Frege's Mathematical Setting[1]

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Note: This essay was commissioned and written circa 1997 for The Cambridge Companion to Frege, which has not yet appeared. Although quite a bit of good work on Frege and Dedekind's mathematical antecedents has since appeared (by Jeremy Avigad, Jamie Tappenden and others), I think this essay still provides the student with a useful introduction to many of the central issues. I have since refined my own conception of Frege's originally intended procedures, which I have also posted on this site as "Ghost World: A Context for Frege's Context Principle."

Although Gottlob Frege was a professional mathematician, having trained at one of the world's greatest centers for mathematical research, it has been common for modern commentators to assume that his interests in the foundations of arithmetic were almost wholly "philosophical" in nature, unlike the more "mathematical" motivations of a Karl Weierstrass or Richard Dedekind. As Philip Kitcher expresses the thesis:

> *The mathematicians did not listen* [to Frege because]... *none of the techniques of elementary arithmetic cause any trouble akin to the problems generated by the theory of series or results about the existence of limits.*[2]

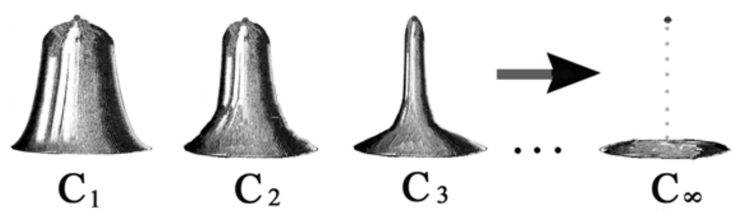
Indeed, Frege's own presentation of his work easily encourages such a reading. Nonetheless, recent research into Frege's professional background reveals ties to a rich mathematical problematic that, pace Kitcher, was as central to the 1870's as any narrow questions about series and limits per se. An appreciation of the basic facts involved, which this essay will attempt to describe in non-technical terms, can only heighten our appreciation of the depths of Frege's thought and of the continuing difficulties that philosophy of mathematics must confront. Furthermore, although it is probably possible to comprehend Frege's basic approach to language on its own terms, some awareness of the rather unusual *examples* that he encountered in the course of his mathematical work can only enhance our understanding of his motivations within linguistic philosophy as well.

To put Frege's problematic in its proper setting, it is useful to begin by asking ourselves: how do *we* determine whether a purported mathematical structure exists? Modern mathematical orthodoxy has settled upon the following response:

mathematics is free to study any subject that can be legitimated as a well defined class within the set theoretical hierarchy. This view can be called *set theoretic absolutism*, for it makes reduction to set theory the final arbiter of mathematical existence. Although such ontological absolutism is "official policy" today, most mathematicians (who often don't like set theory much) harbor in their bosoms a rather different view of mathematical existence, which we can dub *naïve structuralism*: *mathematics should be free to study the properties of any self-consistent, free standing construct*. No barriers should interfere with a mathematician's right to study any structure she chooses.

The adjectival "*free standing*" marks the gulf that divides set theoretic absolutism from naïve structuralism. An example will illustrate the contrast. Modern physics informs us that our universe possesses the structure of a so-called "four-dimensional, Lorentzian manifold". In a geometry of this kind, our familiar Euclidean notion of "distance" is replaced by a "distance" relationship that can take on negative values and where distinct spacetime points are allowed to lie zero "distance" apart. Indeed, *inside* such spacetimes no "spaces" of an Euclidean type typically subsist (under a natural understanding of "subsist"). Nonetheless, an orthodox *mathematical* treatment of Lorentzian manifolds renders their *mathematical existence parasitical* on whether pieces of these supposed structures can be mapped into Euclidean spaces. In other words, mathematical practice is unwilling to treat the *existence* of Lorentzian manifolds as a matter that can "stand free" of the existence of Euclidean spaces, even though in nature the first happily exists without the latter. Worse yet, mathematics requires the existence of the Euclidean spaces to turn upon the existence of certain largish sets. Isn't this odd? We live smack in the middle of a structure that mathematics won't accept without taking a long excursus through *sets*, objects that still worry many of us as teetering on the edge of paradox. Naïve structuralists grumble accordingly, "Well, I guess I can tolerate set theory's authoritarian sway over ontology *as long as it promises to give me everything I would wish to study anyway*".

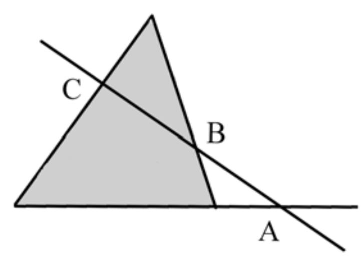
Why should set theory be assigned this odd *epistemological privileging,* so contrary to the preferences of a naïve structuralist?[3] Why can't a mathematician simply *dream up* any structure she wants and let *it* tell her what is true therein? The devastating reply to such a philosophy of genial tolerance is that unfortunate mistakes have been introduced under the banner of ontological freedom. Perhaps the most famous illustration of this danger was provided by the celebrated work in complex function theory by G.F.B. Riemann, a history that was well known to Frege, as his teacher Alfred Clebsch labored mightily that render Riemann mathematically respectable. Riemann had urged that the behavior of many functions can be better understood if placed upon a so-called "Riemann surfaces", figures that cannot be interpreted in terms of familiar spatial forms. For example, the Riemann surface associated with %x is an odd sort of structure like a two floor parking garage *except* that one drive through this garage continuously and never cut through a floor. To prove key facts about his surfaces, Riemann relied upon an existence criterion he dubbed "Dirichlet's Principle": if a collection of functions can be graded by positive number assignments, then some minimal function must exist within this set. A simple illustration[4] of this principle is the following. Suppose one has a wire rim of some shape and places a soap film across it. A soap film stores energy according its degree of bending; we can use a film's total amount "stored energy" as the "positive number" cited in Dirichlet's principle. Intuitively we expect the soap film to settle into the shape that that attaches to the wire with the *least amount of stored energy.* Dirichlet's principle simply declares that at least one minimal surface with the least amount of stored energy must exist (it is possible that several cofigurations might store the same quantity of energy). Unfortunately, this principle cannot be true in general: let our "rim" consist a regular oval *plus* a single point above its center. Consider the sequence of shapes $\mathbf{C}_1$, $\mathbf{C}_2$,... Note that the amount of bending can be decreased continuously but never reach a minimal surface: the proper "limit" $\mathbf{C}_4$ of all these shapes contains a discontinuous "jump" that disqualifies it as a surface altogether. Without some deep repair, Dirichlet's Principle cannot be regarded as reliable.



So while Riemann enjoyed the luxury of discovering wonderful results by daydreaming about his sometimes impossible surfaces, the mathematicians who came after were forced to "redo Riemann" by searching out mathematically grounded expedients that might support his results. Much of Weierstrass' fussing about how power series piece together, for example, arose as a means of building a solid structure underneath Riemann-like results.

The moral is that postulating new structures in mathematics can be risky unless reliable assurance is supplied that such structures exist. In the specific case of Riemann's work, most mathematicians of Frege's time did not attempt to legitimate Riemann's original picture, but simply replaced his surfaces with more orthodox sections of mathematical ontology. However, much of Frege's own philosophy of mathematics seems to have emerged from an established program for certifying other novel mathematical objects. This is the movement I shall call "*relative logicism*" later in the essay.

By the mid-nineteenth century it was realized that astonishing progress could be made in a wide variety of subjects if *new* entities were admitted into mathematics' traditional realms. There were many of these; we shall mainly discuss "ideal" numbers and points here (such supplementary objects will be lumped together under the generic title "extension elements"). Philosophically, however, no rationale stood ready to tolerate these novel elements. The century had begun under the dominion of Immanuel Kant, who believed that the scope of mathematics could reach only to the arithmetical and geometrical questions legitimated within our a priori "intuitions" of quantity and quality. To understand the strong hold of Kantian thinking (to the best of our knowledge Frege never abandoned the conviction that Kant was correct about the sources of geometrical validity), we must avoid conceptualizing "intuition" crudely as a little bird that whispers necessary propositions in our ear. Perform the following thought experiment (usually associated with Moritz Pasch's work in geometry). Construct an arbitrary triangle and extend one of the sides. Select a point A on this line and draw a straight line L over to some B on the near side of the triangle. Is there any way that L can avoid hitting the remaining side at C? It seems not. But *what allows us to know* that L can't escape the triangle before it reaches C? It surely can't be a matter of *logic* for, insofar as logic cares, L can exit the triangle before it encounters the other side. Something else, it seems, forces L to meet the remaining side: some principle of conceptual closure that arises, in Kant's diagnosis, from the structuring of our intuitive faculties.



By attending to cases like this, one can develop a proper sympathy for the view that *some extensive branch of mathematical knowledge can be founded in this array of intuitions*. One can retain this conviction without denying that mathematics can be allowed, if sufficient rationale is provided, to study quasi-"geometrical" structures in which such intuitions fail (Frege, in fact, did study "geometries" of this kind). Unlike Kant, one might allow physics to decide on empirical grounds that Euclidean geometry is not directly instanced in the physical world before us (or to agree with Henri Poincare that the question of geometry's physical instantiation is a matter of scientific convention).

One can even concede, in company with the young Bertrand Russell, that proper Kantian intuition does not reach to everything Euclid proved: any Euclidean propositions that involve the equality of angles or lengths tacitly incorporate considerations of *measurement* whose results can only reflect what empirically transpires when instruments are moved around in physical space.

A neo-Kantian can cheerfully make all of these concessions without altering in any material way the conviction that, *epistemologically*, the ultimate source of a large portion of mathematics' great store of knowledge traces to those otherwise unexplained "intuitively forced closures" that we witnessed with respect to Pasch's triangle configuration. Kant also believed that our *numerical* knowledge stemmed from related "intuitions of temporal succession", but many mathematicians before Frege found this diagnosis less convincing and believed that some essentially different grounding must stand behind our knowledge of numbers.

Whatever story one develops about our geometrical and arithmetical "sources of mathematical knowledge", one is certain to be puzzled by the unexpected *extension elements* that began appearing in mathematics' traditional dominions. As we'll see, nineteenth century geometry began to tolerate "ideal points at infinity" and "ideal points of intersection" between circles and lines that do not visibly cross (such points were assigned Cartesian coordinates that took complex numbers as values). Strange "ideal numbers", also described below, began to pop up in algebra.

From a modern perspective, it may seem odd that these rather specialized extension elements posed the primary challenge to Kantian methods of rationalizing mathematical practice. Prima facie, one might have expected that the growing importance of *complex analysis* (the use of the calculus over complex numbers) would have provided sufficient prompting to question Kant's limited picture of mathematics' ontological prospects. After all, mathematicians like Cauchy, Gauss and Abel forced ordinary looking functions (say, the rule that reports arc length along a planetary orbit) to take values over the complex numbers, a project that sounds totally nonsensical at first telling. However, the complex numbers themselves can be viewed as simply filling an intuitionally tolerated void that traditional mathematics had overlooked. Such numbers can be pictured as vectors within a two dimensional space (that is, as arrows upon the complex plane). A transitional thinker like Sir William Hamilton could then argue that the study of such "vectors" was as directly grounded in our "intuitions" of the plane as regular real numbers were founded upon the line.[5] Such musings led various algebraists to search for further extensions of number that might answer to our "intuitions" of three dimensional vectors, investigations that eventually produced the somewhat unsatisfactory quaternions.

However, by the time Frege had commenced his foundational studies, a novel set of proposals for rationalizing the admission of the extension elements had emerged. This is the program I will dub *relative logicism.* To understand what I have in mind here, we need to look more closely at the specific character of the "extension elements" at issue. I will begin with the "ideal numbers" of algebra and consider their geometrical cousins later. We shall find that many characteristic elements within Frege's philosophical thinking stem directly from two schools that once flourished under the "relative logicist" banner.

The key impetus to the creation of "ideal numbers" came in 1801 when C.F. Gauss wrote of "complex integers" in his Disquisitiones Ariuthmeticae.[6] In itself, a "complex integer" is nothing new; it is simply a number of the form a + bi where and b are normal integers (= "whole numbers") and i = %-1. One of the most salient facts about the regular integers is that they break *uniquely* into *prime factors* (i.e., 24 can be only expressed as 2x2x2x3) whereas a more general number such as π or 6 - 2i can seemingly be broken into myriad sets of factors. The great advantage of prime factors is that they allow a great deal of control over the integers that is lost within the more general fields of number. But Gauss realized that if we remain within a restricted orbit of complex numbers--those "complex integers"--, then a variety of unique factorization persists, with all the advantages to be gathered therefrom. Unique factorization allowed Gauss to answer certain important questions in number theory quite easily, e.g. how to characterize all integers whose fourth powers give a remainder of n when divided by p. In other words, if we envision the regular integers as enriched with a halo of "complex integers", the commonalities of behavior amongst the regular integers become more transparent. This fact impressed Gauss greatly:

> *It is simply that a true basis for the theory of the biquadradic residues* [i.e., the questions about fourth powers] *is to be found only by making the field of the higher arithmetic, which usually covers only the real whole numbers, include also the imaginary ones, the latter being given full equality of citizenship with the former. As soon as one has perceived the bearing of this principle, the theory appears in an entirely new light, and its results become surprisingly simple*[7].

In the 1840's, treating matters connected to Gauss' work and Fermat's "last theorem", E. E. Kummer realized that unique factorization was again lost in certain further sets of generalized "integers". Consider the "integers" that arise when %15 is added to the rational numbers. In this range of numbers, 10 breaks into irreducible factors in two distinct ways: as 2.5 *an*d (5 + %15)(5 - %15). *If* we only had further factors to work with, e.g. %5 and %3, unique factorization could be restored because 2 = (%5 + %3)(%5 - %3), 5 = $(\%5)^2$, 5 + %15 = %5(%5 + %3) and 5 - %15 = %5(%5 - %3). In such terms, 10 can be seen as "really" decomposing into (%5)(%5)(%5 + %3)(%5- %3) and the apparent non-unique factorizations of 10 is explained by pairing these four basic elements in different ways. However, we were supposed to be considering the integers that can built up employing %15 as *sole* additional element; we can't simply throw %5 and %3 in with these, as their closure will generate lots of numbers we don't want. Kummer finessed these issues in an intriguing way: all we really need is *some* number to serve as a *highest commonality* between 2 and 5 + %15 (and ditto for the other pairings); we don't need to represent it concretely by "%5 + %3" or anything else. So let the pairing[8] "(2, 5 + %15)" serve as the name of an *ideal number* to supplement our original set of %15 numbers and not worry about its nature any further. He wrote:

> *In order to secure a sound definition of the true (usually ideal) prime factors of complex numbers, it was necessary to use the properties of prime factors of complex numbers which hold in every case and which are entirely independent of the contingency of whether or not actual decomposition takes place; just as in geometry, if it is the question of the common chords of two circles even though the circles do not intersect, one seeks an actual definition of these ideal common chords which shall hold for all positions of the circles. There are several such permanent properties of complex numbers which could be used as definitions of ideal prime factors..., I have chosen one as the simplest and most general...One sees therefore that ideal prime factors disclose the inner nature of complex numbers, make them transparent, as it were, and show their inner crystalline nature.*[9]

We are supposed to picture the situation as follow. We first isolate a group of generalized "integers". The behavior of that group will naturally cry out for supplementary "ideal numbers" to allow the group to consolidate into a fully satisfactory domain. In a famous letter to L. Kronecker, Kummer compares this process to the postulation of unseen elements in chemistry, a particularly apt comparison because in the chemical doctrine of Kummer's time such "elements" (like the quarks of modern science) were never supposed to appear in "naked" form in nature. In the quotation above, Kummer further justifies his practice by appeal to the manner in which geometers like Jean Victor Poncelet had cited so-called "permanence of form" to add supplements to the ontology of standard Euclidean geometry, a matter to which we'll return.

In the case at hand, Kummer notes that there are standard divisability tests that provide a "yes" or "no" answer as to whether two regular integers share a common factor and that these tests continue to give meaningful answers even when applied to non-standard pairs like 2 and 5 + %15. For Kummer, the fact that divisibility tests applicable to the natural numbers can still be run upon the generalized integers is *sufficient reason to allow mathematics to postulate ideal numbers to corrrelate with positive answers to these tests.* Of course, once these supplements are accepted, Kummer needed to provide what Frege called "recognition statements"--tests that can further determine whether (2, 5 + %15) represents the <u>same</u> ideal factor as (3, 3 + %15).

Even Frege's most causal readers will recognize that Kummer's fuzzy justification for extending his number ranges would have seemed like anathema to Frege, both in respect to the sloppy idea that the "permanence" of certain divisability tests provides an adequate *"definition"* of ideal numbers and the conceit that the extensions can be methodologically justified by analogy with physics' postulation of unseen entities. On the other hand, Kummer's results clearly represented a great advance in the theory of numbers and *some* rationale for the practice needed to be found.

In 1871, Richard Dedekind suggested both an improvement and a rationalization of Kummer's approach in a famous supplement to his edition of Dirichlet's lectures on number theory[10]. He asked, "What does Kummer want his "ideal numbers to do?" Answer: to serve as divisors of a certain collection of regular numbers. Why not let the entire set of numbers we want divided to *count as* the missing "ideal number" itself? That is, replace "ideal number" (2, 5 + %15) by the *infinite* set {2, 3, 3 + %15, 5 + %15, 4,... }, a set that does not priviledge any particular *representation* of the factor. Dedekind explained:

> [I]*t has seemed desirable to replace the ideal number of Kummer, which is never defined in its own right,... by a* noun *for something that actually exists...*[11]

Dedekind's sets, which he dubbed "ideals", are distinguished by the fact that they are closed under the property that if elements λ and μ are already in the ideal, then so is αλ + βμ, where α and β are any rational numbers. Dedekind suggested that we reinterpret Kummer's procedure as follows: rather than *adding* "ideal numbers" into a range of numbers ***R,*** we "jump up" from R to a level of sets by considering all the "ideal" *sets that can be manufactured from* R. The original numbers in R become replaced at the set level by their "principle ideal" surrogates, <u>viz</u>., the sets that consist of all multiples of a single R element. The advantage of working within this higher domain of sets is that, unlike in R , unique factorization obtains. This format for interrelating structures, where one domain is built from another through set theoretic processes, is now standard in modern algebra courses.

The basic trick displayed here--manufacturing "new" entities by forming sets of old objects--is, of course, employed by Frege in his construction of the natural numbers, which wind up treated as equivalence classes of concepts whose extensions can be mapped to one another in one-one fashion. As we shall see later, the *rationale* Frege offers for this process is rather different than that suggested by Dedekind. Nonetheless, both men regarded these set theoretic transitions as *sanctioned by logic*. Once we allow the "laws of thought" to construct new structures from previously existent materials, we have provided a resolution to the puzzle of the extension elements that does not drastically upset mathematics' claim to be a priori and grounded in intuitive sources of knowledge. Logic alone can build the extension elements needed to bring a traditional mathematical domain to satisfactory ontological completion. I call this program "*relative logicism*" because it asks logic to construct new elements once a base structure has been set in place.

To be sure, both Frege and Dedkind were also "absolute logicists" with respect to number, but we will set these matters largely to the side here.

To the modern reader, relative logicism's appraisal of "logic"'s capabilities may seem rather startling, as we no longer expect that "logic" can be so "creative" as to build mighty towers of set theoretic structures on top of any base domain. On the contrary, a "first order" understanding of logic tolerates universes containing a solitary object; it does not feel obliged to move onto sets at all. But this is entirely a modern point of view. Nineteenth century logic tutors were usually cavalier about what we call "first order inference" (often presuming that it was somehow covered by traditional syllogistics), but would wax garruously expansive on the alleged stages that "logic" runs through as it presses onward through increasingly lofty levels of abstract construction.

It was common in many of these texts to appeal to an alleged constructive process of "abstraction": one surveys a range of concrete objects and *abstracts* their commonality. Dedekind almost certainly viewed his invocation of set theory as a mathematical precisification of the "abstraction" process described by the logicians.[12] Undoubtedly the idea of replacing Kummer's ideal number (2, 5 + %15) by {2, 3, 3 + %15, 5 + %15, 4,... } occurred to him because the latter set represented *the objects from whose commonality Kummer had extracted his number*. Indeed, the well known Italian geometer Fredrigo Enriques explicitly rationalized Dedekind's procedures in this vein:

> *For it can be admitted that entities connected by such a relation* [of equivalence class type] *possess a certain property in common, giving rise to a concept which is a logical function of the entities in question and which is in this way* defined by abstraction.[13]

In fact, Dedekind pursued this analogy a step further by recommending that, after one has "jumped up" into the required set theoretical realm, the abstractive process should be completed by *replacing* these sets by "freely created" mathematical objects that retain only the properties we really need. In an often quoted letter to H. Weber, Dedekind wrote, referring to his famous articulation of real numbers as sets (= "sections" or "cuts") of rational numbers:

> *You say that the irrational number ought to be nothing other than the section itself, whereas I prefer it to be created as something new (different from the section) which corresponds to the section and produces the section. We have the right to allow ourselves such a power of creation and it is more appropriate to proceed thus, on account of treating all numbers equally*[14].

The idea is that, although the set theoretical filigree binding the real numbers to the rationals is needed to confer concrete properties upon the reals (in contrast to allegedly fuzzy procedures like Kummer's), these moorings can be erased once we have established our right to treat the real numbers as a "free standing" structure.

However, many German logicians--.e.g., C. Sigwart--maintained that logic did *not* build its higher structures through "abstraction" of this sort. On the contrary, such "abstractive processes" can go forward only if logic has available to it a rich supply of well-articulated *concepts* beforehand. Frege, we can presume, was more sympathetic to this school of logic for the complaints that Frege directs towards "abstractive" accounts of the origin of number in the Grundlagen very much resemble criticisms typical of the "anti-abstractionists". Later we'll examine the logical processes that replace abstraction within Frege's philosophy of mathematics. By an odd twist of fate, within the folklore of modern philosophy Frege is often portrayed as the thinker who tried to argue "philosophically" that numbers *had to be identified* with sets of equinumerous concepts because that identification was the only proposal that *abstracts properly from all of number's potential applications*, whereas the more "mathematical" Dedekind sought only to articulate "freely created" objects sufficient "to do a mathematical job". But there is little textual evidence to attribute such motivations to Frege.

Before we look into an alternative version of relative logicism with which Frege bore greater philosophical affinities, there is an important commonality between Dedekind and Frege that is worth noting. By the 1870's, many mathematicians had come to feel that, when an *infinitely iterated process* is under consideration, *only* logic can determine the nature of its closure. Recall the peculiar configuration C_4 that emerges as the limit of shapes C_1, C_2,..., discussed in connection with Dirichlet's principle. Prior mathematicians made serious mathematical mistakes when they assumed that they could "intuit" the properties that C_4 will inherit from its generating C_i-chain when, in point of fact, only a *logical* investigation can settle which traits will persist in the limit (thus, in the soap film case, "possesses a continuous surface" fails to carry over from the C_i to the C_4). Using the logical resources codified in Frege's Begriffsschrift (i.e., relational expressions and both multiple and second-order quantification), the configuration C_4 and all of its properties become perfectly well-defined in terms of the rule that generates the C_i. So *no room remains for any fresh "intuition" (a la Dirichlet's principle) to assign* C_4 *any further attributes*. Accordingly, "relative logicism" must be true of mathematical structures introduced as the limits of infinitely iterated processes; it is but a small step to assume that "intuition" is not needed in Dedekind-style constructions either.

In this vein (and influenced by his research into algebraic numbers and Galois theory), Dedekind observes that, insofar as "intuition" underwrites the truth of Euclid's basic postulates, such "intuitions" do not force space to be continuous, for

> *all constructions that occur in Euclid's Elements, can, so far as I can see, be just as accurately effected* [in an algebraically constructed discontinuous] *space; the discontinuity of this space would not be noticed in Euclid's science, would not be felt at all....All the more beautiful it appears to me that without any notion of measurable quantities and simply a finite number of simple thought-steps man can advance to the creation of the pure continuous number domain; and only by this means in my view is it possible for him to render the notion of continuous space clear and definite.*[15]

In other words, although the constructions permitted by Euclid force us to place points on a straight line at distances %2, 2%2, etc., the totality of all these constructions still leave a line with many gaps in it. Dedekind claims it is entirely our logical faculty that induces us to fill in these gaps, *not* "intuition".

In a revealing passage where Frege compares the merits of his system of logic to weaker systems such as Boole's or, for that matter, Aristotle's, he turns this same theme to positive advantage:

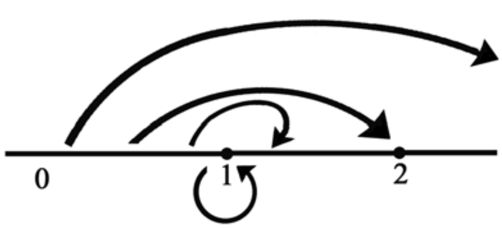
> *If we look at the* [concepts that be defined in a logic like Boole's]*, we*

notice that...the boundary of the concept...is made up of parts of the boundaries of concepts already given...It is the fact that attention is primarily given to this sort of formation of new concepts from old ones...which is surely responsible for the impression one easily gets in logic that for all our to-ing and fro-ing we never really leave the same spot....[But i]*f we compare what we have here with the definitions contained in our examples of the continuity of a function and of a limit and again that of following a series which I gave in §26 of my* Begriffsschrift*, we see that there's no question there of using the boundary lines we already have to form the boundaries of the new ones. Rather totally new boundary lines are drawn by such definitions--and these are the scientifically fruitful ones.*[16]

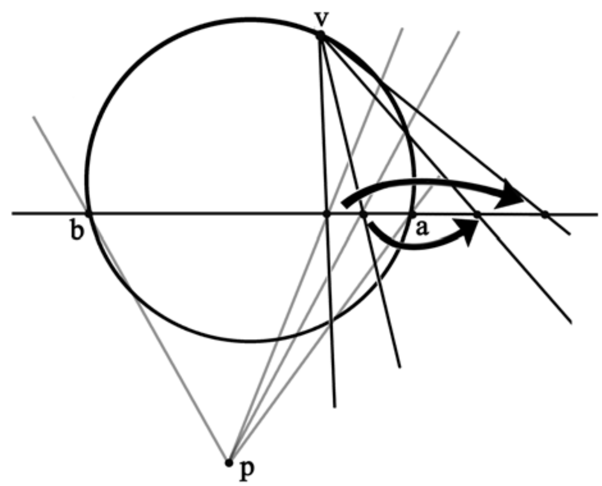
Frege's point is the following. Suppose we start with concepts **A, B** and **C**. From these, traditional formal logic can only build simple compounds like **(A & -B) v C**, which corresponds to a certain region within a standard trio of Euler's circles (a.k.a. "Venn diagrams"). Note that the boundary of **(A & -B) v C** will be comprised of arcs from the circles **A,B,C**. If logic could range no further from home base than that, its powers would truly be as circumscribed as critics like Kant had claimed. But consider the shape C_4 that logic constructs when permitted its full resources. The boundary is "totally new", not coincident with any of the C_i.

Although many of Dedekind's overridding concerns resembled Frege's, the second mathematician seems to have derived much of his idiosyncratic and non-"abstractive" picture of how numbers arise from attitudes that were once current within *geometry*, the subject of his thesis work and much of his teaching. Once again, the motivating problematic revolved around the question of how traditional geometry could tolerate the peculiar "extension elements" that nineteenth century geometers had added to its dominions.

There are two basic strands within geometry that we need to discuss. The first centers on the peculiar way in which the Erlangen geometer Karl von Staudt employed *concepts* as replacements for missing geometrical *objects*. Greek geometers had been intrigued by the phenomenon of *harmonic division*: the pairing of distances upon a line keeps the product of the displacements equal to 1. Consider this division as a function (= mapping) from **x** to **x'** and look at the products that, more generally, supply a constant positive number n. An unexpected recipe (which the unambitious reader needn't follow in detail) for constructing divisions of this sort (whose proper name



is "involution") was discovered: (i) Intersect **L** with a circle **C**. (ii) Draw tangents from **L**'s two intersections with **C**, **a** and **b**, to locate the "polar point" **p**. (iii) Run a "pencil" (= bundle) of lines through **p** back to the circle **C**. (iv) Pair off points **y** and **y'** around the circle if they lie on a common chord from **p**. (v) Locate the "vertex" **v** of the circle (the opposing point whose tangent is parallel to **L**). (vi) Constructing a new pencil of lines through **v**, transfer the **y**/**y'** mapping down onto **L**, resulting in a mapping **x**/**x'** along **L**. This will be the desired "involution" on **L**, which happens to lay down a nesting in mirror image around **b**. The remarkable feature about our roundabout construction is that we have set up an involution without using a ruler to measure anything, despite the fact that our starting definition of "division" was based upon length.



After René Descartes and Pierre Fermat invented analytic geometry in the 1600's, it was found that each step in our recipe could be duplicated by manipulation on formulae. Starting with Cartesian equations for the circle and line, solving for their intersections, then taking derivatives to produce the equations for the tangents, <u>etc</u>., we will eventually stagger our algebraic way to a final equation such as x.x' = n that, indeed, will graph as the involution supplied above. Somebody then noticed a strange fact. What happens if we apply our recipe to equations for lines and circles that *don't* intersect? Well, if we do such a stupid thing, we should expect a stupid answer: we obtain *imaginary coordinates* for the "intersections" of the figures. However, algebraic formulae accept complex numbers as readily as real numbers, so let us calculate onward. At stage (ii), we obtain coordinates for a real pole **p**, but it now lies *inside* **C**. Completing the recipe, we obtain a mapping on **L** of involution type except the results are now *overlapping* rather than *nested*. Indeed, the final equation describing the mapping still takes the form x.x' = n, but n has now shifted to become a *negative number*, suggesting (if n = -1) that our associated points now lie at "harmonically divided" imaginary distances around two imaginary points of intersection! The strange notion that figures that don't appear to intersect nonetheless cross in *imaginary* *points* was originally inspired through algebraic manipulations such as this.

A whole raft of allied cases demonstrated that algebraic methods possess an astounding ability to *unify* under a common treatment situations that, from a strict geometrical point of view, seem unrelated.[17] But how does algebra manage to produce this unity? Certain English mathematicians of the early nineteenth century developed a rather mystical faith that the blind application of algebra would always lead to correct results, even if the paths it pursues seem completely unintelligible. On this point of view, the imaginary points arrive, in the phrase of the mathematician E. Hankel, as "a gift from algebra." Bertrand Russell expressed the obvious objection to this manner of thinking:

> *As well might a postman presume that, because every house in a street is uniquely determined by its number, therefore there must be a house for every imaginable number.*[18]

The great French geometer V. Poncelet, who pioneered the successful exploitation of supplementary entities within geometry, suggested a alternative rationale that did not rely upon any detour through alien branches of mathematics. Poncelet believed that mathematics could extend its natural domains through a primitive "principle of continuity" (or, sometimes, of "persistence of form"). As H.J.S. Smith explained the doctrine in 1851:

> [I]*f we once demonstrate a property for a figure in any one of its general states, and if we then suppose the figure to change its form, subject of course to the conditions with which it was first traced, the property we have proved, though it may become unmeaning, can never become untrue, even if every point and every line, by means of which it was originally proved, should wholely disappear.*[19]

The claim is that, since the properties (iii)-(vi) "persist" as we continuously pull line L outside of the circle, we may postulate "unmeaning" (= without representation in intuition) ideal points to support the continuation of properties observed. Obviously, such a rationale for ontological extension seems even less unconstrained than the "gift from algebra" story; it is as if our postman should posit imaginary houses in vacant lots to domicile the dogs that chase him there. As we saw, Kummer cites this same rationale on behalf of his ideal numbers.

Beginning in the 1840's, the German geometer Karl von Staudt suggested a very unusual program for converting "persistence of form" considerations into a more respectible pattern of extending geometry by straightforward definitions. What is a property that "persists" as the circle and line pull apart? Answer: the property of defining a specific mapping along the line (e.g., "map x to 1/x"). Why not let *this mapping concept*, running in a rightwards sense, become one of our two missing "imaginary intersections" (and let the leftgoing map count as the other)? As Hans Freudenthal[20] rightly remarks, the notion that an *object's* role could be served by a *classificatory relational concept* was unprecedented in mathematics, but, once this unexpected pill is swallowed, von Staudt could rationalize all of Poncelet's maneuvers through a straightforward, if tedious, program of definition. The trick is to amalgamate the new elements into the old world of geometry by redefining old geometrical concepts to suit the new elements. For example, imaginary *lines* are also needed within our extended geometry but they can be introduced in parallel fashion as involution maps upon the *lines* within a "pencil". This supplementation forces a reworking of the old geometrical notion of "lying upon" (call it "lies upon$_1$") so that imaginary points can "lie upon$_2$" imaginary lines. And so forth.

Note that this program employs *concepts* directly as replacements for the entities sought, rather than collecting *sets* in Dedekind's manner. When von Staudt introduced "points at infinity", he thought it sufficient to cite the concept of *direction* as a "commonality" between parallel lines that could serve as their missing "point of intersection". When he desired a line at infinity, he merely appealed to "the something that parallel planes have in common". The reader will note that these are precisely the concepts Frege studies in §§ 64-68 of the Grundlagen as preparation for his approach to number.

The trick in this activity is to circumscribe the proper subset of concepts that can generate the *requisite number* of missing objects. Thus each non-intersecting line and circle pair {L,C} must generate exactly two intersection points, but we also need to determine when two pairs {L,C} and {L',C'} call up the *same* intersections. By choosing the property *set up the same involution*, von Staudt finds a characteristic that settles this second identification question in the right way (hence the involution relation becomes the crucial ingredient in what Frege calls "the recognition statement" for imaginary points). In all of these logical activities, we create nothing: the realm of concepts is already there, with preestablished standards of identity. The "recognition statement" doesn't *define* identity in any sense; it serves merely to foreground, out of the vast sea of candidate concepts, the specific choice that can perfectly play the role of imaginary points. This non-creative approach to the introduction of new entities helps explain the significance of Frege's remark that he is not attempting

> *to define identity specially for this case, but to use the concept of identity, taken as already known, as a means for arriving at that which is to be regarded as being identical.*[21]

We are certain, by the way, that Frege knew von Staudt's work well, because he refers to the latter's definitions several times and because these researches were a prominent topic of discussion during Frege's stay in Gottigen. However, Frege's thinking about mathematical ontology was undoubtedly deepened by a further vein of geometrical considerations that had been promoted by Julius Plücker (who had died a few years before Frege attended his university). Plücker introduced a revolutionary new perspective which allowed previously understood "geometrical contents" to be carved up in novel ways. In so-called "homogeneous coordinates" (see any college geometry text), the equation of a planar straight line looks like $Ax + By + Cz = 0$. Here we naturally think of the list of constants [A,B,C] acting upon the range of variability (x,y,z). In other words, this equation allows [A,B,C] to carve out the *range of points* we call a straight line. But what happens if we hold a point (a,b,c) fixed and allow let the erstwhile [A,B,C] "constants" to vary, i.e., we consider the equation $Xa + Yb + Zc = 0$? Plücker proposed that we view this equation as carving out the full *pencil of lines* through (a,b,c), whose individual components bear "line coordinates" like [A,B,C]. To bring out the symmetries yet further, let us rewrite the claim that "point (a,b,c) lies upon the line [A,B,C]" matrix-multiplication style as "[A,B,C](a,b,c) = 0" Then, according to whether the [] block or the () block is regarded as open to *variation*, we will parse the proposition as representing the action of a different "unsaturated" function acting upon a different saturated "object" (borrowing the terminology Frege introduces in "Concept and Object"). We shouldn't regard [A,B,C] when it acts as a function as quite the same thing as [A,B,C] acting as an object, for otherwise, the thought, in Frege's words, "would fall apart".

Plücker's simple expedient tells us a lot. A curve can be regarded as the intersection of its *tangent lines* just as well as the union of its *points.* Plücker suggested that we examine what happens when the "point equation" of a curve is reexpressed in terms of line coordinates, e.g., when we convert $x^3 - y^2z =0$ into $4X^3 + 27Y^2Z = 0$. If we now "picture" this second "dual" equation *as if* it were about points, the fact that the dual curve is also a cubic turns out to be an indication that the original curve contains funny singularities that we might not have noticed. It is as if we looked at the curve through ultraviolet light and saw features we hadn't noticed previously. Through these means Plücker was able to articulate the eponymous formulae that comprise the beginnings of what is now called "algebraic geometry".

Such work inspired a large number of attempts to reconfigure geometrical intuition by carving space into different choices of basic "elements". The most famous of these subsequent investigations was Sophus Lie's "sphere geometry", but Frege himself worked upon a decomposition where the "elements" were pairs of points treated as a fused unity. For our purposes, note how hidden geometrical facts are uncovered through a transference of "intuition" from one objective content to another. As I reconstruct his thinking, Frege viewed geometry in the following terms: all of the original geometrical facts come to us through Kantian intuition--we *see* how point (a,b,c) lies upon line [A,B,C]; that line L in our Paschian triangle *must* land at C. Nonetheless, by restructuring these contents à la Plücker and adding new logic-based elements constructed in von Staudt's manner, a restructuring can be articulated that will be able to carry a reassigned mantle of "intuition". Under such a transfer, the "objective content" of the original geometrical fact remains constant, while its intuitive appearance shifts to a new geometrical support. That is, "geometrical content" is truly *given to us* in intuition, but the *objective content* so delivered should not to be identified with the *subjective aspect* of the intuitions themselves, which are private to each of us and can be usefully transfered to other objective contents. In § 26 of the Grundlagen, Frege describes two people who have their intuitions assigned to different aspects of geometrical reality:

> *Over all geometrical theorems they would be in complete agreement, only interpreting the words differently in terms of their respective intuitions. With the word "point" for example, one would connect one intuition and the other another. We can therefore still say that this word has for them an objective meaning, providing only that by this meaning we do not understand any of the peculiarities of their respective intuitions*.

It therefore seems likely that Plücker-like considerations on the organization of algebraic equations played some role in shaping Frege's conception of the structuring of logic--why he claimed that his approach differed from Boole's in that

> *instead of putting a judgement together out of an individual as subject and an already previously formed concept as a predicate, we do the opposite and arrive at the concept by splitting up the content of a possible judgement...*[T]*he ideas of these properties and relations are* [not] *formed apart from objects: on the contrary they arise simultaneously with the first judgement in which they are ascribed to things.*[22]

Frege's discussion of how the range of variability under consideration affects the articulation of a judgement |- F(a) into function and concept strikes me as closely analogous to the Plücker understanding of how we determine which factor serves as the active functional element within [A,B,C](a,b,c) = 0 (note also how Frege's assertion sign "|-" compares with Plücker's "= 0").

This background helps clarify the "grammatical approach" to ontology that forms so much of the Grundlagen's fabric: a term corresponds to an object if it can fit in the grammatical locales proper to objects. Specifically, if [A,B,C], qua classifying function, is sufficiently defined so that it can determine for *every* (a,b,c) whether it maps to 0 or not, then the (a,b,c)'s, considered in a dual role as classifiers, must automatically also decide whether the [A,B,C]'s should be sent to 0 or not, so in that sense the [A,B,C] act under the (a,b,c)'s like proper objects. Indeed, such reasoning is still employed in linear algebra to argue that the collection of linear functions over a vector space V constitute a dual vector space V * of their own.

Such thinking, however, leaves the basic elements of plane geometry (points and lines) segregated into dualized domains (P and P *) when intuitively we desire a unified universe (P c P *) where both sorts of object live. To preserve the distinction between function and object, we need a "transfer principle" that can move some objectified surrogate for [A,B,C] into the (a,b,c) realm; call this transferred entity (A,B,C). This transfer allows the articulation of positive statements such as "(a,b,c) is the intersection of (A,B,C) and (D,E,F)", which we can define easily enough. Unfortunately, we are also forced to make sense of unwanted constructions like "[A,B,C](A,B,C)". In the case at hand, we can simply stipulate that all such pairings map to some number (37, say) different from 0. The project of making P c P * behave like a proper domain will require a long sequence of definitions very much like those pioneered by von Staudt.

Such Plücker-like musings amplifies von Staudt's original picture in a natural way. Von Staudt's method of harnessing concepts as objects seems abrupt and unnatural, whereas Plücker offers palliative considerations that allow objects and classifying concepts to interconnect in intimate and interchangeable relationships. In none of this do Dedekind-style sets appear, utilized, for the replacements for ideal points, etc. are discovered by carving up preexistent thoughts in novel ways. Such a program for introducing new entities better accords, probably by coincidence, with the strictures of an "anti-abstractionist" logician like Sigwart.

Many commentators[23] have wondered exactly what *job* in the formal development of the Grundlagen is Frege's famous context principle supposed to serve, given the emphasis Frege assigns to its importance early on. I believe that *if* Frege had consistently pursued a *pure* von Staudt/Plücker strategy throughout, the context principle's philosophical role would not seem mysterious. As a rationale for introducing ideal points, it would serve admirably. However, as has often been noted, Frege's deliberations take an abrupt turn in §68, when suddenly Dedekind-like extensions enter the scene. The background surveyed here suggests the following speculation as to how Frege's thinking developed. Rather early in his career he formulated the general thesis that the real and complex numbers act as logical classifiers of a specific relation R's place within a wider field F of relations generated by certain basic relata G_i. Or, more exactly, he expected these numbers to represent "objectified" *surrogates* for such classifying concepts. In his Begriffsschrift, he successfully articulated the logical tools needed to locate R's place within F. Turning to the natural numbers, Frege expected that they also represent "objectified" surrogates for concepts, but classifiers that merely serve to grade other concepts according to cardinality. Once the requisite classifying concepts had been sharply delineated, Frege originally expected that properly "objectified" surrogates could be constructed by the von Staudt/Plücker strategy. He recognized that the problem of amalgamating dual domains would require a long stream of semi-stipulative definitions[24], but presumed that it would all work out in ideal point fashion. However, when he actually tried to frame these "stipulations" in the course of writing the Grundlagen, he discovered that, at best, the definitions would need to be baroquely tangled in an unexpected manner (unlike the geometrical case, numerical classifiers must be constructed in a manner that allows them to classify structures involving numbers themselves--e.g., "the number of even numbers < 100 is 49"). In the meantime, Dedekind's use of sets in both algebraic number theory and analysis had become well known within the mathematical community. Frege recognized that such classes were widely accepted in logic under the rubric "concepts-in-extension" and, in some desperation, decided to adopt Dedekind's ploy for establishing concept-surrogates within the object domain rather than adhering to the contemplated von Staudt/Plucker strategy. Such a mid-stream switch would explain his puzzling remark in § 68:

> *I believe that for "extension of the concept" we could simply write "concept". But this would be open to the two objections:*
>
> > *1. that this contradicts my earlier statement that individual numbers are objects, as is indicated by the use of the definite article in expressions like "the number two" and by the impossibility of speaking of ones, twos, etc. in the plural, as also by the fact that the number constitutes only an element in the predicate of a*

> *statement of number;*
>
> *2. that concepts can have identical extensions without themselves coinciding.*
>
> *I am, as it happens, convinced that both these objections can be met; but to do this would take us too far afield for present purposes. I assume that it is well known what the extension of a concept is.*

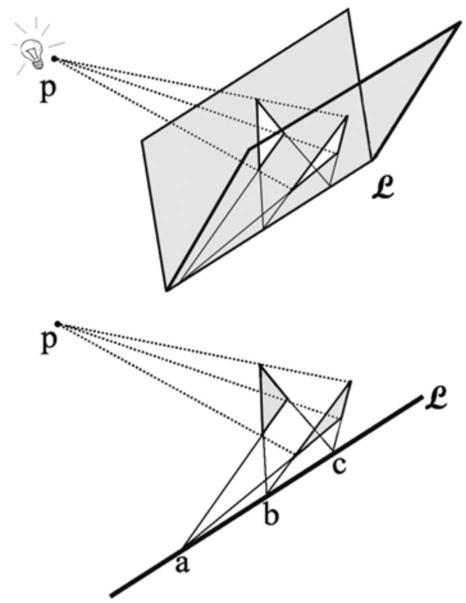
That is, "I believe I could get my project to work utilizing a von Staudt/Plücker strategy, but I will use Dedekind's trick so that I can finish my book!"

If this reconstructed history is correct, it helps explain why the role of the context principle seems so hazy in the Grundlagen and why the slogan is rarely mentioned subsequently. Dedekind's sets, in comparison to Plücker's more delicate vein of thinking, represent fairly blunt instruments and their employment obscures the considerations that connect the context principle to von Staudt-like constructions. Such an explanation would also explain why the notorious Axiom V of the Grundgesetze, which articulates the Dedekind-style transition from the realm of concepts into the realm of objects, emerges so tacitly within the Grundlagen, accompanied by mumblings to the effect "we could do this in other ways if we wish".

In contrast to more radical approaches to the context principle's intended purpose[25], the present suggestion places Frege's approach to ontological issues within the rather *conservative framework* tolerated by the von Staudt school, which labored mightily to keep mathematics' traditional underpinnings more or less in place in the face of the challenge presented by extension elements. It is important to stress, lest the reader be mislead, that the explication offered of the Grundlagen's oddities is idiosyncratic to myself and is based largely upon a dating of when Dedekind-like ideas entered the mathematical mainstream. Such claims can be adequately resolved only as more research is devoted to the mathematical and philosophical community of Frege's time. Although a large amount of rather sophisticated discussion between mathematicians transpired, these events have been left largely uncharted by historians.[26]

However these specific issues sort themselves out, Frege's basic views about mathematical ontology certainly were in general harmony with the "relational logicism" of both von Staudt and Dedekind. But within geometry, at least, von Staudt's approach has long since vanished from the mathematical stage. What happened to it? Its rapid demise can be dated quite precisely to the period of 1900-1910 and is due almost entirely to the rising influence of David Hilbert's contrary views on how issues of mathematical ontology should be addressed. Frege, in fact, engaged in a written exchange with Hilbert and his amanuensis A. Korselt, but these interchanges prove remarkably unrevealing. Hilbert addressed Frege as if he were a naïve fuddy-duddy who needed to be educated in the value of rigor in geometry, failing to recognize that Frege was far more strict in his own investigations than Hilbert had ever been. On his side, Frege endlessly fusses about the proper significance of "definition", etc., complaints that are well taken but which nonetheless seem obtuse in light of the striking results Hilbert found. However, if we look into the details of one of these, we can better appreciate why Hilbert's explications of their significance ran so directly against the grain of relational logicism.

Consider the following observation. First set up two intersecting planes of glass and place a triangle upon the upper plate. Shine a light through the plane to form a shadow triangle on the lower plate. If we extend the sides of the upper triangle indefinitely, they will cast shadow lines that also extend the sides of the lower triangle. As the upper triangle's extended sides stretch to the line of intersection between the two panes of glass, they must be met along that line by the shadow lines that extend the lower triangle's sides. So much should seem obvious. But now *reinterpret* the drawing we have just made as a purely *two dimensional* arrangement lying within the plane of the paper--that is, *erase* all the clues that lead us to interpret the upper diagram as a spatial configuration. A two-dimensional description of our collapsed diagram establishes:



> *If two triangles are placed so that the straight lines connecting corresponding sides meet in a point, then the points of intersection of corresponding sides will lie upon a common line.*

As such, this proposition is called *Desargues' theorem*, after the seventeenth century mathematician who found it roughly through the "projective" reasoning we have just sketched. What intrigued Hilbert about this proof is that it utilizes elementary reasoning about a *three dimensional* configuration in order to establish a *two dimensional* fact. He wondered whether there is any way to obtain the theorem without departing the plane. Accordingly, he isolated the principles needed to effect our "projective" reasoning[27] in a subgroup of axioms and asked: can we derive Desargues' theorem if we only employ the *plane geometry* versions of these axioms? In other words, can the two dimensional inhabitants of E. Abbott's Flatland establish Desargues' theorem for their homeland in a manner that avoids the *lift into three dimensions* that we used? Hilbert discovered that this is possible only if the Flatlanders rely upon a significantly wider array of *concepts* than we did (the easiest purely planar proofs of Desargues' theorem involve considerations of *measurement* that were completely absent in our solid geometry treatment). Hilbert established this "independence" of Desargues' theorem from the planar "projective" axioms by constructing funny models of the axioms in which Desargues' theorem failed.

Underlying this investigation is an assumption that Hilbert's technique of "testing independence by models" allows us to fix what we might call "the intellectual orbit" spanned by a given set of concepts. By such studies, Hilbert hoped to establish how far a given set of ideas could stretch and, if they proved insufficient to fix all of the facts demanded by the *language of geometry*, to search for further axioms that could bring the subject into a satisfactory complete closure. He wrote,

> [W]*e can formulate our main question as follows: What are the necessary and sufficient conditions, independent from each other, to which a system of things has to be subject so that to each property of these things there corresponds a geometrical fact, and conversely--that is, so that there is a complete and simple picture of geometrical reality.*[28]

In public lectures, Hilbert claimed that these discoveries delineated a surprising "fine structure" within our intuition--e.g., that "projective intuition" reaches less far in the plane than in space.[29]

But such a representation of what his results signify would surely raise the hackles of someone working within the relative logicist tradition. According to its appraisal of "logic"'s capabilities, Hilbert's alleged conceptual orbits are far too circumscribed. After all, employing Plücker's and von Staudt's techniques, *logic* allows us to carve a plane into a wide choice of different dimensionalities and to add complex point supplementaries that render a normal Euclidean plane effectively four-dimensional. Any talk of the "fine structure" of intuition should seem pure nonsense; Kantian "intuition" comes to us as indivisible totality. If we want to prove Desargues' theorem in the plane projectively, let *logic* first create new structures that will lift our two dimensional triangle into three dimensions and then transfer intuitive appearances onto this abstract construction. Once this is accomplished, our undivided Kantian intuition tells us, by reproducing the argument sketched above, that the three dimensional version of Desargues' theorem holds for our "lifted" structure. Collapsing everything back into the plane, we obtain a "planar proof" of Desargues' theorem without citing anything except logic and "projective intuition". Indeed, Frege employed such techniques himself[30]; "transferred property" demonstrations from one dimension to another were very common in higher geometry. Clearly, on this way of looking at the issues, Hilbert's discussion of the "independence" of various groups of axioms would appear as incoherent as Frege, without much overt explanation, claims it to be.

What lies at the core of this dispute, of course, is a significant disagreement about the scope of logic. Without clearly recognizing that he has done so, Hilbert has tacitly adopted a position approximating to what we now consider a *first order logic* understanding of logical consequence.[31] Shortly after the Hilbert/Frege exchanges, this fundamental difference in logical appraisal played a significant role in inducing a methodological outcome that Frege must have found outrageous: the rapid abandonment of the entire relative logicist program within geometry. Although this fact is not obvious from The Foundations of Geometry itself, Hilbert shared the naïve structuralist dream that mathematicians should feel free to cook up any internally consistent realm they please, unfettered by tethers to more familiar mathematical territory. Since he expected that the intellectual orbit natural to a specified collection of concepts can be brought into tidy axiomatic closure, he hoped that, through adequate formalization, a set of principles can be articulated that will completely settle *all* propositions pertinent to a specified range of ideas. As long as such axioms can be proved consistent, a mathematician needn't have any qualms that she has articulated a proper arena for mathematical investigation, for the truth-value of all claims in the theory have been settled by the axioms. Hilbert wrote Frege:

> *Of course I must also be able to do as I please in the matter of positing characteristics; for as soon as I have posited an axiom, it will exist and be "true"... If the arbitrarily posited axioms together with all their consequences do not contradict one another, then they are true and the things defined by these axioms exist. For me, this is the criterion of truth and existence.*[32]

If this axiomatized chastening of naïve structuralism can be made viable, then, at one stroke, all of the painful stagewise constructions of a von Staudt can be avoided. If you believe that Euclidean geometry can be better understood if ideal points are added, *directly* specify through axioms the richer structure you seek and then show how Euclidean geometry can be *embedded* within it. *Don't* waste time building up what you seek out as improbable sections of some preestablished domain[33]. Mathematical liberals like Hilbert and Poincare had long worried that overly cautious restrictions on mathematical ontologizing as advocated by a von Staudt or Weierstrass might hamper the "free creativity" required in the genuine mathematician. Hilbert's axiom-based structuralism presented the hope that such worries could be avoided.

It was the almost instantaneous popularity of this "structures can be independently specified through axiomatics" point of view that drove relative logicism into geometrical oblivion. Post-Hilbertian commentators often sarcastically dismissed efforts like von Staudt's as motivated by antiquated, "extra-mathematical" demands upon mathematical existence. Thus the irrepressible E.T. Bell:

> *In proving that geometry could, conceivably, get along without analysis, von Staudt simultaneously demonstrated the utter futility of such a parthnogenetic mode of propagation, should all geometers ever be singular enough to insist upon an exclusive indulgence in unnatural practices.*[34]

The historical irony in all this is that Hilbert, as is now well known, had underestimated the barriers that prevent axiomatic consistency from being established by elementary means. As Kurt Gödel established in his famous second incompleteness theorem, we usually can prove the consistency of a given axiomatic system only if we *assume* the consistency of some yet stronger theory. So once again the problem that sank naïve structuralism returns: how do we ascertain when we're not dreaming of an inconsistent structure?

The collapse of Hilbertian hopes for elementary checks on consistency thus forced questions of mathematical existence ultimately into the unwelcome arms of modern set theory (to those who find sets “unnatural”). Out of sheer necessity, set theory has become our final arbiter of mathematical existence, occasioning the doctrine I dubbed "modern mathematical orthodoxy" at the start of this paper. But how can we *philosophically* defend the claim that the mathematical existence of everything else should rest upon the dictates of this rather abstruse theory? According to the classic defence offered by Kurt Gödel[35] himself, set theory of roughly Zermelo-Fraenkel type should be favored because (i) we possess various "intuitions" about sets that conform to many of the basic ingredients found within that theory and (ii) Zermelo-Fraenkel also offers as our best vehicle for organizing all mathematical activity, just as the postulation of unseen atoms optimally underwrites the behavior witnessed in macroscopic objects. But if such considerations are the best we can offer in defence of set theoretic primacy, consider what a wierd trajectory philosophy of mathematics has followed over the past two centuries. Relative logicism was originally born in the hopes that mathematics could be liberated from the hazy and quasi-empirical appeals to expediency found in a Poncelet or Kummer. As we've seen, sets entered mathematics largely as a tool for restoring traditional certainty to mathematics while accomodating needed extension elements. Yet Gödel's defence of set theory doesn't differ much in its essentials from that offered by a Kummer or Poncelet, except that now we're now expected to possess sundry "intuitions" about very large sets, rather than simply about plain old Euclidean geometry and arithmetic.

Indeed, an odd tension stands at the heart of Gödelian set theoretic absolutism. From the practices of, inter alia, nineteenth century relational logicists like Frege, modern mathematics has acquired a sharp taste for rigor, yet all of that rigor is now directed towards a core structure that we philosophically justify in the smoozy, inductive manner of the physicist. If that's what our standards of mathematical *existence* eventually come down to, why shouldn't we behave equally "inductively" with respect to *derivation*? Physicists, after all, positively revel in their loose employment of what the mathematician Miklin calls "Rabelesian" proof techniques. But *why* should mathematicians behave any differently?

Of course, it would be impossible to cram all of modern mathematics back into the timid confines tolerated by relational logicism, but if one does not sympathize with their attempts to justify new structures according to a coherent diagnosis of mathematics' sources of validity, then one has probably not adequately appreciated the difficulties that still attach to questions of mathematical ontology in our time.

Notes:

1. Some of this material has been expounded in complementary ways in my "Frege: The Royal Road from Geometry", reprinted with a new appendix in William Demopoulos, Frege's Philosophy of Mathematics, Cambridge: Harvard University, 1995. I would like to thank Pen Maddy, Bill Demopoulos, Jamie Tappenden, Tom Ricketts and Michael Friedman for their comments on this essay.

2. "Frege's Epistemology", Philosophical Review 88, 1992. The prevalence of such attitudes is well discussed by Jamie Tappenden, "Extending Knowledge and 'Fruitful Concepts'", Noûs 29, 1995.

3. Philosophers attracted to the notion of "possible worlds" imagine these entities to constitute a range of autonomous universes responsible only to their own internal fabric of laws. But skeptics, for reasons related to the problems that trouble naïve structuralism, doubt that we have any reliable epistemological purchase on such a free standing range of exceedingly complex structures.

4. For a survey of the history, see A. F. Monna, Dirichlet's Principle (Utrecht: Oostheok, Scheltema and Holkema, 1975).

5. "Theory of Conjugate Functions or Algebraic Couples" in Mathematical Papers (Cambridge: Cambridge University Press 1967). In truth, Hamilton's actual approach was more complicated than sketched here. An interesting history, which unfortunately does not investigate the late nineteenth century's research into "screws" and so forth, is D. Crowe, A History of the Vector Calculaus, (New York: Dover, 1994). In the appendix to my "Road" article, I suggested that Frege's early interest in how number systems work was possibly correlated with a search for novel ways in which numbers might capture vector-like decompositions of mechanical movement. Jamie Tappenden's "Geometry and Generality in Frege's Philosophy of Arithmetic", Synthese **102**, 1995, explores other aspects of Frege's interest in framing number in the widest possible manner.

6. K.F. Gauss, Disquisitiones Arithmeticae, A.A. Clark, trans. (New Haven: Yale University Press, 1965).

7. L.W. Reid, The Elements of the Theory of Algebraic Numbers (New York: MacMillan, 1910), p 208.

8. "(n,m)" is standard notation for the greatest common divisor of n and m.

9. D.E. Smith, A Source Book in Mathematics I, New York: Dover, pp. 120-4.

10. In Gesammelte Mathematische Werke, Vol. III, R. Fricke, E. Noether, O. Ore, eds. (Braunscheweig: F. Vieweg and Sohn, 1930). Dedekind's first use of the equivalence class idea emerges, almost in passing, to introduce some modular arithmetics in "Abriss einer Theorie der Höheren Kongruezen in Bezug auf einen Reellen Primzahl-Modulus" in Vol. 1 of the same collection.

11. Theory of Algebraic Numbers, translated by John Stillwell (Cambridge: Cambridge University Press, 1996), p. 94. This 1877 work still provides an excellent introduction to the subject and its motivations. I might add, however, that one should consider carefully whether using sets (or any of the other devices of the "relative logicists") truly represents an *improvement* on Kummer's original suggestion. All of the real mathematics, it might be claimed, comes in the stage of isolating the factors that direct the creation of the ideal numbers. All of Dedekind's set theoretic machinery merely *copies* that idea and surrounds it with an irrelevant superstructure. For a forceful presentation of this viewpoint, as well as a careful charting of the evolution that Dedekind's ideals endured, can be found in Harold Edwards, "The Genesis of Ideal Theory", Archive for the History of the Exact Sciences, **23** (1980). The notion that "set theory does not capture the true motives that prompt mathematical growth" is very much part of the category theorist's unhappiness with set theoretic absolutism.

12. As early as 1854, he announced his interest in He seems to have first used equivalent classes to introduce modular arithmetics in the course of his of 1856, a relatively insignificant application.

13. The Historical Development of Logic, translated by Jerome Rosenthal, New York: Holt, Rinehart and Winston, 1929, p. 132. Burali-Forti uses "direction of" as Frege did

14. “Letter to Weber” in William Ewald, ed., From Kant to Hilbert, vol. 2 (Oxford: Oxford University Press, 1996).

15. Essays on the Theory of Numbers, translated by W.W. Beman (New York: Dover, 1963), p. 38.

16. "Boole's Logical Calculus and the Concept-script" in Posthumous Writings, translated by P. Long and R. White (Chicago: University of Chicago, 1979), p. 34. A similar passage, other aspects of which are discussed in Tappenden, "Extending Concepts", can be found in the Grundlagen, §88.

In the context of Fourier series, A-L. Cauchy had mistakenly assumed that the limit of continuous functions must be continuous, when this property obtains only if the generating functions are "uniformly continuous". This distinction, introduced by Stokes and Weierstrass, hinges on distinctions of quantifier scope. Cauchy's error, which was widely discussed in the 1870's, is probably what Frege has in mind here (although there are analogous examples within the calculus of variations that would have also been familiar to him). In the text, I have stuck with the soap film case to avoid multiplying examples beyond necessity.

17. For an important philsophical examination of this "unity", cf. Ken Manders, "The Euclidean Diagram", forthcoming.

18. An Essay on the Foundations of Geometry (New York: Dover, 1956), p. 44.

19. "On Some of the Methods at Present in Use in Pure Geometry" in Collected Papers, Vol. 1 (New York: Chelsea, 1965), p. 4.

20. "The Impact of von Staudt's Foundations of Geometry" in P. Plaumann and K. Strambach, eds., Geometry--von Staudt's Point of View (Dordrecht: D. Reidel, 1981).

21. Grundlagen, §63. All translations from this work are by J.L. Austin, published as The Foundations of Arithmetic (New York: Harper and Row, 1960).

22. "Boole's Logical Calculus" in Gottlob Frege: Posthumous Writings, Hermes, Kambartel and Kaulbach (Oxford: Basil Blackwood, 1979), p. 17.

23. A good source is Michael Dummett, Frege's Philosophy of Mathematics (Cambridge: Harvard University Press, 1991).

24. It is interesting that Frege displays a continuing attraction for the stipulation strategy in the Grundgesetze.

25. E.g., Crispin Wright's in Frege's Conception of Natural Numbers as Objects (Aberdeen: Aberdeen University Press, 1983).

26. Certainly Frege's unfortunate disposition to lampoon his opponents, without providing a sympathetic airing for their views, has contributed mightily to the modern neglect of his contemporaries. As I tried to indicate in my "Road" article, the views of a Hermann Schubert may sound silly taken if extracted from context, but, in fact, take their roots in very substantial mathematics.

27. Technically, this is a slight misuse of "projective", as Hilbert worked in an affine setting.

28. I am grateful to an unpublished talk by Michael Hallett on Hilbert's lectures on the philosophy of geometry.

29. Given Hilbert's other researches, one wonders if his attention to dimensionality might have been partially inspired by the remarkable ways in which this trait affects behavior in the context of potential theory.

30. "On a Geometrical Representation of Imaginary Forms in the Plane" in Gottlob Frege: Collected Papers on Mathematics, Logic and Philosophy, Brian McGuinness, ed. (Oxford: Basil Blackwood, 1984).

31. The suggestions made here represent my own reconstruction of what might be troubling Frege in his puzzling discussion of "independence" in the second reply to A. Korselt. Frege's primary aim, after all, is to show that the notion of number is completely independent of geometrical intuition because, using the *full resources* of logic, we can get from non-geometrical truths to all of the facts about numbers. Hilbert's notion of "independence" is too weak to underwrite this claim. All the same, Frege's own experience in geometry should have forced some acknowledgement that Hilbert was doing something important for in characterizing the theorems that can be expected to eventuate from specified subgroups of axioms. In this period, geometry continued to make great strides forward through many variations upon the "transferred property" described in conjunction with Plücker's line coordinates. Figures were regularly mapped to other figures, often living in high dimensional spaces, and reliable criteria were needed to decide which geometrical properties could be expected to hold in those exotic realms and how they would "transfer back" to the home domain. Much of the early enthusiasm for symbolic logic grew up among the geometers because they hoped it could aid these concerns; thus Federigo Enriques:

> *The direct comparison of two orders of geometrical properties...*
> *invites us* to translate different forms of intuition into one

another....*Nothing is indeed more fruitful than the increase of our intuitive powers made possible by this principle. It seems as if to the mortal eyes with which we examine a figure under a certain aspect, there were added a thousand spiritual eyes enabling us to contemplate so many different transformations. But in order to use such a principle fruitfully it is necessary for our logical faculties to be exercised in a sure manner... And it is through this exercise that geometricians have nowadays developed* the meaning of what is logical *to a degree that could not be obtained by others*.

(from The Historic Development of Logic, pp. 124-5).

Accordingly, Frege makes an attempt to reconstruct Hilbert's limited conception of logical dependence in terms of models which, as William Demopoulos observes in "Frege, Hilbert, and the Conceptual Structure of Model Theory", History and Philosophy of Logic **15**, 1994, is quite similar to a modern "semantical" explication of first order consequence. But Frege clearly did not view this enterprise as carving out a significant subbranch of logic per se.

32. On the Foundations of Geometry and Formal Theories of Arithmetic, E-H. Kluge, trans. (New Haven: Yale University, 1971), p. 12.

33. This is the policy recommended in O. Veblen and J.W. Young. Projective Geometry (Boston: Ginn, 1910).

34. The Development of Mathematics, p. 349. This Hilbertian heritage has installed the conviction among professional mathematicians that, as Hamlet could tell a hawk from a handsaw, *they* know the difference between mathematics proper and "philosophy" (echoes of this reverberate in our opening quote from Phillip Kitcher). They've forgotten that this self-authenticating view of their own preserves is premised upon a philosophical theory that simply didn't work out. Philosophers, too, often sustain this same dogmaticism--vide the two historical articles in Ernest Nagel's Teleology Revisited (New York: Columbia University Press, 1982), which usefully survey some of the ground visited here, yet only extract naively formalist lessons from the endeavor.

35. "What is Cantor's Continuum Problem?" in P. Benacerraf and H. Putnam (eds.), Readings in the Philosophy of Mathematics, 2nd edition (Cambridge: Cambridge University Press, 1983). To be sure, workers in foundations would be most happy if some "intuitions" might arrive from other parts of mathematics. For a useful survey, cf. Penelope Maddy, "Believing the Axioms I, II", Journal of Symbolic

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