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HUSSERL BETWEEN FREGE'S *Logicism* AND HILBERT'S *Formalism*

Let me begin with a slightly provocative remark: Philosophy and science have many problems in common. One *peculiar* problem of philosophy is this: Very good philosophy can be wrong in *substance*, and substantially correct philosophy can be utterly boring. Kant is an instance of the first, Husserl of the second case.

1. INTRODUCTION

The traditional view regarding the philosophy of mathematics in the twentieth century is the dogma of three schools: Logicism, Intuitionism and Formalism. The problem with this dogma is not, at least not first and foremost, that it is wrong, but that it is biased and essentially incomplete. 'Biased' because it was formulated by one of the involved parties, namely the logical empiricists – if I see it right – in order to make their own position look more agreeable by comparison with Intuitionism and Formalism. 'Essentially incomplete' because there was – and still exists – beside Frege's Logicism, Brouwer's Intuitionism and Hilbert's Formalism at least one further position, namely Husserl's phenomenological approach to the foundations of arithmetic, which is also philosophically interesting. In what follows, I want to do two things: First, I will show that the standard dogma regarding the foundations of mathematics is not only incomplete, but also inaccurate, misleading and

basically wrong with respect to the three schools themselves. In doing this I hope to make room for Husserl and his phenomenological approach as a viable *alternative* in the foundations of arithmetic. Second, I will show how Husserl's phenomenological point of view is a position that fits exactly in between Frege's "logicism", properly understood, and Hilbert's mature proof theory, in which his so called "formalism" turns out to be only a means to an end and not a goal in itself. In other words, what I want to show is the following: (a) Frege is not the logicist he is normally supposed to be, and (b) Hilbert is not the formalist that the logical empiricists assumed he was. Finally, and in order to make the list complete, (c) Weyl is not a radical intuitionist like Brouwer (except for a very short period) but rather defends a much more moderate epistemological position and is actually one of Husserl's closest allies with respect to a *Fundierung* of mathematics – his secret advocate, so to say.

Now, some may wonder how I will be able to defend such an interpretation, standing as it does in opposition to most of what is said and written about Frege, Hilbert and the other 'founding-fathers'. My answer to this legitimate question is simple and straightforward: There is not only *one* Frege, *one* Hilbert and so on, but rather many different Freges, Hilberts, etc. In other words, Frege and Hilbert developed quite a number of different proposals, opinions and philosophical points of view during their scientific careers, views that were later revised or substantially modified and sometimes even given up completely in favor of a new approach. For example, we have to distinguish in Frege at least three different periods in which he promoted fundamentally different positions: (a) the early period of the *Begriffsschrift* (BS) in which he envisaged his project of a proper foundation of arithmetic; (b) the long and productive middle period of the *Grundlagen* (GA) and *Grundgesetze der Arithmetik* (GGA); and (c) the last period of the *Philosophische Untersuchungen* (PU), in which he (unsuccessfully) tried to find a way to avoid Russell's paradox. The case is similar for Hilbert. We should distinguish at least three periods in which Hilbert pursued different projects regarding the foundations of mathematics: (a) the first period from 1890 to 1900, in which he struggled with the *Grundlagen der Geometrie* (GG), (b) the second period from 1915 to 1920, in which he studied with his pupils (among other things) Russell's *Principia* and related writings – an enterprise whose fruits first became known 10 years later as the 'Hilbert-Ackermann' text: *Grundzuege der theoretischen Logik* (GTL) – and (c) the third period from 1920 to 1928 or so, in which he developed, together with his assistant Bernays, a radical, new and ingenious program for the foundations of mathematics, his famous *proof-theory*. These distinctions may seem uncontroversial and almost trivial. But they are not. For instance, one need only take a look at the exegetical literature on Frege. What is presented there as Frege's opinion or Frege's point of view is frequently enough only a mixture of quotations and opinions from different periods, which make little sense and offer no reliable interpretation of Frege's own original opinion. In the case of Hilbert things are even worse: interpreters are often not aware that his work should be assessed in the light of his having gone through different stages of development and tend to throw all of Hilbert's utterances together into the same pot.

In order to avoid such mistakes, let me state in advance which periods of Frege, Husserl and Hilbert I want to investigate and ask whether there exists relationships between the basic views of these three authors. I begin with an investigation of Frege's early writings from the period of the BS and offer a fresh and unorthodox interpretation that differs significantly from the standard exegesis of Frege as the father of logicism. In the beginning Frege took the question of whether mathematics can be reduced to the laws of pure thinking to be an open one: there were too many conceptual and inferential gaps in the proofs of mathematical theorems, which first had to be closed in order to decide this question definitively. Next, I turn to Husserl's early writings. Here I focus mainly on his considerations regarding a "true philosophy of the calculus" and the *Doppelvortrag* that he held in Göttingen in 1901. I will argue that – despite obvious stylistic differences – Husserl's early philosophy is much closer to Frege's original aim in the BS than the later writings suggest. Husserl's main aim was – like Frege's – a gapless foundation (*Fundierung*) of arithmetic; their opinions concerning this matter differing primarily with respect to the question as to where the causes for the gaps were to be located and, hence, by which means they could be removed. I turn finally to Hilbert and his published and unpublished writings from the 1920s concerning the *New Foundations of Arithmetic* and the creation of proof-theory. In this context the year 1920 is important, because in the summer term of 1920 Hilbert had

come to the definitive conclusion that the proposals of Russell and Weyl are not feasible, that they do not lead to the desired result, i.e. a "firm grounding" of mathematics. Hilbert's proof-theory can be seen as a new means to an old end, a basic aim he shares with Frege and Husserl: a logically gapless foundation of mathematics.

2. THE SIGNIFICANCE OF FREGE'S BEGRIFFSSCHRIFT

Frege's first philosophically oriented publication was a small book with the long title *Begriffsschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens* published 1879. The book was no great success – to say the least – even though it laid out a program concerning the foundations of mathematics that kept Frege busy over the next 20 years and became a milestone in the formation of analytical philosophy as we know it today. The poor reception of this work led Frege to attempt, over the next three years, to present his ideas in a series of four essays, two of which were published and two rejected. Perhaps because these essays also failed to have the desired effect, Frege published another small book entitled *Die Grundlagen der Arithmetik* in 1884, in which he investigated from a more philosophical point of view the concept of number and the different opinions of some authors: mathematicians and philosophers of different schools and opposing opinions. Given its subject matter, it is not surprising that this book became another cornerstone of analytical philosophy. After this it took another nine years before Frege published the first volume of his *Grundgesetze der Arithmetik* in 1893.

From the fact that Frege pursued the program he had originally outlined in the *Begriffsschrift* for more than 20 years one should not conclude that he did not change his opinions as to how to achieve the desired goal. On the contrary, he changed his point of view significantly at least two times: the first change occurred sometime between the *Begriffsschrift* and the *Grundlagen*; the second between the *Grundlagen* and the *Grundgesetze*, as Frege himself acknowledged in the preface to *Grundgesetze*. One clarification: when speaking of the *Begriffsschrift* one should always distinguish between two different meanings: the *book* Frege called '*Begriffsschrift*' on the one hand, and the "system of thoughts" Frege denotes by the word '*Begriffsschrift*' on the other; usually the context makes clear which of the two meanings is intended. Now let us ask, first, what is the significance of the book. And second, what does Frege mean by '*Begriffsschrift*'. I will consider the second question first. The best way to do this is to begin with a quotation from the essay "Über den Zweck der Begriffsschrift", in which Frege defended himself against the reproach of Schröder that he, i.e. Frege "had left Boole's achievements out of account":

> The reproach, however, essentially overlooks the fact that my aim was different from Boole's. I did not wish to present an *abstract logic* in formulas, but to express a *content* through written symbols in a more precise and perspicuous way than is possible with words. In fact, I wished to produce, not a mere *calculus ratiocinator*, but a *lingua characterica*, in the Leibnizian sense. In doing so, however, I recognize that deductive calculus is a necessary part of a conceptual notation. If this was misunderstood, perhaps it is because I let the *abstract logical* aspect stand too much in the foreground. (My emphasis. The translation by T. W. Bynum entails a remarkable mistake: Frege's original term 'characterica' was translated as 'characteristica', thus correcting silently a mistake that Frege had made in his answer to Schröder; see the German quotation in the footnote.)[1]

This quotation entails a significant part of the answer to the second question. But a correct understanding of the differences between Frege and Boole seems to be a difficult issue; in my opinion most interpretations are faulty or go completely astray. The reason for the difficulty is not hard to come by; it has two components: (1) contrary to what is usually assumed, Frege's distinction between *calculus ratiocinator* and *lingua characterica* is far from trivial, indeed it is very curious, and (2) most interpreters take it for granted that Frege's reference to Leibniz is correct and justified. But this is not the case, as Patzig noticed more than 30 years ago: the expression "lingua characterica" is not only bad Latin – *characteristica* would be correct – but also impossible for Leibniz because the terms '*lingua*' and '*characteristica*' mean roughly the same! In other words, the compound expression "*lingua character[ist]ica*" is a pleonasmus in Leibnizian terms. The correct Leibnizian terms would be either "*lingua universalis*" or "*characteristica universalis*". But the

error goes much deeper: It's not only a linguistic slip but also a substantial error because for Leibniz *lingua universalis* and *calculus ratiocinator* do not constitute an opposition, but rather complement each other. They are, so to speak, the two sides of one and the same coin. Consequently, something must be wrong with Frege's supposition that his distinction between *calculus ratiocinator* and *lingua character[ist]ica* would be Leibniz' distinction. And indeed his reference to Leibniz is only indirect. Frege relies in this matter on an essay of Trendelenburg, which he mentions in the preface to the *Begriffsschrift* and which is entitled "Über Leibnizens Entwurf einer allgemeinen Charakteristik". But this is not all. Frege also adopts Trendelenburg's interpretation of Leibniz' *characteristica universalis* as a "*Begriffsschrift*" – this is in fact Trendelenburg's term – and uses it to demarcate his own program of establishing a foundation for arithmetic from Boole's intention to analyze and construct logic in terms of algebra. Consequently if one wishes to know how Frege understands the distinction between *calculus ratiocinator* and *lingua characteristica* one must consult, first of all, Trendelenburg's essay, then the writings of Leibniz and finally, of course, Frege's own considerations.

As it occurs, if we follow the quotation, Frege did not want to create a mere *calculus ratiocinator*, an 'abstract logic' – like Boole and Schröder – but a *universal characteristic*, a *Begriffsschrift* as Trendelenburg calls it, by means of which one can express a *content* (*Inhalt*) more transparently and perspicuously than is possible by means of ordinary language. So far, so good. One must, however, still ask what exactly Frege means by '*Begriffsschrift*' - even if we presume that we know what he means by 'abstract logic' (which is not self-evident). Now, the meaning-question has two answers, a wide and a narrow one: The wide meaning of '*Begriffsschrift*' is roughly the same as the meaning of the expression 'scientific theory': both denote the same kind of entities, whatever a scientific theory may be. The narrow meaning of '*Begriffsschrift*' is nearly identical with that what we today call *logic*. Take the following quote:

> We can view the symbols of arithmetic, geometry, and chemistry as realizations of the Leibnizian idea [of a universal characteristic] in particular areas. The "conceptual notation" offered here adds a new domain to these; indeed, the one situated in the middle adjoining all the others. Thus,

> from this starting point ... we can begin to fill in the gaps in the existing formula languages, connect their hitherto separate domains to the province of a single formula language and extend it to fields which up to now have lacked such a language. (Italics are mine; the translation is by T.W. Bynum; it entails an incorrectness because in the last sentence 'the province of a single *Begriffsschrift*' and not of a 'single formula language' is intended.)[2]

What Frege here says is roughly the following: There exists already a small number of concept-scripts that are formed by the symbols and rules of arithmetic, geometry and chemistry. Frege attaches to these already existing concept-scripts a *new* one, which fulfills two tasks: first, it fills in the *gaps* in the existing concept-scripts, and second, it connects their separate territories into a *single* domain. But we have to ask: What does Frege here mean by "gaps" and in what sense does the new concept-script connect the domains of the different sciences into a single domain? The answer to the first question is relatively easy: Not only chemistry but also geometry and arithmetic use – beside their specific symbols – a great number of words from ordinary language in order to express their laws and the proofs of their statements. This is a serious defect in Frege's view, a "gap" with respect to a perfect language of science that has to be removed. Once this gap has been closed by the design of a new concept-script all particular domains of the different sciences become connected with the central domain of the new concept-script that lies in the middle of all the other territories. So far, there can be little doubt what Frege has in mind; there remains only one further question: What exactly is the central domain in the middle, what is the particular territory of the new concept-script? The orthodox answer supposes that it is the territory of logic, more precisely that of first and higher order *predicate logic*. But this does not seem quite correct in the first place, and is furthermore insufficient, because the orthodox view keeps silent about the question as to what the proper domain of predicate logic is. Hence, let me explain what the *correct* answer is (in my view) and where the orthodox view goes wrong.

If one studies the table of contents of the *Begriffsschrift*, one notices rather quickly that the latter contains no axiomatic presentation of logic, only some examples of logically true sentences. This is no accident: At the time Frege wrote the *Begriffsschrift*, he had no theory of logic! He had no idea how to present the laws of pure thinking in an "axiomatic form", i.e. in a *deductive system* that entailed all and only those sentences that are "true" laws of thinking, logical theorems as we say. He did not even consider that question, because he was not interested in a "theory" of logic. What he was interested in and what he did consider in detail was something rather *different*: He wanted to know which sentences of arithmetic could be proved exclusively by logical means i.e. by *inferences* based only on the laws of thinking (and nothing else, in particular not on *pure intuition* as Kant had maintained). The sole logical inference Frege relied on for this purpose was *modus ponens*! Hence, in order to answer the question he had to analyze the *functional role* of the logical concepts and relations in judgments and logical inferences (therefore the name 'concept-script') and to *represent* these functions by appropriate graphic signs. The totality of these signs – together with the *rules* specifying how to use them in inferences – is Frege's *Begriffsschrift* in the *narrow* sense. The invention of the BS is Frege's lasting contribution to the further development of logic. But it is not a *theory* of logic in the proper sense of the word 'theory'. And Frege did not claim that it was. For him, the invention of the *Begriffsschrift* was a *means to an end*, not a goal in itself, and the end he had in mind was to find an answer to the open question whether all sentences of arithmetic are "analytic" in the sense that they could be proven exclusively by *modus ponens* from the "laws of pure thinking". Hence, the real significance of the *Begriffsschrift* lies in the proof-procedure it enables. I say consciously "proof-procedure" and not proof-theory because the procedure proposed by Frege in the BS is not the same as Hilbert's *proof theory*,which was developed forty years later, although it shares some important features with it.

Before I turn to Husserl let me make two points of clarification: 1) In the beginning Frege was not a logicist in the proper sense because he didn't claim that arithmetic is nothing but a branch of logic. On the contrary, this was an open question for him, a question that he wanted to investigate by means of his *Begriffsschrift*. However, a few years later, when he wrote the *Grundlagen*, he was already convinced that at least elementary arithmetic is a branch of logic, although he had no evidence to support this conviction. The proof he would come up with had to wait another ten years until the publication of the *Grundgesetze*. However, the proof turned out to be faulty. 2) The second point is complementary to the first. In the beginning Frege was not the outspoken anti-psychologist that most interpreters take him to be. In the preface of the *Begriffsschrift* Frege distinguishes between two ways of cognition: The first way is the *subjective* cognition of a truth, the second its *objective foundation* (*Begründung* in German). Of course, proofs belong to the objective way of foundation, whereas the grasping of a thought or the immediate recognition of the truth of a sentence (other than by proof) is a subjective ability, which is, at least in part a matter of psychology. In other words Frege does not reject psychology *absolutely*, i.e. under any circumstances; it is just not his business as a mathematician who is interested primarily in the objective foundations of arithmetic and nothing else.

3. HUSSERL'S "WAHRE PHILOSOPHIE DES KALKÜLS"

Husserl – 11 years younger than Frege – was a student of Weierstrass in Berlin when Frege published the *Begriffsschrift*. In 1887 Husserl habilitated with a work entitled *Über den Begriff der Zahl* – three years after Frege had published his second book, *Die Grundlagen der Arithmetik*. The *Habilitation* became part of Husserl's first book *Philosophie der Arithmetik*, which was published in 1891, the same year Frege published his essay *Funktion und Begriff*.

At first sight, Husserl's early work and Frege's BS have little in common. Not only is their *style* of writing and argumentation very different, but their respective philosophical *standpoints* also seem to differ widely. Husserl in *Philosophy of Arithmetic* took a *psychological point* of view – so the standard story goes – whereas Frege argued, already in the *Begriffsschrift*, that logical proofs that disregard the particular characteristic of things have nothing to do with psychology. But if we leave aside for a moment the much-debated issue of psychology and focus instead on the proper topic of their respective works, namely an inquiry into the foundations of arithmetic, then their works are not so far apart as it may appear.

I am aware that my thesis undermines the received view of Føllesdal, according to which Husserl's philosophical point of view underwent a

dramatic change – a conversion from psychologism to anti-psychlogism – as a result of Frege's "devastating attack on the *Philosophy of Aritmetic* in 1894". In fact, I don't mean to challenge this view – it seems to me *cum grano salis* correct. I will rather try to identify the true dimensions of this change of view on Husserl's part. If am right that Husserl's position before 1894 is not so distant from Frege's objective notion of proof as outlined in the *Begriffsschrift*, for Husserl too strived for a gapless foundation of arithmetic, then the change in Husserl's view regarding the true foundations of arithmetic is actually less dramatic than it may seem.

As it turns out, both Frege and Husserl are concerned with the foundations of arithmetic; both are convinced that the arithmetic of their time lacks a safe and gapless foundation. Their opinions differ, however, when it comes to the question as to the real causes for the gaps and, consequently, how a firm and gapless foundation can be achieved. This question is, of course, intimately connected with another fundamental question, namely, what are numbers? Here again Frege's and Husserl's opinions point in different *directions*. But before we can assess the true significance of Husserl's turn from psychologism to anti-psychologism we have to distinguish at least two questions and answer them separately: (1) Did Husserl change his view with respect to the real causes of the gaps in the foundations of arithmetic before 1894? And if so, did he adjust his program of a gapless foundation of arithmetic correspondingly? Because the answer to this question is almost entirely negative, the further question arises: (2) Are the differences between Husserl's and Frege's viewpoints regarding psychology so extreme that one should speak of a "conversion"? In order to answer this question we must take a closer look at their respective positions.

In the *Begriffsschrift*, Frege had hoped – it was not more than a *hope* as it turned out 22 years later – that he could *reduce* all sentences of arithmetic to "laws of pure thinking" by means of his proof-procedure and in this way reduce the concept of number to the concept of *Anordnung in einer Reihe* (order in a sequence). But if we want to do justice to Husserl, we should not exaggerate Frege's achievements and this means that we have to accept the fact that even if Frege had been able to carry out his program successfully, several questions would nevertheless have remained open: what are "laws of pure thinking", how do we recognize them and what is their ontological status? Of course, Frege leaves little doubt as to the fact that he thinks that they are something absolute and "objective", that they do not depend on this or that human being, but only on the "conceptual content" of a sentence. But this answer has at least two serious shortcomings. (1) The *objectivity* of the laws of thinking – even if granted – does not settle the question of their ontological status. Few mathematicians doubt that numbers are something objective, but the ontological question whether they are individual objects or general concepts is not settled by this conviction. (2) In Frege, the presumed objectivity of the laws of thinking drops, so to say, from the sky. Frege does not offer the slightest argument for it in the *Begriffsschrift* and what he later says in this respect in the *Grundlagen* seems to me to be at best inconclusive. Let's consider the *Begriffsschrift*. The main distinction he introduces in the preface is the opposition between two ways of recognition of a scientific truth: (a) the subjective way of guessing, grasping, having sense impressions and experiences in the widest sense; (b) the objective way of drawing logical inferences, either by inductive corroboration [Befestigung] or, more important and reliable, by logical deduction as a consequence from another truth, i.e. by proof. Now the interesting point is that the recognition of the laws of pure thinking is not implied by (b): their truth can neither be proven by logical deduction nor by inductive corroboration. On the contrary, their validity has to be presupposed in every logical inference. This is what I mean when I say that their objectivity drops from the sky: The recognition of "the laws of pure thinking" seems to belong either to (a) – which Frege brands as psychology – or Frege has to admit a third way, a new immediate source of cognition for the laws of pure thinking.

So, why should Husserl have adopted Frege's *reductionist* and anti-psychologist point of view instead of pursuing his own program? I see no plausible reason. At any rate, this is exactly what happened: Husserl held on to his own program even when he changed his view with respect to psychology. It is important to note that Husserl's change to Frege's anti-psychologism fell together with his new interest in the epistemological foundations of logic, which eventually resulted in the *Logical Investigations* of 1901, but had nothing to do with his program of a firm and gapless "*Fundierung*" of arithmetic. The reason for this remarkable fact is not hard to come by. Because Husserl never shared Frege's *logicism,* i.e. his goal of a complete and gapless reduction of arithmetic to the "laws of pure thinking", he located the gaps in the foundations of arithmetic not in the lack of a *Begriffsschrift*, but in certain shortcomings of his own "genetic" program.

So let us see, what Husserl's program of a firm and gapless "*Fundierung*" of arithmetic was, and furthermore, where precisely he felt that certain shortcomings in his program would endanger the goal of a gapless foundation of arithmetic. After this we will better understand in which ways Husserl's genetic approach differs from Frege's program of a reduction of arithmetic to logic.

Husserl's long-winded style of writing as well as the fact that important texts from the early period were first published in 1983 under the title "Studien zur Arithmetik und Geometrie" bring about at least one difficulty. Most of these texts are blueprints for the second volume of the *Philosophie der Arithmetik*, which Husserl did not publish. They contain philosophical considerations concerning the concept of "calculus" that allow us to establish two important facts. First, it allows us to explain what the main point of Husserl's critique of Frege's conception of numbers as extensions of concepts is, and, second, in which way Husserl's conception of calculus is essential for the foundations of arithmetic.

I do not know a shorter way to tackle the first task than to quote Weyl's summary of Husserl's *Philosophie der Arithmetik,* as he presented it in his *Habilitations-Vortrag* in Göttingen in 1910:

> The concept of set and number has run through various stages during the development of the human intellect. At the first stage it concerns the *proper idea of a totality* [Inbegriff] which emerges when representations of several individual objects, recognized as such, are raised by a unified interest to the content of our consciousness and held together. On this level, the lowest numerals, say 2, 3, and 4, designate immediately recognizable differences of the mental act coming into function in forming the idea of a totality.
>
> At the *second* level *symbolic* representations stand for the proper ones. The most significant result of this second period is the procedure of symbolic counting, familiar to every child, which permits us to differentiate more comprehensive sets according to their number.

> Because we are compelled by other irrefutable reasons to introduce infinite sets – indeed analysis alone forces this – when we finally come to the *third* level, where we erect the theory of finite and infinite sets and numbers in a scientifically systematic way by setting up appropriate axioms, definitions and the consequences drawn from them. (Weyl, GA, Vol. 1, pp. 302, my translation)

According to Weyl, Husserl distinguishes between three levels in the formation numbers: the proper, the symbolic, and the axiomatic stages. Considerations of the first two levels already suffice to make clear one principal difference between Husserl and Frege: Frege doesn't accept Husserl's first level as necessary – nor even as helpful – for an analysis of the concept of number. One might think that this difference is not very important because it concerns only the "genesis" of numbers and this is, from Frege's point of view, merely a matter of psychology. But not for Husserl who is mainly interested in the real genesis of numbers and this is not a matter of psychology but of objective construction. Yet, there are further differences involving Husserl's levels two and three, which are important for two reasons: On the one hand, Husserl does not accept Frege's conception of numbers as "extensions of concepts". The conception of number is epistemologically prior to the notion of concepts and their extensions. On the other hand, *Philosophie der Arithmetik* is in a certain sense an unfinished book that is in need of supplementation – a fact of which Husserl was well aware and about which we must be clear in order to understand the need for an "axiomatic level". Let's take the first point first.

Why does Weyl say "At the *second* level *symbolic* representations stand for the proper ones". What is the sense of this statement? The answer has two parts, a negative and a positive one; the negative is this: the genesis of numbers in the 'proper' sense does not go beyond the number 13 or so, because we cannot, according to Husserl, represent more than 13 distinct objects at the same time in our conscious attention. Consequently, if we want to count beyond 13 we have to represent these numbers through "artificial" symbols. This claim is the basis of Husserl's critique of Frege's conception of number: if there is no objective reason (that is, no reason except our epistemic limitations) to distinguish the symbolized numbers from the proper ones, then both

have the same ontological status – both exist independently of any further condition. But if this is the case then Frege's contention "that every number-statement is a statement about a concept" is untenable. Frege's position implies that numbers are dependent on concepts. Because this is misguided from a mathematical point of view (we use numbers in equations without mentioning any concept) Frege tries to improve the definition of numbers by introducing the idea of equivalent classes, and does so in two steps. First he introduces the relation "gleichzahlig mit" and then he switches to the extension [Umfang] of the concept *gleichzahlig*; according to his definition, which states that "the [cardinal] number, which belongs to the concept F, is the extension [Umfang] of the concept 'equal in number [numerically equal?] with the concept F'". (GA, p. 79/80)[3] I think Husserl is right: This "amendment" makes things worse.

In what sense is *Philosophie der Arithmetik* an unfinished book? Of course, I do not have the *trivial* sense of "unfinished" in mind. It is not only that Husserl did not finish the second volume of PA, but rather that his *systematic* perspective remains fragmentary: the first volume does not contain an account of the third, axiomatic level. This level is however absolutely indispensable for a proper foundation of arithmetic, because infinite sets and numbers are first introduced on the third level; without these arithmetic would remain a torso.

At this point two questions arise. (i) How did Weyl know about the third level? (ii) Why did Husserl not include the third level in the first volume but postponed its treatment to the second volume that never appeared? The first question is easy to answer: Weyl, an excellent mathematician, immediately saw what was lacking in Husserl's *Philosophy of Arithmetic*. The second question is far more difficult to answer, because we must not only recognize *which* problem troubled Husserl, but also understand what the *nature* of the problem was and *why* it was so difficult for Husserl to find an appropriate solution and therefore why Husserl postponed the treatment of this question to the second volume.

In order to find the right answer to these questions we must remember where we stand on level two, what we have achieved with the symbolic representation of numbers. It's not much – from a mathematical point of view. Weyl is very precise in this respect when he says that "The most significant result of this second stage is the procedure of *symbolic* *counting*, familiar to every child, that permits us to differentiate more comprehensive sets according to their number [cardinality]." In other words, we have arrived at the level of *symbolic counting* and, with it, to the generation of the potentially infinite sequence of natural numbers or positive integers. What we have not reached, not yet reached at any rate, are the four simple *methods of calculation* or – as Husserl calls them – the "four species of arithmetical operations": addition, subtraction, multiplication and division. The fact that Husserl stopped here is rather strange, because mere counting is certainly not arithmetic in the full sense; it is only a tiny fragment of ordinary arithmetic that does not allow us to express interesting arithmetical laws. For this reason we should ask: Why did Husserl not get to the arithmetical operations? Why is the introduction of the four arithmetical operations apparently a problem for Husserl? Why can we not simply define them? From our contemporary perspective, nothing seems easier than that! And even if it is true that we cannot define them *simpliciter*, what is the obstacle or deeper impediment to a definition in a more elaborate sense? Now, I think that if we find the correct answer to these questions this will also throw some light on two other questions, namely what does Husserl mean when he writes, in the preface to *Philosophie der Arithmetik*: "I will perhaps succeed, at least in some fundamental points, to open the way... to a true philosophy of the calculus" (PA, p. 7) and why he did not achieve this goal, at least not completely insofar as an essential part of his considerations regarding "the true philosophy of the calculus" was missing in the first volume of *Philosophie der Arihtmetik*. But just what is missing? A first hint comes from the table of contents of Husserliana XXI. Here we find (among other things) titles like: "Die formale und wirkliche Arithmetik", "Die wahren Theorien", "Die Theorien der Erweiterung des Zahlgebietes". In particular, the last paper gives a clue as to what is "missing" in PA: we cannot execute the operation of subtraction or division, at least not unlimitedly, because the domain of natural numbers is too small or, better, too restricted: If you want to subtract b from a, then, if b is larger than a, you will end in the "middle of nowhere". In other words, when a is exhausted without our having finished the operation of subtracting b from a, we have to stop, we can go nowhere, because there are no further elements, from which one could subtract the remaining elements of b. In order to execute

the operation completely you have to introduce the so-called "negative" numbers. But these are no numbers in the first place, at least not on the inductive level of "symbolic counting", and second they cannot be introduced by *definition*, because such a definition would have to be "creative in the sense that it generates *a new kind* of numbers that have to be united with the already existing domain of positive integers to form a new comprehensive domain of numbers, the whole numbers". Corresponding arguments are valid with respect to other arithmetical operations like division, the extraction of roots and other arithmetical operations. In other words, the different methods of calculation force us to expand the domain of natural numbers again and again in different directions.

The necessity of *domain-expansion* is the main obstacle when it comes to the problem of getting from the second level of symbolic counting and the potentially infinite sequence of natural numbers to the third level of quantification over all numbers and the formulation of true universal laws and thus to the *actual infinite*. The logical reason for this difficulty is not hard to come by: If the original domain of numbers is not properly defined, then the domain of natural numbers is too restricted to execute the different arithmetical operations and we have to 'expand' it; but if we expand it 'piecemeal' then the domain of numbers is not properly defined. And if the domain of numbers is not properly defined then the domain of individual variables is not properly defined either and the quantifiers loose their precise meaning. But if the quantifiers have no precise meaning you cannot state the mathematical principle of complete induction, which is a necessary precondition for the formulation of true universal laws and this means you will never arrive at the third level, the level of the actual infinite. Consequently, we must find a different way to introduce arithmetical operations than a stepwise expansion of the Ur-domain of natural numbers. But the question is: How can we do it without opening up new gaps? Weyl's proposal in the last passage of the above quote, to pursue an *axiomatic approach*, was not acceptable to Husserl for quite a while (roughly until 1900) because the axiomatic approach *per se* could not avert the danger of gaps in the foundations of arithmetic and analysis. But this changed at the latest in 1901, when Husserl had 'convinced himself' that he had found a method by which he could close (or at least bridge) the gaps in the axiomatic approach between the Ur-domain of natural numbers and the expanded domains of negative, rational, and irrational numbers.

Now, what is Husserl's way out of this difficulty? For some years he did not know how to deal with the problem of "domain-expansion"; he considered different ideas, for example that of a "parallelism" between a mere *calculus* of the four arithmetic operations (set up by a formal calculus) and the *symbolic* arithmetic on level two. But he soon dropped this idea. The eventual solution was presented in Husserl's famous "Doppelvortrag" in Göttingen in 1901. The basic idea of this solution has two parts: First, Husserl accepts Hilbert's "axiomatic point of view" with respect to the presentation of a "calculus" for the different arithmetic operations and their different domains, and then, in a second step, tries to bridge the gap between the domains of the different calculi and the Ur-domain of natural numbers (generated by inductive counting) the calculus and that of real arithmetic on level two by means of the concept of conservative theory-extension of the Ur-domain. I cannot go into the details, (I have done this elsewhere), but I will at least sketch the basic idea.

Let me begin with the preliminary remark that Husserl had to develop this notion "on his own behalf" because the idea underlying this notion was by no means known in 1900 and the expression 'conservative theory-extension' was first introduced much later. The idea is this: If we extend a given theory – let's say the theory of natural numbers – by adding new axioms this can be done in several distinct ways. The two most important ways are the following: (i) we add one (or several) *new* axioms in an already introduced language (i.e. without the introduction of new expressions for individuals and their relations), or (ii) we first extend the given language by new expressions and then state new axioms by using, in addition to the old expressions, the new one. The first case is best known from Hilbert's book *Foundations of Geometry*, in which Hilbert sets up a theory of Euclidean geometry by the stepwise addition of more and more axioms until the theory is *complete* (i.e. categorical in the modern sense). In this way the meaning of the geometrical expressions "point", "line", "between" etc. changes and, more importantly, gradually sharpens until the domain of the geometrical elements cannot be expanded any further for the sake of consistency. This is precisely what the axiom of completeness in Hilbert's *Foundations of*

Geometry asserts. The second case of theory-extension, i.e. the extension of a theory together with the extension of the language of the theory, had not been studied before Husserl's "Doppelvortrag", presumably because most scientists regarded this case as trivial and uninteresting, at least from a logical point of view. But it is not, as we will see in a moment. Husserl is the first, as far as I know, to have investigated this case more closely.

In case of a theory-extension together with a simultaneous extension of language one has to distinguish between two cases, a trivial and an intriguing one. The trivial case is that in which the new expressions stand in no inner relations to the old ones, because there is no law which ties their meaning together. They denote different kinds of entities, which are intrinsically independent of each other. Take for example the 'age of the captain of a ship'. The age of the captain has nothing to do with the 'length of his ship'; both are independent of each other (at least under normal circumstances). The intriguing case is that in which the new expressions stand in an inner relation to the already familiar expressions, like in the case of "theoretical concepts", such as mass and force, and their "observational basis".

Now the interesting point is that in the case of arithmetic the "new" number-expressions for the negative, rational, and irrational numbers also stand in certain, yet not completely clarified relations to the already "familiar" numbers, the positive whole integers. This is, of course, well known. But the remarkable point is now that Husserl set out to analyze these relations by distinguishing two cases with respect to an extension of the theory of arithmetic. In the first case, the addition of new axioms and terms permits a deduction of "new" *theorems* in the "old" language for the domain of the natural numbers, which could not be deduced in the unextended theory. In the second case, such a deduction of new theorems in the old language is *not possible*: the *Mannigfaltigkeit* of valid formulas in the old language is not changed by the addition of new axioms. This kind of theory-extension is called "conservative" – in distinction to the first "creative" type of theory-extension – because it keeps the set of theorems in the old restricted language invariant, i.e. constant; in particular the set of valid equations between the natural numbers is not changed by the new axioms. Husserl has put great emphasis on this distinction because he was convinced that he could bridge the gap between the second and third level of the theory of numbers by sticking to the idea of conservative theory-extension. The leading thought is this: If the theory extension is conservative then the new axioms do not change the "meaning" of the old terms because the new introduced elements (the negative, rational and irrational numbers) do not "interfere" with the old ones, the whole positive integers; they only join them in order to complete and "close" the domain with respect to the 4 arithmetical operations. Whether Husserl was right, I will not discuss here. But it goes without saying that from a philosophical point of view the meta-mathematical notion of a conservative theory-extension has a great deal of attraction for the gapless and rigorous foundation of arithmetic.

I will not close without a brief sketch of Husserl's philosophical position between the two poles of Frege and Hilbert. The best way to characterize the relation between Husserl and Frege is perhaps to borrow a phrase from the philosophy of science: Both pursued similar but different *research programs*, which stood in competition with each other. Both aimed at a firm and gapless foundation of arithmetic. Because Frege identified the principal gaps with errors arising from the use of words from ordinary language in mathematical proofs, he tried to invent a concept-script that could be substituted for ordinary language. But this was not Husserl's point of view – at least not before 1900 – and for this reason he showed little interest in Frege's logical notation. Husserl instead identified the main gaps with the problem of the expansion of the Ur-domain of natural numbers to the larger domains of negative, rational and other numbers in connection with the arithmetical operations. Therefore he tried to find a method to close the gaps by meta-mathematical considerations and the generation of meta-mathematical concepts. The method of conservative theory-extension seemed to him the appropriate instrument. In this connection two points are very important. Husserl's turn from psychologism to Frege's anti-psychologism in 1894 did not affect his research program regarding a gapless *Fundierung* of arithmetic, let alone change his eventual solution. On the other hand Frege didn't recognize that Husserl's approach in the *Philosophie der Arithmetik* was not a psychological but a "genetic" one (as Weyl correctly noticed). Mainly for this reason, I think, Frege could not imagine that Husserl's research program might

be successful, in particular when Husserl eventually adopted Hilbert's meta-mathematical point of view. The latter was so fundamentally different from Frege's own point of view that he could not appreciate its merits.

Notes

[1] Bei jenem Vorwurfe ist aber dies hauptsächlich übersehen, dass mein Zweck ein anderer als Booles war. Ich wollte nicht eine *abstracte Logik* in Formeln darstellen, sondern einen *Inhalt* durch geschriebene Zeichen in genauerer und übersichtlicherer Weise zum Ausdruck bringen, als es durch Worte möglich ist. Ich wollte in der Tat nicht einen blossen "*calculus ratiocinator*", sondern eine "*lingua characterica*" im leibnizschen Sinne schaffen, wobei ich jene schlussfolgernde Rechnung immerhin als einen notwendigen Bestandtheil *einer* Begriffsschrift anerkenne. Wenn dies verkannt wurde, soliegt das vielleicht daran, dass ich in der Ausführung das *abstract Logische* zu sehr in den Vordergund habe treten lassen. (*italics* are mine, except the Latin words in quotation marks)

[2] "Man kann in den arithmetischen, geometrischen, chemischen Zeichen Verwirklichungen des Leibnizischen Gedankens [einer allgemeinen Charakteristik] für einzelne Gebiete sehen. Die hier vorgeschlagene Begriffsschrift fügt diesen ein neues hinzu und zwar das in der Mitte gelegene, welches allen anderen benachbart ist. Von hier aus lässt sich daher ... eine Ausfüllung der Lücken der bestehenden Formelsprachen, eine Verbindung ihrer getrennten Gebiete zum Bereiche einer einzigen [BS] und eine Ausdehnung auf Gebiete ins Werk setzen, die bisher einer solchen [BS] ermangelten." (BS, p XII)

[3] "die Anzahl, welche dem Begriffe F zukommt ist der Umfang des Begriffes "gleichzahlig dem Begriffe F"."

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