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The controversy between Frege and Hilbert

Resumen: *En este artículo enfocaré mi atención en dos aspectos del debate entre Frege y Hilbert sostenido alrededor de 1900 acerca de los fundamentos de la geometría: a) la diferencia entre axiomas y definiciones, b) las pruebas de independencia de los axiomas. Acerca del primer punto, sostendré que Hilbert y Frege tenían diferentes concepciones de 'concepto' y que la posición de Hilbert es problemática. Más tarde, exploro la interpretación de Patricia Blanchette de que las objeciones de Frege a las pruebas de consistencia e independencia de Hilbert surgen de sus diferentes nociones de consecuencia lógica. Sugeriré que esta interpretación no es correcta, incluso si coincide con la mayor parte de la evidencia textual, porque da lugar a ciertas consecuencias desconcertantes.*

Palabras clave: *Definiciones. Prueba de independencia y de consistencia. Conceptos. Consecuencia lógica.*

Abstract: *In this paper, I shall focus my attention on two subjects of the debate between Frege and Hilbert held around 1900 concerning the foundations of geometry: a) the difference between axioms and definitions, b) the proofs of independence of axioms. Concerning the first point, I will hold that Hilbert and Frege had different conceptions of 'concept' and that Hilbert's position is problematic. Later, I shall explore Patricia Blanchette's interpretation that Frege's objections to Hilbert's proofs of consistency and independence arise from his different notions of logical consequence. I will suggest that this interpretation is not correct, even if it fits well with most of the textual evidence, because it gives rise to certain puzzling consequences.*

Key words: *Definitions. Proof of independence and consistency. Concepts. Logical consequence.*

0. Introduction

In 1899, Frege read a monograph of Hilbert's lectures on Euclidian Geometry, which, later on, would be the basis for Hilbert's *Grundlagen der Geometrie* (1899). In December of the same year, Frege wrote a letter to Hilbert criticizing some aspects of his work. They had a brief correspondence of only six letters; however, Frege continued the discussion of the topics in some of his essays. Hilbert's *Foundations of Geometry* has been considered paradigmatic in the sense that it sets out a blueprint for mathematical practice in the XX century. *Foundations of Geometry* is also seen as an example of the use of the modern axiomatic method. For this reason, Frege's comments and objections to this work seem totally anachronistic or inappropriate. Unquestionably, many contemporary readers will conclude, along with certain critics, that Frege was not able to understand the transformations mathematics went through, which later would take form into Hilbert's work. Recently, Frege's texts have been interpreted so as to try to explain and understand his point of view.

Throughout this text, I will focus my attention on two main aspects of this debate:

a) the difference between axioms and definitions,
b) the proofs of independence of axioms.

Regarding the first one, I will uphold the view that Hilbert and Frege had different notions of what 'concept' meant. I will also show that Frege's definitions are not only abbreviations and that Hilbert's position is problematic. Later on, I shall explore two interpretations of Frege's objections to Hilbert's proofs of consistency and independence. The first interpretation, which I think is neither adequate nor satisfactory, was proposed by Tappenden. In the second interpretation, by Blanchette, Frege's objections point to a different notion of logical consequence. I believe the last interpretation is the most appropriate since it fits better with most of the textual evidence. Nonetheless, it also creates certain puzzling consequences. Therefore, I recommend extending this interpretation so as to adjust it to the textual evidence.

1. Axioms vs definitions

1.1. Frege's criticism

Commenting Hilbert's *Grundlagen der Geometrie*, Frege wrote: "Here the axioms are made to carry a burden that belongs to definitions. To me, this seems to obliterate the dividing line between definitions and axioms… I should like to divide up the totality of mathematical propositions into definitions and all the remaining propositions (axioms, fundamental laws, theorems). Every definition contains a sign (an expression, a word) which had no meaning before and which is first given a meaning by the definition… a definition does not assert anything… The other propositions must not contain a word or sign whose sense and meaning, or whose contribution to the expression of a thought, was not already completely laid down, so that there is no doubt about the sense of the proposition and the thought it expresses" (PMC, 35-36). Frege understood the axiomatic method traditionally: definitions are not assertions. They introduce only one new symbol specifying its sense and reference. Axioms should be evidently true propositions. The proof transmits the truth from the axioms to the theorems, and, therefore, no contradiction can be derived within the system. In a schematic formula, we might say that the meaning of the terms precedes the truth of the axioms, and the latter precedes the consistency of the system.

Apparently, for Frege, the only use of a definition in an axiomatic theory (that is to say, within a formal system) is to introduce an abbreviation. In Frege's words:

> We introduce a new name by means of a definition by stipulating that it is to have the same meaning and the same denotation as some name composed of signs that are familiar. (Frege, 1893, 82)

In this sense, definitions are superfluous:

> In fact, it is not possible to prove something new from a definition alone that would be unprovable without it. (PW, 208)

I will present two reasons why these quotes should not be understood literally. On one hand, as it has been pointed out by Boolos and, more recently, by Landini in his formal systems, Frege actually takes a rule of substitution that equals to the introduction of the understanding of axioms. These axioms allow two things. The first is the creation of conceptual terms by replacing constants for variables in terms or in already-finished sentences. The second is that the concepts denoted by formulas built in this way instantiate quantifiers of the same kind. Thanks to these two operations, definitions on conceptual terms turn out to be creative (in a way). On the other hand, I will explain why Frege should not have tolerated definitions that were just abbreviating devices.

Concerning the first point, let's look at an example. I will use Boolos' notations to simplify Frege's derivations. Let's '([x|Fx])' denote the concept denoted by the formula 'Fx'. The next are some of the definitions found in *Begriffsschrift*:

(1) "Her_R (F)" is defined as "(∀x)(∀y)((Fx∧xRy)→Fy)" (B. 69). Frege's notation is of a more complicated typography.
(2) (B.77) "xR*y" is defined as "(∀F)((Her_R(F)∧(∀z)(xRz→F(z)))→Fy)"

Frege proves the next propositions:

(3) (B.84) (Her_R(F)∧Fb∧bR*c)→F(c).

It is easy to imagine the proof of the previous proposition. Given ‘bR*c’ it can be seen that c’s denotation must have the hereditary properties that b’s children have. Particularly, the one represented by ‘F’ is a hereditary property that ‘b’s denotation has, therefore, its children also have it. If someone wants to use this proposition for a particular ‘F’, the letter’s denotation substituting F must the power to instantiate the second quantifier implied by ‘bR*c’.

(4) (B.96) ((aR*b∧bRc)→aR*c).
(5) (B.97) Her_R([x|aR*x]). The proof is by generalization of B. 96 and substitution of [x|aR*x] in the definition of Her_R.

Note how, if the abbreviation introduced by (1) is eliminated, B.97 is the generalization of B.96. Until now the definitions have been resources for abbreviation. Let’s regard the next proposition:

(6) (B.98) (∀x)(∀y)((aR*x∧xR*y)→aR*y). The proof is by substitution of “F” by “[x|R*(a,x)]” in B.84.

Could we regard now the definitions as introducing abbreviations? It seems possible to regard B.84 and its proof as a mere scheme to be applied to the formula [x|aR*x] (i.e. (∀G)((∀u)(∀v)((((Fu ∧uRv)→Fv)∧(∀z)(aRz→G(z)))→Gx))), but we need to be cautious. This term must instantiate a second’s order quantifier (hidden in bR*c) and for this instantiation to be valid, it is necessary to first apply a principle of comprehension. This second order’s quantifier cannot be placed at the beginning of the formula where it has as its scope all the formula. In this sense, a definition contains an assertion. It is not quite true that the definitions are superfluous in the proof.

We can also observe in B.97, or to be more exact in B.98, that Frege used impredicativity. He defined the property of being an individual R-descendant’s as depending on the hereditary properties of their children (immediate descendants). In B.96 he proved that children descendants are descendants too. We can say he proved that the property of being descendant is hereditary. However, this is a way to look at this matter. On the other hand, in the proof of B.98, the implicit comprehension axiom generates the property of being-descendant-of-a and, when instantiating the hidden quantifier in bR*c, it results in being one of the hereditary properties that b’s children have. However, the impredicativity was already implicit in the definition B.77. If the property of being a’s descendant was not one of the hereditary properties that a’s children have, it could happen that the definition did not capture the intuitive property of being descendant. In that case, an individual having the hereditary properties of the children could exist without being a’s descendant.

Let’s analyze a second thing: could Frege accept definitions that were not merely abbreviations? Let’s suppose that for different purposes in arithmetic construction we introduce Hume’s principle: N[xIFx]=N[xIGx]$=_{def}$ [xIFx]eq N[xIGx] We could have equations like N[xIFx]=N[xIGx] and, leaving behind its origin, Frege’s symbolic rules would allow us to recreate ‘N[xIFx]=y’ formula including the variable ‘y’. With the help of the comprehension principle, we have created a concept. For example, if we wondered whether Julius Caesar (or the real object) is subsumed by this concept, then the question makes sense. We have created a question to which we cannot give an answer. In this way, the principle allows us to make new concepts by substituting constants for variables, and it should be restricted. It makes more sense, and it is easier, to prohibit definitions with simultaneous equations, which we will not be able to transform in explicit definitions. In other words, Frege should not tolerate the introduction of symbols through definitions that are simply abbreviations, since the comprehension principles should not be applicable to them.

1.2. Hilbert’s conception of axioms as definitions

In his geometry, Hilbert gives a list of undefined concepts: *point*, *line*, *plane*, *lie on* (a relation between a point and a line), *lie on* (a relation between point and plane), *betweenness*, *congruence* *of pair(s) of points*, and *congruence of angles*. The first axioms are:

(1) To each two points A and B there is a line *c*, which lies on A and B.
(2) To each two points A and B there is no more that one line *c*, which lies on A and B.
(3) On each line there are at least two points. There are at least three points, which do not lie on one line.

In answer to Frege's criticism, Hilbert wrote:

> This is apparently where the cardinal point of the misunderstanding lies: I do not want to assume anything as known in advance; I regard my explanation in section 1 as the definition of the concepts point, line, plane –if one adds again all the axioms of groups I to V as characteristic marks. If one is looking for other definitions of a 'point'… then I must indeed oppose such attempts in the most decisive way; one is looking for something one can never find because there is nothing there, and everything gets lost and becomes vague and tangled and degenerates into a game of hide-and-seek. (PMC, 39)

> You say that my concepts, e. g. 'point', 'between', are not univocally fixed… But it is surely obvious that every theory is only a scaffolding of concepts or scheme of concepts with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. If in speaking of my points I think of some system of things, e. g. the system: love, law, chimney-sweep… and then assume all my axioms as relations between these things, then my propositions, e. g. Phytagoras' theorem, are also valid. In other words: any theory can always be applied to infinitely many systems of basic elements. (PMC, 40-1)

For Hilbert, axioms, if they are consistent, 'implicitly' define the terms that appear in them and, in that sense, cannot be false. The objects or concepts to which a consistent system refers exist. In schematic form: the consistency of the system precedes the existence of the objects (or concepts) to which its terms refer, and the truth of its axioms and theorems. The divergence between both thinkers in this respect is plainly revealed in the following paragraph from Hilbert:

> You write: …'From the truth of the axioms it follows that they do not contradict one another', … I have been saying the exact reverse: *if the arbitrarily given axioms do not contradict one another* with all their consequences, then they are true and *the things defined by the axioms exist. This is for me the criterion of truth and existence*". (PMC, 39-40)

1.3. Frege's way to understand axioms as definition

There are at least three ways of describing an object or concept and therefore there should be at least three types of definitions: the explicit definition, in which the term to be defined appears on its own in the *definiendum* (as ordinarily happens in a dictionary) and it is the only term whose meaning is not known before the definition; the contextual definition, which shows us (at least immediately) how to eliminate the term (or the terms) to be defined only when it (or they) appear(s) in certain contexts; and the implicit or axiomatic definition, which is a series of sentences that define several terms simultaneously in context. Frege compares the axiomatic definition with a series of simultaneous equations. To continue the analogy, the following are respectively examples of these three classes of definitions:

1. $\alpha=3+5$.
2. $\sqrt{\alpha}=2$.
3. $\alpha+\beta=9$, $\alpha\beta=18$.

The second case would also include an equation simultaneously defining two terms (just as with Hume's principle). Frege has nothing against the first type of definition. On a side note, if a singular term is being defined, then there will not be any variables on the right side. Were there to be a functional term, on the other hand, then there will be as many free variables on both sides, as seen in the definition of the relation of being the ancestral of. In the second case scenario, the implicit definition, Frege seems to suggest that it will remain valid as long as it can be reformulated as form 1. Likewise, it will remain valid if α appeared on both sides of the identity symbol. In the event that it could not be transformed in an instance of a case 1, then we would be facing the Julius Caesar problem, were a singular term to be defined. There would not be any way to determine, for some given objects, whether they are the defined objects. Nonetheless, there is a different approach to understanding the Julius Caesar problem, as we will see next.

Frege seems to be against any definition other than the type 1. He says that there should be only one symbol or expression introduced by the definition each time. And he addresses to Hilbert the next objection:

> Given your definitions I do not know how to decide the question whether my pocket watch is a point. (PMC, 45)

If we think that we are defining first order concepts, their extensions are not well determined. But, for Frege, there is no objection if we regard Hilbert's axiomatic system as a definition of a relation of higher level. For instance, take the following set of 'axioms':

$$\forall F[(\forall x\forall y((Fx\wedge xRy)\rightarrow Fy)\rightarrow(Fa\rightarrow Fb)].$$
$$\forall x\forall y(Rxy\&Rxz\rightarrow z=y).$$

These define a relation S between two objects and a first-order relation (a,b,R), which is a function (as understood today) and the first object is an R-ancestor of the second one. It does not make any sense to ask whether an actual given object is the one defined here. Hilbert makes a mistake in thinking that a first-order relation, or a set of first-order relations, has been defined. To Frege, there is only a symbol (or a simple expression) being introduced with each definition. We could transform the aforementioned set of axioms into a Frege-like definition as follows:

$$S(a,b,R)\Leftrightarrow_{Df.}\forall F[\forall x\forall y((Fx\wedge xRy)\rightarrow Fy)\rightarrow(Fa\rightarrow Fb)]\wedge\forall x\forall y(Rxy\&Rxz\rightarrow z=y),$$

where 'S' is the sole symbol being defined, and where 'a', 'b' & 'R' are variables corresponding to 'S' functional nature.

This attempt at minimizing the differences between Hilbert and Frege is not meant to override them, as we will see. I would like to raise a point before doing so: Frege was against the recursive definition of number given by the following equations:

$$N[xIFx]=0\Leftrightarrow_{df.}\forall x\sim Fx.$$
$$N[xIFx]=n+1\Leftrightarrow_{df.}\exists y\ (Fy\wedge N[xIFx\wedge x\neq y]=n).$$

He did so for two reasons. Firstly, we would face the conundrum as to whether Julius Caesar is a number. Secondly, numbers must be objects as opposed to second-order properties. They both may seem intertwined because, if the number has not been defined as a first-order concept, then the question as to whether Julius Caesar is a number will evidently be rendered pointless. However, Frege's objection does not refer to the lack of meaning of the question, but rather to the failure to determine a concept. Both objections could be distinguished by considering the first one as the claim that the defined concept has not been well framed, even as a superior-order concept. Nevertheless, with recursive definitions, like the aforementioned one, this problem could have been solved by turning them into explicit definitions. About the previous definition, we can go in the following fashion. Two new definitions are introduced:

$$0[xIfx]=_{df.}\forall x\sim fx.$$
$$NSM=_{df.}\forall f\forall g(N[xIfx]\wedge M[xIgx])\rightarrow\exists z(Mz\wedge([xIFx]\text{ equinum }[xIgx\wedge\sim x=z])).$$

Lastly, a third-order concept is defined:

$$Num[H]=_{Df.}\forall F((F0\wedge\forall N\forall M(((F[N]\wedge NSM)\rightarrow F[M])\rightarrow F[H]))))).$$

The third-order concept defined as [XINUM[X]] would be well framed. If so, the objection that numbers are not objects is the only one that remains.

Hume's principle (as defined) has been objected to as having the same shape as the infamous

Law V; and Frege –the objection goes– must have adopted the same approach in either case. If he rejected HP, why did he accept the Law V? Without delving further into that discussion, it suffices to note that there are good reasons to not consider the Law V as a definition. The first reason is that it does not seem to be a definition. The second reason is that both conditionals, which it is made up, bear a very different value. There might be, in turn, some arguments to support this claim. The first is that Frege's semantics, once the references for the statements and for the singular terms are determined, forces us, by the principle of composition, to acknowledge that a conceptual term is extensional in nature. If the reference of a conceptual term is a concept, then if the same objects fall under each of two concepts (say, first-order ones to simplify the example), they must have the same extension simply because they are 'equal' (given we are dealing with a first-order relation, we should not say 'equal' strictly speaking). Thus, this property could be postulated by means of axioms of comprehension. As Landini says, Frege has a problem, for he does not have the means to express every axiom of comprehension in a single formula. The second argument, which further reinforces the first one, shows that, confronted with the discovery of Russell's paradox, Frege only gives up one of the conditionals from the Law V.

1.4. Hilbert's position

Nonetheless, can we say Hilbert is wrong when he thinks that he has defined first-order concepts (*dot*, *line*, and so on)? Although Frege may have tailored Hilbert's perspective to suit his own, this is not merely a verbal disagreement. Underlying this, there is a different understanding of concepts, one which can be clearly seen in the following quotes:

Frege:

> I demand from a definition of a point that by means of it we be able to judge of any object whatever –e.g. my pocket watch– whether it is a point. (FG1, 63)

Hilbert:

> As I see it, the most important gap in the traditional structure of logic is the assumption made by all logicians and mathematicians up to now that a concept is already there if one can state of any object whether or not it falls under it. This does not seem adequate to me. What is decisive is the recognition that the axioms that define the concepts are free from contradiction. (PMC, 51-52)

To Hilbert, the question as to whether a couple of real numbers is a dot is meaningless *per se*, as opposed to comparing those against an 'interpretation' which provides meaning to all basic terms in geometry simultaneously. This is a more radical standpoint than it seems. In a nutshell, let us assume a set of axioms that simultaneously define 'dot', 'line', 'being at' (a relation between a line and a dot). For Frege, we have defined a higher-order S relation, which subsumes threesomes made up of two first-order concepts and a first-order relation. Thus, the *dot*, *line* and *being at* in Euclidean geometry fall into this general relation. Likewise, under an interpretation of Hilbert's axioms, *ordered real number pairs*, *first-order equations*, and *(x,y) being a solution of the first-order E equation* are two concepts and a relation that fall under S. There are, in Frege's motley universe, for instance, the general relation S, the concepts acting as first parties in this relation, and a second-order concept that comprises the latter two, none of which are the *dot* concept that Hilbert defines. This is a first-order concept that only comprises dots in Geometry but only from a special conceptual perspective. Julius Caesar may or may not be the number two depending on whether it is considered as part of a structure with given characteristics. Hilbert's concept does not exist in Frege's universe.

Hilbert seems to be adopting the position known as 'structuralism'. According to Shapiro, this perspective has three versions: a) eliminative structuralism, or *in re* structuralism, according to which, the mathematician studies structures that are common to several systems, but without supposing that these structures are something different from, at a higher level, the systems that exemplify them. It is a structuralism without structures; b) *ante rem* structuralism, according to which the mathematician studies structures independently of whether there are or not systems that satisfy them; and c) modal structuralism. Again, it is a structuralism without structures. The difference is that the systems that instantiate these structures don't have to be real, they need only to be possible. Considering only the few paragraphs he wrote on the topic, it is very difficult to attribute to Hilbert any of these positions. For instance, when he says:

> all the statements of the theory of electricity are of course also valid for any other system of things which is substituted for the concepts magnetism, electricity … provided only that the requisite axioms are satisfied (PMC, 41) [,]

he seems to adopt an eliminative structuralism. But when he says that the things defined by consistent axioms exist, he seems to adopt *ante rem* structuralism. In any case, every brand of structuralism has its own problems. This is obvious for modal structuralism, a posture difficult to be attributed to Hilbert. For *in re* structuralism, the problem is that the systems that instantiate the structures described by axiomatic systems must belong to a background ontology. First, this ontology must be huge because if not, mathematical statements can be vacuously true and, second, must be described in a non-structuralist way. For instance, set theory (the paradise created to us by Cantor) could provide this background ontology. But in that case

> set theory itself is not treated structurally: its axioms are not understood as defining conditions of structures of interest but are taken as assertions of truths in an absolute sense. (Hellman, 2007, 540)

This objection has a Fregean flavor. In the end, we need axioms to be true assertions about objects which cannot be treated in a structuralist way.

If we accept *ante rem* structuralism, another problem is lurking around the corner. To see it I follow Hellman:

> what, for example, can it mean to speak of "the ordering" of "natural numbers" as objects of an ante rem structure unless we already understand what these numbers are apart from their mere position in that ordering? Surely the notion of "next" makes no sense except relative to an ordering or function or arrangement of some sort, something Dedekind was careful to take into account when describing simply infinite systems, which always involve objects "set in order by transformation f" (clearly, anything whatever can be "next after" anything else in some system or other). Thus the notion of an ante rem structure seems to involve a vicious circularity; such a structure is supposed to consist of purely structural relations among purely structural objects, but understanding either of these requires already understanding the other. (Hellman, 2007, 545)

From a historical point of view, it is difficult to attribute to Hilbert a concern for the difference among these kinds of structuralism. With these brief remarks, I would like only to suggest that every option has its own problems.

2. Consistency and independence

2.1. Hilbert's proofs of relative consistency

As it is well known, in his address of 1900 to the International Congress of Mathematicians in Paris, Hilbert outlined twenty-three major problems to be studied in the coming century. The second of these problems was to prove the consistency of arithmetical axioms. The solution of this problem would be the culmination of a series of proofs of independence and consistence for sets of geometrical axioms. Hilbert had used the technique of reinterpretation of the non-logical terms of an axiomatic system. In this way, as he wrote:

> Any contradiction in the deductions from the geometrical axioms must thereupon be recognizable in the arithmetic of this field of numbers. ("Mathematical Problems", in Ewald, 1999, 1104)

In this way, the consistency of arithmetical axioms implies the consistency of geometrical axioms, but the first one cannot be proved in a similar way:

> I am convinced that it must be possible to find a direct proof for the compatibility of the arithmetical axioms. (*Ibid*, 1104)

2.2. Frege's first objection: circularity existence-consistence

Frege presents at least two objections to the proofs of logical independence or consistency proposed by his opponent. The first one is that Hilbert proves the logical consistency of groups of axioms by appealing to the existence of certain models, but, as we have seen, he also defines existence in terms of consistency.

> Is there some other means of demonstrating lack of contradiction besides pointing out an object that has all properties? If we are given such an object, then there is no need to demonstrate in a roundabout way that there is such an object by first demonstrating lack of contradiction. (Frege, PMC, 47)

Frege was right. Hilbert did not know how to prove consistency in an absolute way. Some years later he devised a proof of syntactical consistence, one of the main ideas of his formalistic program. Would Frege have accepted this kind of proofs? As we will see, the answer is no.

2.3. Frege's second objection

Frege's second objection is much more enigmatic:

> If you are merely concerned to demonstrate the mutual independence of axioms, you will have to show that the non-satisfaction of one of these axioms does not contradict the satisfaction of others (I am here adopting your way of using the word 'axiom'.) But it will be impossible to give such an example in the domain of elementary Euclidean geometry because all the axioms are true in this domain. *By placing yourself in a higher position from which Euclidean geometry appears as a special case of a more comprehensive theoretical structure*, you widen your view so as to include examples which make the mutual independence of those axioms evident... And indeed the mutual independence of the axioms, *if it can be proved at all*, can only be proved in this way. *Such an undertaking seems to me to be of the greatest scientific interest if it refers to the axioms in the old traditional meaning of elementary geometry.* If such an undertaking extends to a system of propositions which are arbitrarily set up, it should *in general* be of far less scientific importance. Whether it is possible to prove the mutual independence of the axioms of Euclidean geometry in this way, I dare not decide, because of the doubt I indicated above. (PMC, 44)

2.4. Why Frege's objection is so enigmatic?

To better understand his disagreement, Frege needs to be quoted yet again:

> Given that the axioms in special geometries are special cases of general axioms, one can conclude from the lack of contradiction in a special geometry to the lack of contradiction in the general case, but no to the lack of contradiction in another special case. (PMC, 48)

My understanding is that Frege means by 'general axioms' the partially-interpreted propositions that were postulated as axioms in Hilbert's geometry; and by "special geometry", the sets of propositions resulting from these axioms being interpreted, that is, when their schematic terms are replaced by suitable terms from the same grammatical category. Thus, for instance, in the general axiom 'for every two dots, A and B, there is a line, C, such that A and B are on C', where'dot' and 'line' are schematic terms. When 'dot' and 'line' are understood as referring to the dot and the line in the Euclidian geometry, what we get is a special geometry. Or we may understand these terms as a pair of real numbers and a linear equation. Doing this gives us another special geometry. Frege shows that consistency of a special geometry implies consistency of the general axioms, but this property is not transferable to another special geometry; a remark that seems to contradict the normal understanding of logic consistency.

Frege seems to criticize Hilbert for taking schemes of propositions as *relata* of logical relations. What is important in his view is to demonstrate the mutual independence of axioms in the traditional meaning of 'axioms', that is to say, as propositions (thoughts, in Frege's sense of this word). But even in that case, the logical relations among propositions are formal. First, because the implicit rules of axiomatic method establish that, once the axioms are postulated, the mathematician cannot use another property of the objects involved except those consigned in the axioms. This is clear for the modern axiomatic method. But even in the Antiquity, according to Kline (Kline, 1990, 1005), Euclid was criticized by assuming, in the proof of his first proposition, something that was not established in the axioms. As Tarski wrote:

> our knowledge of the things denoted by primitive terms… is by no means exhausted by the adopted axioms. But this knowledge is, so to speak, our private concern which does not exert the least influence on the construction of our theory. (Tarski, 1995, 121-122)

In that sense, the subject matter of an axiomatic theory is the meaning of the terms as it is established by the axioms. Second, the logical "must" incorporated in the Aristotelian definition of 'logical consequence' has been explained in terms of invariance of truth under any substitution of the non-logical vocabulary of a sentence. Let's call this definition or explanation of logical truth the substitutional explanation. It was implicit in Aristotelian proofs of logical independence, in Bolzano's or in Quine's definition of logical truth (Quine, 1982, 4). If S is a set of sentences and E a sentence and there is no any substitution of the non-logical terms of S and E that turn out all the sentences of S true and E false, I will say that E is substitutional consequence of S. If it is not the case, I will say E is substitutionally independent of S. Frege seems to hold a different view of logical consequence.

2.5. New data

The problem becomes more severe because, on the 1906 essay on geometry fundamentals, Frege outlines a method to prove the independence of axioms, where he explicitly defines 'independence' as follows:

> Let S be a group of true thoughts. Let a thought G follow from one or several of the thoughts of this group by means of logical inference such that apart from the laws of logic, no proposition not belonging to S is used. Now, let us now form a new group of thoughts by adding the thought G to the group S. Call what we have just performed a logical step. Now if through a sequence of such steps, where every step takes the result of the preceding one as its basis, we can reach a group of thought that contains the thought A, then we call A *dependent* on the group S. If this is not possible, we call A *independent* of S. The latter will always occur when A is false. (CP, 334)

The method outline:

> Let us now consider whether a thought G is dependent on a group of thoughts S. We can give a negative answer to this question if… [after a suitable substitution of the non logical vocabulary by terms of the same grammatical category], to the thoughts of group S correspond a group of true thoughts S' while to the thought there corresponds a false thought G'. (CP, 338)

To these data can be added what Tappenden calls the Jourdain statement, namely, "that the axiom of the parallels cannot be proved" (PMC, 183), written by Frege in 1910. How can we reconcile Frege's several assertions?

3. The interpretations

We can now move on to some interpreters' comments. An option we will reject consists in assuming that either Frege was not aware of, or did not understand, the commonplace methods for proofs of independence in the geometry of his time. Take, for instance, H. Scholz's interpretation:

> it is an undeniable fact that, while Frege himself set the cornerstones of the classical understanding of geometry, he was unable to grasp Hilbert's radical transformation in the conception of geometry. The result of his critiques, sharp and worthy of reading today [1969] though they are, must essentially be considered as unfit. (Scholz, 1969, 222)

Yet another interpretation is Tappenden's. He draws attention to the vast knowledge that Frege had of the geometry of his time. According to this author, Frege highlights two approaches to the arguments of independence, and his intriguing observations can only be applied to one of them. They cannot be used to support Frege's alleged rejection of the metatheory. Tappenden dedicates a great part of his paper to show that the Jourdain statement should not be taken so seriously.

During his exchange of letters with Hilbert, Frege would have conceived independence as

> the non-satisfaction of one of these axioms does not contradict the satisfaction of others (see PMC, 43) [,]

a statement to which the proofs of independence by Hilbert will be associated. Why Frege was against this conception? He did so because such a conception would imply considering an axiom as being false. This, in turn, can be explained by the fact that Frege considered the modality as being external to the content of a judgment and the axioms as obviously true, maybe because he presented an apparent opposition to the way to settle relations of logical independence between sentences, one of which might be false. Let's see each step in turn.

Let's redraft what was said under the first hypothesis: the method to prove the independence of an axiom E from a group of axioms S will be the consideration of the possibility that every sentence in S is true and E is false. However, it would be useless to even mention that possibility because either modalities would be external to the judgement or simply because it would be absurd to consider false an axiom. According to the author of the *Grundlagen*, saying that I am considering a proposition E as possible would only confirm that I have not enough reasons to ensure not-E. Even if this modality is external to the judgment, an axiom is obviously true and we have a very solid justification to ensure it is so because there is no reason to say that it might be false. Tappenden believes that the limited textual evidence in Frege's work on this matter does not allow us to attribute to him a position that supports a rejection of the independence proofs. I do agree. We even can find places where Frege points in the opposite directions. For instance, on one side, it looks as if we cannot think of an axiom as being false:

> I can only say: so long as I understand the words 'straight line', 'parallel', and 'intersect', I cannot but accept the parallels axiom. (PW, 247)

But we can read in *Grundlagen*:

> Under the conceptual thought we can always think opposite of this or that geometric axiom without actually being contradictory with oneself. (Gr, paragraph 14)

This statement is part of a very important argument because it allows the separation between geometry and arithmetic based on the difference in source of the knowledge we have of those disciplines. I believe that Tappenden is right. It does not seem to be the point.

Let us analyze the second hypothesis. According to what it says, Frege is reluctant to consider an axiom as false to prove that "the non-satisfaction of one of these axioms does not contradict the satisfaction of others" because that would mean that a false thought has logical consequences or that from a false thought we can infer something else, and that would be unacceptable. There are plenty of fragments in his work that seem to support this interpretation. An example is the following quote:

> if a group of propositions has a proposition whose truth is not yet proven or is certainly false, this proposition cannot be used to make any inferences. (1960)

To any contemporary logician, this might seem a surprising idea. Tappenden refers to Mancosu's study on the indirect proof in order to show that this idea was not as strange in the XIX century as it is now.

Mancosu analyzes the distinction Kant makes between philosophy and mathematics related to their methods and, specially, to the proof of reduction to absurdity, which is valid for the latter, but not for the former. This proof may roughly be characterized as the one that "starts from assuming as a premise the negation of the proposition to be proved" to derive a false statement from this, either an absurd or a contradiction, in order to conclude the rebuttal of that premise. Among the several objections pointed out by Kant against this type of proof, we have the following:

> the apagogical proof, on the other hand, while it can indeed yield certainty, cannot enable us to comprehend truth in its connection with the grounds of its possibility. The latter is therefore to be regarded rather as a last resort than a mode of procedure which satisfies all the requirements of reason. (Kant, *CPR*, B817)

The reduction to absurdity produces conviction but does not show the grounds of the proven proposition besides the fact that, according to Kant, it has the disadvantage of this being invalid under cases such as the antinomies of reason where a proposition and its denial leads to absurd. Even though he was not an advocate of distinctions between philosophy and mathematics methods, Bolzano objected the apagogical proofs and tried to demonstrate that they can always be transformed into direct proofs. We must notice that if there was a roughly algorithmic method of transforming an indirect proof into a direct one, as Bolzano pretended, then the objection against the first type of proofs would come from a lack of perspicuity rather than the possibility of creating a fallacy out of them. On this thought, the objection Bolzano stated against this type of proofs is weaker than the one Kant stated.

There is a fragment in a Mancosu's paper where he summarizes a comment Frege did to a Schönflies text. There, Schönfles points out to something similar to what Kant says about indirect proofs. Schönflies ascribes paradoxes such as Rusell's to the application of classic logic to contradictory concepts. To Frege, there is no mistake in using (terms for) contradictory concepts with the exception of the case in which we form singular terms with them. As we know, several *Grundgesetze* paragraphs are dedicated to argue that all the names already introduced have denotation.

Nevertheless, the quote we mentioned before suggests that Frege opposed not only to the indirect proof but also to any proof with a false premise or even with a premise whose truth has not been proved yet. For instance, Dummet says that

> Frege… consistently rejected the legitimacy of deriving consequences from a mere supposition: all inference must be from true premises. (Dummett, 1991, 25-26)

Not every proof with a false premise is an indirect proof. Let us remember the 1906 definition of logical independence of a proposition E in relation to a group S of propositions. It only encompasses the scenario where all of the S elements are true. Frege's view seems to be that false propositions do not have logical consequences. I do not really think this is the case and his observations about this case are mainly pragmatic.

According to Frege, a proof has functions other than generating conviction in the truth of a theorem. *Grundlagen* proofs were supposed to show not only the truth of arithmetic sentences, but also the basis for their truth. Nevertheless, to Frege, the important part is that the proof uses the most general premises possible. The proof of a geometric proposition that uses the laws of physics does not show its real base. Likewise, the real character of a geometric proposition is not revealed by a proof that uses geometry.

For instance, it does not seem that the author of the *Begriffsschrift* wanted to do any distinctions between different logical proofs of an arithmetic proposition. I think that the attribution of a concern for a distinction between proofs that show the basis of truth and proofs that do not (considering the exceptions made), as Bolzano had, oversteps the textual evidence. As I said, he is not worried about the use of contradictory concepts, except when they are used to form singular terms (topic not discussed by Hilbert).

I believe Hodges' explanation to be more credible: in his remarks on the conditional proof, Frege tries to separate the logical part from the epistemological one.

> Yet you may ask, can't we conclude a statement from a [E] sentence, which may be false, to see what we get if it is true? Yes, somehow it is possible… [but] the condition "If E is the case" is retained in every moment. (PW, 244-245)

In his systems, there is nothing like a conditional proof and when concluding from a hypothesis we must not forget that every one of them is a fragment of a conditional sentence. This is particularly relevant for the axiomatization under the Hilbert method. According to Frege, the by Hilbert's apparent theorems are only pseudo-propositions. There is a double grammatical dependency of the letters present on them: some of them are implicit variables bounded inside the proposition, others are linked to their presence in the axioms. Thus, theorems are always conditional propositions. Several of their grammatical elements remain implicit. If this interpretation is correct, Frege's remarks on the indirect proofs are not surprising anymore but will not explain his objections to Hilbert's independence proofs.

Despite its merits, Tappenden's interpretation faces several objections. One of them is that the association of a so-called notion of logic independence, the one codified under the words "that the non-satisfaction of one of these axioms does not contradict the satisfaction of others", with Hilbert's proofs is not clear. Hilbert does not consider the possibility of an axiom (a Fregean one) being false. Nor his proofs draw consequences from a false sentence. In any case, Frege is only arguing against a slob mode of formulating the matter where axiom schemes (or general axioms) are called axioms and the consideration of them "being false" is finding false interpretations for them. Frege's objection is clearly more severe.

Tappenden says that we cannot take too seriously the Jourdain sentence and that this is the only moment where Frege seems to doubt the possibility of proving axiom independence. Nevertheless, the phrase "the mutual independence of the axioms, *if it can be proved at all…*" In the correspondence with Hilbert seems to involve the same thing. In the correspondence with Liebmann (1900), he says:

> I have reasons to believe that the mutual independence of Euclidean Geometry cannot be proved. (PMC, 91)

Even if we set apart this matter, there is still one problem in Tappenden's interpretation: why Hilbert's independence proofs cannot be re-interpreted with the method Frege sketched in 1906? When Frege outlined the generalities of his method, he seems to be describing the ordinary proofs of axiom independence.

3.1. Blanchette's interpretation

Blanchette's interpretation below raises an interesting question over the role of analysis in finding logical relations. Every sentence in a logically perfect language expresses a thought. In the same way, we might suppose that all the sentences that express the same thought are equivalents from a logical point of view. For Frege, according to Blanchette, this is not so. A thought can be expressed in sentences that differ in complexity and logical structure. Further analysis of a concept can reveal a complexity in it that causes a thought that we previously expressed with a relatively simple sentence to now be expressed by a more complex sentence. Conceptual analysis is essential to the search for logical relations, as Frege clarifies in the following passage:

> In the development of a science it can indeed happen that one has used a word, a sign,

> an expression, over a long period under the impression that its sense is simple until one succeeds in analyzing it into simpler logical constituents. By means of such an analysis, we may hope to reduce the number of axioms; for it may not be possible to prove a truth containing a complex constituent so long as that constituent remains unanalyzed; but it may be possible, given an analysis, to prove it from truths in which the elements of the analysis occur. (PW, 209)

Let us suppose that a group of thoughts P is expressed by a set of sentences S and that a sentence e expresses a thought t. If t is the substitutional consequence of S, then t is a logical consequence of P, but not the other way around. The fact that we can give an interpretation in which all the sentences of S are true and e is false does not imply that t is independent of P, for it may be that a later conceptual analysis reveals a logical dependence between these thoughts. This is, in essence, Blanchette's interpretation and it explains why Frege considers Hilbert's method unproductive and why he does not believe that a proof of independence can be given. For example, a conceptual analysis of geometrical concepts and relations can reveal that Hilbert's models using real numbers as "points" are not useful because his interpretation goes against a content implicit in the term 'point' but not captured by the axioms. Naturally, this is of no concern to Hilbert because he does not take 'straight line' to refer to the same concept as his opponent does. For him, there is nothing more to the concept of point than what is expressed in the geometrical axioms. Anything else is "hide and seek".

Certainly, if we were sure of having reached the final analysis of the concepts, then the semantic independence of e with regard to S would prove that t is independent of P. But Blanchette holds that

> from Frege's point of view there is never a guarantee that the language in question is in fact "fully analyzed". (Blanchette, 2012, 129)

I think that this interpretation fits with almost all the textual evidence.

From Blanchette's interpretation, certain consequences for the interpretation of Fregean philosophy follow.

a) If there is no final analysis, Frege cannot give a substitutional explanation of logical truth. In that case, Frege's conception of logical truth is mysterious and goes against a tradition than began in Aristotle and was hold by Bolzano and Quine. We can agree with Frege that axioms are thoughts and that the *relata* of logical relations are thoughts, not sentences, but what does it mean that a thought is a logical truth? That a sentence that expresses this thought is true and remains true after each possible substitution of its non-logical terms. And the justification of a syntactic method of deduction is the verification that each rule of inference preserves this property of invariance of truth under any substitution of the non-logical vocabulary of a sentence. But, as we see, this linguistic criterion of logical truth is lost with Blanchette's exegesis.
b) If we are not sure as to whether the final analysis has been reached (whether there is one or not), no proof of axiom independence (for those are thoughts) can be given, not even the syntactic-method based ones (as the ones proposed by Hilbert's program).
c) Apparently, conceptual analysis can reveal new logical links, but cannot reveal that the deductive relations that we believed existed between thoughts were illusory. It could never happen, according to this view, that an analysis of the concept of number would show that arithmetic is logic, but a later analysis shows this to be false. I do not see what the justification of this could be. I think this asymmetry between logical independence and logical consequence is unjustified. Recall the main idea of Blanchette's interpretation: a thought can be expressed in sentences that differ in complexity and logical structure. Further analysis of a concept can reveal a complexity in it that causes a thought that we previously expressed with a relatively simple sentence to now be expressed by a more complex sentence. Suppose

that in a certain situation, the derivation of e from S, would reveal an apparent deductive connection between t and P. However, other analysis of the concepts involved could result in t being actually expressed by d and P by M, where d is substitutionally independent of M. For instance, a new analysis of the concept could add complexity not only to the premises of our previous argument but also to the conclusion turning a valid argument into an invalid one. The analysis may have also revealed that the logical form of a premise is not the one we assumed, and some apparently valid inference before the analysis was later revealed as incorrect. For example, an identity, which allows replacing a term for another *salva veritate*, may turn out to have a different logical form after being analyzed, as with some equations, according to Russell's theory of defined descriptions. This would also happen if a statement on numeric identity were analyzed under Hume's principle. If this were to take place, then we will have neither proof of independence nor proof of logical consequence between thoughts, which would naturally go against every Fregen project. And so, if Blanchette's interpretation is correct, the analysis must allow logical consequence but not independence (or consistency).

This last point set aside, a problem with Blanchette's interpretation remains; it contradicts the last paragraphs of *Foundations of Arithmetic*, whereby Frege outlines a method to prove independence. To better adjust it to this piece of written evidence, I consider worthwhile to seriously consider Frege's explanation on his 1906 article of the insufficiency of his method to prove independence. Among the points to further elaborate is the question of what should count as a logical inference. In other words, the outlined method is still relative to determining the logical vocabulary. It reads

> with this we have an indication of the way in which it may be possible to prove independence of a real axiom from other real axioms. Of course, we are far from having a more precise execution of this. In particular, we will find that this basic law which I have attempted to elucidate by means of the above-mentioned vocabulary still needs a more precise formulation, and that to give this will not be easy. Furthermore, it will have to be determined what counts as a logical inference and what is proper to logic… If, following the suggestions given above, one wanted to apply this to the axioms of geometry, one would still need propositions that state, for example, that the concept point, the relation of a point's lying in a plane, etc. do not belong to logic. These propositions will probably have to be taken as axiomatic. Of course, such axioms are of a very special kind and cannot otherwise be used in geometry. But we are here in unexplored territory. (CP, 339)

Frege's remarks are rather surprising but could explain how in a single article there are critiques to Hilbert's methods, as well as the outline of a method to prove independence in terms of replacing non-logical vocabulary. Among the points to further elaborate to really provide a method to prove axioms independence would be determining what should count as a and, particularly, what terms or constants belong to the logical vocabulary. That is, the outlined method is still relative to determining to this lexicon. This is part of an unexplored territory and a matter of a yet-to-be-created science. The axioms of this still unborn discipline would not belong to geometry, but rather to a different level. It seems striking that Frege considers as a possibility that some geometric terms were part of the logical vocabulary. This could be explained because, at the time that was written, his author had renounced to logicism in favor of other theoretical possibilities. But that might as well have been a rhetorical resource. If some geometric terms were logical constants, then the proof of independence of geometric axioms might be incorrect. The paragraph goes hand in hand with Russell's one, to which the Jourdain statement is referring to. Russell highlights that a proof of independence of a logical axiom cannot take place with the same method used for geometrical axioms, for, as a logical axiom is assumed incorrect, the deductive relations or logical consequences are altered. We cannot figure out what follows from some axioms or what holds an independent relation if we assume that the principles ruling the relation have been altered.

These remarks could improve Blanchette's interpretations in at least two regards. The first one is that the last paragraphs of the 1906 article will be explained. The second one is that the aforementioned asymmetry problem could be explained as follows. Let us assume, for instance, that two thoughts, A and C, have been respectively expressed by two statements, A' and C', in such a way that C' can be deduced (or to be a substitutional consequence) from A'. Thus, the analysis of terms (that is, their senses) within A' or C' could reveal a richer structure just within those statements, without altering the conditional (A'→C') value. On the other hand, the accurate determination of the logical vocabulary would only add more constants which, then again, would reveal greater structure within A' and C' without altering the already found logical relations between statements and, thus, between the thoughts they express. Nonetheless, in order to be fully compatible with the 1906 final paragraph, the interpretation should admit the possibility of reaching an alleged final analysis. Otherwise, no proof of independence is possible.

4. Conclusions

To wrap up, I will recap the results that I think I have obtained son far.

a) Firstly, I have pinpointed, following Boolos and Landini, the importance of the rule of replacement implicitly used by Frege in his two *Begriffsschrifts*. This rule comprises two parts, allowing the creation of new conceptual terms (functional, more generally) from replacing singular or functional terms for suitable variables in the terms they occur (for instance, as statements). This grants to Fregean logic more power than, for instance, Boole's logic. Secondly, the rule of replacement allows the concepts (or functions) denoted by the resulting terms to become part of the domain of suitable quantifiers. Allowing both operations is of the utmost importance for constructing arithmetic from logic, as already stated in the third part of *Begriffsschrift*. I also highlighted the need for non-predicative definitions to this end. And so I concluded that: a) Frege's definitions go beyond mere abbreviation files, and that b) Frege would not (or should not) have tolerated contextual definitions (which allow for defined terms to be get rid of just in a number of contexts) because then the principles referred to in a) and b) would quickly allow meaningless questions within formalism, that is, neither true nor false formulas.

b) I showed that Frege has an understanding of Hilbert's axioms as high-order relation definitions, but that the difference between both authors is not merely a verbal one. Underliying it, there are different conceptions of what a concept is. Hilbert's is a structuralist one, though it is hard to attribute him some of the particular structuralist varieties mentioned in the more recent literature.

c) I also highlighted the difficulty of two interpretations of the paragraphs where Frege rejects Hilbert's proofs of axioms independence. For that, I interpreted Frege's alleged reluctancy to derive something of a false premise as a merely pragmatic caution. I was inclined to Blanchette's interpretation, where conceptual analysis plays a role in discovering logical relations. I highlighted their consequences and difficulties. To pair it to the written evidence, I suggested we should take very seriously Frege's paragraphs on the need to more accurately determine what a logical inference is and what a logical constant should be. I believe this could improve Blanchette's interpretation, though Frege's actual view concerning these matters will seem to remain speculative because of the lack of further written evidence.

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