sions of these other categories. Pursuing further a line of thought Sluga initiated has shown that this assumption is indeed unwarranted, and that even Frege's attempted stipulation of coincidence of relational and nonrelational senses of 'reference' in the case of singular terms has not been justified by Frege's own standards. Thus extending Sluga's argument permits better understanding of the philosophical status of Frege's technical concepts in general, and in particular of the two sides of the concept of reference which Bell, following Dummett, has so usefully distinguished.

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# The Significance of Complex Numbers for Frege's Philosophy of Mathematics

## I. Logicism and Platonism

The topic announced by my title may seem perverse, since Frege never developed an account of complex numbers. Even his treatment of the reals is incomplete, and we have only recently begun to get a reasonable understanding of how it works.[1] Presumably for that reason, the secondary literature simply does not discuss how complex numbers might fit into Frege's project.[2] As I will show, we can be quite confident from what little he does say that Frege intended his logicist program to extend to complex numbers. What we do not know is how he might have gone about it. I will try to show that *however* he approached this task, he was bound to fail. This fact has profound implications, not just for his approach to arithmetic but for his whole understanding of mathematics—and indeed, for his understanding of what is required to secure reference to particular *objects* generally.

Frege is famous for his *logicism*. This is a doctrine not about mathematics generally, but only about one part of it: arithmetic, the science that studies *numbers*. Logicism is the thesis that arithmetic can be reduced to purely logical principles, by the application of logical principles alone. But Frege endorsed a very special form of logicism, what Dummett calls *platonistic* logicism. This is the thesis that numbers are purely logical *objects*. To call something a "logical object" in Frege's sense is to say that it is an object whose existence and uniqueness can be proven, and reference to which can be secured, by the application of purely logical principles.[3]

The mere reducibility of arithmetic discourse to logical discourse need not involve the further commitment to the existence of logical *ob-*

jects. The general logicist program might instead be pursued along the lines of *Principia Mathematica*, where arithmetic discourse is analyzed in terms of second- and third-order logical properties and relations. Frege, of course, also appeals to such higher-order properties and relations. But he insists in addition that numerical expressions are singular terms, and that those that occur essentially in true arithmetic statements refer to objects of a special kind. Endorsing the reducibility thesis of logicism notoriously entails shifting the boundary Kant established between the analytic and the synthetic, so as to include arithmetic in the former category. It is less often noticed that endorsing the analysis of numbers as logical objects that is distinctive of the specifically platonistic version of logicism similarly entails shifting the boundary Kant established between general and transcendental logic. For transcendental logic in Kant's sense investigates the relationship our representations have to the objects they represent. Formal logic, Kant thought, must be silent on such aspects of content. Platonistic logicism about numbers maintains on the contrary that, at least for arithmetic discourse, purely formal logic can deliver the whole of content, including reference to objects. In his *Grundlagen der Arithmetik*, Frege is pursuing the same project of transcendental logic that Kant pursues in his first *Kritik*, albeit exclusively for a kind of non-empirical discourse.

It is precisely the platonism that distinguishes Frege's variety of logicism that I will claim cannot be made to work for the case of complex numbers. Usually when questions are raised about Frege's logicism, the focus is on the claim that numbers are *logical* objects. But I will ignore those troubles and focus on the claim that they are logical *objects*. The difficulty is that structural symmetries of the field of complex numbers collide with requirements on singular referentiality that are built deep into Frege's semantics. That collision raises fundamental questions about Frege's conception of objects—and so about commitments that are at least as central as his logicism. After all, Frege eventually gave up his logicist project, in the face of Russell's paradox, while he never gave up either his platonism or the conception of objects that turns out to cause the difficulties to be identified here.

## II. Singular Terms and Complex Numbers

Frege introduces what has been called the "linguistic turn" in analytic philosophy when in the *Grundlagen* he adopts the broadly Kantian strategy of treating the question of whether numbers are objects as just another way of asking whether we are entitled to introduce singular terms to pick them out. Although Frege's avowed topic is a very special class of terms and objects, namely numerical ones, it turns out that this narrow class is particularly well suited to form the basis of a more general investigation of the notions of singular term and object. For one thing, natural numbers are essentially what we use to count, and objects in general are essentially countables. So Frege's account of counting numbers depends on his discussion of the ordinary, nonmathematical, sortal concepts that individuate objects. For another, one evidently cannot hope to understand the semantic relation between singular terms and the objects they pick out simply by invoking causal relations between them (relations of empirical intuition, in Frege's neo-Kantian vocabulary) if the objects in question are, for instance, abstract objects. Since there are no causal (or intuitive) relations in the vicinity, one must think more generally about what it is for a term to pick out a particular object.[4]

Singular terms are essentially expressions that can correctly appear flanking an identity sign.[5] The significance of asserting such an identity is to license intersubstitution of the expressions flanking it, *salva veritate*.[6] If we understood how to use one paradigmatic kind of singular term, those principles would tell us how to extend that understanding to the rest. Frege takes *definite descriptions*, in which "a concept is used to define an object," as his paradigm:

> We speak of "*the* number 1," where the definite article serves to class it as an object.[7]

> The definite article purports to refer to a definite object.[8]

The question of when we are entitled to use an expression as a singular term—as "purporting to refer to a definite object," and in case the claim it occurs in is true, as succeeding in doing so—then reduces to the question of when we are justified in using the definite article.[9] The conditions Frege endorses are straightforward and familiar:

> If, however, we wished to use this concept for defining an object falling under it [by a definite description], it would, of course, be necessary first to show two distinct things:
> 1. that some object falls under the concept;
> 2. that only one object falls under it.
>
> Now since the first of these propositions, not to mention the second, is

false, it follows that the expression "the largest proper fraction" is senseless.[10]

Securing reference to particular objects (being entitled to use singular terms) requires showing *existence* and *uniqueness*. (This requirement is not special to definite descriptions, as Frege's discussion of criteria of identity and the need to settle the truth of recognition judgments shows. It is just that the definite article makes explicit the obligations that are always at least implicitly involved in the use of singular terms.)

In the context of these thoughts, Frege himself explicitly raises the issue of how we can be entitled to use singular terms to pick out complex numbers:

> It is not immaterial to the cogency of our proof whether "$a + bi$" has a sense or is nothing more than printer's ink. It will not get us anywhere simply to require that it have a sense, or to say that it is to have the sense of the sum of $a$ and $bi$, when we have not previously defined what "sum" means in this case and *when we have given no justification for the use of the definite article.*[11]

> Nothing prevents us from using the concept "square root of $-1$"; but we are not entitled to put the definite article in front of it without more ado and take the expression "the square root of $-1$" as having a sense.[12]

What more is required? To show the existence and uniqueness of the referents of such expressions. Usually in discussions of Frege's logicism, questions are raised about what is required to satisfy the *existence* condition. In what follows, I ignore any difficulties there might be on that score and focus instead on the at least equally profound difficulties that arise in this case in connection with the *uniqueness* condition.

> *How are complex numbers to be given to us* then . . . ? If we turn for assistance to intuition, we import something foreign into arithmetic; but if we only define the concept of such a number by giving its characteristics, if we simply require the number to have certain properties, then there is no guarantee that anything falls under the concept and answers to our requirements, and yet it is precisely on this that proofs must be based.[13]

This is our question. The sense of "given to us" is not to begin with an *epistemic* one but a *semantic* one. The question is how we can be entitled to use singular terms to pick out complex numbers—how we can stick our labels on *them*, catch them in our semantic nets so that we can talk and think about them at all, even falsely.



## III. The Argument

Here is my claim: In the case of complex numbers, one cannot satisfy the uniqueness condition for the referents of number terms (and so cannot be entitled to use such terms) because of the existence of a certain kind of symmetry (duality) in the complex plane. Frege's *semantic* requirements on singular term usage collide with basic *mathematical* properties of the complex plane. This can be demonstrated in three increasingly rigorous and general ways.

1. *Rough-and-ready* (quick and dirty): Moving from the reals to the complex numbers requires introducing the imaginary basis $i$. It is introduced by some definition equivalent to: $i$ is *the* square root of $-1$. But one of the main points of introducing complex numbers is to see to it that polynomials have *enough* roots—which requires that *all* real numbers, negative as well as positive, have *two* square roots. In particular, once $i$ has been properly introduced, we discover that $-i$ is *also* a square root of $-1$. So we can ask: *Which* square root of $-1$ is $i$? There is no way at all, based on our use of the real numbers, to pick out one or the other of these complex roots *uniquely*, so as to stick the label "$i$" onto it, and not its conjugate.

Now if we ask a mathematician, "*Which* square root of $-1$ is $i$?" she will say, "It doesn't matter: pick one." And from a *mathematical* point of view this is exactly right. But from the *semantic* point of view we have a right to ask how this trick is done. How is it that I *can* "pick one" if I cannot tell them apart? What must I do in order to be *picking* one—and picking *one*? For we really *cannot* tell them apart—and as the results below show, not just because of some lamentable incapacity of ours. As a medieval philosopher might have said, they are merely *numerically* distinct. Before we proceed, it is worth saying more precisely what the denial that the uniqueness condition on singular reference can be satisfied for complex numbers actually comes to.

2. *More carefully:* The extension of the reals to the complex numbers permits the construction of a particular kind of *automorphism* (indeed, it is an *involution*, a principle of duality—but our argument will not ap-

peal to the cyclic properties that distinguish this special class of automorphisms), that is, a function that:

is 1 − 1 and onto, with domain and range both being the complex numbers;
is a homomorphism with respect to (that is, that respects the structures of) the operations that define the complex plane, namely, addition and multiplication;
has a fixed basis, that is, is an identity mapping on the reals.

Such an automorphism (homomorphism taking the complexes into themselves)—call it a "fixed-basis automorphism"—is:

(i) a *trivial* (identity) mapping for the base domain of the definition (the reals), and
(ii) a *non*trivial mapping for the extended domain (the rest of the complex plane).

The existence of such a fixed-basis automorphism would show that the extended domain cannot be *uniquely* defined in terms of the basis domain—in this case, that the reals (together with the operations of complex addition and multiplication on pairs of them) do not suffice *uniquely* to identify or define particular complex numbers.

Here is such a mapping, taking each complex number into its *complex conjugate:*

$$f(x + yi) = x - yi$$

If $r$ is real, $f(r) = r$; so the basis is fixed.
Clearly the mapping is 1 − 1 and onto.
The complex plane is an algebraic *field*, which can be represented by a set of pairs of real numbers, together with operations of addition and multiplication.
So to show that $f$ is a homomorphism, it must be shown that:

(a) $f[(a+bi) + (c+di)] = f(a+bi) + f(c+di)$ and
(b) $f[(a+bi) * (c+di)] = f(a+bi) * f(c+di)$.

To see (a): By the definition of +,

$$(a+bi) + (c+di) = (a+c) + (b+d)i.$$



So by the definition of $f$,

$$f[(a+bi) + (c+di)] = f[(a+c) + (b+d)i] =$$
$$f(a+bi) = \text{a-bi}, \text{ and } f(c+di) = \text{c-di}.$$
$$(a-bi) + (c-di) = (a+c) + (-b-d)i = (a+c)-(b+d)i.$$

To see (b): By the definition of *,

$$(a+bi) * (c+di) = (ac\text{-}bd) + (ad+bc)i.$$
$$f[(ac-bd) + (ad+bc)i] = (ac-bd)-(ad+bc)i.$$
$$f(a+bi) * f(c+di) = (a-bi) * (c-di) =$$
$$(ac-(-b)(-d)) + (-ad-bc) = (ac-bd)-(ad+bc)i.$$

So $f$ is a fixed basis automorphism with respect to +, *, which extends $\mathfrak{R}$ to C.

3. Using a bit of (well-known) *algebraic power* to establish the same result with greater generality:

Definition: Let E be an algebraic extension of a field F. Two elements, $\alpha, \beta \varepsilon$ E are *conjugate over* F if $\text{irr}(\alpha, F) = \text{irr}(\beta, F)$, that is, if $\alpha, \beta$ are zeros of the same irreducible polynomial over F.
Theorem: The *Conjugate Isomorphism Theorem* says: Let F be a field, and let $\alpha, \beta$ be algebraic over F with $\deg(\alpha, F) = n$. The map $\Psi_{\alpha,\beta}$: $F(\alpha) \rightarrow F(\beta)$ defined by

$$\Psi_{\alpha,\beta}(c_0 + c_1\alpha + \ldots + c_{n-1}\alpha^{n-1}) = c_0 + c_1\beta + \ldots + c_{n-1}\beta^{n-1}$$

for $c_i \varepsilon F$ is an isomorphism of $F(\alpha)$ onto $F(\beta)$ if and only if $\alpha, \beta$ are conjugate over F.
Fact: The *complex conjugates* appealed to in defining the fixed-basis automorphism $f$ in the argument above are *conjugate over* $\mathfrak{R}$ in the sense of the previous definition and theorem. For if $a, b \varepsilon \mathfrak{R}$ and $b \neq 0$, the complex conjugate numbers $a + bi$ and $a - bi$ are both zeros of $x^2 - 2ax + a^2 + b^2$, which is irreducible in $\mathfrak{R}[x]$.

The upshot of these results is that systematically swapping each complex number for its complex conjugate leaves intact all the properties of the real numbers, all the properties of the complex numbers, and all the relations between the two sorts of numbers. It follows that those properties and relations do not provide the resources to describe or otherwise pick out complex numbers uniquely, so as to stick labels on them. So it is *in principle* impossible to satisfy Frege's own criteria for being entitled to use complex-number designators as singular terms—that is, terms that purport to refer to definite objects. Frege is *mathematically* precluded from being entitled *by his own semantic lights* to treat complex numbers as *objects* of any kind, logical or not. Platonistic logicism is false of complex numbers. Indeed, given Frege's strictures on reference to particular objects, *any* and *every* kind of platonism is false about them. (At the end of this chapter I suggest one way those strictures might be relaxed so as to permit a form of platonism in the light of these observations.)

These are the central conclusions I want to draw. The results can be sharpened by considering various responses that might be made on Frege's behalf. But first it is worth being clear about how the problem I am raising differs from other criticisms standardly made of Frege's logicist program.

## IV. Other Problems

Here are some potential problems with Frege's logicism that should *not* be confused with the one identified here. First, the problem does not have to do with whether the logicist's reduction base is really *logical*. This is the objection that arithmetic is not really being given a logical foundation, because one branch of mathematics is just being reduced to another: set theory. (For to perform the reduction in question, logic must be strengthened so as to have expressive power equivalent to a relatively fancy set theory.) One of the main occupations of modern mathematics is proving representation and embedding theorems that relate one branch of mathematics to another. One gains great insights into the structures of various domains this way, but it is quite difficult to pick out a privileged subset of such enterprises that deserve to be called "foundational."

Second, the problem pointed out here does not have to do with the definition of extensions—Frege's "courses of values." All the logical objects of the *Grundgesetze* are courses of values, and various difficulties have been perceived in Frege's way of introducing these objects as correlated with functions. Of course, one feature of Axiom V of the *Grundgesetze* (where courses of values are defined) that has seemed to



some at least a minor blemish is that it leads to the inconsistency of Frege's system—as Russell pointed out. This is indeed a problem, but it has nothing to do with *our* problem. Although it is a somewhat unusual counterfactual, there is a clear sense in which we can say that the issue of how a platonistic logicist might satisfy the uniqueness condition so as to be entitled to introduce singular terms as picking out complex numbers would arise even if Frege's logic *were* consistent.

Again, the method of abstraction by which logical objects are introduced has been objected to on the grounds that it suffers from the "Julius Caesar problem" that Frege himself diagnosed in the *Grundlagen*.[14] As he puts it there, if we introduce *directions* by stipulating that the directions of two lines are identical just in case the lines are parallel, we have failed to specify whether, for instance, Julius Caesar is the direction of any line. The worry considered here does not have this shape, however; the question is not whether the logical objects that are complex numbers can be identified with anything not so specified, but rather in what sense two objects specified as complex numbers can be told apart in the case where they are related as complex conjugates of each other.

Nor is the problem whether or in what sense Frege can be successful in demonstrating the *existence* of complex numbers as logical objects. The issue concerns the uniqueness condition on entitlement to use singular terms, not the existence condition. Indeed, the concern here should be distinguished from two other sorts of objections to Frege's procedure that can be forwarded under the heading of uniqueness. In "What Numbers Could Not Be,"[15] Paul Benacerraf argues that there can be no sufficient reason to identify numbers with one set-theoretic object rather than another—for instance, no reason to identify 0,1,2,3 . . . with, for example:

$$\varnothing, \{\varnothing\}, \{\{\varnothing\}\}, \{\{\{\varnothing\}\}\} \ldots$$

rather than with

$$\varnothing, \{\varnothing\}, \{\varnothing,\{\varnothing\}\}, \{\varnothing,\{\varnothing\},\{\varnothing,\{\varnothing\}\}\} \ldots$$ [16]

This is indeed a uniqueness problem, but it concerns the uniqueness of an identification of the complex numbers with things apparently of *another* kind, logical or purely set-theoretic objects specified in a different vocabulary. Our problem arises within complex-number talk itself.

Finally, the uniqueness problem for complex numbers identified here should be distinguished from the uniqueness problem that arises from the methodology of piecemeal extensions of definitions of number in the *Grundgesetze* (a methodology that Frege elsewhere rails against). Natural numbers, for instance, are initially defined as in the *Grundlagen*. But then rational numbers are defined as ordered pairs of integers. Since the natural numbers *are* (also) rational numbers, this raises a problem: What is the relation between, say, the rational number <2,4> and the natural number 2? Will the true natural number please stand up? This uniqueness problem ramifies when the reals are defined (or would if Frege had finished doing so), since both natural numbers and rational numbers are also real numbers. Frege does not say how he would resolve this problem.

## V. Possible Responses

With the problem of how one might satisfy the uniqueness requirement on the introduction of singular terms for the case of designations of complex numbers identified and distinguished from other problems in the vicinity, we can turn to possible responses on Frege's behalf. In this section we consider four ultimately inadequate responses. In the following section we consider a more promising one.

One response one might entertain is "So much the worse for the complex plane!" Or, to paraphrase Frege when he was confronted with the Russell paradox: "(Complex) arithmetic totters!" That is, we might take ourselves to have identified a hitherto unknown surd at the basis of complex analysis. Even though this branch of mathematics seemed to have been going along swimmingly, it turns out on further reflection, we might conclude, to have been based on a mistake, or at least an oversight. But this would be a ridiculous response. The complex plane is as well studied and well behaved a mathematical object as there is. Even when confronted with the inconsistency of the only logic in terms of which he could see how to understand the natural numbers, Frege never seriously considered that the problem might be with *arithmetic* rather than with his account of it. And if principles of semantic theory collide with well-established mathematical practice, it seems clear that we should look to the former to find the fault. So, confronted with the difficulty we have identified, Frege never *would* have taken this line, and we *should* not take it.

A second response might be exegetical: perhaps Frege did not intend



his logicist thesis to extend to complex numbers. After all, he only ever actually got as far as taking on the reals. Or, to vary the response, even if he was at one time a logicist about complex numbers, perhaps that is something he changed his mind about. Neither of these suggestions can be sustained, however. I have already cited some of Frege's remarks about complex numbers in the 1884 *Grundlagen*. Here is another passage that makes it clear that, at least at that point, Frege intended his logicism to encompass complex numbers:

> What is commonly called the geometrical representation of complex numbers has at least this advantage . . . that in it 1 and *i* do not appear as wholly unconnected and different in kind: the segment taken to represent *i* stands in a regular relation to the segment which represents 1 . . . A complex number, on this interpretation, shows how the segment taken as its representation is reached, starting from a given segment (the unit segment), by means of operations of multiplication, division, and rotation. [For simplicity I neglect incommensurables here.] However, even this account seems to make every theorem whose proof has to be based on the existence of a complex number dependent on geometrical intuition and so synthetic.[17]

Perhaps Frege gave up this view, then? In the second sentence of the introduction to the *Grundgesetze* of 1893, Frege says:

> It will be seen that negative, fractional, irrational, and complex numbers have still been left out of the account, as have addition, multiplication, and so on. Even the propositions concerning [natural] numbers are still not present with the completeness originally planned . . . External circumstances have caused me to reserve this, as well as the treatment of other numbers and of arithmetical operations, for a later installment whose appearance will depend upon the reception accorded this first volume.

A few years after the publication of the second volume of the *Grundgesetze*, Frege writes to Giuseppe Peano:

> Now as far as the arithmetical signs for addition, multiplication, etc. are concerned, I believe we shall have to take the domain of common complex numbers as our basis; for after including these complex numbers we reach the natural end of the domain of numbers.[18]

And as we know, even when, at the end of his life, Frege gave up his logicist program to turn to geometry as the foundation of arithmetic, his plan was to identify first the complex numbers, and the rest only as special cases of these.

Since this exegetical response will not work, one might decide to ignore what Frege *actually* intended, and insist instead that what he *ought to* have maintained is that, appearances to the contrary notwithstanding, complex numbers are not really numbers. That is, they belong on the *intuitive*, rather than the *logical*, side, of Frege's neo-Kantian partition of mathematics into geometry (which calls upon pure intuition for access to its objects), and arithmetic (which depends only on pure logic for access to its objects). After all, as Frege reminds us in the passage about the geometrical interpretation of complex numbers quoted above, multiplication by the imaginary basis $i$ and its complex conjugate $-i$ correspond to counterclockwise and clockwise rotations, respectively. According to this proposed friendly amendment, Frege's Platonist logicism is not threatened by the impossibility of satisfying the uniqueness condition for introducing terms referring to complex numbers. For that result shows only that the boundaries to which that thesis applies must be contracted to exclude the offending case.

There are two difficulties with this response. First, uniquely specifying one of the directions of rotation (so as to get the label "$i$" to stick to *it)* requires more than pure geometrical intuition; it requires actual empirical intuition of the sort exercised in the use of public demonstratives. Second, if it *were* possible to pick one of the directions of rotation out uniquely in pure intuition, Frege is committed to taking the distinction that would thereby be introduced not to be an objective one—and so not one on which a branch of mathematics could be based.

For the first point: That multiplication by $i$ or $-i$ corresponds geometrically to a rotation of $\pi/2$ radians is not conventional. But which *direction* each corresponds to is entirely conventional; if we drew the axes with the positive $y$ axis below the $x$ axis, $i$ would correspond to clockwise instead of counterclockwise rotation. The question then is what is required to specify one of these directions uniquely, so as to be able to set up a definite convention. This problem is the same problem (in a mathematically strong sense, which we can cash out in terms of rotations) as asking, in a world that contains only the two hands Kant talks about in his *Prolegomena*, how we could pick out, say, the *left* one—for that is the one that, when seen from the palm side, requires *clockwise* rotation to move the thumb through the position of the forefinger to the position of the little finger. In a possible world containing only these two hands, we are faced with a symmetry—a duality defined by an involution—exactly parallel to that which we confronted in the case of the complex numbers. In fact it is exactly the *same* symmetry. Manifesting it geometrically does not significantly alter the predicament. If the world in question also contained a properly functioning clock, we could pick out the left hand as the one whose thumb-to-forefinger-to-little-finger rotation went *that* way—the same way *that* clock hand moves. But demonstrative appeal to such a clock takes us outside the hands, and outside geometry.



Inside the hands, we might think to appeal to biology. Because the four bonds of the carbon atom point to the vertices of a tetrahedron, organic molecules can come in left- and right-handed versions: enantiomers. Two molecules alike in all their physical and ordinary chemical properties might differ in that, treating a long chain of carbons as the "wrist," rotation of the terminal carbon that moved from an OH group through an $NH_2$ group to a single H is clockwise in the one and counterclockwise in the other. The sugars in our body are all right-handed in this sense (dextrose, not levose, which is indigestible by our other right-handed components). So we might think to appeal these "internal clockfaces" in the molecules making up the hands—appealing to biology rather than to geometry. But there is nothing biologically impossible about enantiomeric Doppelgänger, and for all Kant or we have said, the hands in question could be such. To pick out the left hand, it would have to be settled how the rotations defined by *their* sugars relate to *our* clocks. And biology won't settle *that*.

Similarly, we cannot break the symmetry of chirality, of handedness, by appeal to physics. The right-hand screw rule is fundamental in electromagnetic theory: If current flows through a wire in the direction pointed to by the thumb, the induced magnetic field spirals around the wire in the direction the fingers curl on a right hand: counterclockwise. But this fact does not give us a nondemonstrative way to specify counterclockwise rotation. For antimatter exhibits complementary chiral behavior. There is nothing physically impossible about antimatter hands, and for all Kant or we have said, the hands in question could be such. To pick out the left hand, it would have to be settled how the rotations defined by *their* charged particles relate to *our* clocks. And physics will not settle that.

So the geometrical interpretation in terms of directions of rotation will not allow us to specify uniquely *which* square root of $-1$ $i$ is to be identified with, because we can only uniquely specify one direction of rotation by comparison with a fixed reference rotation, and geometry does not supply that—indeed, neither do descriptive (= nondemonstrative) biology, chemistry, or physics. This observation puts us in a position to appreciate the second point above. Even if pure geometrical intuition *did* permit us each to indicate, as it were internally, a reference direction of rotation ("By *i* I will mean *that* [demonstrative in pure inner intuition] direction of rotation"), nothing could settle that you and I picked the *same* direction, and so referred to the *same* complex number by our use of *i*. For the symmetry ensures that nothing we could *say* or *prove* would ever distinguish our uses. Frege considers a parallel case in the *Grundlagen:*

> What is objective . . . is what is subject to laws, what can be conceived and judged, what is expressible in words. What is purely intuitable [*das rein Anschauliche*] is not communicable. To make this clear, let us suppose two rational beings such that projective properties and relations are all they can intuit—the lying of three points on a line, of four points on a plane, and so on; and let what the one intuits as a plane appear to the other as a point, and vice versa, so that what for the one is the line joining two points for the other is the line of intersection of two planes, and so on, with the one intuition always dual to the other. In these circumstances they could understand one another quite well and would never realize the difference between their intuitions, since in projective geometry every proposition has its dual counterpart; any disagreements over points of aesthetic appreciation would not be conclusive evidence. Over all geometrical theorems they would be in complete agreement, only: interpreting the words differently in 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, provided only that by this meaning we do not understand any of the peculiarities of their respective intuitions.[19]

Of course, in our case the "peculiarities of their respective intuitions" include just which complex number they indicate by 'i'. So relinquishing logicism for the complex numbers in favor of the geometrical interpretation will not suffice to make a safe place for complex numbers in Frege's philosophy of mathematics.

As a fourth possible response, then, one might suggest that Frege give up his partition of mathematics into arithmetic and geometry: the bits where expression and demonstration can proceed by purely logical means and the bits where pure intuition is also required. In fact, Frege never seems to have considered relinquishing this neo-Kantian demarcation. As already remarked, even when he finally despaired of founding arithmetic on logic, he turned to geometry. But in fact there is no succor available for him through such a move in any case. For the problem lies not in the conception of logic or of geometry, but in the incapacity of his semantic requirements on singular terms to accommodate certain kinds of global symmetries. But structural symmetries of the sort rehearsed in detail for the complex numbers—symmetries that preclude demonstrations of uniqueness of the sort Frege demands to secure reference to objects—are ubiquitous in modern mathematics. Here are two examples chosen almost at random:

(a) The multiplicative group $U_3$ of the three solutions to $x^3 = 1$, namely,

$$\{1, -1/2 + (\sqrt{3}/2)*i, -1/2-(\sqrt{3}/2)*i\}.$$

This is a concrete instance of the abstract group whose table is:

| * | e | a | b |
|---|---|---|---|
| **e** | e | a | b |
| **a** | a | b | e |
| **b** | b | e | a |

This has a permuting automorphism $\Psi$ defined by: $\Psi(e) = e$, $\Psi(a) = b$, $\Psi(b) = a$. Similar results obtain for the abstract groups instantiated by the rest of the $U_n$.

(b) Klein's Viergruppe, V (which has nothing to do with complex numbers), has group table:

| * | e | a | b | c |
|---|---|---|---|---|
| **e** | e | a | b | c |
| **a** | a | e | c | b |
| **b** | b | c | e | a |
| **c** | c | b | a | e |

V has a permuting automorphism $\Psi$ defined by:

$$\Psi(e) = e, \Psi(a) = c, \Psi(b) = b, \Psi(c) = a.$$

I have chosen examples from abstract group theory in part because Frege was certainly familiar with it. The definitive nineteenth-century German work on abstract algebra, Heinrich Weber's *Lehrbuch der Algebra,* was published in two volumes, the first appearing before Frege published the first volume of his *Grundgesetze,* the second well before the publication of Frege's second volume, at a time when Frege was still an active member of a mathematics department. Although Frege seems never to have used the word *'Gruppe',* in the second volume of the *Grundgesetze* he in fact proved an important theorem in group theory—one that would elude more conventional algebraists for more than fifteen years.[20]

## VI. Categorically and Hypothetically Specifiable Objects

So complex numbers are just the tip of the iceberg. Large, important stretches of mathematics exhibit symmetries that preclude the satisfaction of Frege's uniqueness requirement on the introduction of singular terms. Is there any way to relax that requirement while remaining true to his motivations in introducing it? Here is a candidate. Frege's uniqueness requirement can be decomposed into two components, which we might designate *distinguishability* and *isolability.* Elements of a domain are distinguishable in case they are *hypothetically specifiable,* that is, specifiable (uniquely) *relative* to some other elements of the same domain, or *assuming* the others have already been picked out. Elements of a domain are isolable in case they are *categorically specifiable,* that is, can be specified uniquely by the distinctive role they play within the domain, or in terms of their distinctive relation to what is *outside* the domain, to what can be specified *antecedently* to the domain in question. Both of these notions can be defined substitutionally. Here are three examples: Suppose a geometer says, "Consider a scalene triangle. Label its sides 'A,' 'B,' and 'C.'" Now if someone asks, "*Which* side is to be labeled 'A'?" answers are readily available, for instance: "The one that subtends the largest angle." The case would be different if the geometer had said instead, "Consider an equilateral triangle. Label its sides 'A,' 'B,' and 'C.'" Now if someone asks "*Which* side is to be labelled 'A'?" there need be no answers available. In both cases the three sides are *distinguishable.* That is, it has been settled that the three sides are *different* from one another. For if, say, "A" and "B" labeled the *same* line segment, there would be no



triangle to discuss. So "A" could not be substituted for "B" indiscriminately, while preserving truth. And assuming that references have been fixed for "A" and "B," we can say, "'C' is the *other* side of the triangle," even in the equilateral case. But the symmetries involved in the equilateral case preclude our doing there what we can easily do in the scalene case, namely, *isolate* what the labels pick out: *categorically* specify which sides are in question.

Next, consider extending the field of the natural numbers (with addition and multiplication) to the integers. Now consider the mapping on the extension field defined by $f(n) = -n$. We could say that this mapping mapped each integer onto its *sign conjugate* (or complement). Such sign conjugates are clearly *distinguishable* from one another, for we cannot substitute "$-n$" for "n" in the second place of $n * n = n^2$, *salva veritate,* since $n*(-n) = -n^2$. Nonetheless, $f$ is a homomorphism with respect to addition. Are the elements of the extension field nonetheless categorically specifiable? Yes. For $f$ is *not* a homomorphism with respect to multiplication. There is an underlying asymmetry between the positive and negative integers with respect to multiplication: multiplying two positive numbers always results in a positive number, while multiplying their negative conjugates results in the same, positive number. So the positive numbers can be not only *distinguished* from the negatives (as above), but also *categorically specified* as the numbers whose sign is not changed by multiplying them by themselves.

Contrast the *complex conjugates,* which are distinguishable but *not* isolable—hypothetically but not categorically specifiable. The first notion can be defined substitutionally by looking at *local* or *piecemeal* substitutions:

$$a + bi \neq a - bi,$$

since the former cannot be substituted for the latter, *salva veritate,* in:

$$(a + bi)*(a - bi) = a^2 + b^2, \text{ while}$$
$$(a + bi)*(a + bi) = a^2 - b^2 + 2abi.$$

In this sense, the complex conjugates are *distinguishable* from one another. This means each element is *hypothetically specifiable:* specifiable *if* some other elements are.

The second demands the absence of *global* automorphisms (substitutional permutations). And that we have seen is *not* the case for the complex numbers.

Here is a third example. The group V above admits the automorphism $\Psi$. So its elements are not antecedently categorically specifiable (isolable). They are distinguishable, however, for if we substitute c for a in e * a = a, we get e * a = c, which is not true. Thus a and c cannot be *identified* with one another. They are *different* elements. It is just that we cannot in advance of labeling them say which is which, since the automorphism shows that they *play the same global role* in the group.

By contrast: The (nonabelian) Dihedral Group D4 of symmetries of the square consists of the following eight permutations (with the four vertices of the square labeled 1–4), together with the operation * (corresponding to composition) defined by the table below:

$\rho_0 = (1,2,3,4) \rightarrow (1,2,3,4)$ $\mu_1 = (1,2,3,4) \rightarrow (2,1,4,3)$
$\rho_1 = (1,2,3,4) \rightarrow (2,3,4,1)$ $\mu_2 = (1,2,3,4) \rightarrow (4,3,2,1)$
$\rho_2 = (1,2,3,4) \rightarrow (3,4,1,2)$ $\delta_1 = (1,2,3,4) \rightarrow (3,2,1,4)$
$\rho_3 = (1,2,3,4) \rightarrow (4,1,2,3)$ $\delta_2 = (1,2,3,4) \rightarrow (1,4,3,2)$

(So $\rho_i$ are *rotations*, $\mu_i$ are *mirror images*, $\delta_i$ are *diagonal flips*.)

| * | $\rho_0$ | $\rho_1$ | $\rho_2$ | $\rho_3$ | $\mu_1$ | $\mu_2$ | $\delta_1$ | $\delta_2$ |
|---|---|---|---|---|---|---|---|---|
| $\rho_0$ | $\rho_0$ | $\rho_1$ | $\rho_2$ | $\rho_3$ | $\mu_1$ | $\mu_2$ | $\delta_1$ | $\delta_2$ |
| $\rho_1$ | $\rho_1$ | $\rho_2$ | $\rho_3$ | $\rho_0$ | $\delta_1$ | $\delta_2$ | $\mu_2$ | $\mu_1$ |
| $\rho_2$ | $\rho_2$ | $\rho_3$ | $\rho_0$ | $\rho_1$ | $\mu_2$ | $\mu_1$ | $\delta_2$ | $\delta_1$ |
| $\rho_3$ | $\rho_3$ | $\rho_0$ | $\rho_1$ | $\rho_2$ | $\delta_2$ | $\delta_1$ | $\mu_1$ | $\mu_2$ |
| $\mu_1$ | $\mu_1$ | $\delta_2$ | $\mu_2$ | $\delta_1$ | $\rho_0$ | $\rho_2$ | $\rho_3$ | $\rho_1$ |
| $\mu_2$ | $\mu_2$ | $\delta_1$ | $\mu_1$ | $\delta_2$ | $\rho_2$ | $\rho_0$ | $\rho_1$ | $\rho_3$ |
| $\delta_1$ | $\delta_1$ | $\mu_1$ | $\delta_2$ | $\mu_2$ | $\rho_1$ | $\rho_3$ | $\rho_0$ | $\rho_2$ |
| $\delta_2$ | $\delta_2$ | $\mu_2$ | $\delta_1$ | $\mu_1$ | $\rho_3$ | $\rho_1$ | $\rho_2$ | $\rho_0$ |

This group does not have a global automorphism: each element plays a unique role, and so not only is distinguishable from the others but is categorically specifiable (isolable) as well. Yet we want to be entitled to label the elements of the abstract group V, no less than those of $D_4$. We want to be able to say, "Call one of the elements that behaves this way [specification of its role with respect to e and b], 'a' and the other 'c.' It doesn't matter which is which."

Frege in fact recognizes this distinction. He appeals to it in distinguishing arithmetic from geometry:



> One geometrical point, considered by itself, cannot be distinguished in any way from any other; the same applies to lines and planes. Only when several points, or lines, or planes, are included together in a single intuition, do we distinguish them . . . But with numbers it is different; each number has its own peculiarities.[21]

That is, the natural numbers are *antecedently categorically specifiable (isolable)*, while geometrical objects are not (though they must still be distinguishable).

Here, then, is a suggestion. We could relax Frege's uniqueness requirement on entitlement to introduce singular terms by insisting on *distinguishability* but not on *isolability*—requiring the *hypothetical* specifiability of referents but not their *categorical* specifiability. The rationale would be that this seems in fact to be what we insist on in the case of mathematical structures that exhibit the sorts of symmetry we have considered. In the context of the *Grundlagen* project where it is introduced, uniqueness mattered originally because it was necessary for countability—where once existence has been settled, the issue of one or two or more is of the essence. But *distinguishability,* by *local* substitutions that do *not* preserve truth, is sufficient for countability. For this purpose we do not *also* have to insist, as Frege does, on *categorical specifiability,* which requires the absence of certain kinds of *global* truth-preserving substitutions or permutations. Since the latter requirement would oblige us to condemn vast stretches of otherwise unimpeachable mathematical language as unintelligible or ill formed, it seems prudent to refrain from insisting on it.

There are two ways in which such a relaxation of half of Frege's uniqueness condition might be understood—confrontational or accommodating. One would construe the move as reflecting disagreement about the proper characterization of a common category of expressions: singular terms. The other would take the suggestion as recommending recognition of a second, related category of expressions: (say) schmingular terms. According to the first sort of line, Frege was just wrong in thinking that categorical specifiability is a necessary condition for introducing well-behaved singular terms. According to the second, he was quite right about one kind of singular term, what we might call "specifying" terms, and wrong only in not acknowledging the existence of another kind, what we might call "merely distinguishing" terms.

The accommodating reading is surely more attractive. The confrontational stance seems to require commitment to a substantive and (so) potentially controversial *semantic axiom of choice* that stipulates that one can label arbitrary distinguishable objects.[22] One would then naturally want to inquire into the warrant for such a postulate. Going down this road seems needlessly to multiply the possibilities for metaphysical puzzlement. Frege's practice in the *Grundlagen* would seem to show that what matters for him is that we understand the proper use of the expressions we introduce: what commitments their use entails, and how we can become entitled to those commitments. We can be entitled to use merely distinguishing terms, for instance, the labels on the sides of a hypothetical equilateral triangle, provided we are careful never to make any inferences that depend on the categorical specifiability of what is labeled—that is, that our use of the labels respects the global homomorphisms that precluded such specifiability. This is a substantive obligation that goes beyond those involved in the use of (categorically) specifying terms, so it makes sense to distinguish the two categories of singular terms. But there is nothing mysterious about the rules governing either sort. If Frege thought there was something conceptually or semantically incoherent about merely distinguishing terms, then he was wrong—as the serviceability and indispensability of the language of complex analysis (not to mention abstract algebra) shows.

## VII. Conclusion

So here are some of the conclusions I think we can draw to articulate the significance of complex numbers for Frege's philosophy of mathematics. First, structural symmetries of the field of complex numbers entail that Frege's *Platonistic* or *objectivist* version of logicism cannot be made to work in his own terms for this area because of a collision with requirements on singular referentiality built deeply into his semantics. Second, as a consequence, Frege's partition of mathematics into:

(a) the study of *logical* objects, and
(b) the study of the deliverances of pure (geometrical) *intuition*

cannot be sustained in his terms. For once we have seen how things are with the complex plane, it becomes obvious that vast stretches of modern mathematics, including most of abstract algebra, will not fit



into Frege's botanization. For the sorts of global symmetries they share with the complex plane preclude Frege from allowing them in the first category, and they are not plausibly assimilated to the second. More constructively, however, I have suggested that we can make sense of reference to mathematical objects in the face of such symmetries if we are willing to relax Frege's requirements on entitlement to use singular terms, by insisting on *distinguishability* (hypothetical specifiability), but not on *categorical specifiability*.[23] Thus, looking hard at how complex numbers fit into Frege's theorizing in the philosophy of mathematics promises to teach us important lessons about the semantics of singular terms. This suggests a final general lesson: the philosophy of mathematics must pay attention to the details of the actual structures it addresses. Semanticists, metaphysicians, and ontologists interested in mathematics cannot safely confine themselves, as so many have done, to looking only at the natural numbers.