this thesis–antithesis pair is familiar. I expect that most people with even a glancing interest in mathematics or philosophy have heard it. For those of my generation I think it fair to describe it as the 'received view' against which the subsequent counter-myth struck a powerful blow. I don't know the precise origins of the myth, but I expect that a genealogy would assign a large role to Russell's popular writings like *Introduction to Mathematical Philosophy* and their echoes in writers like Quine. In this familiar tale, the foundations of mathematics develops as a series of reductions of clearly delineated and well-understood fragments of real (not complex) analysis. Derivatives are reduced to limits of reals,

[5] To keep some focus to this already extensive discussion I am leaving out several topics that would be relatively distracting here, though of course they would have to be included in a complete account of Riemann's influence on our conception of geometry and complex analysis. In particular, I'm leaving out Riemann's role in the early development of topology ('*analysis situs*') and non-Euclidean geometry. Some of these are addressed in José Ferrieròs' contribution to this volume. This omission is not to be taken to suggest that these developments are uninteresting, or unrevealing either in themselves or as background to Frege's work. It is just an editorial decision about how to present this piece of the story.

reals and limits of reals are reduced to sets of rationals, rationals are reduced to sets of pairs of integers, and finally integers are reduced to sets via a (so-called) 'Frege–Russell' definition of number.

One objective is taken to be the introduction of rigour through the elimination of geometry in favour of the 'arithmetization of analysis' represented by Weierstrass. Frege and Russell are viewed as the culmination of a mathematical trend that begins with Cauchy and winds through the 'δ-ε' definition of continuity and other concepts, and the approach to functions in terms of convergent infinite series. The payoff of the reduction is taken to be the possibility of knowing mathematical truths for certain. Another desideratum, on this picture, is reduction in the number of primitive logical and ontological categories. From this point of view, the paradigms of foundational work are the reduction of ordered pairs to sets (Quine), and the Sheffer stroke (Russell).[6]

Of course, if the nineteenth-century foundational enterprise could be summed up in the myth, that enterprise would be *mathematically* idle. Reduction motivated purely by 'bare philosophical certainty' and ontological economy – whatever its philosophical merits – is in practice of little interest to the working mathematician.[7] Consequently the efforts of Frege and others also appears mathematically idle, a point upon which the countermyth fastens.

A related, similarly misleading picture locates the momentum for logical foundations of mathematics in a formless 'loss of certainty' arising from the realization that Euclid's parallel postulate is not a certain truth about space and reinforced by the discovery of the set-theoretic paradoxes.[8] On this picture, the purpose of logical foundations is to restore the unshakable confidence in mathematics that had been lost.

As far as Frege and German work in foundations is concerned, this thumbnail picture is so drastically off the mark that it is hard to find anything right about it, but I want to concentrate on one particular cluster of shortcomings. The picture completely neglects the diagnostic need for a more adequate understanding of key

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[6] For Quine's views on the reduction of ordered pairs to sets as a 'philosophical paradigm' see (for example) Quine [1960]. The jaw-dropping Russell assessment of the importance of the Sheffer stroke appears in Russell and Whitehead ([1927] p. xiv–xv).

[7] I do not mean to deny that reductions of one theory to another are sometimes seen as important successes in mathematical practice. The point is just that such reductions are rarely, if ever, motivated by 'ontological economy'. Unifications of diverse theories that reveal crucial structure as achieved through the Klein program in geometry, the unification of the theories of algebraic functions and of algebraic numbers in Dedekind–Weber [1882], or more recently the unification of algebraic geometry and number theory via the concept of 'scheme' in algebraic geometry are valued, to cite just three examples. But reductions like those given by the Sheffer stroke or ordered pair, which have as their sole advertisement a reduction in the number or kind of basic entities or expressions seem to be regarded with indifference in practice.

[8] I don't know if the view that a foundational crisis arose from the 'loss of certainty' inspired by non-Euclidean geometry and the paradoxes has ever been given a serious scholarly defense, but I have often encountered it proposed or apparently assumed in conversation and lectures and in writing. Certainly it informs much writing on Frege.

topics. It assumes that 'mainstream' mathematical practice was more or less in order as it stood, when in fact key areas were, and were recognized to be, in wild disarray. Though it is hard to tell from Russell's bloodless reconstruction, the early foundational researchers were responding to direct and pressing mathematical needs. This is also, as it happens, the direction taken by what I will call the 'countermyth', which takes over the basic historical picture of the myth and argues that Frege should be relocated outside the mathematical stream.

b) The Countermyth (Kitcher, recent consensus) Weierstrass and Frege: the Mathematics/Philosophy boundary?[9]

The countermyth holds Frege to be crucially *different* from Weierstrass and, by extension, from nineteenth-century mathematics generally, in that Frege was purportedly *not* moved by mathematical considerations.[10] According to the countermyth Frege was proposing a foundational program that makes sense only according to *philosophical* desiderata while Weierstrass exemplified the *mathematical* tradition. He proposed his rigourous definitions to solve well-defined mathematical problems, such as: 'If a sequence of continuous functions converges to a function, is the result continuous?' Proofs demanded distinctions like that between uniform and pointwise convergence, requiring in turn more rigorous definitions of the ingredient concepts. This conclusion – perhaps best articulated by Philip Kitcher ([1981], [1984] (ch.10) and elsewhere) – seems to have been widely accepted in philosophical circles.[11] Indeed, I have the impression that it has displaced its predecessor as 'reigning conventional wisdom'.

Kitcher draws attention to some of the delicate ways that problem-solving efforts and increases in rigour have historically interacted, with specific attention to the events presented in the myth: the foundational treatments of sequences,

9 Lest my use of the terminology of 'myth' and 'countermyth' leave a misimpression, I should stress that despite my disagreements on points of detail, I regard Kitcher's work as important and as genuinely driven by an honest and diligent engagement with the historical record. I speak of the 'countermyth' as itself having the character of a myth not because of Kitcher's original writings but rather because of a subsequent uncritical ratification. The subsequent wide acceptance seems to me to be based more on conformity to philosopher's prejudices about mathematical activity than on the historical evidence.

In this connection it is worth mentioning a particularly astute point that Kitcher has emphasized in his writing on Dedekind: We gain only a meager part of Dedekind's method if we restrict attention to his explicit methodological *dicta*. We have to recognize how much methodology is implicit in the specific decisions that inform the presentation of his actual mathematics. Part of my objective here is to bring out how much unstated methodology is implicit in Frege's mathematical choices as well.

10 I am concentrating on Weierstrass for the sake of focus, but of course the countermyth also separates Frege from Dedekind, Cantor, etc., along the 'mathematical motive'/'philosophical motive' boundary.

11 In addition to the writings of Weiner to be discussed in a moment, the Kitcher version of the countermyth is endorsed by Wagner [1992] p. 95–96 and Currie [1982], to cite just two. Furthermore, Dummett ([1991] p. 12) responds to Kitcher's point in a way that seems to reflect a grudging acceptance of the core historical thesis, though he quarrels with Kitcher's 'spin'.

derivative, limits and continuity for functions of real numbers. Kitcher's conclusion is that Frege stands outside the mathematical mainstream, and was just mistaken to think himself in its midst:

> If we disentangle the factors which led to the Weierstrassian rigorization of analysis, we find a sequence of local responses to mathematical problems. That sequence ends with a situation in which there were, temporarily, no further such problems to spur foundational work. Frege was wrong to think of himself as continuing the nineteenth-century tradition... (Kitcher [1984] p. 269–270)
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> [Frege advanced] an explicitly philosophical call for rigour. From 1884 to 1903 Frege campaigned for major modifications in the language of mathematics and for research into the foundations of arithmetic. The mathematicians did not listen... [because] none of the techniques of elementary arithmetic cause any trouble akin to the problems generated by the theory of series or the results about the existence of limits. Instead of continuing a line of foundational research, Frege contended for a new program of rigour at a time when the chain of difficulties that had motivated the nineteenth-century tradition had, temporarily, come to an end. (Kitcher [1984] p. 268)

To the extent that Kitcher showed the need to set aside the Russell picture, his work represents a clear advance over previous research: it is absolutely correct that *the myth* is inaccurate. There *were* important differences between Weierstrass and Frege (and even greater differences between Weierstrass and Russell) in the way they were engaged with mathematical practice. Furthermore, if all that had been going on were the problems of taming infinite series and continuity of real functions, Frege would have indeed come on the scene decades after the shouting had died down. But Kitcher has only looked at parts of the story. The picture looks quite different if we extend our view from real analysis to include the areas of mathematics that Frege was professionally engaged with. Besides foundations, these were principally geometry and *complex* analysis (especially elliptic and more generally Abelian integrals and functions). In these areas there were new central questions that wouldn't be satisfactorily addressed until into the twentieth century.

A problem shared by *both* myth and countermyth is that the motives driving the nineteenth-century evolution to more explicit foundational grounding are presented in an emaciated form that leaves it hard to understand why that evolution should have gripped many of the greatest minds of that century. It is important to get the history right, both because it is rich and interesting in its own right and for more specific metaphilosophical reasons. The specific reasons arise from the fact that Kitcher's picture has come to be used rather uncritically in the Frege literature, for example in the writing of Joan Weiner.[12] This is unfortunate not just because the picture is historically inaccurate. It also nourishes an unduly meager conception of the relations of mathematical and philosophical

[12] See Weiner [1984] and [1990], especially Chapters I and II.

investigation. If we take Frege to be a paradigmatic analytic philosopher, these presumptions can support a quietism about philosophy that sees it as rightly disengaged from mathematical practice. To the contrary, I'll argue that both in our reading of Frege, and in our attitude to the relations of philosophy and mathematics, we should view the intellectual streams as essentially interwoven.

It is easy to see how the countermyth could gain a foothold in Frege studies. The myth induces narrow horizons around real analysis. This in turn narrows our vision when reading Frege's words. Here is one example, a passage both Kitcher and Weiner cite, that appears in the motivational opening pages of *Grundlagen*:

> Proof is now demanded of many things that formerly passed as self-evident. Again and again the limits to the validity of a proposition have been established for the first time. The concepts of function, of continuity, of limit, and of infinity have been shown to stand in need of a sharper definition . . ..
>
> In all directions the same ideals can be seen at work – rigor of proof, precise delimitation of extent of validity, and as a means to this, sharp definition of concepts. [FA] p. 1

If we take Frege to be speaking exclusively of functions of a real variable the countermyth might seem reasonable. Central questions in the foundations of *real* analysis had been settled for decades, and Frege's work could be argued to be importantly different.[13] But Frege says nothing to prompt that restriction. In what was called *Funktionentheorie* – functions of a *complex* variable – the foundations were in wild disarray. Questions of limit and boundary behaviour, behaviour at infinity and the nature of infinity, the proper construal of functions and legitimate patterns of function-existence arguments, and the natural definitions of continuity and differentiability *remained* up in the air.

That Frege has *complex* analysis in mind is clear throughout his writings; for example, in the essay [FC] he describes what he means when he speaks of the widening of the concept of function. As in *Grundlagen* ([FA] p. 1) he describes the expansion as resulting from the development of 'higher analysis' ([FC] p. 138) where, as Frege puts it, 'for the first time it was a matter of setting forth laws holding for functions in general.'[14]

[13] Since it is worthwhile to concede points to bring out how far off target the myth is, and I don't intend to contest the point here, I'll temporarily concede that the foundations of real analysis were settled. But I should register a footnote demurral. Even with this tunnel vision Frege's motives don't turn out as starkly different from those of the other mathematicians around him as Weiner takes them to be. (For example, even with the restriction to real analysis it is hard to explain Weiner's neglect of the definition of integral, which even for functions of a real variable remained an unsettled topic of research throughout the second half of the nineteenth century and (with the introduction of the Lebesgue integral) into the twentieth century.)

[14] That 'higher analysis' was understood to include complex analysis is easy enough to show by consulting any of the many textbooks devoted to it. For concreteness, consider a text Frege took out of the Jena mathematics library in the 1870s: Schlömilch's *Übungsbuch der Höheren Analysis* of 1868. About a seventh of the book is devoted exclusively to functions of a complex variable.

> Now how has the word 'function' been extended by the progress of science? We can distinguish two directions in which this has happened.
>
> In the first place, the field of mathematical operations that serve for constructing functions. Besides addition, multiplication, exponentiation and their converses, the various means of transition to the limit have been introduced. . .
>
> Secondly, the field of possible arguments and values for functions has been extended by the admission of complex numbers. . . ([FC] p. 144)

Weiner suggests in a recent book that so far as the relation between geometry and analysis was concerned:

> The attempt to clarify these notions (of limit, and continuity) involved arithmetizing analysis, that is, showing that its truths could be proved from truths of arithmetic. By the time Frege began his work, most proofs of analysis had been separated from geometry and the notion of magnitude. It is not surprising, then, that it would have seemed less evident to Frege that the truths of analysis are synthetic a priori. (Weiner [2000] p. 19–20)

This opinion completely *inverts* the historical situation in a way that crucially misrepresents the relation between Frege's foundations and ongoing non-foundational research. *Prior* to Riemann's work (which drew on Gauss' geometric representation of complex numbers) complex analysis in Germany had been done almost exclusively computationally, as 'algebraic analysis' with no connection to intuition.[15] It was in the years *after* Riemann's revolution in the 1850s that complex analysis became something different. During this period, it seemed *less* obvious that complex analysis could be carried out independently of appeals to geometric facts than it had before. In this area, Weierstrass' 'arithmetization' program conservatively clung to old certainties in the face of a revolutionary novel style of mathematical reasoning.

One terminological refinement: there was a great deal of talk about 'arithmetizing mathematics' even by people like Dedekind and Felix Klein who were

[15] In connection with Jena in particular, the textbooks of Oskar Schlömilch flesh out the context, providing a glimpse into the computational flavour of the 'algebraic analysis' that underwrote most pre-Riemannian research in complex analysis. See Schlömilch [1862] and [1868]. Frege checked the latter book out of the library; it is safe to assume that Frege consulted either Schlömilch [1862] or some other text with essentially the same content when preparing for and teaching courses that are explicitly devoted to 'algebraic analysis'. In these textbooks the treatment of complex analysis is relentlessly computational, without a hint of any appeal to intuition or geometric fact.

Also illuminating in this connection is Enneper [1876], which Frege borrowed from the library when preparing to teach courses on Abelian/Elliptic functions/integrals. Here again the presentation is relentlessly computational, with geometry appearing only for illustration, never in arguments. The only reference to Riemann is to rule his work outside the scope of the treatment. (Enneper [1876] p. 82).

ideologically and stylistically in sympathy with Riemann (though in different respects) and opposed to much of Weierstrass' method.[16] So for example, Dedekind wrote 'From just this point of view it appears as something self-evident and not new that every theorem of algebra and higher analysis, no matter how remote, can be expressed as a theorem about natural numbers – a declaration I have heard repeatedly from the lips of Dirichlet.' (Dedekind [1888/1901] p. 35) Everyone knows Kronecker's 'God created the natural numbers, all else is the work of man.' However, such pronouncements often meant very different things and pointed in very different directions.[17] In this chapter when I write of 'the arithmetization of analysis' I mean specifically *Weierstrass'* style. The features of Weierstrass' program that are typically at issue when philosophers discuss 'arithmetization of analysis' in connection with Frege (such as the use of power series and the broadly computational, series-based perspective) were not shared by Dedekind or Klein.[18] Indeed, the continuation of Riemann-follower Dedekind's quote represents the throwing down of a methodological gauntlet in an anti-Weierstrass direction:

> But I see nothing meritorious – and this was just as far from Dirichlet's thought – in actually performing this wearisome circumlocution and insisting on the use and recognition of no other than rational numbers. On the contrary, the greatest and most fruitful advances in mathematics and other sciences have invariably been made by the creation and introduction of new concepts, rendered necessary by the frequent recurrence of complex phenomena which could be controlled by the old notions only with difficulty. On this subject I gave a lecture (Dedekind [1854])... but this is not the place to go into further detail. Dedekind [1888/1901] p35–36

Weierstrass differed from Frege in many deep ways: in his opposition to mixing metaphysical reflection and mathematics, in his conditions on an adequate foundation for mathematics, in his view of the connection between pure mathematics and applications, and in many other ways as well. But this does not reflect a divide between Frege and 'mathematicians'. Weierstrass differed from *many* mathematicians of the nineteenth century in these ways. To argue that Frege was different from Weierstrass in some respects isn't yet to argue that Frege is outside

16 For Klein see his [1895]. It is worth mentioning in passing – since Klein's writings are sometimes described as presenting Riemann's point of view and I myself had long understood them that way – that in fact the extent (if any) that Klein's presentation reflects Riemann's own conception is quite unclear. It was indeed a matter of considerable controversy among Riemann's students just how much of the story was Riemann's own understanding and how much was Klein's elaboration. I'm grateful to Jose Ferrierós for helping me appreciate the force of this point. Conversations with him, and his contribution to this volume, have left me convinced that Klein departed significantly from Riemann's understanding.

17 The fragmented character of nineteenth-century 'arithmetization' is dissected in Schappacher [2003?].

18 Kronecker was also opposed, though from the 'right wing', so to speak.

the 'mainstream' of mathematics. The members of a broad and internally varied cluster of schools inspired by Riemann were different from Weierstrass and his followers, and Frege was in that Riemannian tradition.

The next two sections will be devoted to explaining the key features of the Riemannian tradition (Section II) and Frege's immersion within it (Section III). Here are the features of the Riemann style and the contrast with Weierstrass to be developed:[19]

- Weierstrass and Riemann had different definitions of the object of study in complex analysis, and the difference had significant ramifications in practice.
- Riemann's approach involved *apparent* appeals to geometric intuition (for example, in the construction of Riemann surfaces) and intuitions about physical situations (for example, in the Dirichlet principle). Distinguishing geometry and analysis was therefore a crucial unresolved topic. The Weierstrass approach, restricting itself to algorithmic techniques for working with power series, faced no such problems.
- Riemann's approach treated functions as given *independently* of their modes of representation. Riemann's techniques systematically exploited indirect function—existence arguments that need not correspond to any formula. Weierstrass dealt with explicitly given representations of functions. (Weierstrass: 'The whole point is the representation of a function'.)
- Riemann's methods were directly bound up with applications in electromagnetism, hydrodynamics and elsewhere. Weierstrass' work was relatively 'uncontaminated' with applications in physics.
- Overall, Riemann was introducing an entirely new style of mathematics that presented a different family of methodological problems. Weierstrass confronted the problems involved with managing intricate algorithms (finding more efficient procedures, discovering convenient and simple normal forms to reduce complicated expressions to, . . .) Riemann and his successors addressed problems in a way that made even the formulations of problems and *the choice of fundamental concepts* up for grabs. This made it especially important to identify the most *fruitful* ways to set problems up, as well as the proper contexts in which to address them.

Frege, though well versed in the prior tradition of algebraic analysis, was also (with Dedekind) the first significant philosopher to be immersed in mathematics of this recognizably modern form. In this light, Frege's reflections on mathematical method (for example on fruitful definitions or the nature of mathematical reasoning) take on a special force as early confrontations with a new style of mathematical reasoning.

19 The technical terms in this list of points will be explained in the coming pages.

II Beyond the Myth and Countermyth: Riemann versus Weierstrass on Complex Analysis

> One of the most profound and imaginative mathematicians of all time, [Riemann] had a strong inclination to philosophy; indeed, was a great philosopher. Had he lived and worked longer, philosophers would acknowledge him as one of them. His style was conceptual rather than algorithmic – and to a higher degree than that of any mathematician before him. He never tried to conceal his thought in a thicket of formulas. After more than a century his papers are still so modern that any mathematician can read them without historical comment, and with intense pleasure. (Freudenthal [1975] p. 448b)

If we look at the record – taking into account Frege's lectures and seminars, his education, his teachers, mentors and colleagues, his library borrowings and *Nachlass* fragments, and the other benchmarks that give us what reference points we have on Frege's life as a mathematics professor, a regular theme from his undergraduate studies to his late lectures is the theory of functions of a complex variable, in the distinctive style of Riemann.[20] The rest of this section will explain what the Riemann style is and why we should regard it as significant that Frege worked in this intellectual environment. To set the stage, here is a broad observation: it is oversimplified and admits qualifications and exceptions, but for preliminary orientation the simplifications should be harmless.[21] Berlin and Göttingen were distinct centres of activity with profoundly different senses of what counted as core subjects, as acceptable and preferred methods, as central problems, and even as favoured journals for publication.[22] They diverged on

[20] This represents a large fraction of Frege's non-foundational research. The other component – algebraic projective geometry – is treated in Tappenden [----].

[21] Of course there was a great deal of variation and internal differentiation within the streams as well. In particular, both Dedekind and Klein devoted themselves to developing Riemann's results as well as his conception of mathematics, but this took them in strikingly different directions. A particularly stark contrast is the purely algebraic reconstruction of Riemann's theory of algebraic functions in Dedekind–Weber [1882] set beside to the presentation of essentially the same theory structured around geometric intuition and physical examples in Klein [1884/1893]. For two more recent presentations, Chevalley [1951] is faithfully in the style of Dedekind, while the early chapters of Springer [1957] are in large part a clear and readable rigorous reworking of Klein [1884/1893], with some good pictures.

[22] There is some useful discussion of some of the institutional and individual differences separating late nineteenth-century Berlin and Göttingen mathematics in Rowe [1989] and [----a]. A good introduction to some of this general background is Laugwitz [1999], especially Chapter 4, which surveys some of the broader philosophical themes animating Riemann's work and some of the mathematicians he influenced. This chapter also contains some useful insights into some of the 'conceptual' and more broadly metaphysical elements dividing the Riemann stream in Göttingen with the 'anti-metaphysical' and 'computational' attitudes of Berlin.

the role of geometry in analysis and on the importance of physical applications to pure research. There were different paradigmatic figures. Riemann's methods (and, after 1866, his memory) dominated Göttingen in the 1860s and 1870s in a way that found no echo in the Berlin dominated by Weierstrass, Kronecker and Kummer. The circle around Clebsch at Göttingen at the time of his death in 1872 (Klein, Lie, Brill, M. Noether, Lindemann, Voss. . .) carried on lines of investigation growing out of the earlier Göttingen ones, retaining a principled independence of developments in Berlin. Writing a little over twenty years later, Lie reflects on the differences in style in one of the rare books that we can document that Frege read.[23]

> Riemann. . . knew how to apply geometric tools to analysis magnificently. Even though his astonishing mathematical instinct let him see immediately, what his time didn't allow him to prove definitively by purely logical considerations, nonetheless these brilliant results are the best testimony to the fruitfulness of his methods.
>
> I regard Weierstrass, Riemann's contemporary, as also a successor of Abel, not only because of the direction of his investigations, but even more because of his purely analytical method, in which the appeal to geometric intuition is strongly avoided. However outstanding Weierstrass' accomplishments may be for the foundations and supreme fields of analysis, I nevertheless think that his one-sided emphasis on analysis has not had an entirely favorable effect on some of his students. I believe I share this opinion with Klein, who like Riemann has understood so well how to take from geometric intuition fruitful stimuli for analysis.[24] (Lie and Scheffers [1896] p. v–vi)

The basic datum is the just-mentioned divide in approaches to functions of a complex variable between a Riemann–Göttingen axis and a (then relatively dominant) Weierstrass–Berlin axis.[25] A sad accident of history shaped the events that followed. After transforming complex analysis (and other fields) with work that

[23] The *Nachlass* catalogue records 41 pages of notes, calculations and diagrams on 'Two remarkable proofs in *Geometry of Contact-Transformations* by Lie and Scheffers'. (Cf. Veraart [1976] p. 101)

[24] Unless otherwise indicated, translations from German are my own. To save space I haven't reproduced the German original text in footnotes, though it will be available in the book version.

[25] By narrowing the scope I am omitting many interesting things, such as a contrast with a more algorithmic approach worked out by Eisenstein and Kronecker. Here too there is a contrast with Riemann:

> Riemann later said that [he and Eisenstein] had discussed with each other the introduction of complex magnitudes into the theory of functions, but that they had been of completely different opinions as to what the fundamental principles should be. Eisenstein stood by the formal calculus, while [Riemann] saw the essential definition of a function of a complex variable in the partial differential equation. [i.e. The Cauchy–Riemann equations] (Dedekind, cited in Bottazini [1986] p. 221)

was stunningly novel but also full of compressed and often opaque arguments, Riemann died in 1866. The mathematical community, especially in Göttingen, where Frege was to attend graduate school four years later, was left to pore over and decode what Ahlfors [1953] calls Riemann's 'cryptic messages to the future'. The ensuing years saw several fundamentally different schools of thought emerging out of Göttingen, each of which made a plausible case that they were following through on Riemann's conception of function theory and algebraic geometry.[26] By contrast, in the Berlin stream, the most characteristic Riemann techniques were avoided in principle.

The division extended even down to the level of elementary textbooks. As late as 1897 a textbook writer could state:

> Nearly all of the numerous present German textbooks on the theory of functions [of a complex variable] treat the subject from a single point of view – either that of Weierstrass or that of Riemann... In Germany, lectures and scientific works have gradually sought to unify the two theories. But we are in need of a book of moderate length that suffices to introduce beginning students to both methods. I appreciated the need of such a book as I undertook to write this introduction to the theory of functions. Riemann's geometrical methods are given a prominent place throughout the book; but at the same time an attempt is made to obtain, under suitable limitations of the hypotheses, that rigor in the demonstrations that can no longer be dispensed with once the methods of Weierstrass are known. (Burkhardt [1897] p. V)

In the next edition, Burkhardt remarked that his synthesis had met with approval 'outside of the strict disciples of Weierstrass'. (Burkhardt [1906/1913] p. vii)

A similar indication of the schism, and the dominance of Weierstrass' methods, appears in the 1899 description by Stahl of the distinctive approach of Riemann's lectures on elliptic functions (attended by Frege's teacher Abbe):

> ... the peculiarities of Riemann's treatment lie first in the abundant use of geometrical presentations, which bring out in a flexible way the essential properties of the elliptic functions and at the same time immediately throw light on the fundamental values and the true relations of the functions and integrals which are also particularly important for applications. Second, in 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*

[26] The details of these subschools would take up too much space here; I go into more detail in Tappenden [----]. For rough orientation, we can distinguish a stream that interpreted Riemann in terms of old-fashioned computational algebraic geometry (Clebsch, Brill, Max Noether...), a stream that took especially seriously the connection to visual geometry and transformations (Klein, Lie...), and a stream that interpreted Riemann in terms of a recognizably contemporary structural algebraic geometry (Dedekind, later Emmy Noether...). There were also some mathematicians (Roch, Prym, Thomae...) who worked in a kind of orthodox Riemannian complex analysis without the kind of reinterpretations defining the other streams.

> *guarantees a multifaceted view into the nature of the problem and the variety of its solutions*. Because of these features, Riemann's course of lectures forms an important complement to the analytical style of treatment that is currently, in connection with Weierstrass' theory, almost exclusively developed. (Stahl [1899] p. III emphasis mine)

Stahl is drawing on clichéd language and rhetoric ('solely on the basis of their characteristic properties' 'nearly without computing',. . .) favoured by Riemann and students like Dedekind. For example, Riemann sums up his point of view thus, in a methodological overview of article 20 of his thesis [1851]:

> A theory of these functions on the basis provided here would determine the presentation of a function (i.e. its value for every argument) independently of its mode of determination by operations on magnitudes, because one would add to the general concept of a function of a variable complex quantity just the attributes necessary for the determination of the function, and only then would one go over to the different expressions the function is fit for. ([1851] p. 38–39)

In subsequent lines, he makes clear that the 'necessary attributes' were properties like the location of discontinuities, boundary conditions, etc. A few years later, mentioning Article 20 explicitly, he speaks of those methods as bearing fruit in the paper [1857a] by reproducing earlier results 'nearly without computing' ('*fast ohne Rechnung*'). ([1857b] p. 85) In [1857a] itself, Riemann describes himself as having used his methods to obtain 'almost immediately from the definition results obtained earlier, partly by somewhat tiresome computations (*mühsame Rechnungen*). . .' ([1857a] p. 67)

Weierstrass, in his lectures, announces his contrasting stance, so carefully echoing this language as to make it clear that he has the Riemann style in mind as the adversary:

> At first the purpose of these lectures was to properly determine the concept of analytic dependence; to this there attached itself the problem of obtaining the analytic forms in which functions with definite properties can be represented. . . for the representation of a function is most intimately linked with the investigation of its properties, even though it may be interesting and useful to find properties of the function without paying attention to its representation. The *ultimate* aim is always the representation of a function. (Weierstrass [1886/1988] p. 156 emphasis in original)

It is important to appreciate what a simple and fundamental methodological difference is at issue here. On the one hand, Weierstrass holds that there can be no dispute about the kind of thing that counts as a basic operation or concept: the basic operations are the familiar arithmetic ones like plus and times. Nothing could be clearer or more elementary than explanation in those terms. Series representations count as acceptable basic representations because they use only these terms. By contrast, the Riemannian stance is that even what is to count as a characterization in terms of basic properties should be up for grabs. What is to count as fundamental in a given area of investigation has to be *discovered*.[27] In the following remarks, Dedekind sums up the way Riemann's approach to complex function theory understood the quest for the 'right' definition of key functions and objects. As Dedekind sees it, Riemann showed that there is a great *mathematical* advantage to be gained by defining the objects of study in a representation-independent way. Dedekind employed this method in his own profound work in function theory; for example his [1877] treatment of elliptic modular functions exploits Riemannian methods to powerful effect. As he puts it in an 1876 letter to Lipschitz:[28]

> My efforts in number theory have been directed toward basing the work not on arbitrary representations or expressions but on simple foundational concepts and thereby – although the comparison may sound a bit grandiose – to achieve in number theory something analogous to what Riemann achieved in function theory, in which connection I cannot suppress the passing remark that Riemann's principles are not being adhered to in a significant way by most writers – for example even in the newest works on elliptic functions. Almost always they mar the purity of the theory by unnecessarily bringing in forms of representation which should be results, not tools, of the theory. (Dedekind [1876a] pp. 468–469)

In a later essay, Dedekind puts forward his Riemann-inspired approach – 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. . . 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

[27] In Tappenden [----] and [---a] I explore one example of this contrast: the definition and study of elliptic functions. For Weierstrass, the key foothold is a scheme for representing every elliptic function in terms of a distinguished class of series. For Riemann, the keys include the topology of the natural surface on which the functions are defined.

[28] This letter was brought to my attention by Edwards [1987] (p. 14) I've also taken the translation from that article.

> properties, 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] p. 54–55)

Of course, the philosophical question of how to distinguish 'fundamental characteristics' or 'internal characteristic properties' that allow you to 'predict the results of calculation' from 'forms of representation that should be results, not tools, of the theory' is complicated indeed if we see it as an issue in general metaphysics and method. But in the specific cases at issue in complex analysis, the cash value of this contrast was well known, and it would have been transparent to the readers what he was referring to (even if they couldn't give a definition of what he was talking about).[29]

Points of difference as they appeared from the Berlin side (in striking contrast to the above words of Lie, Stahl and Dedekind) emerge in an 1875 letter by Mittag-Leffler, describing his experiences in Weierstrass' seminars on complex analysis:

> . . . starting from the simplest and clearest foundational ideas, [Weierstrass] builds a complete theory of elliptic functions and their application to Abelian functions, the calculus of variations, etc. What is above all characteristic for his system is that it is completely analytical. He rarely draws on the help of geometry, and when he does so it is only for illustrative purposes. This appears to me an absolute advantage over the school of Riemann as well as that of Clebsch. It may well be that one can build up a completely rigorous function theory by taking the Riemann surfaces as one's point of departure and that the geometrical system of Riemann suffices in order to account for the till now known properties of the Abelian functions. But [Riemann's approach] . . . introduces elements into function theory, which are in principle altogether foreign. As for the system of Clebsch, this cannot even deliver . . . [results we won't be discussing here – JT] . . . which is quite natural, since analysis is infinitely more general than is geometry.
>
> Another characteristic of Weierstrass is that he avoids all general definitions and all proofs that concern functions in general. For him a function is identical with a power series, and he deduces everything from these power series. At times this appears to me, however, as an extremely difficult path. . . (Frostman [1966] p. 54–55)[30]

Mittag-Leffler closes by praising the precision, clarity, and 'fear of any kind of metaphysics that might attach to their fundamental mathematical ideas' that he

[29] I've discussed this historical debate and its philosophical ramifications elsewhere (Tappenden [2005]) so I'll refer to that paper and move on.

[30] The translation is taken from Rowe [2000].

takes to mark the work of Weierstrass and Kronecker.[31] The sentiments expressed are characteristic of those held in the Weierstrass circle.[32]

Familiar themes found in Frege's writings appear here. In addition to the well-known concern for rigour, Frege also states that geometric interpretations of the complex numbers 'introduce foreign elements' into analysis.[33] The view that 'analysis is infinitely more general than geometry' was a central theme for Frege (as well as Dedekind) and he took the demonstration of this greater generality to be one of his defining objectives.[34] In these regards Frege's sentiments fit with Weierstrass'. But there are discordant notes. Weierstrass' position about using only a restricted range of functions is one. His treatment of function quantification pre-supposes the most general notion of function, irrespective of available expressions and definitions. Also, as we saw above, Frege took applications to be of paramount importance in assessing the value of mathematics. Another point of divergence is that Frege *did* work in geometry, and the evidence indicates that he continued to work on geometrical questions past the turn of the century. The mathematics of Clebsch and Riemann – the two mathematicians mentioned by Mittag-Leffler – was the mathematics Frege knew best, and more importantly (so far as his teaching and research into complex analysis is concerned) it is the mathematics he *did*. This raises an issue that Frege scholarship glides over. If we ask 'Just what was Frege trying to lay the logical foundations

31 The principled separation of metaphysics and mathematics noted by Mittag-Leffler as a characteristic of Weierstrass is another Weierstrass–Riemann contrast that shows itself in connection with Frege. In contrast to Weierstrass' aversion noted here, Riemann read and wrote extensively in philosophy, and in some cases (Herbart's epistemology) he plausibly describes this as shaping his mathematics.

It is well known that Frege said in *Grundgesetze* that he had little hope of gaining readers among those mathematicians who state '*metaphysica sunt, non legentur*'. ([BLA] p. 9) It is not clear to what extent Frege had any specific people or groups in mind, but it is worth noting that possibly the first time Frege uses the turn of phrase '*metaphysica sunt, non legentur*' it is directed, with what feels to be an allusive and knowing tone, at the Weierstrass surrogate Biermann in Frege's draft review of Biermann's account of number in his [1887] (dating uncertain). ([OCN] p. 74) It wouldn't surprise me to learn that this was a recognized catchphrase, so that by speaking obliquely of 'those mathematicians who think...' he was sending a signal whose overtones we now miss.

32 Here is another example, from a potentially long list, of the Riemann–Weierstrass contrast from the Weierstrass point of view. (This is from a retrospective by a Weierstrass student of late 1850/early 1860)

> At the same time, all of us younger mathematicians had at the time the feeling that Riemann's intuitions and methods no longer belonged to the rigorous mathematics represented by Euler, Lagrange, Gauss, Jacobi, Dirichlet and their like. (Königsberger) Königsberger [1919] p. 54

(This quote is from Laugwitz [1999]. I've altered Shenitzer's translation to be consistent with the other translations here.)

33 [FA] p. 112, [GzII] (p. 155 fn. 1) and elsewhere.

34 I have discussed his concern with the greater generality of analysis in relation to geometry in (Tappenden [1995a]).

of ?' the answer is usually blandly 'arithmetic and analysis' or 'all mathematics besides geometry' or something like that. There is a tacit presumption that just what counts as 'analysis' or 'mathematics' can be treated as unproblematic. But this was under dispute: mathematics in Riemann's sense was a more ambitious discipline. If Frege's foundations were an attempt to ground and diagnose 'analysis', his practice indicates that the target would have been complex analysis *as Riemann did it*. Thus we arrive at the first indication of the point I indicated at the outset: the widely assumed affinity in mathematical attitudes between Frege and Weierstrass is, at bottom, superficial and misleading, while the affinity with Riemann (and with mathematics as conceived and practiced in the Riemannian tradition) is profound despite seeming differences in their standards of rigour.

Specific details: Riemann, Weierstrass, and Epigones on Complex Analysis

> 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. Hyperelliptic integrals gave much trouble, but no one knew why. ... Despite Abel's theorem, integrals of general algebraic functions were still a mystery.... In 1851, the year in which Riemann defended his own thesis, Cauchy had reached the height of his own understanding of complex functions. Cauchy had early hit upon the sound definition of the subject functions, by differentiability in the complex domain rather than by analytic expressions. He had characterized them by means of what are now called the Cauchy-Riemann differential equations. Riemann was the first to accept this view wholeheartedly... [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. 449a)

> I have tried to avoid Kummer's elaborate computational machinery so that here too Riemann's principle may be realized and the proofs compelled not by calculations but by thought alone. (Hilbert [1897/1998] p. X)

Weierstrass' 'arithmetization' approach takes as basic the definition of an *analytic function* centered at z_0 as one that can be represented as a power series $f(z) = \Sigma a_i(z - z_0)^i$ where the a_i are complex numbers. The definition is intrinsically local: the series need converge, and hence the function need be defined, only within some given radius. This is not the handicap it might seem to be at first: when the analytic function on an open set extends to a multiple-valued function ('multifunction') on the entire complex plane, this continuation is unique. Note though, that a 'multifunction' is not a function, as these are defined in elementary textbooks, since it assigns several values to one argument.

The Riemann approach differs even in its definition of the basic object of study. Functions generally are accepted, with the functions to be studied marked out as those satisfying the *Cauchy–Riemann conditions*:

> With z as the complex variable, and writing the real and imaginary parts of f as u and v (so $f(z) = u + iv$), f is differentiable at (x, y) if these partial derivatives exist and these relations hold:
>
> $$\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y} \quad \frac{\partial u}{\partial y} = -\frac{\partial v}{\partial x}$$

These two definitions of analytic function/differentiable function are now known to be essentially equivalent, but this was not established until after 1900.[35] During most of Frege's productive career, these were seen as distinct and indeed competing definitions, with Riemann's potentially wider.[36] Weierstrass rejected Riemann's definition (though he recognized that the definitions agreed on the most important cases) because he held that the functions differentiable in Riemann's sense couldn't be precisely demarcated.[37]

Fundamental to Riemann's approach is the idea of a 'Riemann surface' on which a multifunction can be redefined so as to be actually a function, by unfolding it on several separate, though connected, sheets. The device allowed complex functions to be visualized (and in fact it was dismissed by Weierstrass as 'merely a means of visualization'), though its importance went well beyond visualization.

Riemann's geometric approach is further exhibited in his systematic use of *isogonal* (angle-preserving) and especially *conformal* (angle and orientation preserving) mappings (neither Cauchy nor Weierstrass used them systematically). Conformal mappings concern *local* behaviour – the preservation of angles makes sense in arbitrarily small neighbourhoods of a point. By linking conformal mappings and (what came to be understood as) topological properties, Riemann displayed one of his trademark methodological innovations: exploiting interactions between local and global properties.

A third feature of Riemann's approach relevant here is its connection to physics and geometry. (Tight connection to applications was characteristic not just of

[35] The qualifier 'essentially' in the text reflects the fact that additional minor assumptions are needed to secure the equivalence. See Gray and Morris [1978].

[36] I should emphasize that what is at issue is not the use of power series expansions. Everybody used them sometimes, even Riemann. What was distinctive of Weierstrass and his tradition was their use as a systematic basis for his theory. (Especially at a time when it had not been rigourously proven that *all* functions satisfying the Cauchy–Riemann equations would be so representable.)

[37] This is recorded in a set of lecture notes by a student who attended Weierstrass' lectures in 1877–78, Pincherle [1880] (p. 317–318). I am grateful to Ferrierós [1999] (p. 36) for drawing my attention to this feature of these notes, and to Bottazini [1986] p. 287–288 for information about their genesis.

Riemann but of Göttingen mathematicians as a group.[38]) Complex analysis in the style of Riemann was bound up with its applications in a much more direct and immediate way than Weierstrass' was. The point was similarly expressed by the like-minded Felix Klein (though to be sure in caricature):

> With what should the mathematician concern himself? Some say, certainly 'intuition' is of no value whatsoever; I therefore restrict myself to the pure forms generated within myself, unhampered by reality. That is the password in certain places in Berlin. By contrast, in Göttingen the connection of pure mathematics with spatial intuition and applied problems was always maintained and the true foundations of mathematical research recognized in a suitable union of theory and practice. (Klein [1893b]; quoted in Hawkins [2000] p. 137)

One key bridge to applications is via *potential theory*, which was becoming the core of the theoretical foundations of electricity and hydrodynamics.[39] The formal connection arises directly for Riemann since the equation for potential in two variables is an immediate consequence of the Cauchy–Riemann equations. Consequently, as Ahlfors put it in a retrospective essay, Riemann 'virtually puts equality signs between two-dimensional potential theory and complex function theory.' (Ahlfors [1953] p. 4) On Weierstrass' approach, the connection to these applications is distant, and this distance appears not to have troubled Weierstrass or anyone swimming in his wake.

Both this direct connection to applications and the role of conformal mapping in Riemann's methods flow into a final crucial point – the role of a function – existence principle that has come to be known (following Riemann) as 'Dirichlet's principle'.

A treatment of this principle would require too much space here, so I'll just nod to the longer treatment and some textbooks.[40] All we need to know here is that the principle asserts the existence of certain functions given certain conditions, that it seems most plausible in physical situations, and that it was central to some of Riemann's most important theoretical results.

The situation is complicated by the fact that the Dirichlet principle was not stated sharply enough to delimit its range of validity. Some general forms of

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[38] On the Göttingen tradition of connections between mathematics and physics see Jungnickel and McCormach [1986] p. 170–185.

[39] The connection between Riemann's complex analysis and physical applications emerges principally through research in electricity and magnetism, and related problems of hydrodynamics. See Riemann [1854/1868a]. The connections between the mathematical theory of potential and the developments in theories of electricity and magnetism have been well documented in secondary literature, so I will just stick to the main point here. On the potential theory itself, see, for example, Kellogg [1929]. A good historical overview of these mathematical events through the lens of potential theory is in Temple ([1981] ch.15)

[40] See Tappenden [----] for a more extended discussion in this specific connection and Courant [1950] for the principle itself.

the principle are false; Weierstrass supplied one famous counterexample. Hence some of Riemann's central arguments contain gaps that in applied cases are filled by physical or geometric intuition, and so the next domino falls: some keys to Riemann's approach remain strictly speaking unproved, leaving genuine questions as to the extent to which intuition grounds fundamental parts of Riemannian analysis.

A diagnostic question then arises: what additional restrictions make the Dirichlet Principle appear evident in physical situations? Can they be formulated sufficiently generally to support Riemann's arguments and make an abstract form of the principle fruitful and interesting, with a broad range of validity? Some of these discussions are especially prone to remind one of Frege's discussions of foundations. He emphasizes repeatedly at the outset of *Grundlagen* that a crucial function of a disciplined proof is that it often reveals the 'limits to the validity' (*Gültigkeitsgrenzen*) of a proposition:

> It not uncommonly happens that we first discover the content of a proposition, and only later give the rigourous proof of it, on other and more difficult lines; and often this same proof also reveals more precisely the conditions restricting the validity *(Bedingungen der Gültigkeit)* of the original proposition. ([FA] 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 Gültigkeit*) of a proposition have been in this way established for the first time. ([FA] p. 1)
>
> In all directions the same ideals can be seen at work – rigour of proof, precise delimitation of extent of validity (*Gültigkeitsgrenzen)*, and as a means to this, sharp definition of concepts. ([FA] p. 1)

At the same time, the Dirichlet principle was widely discussed in these terms. For one example, in a work on potential theory (Betti [1885] p. VII), the author states that he has avoided the Dirichlet principle not because it is simply invalid, but because the limits to its validity (*die Grenzen seiner Gültigkeit*) have not been established, and this turn of phrase is repeated verbatim in the review of the book in *Jahrbuch über die Fortschritte der Mathematik* for 1886.[41]

Much of the investigation of the Dirichlet principle was carried out in direct connection with physics and applied geometry. We should recall that Frege did not

[41] Of course, it wasn't just in connection with the Dirichlet principle that people spoke of 'limits to validity' in these ways. Another example (one that Frege read, cites and discusses) is Riemann's rigourous definition of (what we now call) the Riemann integral. Riemann characterizes his objective repeatedly as the clarifying of 'the extent of validity' *(den Umfang seiner Gültigkeit*) of the concept of definite integral. (Riemann [1954/1868a] (p. 227, twice on p. 239 (a page Frege cites) p. 240 p. 269). In his article on the distribution of prime numbers, Riemann places stress on replacing a function defined only on the upper complex plane with 'an expression of the function which is everywhere valid (*immer gültig)*.' (Riemann [1859/1974] p. 299)

think that there was anything intrinsically improper about appeals to intuition, nor did he think that arguments containing ineliminable appeals to intuition must lack cogency.[42] Indeed, he believed that Euclidean geometry was synthetic a priori and founded on a distinct intuitive 'source of knowledge'. The price of appeals to intuition was a loss of generality; one objective of the *Begriffsschrift* was the diagnostic job of paring off logical arguments from intuitive/logical blends by 'letting nothing intuitive penetrate unnoticed'.

The Dirichlet principle, in a form sufficiently general to support Riemann's proof techniques, was finally proved in Hilbert [1901]. However, in the period 1870–1900 that we are most concerned with, the issue was cloudy. Riemann's methods were used and explored in Germany during 1870–1900 but only by a relatively small band of true believers. Ways to avoid the Dirichlet principle were hammered out, and restricted positive solutions that avoided the Dirichlet principle and sufficed for Riemann's arguments on Abelian functions were carried through.[43] There were also more ambitious attempts to save Riemann's techniques and results by reworking them in novel ways (notably by Dedekind and Clebsch–Brill–M. Noether).

Weierstrass' attitude to Riemann was ambivalent, and his view of those who took up Riemann's mantle (Clebsch, Klein, …) was harsh. Weierstrass and Riemann were on good terms as young men. But Weierstrass' remarks after Riemann's death, tended to be ungracious. Even praise was doled out with sour addenda, and most of his published references to Riemann's methods are belittling. It also indicates what was said in private that Weierstrass' students tended to an unjustly dim view of Riemann's style.

Returning to the Dirichlet principle, note how the history is recounted by Brill and Max Noether, who were perhaps the most rigorous and 'algebraic' of those in the Riemann stream.[44] Writing after much of the smoke has cleared, but a few years before Hilbert's proof, they emphasize the fruitfulness, organic unity and connections to applications of Riemann's approach, while placing the flaw in the original reasoning in what they view as an overly general, uncontrolled concept of function:

> The application of the Dirichlet principle in the generality sought by Riemann is subject to, as we now recognize, considerable misgivings,

[42] Note for example: 'It seems to me to be easier still to extend the domain of this formula language to include geometry. We would only have to add a few signs for the intuitive relations that occur there. In this way we would obtain a kind of *analysis situs*.' ([B] p. 7)

[43] By Schwartz [1870], C. Neumann [1877] and later Poincaré.

[44] That is, they are 'algebraic' in the sense of older-fashioned computational algebra as you find in Chrystal [1886]. Dedekind is 'algebraic' in a different, more contemporary sense. Being 'algebraic' in the sense of Clebsch, Brill and Noether is not incompatible with being 'geometric' in a different sense.

> directed against the operation with functions of indeterminate definition in the Riemannian style. The function concept in such generality, incomprehensible and evanescent, no longer leads to reliable conclusions. Recently the exact Riemann path has been departed from in order to precisely bound the domain of validity [*Gültigkeitsbereich*] of the stated theorems. [Schwartz [1870] and Neumann [1877]] have more rigorously, though with circuitous methods described the precise conditions – paying heed to conditions on boundary curves of the surface, discontinuities of the function and so forth – under which the existence proofs that the Dirichlet principle was intended for are possible. It has turned out in fact, that the conclusions Riemann drew for specifically his theory of the Abelian functions remain correct in full generality.
>
> However, we should not set aside Riemann's distinctive style of proof too hastily. It has the virtue of the brevity and relative simplicity of the train of thought; it stands in organic connection to the problems of the mathematical physics from which [Dirichlet's] principle originated; Modelled by nature, Riemann's methods may some day experience a revival in a modified form. (Brill–Noether [1894] p. 265)

These remarks incorporate parts of what might be called 'the conventional wisdom' among mathematicians in the Riemann stream at the time, (apart from the heresy that the problem with Dirichlet's principle was the general concept of function). Similar sentiments were expressed earlier in connection with the Göttingen style:

> . . . Riemann makes possible a more general determination of functions, by means of suitable systems of strictly necessary and sufficient conditions. *Independently of the statement of an analytic expression, these permit. . . the treatment of questions more with pure reasoning than with calculation.* The use of Dirichlet's principle as an analytic instrument as well as [Riemann surfaces] as geometrical support, is characteristic of the theory of functions taught in Göttingen (Casorati [1868] p. 132–3 quoted in Bottazini [1986] p. 229 my emphasis)

Some aspects of the history of the concept of function have already been well-documented and studied in the philosophical literature. So I should stress at the outset that *in addition* to the already well-known developments concerning the concept of function, *Riemann added something importantly new*. To get our bearings, recall two developments flanking the period 1840–1900. Prior to (say) 1750, a 'function' was essentially a finite analytic expression like a polynomial. Famously, this conception came under pressure when it proved impossible to represent physical problems such as the behaviour of a vibrating string.[45] This initiated an

45 A clear and engaging thumbnail history of some of the questions raised by vibrating strings – that, incidentally, we know Frege read, since he discusses it – is in Riemann [1854/1868a].

evolution toward a conception of function as arbitrary correspondence in various authors. After 1900, with the broader conception of function relatively established, it became the focus of a different skirmish between early advocates of what we now call constructivism (Baire, Borel, Lebesgue) and opponents favouring a conception not tied to definability (Zermelo, Hadamard).[46]

Less well known in philosophical circles is what occurred between these interstices; that is of course the period relevant to Frege. With Riemann the shift to a conception of function that wasn't connected to available expressions becomes systematic and principled: unlike the vibrating-string problems, what was at issue was not a collection of individual anomalies or points of conceptual clarification but a methodical appeal to proof techniques with indirect function – existence principles at their core. Riemann would catalogue the singularities of a function (points where it becomes infinite or discontinuous), note certain properties, then prove that *there must exist* a function with these properties without producing an explicit expression.[47] There was as yet no guarantee that the functions proven to exist could be expressed in any canonical way. Nor need such representations be helpful even if they could be found.[48] It was not just that Riemann had a potentially wider conception of function than Weierstrass; he was committed to methods that only made sense if the wider conception were pre-supposed.[49] This is part of what gave urgency to Frege's effort to clarify the role of the concept of function in logical reasoning and to clarify the legitimate patterns of function existence argument. If we don't follow Weierstrass and

46 Among the useful discussions are Monna [1972], Moore [1982], Maddy [1997], and Hallett [1984].

47 So, for example, addressing a topic that had been typically treated computationally, Riemann remarks:

> Everything in the following treatise contains brief hints concerning the application of this theorem which (*as one sees easily with our method that is supported by the determination of a function through its discontinuities and its infinite values*) must form the basis of the theory of the Abelian functions. (Riemann [1865] p. 212 emphasis mine)

48 In the longer presentation of this material I discuss a high profile example: one of Riemann's innovations elsewhere was a global definition of the ζ-function over the complex plane (minus one point). As an analytic function, this has a power-series representation, but in practice this representation is of no use.

49 This feature of Riemann's work – the novel systematic use of abstract function existence arguments as a characteristic method – was drawn to my attention by Ahlfors [1953]. It is elaborated throughout Laugwitz [1999]. The only philosopher I know to have recognized the importance of Riemann to the extension of the function concept as it relates to Frege is Bill Demopoulos (drawing on some observations of Bottazini). ([1994] p. 86) Even Demopoulos' astute observation stops short of a notice of the *systematic* use of function – existence arguments in Riemann brought out by Ahlfors.

restrict our principles for representing functions, what logical principles *do* govern function-existence?[50]

The project of distinguishing geometry and analysis – paring the contribution of intuition from the pure logical content – had intricate motivations and complicated consequences. Many of these – notably the contribution of the foundations of geometry to the emergence of formal semantics – must be left for elsewhere. But it is worth pausing to emphasize how the issues explored so far reveal overlap in the geometry/analysis, logic/intuition and pure/applied divides. As we saw above in the quote from Lie, the value of Riemann's apparent appeals to geometry were the 'fruitful methods' and 'fruitful stimuli for analysis'. The project of disentangling analysis from geometry and the project of providing logical foundations for the 'truly fruitful concepts' in mathematics and natural science are, in complex analysis, the same project.

The history is complicated, and the above is just the beginning of the story in outline. Followers of Riemann disagreed on just how to elaborate their shared positions. In particular, though (apart from Dedekind) the followers of Riemann agreed on the importance of the 'geometrical' point of view, what they took to be the core of 'the geometrical' could be strikingly different. However, the point here has been to emphasize that among topics that were salient to most Riemannians, there was a crucial and deep-rooted *interrelation* among these central methodological themes such as the relation of analysis and geometric intuition, the concept of function and its generality, the fruitfulness of 'geometric' methods and their potential independence from intuition, the opposition to procrustean restrictions to Weierstrassian methods, the role of fruitfulness rather than reduction to antecedently given 'elementary' concepts bequeathed by history, like plus and times, as a guide to the 'internal nature' of concepts and so on. These were not merely discrete characteristic marks of a school but rather aspects of one orientation, in which 'every element is intimately, I might even say organically, connected to the others', to borrow a Fregean phrase.

The concentration on Weierstrass' arithmetization of analysis is a mistake because it considers a single strand of opinion: a forceful conservative thrust that held firmly to a broadly computational view of what truly rigourous mathematics consisted in.[51] To the extent that philosophers have formed a picture of mathematical activity in the late nineteenth century, it seems to draw solely from the research programs and characteristic outlooks of the conservatives, with a consequent impoverishment of our conception of what was mathematically interesting about the nineteenth century and what gave urgency to many of the deepest foundational developments.

[50] This emphasis on introducing functions without reference to specific formulas was taken (especially in Dedekind's hands) in a direction that strikingly anticipates Frege's concern with the mechanics of introducing objects as 'self-subsistent'. I explore this point further in Tappenden [----] and [----a].

[51] Of course, it might be better to say 'overlapping cluster of schools' to avoid a facile identification of Weierstrass with Kronecker or Kummer.

In this environment, Frege's principle that 'fruitfulness is the acid test of concepts' is not merely an idle platitude. It is at a statement of allegiances. So too Frege's use of a general concept of function as a basis of his logic, his project of disentangling intuition from analysis, his quest for general rather than piecemeal definitions, and other features of his philosophical foundations could reverberate significantly with this environment. Well, *was* this Frege's environment? Can we expect that he would have been aware of these developments and anticipate the relevant reactions? The short answer is yes.

III Beyond the Myth and Countermyth: Riemann and the Riemannian tradition as part of Frege's intellectual context

As we've noted, a variety of Fregean comments – those on fruitful concepts and on delimiting the extent of validity, to mention just two examples – would have seemed to mathematicians around Frege to be loaded remarks alluding to well-recognized disputes. This supports a prima facie assumption that Frege did indeed choose those words and issues deliberately. But leaving general observations aside, what can we say specifically about Frege and his milieu? The following is a quick overview of some of the substance of the intellectual world that can be reconstructed. There isn't space here to lay out all the varieties of fine detail necessary for a lifelike reconstruction of an intellectual environment. My purpose here is just to give an overview of some of what is available, with the full story to appear elsewhere.[52]

a) Colleagues and Teachers

The mathematicians around Frege – his teachers, colleagues, friends and correspondents – were almost all in Göttingen streams rather than Berlin streams.[53] The most important mathematicians in Frege's environment who concerned themselves with complex analysis – his mentor Abbe, his early geometry teachers Clebsch and Voss, his supervisor Schering, and his colleague Thomae – were all followers of Riemann in one way or another. Abbe, probably the most important

[52] There are other details fleshing out the picture, like shared terminology ('Begriffsbestimmung', 'Gebeit', . . .) mutual correspondents, etc. The material in this section should suffice to exemplify how richly the environment can be reconstructed.

[53] To help give a flavour for the intricacy of some of the historical questions – and especially to bring out that 'mathematical practice' is not a monolith – I've concentrated on two competing German centres and roughly contemporaneous styles. However, it is worth noting in addition that there was an even wider gap between mathematics in Germany *tout court* and most of the mathematics in Great Britain. I emphasize this especially because Frege scholarship and Russell scholarship often run on parallel tracks.

intellectual figure in Frege's life, was a devoted student of Riemann at Göttingen. Correspondence between Abbe at Göttingen and a student friend in Berlin in the 1860s reveals Abbe to be a well-informed, enthusiastic partisan of the Riemann sides of fundamental debates with Weierstrass. Frege's only graduate course in complex analysis came from his supervisor Schering, who taught from an annotated copy of Riemann's lectures. This is especially significant since Frege taught classes in complex analysis and advanced topics (Abelian functions) from the very beginning of his time at Jena. Frege's geometry teacher Clebsch was also working out some of the details of Riemann's analysis, within algebraic curve theory.[54]

b) Library Records: Frege's Reading

Surviving library records reveal that complex analysis and especially elliptic and Abelian functions were one of three large clusters of reading activity for Frege between 1873 and 1884. The others were pure geometry and Kantian/anti-Kantian philosophy of science, which reinforces the point that Frege was approaching complex analysis with a geometric basis and a methodologically sensitive eye. Jacobi [1829], a classic treatment that initiated several lines of research into Abelian functions as problems in complex analysis is on a short list of books that Frege borrowed from the library between 1873 and 1879.[55] Two of the others were Enneper [1876] and Schlömilch [1868]. The former is a survey of results about elliptic functions, the latter a book of school exercises in 'algebraic analysis'. A fourth entry is Clebsch–Gordan [1866], which is a text on Abelian functions cowritten by one of Frege's Göttingen teachers. (Frege borrowed Clebsch–Gordan [1866] again in 1883–1884.)[56] Frege also checked out Abel's *Oeuvres Complétes* in 1883. Frege might have been interested in many things there. But in light of his other library borrowings at the same time, it seems reasonable to expect that most or all of Frege's reading from the volume concerned Abel's work on elliptic functions within complex analysis, especially the epochal Abel [1827/1828]. Other borrowings (Gauss's *Disquisitiones* and Bachmann's book on cyclotomy) had recognized connections to elliptic functions, but in complicated ways that I'll not go into here. If we add the classes Frege had taken on Riemann's theory from Abbe and Schering, the Riemann lecture notes he had access to, and the Thomae monograph he reviewed, what emerges is a picture of Frege immersed in several different approaches to

54 In addition, there is a lacuna on the opposite side. Frege's environment was largely devoid of representatives of the Berlin perspective. Frege corresponded with Cantor and Husserl, but neither of these could be called orthodox followers of a Weierstrass line. Otherwise, none of his colleagues at Jena was connected to Weierstrass or Berlin, nor were any of his correspondents.

55 cf. Kreiser [1984] p. 21 for the library records.

56 cf. Kreiser [1984] p. 25

Abelian (and the special case of elliptic) functions/integrals, in the years leading to *Grundlagen.*

c) Frege's Lectures and Seminars

I've already noted that Frege regularly taught courses in complex analysis and the advanced subtopics of Abelian integrals and elliptic functions. In fact, Frege offered classes in complex function theory or advanced subtopics 17 *times* between 1874 and 1906.[57] On two occasions ((1903), (1906/07)) a course was titled explicitly 'Complex function theory according to Riemann'. In addition, he offered several courses and seminars on conformal/isogonal mapping. Note too that Frege offered lecture courses on 'Elliptic and Abelian Functions' (Summer 1875) and 'Theory of Functions of a Complex Variable' (Winter 1876/77 and S 1878) and 'Abelian Integrals' (S 1877 and W 1877/78) very early in his teaching career, when he could be expected to have a heavier debt to his teachers, and during which time the generalization of the idea of function in *Begriffsschrift* and the analysis of methods of arithmetic in *Grundlagen* were gestating.

During the late 1880s and 1890s when *Grundgesetze I* was being finished for press and the material for *Grundgesetze II* was presumably under development, there is an especially striking concentration of graduate seminars in Riemannian complex analysis and border areas like conformal mapping and potential theory.[58]

The picture that emerges from the courses for which we do have descriptions supports extrapolations to the many courses for which we have only titles. Altogether it reinforces the observation that Frege spent a large fragment of his teaching career covering the signal topics and techniques of Riemannian complex function theory.

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57 References to courses and descriptions are assembled from Kratzsch [1979], Kreiser [1984], Kreiser [1995], and [Matsem] for the corresponding years.

58 In the one case where Frege's graduate seminar on complex analysis has an extended description (1892/1893) Frege hews carefully to the Riemann path: the object of study is defined directly in terms of the Cauchy–Riemann conditions, and multifunctions are unfolded on Riemann surfaces. In 1893 Frege studies conformal mappings in complex analysis. Conformal and isogonal mappings are also the topic in the seminars of 1888/89, 1893/94 and 1897/98. The informative descriptions for the 1888/89 and 1897/98 seminars indicate that one objective was to study conformal mappings to resolve questions about complex integration on Riemann surfaces. The 1888/89 seminar dealt with conformal plane-sphere mappings in connection with evaluating elliptic integrals. In 1901 the seminar may have addressed potential theory.

The 1893/94 seminar in conformal mapping is given the 'red flag' description 'part analytic part geometric'. The 1903/04 seminar on 'mappings' has no description, but it is concurrent with lectures on 'Complex Function Theory according to Riemann'; given that several of the preceding seminars had covered conformal or isogonal mappings, it seems reasonable to expect that this one covered them too, at least in part. The 1896/1897 seminar on mechanics touch on potential theory and the dynamics of compressible fluids. The 1882/1883 and 1900/1901 seminars on mechanics also touches on topics that would have naturally prompted a detour through potential theory, but specific details on proof methods are absent. Of the seminars that didn't touch on complex analysis and near cognate topics, most covered geometry, a further indication of Frege's mathematical inclinations.

d) Frege's research record and Nachlass

Frege's only publication in complex analysis is a thumbnail review of Thomae [1876], a work on elliptic functions and their generalizations. (As Frege notes in the review, Thomae's book is avowedly Riemannian in its approach.) However, the *Nachlass* and records of Frege's early lost scientific lectures indicate that his research on the topic was more extensive. In 1875 Frege gave a lecture to the Jena mathematical society entitled 'On some connections between complex function-theory and geometry'.[59] In that context, with that title, the topic would have been an exploration of connections in a Riemannian vein. The *Nachlass* catalogue indicates that Frege continued to carry out research, as he kept notes on power series (p. 103) and analytic functions (p. 96) as well as 17 models of Riemann surfaces (p. 102).

Frege was also active with investigations into potential theory. In 1870 and 1871, he gave several talks to the Jena mathematical society on the derivation of laws of current.[60] This interest was preserved later: the *Nachlass* catalogue lists 9 pages on 'Potential' (p. 102) and 3 notebooks containing 54 pages on 'Hydrodynamics' (p. 102).

e) Frege's Consistent, Decades Long Anti-Weierstrass Stance

Frege harshly criticized Weierstrass' theory of real numbers in *Grundgetze II*. This is taken to come out of the blue, but in fact Frege is a consistent critic of Weierstrass from *Grundlagen* on. That Frege's critical stance was this long-standing has been overlooked because his earliest shots were aimed at now-forgotten surrogates. Weierstrass was slow to publish, so textbooks that were taken to record his lectures were used as sources. This was true of Kossak [1872] and Biermann [1887], which Frege cites frequently. To read Frege's work as his readers would have, remembering that to a nineteenth-century mathematician references to these two sat under a bright sign flashing 'Weierstrass'.

Consider first Biermann [1887]. Frege wrote a cranky draft review [OCN] and fired blunderbuss asides in a draft [DRC] of his review of Cantor [RC] and in his review [RH] of Husserl's *Philosophy of Arithmetic*. Biermann's book was not just any old analysis text pulled randomly off the shelf. It represented itself as, and it was received as, the first published presentation of the foundations of complex analysis worked out in Weierstrass' Berlin seminars.[61] Frege's complaint that

[59] Kratzsch [1979] p. 544–55; Schaeffer [1877] p. 24

[60] Schaeffer [1877] p. 18

[61] Contrary to the statement by the editors in [NS] p. 81, Biermann was not a student of Weierstrass, but just someone who had obtained notes of Weierstrass' lectures and used them as the basis of a textbook. However, Frege, like most mathematicians of the time, took Biermann's self-presentation at face value. In [Gz II], for example, Frege says that in his criticism of Weierstrass he is drawing on only three sources – Kossak [1872], Biermann [1887] and some handwritten notes from Weierstrass' lectures. ([Gz II] p. 149).

Biermann made mistakes he could have avoided had he understood the definition of number in *Grundlagen*, his grumpy 'Could the author have learned this from Mr. O. Biermann?' in the review of (Weierstrass influenced) Husserl ([RH] p. 205) and Frege's general air of wounded exasperation are in part explained by the fact that these shots at Biermann [1887] were shots at the Weierstrass school as a whole, which had long failed to address what Frege regarded as his well-founded criticism.

Kossak too presented his textbook as based on Weierstrass' lectures, and it was generally so taken.[62] Kossak [1872] is cited four times in *Grundlagen*. One of the citations (p. 74) refers to the by then widespread use of 1–1 correlation as a criterion for numerical identity. Frege's discussion of this is not obviously critical, but under examination turns out to be a jab at Kossak/Weierstrass for inadequate rigor. Three of the citations are clearly critical ([FA] p. 106 p. 112–113). Two are variations on the theme that Kossak/Weierstrass 'proceeds as if mere postulation were equivalent to its own fulfillment' (p. 106, p. 111). One appears in a section on geometric and temporal interpretations of complex numbers rejected because they 'import something foreign to arithmetic'. (§103 p. 112–113) In this context he adds that the Weierstrass account:

> . . . appears to avoid introducing anything foreign, but this appearance is only due to the vagueness of the terminology. We are given no answer to the question, what does $i + 1$ really mean? Is it the idea of an apple and a pear, or the idea of toothache and gout?. . . Kossak's statement once again does not yet give us any definition at all of complex number, it only lays down the general lines to proceed along. But we need more; we must know definitely what '*i*' means, and if we do proceed along these lines and try saying it means the idea of a pear, we shall again be introducing something foreign into arithmetic. ([FA] p. 113)

This is an open slap at the Weierstrass program, and to appreciate the passage we need to see what sort of a slap it is. Earlier we encountered a commonplace of the Weierstrass circle: Weierstrass' methods were superior to Riemann's because *even if* the Riemann methods could be worked out rigorously, Weierstrass's method would *still* have the edge because it does not import anything foreign into arithmetic. Frege's rejoinder is that the purported advantage is illusory. Indeed, Weierstrass represents a step *backward*; the geometrical approach to analysis has at least the advantage that it has proven to be a fruitful way to organize the subject. By shunning Riemannian principles to keep out foreign elements and then failing to keep out foreign elements, Weierstrass has given up something of value and attained nothing in return.

[62] So, for example, the review of the book in *Jahrbuch über die Fortschritte der Mathematik* emphasizes that Weierstrass was the source. Though this is something 'everyone knew', and therefore by universal instantiation Frege knew it, it is still comforting to be able to tie Frege directly to the information: Schröder [1873] (p. 8) quotes Kossak [1872] (p. 16) and states that the view came from Weierstrass' lectures. This occurs on a page Frege cites ([FA] p. 74 fn.) in a footnote in which Kossak [1872] (p. 16) is also cited.

That Frege had Weierstrass in mind in critical discussions even as early as *Grundlagen* is reinforced by some remarks in 1906, referring back to 1884:

> Why must I always repeat the same arguments? Twenty-two years ago, in my Foundations of Arithmetic §§34–48 I presented at length what must be considered when dealing with this question... At that time, twenty-two years ago and also afterwards, even a Weierstrass could utter a farrago of balderdash when talking about the present subject. ([RT] p. 345)

In his lectures of 1914 (p. 215–223) Frege continues attacking Weierstrass by name. Here he finds room for the heavyhanded, unfunny jokes that became part of his signature in the later years. He takes up a few words of Weierstrass': 'A number is a series of things of the same kind' and characteristically presses them *ad absurdum*:

> A train is a series of objects of the same kind which moves along rails on wheels. It may be thought that the engine is nevertheless something of a different kind. Still that makes no essential difference. And so such a number comes steaming here from Berlin. ([LIM] p. 216)[63]

In the *Grundgesetze II* discussion, Frege restricts himself to Weierstrass' account of natural numbers; Frege doesn't require more because, he asserts, the defects he identifies in Weierstrass' account of natural numbers will carry over to his account of the reals, and because he takes Cantor's account of the reals to have superseded Weierstrass'. So his critique of Cantor is also a supplementary critique of Weierstrass. *Inter alia*, Frege objects that their accounts of the reals do not allow us to make sense of *applications*.

Frege criticized Weierstrass and surrogates because, in his view, they were not as rigorous as they purported to be on the concept of number. But Frege also has independent complaints about real and complex numbers that would survive even if Weierstrass took over Frege's theory of *natural* numbers *in toto*. We should not be blinkered because Frege–like Dedekind and Weierstrass–viewed appeals to geometric intuition as 'introducing something foreign to arithmetic'. Frege differed from these three on the value of geometry as an autonomous discipline and the importance of incorporating the potential for applications into mathematical frameworks. In these respects, Frege was solidly in the orthodox Riemann camp. This makes sense given his view that 'scientific workshops are logic's field of observation.'

[63] Lest we fail to twig onto the geographic jab, Frege repeats it a few paragraphs later:

> This afternoon at approximately 5:15 an express train, which is likewise a number, arrives at Sall station from Berlin...the result of multiplying our series of books by the Berlin express would again have to be a series of things of the same kind. ([LIM] p. 216)

f) Frege as Charitable Reader of Riemann

Frege's discussion of Riemann in an unpublished fragment from around 1898–1903 ([LDM] p. 158) is interesting for reasons that are sufficiently involved to require development elsewhere.[64] Here I'll restrict myself to a lighthearted observation. In these lines Frege defends Riemann against the charge of confusing sign and signified. Contrary to Frege's usual practice, he strains to read Riemann's words charitably. This is remarkable, given that we are talking about Frege, especially late Frege. This is one of the only places in Frege's writing where Frege actually goes out of his way, breaking the flow of his own arguments, to say anything complementary about anyone.

g) Infinitesimals, Magnitude, and Negative Numbers

A complicated topic in outline: Several of Frege's manoevres in his account of real numbers – often seen as idiosyncratic – are not unexpected from a Riemannian. Frege's account of the reals as magnitudes has striking affinities to Riemann's account of magnitude as distinguished from number and as conceptually connected with measurement, as articulated in his *Habilitation* lecture (Riemann [1854/1868]).[65] Also, Frege's account of negative numbers in terms of the converse of a relation (also widely regarded as a Fregean quirk) is the account in Abbe's notes on Riemann's lectures on complex analysis. Finally, Frege's puzzling view of infinitesimals – that they are acceptable if introduced by contextual definition – is in opposition to a known Weierstrassian position.

h) Geometry and Spatial Intuition: 'Changes of Space-Element'

Frege's principal non-foundational pre-occupation – analytic geometry in a 'pure projective' vein – is another signal of his preferred mathematical style. Here I'll just note three connected points that are relevant. A) Engagement with geometry is a further signal of Frege's mathematical allegiances. Indeed, in the most sophisticated geometric work Frege was engaged in the late 1890s – the study of 'contact transformations' – we find writers drawing the same Riemann vs. Weierstrass contrasts that were common in complex analysis, in connection with the problem families (Abelian integrals) that Frege is immersed in. B) Here too the contrast of 'conceptual' and 'computational' is widely discussed, in a way that allies the 'geometrical' approach with 'conceptual' thinking. C) Frege's favoured approach to geometry exploited a proof technique called 'changing the space-element', using mappings between different choices of basic geometric building blocks (lines and points, for example, or points and spheres). This development was bound

[64] The fragment is written in such a way as to make it fairly clear that it was originally intended as part of *Grundgesetze* II part III before being edited out of the final manuscript. That is, it was meant for the same section of *Grundgesetze II*, in which the uncontrolled rant about Weierstrass appears.

[65] José Ferreirós' contribution to this volume is an eye-opening discussion of further philosophical ramifications of Riemann's work in this area.

up with a kinematical reinterpretation of Kantian intuition, initiated by Helmholtz, and inspired by Riemann's famous *Habilitationsvortrag* that de-emphasized construction and replaced it with transformation invariance.[66] Frege's occasional remarks on geometry and intuition suggest he understood intuition in this transformation-based way.

IV Summing up and Looking Forward: Logic and Mathematical Practice

The work adumbrated above, plus work I present elsewhere on Frege's views on pure geometry, serve us a picture of Frege as professionally engaged with a cluster of questions relative to which the relation between geometry and pure arithmetic and analysis was fundamentally in question, in an environment where these questions were typically addressed in a distinctively Riemannian style. In some of these cases, such as the extent of validity of the Dirichlet principle, the problems remained the object of active investigation throughout the period when Frege was composing *Grundgesetze* I and II. Hence the diagnostic questions of what depends on intuition and what on logic, were especially pressing.

Though a complete treatment requires another paper, it will be worthwhile to reflect quickly on how a richer conception of Frege's context touches on our sense of what Frege was setting out to do, and what the significance of the resulting position might be. We know – it is a cliché – that Frege sought to derive, using logic alone, all those parts of mathematics that are not founded on geometry. To put it another way: Frege sought to derive all of arithmetic and analysis from logic. When such variations on the cliché are uttered, it is standard to assume that the questions 'What is mathematics / what is analysis / what is arithmetic?' are unproblematic. Of course, the philosophical question 'what is mathematics?' may be tricky, but it is assumed there is nothing to worry about in the simple descriptive questions: What is the target? Just what are you setting out to prove? But in a case where there is widespread, principled disagreement over just what 'analysis' *is* we can't be naïve about this question. Does 'analysis' include a definition of a Riemann surface and exploit conformal mapping or not? Is it engaged directly with applications? Does it contain a rigorous version of Dirichlet's principle? Are functions fixed globally by specifying singularities and relying on indirect existence theorems, or defined locally by power series? In short, does 'analysis' mean analysis as understood in Riemann's tradition, or in Weierstrass'? For Frege, 'complex analysis' is *Riemann's* complex analysis. So understood, the Fregean attitude takes a direction similar to that of Dedekind, as successors to the tradition of 'Gaussian' rather than 'Weierstrassian' rigour,

[66] This point is developed by Michael Friedman. (Friedman [2000])

which we might sum up in the slogan: 'Rigour, yes, but also clarity, conceptual simplicity and methodological awareness in lieu of brute calculation'.

There are consequences implicit in the above for our assessment what, for Frege, logic is (to the extent that 'logic' includes an account of what the fundamental logical concepts are). Frege's views on this point are rarely made explicit, and the remarks he does make don't indicate a stable position. He suggests that Dedekind's foundational work does not supported Dedekind's view that arithmetic is logic because (among other reasons) Dedekind's primitives 'system' and 'thing belongs to a thing' 'are not usual in logic and are not reduced to what is recognized as logical' ([BLA] VIII). This is an odd objection to make, and not just because it seems to rest on a brute appeal to a logical tradition that Frege himself is upending. More to the point is a *tu quoque:* the mathematical concept of *function*, which Frege takes as basic and unreduced (with 'concept' defined in terms of 'function'), was then no more usual nor more generally recognized as logical. As we've seen, adopting a general concept of function as basic was seen as methodologically charged in the circles Frege knew. What is Frege's rationale? An explicit answer is not forthcoming. Even when the issue of sharply identifying logical notions is addressed, as in [FGII], Frege says only that the answer won't be easy. In 'Function and Concept', he articulates the logical concept of function, denies that functions are expressions, and describes the pressures from science (such as complex arguments) forcing the extension of the *bedeutung* of 'function', but doesn't state a principled basis for choosing primitives delivering 'function' as the right choice.

But the absence of an explicit rationale notwithstanding, we've already seen Frege's most compelling reason for opting for the concept of function as a basis: its value in 'scientific workshops, logic's true field of observation'. Indeed, he appeals to function/argument decomposition to explain how logical reasoning extends knowledge:

> . . . of all the ways to form concepts, [listing characteristics] is one of the least fruitful. If we look through the definitions given in 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. . . [W]ith the more fruitful type of definition . . . [t]he conclusions we draw extend our knowledge. . . , and yet they can be proved by purely logical means . . . ([FA] p. 100–101)[67]

This language pops up even in those moments where Frege is taken to be the most 'philosophical', as in his account of the requirements upon an adequate specification of an object (the 'Caesar problem'). Among the (many) reasons Frege cites for crafting definitions as he does is a point about the need for

[67] That Frege is gesturing at function–argument decomposition with these metaphors is evident from his use of these metaphors elsewhere, as I explain in (Tappenden (1995)).

representation-independent definitions, and a range of different presentations of an object, to extend knowledge:

> If one were to say: q is a direction if it is introduced by means of the definition offered above, then the way in which the object q is introduced would be treated as a property of it, which it is not... If this way out were chosen, it would presuppose that an object can only be given in one single way... all equations would come down to this: that whatever is given to us in the same way is the same. But this is so self-evident and so unfruitful that it is not worth stating. The multitude of meaningful uses of equations depends rather on the fact that something can be reidentified even though it is given in a different way. ([FA] §67)[68]

There is more to be said, but this will provide a first foothold, indicating how thoroughly Frege's method was entwined with ongoing mathematical inquiry. His preference for general over piecemeal definitions, his choice of 'function' as a basis for his logic, his treatment of magnitude and infinitesimals, and even his account of objects: these and other points of his foundations are rich with significance for the mathematics around him. These examples reveal the extent to which Frege's 'acid test' of scientific fruitfulness for concepts was embedded in his philosophy as a whole. His philosophical treatment would not have appeared to those around him to be mathematically neutral.

To be sure, any connection to Frege is a bonus: the methodology of Riemannian mathematics, as articulated in diverse ways by subsequent followers, is of considerable philosophical interest independently of any links to the figures studied in 'official' histories of philosophy. But the hook to Frege does induce a re-evaluation of how we conceive the relations between the history of analytic philosophy and the history of science and mathematics. A tacit assumption apparently guiding much recent philosophy of mathematics is that it requires little or no concern for the history and contemporary status of the frontiers of mathematical investigation. Those of us who adopt the opposing stance that our choice of fundamental concepts should be sensitive to the value of those concepts as revealed in practice can not only draw upon the rich Riemannian tradition but can also take heart from finding its echoes at the well-springs of the analytic tradition.

References

Ahlfors L. [1953] 'Development of the Theory of Conformal Mapping and Riemann Surfaces through a Century' in *Contributions to the theory of Riemann Surfaces*, Annals of Mathematics Studies **30** Princeton: Princeton University Press pp. 3–13.

[68] The mathematical work that fits most closely these remarks is Dedekind's Riemann-inspired treatment of the theory of ideals, in the wake of Kummer's prior account. I don't discuss this work here, but rather in Tappenden [2005].

[1966] *Complex Analysis* (2nd edn) New York: McGraw-Hill.
Bachmann P. [1872] *Die Lehre von der Kreistheilung und ihre Beziehungen zur Zahlentheorie* Leipzig: Teubner.
H. Benke and K. Kopfermann (eds) [1966] *Festschrift zur Gedächtnisfeier für Karl Weierstraß, 1815–1965* Köln: Westdeutscher Verlag.
Betti E. [1885] *Lehrbuch der Potentialtheorie und ihrer Anwendungen auf Elektrostatik und Magnetismus*. (German translation by W. F. Meyer) Stuttgart: Kohlhammer.
Biermann O. [1887] *Theorie der Analytischen Funktionen* Liepzig.
Bottazini U. [1986] *The Higher Calculus: A History of Real and Complex Analysis from Euler to Weierstrass* W.van Egmund (trans.) Berlin: Springer.
[1994] 'Three Traditions in Complex Analysis: Cauchy, Riemann and Weierstrass' in I. Grattan-Guinness (ed.) *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences* vol I London: Routledge pp. 419–431.
Bottazini U. and Tazzioli R. [1995] '*Naturphilosophie* and its Role in Riemann's Mathematics' *Revue d'histoire des Mathématiques* **1** pp. 3–38.
Brill A. and Nöther M. [1894] 'Die Entwickelung der Theorie der algebraischen Functionen in älterer und neuerer Zeit' *Jahresbericht der Deutschen Mathematiker-Vereinigung* **3** pp. 107–566.
Burkhardt, H. [1897] *Einführung in die Theorie der analytischen Funktionen einer complexen Veränderlichen* Leipzig: Verlag Von Vliet.
Casorati, F. [1868] *Teorica delle funzioni di variabili complexi* Pavia.
Chevalley, C. [1951] *Introduction to the Theory of Algebraic Functions of One Variable* New York: American Mathematical Society, Reinhart and Wilson.
Clebsch A. and Lindemann F. [1876] *Vorlesungen Über Geometrie* (lectures by Clebsch transcribed and edited by Lindemann) (vol. 1) Leipzig.
Clebsch, A. and Gordon, P. [1866] *Theorie der Abelschen Funktionen* Leipzig: Teubner.
Courant R. [1950] *Dirichlet's Principle, Conformal Mapping and Minimal Surfaces* New York: Wiley interscience.
Currie, G. [1982] *Frege: An Introduction to his Philosophy* Totowa N.J.: Barnes and Noble.
Dedekind, R. [1854/1996] 'On the Introduction of New Functions in Mathematics' W. Ewald (trans.) in Ewald [1996] p. 754–762 (Delivered June 1854) (German original in Dedekind *Werke* III p. 428–438).
[1872/1901] 'Continuity and Irrational Numbers' W. Behman (trans.) Chicago: Open Court (original pub. 1872, translated 1901, page reference to Dover reprint 'Essays on the Theory of Numbers' of 1963).
[1876a] 'Letter to R. Lipschitz ' (=*Werke* III p. 464–482).
[1877] 'Schreiben an Herrn Borchardt über die Theorie der elliptischen Modulfunctionen' *Journal für Reine und Angewante Mathematik* **83** pp. 265–292 (=Werke I pp. 174–201).
[1888/1901] 'The Nature and Meaning of Numbers' W. Behman (trans.) Chicago: Open Court (original pub. 1888, translated 1901, page reference to Dover reprint 'Essays on the Theory of Numbers' of 1963).
[1895] 'Über die Begründung der Idealetheorie' *Nachrichten von der Königlichen Gesellschaft der Wissenschaften zu Göttingen* Mathem. – Phys. Klasse (pp. 106–113) (page references to Werke vol. II pp. 50–58).
Dedekind, R. and Weber H. [1882] 'Theorie der algebraischen Funktionen einer Veränderlichen' *Journal für Reine und Angewante Mathematik* **92** p. 181–290 (dated 1880) (page reference to the reprinting in Dedekind's *Werke* vol. I p. 238–349).

Demopoulos, W. [1994] 'Frege and the Rigorization of Analysis' *Journal of Philosophical Logic* **23** pp. 225-245, page references to the reprinting in Demopoulos [1995].
Dühring E. [1873] *Kritische Geschichte der Allgemeinen Prinzipien der Mechanik* Berlin.
Dummett M [1991] *Frege: Philosophy of Mathematics* Cambridge Mass.: Harvard University Press.
Enneper A. [1876] *Elliptische Funktionen: Theorie und Geschichte* Halle.
Ewald W. [1996] *From Kant to Hilbert: A Source Book in the Foundations of Mathematics* Oxford: Oxford University Press.
Ferrierós J. [1999] *Labyrinth of Thought: A History of Set Theory and its Role in Modern Mathematics* Boston: Birkhäuser.
Frege, G. [1877] Review of Thomae [1876a] in Frege [1984].
[1879/1967] *Begriffsschrift, A Formula Language Modeled on that of Arithmetic, for Pure Thought* Stephan Bauer-Mengelberg trans. In *From Frege to Gödel* van Heijenoort, J. (ed.) (Cambridge: Harvard University Press).
[1882] 'Boole's Logical Calculus and the Concept-Script' in Frege [1979].
[1884] *The Foundations of Arithmetic* J.L. Austin trans. 2nd rev. edn Evanston: Northwestern University Press.
[1890?] Draft of a review of Cantor's *Gesammelte Abhandlungen zur Lehre vom Transfiniten* in Frege [1967].
[1891] 'Function and Concept' in Frege [1984].
[1892] Review of Cantor, G. *Zur Lehre vom Transfiniten: Gesammelte Abhandlungen aus der Zeitschrift für Philosophie und philosophische Kritik* (1892) in Frege 1984.
[1892?] 'On The Concept of Number' in Frege [1979].
[1893/1964] *Basic Laws of Arithmetic: Exposition of the System* Furth, M. trans. Berkeley: University of California Press 1964 (1893) (Gz II appendix: 1903).
[1894] Review of Husserl, E. *Philosophy of Arithmetic* in Frege [1984].
[1899?] 'Logical Defects in Mathematics' in Frege [1979].
[1903] *Grundgesetze der Arithmetik* vol II Jena.
[1906] 'On the Foundations of Geometry: Second Series' in Frege [1984] *Collected Papers*.
[1906a] 'Reply to Mr. Thomae's Holiday *Causerie*' in Frege [1984].
[1914] 'Logic in Mathematics' in Frege [1979].
[1979] *Posthumous Writings* Hermes H. Kambartel F. Kaulbach F. (eds.) (Oxford: Basil Blackwell).
[1984] *Collected Papers on Mathematics, Logic and Philosophy* McGuinness B. (ed.) Oxford: Basil Blackwell.
Freudenthal, H [1975] 'Riemann' in *Dictionary of Scientific Biography* vol. 11 New York, pp. 447–56.
Friedman, M. [2000] 'Geometry, Construction and Intuition in Kant and his Successors' *Between Logic and Intuition* Sher, G. and Tieszen R. (eds.) Cambridge: Cambridge University Press p. 186–218.
Frostman O. [1966] 'Aus dem Briefwechsel von G. Mittag-Leffler' in H. Benke and K. Kopfermann (eds) [1966] p. 53–56.
Gauss, F. [1801] *Disquisitiones Arithmeticae* reprinted as Gauss' *Werke* vol. I.
[1966/1801] *Disquisitiones Arithmeticae* (English translation by A. Clarke of Gauss [1801]) New Haven: Yale University Press.
Goursat E. [1900] 'Sur La Définition Générale des Functions Analytiques, d'après Cauchy' *Transactions of the American Mathematical Society* **1** 1 pp. 14–16.

Griffiths, H. [1976] *Surfaces* Cambridge: Cambridge University Press.
Gronau D. [1997] 'Gottlob Frege, A Pioneer in Iteration Theory' in *Iteration Theory (ECIT 94) Proceedings of the European Conference on Iteration Theory, Opava* Grazer mathematische Berichte #334 p. 105–119.
Grünbaum B. [1976] 'Lectures in Lost Mathematics' unpublished mimeographed notes.
Guinand A. [1979] 'The Umbral Method: A survey of Elementary Mnemonic and Manipulative Uses' *American Mathematical Monthly* **86** 3 pp. 187–195.
Hallett M. [1984] *Cantorian Set Theory and Limitation of Size* Oxford: Oxford University Press.
Hawkins, T. [2000] The Emergence of the Theory of Lie Groups. An Essay in the History of Mathematics 1869–1926 Berlin: Springer Verlag.
Hilbert D. [1901] 'Über das Dirichletsche Prinzip' *Jahresbericht der Deutschen Mathematiker-Vereinigung* **8** pp. 184–8.
[1904] 'Über das Dirichletsche Prinzip' *Mathematische Annalen* **59** pp. 161–86.
Husserl E. [1891] Philosophie der Arithmetik. Psychologische und Logische Untersuchungen vol 1 Halle: C. Pfeffer.
Jacobi C. [1829] *Fundamenta Nova Theoriae Functiorum Ellipticarum* Frankfurt.
Jungnickel C. and McCormmach R. [1986] *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* vol. 1 Chicago: University of Chicago Press.
Kellogg O. [1929] *Foundations of Potential Theory* New York: Dover (1959 reprint).
Kerry B. [1889] 'Anschauung und ihr Psychische Verarbeitung' (5^{th} article) in *Vierteljahrsschrift für Wissenschaftliche Philosophie* pp. 71–124.
Kitcher P. [1984] *The Nature of Mathematical Knowledge* Oxford: Oxford University Press.
[1986] 'Frege, Dedekind, and the Philosophy of Mathematics' in L. Haaperanta and J. Hintikka (eds.) *Frege Synthesized* Dordrecht: D. Reidel pp. 299–344.
Klein F. [1884/1893] 'On Riemann's Theory of Algebraic Functions and Their Integrals' Cambridge: McMillan and Bowes 1893 (page references to Dover reprint, German original published 1884).
[1895] 'Arithmetizing of mathmatics' English translation by I. Maddison appears in Ewald [1996] p. 965–971 page references to this translation.
Kreiser L. [1983] 'Nachschrift einer Vorlesung und Protokolle mathematischer Vorträge Freges' Supplement to G. Frege *Nachgelassene Schriften* H. Hermes, F. Kambartel and F. Kaulbach eds. Hamburg: Felix Meiner Verlag.
Laugwitz D. [1992] ' 'Das letzte Ziel ist immer die Darstellung einer Funktion': Grundlagen der Analysis bei Weierstraß 1886 historische Wurzeln und Parallelen' *Historia Mathematica* **19** pp. 341–355.
[1999] *Bernhard Riemann:* 1826–1866 *Turning Points in the Conception of Mathematics* A. Schnitzer (trans.) Boston: Birkhäuser.
Lie S. and Scheffers G. [1896] *Geometrie Der Berührungstransformationen* Leipzig (1956 reprint: Chelsea, New York).
Monna, A. [1972] 'The Concept of Function in the 19^{th} and 20^{th} Centuries in Particular with Regard to the Discussions between Baire, Borel, and Lebesgue' *Archive for the History of Exact Sciences*.
[1975] *Dirichlet's Principle: A Mathematical Comedy of Errors and its Influence on the Development of Analysis* Utrecht: Oosthoek, Scheltema & Holkema.
Moore G. [1982] *Zermelo's Axiom of Choice: Its Origins, Development and Influence* Springer: New York.

Neuenschwander E. [1997] *Riemanns Einführung in die Funktionentheorie. Eine quellenkritische Edition seiner Vorlesungen mit einer Bibliographie zur Wirkungsgeschichte der Riemannschen Funktionentheorie* Göttingen: Vandenhoeck and Ruprecht.

Pincherle S. [1880] 'Saggio di una introduzione alla teoria delle funzione analitiche secondo i princippi del prof. C. Weierstrass' Giornale di Mathematiche 18 pp. 178–254, 314–57.

Riemann, B. (Page references to *Gess. Werke*. are to the second edition [1892/1902]; this is reproduced with page numbers included in [1990]).

[1851] *Grundlagen für eine allgemeine Theorie der Funktionen einer veränderlichen complexen Grösse* Inaugural dissertation Göttingen (=*Werke*, p. 3–45).

[1854/1868] 'Ueber die Hypothesen, welche der Geometrie zu Grunde Liegen' (Habilitation lecture, first published in posthumously in *Abhandlungen der Koenigliche Gessellschaft d. Wissenschaft zu Göttingen* **XIII** (=*Werke* p. 272–287) English translation by W. Clifford (revised by Ewald) in Ewald [1996] p. 652–661 page references to Ewald translation.

[1854/1868a] 'Ueber die Darstellbarkeit einer Function durch eine Trigonometrische Reihe' (Habilitation paper, first published posthumously in *Abhandlungen der Koenigliche Gessellschaft d. Wissenschaft zu Göttingen* **XIII** (=Werke p. 272–287).

[1857a] 'Theorie der Abel'schen Funktionen' *Journal für Reine und Angewandte Mathematik* (Crelle's Journal) **54** pp. 101–55 (=*Werke* pp. 88–153).

[1857b] 'Beiträge zur Theorie der durch die Gauss'sche Riehe F(α, β, γ, x) darstellbaren Functionen' *Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen* **6** (=*Werke* pp. 67–83).

[1859] 'Über die Anzahl die Primzahlen unter einer gegebener Grösse'.

[1892/1902] *Gesammelte Mathematische Werke* H. Weber (ed.) (2nd edn 1892 with supplement 1902) New York: Dover Publications (1953 reprint).

[1990] *Gesammelte Mathematische Werke, Wissenschaftlicher Nachlass, und Nachträge Collected Papers* R. Narasimhan (ed.) (critical 3rd edn) Berlin: Springer.

Roch G. [1863] 'Ueber functionen complexer Grössen' *Zeitschrift für Mathematik und Physik* **8** p. 12–26 p. 183–203.

[1865] 'Ueber functionen complexer Grössen' *Zeitschrift für Mathematik und Physik* **10** p. 169–94.

Russell B. [1919] *Introduction to Mathematical Philosophy* London: Allen and Unwin.

Russell, B. and Whitehead A. [1927] *Principia Mathematica* (2nd edn) Cambridge: Cambridge University Press.

Schappacher N. [2003?] 'What is Arithmetization?' forthcoming in the proceedings of the conference *Two Hundred Years of Number Theory after Carl Friedrich Gauss' Disquisitiones Arithmeticae* Berlin: Springer.

Schlömilch O. [1862] *Handbuch der Algebraischen Analysis* Jena: Frommann.

[1868] *Übungsbuch der Höheren Analysis* Leipzig: Teubner.

Scholz H. and Bachmann, F. [1935] 'Der Wissenschaftliche Nachlass der Gottlob Frege' *Actes du Congrès International de Philosophie Scientifique VII: Histoire de la Logique ou de la Philosophie Scientifique* Paris: Sorbonne p. 24–30.

Schwartz H. [1870] 'Über einen Grenzübergang durch altirneirendes Verfahren' *Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich* **15** 1870 pp. 272–286. (=*Abhandlungen* **2** pp. 133–143).

Simons, P. [1987] 'Frege's Theory of Real Numbers' reprinted in Demopoulos [1995] pp. 358–85.

Springer G. [1957] *Introduction to Riemann Surfaces* New York: Addison-Wesley.
Stahl H. [1896] *Theorie der Abel'schen functionen* Liepzig: Teubner.
[1899] *Elliptischen functionen. Vorlesungen von Bernhard Riemann* Liepzig: Teubner.
Tappenden J. [1995] 'Extending Knowledge and 'Fruitful Concepts': Fregean Themes in the Foundations of Mathematics' *Noûs* **29** pp. 427–467.
[1995a] 'Geometry and Generality in Frege's Philosophy of Arithmetic' *Synthèse* **102** pp. 319–361.
[2005] 'The Mathematical Background of the Caesar Problem' Forthcoming in a *Dialectica* special issue on the Julius Caesar problem.
[----] *Philosophy and Mathematical Practice: Frege in his Mathematical Context* to appear with Oxford University Press.
[----a] Explanation and Mathematical Proof: why do Elliptic Functions have Two Periods? in preparation.
Temple, G. [1981] 100 *Years of Mathematics* Berlin: Springer.
Thomae J. [1876] *Sammlung von Formeln, welche bei Anwendung der Elliptischen und Rosenhainschen Funktionen Gebraucht Werden* 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. ed. *Studien zu Frege/Studies on Frege vol I* Stuttgart-Bad Cannstatt: Fromann-Holzboog 49–106.
Wagner S. [1992] 'Logicism' in M. Detlefsen (ed.) *Proofs and Knowledge in Mathematics* Routledge, London pp. 65–110.
Weierstrass K. [1870] 'Über das sogennante Dirichlet'sche Prinzip' read in 1870 (= *Werke* **2** [1895] pp. 49–54).
[1875] 'Letter to H. Schwartz' *Werke* **2** p. 235.
Weiner, J. [1984] 'The Philosopher Behind the Last Logicist' in C. Wright (ed.) *Frege: Tradition and Influence* Oxford: Blackwell.
[1990] *Frege in Perspective* (Ithaca NY: Cornell University Press).
[2000] *Frege* Oxford: Oxford University Press.
Youskevich A. [1976] 'The Concept of a Function up to the Middle of the Nineteenth Century' *Archive for the History of Exact Sciences* **7** pp. 37–85.
Edwards, H. [1987] 'Dedekind's Invention of Ideals' in E. Phillips (ed.) *Studies in the History of Mathematics* MAA monographs **26** pp. 8–20.
Chrystal G. [1886] Algebra (vols I and II) Edinburgh.
Stein, H. [1988] 'Logos, Logic and Logistiké: Some Philosophical Remarks on the Nineteenth-Century Transformation of Mathematics' in Aspray, W. and Kitcher, P. *History and Philosophy of Modern Mathematics* Minneapolis: University of Minnesota Press pp.238–259.
Enneper 1875 should indeed read Enneper 1876.
Brill\Noether 1892 should indeed read Brill\Noether 1894.
Krieser 1984 should indeed be 1983.
Abel N. [1827/1828] 'Recherches sur les fonctions elliptiques' *Journal für die Reine und Angewandte Mathematik* pp. 101–181, pp. 160–191 (= *Oeuvres* I pp. 263–388).
Schappacher should be (I believe) 'to appear'.
Kossak E. [1872] *Die Elemente der Arithmetik* Berlin: Nicolai'sche Verlagsbuchhandlung.
Gray and Morris 1978: When is a Function that Satisfies the Cauchy-Riemann Equations Analytic?

J. D. Gray, S. A. Morris *American Mathematical Monthly*, Vol. 85, No. 4 (Apr., 1978), pp. 246–256.
Maddy P. [1997] *Naturalism in Mathematics* Oxford: Oxford University Press
Rowe:
[1989] 'Klein, Hilbert, and the Göttingen Mathematical Tradition' *Osiris* 2nd Series **5** pp.186–213.
[2000] 'Episodes in the Berlin – Göttingen Rivalry 1870–1930' *Mathematical Intelligencer* **22.1** pp. 60–69.
[-a-] 'The Philosophical Views of Klein and Hilbert' to appear.
Burkhardt, H. [1906/1913] *Einführung in die Theorie der analytischen Funktionen einer complexen Veränderlichen* (2nd ed.) Leipzig: Verlag Von Vliet.
Riemann, B. [1865] 'Ueber das Verschwinden der Theta – Functionen' *Journal für Reine und Angewandte Mathematik* (Crelle's Journal) **65** (=*Werke* p. 212–224).
Hilbert D. [1897/1998] *The Theory of Algebraic Number Fields* Adamson, I. (Trans.) (originally published 1897) Berlin: Springer Verlag.
Neumann, C. [1877] *Untersuchungen über das Logarithmische und Newton'sche Potential* Leipzig: Teubner.
Weierstrass, K. [1886/1988] *Ausgewählte Kapitel aus der Funktionenlehre. Vorlesung, gehalten in Berlin. Mit der akademischen Antrittsrede, Berlin 1857 und drei weiterenn Originalarbeiten von K. Weierstrass aus den Jahren 1870 bis 1880/6* edited with commentary by R. Siegmund-Schultze Teubner – Archiv zur Mathematik, Bd. **9** Liepzig: Teubner.
Kratzsch, I. [1979] 'Material zu Leben und Wirken Freges aus dem Besitz der Universitätsbibliothek Jena' in *Begriffsschrift – Jenaer Frege Konferenz*, Fr. Schiller Universität Jena pp. 534–46.
Kreiser, L. [1984] 'G. Frege Grundlagen der Arithmetik – Werk und Geschichte' in G. Wechsung (ed.) *Frege conference 1984* Berlin.
[1995] 'Die Hörer Freges und sein Briefpartner Alwin Korselt' in *Wittgenstein Studies*, Diskette 1/1995.
The Koenigsberger 1919 reference is to ***Mein Leben*** Heidelberg 1919.
Schaeffer H. (ed.) [1877] *Erinnerungsblätter der Mathematischen Gessellschaft zu Jena* Jena.
Kitcher. P. [1981]: 'Mathematical Rigor – Who Needs It?' *Noûs* **15** pp.469–93.
Replace the reference to '{Mathsem]' with:
'The records, compiled and published by Thomae, of the mathematical seminar at Jena, from Easter to Easter of each year)'
Quine, W. [1960] *Word and Object* Cambridge: MIT Press.
Key to Frege References:
[dates of publication (composition if unpublished) are indicated after the references]
Collected Papers on Mathematics, Logic and Philosophy McGuinness B. (ed.) Oxford: Basil Blackwell 1984.
Conceptual Notation and Related Articles Bynum T. (ed.) (Oxford: Oxford University Press: 1972).
Posthumous Writings Hermes H. Kambartel F. Kaulbach F. (eds.) (Oxford: Basil Blackwell 1979).
On the Foundations of Geometry and Formal Theories of Arithmetic Kluge, E.H. ed. (New Haven: Yale University Press 1971).
Nachgelassene Schriften (second, expanded edition) Hermes H. Kambartel F. Kaulbach F. (eds.) (Hamburg: Felix Meiner Verlag).

AQ: Kindly confirm the replacement of [Mathsem].

[ASC] 'The Argument for my Stricter Canons of Definition' in *Posthumous Writings*.
[B] *Begriffsschrift, A Formula Language Modeled on that of Arithmetic, for Pure Thought* Stephan Bauer-Mengelberg trans. In *From Frege to Gödel* van Heijenoort, J. (ed.) (Cambridge: Harvard University Press 1967).
[Brief] *Wissenschaftlicher Briefwechsel* Gabriel, G, Hermes, H. Kambartel, F. Thiel, C and Veraart, A. (eds.) Hamburg: Felix Meiner Verlag 1976.
[BLA] *Basic Laws of Arithmetic: Exposition of the System* Furth, M. trans.Berkeley: University of California Press 1964 (1893) (Gz II appendix: 1903).
[BLC] 'Boole's Logical Calculus and the Concept-Script' in *Posthumous Writings*.
[CO] 'On Concept and Object' in *Collected Papers* (1892).
[Corr] *Philosophical and Mathematical Correspondence* Gabriel, G, Hermes, H. Kambartel, F. Thiel, C and Veraart, A. Abridged from the German edition by McGuinness, B. Translated by Kaal, H. Chicago: University of Chicago Press, 1980.
[CT] 'Compound Thoughts' in *Collected Papers* (1923–26).
[DRH] Draft towards a Review of Cantor's *Gesammelte Abhandlungen zur Lehre vom Transfiniten* in *Posthumous Writings* (1890–1892?).
[ESL] 'A Critical Elucidation of some Points in E. Schröder, *Vorlesungen über die Algebra der Logik* in *Collected Papers* (1895).
[FA] *The Foundations of Arithmetic* J.L. Austin trans. 2nd rev. ed. Evanston: Northwestern University Press (1884).
[FC] 'Function and Concept' in *Collected Papers* (1891).
[FG] 'On the Foundations of Geometry: First Series' in *Collected Papers* (1903).
[FGII] 'On the Foundations of Geometry: Second Series' in *Collected Papers* (1906).
[FTA] 'Formal Theories of Arithmetic' in *Collected Papers* (1885).
[IFP] 'On A Geometrical Representation of Imaginary Forms in the Plane' in *Collected papers* (1873).
[Gz] *Grundgesetze der Arithmetik* vol I Jena 1893.
[GzII] *Grundgesetze der Arithmetik* vol II Jena 1903.
[LDM] 'Logical Defects in Mathematics' in *Posthumous Writings* (ca.1899–1903).
[LOI] 'On the Law of Inertia' in *Collected Papers* (1891).
[LG] 'Logical Generality' in *Posthumous Writings* (1923 or later).
[LGP] 'Lecture on the Geometry of Pairs of Points in the Plane' *Collected Papers* (1883).
[LM] 'Logic in Mathematics' in *Posthumous Writings* (1914).
[LWC] 'Lecture on a Way of Conceiving the Shape of a Triangle as a Complex Quantity' in Collected Papers (1878).
[MCE] Methods of Calculation Based on an Extension of the Concept of Quantity' in *Collected Papers* (1874).
[Nach] *Nachgelassene Schriften* (second, expanded edition) Hermes H. Kambartel F. Kaulbach F. (eds.) (Hamburg: Felix Meiner Verlag).
[NAF] 'A New attempt at a Foundation for Arithmetic' in *Posthumous Writings* (ca.1924).
[OCN] 'On The Concept of Number' in *Posthumous Writings* [1891/1892?].
[OEG] 'On Euclidean Geometry' in *Posthumous Writings* (1899–1906?).
[RGW] Review of Gall and Winter: *Die Analytische Geometrie des Punktes und der Geraden und Ihre Anwendung auf Aufgaben* in *Collected Papers* (1877).
[RC] Review of Cantor, G. *Zur Lehre vom Transfiniten: Gesammelte Abhandlungen aus der Zeitschrift für Philosophie und philosophische Kritik* (1892).
[RH] Review of Husserl, E. *Philosophy of Arithmetic* in *Collected Papers* (1894).

[Rho] Review of Hoppe R. [1880] in *Collected Papers* (1880).
[RTh] Review of Thomae [1876a] in *Collected Papers* (1877).
[RP] 'Renewed Proof of the Impossibility of Mr. Thomae's Formal Arithmetic' in *Collected papers* (1908).
[RT] 'Reply to Mr. Thomae's Holiday *Causerie*' in *Collected papers* (1906).
[RTh] Review of Thomae [1876a] in Collected Papers (1877).
[SKM] 'Sources of Knowledge of Mathematics and the Mathematical Natural Sciences' in *Posthumous Writings* (1924/1925).
[SM] 'On Sense and Meaning' in in *Collected papers* (1892).
[SN] 'On Mr. Schubert's Numbers' in *Collected Papers* (1899).
[SJCN] 'On the Scientific Justification of a Conceptual Notation' in *Conceptual Notation and Related Articles* (1882).
[Th] 'Thoughts' in *Collected Papers* (1918).
Special abbreviations:
[MathSem (year) – (year) + 1]: The records, compiled and published by Thomae, of the mathematical seminar at Jena, from Easter of (year) to Easter of (year)+ 1
[Much of the material in these records has been reprinted in Kreiser [2001] p.301–320].