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The Review of Symbolic Logic
Volume 4, Number 4, December 2011

DEDEKIND AND HILBERT ON THE FOUNDATIONS OF THE DEDUCTIVE SCIENCES

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Abstract. We offer an interpretation of the words and works of Richard Dedekind and the David Hilbert of around 1900 on which they are held to entertain diverging views on the structure of a deductive science. Firstly, it is argued that Dedekind sees the beginnings of a science in concepts, whereas Hilbert sees such beginnings in axioms. Secondly, it is argued that for Dedekind, the primitive terms of a science are substantive terms whose sense is to be conveyed by elucidation, whereas Hilbert dismisses elucidation and consequently treats the primitives as schematic.

si on n'assure le fondement on ne peut assurer l'édifice

De l'art de persuader
Blaise Pascal

§1. Introduction. One distinguishes the concepts of a science from its judgments. New concepts are obtained from already established concepts through definition; new judgments are obtained from already established judgments through demonstration. There needs, however, to be a foundation for the construction of concepts and a foundation for the construction of judgments. I will be concerned here with how Richard Dedekind and the David Hilbert of around 1900 viewed these foundations. Firstly it will be argued that Dedekind operates with a conception on which the foundation of the judgments of a science—that is, the foundation of the science seen as an ordered totality of judgments—lies in certain concepts and their description. Hilbert, on the other hand, it will be argued, sees the foundation of a science in certain basic judgments, what he calls the axioms of the science. I will not argue for the stronger claim that Dedekind possessed an explicit and reflective view of sciences as grounded in concepts, but only that one finds this view operative in his words and works; this contrasts with Hilbert, who was quite explicit that sciences ground in axioms. Secondly it will be argued that while both Dedekind and Hilbert operate with primitive terms of whatever science is in question, they treat such terms very differently: for Dedekind they are terms with a substantive sense fixed by description; for Hilbert, on the other hand, the primitive terms are variables, schematic terms with a merely formal sense that allow for a variety of materializations, a variety of ways of being filled with material content. In this case as well, Dedekind is not explicit on the matter, whereas Hilbert is.

Received: April 18, 2011.

doi:10.1017/S1755020311000232

As has been emphasized in the literature[1] on Dedekind and Hilbert, there are several points of agreement in the methodology of these two mathematicians. It seems to me, however, that the literature has gone too far in seeking similarities only. Upon reading the two in tandem, this reader, at least, feels that significant differences can be traced. The following is an attempt to transform this mere feeling into interpretative theses. As just outlined, I have done that by locating presuppositions regarding the structure of sciences with which I find the two authors operating. These presuppositions show themselves more in how the mathematics is presented than in the kind of mathematics the two pursue.[2] Presuppositions influencing the latter would rather be methodological principles such as that of arithmetization,[3] of freedom in concept formation,[4] or that expressed in the "decision for the inner against the outer."[5]

Above I spoke of the description of a concept; this term is meant to comprehend both nominal definition and what I will call elucidation. In a nominal definition—or, more simply, in a definition—one introduces a new term as the abbreviation of a combination of terms already understood.[6] As stated in the opening paragraph above, not every concept belonging to a particular science can be described by definition in that science—namely, its fundamental concepts will either have to be taken from another science in which they receive their definition, or else they will be fundamental in a more absolute sense, namely in the hierarchy of concepts;[7] in the latter case the concept is described by what I shall call—following Frege—elucidation (cf. Section 6.2 below). An axiom is an immediately evident general judgment: it is a general judgment which can be known from a grasp of the meanings of its terms alone. I take this to be in line with a traditional conception of the notion of an axiom, arguably going back to Aristotle's *Posterior Analytics*.[8] That this traditional notion is not chimerical, a philosopher's dream without root in the ink of modern mathematical literature, is witnessed by Constructive Type Theory (cf. Martin-Löf, 1984), in which the various rules laid down are made evident on the basis of meaning explanations; likewise, the project of Boolos (1971) and Shoenfield (1977), as I see it, is to make various axioms of set theory evident on the basis of meaning explanations of the notion of set.

The notions of definition and axiom which I assume here form part of a general conception of science, codified by Scholz (1930) as the ancient axiomatic theory, and by Betti &

[1] In particular by Sieg (1990) and Ferreirós (2009).

[2] Indeed, Hilbert (1897) deals with many of the same topics as Dedekind (1894).

[3] Arithmetization is in fact the name of several quite different methodological principles; cf. Petri & Schappacher (2007).

[4] A classical statement is Cantor (1883, §8).

[5] Cf. Dedekind (1932a, pp. 54–55): "In diesem letzten Worten liegt, wenn sie im allgemeinsten Sinne genommen werden, der Ausspruch eines großen wissenschaftlichen Gedankens, die Entscheidung für das Innerliche im Gegensatz zu dem Äußerlichen." The words in question are from Gauss (1966, article 76): "But in our opinion truths of this kind should be drawn from the ideas involved rather than from notations."

[6] As Tappenden (2008a) makes clear, there is definition and definition: some *definientia* are more "joint-carving" (cf. Tappenden, 2008b) than others—Tappenden discusses the Legendre symbol; and some *definienda* are especially felicitous with respect to already established terminology—Tappenden discusses 'prime number'.

[7] No assumptions are made here to the effect that this order and connection of ideas corresponds to an order and connection of things. For current ends one might as well hold that a concept's being fundamental is always relative to a given system of knowledge.

[8] Cf. Scholz (1930) and Oeing-Hanhoff (1971).

de Jong (2010) as the classical model of science.[9] Betti & de Jong suggest the use of that model as a tool for historical–philosophical research, and it might be helpful already at the outset to see how, according to the following discussion, Dedekind and Hilbert fit into its grid. Dedekind's methodology seems to be the more traditional of the two due to his employment of elucidation of the primitive terms; what makes Dedekind's presupposed methodology slightly idiosyncratic is the fact that definitions take the role traditionally conceded to axioms, namely as that from which theorems are derived.[10] With his emphasis on what he calls axioms, Hilbert's methodology would apparently seem to agree with the tradition; but with Hilbert's dismissal of elucidation and the consequent treatment of the primitives as schematic, the bond with the tradition would seem to have been cut. It does no longer make sense to speak of the evidence[11] of the axioms, and the science in question can no longer be said to be concerned with some homogeneous domain of being, but its objects are what Hilbert at times calls thought objects (*Gedankendinge*), what I will call mere entities.[12]

§2. The primacy of definition. As far as I know, Dedekind never refers to a hierarchy of concepts upon which his mathematics is based. The systematic character of his work suggests, however, that he at some level operated with such a hierarchy. Given Dedekind's view that mathematical theories begin in concepts (see the following pages), one can accordingly distinguish between theories based on fundamental concepts and theories based on defined, or derived, concepts. If anything is a theory of the first kind in Dedekind's work it must be the theory (if one may so call it) of sets and mappings developed in *Was sind und was sollen die Zahlen?* (Dedekind, 1888). The rest of his theories—arithmetic, the theory of real numbers, of ideals, and so on—seem to be based in concepts derived from the fundamental concepts of that booklet. In fact this may be only partly true, for Dedekind seems to have operated with several notions of function, only one of which is fundamental in *Was sind*.[13] The details of this does, however, not matter for current ends, and they do not detract from the impression that Dedekind at some level operated with a hierarchy of concepts. In the following it will be assumed

[9] On the latter model, axioms are not required to be self-evident.

[10] This point has already been made by Ferreirós (1996, 1999).

[11] Throughout, 'evidence' is used in the sense of "evidentness," that is clearness or vividness, so that the correlation holds: judgment J is evident—J has evidence. This use of 'evidence' is not in line with its use in current epistemology, where evidence is generally taken to be that which justifies belief, or gives reason to believe (cf. Kelly, 2008); but it is in line with the first definition given of 'evidence' in the *OED* and also with the German *Evidenz* (although this word also has other uses). The latter justifies its use in the current setting. See Sundholm (2009, footnote 48) for the distinction drawn here, and Halbfass (1971) for a very helpful brief history of evidence in the current sense.

[12] On thought objects, see Hilbert (1905), as well as the discussion of Hallett (1994, p. 167) and the citations given there. In connection with his idea of a *Mannigfaltigkeitslehre*, Husserl sometimes speaks of *Denkobjekte*, objects which are determined only as to their form; see §70 of the *Prolegomena* (Husserl, 1900), where '*Denkobjekt*' is said to be a favorite term of the mathematician, as well as §§28–35 in *Formal and Transcendental Logic* (Husserl, 1929).

[13] Thus in addition to *Abbildung*, there is also *Operation*, as well as *Funktion*. Göran Sundholm has on various occasions (though not yet in print) distinguished three notions of function: analytic expressions/dependent objects of lowest type (Euler), mappings/independent objects of higher type (Riemann, Dedekind), graphs/independent objects of lowest type (e.g., Hausdorff). I hope to discuss Dedekind's different notions of function on some other occasion.

that this impression accords with the truth. Now given this assumption, it will become clear that most of Dedekind's mathematical theories begin not in fundamental concepts and their elucidation, but rather in derived concepts and their definition. In the following I will therefore mostly speak of Dedekind's view that sciences begin in definitions, that is, nominal definitions. It is in fact an interesting question—which we will not pursue here—whether Dedekind would view something as a science at all which was based in fundamental, thus undefined, concepts. This question is obviously closely related to the question of Dedekind's view of logic.[14] But again, that is not a question which will be pursued here; rather we will now argue on the basis of textual and systematic considerations that Dedekind took the basis of a science to lie in concepts, and in this section these are always derived concepts (primitive concepts will be discussed in Section 6 below). Unless otherwise noted, all translations in what follows are mine. The original will in most cases be reproduced in a footnote.

2.1. Textual support. That Dedekind took the basis of a science to be concepts and their definitions can be seen from a large range of passages. In *Stetigkeit und irrationale Zahlen* (Dedekind, 1872), Dedekind says that he sought "a precise characteristic of continuity that can serve as the basis for actual deductions";[15] this precise characteristic is given in the definition of cuts. In what was intended as an official reply to Keferstein's criticism (Keferstein, 1890), Dedekind discusses a second definition of the notion of infinite system, and says that "everything that may be derived from the one definition follows at once also from the other."[16] Earlier, in the first draft of *Was sind*, Dedekind speaks of "inferences from the concept of a simply infinite system."[17] In §5 of *Stetigkeit*, establishing ordering properties of the domain of cuts, Dedekind notes that he "suppresses the demonstrations of these theorems, which follow immediately from the definitions of the preceding section."[18] Finally, as demonstration for some of the theorems of *Was sind*—for example, those in articles 4, 5, 7, 9, 10—Dedekind presents nothing apart from a reference to preceding definitions; this will be discussed further in Section 2.4 below.

These passages indicate a view on which demonstrations are based in definitions; together with a view of a science as an ordered totality of demonstrated judgments, this leads to a view of sciences as based in definitions, or in the concepts thereby defined. Thus, in the introduction of an 1878 paper on ideal theory, Dedekind says "my new theory, on the other hand, bases itself exclusively on such concepts as that of a *field*, of *whole number*, of *ideal*";[19] and likewise in a letter to Lipschitz: "My efforts in the theory of numbers are

[14] Cf. Ferreirós (1996, forthcoming) on Dedekind and logic.

[15] Dedekind (1932c, p. 322, *Stetigkeit* §3): "... es kommt darauf an, ein präcises Merkmal der Stetigkeit anzugeben, welches als Basis für wirkliche Deduktionen gebraucht werden kann."

[16] Dedekind (1890b, p. 264): "... dass Alles, was aus der einen Definition abgeleitet werden kann, sofort auch aus der anderen folgt."

[17] Dugac (1976, p. 297): "Folgerungen aus dem Begriff eines unendlichen Systems." See Sieg & Schlimm (2005) for a discussion of this and other drafts of *Was sind*.

[18] Dedekind (1932c, p. 328): "Der Kürze halber, und um den Leser nicht zu ermüden, unterdrücke ich die Beweise dieser Sätze, welche unmittelbar aus den Definitionen des vorhergehenden Paragraphen folgen."

[19] Dedekind (1932a, pp. 202–203): "Meine neuere Theorie dagegen gründet sich ausschließlich auf solche Begriffe, wie die des K ö r p e r s, der g a n z e n Z a h l, des I d e a l s..."

directed towards basing the research not on arbitrary forms of presentation and expressions, but rather on simple fundamental concepts."[20]

A passage from the 1894 *XIth Supplement* (Dedekind, 1894) makes quite vivid this image of sciences, or theories, as based in definition; directly upon having given his definition of an ideal, Dedekind remarks:

> Our task now consists in deriving from this definition [sc. of ideal] all properties of the ideals contained in $\mathfrak{o}$ and all their relations to each other. In this *theory of ideals* all the laws of divisibility of numbers within $\mathfrak{o}$ are completely contained.[21]

Thus Dedekind presents what he calls the theory of ideals as consisting of the properties of, and relations among, ideals derived from the definition of an ideal.[22] Readers of *Was sind* may recognize in Dedekind's description of the theory of ideals his remarks on "the science of numbers, or arithmetic" from article 73 of that work:

> The relations or laws which may be derived solely from the conditions α, β, γ, δ in 71, and which therefore are always the same in all ordered simply infinite systems, no matter how the individual elements happen to be named, constitute the primary object of the *science of numbers*, or of *arithmetic*.[23]

Here Dedekind describes arithmetic as those "relations and laws" that may be derived from the (conditions occurring in the *definiens* of the) definition of a simply infinite system. Thus in both of these in many ways parallel passages Dedekind seems to be presupposing a view on which the beginnings of a science, or a theory, lie in definitions as that from which the theorems of the science are derived.

The only passage I know of which appears to tend in another direction is found in *Über Zerlegungen von Zahlen durch ihre größten gemeinsamen Teiler* (1897), the first of Dedekind's two papers on what he called dual groups, what one today would call lattices;[24] there Dedekind speaks of his "endeavours to trace" the theory of modules "back to the smallest number of basic laws."[25] Scholars have noted that Dedekind's dual group theory is even more "modern" or "abstract" than his algebra. I share this sentiment, but there is no need to appeal to whatever development in Dedekind's conception of mathematics

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[20] Dedekind (1932c, p. 468, letter dated October 6, 1876): "Mein Streben in der Zahlentheorie geht dahin, die Forschung nicht auf zufällige Darstellungsformen oder Ausdrücke sondern auf einfache Grundbegriffe zu stützen."

[21] Dedekind (1932c, pp. 117–18): "Unsere Aufgabe besteht nun darin, aus dieser Erklärung alle Eigenschaften der in $\mathfrak{o}$ enthaltenen Ideale und alle ihre Beziehungen zueinander abzuleiten. In dieser Theorie der Ideale sind jedenfalls die Gesetze der Teilbarkeit der Zahlen innerhalb $\mathfrak{o}$ vollständig enthalten."

[22] Recent philosophical work on Dedekind's ideal theory includes Tappenden (2005) and Avigad (2006).

[23] Dedekind (1932c, p. 360, *Was sind* article 73): "Die Beziehungen oder Gezetze, welche ganz allein aus den Bedingungen α, β, γ, δ in 71 abgeleitet werden und deshalb in allen geordneten einfach unendlichen Systemen immer dieselben sind, wie auch die den einzelnen Elementen zufällig gegebenen Namen lauten mögen, bilden der nächsten Gegenstand der Wissenschaft der Zahlen oder der Arithmetik."

[24] See Schlimm (2011) for a recent philosophical discussion.

[25] Dedekind (1932c, p. 113): "Bei dem Bestreben, diese Theorie [sc. der Moduln] auf die kleinste Anzahl von Grundgesetzen zurückzuführen..."

might underlie this more abstract flavor of the dual group theory to explain the occurrences in Dedekind (1897) of the word 'law'. By going through these occurrences one can see that Dedekind uses 'law' only for certain equations, such as $\alpha + \beta = \beta + \alpha$. Thus, I think Dedekind applied this word here on the model of 'commutative' or 'distributive law' (*kommutatives*, *distributives Gesetz*), epithets that were in frequent use also in the nineteenth century. Dedekind's aim alluded to in the cited passage will therefore have been to base module theory, not as he had done previously, on the internal property of being a set closed under subtraction, but rather on the external operations of module addition and module intersection together with a small number of equations, or "fundamental laws," that these operations must satisfy. 'Law' thus means nothing more than condition, and 'condition' (*Bedingung*) is in fact also the word Dedekind uses for the relevant equations in his definition of dual groups:

> A system $\mathfrak{A}$ of whatever things α, β, γ,... will be called a *dual group* whenever there are two operations $\pm$, which from two things α, β produce two things $\alpha \pm \beta$ likewise contained in $\mathfrak{A}$, and which at the same time satisfy the conditions A.[26]

Thus a dual group is any system of things closed under two operations $+$ and $-$ satisfying the following equations A:

$$\begin{aligned} \alpha + \beta &= \beta + \alpha \\ \alpha - \beta &= \beta - \alpha \\ (\alpha + \beta) + \gamma &= \alpha + (\beta + \gamma) \\ (\alpha - \beta) - \gamma &= \alpha - (\beta - \gamma) \\ \alpha + (\alpha - \beta) &= \alpha \\ \alpha - (\alpha + \beta) &= \alpha \end{aligned}$$

In prose, the operations $+$ and $-$ in a dual group are to satisfy the commutative and the associative laws, as well as the so-called absorption laws. The reader may want to verify that the idempotent laws $\alpha \pm \alpha = \alpha$ follow from absorption.

2.2. Two points of view on structural mathematics. Contrary to what we took to be Dedekind's description of arithmetic in article 73 of *Was sind*—namely as a science based in a certain definition, to wit the definition of a simply infinite system—it is common to view the conditions α)–δ) in article 71 of *Was sind* as axioms, and not as conditions in a definition; indeed when phrased as axioms, they are often called the second-order Peano–Dedekind axioms for arithmetic. Similarly one could say of the conditions A above that they form the axioms of dual group theory, in line with how one today may speak of the axioms of lattice theory. What occasions this disagreement seems to me to be different views on what may loosely be termed structural mathematics. The disagreement should therefore be settled by clarifying these views and finding which one seems most in line with Dedekind's writings. Thus, consider the following well-known

[26] Dedekind (1932b, p. 113): "Ein System $\mathfrak{A}$ von irgendwelchen Dingen α, β, γ... soll eine D u a l g r u p p e heißen, wenn es zwei Operationen $\pm$ gibt, welche aus je zwei Dingen α, β zwei ebenfalls in $\mathfrak{A}$ enthaltene Dinge $\alpha \pm \beta$ erzeugen und zugleich den Bedingungen A genügen."

DEFINITION 2.1 *A group is a set G equipped with a binary operation* $\circ$ *and a distinguished element e, such that the following holds:*

1. for all $x, y, z \in G$, $(x \circ y) \circ z = x \circ (y \circ z)$;
2. for all $x \in G$, $x \circ e = x$;
3. for all $x \in G$ there is an $y \in G$ such that $x \circ y = e$.

According to one point of view, the conditions 1.–3. of this definition are to be seen as axioms in the modern sense; they are schematic, or formal, judgments, and from these one obtains through formal inference the theorems of group theory, which consequently are themselves seen as formal judgments. It is not straightforward how to understand the 'formal' here; it cannot be understood as the 'formal' of 'formal system', for a formal system is an inductively generated set of *objects*, whereas group theory as practiced according to the current, or indeed any, conception does not issue in objects, but rather in theorems, and these are judgments made, judgment noemata in the sense of Husserl (1913, §94). Further discussion of this topic is left for another occasion; for current purposes it suffices to see the similarity between this point of view on group theory and that quite clearly taken by Hilbert in the *Grundlagen der Geometrie* and *Über den Zahlbegriff* (these works will be discussed below): axioms employing schematic letters are set out, and from these one obtains by formal inference schematic theorems. We need to give this point of view a name, so let us choose 'schematic'.

According to the other point of view, Definition 2.1 is on a par, for example, with Definition VII.11 of the *Elements*:

> A prime number is that which is measured by a unit alone.

Certain conditions are laid down such that something of the appropriate type—there a set equipped with a binary operation and a distinguished element, here a number—is a group, respectively a prime number, if and only if it satisfies these conditions. The difference between the two cases lies in the definition of a group's being a definition of a higher-level concept, that is a concept under which fall structures or domains, sets with some structure on them, while the definition of prime number is the definition of a first-level concept under which fall, in this case, numbers. Theorems of group theory on this view have the form: in a group G with binary operation $\circ$ and distinguished element e, the following holds... That is, a theorem is prefaced with variable-binding operators such that 'G', '$\circ$', 'e' do not occur as free but as bound variables, and thus the theorems are not schematic, or formal, but fully substantive. What is characteristic of structural mathematics according to this second view is not its schematic character, but rather the fact that it deals with higher-level notions such as that of a group and other algebraic structures. I will call this the 'higher-level' point of view.[27]

As with many other remarks on Dedekind in this paper, what we have just said is already present in some form in the writings of Ferreirós; in particular, the distinction between the schematic and the higher-level point of view is in essence the one Ferreirós (2009, p. 49) draws between "Peano-style axioms affecting the elements and Dedekind-style conditions affecting sets or subsets." Ferreirós argues that, as long as set theory is assumed in the logical background, there is no essential difference between these two points of view; for when set theory is thus assumed, sets are treated as objects, and so reasoning about sets

[27] The position discussed by Reck & Price (2000, §8) seems closely related to this higher-level point of view.

is no different from reasoning about other objects. As we will see in Section 7.2 below, set theory is indeed applied, at least implicitly, in Hilbert's geometry for the definition of various terms such as 'segment' and 'angle'. That set constructions are thus part of the theory is, however, not to say that the axioms (in Hilbert's sense) of the theory are thought of as themselves defining structured sets. For instance, set theory itself may be pursued in a formal manner, such that the variables are thought of as ranging over mere entities, and the epsilon relation is a merely schematic relation on those objects governed by the axioms. In set theory, of course, sets are treated as objects; but this is not to say that the axioms of set theory need to be thought of as defining structured sets, in today's terminology pairs $(V, \in)$; indeed, in general, this point of view would seem to be excluded on logical grounds, since a structure of the appropriate kind would have to be "class-sized" and hence not a set. In other words, set theory is an example of a mathematical theory in which sets are treated as objects, but which would seem to allow only for the schematic, and not for the higher-level point of view.[28] Thus I will insist on the difference, even in the presence of set theory, between these two points of view of structural mathematics.

It seems to me that the higher-level point of view is operative throughout Dedekind's work. He viewed the beginnings of mathematical theories (arithmetic, ideal theory, dual group theory, etc.) in the definitions of higher-level notions. Thus, the definition of the notion of a field simply lays down in its *definiens* conditions on a set of complex numbers;[29] as does the definition of a module,[30] and an ideal is a special kind of module. And we saw that the theory of ideals is thought in effect to be the totality of judgments derivable from the definition of an ideal. Directly following his definition of dual groups, Dedekind remarks:

> In order to show how varied are the domains to which this concept may be applied, I mention the following examples.[31]

This remark seems to presuppose the higher-level point of view. For the definition of dual groups is said to be the definition of a certain *concept*, namely the concept of a dual group, and under this concept is said to fall *domains*; thus we have the definition of a higher-level concept, higher-level inasmuch as what falls under it are domains; as examples of such domains Dedekind lists, among others, sets equipped with union and intersection, and $\mathbb{R}^n$ equipped with coordinate-wise maximum and minimum. In line with Dedekind's remark on ideal theory, one could add that the theory of dual groups consists of those judgments derivable from the definition of a dual group. What now of the conditions α)–δ)

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[28] Assuming, of course, that the set theory in question is not "fragmentary" in the way of Kripke–Platek set theory—this set theory does have set models—but that it has the pretense of being a universal mathematical theory. Working set theorists might not take the schematic point of view, but rather a point of view in line with that described in the introduction to this paper, that is a contensive (to use a neologism that Curry, 1941, p. 222, convincingly argues is the best translation into English of the German *inhaltlich*—think of intent, extent, content) view on which the axioms are taken to be evident from the meaning of the terms 'set' and 'element of'.

[29] Dedekind (1932c, p. 20): "Ein System A von reellen oder komplexen Zahlen a soll ein K ö r p e r heißen, wenn die Summen, Differenzen, Produkte und Quotienten von je zwei dieser Zahlen a demselben System A angehören."

[30] Dedekind (1932c, p. 60): "Ein System $\mathfrak{a}$ von beliebigen reellen oder komplexen Zahlen soll ein M o d u l heißen, wenn dieselben sich durch Subtraktion reproduzieren, d.h. wenn die Differenzen von je zwei solchen Zahlen demselben System $\mathfrak{a}$ angehören."

[31] Dedekind (1932b, p. 113): "Um zu zeigen, wie verschiedenartig die Gebiete sind, auf welche dieser Begriff angewendet werden kann, erwähne ich folgende Beispiele."

of article 71 in *Was sind*? Given that these are called conditions by Dedekind; given that these conditions occur within an article headed by '*Erklärung*', which throughout *Was sind* indicates definition;[32] given the parallel between this article and Dedekind's remark on the theory of ideals; finally, given that Dedekind everywhere else seems to take the higher-level point of view: then it seems reasoned to maintain that also the conditions $\alpha)$–$\delta)$ are seen by Dedekind as part of the definition of a higher-level concept, namely that of a simply infinite system, in which arithmetic finds its base. If this is correct, then it is somewhat misleading to call these conditions $\alpha)$–$\delta)$ axioms: they are neither axioms in the traditional sense of immediately evident general judgments, nor axioms in the Hilbertian sense of schematic postulates, but rather conditions in the definition of a simply infinite system on a par with the condition of being measured by a unit alone in Euclid's definition of prime number.

2.3. Dedekind's conceptualism. What may be called Dedekind's conceptualism —the view that sciences ground in concepts and their description—can in fact be traced already in his 1854 *Habilitationsrede*. After a prefatory remark, Dedekind there briefly outlines what he takes to be the task of any science, and what the limitations of man imply for the sciences as we find them historically given; such limitations have as their consequence that two theories may compete in describing the phenomena. Dedekind considers the example of mineralogy, in which the theory based on the chemical constitution of mineral bodies competes with that based on the crystallographic, morphological, constitution. The significant point for us is that Dedekind sees the difference between the two theories as originating in different sets of concepts taken to be fundamental: one theory is distinguished from the other through its choice of fundamental concepts. More generally, Dedekind says that each science employs a different characteristic (*Merkmal*, which is here used interchangeably with *Begriff*) as its principal means of classification (*Haupteinteilungsgrund*); further he speaks of such a characteristic as a motive for the design of the system, and which is introduced as a hypothesis into the science.[33] In the ensuing discussion, Dedekind employs another example, namely legal science; the "systematizer" of this field

> constructs certain concepts, e.g., that of legal institution, which enter as definitions in the science, and with the help of which he is able to

[32] Dedekind seems to prefer the term *Erkläurng* to *Definition*, although the latter also occurs in his writing; I will treat the two as on a par, as I see no clear way of distinguishing in Dedekind's writing an *Erklärung* from a *Definition*. As the translation of *Erklärung* in this context I suggest 'declaration'; this accords with the use of these terms in contexts such as 'declaration of independence', or 'of human rights', and seems to harmonize well with the use of 'declaration' in computer science. Hallett & Majer (2004, p. 421) discuss Hilbert's use of these terms in the *Grundlagen der Geometrie*, and suggest other translations. On this topic, it may be added that for Bolzano, an *Erklärung* is a special kind of *Verständigung*, namely one which lists in the appropriate order and manner the representations (*Vorstellungen*) that compose the representation to be defined (cf. Bolzano, 1975, §9); further, that Kant lamented in the *Critique of Pure Reason* (A730/B758) that the German language has but one term, namely *Erklärung*, for all the four latinate terms *Exposition*, *Explikation*, *Deklaration*, and *Definition*.

[33] Dedekind (1932c, p. 429): "Die Einführung eines solchen Begriffs, als eines Motivs für die Gestaltung des Systems, ist gewissermaßen eine Hypothese, welche man an die innere Natur der Wissenschaft stellt."

> state the general truths recognizable from the infinite manifold of the singular.[34]

Thus general truths are enunciated on the basis of definitions; this must mean that definitions are seen as lying at the foundation of the science in question, for a science consists precisely of such general truths. The discussion of legal science ends with the famous remark on the greatest art of the systematizer, that it consists in the constant twisting and turning of definitions. This remark underlines the basic role Dedekind envisioned for definitions in the habilitation lecture: they are organizing principles for bodies of knowledge.[35]

2.4. Logical considerations. One may question the very idea of definitions being the beginning of a theory. How does one obtain a theorem from definitions alone; for what would then be the starting point of its demonstration? Consideration of the first section of *Was sind* suggests one answer to this question: to follow from definition alone means to follow from immediately evident judgments of a logical character together with those very simple rules of inference that allow substituting a *definiens* for the corresponding *definiendum* and vice versa. In line with the systematic organization of *Was sind*, each of its demonstrations makes explicit the earlier articles upon which it depends. Given that the booklet opens with definitions and states no postulates or axioms, the demonstration of the first theorem, indeed those of the first five theorems, make reference only to articles containing definitions. As already noted in Section 2.1, this is in line with what we take to be Dedekind's presupposed view of science, but one should try to spell out how an appeal to definition alone can be sufficient justification for a theorem. Thus consider article 4:

> 4. Theorem. According to 3, $A \subseteq A$.[36]

In article 3 we find the definition of the expression '$A \subseteq S$'.

> 3. Declaration. A system A is said to be **part** of a system S when each element of A is an element of S as well. As this relation between a system A and a system S will continually come to the fore in the following, we will express it briefly by the sign $A \subseteq S$.[37]

Inclusion is indeed defined here, and not introduced through elucidation, for the notions of system and elementhood have been described in article 2. Since '$A \subseteq S$' is therefore a defined expression we may in any demonstration substitute for it its *definiens*, and

[34] Dedekind (1932c, p. 430): "…bildet der Systematiker gewisse Begriffe, z.B. die der Rechtsinstitute, welche als Definitionen in die Wissenschaft eintreten, und mit deren Hilfe er imstande ist, die aus der unendlichen Mannigfaltigkeit des Einzlenen erkennbaren allgemeinen Wahrheiten auszusprechen."

[35] This conceptualism of Dedekind, or Dedekind's definitional method, was emphasized already by Ferreirós (1996, §4.3.3) and Ferreirós (1999, chap. VII.5.3); Ferreirós (2009) seems to have revised his reading of Dedekind at this point, and that in light of criticism launched by Sieg & Schlimm (2005); see the postscript to Ferreirós (2007). One can regard parts of the current paper as developing the view initially defended by Ferreirós.

[36] Dedekind (1932c, p. 345, *Was sind*): "4. Satz. Zufolge 3 ist $A \subseteq A$."

[37] Dedekind (1932c, p. 345, *Was sind*): "3. Erklärung. Ein System A heißt Teil eines Systems S, wenn jedes Element von A auch Element von S ist. Da diese Beziehung zwischen einem System A und einem System S im folgenden immer wieder zur Sprache kommen wird, so wollen wir dieselbe zur Abkürzung durch das Zeichen $A \subseteq S$ ausdrücken."

vice versa, its *definiens* for itself. This is indeed the rule one has to appeal to when writing out the demonstration of article 4 in full:

each element of A is an element of A (immediately evident, "logic")

$A \subseteq A$ (substitution of *definiendum* for *definiens*).

Thus we have a two-line demonstration of article 4, and it is relatively clear in what sense this theorem follows from a definition: it follows from what one today could call a logical truth together with rules for substituting *definiens* and *definiendum*. The reader may check that the same holds for the theorems in articles 5, 7, 9, 10, 13, 18, 19, 20, and 22: they all follow from logic and substitution.

A precise logical analysis of *Was sind* is not my business here, but I do not want to leave the impression that Dedekind's mathematics is as innocent as the foregoing might suggest, constructed, as it were, out of "simple logic" and nominal definition alone; so to counteract such an impression I note that Dedekind's definitions and demonstrations typically make existence assumptions which are not made explicit, and hence neither supplied with justification. Thus the definitions of union and intersection assume the existence of these sets; the definition of the image of a set, $\varphi(A)$, assumes the existence of this set (cf. the axiom of replacement);[38] the definition of the chain of a system takes the intersection over a set of sets, and would therefore seem to assume the existence of the "full" power set; the demonstration in article 159 assumes the existence of the set of functions from one set to another; the latter assumes as well the existence of a countable choice set. Interestingly, all of this contrasts with the case of an infinite set, the existence of which Dedekind does not assume, but seeks to demonstrate in article 66. Dedekind would presumably say that the existence of the kinds of set he merely assumes is evident from the explanation of the notion of set. At this point it seems in any case that Frege's criticism was fair when he noted of *Was sind* in the *Grundgesetze* that "nowhere can one find there an inventory of the logical or other laws that are there taken for granted."[39] Finally, note that we have here another at least partial explanation of why Dedekind could take theorems to follow from definitions alone: when the existence assumptions are built into the definitions, one does not need to appeal to axioms in which these assumptions are asserted.[40]

§3. The primacy of axiom. Dedekind's conception of a science's being based on definitions contrasts with Hilbert's conception, according to which what Hilbert calls axioms are at the base of a science; here and in the following I rely on the context's making it clear whether I mean axiom in Hilbert's or in the traditional sense. A programmatic statement of Hilbert's on the primacy of axiom is given in his 1899 address *Über den Zahlbegriff* (Hilbert, 1900b). As is well-known, this lecture introduces the idea of the axiomatic method, a method for which Hilbert claims preference over what he terms the genetic method; in particular, Hilbert says that "for the ultimate presentation and complete

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[38] The existence of $\varphi(A)$ is not trivial within Dedekind's scheme, since for him it is the notion 'φ is a mapping of A' and not the notion 'φ is a mapping of A into B', which is primitive (cf. Section 6 below); the latter notion is defined in article 36 of *Was sind*.

[39] Frege (1893, p. VIII): "Nirgends ist bei ihm eine Zusammenstellung der von ihm zu Grunde gelegten logischen oder andern Gesetze zu finden…"

[40] Cf. the somewhat different analysis of these matters in Ferreirós (forthcoming).

logical conservation of the content of our knowledge, the axiomatic method deserves preference."[41] The genetic method is genetic in the sense that, in its way the number concept—which here seems to mean the concept of real number—is generated in a stepwise fashion starting from the concept of the number one. Hilbert's language does not decide whether the 'generation' here should be read dynamically or statically: for he speaks of the natural numbers as arising through the process of counting, implying something dynamic, while the rational as well as the real numbers are said to be defined, implying something static. Perhaps a more neutral term would be 'description'. Accordingly one could say that a genetic description of the concept of a real number is one which proceeds by means of successive "subdescriptions" of more and more general number concepts. In contrast to such stepwise description, on the axiomatic method one rather assumes from the outset the existence of a domain of mere entities, and "one then places these elements in relations through certain axioms" (cf. Hilbert, 1900b, p. 181).

For Hilbert the ideal would thus seem to be that the numbers be introduced all at once. Dedekind, in fact, gave expression to a similar ideal with his requirements on the "introduction or creation of new arithmetic elements." One of these requirements was namely that "all real irrational numbers be engendered simultaneously by a common definition, and not successively as roots of equations, as logarithms, etc."[42] Indeed, Dedekind's terminology here is strikingly similar to that of Hilbert: where Dedekind spoke of an *engendrer á la fois, et non successivement*, Hilbert speaks of a *successives Erzeugen*. In spite of these similarities, however, it seems clear that Hilbert does not equate, but rather contrasts, his own axiomatic method with whatever he takes to be Dedekind's method;[43] for Hilbert refers to Dedekind cuts as one way of describing the real numbers genetically, hence he would seem to class Dedekind's method as genetic. Moreover, one might recognize in that part of Hilbert's delimitation of the genetic method that concerns natural numbers—how natural number arithmetic arises through the process of counting—what Dedekind said in *Stetigkeit* §1 on the same matter. By 1899, of course, Dedekind's view of arithmetic was quite different,[44] but on the basis of this resemblance one might suspect that for Hilbert at the time, the paradigmatic "geneticist" was the Dedekind of *Stetigkeit*.[45] Since the genetic aspect of the latter's methodology, namely that the notion of real number is reached through successive subdescriptions, would presumably not have disappeared with Dedekind's new account of arithmetic in *Was sind*—for that merely changed the initial

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41 Hilbert (1900b, p. 181): "Meine Meinung ist diese: Trotz des hohen pädagogischen und heuristischen Wertes der genetischen Methode verdient doch zur endgültigen Darstellung und völligen logischen Sicherung des Inhaltes unserer Erkenntnis die axiomatische Methode den Vorzug."

42 Dedekind (1877, p. 269, footnote): "...on devra exiger que tous les nombres réels irrationels être engendrés à la fois par une commune définition, et non successivement comme racines des équations, comme logarithms, etc." This footnote is discussed in detail by Ferreirós (1999, chap. III.4.1).

43 Thus at this point I am in agreement with Ferreirós (1996), but in disagreement with the later Ferreirós (2009, p. 41) who maintains that Hilbert does not associate the genetic method with Dedekind.

44 Sieg & Schlimm (2005) trace the development.

45 Though it should be noted that Hilbert himself seems to have held a similar view of arithmetic in his 1891 lecture on projective geometry (Hallett & Majer, 2004, p. 22): "Zum Begriff der ganzen Zahl können wir auch *durch reines Denken gelangen*, etwa indem ich die *Gedanken selber* zähle." Here one might be reminded of Kant's description of number as the pure schema of quantity at KrV A142–143/B182.

stage of the genesis—one would think that, in Hilbert's eyes, Dedekind's method remained genetic.

The contrast of the axiomatic method to Dedekind is explicit in Hilbert's 1904 address *Über die Grundlagen der Logik und der Arithmetik* (Hilbert, 1905). Here Dedekind's name occurs within a longer list of approaches to the grounding of arithmetic with each of which Hilbert sees difficulties that he meant the axiomatic method could handle. It must be admitted that Hilbert is not here criticizing Dedekind for making use of the genetic method, but rather for his relying in article 66 of *Was sind* on "the totality *S* of all things that can be the object of my thought."[46] By 1904 Hilbert (as well as Dedekind) had become convinced that this totality of all things is, in Cantor's language, an inconsistent multiplicity.[47] But Hilbert would seem to imply by his criticism that with the axiomatic method one would not have to invoke this totality, hence this method was to be preferred to Dedekind's method of grounding arithmetic. Whence the point I wished to make by mentioning this address: that Hilbert (again) contrasts his own method with Dedekind's.

In accordance with his preference for the axiomatic method, Hilbert (1900b) gives an axiomatic presentation of real number arithmetic. Along the same lines, he gives in the *Grundlagen der Geometrie* (Hilbert, 1899) an axiomatic presentation of Euclidean geometry. But not only of these two basic mathematical disciplines did Hilbert give axiomatic presentations; in a lecture course entitled *Logische Prinzipien des mathematischen Denkens* held at Göttingen in 1905, Hilbert suggested axiomatizations of various branches of physics and even an axiomatization of psychophysics.[48] Indeed, the sixth Hilbert problem (Hilbert, 1900a), asks for the "Mathematical Treatment of the Axioms of Physics," a problem with which Hilbert himself would be occupied in ensuing years.[49] It is only somewhat later, in the 1917 address *Axiomatisches Denken* (Hilbert, 1918), that Hilbert devotes a whole paper to the axiomatic method; in this address a large range of theories, and often ones that do not immediately spring to mind as axiomatic, such as the theory of surfaces, the theory of equations, and the theory of prime numbers, are spoken of as axiomatic. The length to which Hilbert goes in this address in locating axioms for various theories—viewing, for instance, the fundamental theorem of algebra as an axiom for the theory of equations—indicates how strongly he at this point, namely in 1917, was committed to the ideal of axiomatic organization. In light of the foregoing discussion, however, and in light of the fact that the ideal of axiomatic organization was stressed by Hilbert already in his first lecture course on foundational matters, that is, in the 1894 course on the foundations of geometry—the notes for that course ends with a call for the axiomatization of "all other sciences, after the pattern of geometry" (Hallett & Majer, 2004, p. 121)—, it seems correct

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[46] It is worth noting that neither Hilbert (1905) nor Zermelo (1908, p. 266, footnote 2) raise any criticism against Dedekind's invocation of *meine Gedankenwelt*; rather they restrict their criticism to the assumption of a universal set.

[47] Cf. Ferreirós (1999, chap. VIII.8).

[48] The relevant part of these lectures are discussed by Corry (1997). The lectures will be published in volume 2 of the series *David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933*, and will probably shed much light on Hilbert's conception of the axiomatic method.

[49] Thus, for instance, in the papers (Hilbert, 1914, 1924) the physical theories under consideration are given an axiomatic presentation, and the former even contains consistency proofs. Corry (2004) discusses in detail Hilbert's involvement with physics and the role of the axiomatic method therein.

to say that already by 1900, the axiomatic method was of major concern to Hilbert. Indeed, one could say that Hilbert's work on foundational matters prior to the conception of proof theory to a large extent coincides with the investigation of mathematical and physical theories by means of the axiomatic method.

§4. Two case studies. Thus, by looking separately at the words and works of Dedekind and Hilbert, I have argued for the conceptualism of the former and the "axiomatism" of the latter. Two smaller case studies will help to bring out the contrast even more markedly.

4.1. Completeness. In his codification of the classical conception of axiomatic science, Scholz (1930) states two criteria which this conception requires of the *Grundsätze* (in Aristotle's Greek: *axiōmata*, *arkhai*, *prōta*): that they be "immediately evident and therefore indemonstrable," and that they be "sufficient, in the sense that, for the demonstration of the theorems, the rules of logic are the only other things needed" (ibid. p. 29); in short, according to the classical conception, axioms should be immediately evident and complete, that is, sufficient for the construction of the theory in question. Hilbert did not adhere to the classical conception and seems to have operated instead mainly with the following three criteria of axiomhood: that the axioms be consistent (*widerspruchslos*, *verträglich*), that they be independent, and that they be complete.[50]

Following Scholz, one can view Hilbert's criterion of consistency as replacing the Aristotelian criterion of evidence.[51] Of course, from a traditional point of view consistency is a weaker requirement than evidence, as Frege noted;[52] for if the axioms are evident, then they cannot contradict each other—in Husserlian terminology, the discovery of an inconsistency would "explode" the evidence. From Hilbert's point of view, however, given that the primitives are variables (see Section 7.1 below), it would seem not even to make sense to speak of the evidence of the axioms. Whence consistency, in the sense of satisfiability, is a natural substitute. We will not have much more to say about consistency here; rather, this section will focus on the criterion of axiomhood that Hilbert shares with "the tradition," namely completeness. The following section will then discuss the criterion of independence.

Completeness is in fact the central criterion in Hilbert's discussions of the process of axiomatization; that is to say, when Hilbert describes how we first come to organize a body of judgments into an axiomatic science, completeness of the axiom system is the key criterion; the task of showing independence and consistency enters only after the fact of axiomatization. Thus in his 1894 lectures, the notion of an axiom of geometry is introduced as follows:

> Since, however, not not all concepts are derivable through *pure logic*, but rather stem from *experience*, the important question which will be treated in these lectures, is that concerning the *fundamental facts* which suffice

[50] Hilbert also operates with some more minor requirements: that the axioms be few in number (merely finite is presumably not enough here), and that they be simple (*einfach*). Cf. the discussion of Hallett (1994, p. 169 and footnote 24).

[51] Scholz himself was supportive of this substitution of consistency for evidence (cf. Scholz, 1930, pp. 35–37); in fact, he seems to have embraced Hilbert's ideas on logic and methodology more generally (cf. Scholz, 1942).

[52] Frege (1976, p. 63, Letter XV/3, dated December 27, 1899): "Aus der Wahrheit der Axiome folgt, dass sie einander nicht widersprechen."

> for the construction of the *whole* of geometry. These *indemonstrable* facts we have to lay down on beforehand, and we call them *axioms*.[53]

Certain fundamental facts suffice for the construction of the whole of geometry; and, given that evidence is not in question, it is this sufficiency that licences calling these facts fundamental and indemonstrable. Thus here, completeness would seem to be the only criterion of axiomhood brought to bear. In the *Axiomatisches Denken* lecture, held more than 20 years later, Hilbert's view of axioms has crystalized and is presented in one of its opening passages, a passage that arguably employs only such concepts as Hilbert possessed already by 1900:

> If we consider a particular theory more closely, we always see that a few distinguished propositions of the field of knowledge underlie the construction of the framework of concepts, and these propositions then suffice by themselves for the construction, in accordance with logical principles, of the entire framework.
> [...]
> These underlying propositions may from an initial point of view be regarded as the *axioms of the respective field of knowledge*...[54]

We inspect a certain ordered body of theorems and find that a few of these theorems suffice for the construction of the whole body; these theorems may then be viewed as the axioms of the theory—that they are said to be so "from an initial point of view" is perhaps to accommodate the fact that later investigations into independence, or indeed consistency, may force revisions in the set of axioms. In any case, completeness is again seen as the central criterion of axiomhood: if a theorem (or a set of theorems) suffices for the construction of the theory, then it may be regarded as the axiom, and hence as the beginning, of that theory.

If we then look at the preface to *Stetigkeit*, the contrast we have developed in Sections 2 and 3 above becomes apparent. In that preface, Dedekind considers the theorem stating that "every magnitude which grows continually, but not beyond all limits, must approach a limit value." For the purposes of this section let us call this the Monotone Convergence Theorem.[55] A careful investigation, says Dedekind, had convinced him that this theorem "may in some ways be regarded as a sufficient fundament for the infinitesimal analysis" (Dedekind, 1932c, p. 316). On the view that we have just attributed to Hilbert, this fact would by itself licence taking the Monotone Convergence Theorem as an axiom for the calculus—one could construct the calculus on its basis, hence it could be taken as an axiom.

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[53] Hallett & Majer (2004, p. 72): "Da nun nicht alle Begriffe durch *reine Logik* abzuleiten sind, sondern vielmehr aus der *Erfahrung* stammen, so ist die wichtige Frage, die wir in dieser Vorlesung behandeln werden, die nach den *Grundthatsachen*, welche zum Aufbau der *ganzen* Geometrie hinreichen. Diese nicht *beweisbaren* Thatsachen müssen wir von vornherein festsetzen und nennen sie *Axiome*."

[54] Hilbert (1918, p. 406): "Wenn wir eine bestimmte Theorie näher betrachten, so erkennen wir allemal, daß der Konstruktion des Fachwerkes von Begriffen einige wenige ausgezeichnete Sätze des Wissengebietes zugrunde liegen und diese dann allein ausreichen, um aus ihnen nach logischen Prinzipien das ganze Fachwerk aufzubauen. [...]
Diese grundlegenden Sätze können von einem ersten Standpunkte aus als die *Axiome der einzelnen Wissensgebiete* angesehen werden..."

[55] As Dedekind (1932c, p. 332, *Stetigkeit* §7) remarks, it is equivalent to the least upper bound principle.

But instead of taking the Monotone Convergence Theorem thus as an axiom, Dedekind states that he wished to find its origin in a definition of the continuous number line:

> It only remained to discover its proper origin in the elements of arithmetic, and thereby to reach a true definition of the nature of continuity.[56]

It was not enough merely to have a *theorem* over which the calculus might be built; what was required was an arithmetical *definition* of a continuous domain in which that theorem would have its true origin. There may of course be several factors that motivated Dedekind's search for the "proper origin" of the Monotone Convergence Theorem—the ideal of arithmetization certainly played its role—but no matter which other motivations he had, I think we see here a clear manifestation of Dedekind's conceptualism, the view that sciences find their beginning in certain concepts and their description.

4.2. Independence. Independence was a major concern in Hilbert's axiomatic investigations into geometry, both as a requirement on axioms, and in the investigation, for instance, of Desargues's and Pascal's theorems.[57] In the *Grundlagen*, independence is established only for the parallel and the congruence axioms, but it is remarked in a footnote that more independence proofs are found in the Von Schaper *Ausarbeitung* of Hilbert's 1898–1899 lectures on the foundations of geometry.[58] Already in the 1894 lectures did Hilbert claim ("*cum grano salis*") the mutual independence of the axioms (cf. Hallett & Majer, 2004, p. 79), but the clearest explanation of the notion itself is found in this Von Schaper *Ausarbeitung*:

> In order to show that an axiom $\mathfrak{A}$ does not follow logically from the axioms $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$,..., we supply a system of things in which $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$,... are valid, but $\mathfrak{A}$ is not.[59]

A similar explanation of how to demonstrate independence had been given by Schröder (1890), indeed Schröder states that this method of exemplification is the only method of demonstrating independence.[60] For an example, recall that Hilbert's axiom I.1 says that two points always determine a line; axiom I.2 says that any two points on a line determine that line. Hilbert shows that I.2 is independent of I.1 by taking points to be the positive integers, lines to be the negative integers, and saying that points A and B determine the line $-\lfloor \frac{A \cdot B}{2} \rfloor$. Then the points 1 and 2 determine the same line as the points 1 and 3, namely -1,

56 Dedekind (1932c, p. 316): "Es kam nur noch darauf an, seinen eigentlichen Ursprung in den Elementen der Arithmetik zu entdecken und hiermit zugleich eine wirkliche Definition von dem Wesen der Stetigkeit zu gewinnen."

57 Hilbert's work on independence in geometry is discussed by Hallett (2008, §8.4).

58 This *Ausarbeitung* as well as Hilbert's own lecture notes for the same course have been published in Hallett & Majer (2004).

59 Hallett & Majer (2004, p. 306): "Um zu zeigen, daß ein Axiom $\mathfrak{A}$ keine logische Folge der Axiome $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$,... ist, geben wir ein System von Dingen an, bei welchem $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$,... gelten, $\mathfrak{A}$ aber nicht."

60 Schröder (1890, pp. 286–287): "Ein solcher 'negativer' Beweis [i.e. of independence] kann nur durch *Exemplifikation* geleistet worden. [...]
Dass ein Satz *A* aus einer Gruppe von Definitionen, Axiomen und Sätzen *B* *nicht* mit Notwendigkeit folgt, wird jedenfalls dann unzweifelhaft erwiesen sein, wenn es gelingt, ein Gebilde als wirklich oder denkmöglich nachzuweisen, welches die Definitionen, Axiome (und Sätze) der Gruppe *B* sämtlich bewahrheitet und gleichwohl den Satz *A* nachweislich *nicht* erfüllt—kurz: wenn man zeigt, dass irgendwo die Sätze *B ohne A* geltend vorkommen."

but not the same line as the points 2 and 3. Thus in this "system of things" axiom I.1 is valid (by definition), whereas axiom I.2 is not valid (cf. Hallett & Majer, 2004, p. 306).

In the *Zerlegungen* paper on dual group theory introduced in Section 2.1 above, Dedekind considers independence questions in the sense of Hilbert.[61] Dedekind's phrasing of independence questions is, however, different from Hilbert's. I wish to highlight this difference as another manifestation of their contrasting views of science. Before proceeding, however, it might be helpful for the reader to look back at the end of Section 2.1 above where we cited Dedekind's definition of the notion of a dual group and listed the defining equations A; in particular, recall that these equations require the operations $\pm$ in a dual group to satisfy commutativity, associativity, as well as the so-called laws of absorption $\alpha \pm (\alpha \mp \beta) = \alpha$.

Dedekind's study of dual group theory had been occasioned by his study of the concept of a module (Dedekind, 1897, p. 113); in a rough-and-ready description, one could say that as the theory of modules serves as a foundation for Dedekind's ideal theory, dual group theory was intended as a foundation for module theory, a more general theory of which module theory would be a special case.[62] For this reason, two sets of equations were of special interest to him. The first set consists of the equations asserting the distributivity of $+$ over $-$, and of $-$ over $+$:

$$\begin{aligned} \alpha + (\beta - \gamma) &= (\alpha + \beta) - (\alpha - \gamma) \\ \alpha - (\beta + \gamma) &= (\alpha - \beta) + (\alpha - \gamma) \end{aligned}$$

The significance of these laws for Dedekind lay in their being satisfied by the lattice of ideals in any ring of algebraic integers, where ideal addition interprets '$+$' and set-theoretical intersection interprets '$-$'. The other set consists of three equations that Dedekind proves equivalent over A, and one of which is the following so-called modular law:

$$(\alpha + \beta) - (\alpha + \gamma) = \alpha + (\beta - (\alpha + \gamma))$$

This law is satisfied by any lattice of modules. Dedekind shows that if one adds the distributive laws to A, then the modular law follows. His interest in these sets of equations therefore centered on the questions, firstly of whether the modular law follows from A alone, and secondly of whether the distributive laws follow from A plus the modular law. It is here that the contrast with Hilbert shows, for Dedekind does not state these questions of independence in Hilbertian terms: does this equation follow from those equations? Rather, the question is rephrased in terms of the having and not-having of certain properties. Dedekind calls dual groups satisfying the modular law 'groups of modular type' (*Modultypus*), and those satisfying the distributive law 'groups of ideal type' (*Idealtypus*). His questions of independence are then literally these (Dedekind, 1897, p. 116):

> Are there dual groups that do not possess modular type?
> Are there dual groups of modular type that do not possess ideal type?

In Hilbertian terms, on the other hand, the questions would presumably take something like the following form: is it possible through logical inferences to derive the modular law from the axioms A; is it possible through logical inferences to derive the distributive law

[61] With meticulous page references characteristic of his style, Dedekind remarks that he had been anticipated by Schröder (1890); in this work, Schröder had in effect established the independence of the distributive law from the lattice axioms. Cf. the previous Footnote 60.

[62] For a more detailed account of the relation of module theory to dual group theory, see Mehrtens (1979, chap. 2.1) and Corry (1996, chap. 2.3).

from the axioms *A* with the modular law added?[63] Thus in this case we have the question of whether a certain formal judgment follows from certain other formal judgments; in Dedekind's case, on the other hand, we have the question of whether things with certain properties of necessity also possess certain other properties. Dedekind's answer to that question upholds the perspective of property possession (Dedekind, 1897, p. 116):

> I have discovered—not without effort—that both these questions are to be answered affirmatively, in each case by seeking the smallest dual group which possesses the relevant property.

Thus he describes by means of group tables the dual groups known as the pentagon and the diamond, and shows that the first is not modular, while the second is modular but nonideal. The technique is surely the same as that which an axiomatist would make use of: as Schröder noted, the only way to demonstrate independence is through exemplification. But the example put forth thereby is by Dedekind not thought to satisfy such and such axioms while not satisfying certain other formal judgments, rather it is thought to be an object possessing such and such properties while not possessing certain other properties.

§5. Axiom and implicit definition. Structuralism. In his *Grundlagen*, Hilbert famously claims that a certain group of his axioms for geometry defines the notion of betweenness;[64] in a letter to Frege, Hilbert says that he regards the whole set of axioms together with the declaration introducing the notions of point, line, and plane as defining those notions.[65] As Frege's correspondence with Hilbert and Liebmann bears witness to, it is not straightforward how to understand this. I follow Gabriel (1978, p. 420) in holding that if one is to regard Hilbert's axioms as defining anything at all, then one has to regard them as defining a higher-level concept, namely a concept under which fall structures or domains, in modern terminology n-tuples for suitable n;[66] in other words, in that case one must take the higher-level point of view on Hilbert's axioms (cf. Section 2.2 above). Against this it could be suggested that with Hilbert's axioms the concepts of point, line, plane, betweenness, and so on receive, as it were, a holistic definition: points, lines, planes, and so on are whatever satisfy the axioms, and a point is a point only relative to the things, whatever they may be, that serve as lines and planes. The problem with this kind of view—defended, for instance, by Schlick (1918, §7)—is that it would seem to depend on a novel notion of concept which has not been clarified. Carnap (1927) called such an "implicitly defined" concept *improper*, for the question of whether something falls under it is in effect meaningless (ibid. p. 367); as Frege famously notes, with Hilbert's definitions—that is, the axioms when viewed as definitional in this holistic sense—one

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[63] Cf. Hilbert's gloss on the independence of the axioms in *Grundlagen* §10 (Hilbert, 1899, p. 22): "In der That zeigt es sich, dass keines der Axiome durch logische Schlüsse aus den übrigen abgeleitet werden kann."

[64] Hilbert (1899, p. 6): "Die Axiome dieser Gruppe definiren den Begriff 'zwischen'..."

[65] Frege (1976, p. 66, Letter XV/4, dated December 29, 1899): "Ich sehe in meiner Erklärung in §1 die Definition der Begriffe Punkte, Gerade, Ebenen, wenn man wieder die sämtlichen Axiome der Axiomgruppen I–V als die Merkmale hinzunimmt."

[66] On my count, $n = 8$ in the case of Hilbert's geometry; cf. Section 7.2 below.

cannot settle whether someone's pocket watch is a point or not.[67] On that background I think the onus is on he who wants to defend this holistic reading: he must explain what is thereby understood by a concept, and hence also what is thereby understood by the definition of a concept—for on a traditional way of understanding the notions of concept and definition, the view that the axioms holistically define the terms occurring in them is simply confused.

It seems that Carnap (1927) is written as a reaction to the very idea of implicit definition.[68] Thus Carnap explains how any implicit definition can be regarded as the definition of a proper concept, that is, of a concept for which the question of whether something falls under it has a determinate answer. In particular, he explains how Hilbert's axioms can be viewed as properly defining an n-ary higher-level relation:

> Also through the Hilbertian axiom system, as through any axiom system, a determinate *proper concept* is explicitly defined. If we designate the three basic classes of the axiom system by p, g, and e, the three basic relations by I, Z, K, then the proper concept is the six-place relation H, whose arguments are to be designated by the six basic variables:
>
> $$H = \hat{p}\hat{g}\hat{e}\hat{I}\hat{Z}\hat{K}[\ldots \text{(logical product of the axioms)} \ldots]\text{Df.}$$ [69]

This is precisely how one would regard Hilbert's axioms from the higher-level point of view. Now it is widely held, I think, that already Frege interpreted Hilbert's axioms along such higher-level lines. However, although Frege with his interpretation of the primitive terms as variables and his appeal to concepts of the second level made several crucial steps towards this point of view, there is, as far as I have seen, no statement in Frege's work to the effect that Hilbertian axioms define a kind of structure. In fact, it seems that Frege lacked a sufficiently clear notion of domain or structure. A passage which seems to reveal this is the following comment upon Hilbert's geometry:

> Thus if a concept is defined by [the Hilbertian axioms], then it has to be a concept of second order. It must, however, be doubted whether a concept is defined, for not only the word 'point', but also the words 'line' and 'plane' occur.[70]

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67 Frege (1976, p. 73, Letter XV/5, dated January 6, 1900): "Ich weiss nicht, wie ich mit Ihren Definitionen die Frage entscheiden soll, ob meine Taschenuhr ein Punkt sei."

68 It is worth noting that the term 'implicit definition' itself is ambiguous: Gabriel (1978) distinguishes three quite different ideas connected to it.

69 Carnap (1927, p. 369): "Auch durch das Hilbertsche Axiomensystem wird, wie durch jedes Axiomensystem, ein bestimmter, *eigentlicher Begriff* explicit definiert. Bezeichnen wir die drei Grundklassen des Axiomensystem mit p, g, e, die drei Grundrelationen mit In, Z, K, so ist dieser eigentlicher Begriff die sechsstelllige Relation H, deren Argumente durch die sechs Grundbegriffsvariabeln zu bezeichnen sind:

$$H = \hat{p}\hat{g}\hat{e}\hat{I}\hat{Z}\hat{K}[\ldots \text{(logisches Produkt der Axiome)} \ldots]\text{Df.''}$$

The symbol ˆ here is an abstraction operator.

70 Frege (1903, p. 374): "Wenn also durch sie ein Begriff definirt wird, so kann es nur ein Begriff zweiter Stufe sein. Ob ein Begriff definirt werde, muß freilich bezweifelt werden, weil nicht nur das Wort "Punkt", sondern auch die Wörter "Gerade" unde "Ebene" vorkommen." In a letter to Liebmann, Frege described this state of affairs as a *Monstrum* (Frege, 1976, p. 148, Letter XXVII/1, dated July 29, 1900).

That is, Frege did not see how one should account for the intertwinement of primitive terms within Hilbert's axioms; but given a notion of structure this is quite easy: the axioms do not define a second-level concept of point, but a second-level concept of Euclidean geometry, in the Fregean scheme, a many-termed relation.[71]

I would like to suggest that Dedekind, on the other hand, did possess a clear notion of structure, and hence that he at least had the resources to read Hilbert in a higher-level manner. Dedekind's definition of a dual group is nothing but the definition of a kind of structure, and the examples he gives of dual groups are structures, or domains as he calls them (cf. Section 2.1 above). Indeed our arguments in Section 2.2 above that Dedekind took the higher-level point of view are also arguments that he possessed a notion of structure. What one might call Dedekind's mathematical structuralism is in fact to a large extent coincidental with his higher-level point of view. It is a conception of the domains of mathematics as consisting not mainly of first-level objects such as numbers and triangles, but rather of higher-level objects such as fields, modules, and dual groups. As Corry (1996) has argued, mathematical structuralism in this sense is less developed in Dedekind than what it is for instance in the textbook of van der Waerden (1930); at least that is the case with regards to the theory of fields and modules. The difference between Dedekind and van der Waerden in this regard can be summarized in two points. Firstly, most of Dedekind's mathematical theories "live" in a restricted universe, namely that of the complex numbers; in van der Waerden, on the other hand, the universe is the universe of sets. Secondly, in Dedekind's theory of fields and modules, the underlying algebraic operations are fixed, namely as ordinary addition, multiplication, and so on, whereas in van der Waerden they are schematic. As we have seen, Dedekind's dual group theory has both a universal background domain as well as schematic operations $+$ and $-$, so this theory would seem to manifest mathematical structuralism in its fully developed form; but, to repeat, that is not the case with the field and module theory, even that in its most developed form of Dedekind (1894).

One should be careful now to distinguish such mathematical structuralism from philosophical ones.[72] Dedekind's philosophical structuralism, by which I merely mean his construal of the notion of natural number as the abstract type of simply infinite systems,[73] enters, as I see it, at the level of explication in the sense of Carnap (1947, §2): on the one hand we have a concept of number that we apply in "everyday life," and on the other the mathematical concept of a simply infinite system—Dedekind's philosophical structuralism is the construal of the one in terms of the other, thus the construction of a bridge between a scientific and an everyday concept. The considerations in article 134 of *Was sind* can

[71] Heck (1995) calls attention to *Grundgesetze* §144, where Frege speaks of "whenever the objects falling under [a] concept can be ordered in a series which begins with a certain object and continues endlessly..." From a current point of view, the following theorem 263 demonstrates that such a "structure" is isomorphic to the natural number structure. The question is whether this current point of view was also Frege's.

[72] Thus Reck & Price (2000) distinguish structural methodology in mathematics from structuralist philosophies of mathematics.

[73] Dedekind (1890a, p. 275): "7) Nachdem in meiner Analyse der wesentliche Charakter des einfach unendlichen Systems, dessen abstrakter Typus die Zahlenreihe *N* ist..." Cf. *Was sind* article 73. On the abstraction in question, see Tait (1997) and Reck (2003), as well as the illuminating citation from the antepenultimate draft of *Was sind* given by Sieg & Schlimm (2005, p. 152).

then be seen as a soundness argument, namely to the effect that no theorem belongs to the science of numbers, or arithmetic, which is not also a truth about the numbers of everyday life when explicated as the abstract type of simply infinite systems, and vice versa, nothing is a truth about these explicated everyday life numbers which is not also a theorem of arithmetic. Returning finally to implicit definitions, can we say that Hilbert had the resources to view his geometry in higher-level terms, as defining a kind of structure? It seems reasonable to claim that if he did have such resources, then he would not have said that the primitives of an axiom system are defined by that system, since putting it this way only obscures the matter. Thus I am inclined to hold that the Hilbert who wrote *Grundlagen*, unlike Dedekind (the author of *Zerlegungen*, say), did not possess such resources.[74] To the extent that this is the case, we have here a difference between Hilbert and Dedekind which I think again may be seen as stemming from their differing views on the structure of mathematical science.

§6. Dedekind's substantive primitive terms. Regardless of whether Dedekind conceived of a hierarchy of concepts around which his mathematics is built, it will be evident to any reader of *Was sind* that Dedekind there assumes certain fundaments upon which the edifice is erected.[75] On my reckoning, this fundament is made up of the following notions:

- a is a thing
- $a = b$ (a is the same thing as b)
- A is a system
- $a \in A$ (a is an element of A)
- φ is a mapping of the system A
- $\varphi(s)$ (the φ-image of s)

In prose, the primitives of *Was sind* are the notions of thing, thing identity, system, elementhood, mapping, and image. It is remarkable that Dedekind does not take these primitive terms for granted, but, as I will presently argue, introduces them through elucidation. Elucidation presupposes a substantive sense of that which is elucidated. The claim of the present part of the paper is that Dedekind differs from Hilbert in maintaining this presupposition, since for Hilbert the primitive terms do not have a substantive sense, but are merely schematic, and without substantive sense.

6.1. Reinterpretation, relabeling, temporarily disregarding sense. Some distinctions should be drawn here to clarify what is involved in this claim. Firstly, one must distinguish between, on the one hand, the recognition that the terms of a theory may be reinterpreted, and on the other, the terms's being schematic. In the first case the terms are substantive and a reinterpretation merely "swaps" one sense for another, leaving the sign (inscription, acoustic image, etc.) intact; in the second case the terms are through and through nonsubstantive, with a formal sense only, and hence in need of interpretation for attaining substantive sense. The distinction is illustrated by comparing Bolzano's and Quine's notions of what Bolzano (1837, §148) called logical analyticity of a proposition

74 Resnik (1974, p. 394) reports that he has "been unable to locate a statement by Hilbert that his axioms define structures."

75 In his official reply to Keferstein, this edifice is described as his "kunstvoll gefügte, in allen seinem Teilen fest geschlossenene, unerschütterliche Gebäude…" (cf. Dedekind, 1890b, p. 259).

and Quine (1982, chap. 6) called validity of schema. A proposition is logically analytic if all reinterpretations—or properly speaking, variations, since for Bolzano a proposition (*Satz an sich*) is not an expression—of its nonlogical terms leave unaltered the truth or falsity of the proposition. A schema is valid if all of its interpretations are true. In the first case, we set out with a proposition and through varying its nonlogical terms we obtain new propositions, but we never, as it were, leave the realm of propositions to enter that of "schematic propositions" or "propositional forms"; in the second case, however, we set out precisely with such a form, a schema, and this needs to be "filled," that is interpreted, before one obtains any proposition at all.

Dedekind grasped the idea of reinterpretation very well, indeed he grasped its significance for the investigation of logical dependencies. This is shown in the preface to *Was sind*, where, developing thoughts present already 12 years earlier in his exchange with Lipschitz, Dedekind reinterprets the notion of point so that an "everywhere non-continuous" space results in which,

> as far as I can see, all constructions that appear in Euclid's *Elements* may be carried out just as they can in the completely continuous space; the discontinuity of this space would thus not at all be noticed, not at all be perceived, in Euclid's science.[76]

Dedekind here conceives of a reinterpretation of the terms of Euclid's geometry: instead of their ordinary, or intuitive meaning, on which space is completely continuous, they are given a new meaning explained in terms of the intuitive notion, namely by assuming only enough intuitive points to be present for the geometrical constructions to go through. Thus Dedekind does not here treat the terms of geometry as schematic, for he never leaves the sphere of the substantive, but rather gives a new meaning to 'point' (the meaning of 'line' and 'plane' being naturally induced by the meaning of 'point'). Hence our claim is not that Dedekind does not recognize that the terms of a science may be reinterpreted, for instance towards the investigation of logical dependencies; the claim is rather that he never treats the terms of a theory as schematic.

Reinterpretation of the primitive terms must be distinguished from the dual operation of relabeling. Roughly, in a reinterpretation one changes the sense and keeps the sign; in a relabeling one changes the sign and keeps the sense. Relabeling is of help in the rigorous development of a science, since giving the terms foreign names is a way of hindering associations from motivating inferences not justified by the axioms and rules of inference. Dedekind was well-aware of this, as is witnessed by the following famous passage from a letter to Lipschitz:

> an infallible method for such an analysis consists to my mind in this, that one replaces all technical terms by arbitrary and newly invented (hitherto meaningless) words; the edifice, if correctly constructed, shall

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[76] Dedekind (1932c, pp. 339–340, *Was sind* Preface): "…aber trotz der Unstetigkeit, Lückenhaftigkeit dieses Raumes sind in ihm, so viel ich sehe, alle Konstruktionen, welche in Euklids Elementen auftreten, genau ebenso ausführbar wie in dem vollkommen stetigen Raume; die Unstetigkeit deises Raumes würde daher in Euklids Wissenschaft gar nicht bemerkt, gar nicht empfunden werden."

> not thereby collapse, and I claim, e.g., that my theory of real numbers passes this test.[77]

The literature has frequently compared this remark of Dedekind's with Hilbert's famous remark on "tables, chairs, and beer mugs."[78] To my mind, it seems that these two remarks should be kept apart, for in the first, relabeling is at issue, whereas reinterpretation would seem to be at issue in the second. However that may be, for current purposes the main point is that seeing the possibility of relabeling the terms of a theory does not amount to treating them as schematic.

Finally, one must distinguish treating terms as schematic from how one treats terms when "thinking in symbols." In the latter case the terms are substantive at the outset, but one temporarily disregards their sense and operates on the signs as such, letting the symbols do the thinking, as it were. Already Leibniz, Lambert, and others realized the potential of symbolic thinking; thus Lambert speaks warmly of "reducing the theory of things to the theory of signs."[79] With reference to a symbol system for numbers in base X (e.g., 10), Husserl describes the procedure as follows:

> . . . in practical tasks of counting given sets, as well as in tasks of computationally deriving numbers from numbers, one can obtain the solution purely mechanically, provided that one substitutes the names for the concepts and then, by means of the system of names, derives names from names in a purely superficial procedure, whereby names at last result whose conceptual construal necessarily yields the result sought for.[80]

Thus, one starts out with substantive terms, but rather than thinking through the concepts signified by these terms one thinks through the signs according to some preestablished system; it is, however, not the outcome of this "superficial procedure" which is regarded as the result sought for, rather the result sought for is this outcome in its conceptual construal. Characteristic of this method is it thus that one leaves the substantive only temporarily, in general thereby facilitating the attainment of the result; in particular, the terms are still at root substantive, for the beginning as well as the end of the procedure consist in substantive terms and substantive judgments.

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77 Dedekind (1932c, p. 479, letter dated July 27, 1876): "Eine untrügliche Methode einer solchen Analyse besteht für mich darin, alle Kunstausdrücke durch beliebige neu erfundene (bisher sinnlose) Worte zu ersetzen, das Gebäude darf, wenn es richtig construirt ist, dadurch nicht einstürzen, und ich behaupte z.B., daß meine Theorie der reellen Zahlen diese Probe aushält."

78 The comparison is made, for example, by Stein (1988, p. 253), Ferreirós (1999, p. 132), Schlimm (2000, p. 16), and Sieg & Schlimm (2005, p. 155). Hilbert's remark is reported by Blumenthal (1935, pp. 402–403).

79 Lambert (1764, §24): "Die Theorie der Sache auf die Theorie der Zeichen reduciren, will sagen, das dunkle Bewußtseyn der Begriffe mit der anschauenden Erkenntniß, mit der Empfindung und klaren Vorstellung der Zeichen verwechseln."

80 Husserl (1891, p. 239): " . . . daß man sowohl bei Aufgaben praktischer Zählung gegebener Mengen als auch bei solchen der rechnenden Herleitung von Zahlen aus Zahlen die Lösung rein mechanisch gewinnen kann, indem man die Namen den Begriffen substituiert und dann an Hand der Systematik der Namen in rein äußerlicher Prozedur Namen aus Namen herleitet, wobei schließlich Namen resultieren, deren begriffliche Deutung das gesuchte Resultat notwendig ergibt."

As is perhaps to be expected of an algebraist, Dedekind was familiar with the idea of thinking in symbols. This is especially clear from his *Zerlegungen* paper. There Dedekind makes a discrimination between what it is for an equation E to follow from the equations A and what it is for E to be true in virtue of the sense that has been given to its terms. Thus Dedekind says of the demonstration of a certain theorem that it:

> results immediately from the theorems above without its being necessary to have recourse to the signification of our signs.[81]

Dedekind does not state explicitly what it is for an equation E to follow from other equations, but whenever he derives an equation from the initial system A, it is by means of substitutions, assuming reflexivity, transitivity, and symmetry of the identity sign. In the case at hand, however, Dedekind is not operating with terms that are schematic from the outset; he is working within the theory of finite sets, and he claims in effect that the theorem in question would follow from the equations A even by calculation on the signs themselves, by temporarily disregarding the sense of the terms involved. A few pages later Dedekind gives his definition of dual groups; as was argued in Section 2.2, in that definition the higher-level point of view is in full force, whence a dual group is accordingly regarded as a certain kind of structure or domain, and dual group theory as a theory built around substantive primitives. This fact, therefore, that Dedekind sometimes alludes to thinking in symbols does not show that the primitive terms of his theories were not substantive.

6.2. Elucidation. Most definitions in *Was sind* have the form of a nominal definition; the definition of an infinite system in article 64 may serve as an example:

> A system S is said to be **infinite** when it is similar to a proper part of itself.[82]

The word marking definitional equivalence here is ‘is said to be’ (‘heißen’), but one also finds ‘designate’ (‘bezeichnen’), ‘then we say that’ (‘so sagen wir’) and like phrases indicating abbreviation; moreover, these definitions have a grammatical form that will allow for the substitution of the *definiens* for the *definiendum*. The descriptions of the primitive notions, however, are not accompanied by indicators of nominal equivalence, or at least they have a grammatical form that will not license substitution:

- In the following I understand by a **thing** any object of our thought.[83]
- A thing a is the same as b (identical to b), and b the same as a, when everything that can be thought of a can also be thought of b, when everything that holds of b also can be thought of a.[84]

[81] Dedekind (1932b, p. 111): “Der B e w e i s ergibt sich unmittelbar aus den obigen Sätzen, ohne daß es nötig wäre, auf die Bedeutung unsererer Zeichen zurückzukommen.”

[82] Dedekind (1932c, p. 356, *Was sind* article 64): “Ein System S heißt u n e n d l i c h, wenn es einem echten Teile seiner selbst ähnlich ist.”

[83] Dedekind (1932c, p. 344,*Was sind* article 1): “Im folgenden verstehe ich unter einem D i n g jeden Gegenstand unseres Denkens.”

[84] Dedekind (1932c, p. 344,*Was sind* article 1): “Ein Ding a ist dasselbe wie b (identisch mit b), und b dasselbe wie a, wenn alles, was von a gedacht weden kann, auch von b, und wenn alles, was von b gilt, auch von a gedacht werden kann.”

- It very frequently happens that, for some reason or other, different things a, b, $c\ldots$ are comprehended under a common point of view, or composed in the mind, and one says then, that they form a **system**; the things a, b, $c\ldots$ are called the **elements** of the system S, they are **contained** in S; conversely, S **consists** of these elements. Such a system S (or aggregate, manifold, totality), as an object of our thought, is likewise a thing.[85]
- By a **mapping** φ of a system S I understand a law according to which there belongs to every element s of S a determined thing, which is called the **image** of s and designated by $\varphi(s)$.[86]

One sees clearly that the descriptions given here of system, of elementhood, of mapping, and of image are all nonnominal: they do not declare a certain term to be the abbreviation of a combination of certain other terms already understood. The descriptions of the notions of thing and thing identity are perhaps less clearly nonnominal. ‘Thing’ can in principle be seen as the *definiendum* in a nominal definition whose *definiens* is ‘object of thought’, and thing identity can be seen as defined through Leibniz’s principle. In the following I will treat the notion of thing and identity among things as primitive to Dedekind’s theory, but nothing essential will hinge upon them being so taken; note, however, that identity for systems is not primitive, but is defined in terms of the notion ‘$a \in A$’ and the principle of extensionality. Setting aside these somewhat problematic, yet for our purposes inconsequential, cases of thing and thing identity, I suggest, as already indicated, to read Dedekind’s descriptions of his primitive terms as elucidations in the sense of Frege’s *Erläuterungen*.[87] In the second installment of *Über die Grundlagen der Geometrie* (1906), Frege introduced this notion as follows:

> As soon as the researchers have made the primitive elements and their significations understood, then the understanding of the logically composite through definition is easy to achieve. However, since the latter is not possible with regards to the primitive elements, something else has to enter here; I call that elucidation.[88]

Applying to the construction of concepts the arguments that Aristotle made in *Posterior Analytics* I.3 with respect to the justificational structure of judgments, one reaches the

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[85] Dedekind (1932c, pp. 344–345, *Was sind* article 2): “Es kommt sehr häufig vor, daß verschiedene Dinge a, b, $c\ldots$ aus irgendeiner Veranlassung unter einem gemeinsamen Gesichtspunkte aufgefaßt, im Geiste zusammengestellt werden, und man sagt dann, daß sie ein System S bilden; man nennt die Dinge a, b, $c\ldots$ die Elemente des Systems S, sie sind enthalten in S; umgekehrt besteht S aus diesen Elementen. Ein solches System S (oder ein Inbegriff, eine Mannigfaltigkeit, eine Gesamtheit) ist als Gegenstand unseres Denkens ebenfalls ein Ding.”

[86] Dedekind (1932c, p. 348, *Was sind* article 21): “Unter einer Abbildung φ eines Systems S wird ein Gesetz verstanden, nach welchem zu jedem bestimmten Element s von S ein bestimmtes Ding gehört, welches das Bild von s heißt und mit $\varphi(s)$ bezeichnet wird.”

[87] For a recent discussion of elucidation and primitive notions in Frege, see Tolley (2011).

[88] Frege (1906, p. 301): “Wenn sich die Forscher über diese Urelemente und ihre Bezeichnungen verständigt haben, ist das Einverständnis über das logisch Zusammengesetze durch Definition leicht erreichbar. Da bei den Urelementen diese nicht möglich sind, muß hier etwas anderes eintreten; ich nenne es Erläuterung.”

conclusion that (nominal) definition cannot go on forever.[89] As stated in the Introduction above, there has to be a foundation for the construction of concepts, that is one has to accept primitive terms, and if these primitives are also primitive in the hierarchy of concepts, then one cannot convey their meaning through nominal definition, but "something else has to enter." What this something else is need not be laid down beforehand; any means necessary to get the learner to grasp the sense of the primitive terms are permitted. Frege's use of metaphorical language in explaining the primitive notion of function is one example; another example is description of how one should go about generating that which is signified by the term thereby elucidated, along the lines of Dedekind's explanation of the notion of system. A third mode of elucidation is definition by ostension, as Pasch (1882, p. 16) recommends for the primitive concepts of geometry,[90] and Locke sees as necessary for coming to understand the name of a simple idea (*Essay* III.iv.11);[91] finally, using the primitive in a context, along the lines that Wittgenstein seems to envision at *Tractatus* 3.263, is a fourth.[92] This openness of method is one point at which elucidation contrasts with nominal definition—for the latter, stringent rules may be laid down.[93] Another point of contrast is that elucidation, unlike nominal definition, does not allow for substitution of the *definiens* for the *definiendum*. Indeed, in many cases such substitution is prohibited already on grounds of grammar: for instance, an ostension does not have the "grammar" of a term.

Lack of substitutability would seem to be what underlies Frege's insistence that elucidation belongs to the "courtyard of science," is dispensable, and is not proper to the

89 Such an argument can be found, for example, in Locke's *Essay* III.iv.5. According to Scholz (1945, p. 121) it was Pascal (1657) in *De l'esprit géométrique* who first clearly realized the parallel between the structure of judgments and the structure of concepts. As Scholz argued elsewhere, however, there are passages in *Posterior Analytics* (in particular in I.10 and II.9) which suggest that already Aristotle had seen the parallel and had recognized along with the notion of axiom also the notion of primitive term (Scholz, 1930, pp. 38–39). In fact, there is a striking similarity between the passage from *Über die Grundlagen der Geometrie* recently cited and *Posterior Analytics* II.9, $93^{b}22$–24:

> Of some things there is something else which is their explanation [*aition*], of others there is not. Hence it is plain that in some cases what something is is immediate and a principle; and here you must suppose, or make clear in some other way [*allon tropon fanera poiēsai*], both that the thing exists and what it is. [Translation from Barnes, 1993]

It may be noted that Pascal in the mentioned fragment does not appeal to any notion of elucidation, but holds that primitive terms are *claires et constantes par la lumière naturelle* (Lafuma, 1963, p. 350).

90 See Schlimm (2010) for an account of this and other aspects of Pasch's philosophy of mathematics; ostension is discussed there, in connection with Pasch's empiricism, at page 100.

91 Locke (1975, p. 425): "And therefore he that has not before received into his Mind, by the proper Inlet, the simple *Idea* which any Word stands for, can never come to know the signification of that Word, by any other Words, or Sounds, whatsoever put together, according to any Rules of Definition. The only way is, by applying to his Senses the proper Object; and so producing that *Idea* in him, for which he has learn'd the name already."

92 It would seem to be this fourth method of elucidation which Frege mocks in his musings on "Jedes Anej bazet wenigstens zwei Ellah" (Frege, 1906, p. 397). On the notion of elucidation presupposed at TLP 3.263, see Hacker (1986, pp. 75–78).

93 See, for example, Frege (1893, pp. 51–52) and Leśniewski (1931).

science itself: the science itself begins with the primitive terms, and nothing is there with which they could be substituted. Elucidation is dispensable, that is to say, elucidation, like Wittgenstein's ladder, may be thrown away once climbed.[94] Now, in climbing the ladder, in coming to comprehend the primitives through elucidation, one will naturally have to make use of knowledge and skills obtained in contexts outside the science in question; but this is not to say that the science depends logically on this knowledge and these skills. This is important to appreciate in considering, for instance, Frege's use of elucidation: does that put the logicist project in danger, in that the primitive notion of function, for instance, in some sense comes to depend on extra-logical knowledge? The answer is no, I think, for the knowledge one makes use of in carrying out elucidation need not be logically prior to the notions elucidated.[95]

§7. Hilbert's schematic primitive terms.

7.1. Hilbert's conception of the primitive terms. It seems to be against the very idea of elucidation that Hilbert is inveighing in the following passage from his most extended letter to Frege:

> If one seeks other definitions of 'point', say through a rephrasing such as 'without extension', then I have to disapprove in a most determined way of any such attempt; one is then in search of something that one shall never find, as there is nothing there, and everything gets lost in confusion and obscurity, and evolves into a game of hide-and-seek.[96]

The aim of elucidation is to convey the sense of the primitive terms to the learner. There is no guarantee that this will succeed, that the learner will indeed grasp the sense one intends in the primitive terms; one has to "count on a certain amount of good will, on a receptive understanding, on guessing."[97] This may seem to leave an element of imprecision at the very beginning of a science. Such apparent imprecision might be the reason why Hilbert did not accept the idea that the sense of the primitive terms is to be fixed by elucidation. In fact, Hilbert (1905), in his discussion of Cantor, can be seen as criticizing the latter for admitting imprecision into the foundations of logic and arithmetic; paraphrasing Hilbert, although Cantor had realized the contradiction implicit in the notion of the totality of all things, and had consequently distinguished consistent from inconsistent multiplicities, he had given no precise criterion for applying this distinction; that is to say, Cantor's "conception regarding this matter [. . .] gives leeway for subjective considerations and warrants

[94] Cf. TLP 6.54, which besides employing the ladder metaphor also employs the verb *erlaütern*; this may have occasioned Neurath (1932, p. 214) to speak of Wittgenstein's conception of philosophy in the *Tractatus* as an "elucidatory ladder" (*Erläuterlungsleiter*).

[95] Cf. Hallett (2010, p. 436).

[96] Frege (1976, p. 66, Letter XV/4, dated December 29, 1899): "Wenn man nach andern Definitionen für "Punkt", etwa durch Umschreibungen wie ausdehnungslos etc. sucht, so muss ich solchem Beginnen allerdings aufs entschiedenste widersprechen; man sucht da etwas, was man nie finden kann, weil nichts da ist, und alles verliert sich und wird wirr und vage und artet in Versteckspiel aus."

[97] Frege (1906, p. 301): "Dabei muß auf etwas guten Willen, auf entgegenkommendes Veständnis, auf Erraten gerechnet werden können."

therefore no objective certainty."[98] I think Hilbert could have raised similar objections against the notion that the primitive terms are to be understood through elucidation.

If the sense of the primitives is not to be fixed by elucidation, however, then there would seem to be nothing at all prior to the system of the science that could fix their sense; indeed, one can read Hilbert as saying that nothing *should* fix the sense of the primitives, when he writes to Frege that "I do not want to assume anything as known"[99]—not even knowledge of the meaning of the primitive terms is to be assumed. Hilbert's way of achieving this is to let the primitive terms have only a formal sense, to let them be variables, or schematic letters. I would like to suggest that this be viewed as a consequence of Hilbert's apparent dismissal of elucidation as a signpost to the sense of the primitive terms. For if someone is to construct a science on the foundation of certain substantive terms, then he shall presumably be in a position to elucidate those terms; the terms need to be sufficiently clear to him, for otherwise he would presumably not be able to construct the science in question. Hence, if asked what he means by a certain primitive, if asked to give elucidations, he should be able to give an answer. That this answer may be more or less felicitous is just a reflection of the fact that elucidation itself may be more or less felicitous. But since Hilbert apparently denies that the primitives should or indeed could be elucidated, he would therefore seem to be denying any substantive sense at all to the primitives. In this way, then, Hilbert's treatment of the primitives as schematic may be regarded as a consequence of his view of elucidation of the primitives as futile.

Thus Hilbert's primitives are of a schematic character, and this character spreads through the whole theory. What results is, as Hilbert says, a schema, or scaffolding, open for various ways of being filled with material. Hilbert held this conception of mathematical theories already by 1894:

> Our theory yields only the schema of concepts, which are connected with each other through the immutable laws of logic. It is left to the human intellect how it applies this schema on the phenomena, how it fills it with material.[100]

Similar ideas were entertained by other thinkers at the time. Thus, in a remarkable paper, Weber (1893) speaks of his presentation of Galois theory as a "pure formalism which attains content and life," only through furnishing the terms with "number values."[101] And Husserl remarks in the *Philosophie der Arithmetik* that

[98] Hilbert (1905, p. 176): "Indem er aber meiner Meinung nach für diese Unterscheidung kein scharfes Kriterium aufstellt, muß ich seine Auffassung über diesen Punkt als eine solche bezeichnen, die dem *subjektiven* Ermessen noch Spielraum läßt und daher keine objektive Sicherheit gewährt."

[99] Frege (1976, p. 66, Letter XV/4, dated December 29, 1899): "Ich will nichts als bekannt voraussetzen."

[100] Hallett & Majer (2004, p. 104): "Unsere Theorie liefert nur das Schema der Begriffe, die durch die unabänderlichen Gesetze der Logik mit einander Verknüpft sind. Es bleibt dem menschlichen Verstande überlassen, wie er dieses Schema auf die Erscheinung anwendet, wie er es mit Stoff anfüllt." Earlier in the same lectures (ibid. p. 72) Hilbert uses the metaphor of scaffolding of concepts (*Fachwerk der Begriffe*).

[101] Weber (1893, p. 521): "Die Theorie erscheint bei dieser Auffassung freilich als ein reiner Formalismus, der durch Belegung der einzelnen Elemente mit Zahlwerthen erst Inhalt und Leben gewinnt."

> It is a fact of the utmost significance for the deeper understanding of mathematics, that one and the same system of symbolism can serve *several* systems of concepts, which, being different in content, show analogies in their form of construction only.[102]

Hilbert would concur with the view expressed here, that in mathematics several theories might be "isomorphic," as one could say—they share the same theory form.[103] Hilbert's conception of a science as schematic would, however, seem to go deeper. It seems that for Hilbert, the ultimate presentation of a science is an axiomatic *and* schematic presentation (cf. Footnote 41 above). Indeed, a preliminary study of what Hilbert says of axioms and axiomatic science in the lecture notes on geometry published in Hallett & Majer (2004) suggests that he saw the process of axiomatization as taking place in two steps: first one singles out according to criteria mentioned in Section 4.1 above a set of judgments which suffices for the construction of the theory at hand; in a second step one then "formalizes," or "de-materializes" the primitive terms of the theory so as to be left with a schema.[104] Only upon completing these two steps will one have reached the ultimate presentation of the theory; hence, Hilbert's call to "axiomatize all other sciences after the pattern of geometry" is a call to present all sciences axiomatically and schematically. One could ask in turn why a schematic presentation was seen as ideal. In response one could, firstly, refer to Hilbert's rejection of elucidation and the consequent conception of the primitive terms as schematic—when the primitives are schematic, then so must be the theory built around those terms. But secondly, although this is mere speculation, it could also be suggested that what underlies this schematic ideal is a view of sciences as mirroring the intellect as an organizing frame for the material of experience;[105] here we would be taking initial steps in a reading of Hilbert through Kant, the further development of which lies outside the scope of this paper.

7.2. The primitives of the Grundlagen der Geometrie. In the famous opening of §1 of the *Grundlagen*, Hilbert introduces the three primitives 'point', 'line', and 'plane' (Hilbert, 1899, p. 4):

> **Declaration.** We conceive three systems of things; we call the things of the first system *points*, and signify them by A, B, C,...; we call the things of the second system *lines*, and denote them by a, b, c,...; we call the things of the third system *planes*, and denote them by α, β, γ,...[106]

These are the most basic of Hilbert's primitives, as they are thought to give domains of objects which form the basis over which the other primitives are to range; if one views Hilbert's primitives as variables, then these three basic primitives may be termed sortal

[102] Husserl (1891, p. 258): "Es ist eine für das tiefere Verständnis der Mathematik höchst bedeutsame Tatsache, daß ein und dasselbe System der Symbolik m e h r e r e n Begriffssystemen dienen kann, welche, ihrem Inhalte nach verschieden, nur in der Bildungsform Analogien aufweisen."

[103] Cf. Husserl (1900, §69).

[104] On this use of 'formalize' cf. Husserl (1913, §13).

[105] Hallett (2008, p. 217) makes a similar suggestion.

[106] Hilbert (1899, p. 4): "E r k l ä r u n g. Wir denken uns drei verschiedene Systeme von Dingen: die Dinge des e r s t e n Systems nennen wir *Punkte* und bezeichnen sie mit A, B, C,...; die Dinge des z w e i t e n Systems nennen wir *Gerade* und bezeichnen sie mit a, b, c,...; die dinge des d r i t t e n Systems nennen wir *Ebenen* und bezeichnen sie mit α, β, γ..."

variables. In the sections that follow, and where Hilbert lays down the axioms of geometry, it is not always made explicit what is primitive and what is or can be defined, but it seems that five more primitives are needed for a faithful representation of Hilbert's theory:[107]

- A and B lie on a (or, A and B are incident1 on a)
- A, B, and C lie in α (or, A, B, and C are incident2 on α)
- A lies between B and C
- $\overline{AB}$ is congruent1 to $\overline{CD}$
- $\angle(h, k)$ is congruent2 to $\angle(h', k')$

In addition, and as has been emphasized by Ferreirós (2009), naive set theory is assumed. Thus the *segment* $\overline{AB}$ is defined as "the system of the two points A and B that lie on a line" (Hilbert, 1899, pp. 6–7).[108] The variables 'h' and 'k' range over what Hilbert calls *half-rays*, which notion is defined as "all the points that lie on one and the same side of a point on the line";[109] although Hilbert does not use set terminology in this definition, it would seem natural to assume, in view of the definition of the notion of segment, that Hilbert does think of a ray as a *set* of points. The *angle* $\angle(h, k)$ is defined as the system of two half-rays starting in the same point O, thus an angle is a set of sets of points. As one sees, the use of set theory is therefore both natural and, unless one uses higher-order logic, necessary for a faithful representation of Hilbert's geometry. On the other hand, the notions of system and thing are not themselves part of Hilbert's geometry, there are no primitives 'a is a thing' or 'A is a system', as there are in Dedekind. Hilbert's geometry is built around the eight primitives just listed, these are the primitives involved in Hilbert's axioms, they define the "subject matter" of the theory.

7.3. The semantics of Hilbert's primitive terms. Thus, in the *Grundlagen der Geometrie* Hilbert does not give elucidations of the primitives, rather he treats them as schematic, namely as variables. This is, however, not to say that he treats the terms as meaningless, that is with no sense at all, on a par with letters arbitrarily jumbled together: xkrqkaaa! We read Hilbert's text and feel certain that we understand it, we can follow its reasonings and accept its theorems. Hence the question naturally arises, what might be the semantic status of the primitive terms of Hilbert's geometry, as that around which this geometry is constructed? Given our claim that these terms are to be viewed as variables, the question in effect reduces to giving a semantics for variables, a task which lies outside the scope of this paper.[110] But I wish to end by discussing briefly one prominent reading of Hilbert's primitive terms, namely as nonlogical constants.[111] The problem with this reading, as I see it, is that the notion of a nonlogical constant allows for at least three very different

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[107] Carnap (1927, p. 369) as well as Bernays (1942) recognize only one relation of incidence and only one relation of congruence, and hence they have six primitives in total. From considering the arity of these relations it seems that one would need two relations of incidence. One could presumably introduce a disjunctive congruence relation, although I would claim that it is more faithful to Hilbert to have two separate such relations.

[108] In the 1898–1899 lectures, the segment $\overline{AB}$ was defined as the aggregate [*Inbegriff*] of all points between A and B (cf. Hallett & Majer, 2004, p. 308).

[109] Hilbert (1899, p. 8): "Die sämtlichen auf ein und derselben Seite von O gelegenen Punkte der Gerade heissen auch ein von O ausgehender *Halbstrahl*."

[110] Semantics of variables have been given by Fine (1985), Tichý (1988), and Breckenridge & Magidor (forthcoming).

[111] This reading was first put forth by Demopoulos (1994), who has been followed by Hallett (1994, p. 163). My disagreement is more a matter of terminology than substance, I think.

interpretations. Only one of these interpretations can be applied in reading the primitive terms of the *Grundlagen*, but that is the interpretation of nonlogical constants as variables; indeed, there is a very natural interpretation of the notion of nonlogical constant that will not fit Hilbert's primitive terms.

To start with the most natural interpretation of the notion of a nonlogical constant, consider what Frege says towards the end of the preface to *Begriffsschrift* on the prospects of applying his ideography to fields of science other than logic:

> It seems to me easier still to extend the domain of this formula language to geometry. One would only have to add some signs for the intuitive relations that are found here.[112]

Thus Frege thinks of his ideography as extended by terms that, firstly, have a fixed sense, namely as standing for the primitive "relations" of geometry, and secondly, are nonlogical, inasmuch as they are neither part of, nor definable from, the primitive symbols of the ideography; hence these terms are naturally called nonlogical constants. It is clear, however, that they are not of a kind with Hilbert's primitive terms, for it is precisely the mark of these primitives that their sense is not fixed.

On the second interpretation of the notion of a nonlogical constant they are the elements of the signature of a formal language; here, a formal language is that kind of inductively generated set which is called language in metamathematics, and which is not really a language at all, but rather a set of mathematical objects. For that reason, neither can the notion of nonlogical constant on this interpretation be used in reading Hilbert's *Grundlagen*. Hilbert intended to express thoughts, but the "symbols" of a formal language in the current sense are not apt for that purpose; such so-called symbols and their concatenations are not expressions but rather a kind of mathematical object; what are called symbols in the technical terminology of metamathematics are not used for the expressing of thoughts, but fall in the domain of objects of metamathematics.[113] This is not to say that we cannot "model" or "formalize" Hilbert's geometry in a formal system—we can very well do that; but that does not mean that the geometry itself is materialized in such a system.

With the third interpretation we reach what I think is the correct description of Hilbert's primitive terms. On this interpretation, a typical example of a nonlogical constant is the '$\circ$' as it would appear in a book on group theory. What kind of symbol is this group-operation symbol? To my mind, it is simply a variable. Since our interest is in Hilbert, I am here assuming that group theory is read schematically in the sense of Section 2.2 above; hence the '$\circ$' is not a bound, but rather a free variable. Now there are at least two possible interpretations of a free variable: the universal interpretation and what I will call

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112 Frege (1879, p. VI): "Noch leichter scheint es mir zu sein, das Gebiet dieser Formelsprache auf Geometrie auszudehnen. Es müssten nur für die hier vorkommenden anschaulichen Verhältnisse noch einige Zeichen hinzugefügt werden."

113 This state of affairs has been beautifully described by Kleene (1952, p. 64):

> Metamathematics must study the formal system as a system of symbols, etc. which are considered wholly objectively. This means simply that those symbols, etc. are themselves the ultimate objects, and are not being used to refer to something other than themselves. The metamathematician looks at them, not through and beyond them; thus they are objects without interpretation or meaning.

The nonsymbolic nature of metamathematical "symbols" has been stressed on many occasions by Sundholm (2002).

the as-if interpretation. In the equation $a + b = b + a$, for instance when taken to express the commutative law for the integers, the letters receive the universal interpretation: the equation is taken to hold for all integers. In a book on group theory, however, I think the group operation symbol typically receives the as-if interpretation; it is "as if" $\circ$ were some specific group operation. Thus the '$\circ$' on a schematic reading of group theory plays the same role as do the capital Greek letters in Frege's presentation of his ideography in the *Grundgesetze*:

> I here use the capital Greek letters as names as if they signified something, though I do not specify the signification.[114]

Or consider the demonstration of a general judgment, that is of a judgment of the form

$$(\forall a \in A)\ B(a) \text{ true.}$$

In such a demonstration one typically sets out by declaring "let $a \in A$!" Until this 'a' gets bound, it is "as if" a is some specific element of A. At the stage where 'a' does get bound, one has, as it were, taken a step back from the as-if reading and passed on to the universal interpretation of the variable; this would seem to be what we do after declaring "but a was arbitrary" and before applying universal introduction. Thus Frege used three kinds of variables in the *Grundgesetze*: capital Greek letters, requiring an as-if reading; latin letters, requiring a universal reading; and fraktur letters for bound variables.

Frege in his interpretation of Hilbert's geometry as an *allgemeiner Lehrsatz* (Frege, 1906, p. 380) treats the primitives as latin letters. I would be inclined to treat them as variables with an as-if interpretation. But it may not matter much which interpretation one chooses, for one can easily pass back and forth between the two; indeed, as the consideration in the previous paragraph of the demonstration of a universal judgment suggests, it might even be necessary to make use of both interpretations. When we are working through Hilbert's *Grundlagen*, when we live (as Husserl might have said) in its definitions, theorems, and demonstrations, the primitives have an as-if character. In a reflective attitude we then pass to their universal interpretation, and what results is an *allgemeiner Lehrsatz*: it is a conditional whose antecedent is the conjunction of the axioms, whose consequent is the conjunction of the theorems, and in which all the primitive terms are substituted by latin letters, that is by free variables requiring the universal interpretation. If we are to continue the development of the geometry, however, we have to go back to the as-if interpretation, we have to think of the primitives "as if" they signified something specific.

However that may be, as variables, on the as-if as well as on the universal reading, the primitive terms are indeed seen as having a merely formal sense. Grasping the signification of such a variable requires grasping its grammatical category—is it a unary predicate variable, a binary relation variable?—as well as its range of significance—of what kind can the property, relation, object be that is signified by a possible substituent for the variable?—but this would seem to be all one needs to grasp in order to grasp the primitive terms. As such the primitive terms may be viewed as grids open for various materializations, for various ways of filling with material content, and their meaning itself may be termed formal.

[114] Frege (1893, p. 9, footnote): "Ich gebrauche hier die grossen griechischen Buchstaben als Namen so, als ob sie etwas bedeuteten, ohne dass ich die Bedeutung angebe."

§8. Acknowledgements. For comments on earlier drafts, discussion, and encouragement, I wish to thank Michael Hallett, Dirk Schlimm, and Göran Sundholm. Several improvements in the final draft were instigated by suggestions of the referee, to whom I am also grateful.

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