ISSN 1420-8520 Juillet 2012 18

TRAVAUX DE LOGIQUE

Université de Neuchâtel unine

Contemporary Perspectives on Logicism and the Foundation of Mathematics

Edited by
Pierre Joray

(2e édition)

CdRS

Centre de Recherches Sémiologiques
Travaux de logique
N° 18 – janvier 2007

Contemporary Perspectives on Logicism and the Foundation of Mathematics

Edited by
Pierre Joray

CdRS

Université de Neuchâtel

Centre de Recherches Sémiologiques
Université de Neuchâtel
Espace Louis-Agassiz 1
CH-2000 Neuchâtel (Switzerland)

Contents

Foreword

In 2003, the Swiss National Science Foundation decided to give its support to a research programme devoted to logicism at the Institute for Logic of the University of Neuchâtel. Continuing Neuchâtel's tradition in the study of Polish logic, the aim of that project was the construction of Peano-Dedekind arithmetic within the framework of S. Leśniewski's logic. After the publication of the technical and first philosophical results of this programme[1], it was time to organise an international conference about logicism and its future. The conference held at the University of Neuchâtel in April 2005 under the title "Contemporary Perspectives on Logicism" and was associated with the symposium "Constructivism" organised by the Swiss Society for Logic and Philosophy of Science. The speakers of both events were: Jean-Yves Béziau (Neuchâtel University), Laura Crosilla (University of Florence), Dirk van Dalen (Utrecht University), Wolfgang Degen (Erlangen University), Cédric Degrange (Neuchâtel University), Jacques Dubucs (Université de Paris 1), Nadine Gessler (Neuchâtel University), Jean-Pierre Ginisti (Université de Lyon 3), Bob Hale (University of Sheffield), Pierre Joray (Université de Rennes 1), Mathieu Marion (Université du Québec à Montréal), Per Martin-Löf (University of Stockholm), Hartley Slater (University of Western Australia), Göran Sundholm (University of Leiden), Klaus Thiel (LMU München) and Anna Zielinska (Université de Grenoble 2).

This issue of the *Travaux de Logique* is a partial publication of the papers presented in Neuchâtel, including also the contributions of Philip Ebert and Marcus Rossberg (University of St. Andrews) and Luca Incurvati (University of Cambridge), who were not able to join the conference. I must precise that the participants whose papers do

[1] See, in particular, *Travaux de logique* 16 (2005).

not appear here decided, for various reasons, to publish by other means, the quality of their work being thus absolutely not in question.

Contrary to quite a common opinion, logicism and reflexions about the foundations of mathematics are not only of historical interest. In spite of the failure of classical logicist programmes and the discovery of important internal limitations of formalisms, foundationalist projects have been the source of hudge developments and progress in logic and the philosophy of mathematics. I hope the reader will see in this volume how logicism and foundationalism, today usualy far from any strong reductionist thesis, still form a living and stimulating research area.

As the former director of Neuchâtel's research programme and the organizer of the conference, I would like to thank the authors for the quality of their contributions and the richness of their participation to the scientific discussions. It is also a pleasure for me to express my gratitude to my collaborators, Nadine Gessler and Cédric Degrange, and especially to Denis Miéville, the Director of the Institute for Logic. Without his constant and friendly support, the project would not have been this successful. Let me also thank for their decisive financial contributions the Swiss National Science Foundation, the University of Neuchâtel, the Centre Romand de Logique, Histoire et Philosophie des Sciences and the Swiss Society for Logic and Philosophy of Science.

Pierre JORAY

UFR de Philosophie de l'Université de Rennes 1
Membre associé du Centre de Recherches Sémiologiques
de l'Université de Neuchâtel

Snapshots from Brouwer's Universe

Dirk van Dalen

Almost a century ago, Brouwer launched his first intuitionistic programme for mathematics. He did so in his dissertation of 1907, where he formulated the basic act of creation of mathematical objects, known as the *ur-intuition* of mathematics. Mathematics, in Brouwer's view, was an intellectual activity of men (of the *subject*), independent of language and logic. The objects of mathematics come first in the process of human cognition, and description and systematization (in particular logic) follow later. The formulation of the ur-intuition is somewhat hermetic, but in view of its fundamental role, let us reproduce it here.

> Ur-intuition of mathematics (and every intellectual activity) as the substratum, divested of all quality, of any perception of change, a unity of continuity and discreteness, a possibility of thinking together several entities, connected by a 'between' that by the interpolation of new entities never gets exhausted.

As we see, Brouwer sees the ur-intuition as the genesis of both the discrete part of mathematics, let us say, the natural numbers, and of the continuous part, i.e. the continuum. Neither of these can be reduced to the other.

A more refined analysis was given in the Vienna lectures (although it is foreshadowed in the so-called 'rejected parts' of the thesis), where the notion of the falling apart of a moment of life is introduced. In the final presentation, *Consciousness, Philosophy and Mathematics* (CPM), [Brouwer 1949a], this phenomenon is described as the *move of time*: 'By a move of time a present sensation gives way to another present sensation in such a way that consciousness retains the former one as a past sensation and moreover, through this distinction between present and past, recedes from both and from stillness and becomes mind.' Thus the subject has created a 'twoity' of a past and present sensation. The process evidently can be iterated, and complexes and strings of sensation become the object of attention. The sensation complexes form a bewildering mixture, in which a certain order is introduced by the *causal attention.* This carries out a process of *identification.* One may think of the identification of 'similar' complexes, or of *abstraction.* By abstracting from all accidental features of twoities, the *empty twoity* is obtained. In other words, by identifying all twoities one obtains the object where only order and distinction are recognized. This empty twoity then can take the place of the number 2. From there it is not difficult to generalize to the individual natural numbers, and the next step — the recognition of the iteration of the 'next number' step as a legitimate mental construction, together with the corollary, the (potentially infinite) set of natural numbers — is mentioned in passing by Brouwer . He speaks of 'unlimited unfolding' (CPM, p. 1237).

Thus the basic material of 'discrete mathematics' is at the disposition of the subject. This part of the process of creating is later called *the first act of intuitionism.* We should note that the aspect of simultaneous creation of discrete and continuous, is played down, but as late as the Vienna lectures (1928) Brouwer pointed out that both acts of intuitionism are grounded in the ur-intuition. The continuum is given in the move-of-time act as the 'between'. In his Rome lecture (1908) Brouwer explicitly points out that 'the first and the second are thus kept together, and the intuition of the continuous (continere = keeping together) consists of this keeping together'. And he adds: 'This mathematical ur-intuition is nothing but the contentless abstraction of the sensation (experience) of time'. Time is thus created by the subject through the 'move of time', together with the continuum and the natural numbers. *The second act of intuitionism* is the creation of 'more or less freely proceeding infinite sequences of mathematical entities previously acquired' and of 'species', i.e. 'properties supposable for mathematical entities previously acquired'.

In CPM the two acts are tacitly lumped together under the act of 'unlimited unfolding'. The process of creation of causal sequences and complexes does extend beyond the realm of mathematics; indeed the physical world, as well as the social one is made up of those objects. If we look for a moment at the physical phenomena, then we can see the role of mathematics as follows. The objects of the physical world are obtained by abstraction from sensation complexes, a further abstraction gets the subject to mathematical objects and structures. And hence there is a natural connection between the physical universe and the mathematical, something like a projection. Although this does not explain the success of mathematics in full, it shows that the connections do not come out of the blue.

By and large, the above sketches the genesis of Brouwer's mathematical universe. In the dissertation Brouwer goes to great lengths to determine the possible sets in mathematics on the basis that there are no sets but those we can create ourselves. After the introduction of choice sequences (cf. the second act) he revised his views. The extent of the mathematical universe is modest compared to the traditional Cantorian universe, from a classical point of view, Brouwer's universe does not get beyond ω_1. But what it lacks in 'height' is compensated by the extra fine structure which is inherent to the intuitionistic approach (and its logic).

The most spectacular part of the universe is the second-order part, let us say second-order arithmetic with sequences, species, or both. Where the first-order part yields more-or-less a subtheory of classical arithmetic, the second-order part has certain specific properties that are incompatible with classical mathematics.

We will look at a few of these principles. The first and most striking principle was introduced by Brouwer in his courses on pointset theory of 1915-1917. The principle appeared in print in 1918, in modern formulation it reads 'A mapping F from choice sequences to natural numbers has the property that each $F(\alpha)$ is determined by an initial segment $\overline{\alpha}k$ $(= (\alpha 0, \alpha 1, \alpha 2, \ldots, \alpha(k-1))$

Formalized: $\forall\alpha\exists x\forall\beta(\overline{\alpha}x = \overline{\beta}x \rightarrow F(\alpha) = F(\beta))$

The principle finds a more general form in the *Principle of weak continuity*

$$WC \qquad \forall\alpha\exists x A(\alpha, x) \rightarrow \forall\alpha\exists x\exists y\forall\beta(\overline{\alpha}y = \overline{\beta}y \rightarrow A(\beta, x))$$

Brouwer formulated his functional version in a proof, giving no argument for it. A first attempt at a justification could run as follows: in order to compute the natural number $F(\alpha)$ a finite number of steps is required; when the computation is finished only finitely many members of the sequence α have been generated, and so only this initial segment enters into the computation. Hence any sequence β with the same initial segment yields the same value under F. This argument only works in the case that only numerical information of α is used. In general, however, information of a different kind may be used.

Here is an example, formulated as a game (Brouwer introduced game formulations in his Groningen Lectures, 1930). There are two players, I and II. I provides successively information about α and II has an algorithm for computing $F(\alpha)$. At each step II may ask for more information or show the output. In our example II simply takes $F(\alpha) = \alpha(100)$

	I	II
0	7	?
1	2	?
2	301	?
⋮	⋮	⋮
13	5 and α becomes stationary	$F(\alpha) = 5$

Note that I may (and perhaps *must*) give more information than just the numerical values of α. Indeed, if one accepts the idea of mathematics as a solitary play of the subject, then I and

II are no more than puppets controlled by the subject. Thus the availability of full information is obvious.

Now there obviously are β's with the same initial segment $\overline{\beta}14 = \overline{\alpha}14 = (7, 2, 301, \ldots, 5)$ with $F(\beta) \neq 5$. This failure of the simple argument is caused by the fact that suddenly a condition of a higher order is put on α. And higher order conditions cannot be avoided, if only because one wants to allow lawlike sequences (think of the difference between the decimals of π and those determined by flipping a coin). Hence a better argument is required. One was provided by Mark van Atten in a setting which slightly, but justifiably, extended Brouwer's framework. He showed that higher order conditions could not qualify as inputs for the computation, see [van Atten – van Dalen 2002]. The analysis lays down certain conditions on the class of sequences for the validity of the continuity principle. The principle is in fact justified for the holistic universe, but we can see that there is a new problem for research: for which universes does *WC* hold? A simple example of a universes that violates the continuity principle is the one in which each sequence eventually becomes constant. The function F assigns this constant value to α; F is obviously not continuous. There is a rich literature on the continuity principle, see for example [van Dalen–Troelstra 1988a and 1988b]. The continuity principle has striking consequences in everyday mathematics e.g. *Brouwer's continuity theorem - all real functions are continuous* and the *indecomposability of the continuum - $\mathbb{R}$ cannot be split into two non-empty parts.* Both results confirm the above mentioned incompatibility, in particular the latter shows that the principle of the excluded middle is false: $\neg\forall x \in \mathbb{R}(x = 0 \vee x \neq 0)$

A further analysis, making use of transfinite principles (*the principle of Bar Induction*, established the *bar theorem, the fan theorem*, and the *locally uniform continuity theorems* (real functions on intuitionistically compact subsets of $\mathbb{R}$ are uniformly continuous). For the practical consequences of these properties of Brouwer's universe see [van Dalen–Troelstra 1988a and 1988b].

So far the treatment of the universe was completely uniform, but in the twenties Brouwer started to make the distinction between the lawlike and the full continuum. Equivalently, between the set of lawlike sequences and the set of (all) choice sequences. Historically speaking, there was a perfect reason to do so. When dealing with infinite processes algorithms are the first things that come to mind, for the law is the thing that guarantees infinite continuation. The first Brouwerian counterexamples, were, not surprisingly, based on an algorithm: the decimal expansion of π. However, once choice sequences were recognized by him as legitimate objects (the subject is free to make choices), it was natural to look for a counterpart of the (lawlike) Brouwerian counter examples where one uses a decidable property of a lawlike sequence, which has neither been proved, nor rejected. One should fully exploit the choice-character of sequences in the hope of exploiting the properties of the full Brouwerian universe. In 1927 there are the first signs of the new method, which was published some twenty years later, and which goes by the name of the 'creating subject'. The underlying idea is that the subject investigates some particular property, while he carries out a convenient bookkeeping at the same time: if at moment n A has not yet been established, put down a 0, otherwise a 1. Brouwer uses the expression 'the creating subject experiences the truth of A'. Here it is tacitly assumed that 'the creating subject experiences the truth or he does not', the simple argument being that 'in doubt, one does not experience the truth'. A reasonable assumption. In view of the fact that the ur-intuition, in its function as a time-measuring and -introducing principle, provides the subject with a sequence of moments ordered like the natural numbers, the time parameter n is a natural one. The effect of the activity of the creating subject is that a choice sequence α is in the following way associated to a proposition A:

$$\exists\alpha(A \leftrightarrow \exists x(\alpha x \neq 0))$$

This formalization of Brouwer's argument is due to Kripke and is called *Kripke's Schema, KS* Note that KS is an extra condition on the richness of the Brouwerian universe. It asserts the existence of particular sequences, compare the role of the axiom of choice. Thus it is not automatically seen that the old principles still hold. It has in fact been shown that KS is consistent with most principles. Kreisel formulated an interesting 'tensed modal' extension of the existing theories which captures the properties of the creating subject, and which is equivalent to the extension by *KS* [Kreisel 1967], [van Dalen 1978].

The classically inclined logician will note that KS is a very weak comprehension principle, which is provable in the classical setting. So whatever strength one can expect from KS, it has to come from suitable extra principles, such as the continuity principle.

We will now proceed to show a number of consequences of KS in practical mathematics, consequences which are not mere curiosities, but which make manifest certain features of the universe one would expect, and some unexpected phenomena to boot. The proofs are carried out under the assumption of the continuity principle and Kripke's Schema. It turns out to be convenient to reformulate Kripke's Schema, such that there is at most one 1 in the sequence $\alpha : \forall x(\sum_{y\leq x}\alpha(y) \leq 1)$. Let us call such a sequence satisfying $A \leftrightarrow \exists x(\alpha x = 1)$, a *Kripke sequence for A.*

(1) $\neg\forall xy \in \mathbb{R}(x \neq y \rightarrow x \# y)$

(2) $\neg\forall xy \in \mathbb{R}(\neg\neg x < y \rightarrow x < y)$

(2) was shown by Brouwer in [1949b], and (1) follows by a completely similar argument.

(3) *The Principle of $\forall\alpha\exists\beta$-continuity fails*, [Myhill 1966].

Proof: We apply KS to $\forall x(\alpha(x) = 0)$: $\exists\beta(\forall x(\alpha(x) = 0 \leftrightarrow \exists y(\beta(y) = 1))$. Hence $\forall\alpha\exists\beta(\ldots)$; by $\forall\alpha\exists\beta$-continuity there should be a continuous functional G: $\mathbb{N}^{\mathbb{N}} \rightarrow \mathbb{N}^{\mathbb{N}}$ such that $\forall\alpha((\forall x(\alpha(x) = 0 \leftrightarrow \exists y(G(\alpha)(y) = 1))$. Hence we have a continuous functional G testing if an α is the zero-sequence $\mathbf{0}$. I.e. G is $\mathbf{0}$ on all sequences distinct from $\mathbf{0}$, and non-zero on $\mathbf{0}$. This functional is clearly discontinuous.

Note that therefore there is a real foundational choice to be made here: adopt KS or $\forall\alpha\exists\beta$-continuity, but not both.

(4) *All negative dense subsets of* $\mathbb{R}$ *are indecomposable.*

By a negative subset X we mean one for which $X = X^{cc}$ (in particular the complement of a set is negative).

Proof. This theorem follows from two lemma's. Let X be negative and dense in $\mathbb{R}$.

(4.1) If $X = A \cup B$, with $A \cap B = \emptyset$, then converging sequences (a_i) and (b_i) in respectively A and B cannot have the same limit.

Assume $\forall k \exists n \forall m(|a_{n+m} - b_{n+m}| < 2^{-k})$. We consider the Kripke sequences α for $r \in \mathbb{Q}$ and β for $r \notin \mathbb{Q}$, where r is an arbitrary real number.

We define new sequences γ and c_i by

$$\begin{cases} \gamma(2n) & = & \alpha(n) \\ \gamma(2n+1) & = & \beta(n) \end{cases} \quad \text{and} \quad \begin{cases} c_{2n} & = & a_n \\ c_{2n+1} & = & b_n \end{cases}$$

Now we introduce a new sequence (d_i)

$$d_n = \begin{cases} c_n \text{ if } \forall k \leq n(\gamma(k) = 0) \\ c_k \text{ if } k \leq n \text{ and } \gamma(k) = 1 \end{cases}$$

Claim: $d \in X$.
If $d \notin X$, then $d \notin A$; hence (d_n) does not become stationary in A. So $\alpha(n) = 0$ for all n. And by the definition of Kripke sequence we get $r \notin \mathbb{Q}$.
Similarly $d \notin B$; hence (d_n) does not become stationary in B. Therefore $\beta(n) = 0$ for all n, and thus $r \notin \mathbb{Q}$. Contradiction.
So $\neg\neg d \in X$. But since X is negative, we find $d \in X$.

As $X = A \cup B, d \in A \vee d \in B$. If $d \in A$ then (d_n) does not become stationary in B, hence $\forall n \beta(n) = 0$. By the definition of β this implies $\neg\neg r \in \mathbb{Q}$. A similar argument shows that $\neg r \in \mathbb{Q}$ if $d \in B$. As a result we get $\neg r \in \mathbb{Q} \vee \neg\neg r \in \mathbb{Q}$. As r was an arbitrary real, we have established $\forall r \in \mathbb{R}(\neg r \in \mathbb{Q} \vee \neg\neg r \in \mathbb{Q})$, which contradicts the indecomposability of $\mathbb{R}$. Therefore $lim(a_n) \neq lim(b_n)$.

(4.2) If the above sets A and B are inhabited (i.e. contain an element), then there are sequences in A and B converging to the same point.
The proof is a piece of elementary analysis, see [van Dalen 1999].

Conclusion: X is indecomposable.

This theorem shows that there are lots of indecomposable subsets of the continuum, for example the irrationals, $\mathbb{Q}^c$, and the not-not-rationals, $\mathbb{Q}^{cc}$. The continuum is clearly extremely 'connected'; even if we punch holes in it, it still remains indecomposable. Note that classically $\mathbb{Q}^c$ is *not* connected. It is even zero-dimensional. Intuitionistically it has dimension 1. The moral is that the intuitionistic continuum is very tight, and that its topology will offer unknown surprises and difficulties.

(5) *The powerset of* $\mathbb{N}$ *exists.*

More precisely: each subset of $\mathbb{N}$ can be represented by a suitable $0-1$ choice sequence.

The basic idea of the proof is that, given a subset X there is for each n a Kripke sequence α_n such that $n \in X \leftrightarrow \exists x(\alpha_n(x) = 1)$ All these α_n's can be glued together to form one α that tests membership for X. For the technical details, see [van Dalen 1975].

(6) *If $\mathbb{R}$ is indecomposable, then there are no discontinuous functions*, ([van Dalen 2001]).

The converse is obvious, and it allows one to conclude the indecomposability on the basis of Brouwer's negative version of the continuity theorem (cf. [Brouwer 1927]).

Proof. Let f be discontinuous, say in 0. It is no restriction to assume $f(0) = 0$. Then:

$\exists k \forall n \exists x(|x| < 2^{-n} \wedge |f(x)| > 2^{-k})$.

After determining k we can find a sequence (x_n) with $|f(x_n)| > 2^{-k}$ and $|x_n| < 2^{-n}$.

Let α and β again be Kripke sequences for $r \in \mathbb{Q}$ and $r \notin \mathbb{Q}$. Put

$$\begin{cases} \gamma(2n) = \alpha(n) \\ \gamma(2n+1) = \beta(n) \end{cases} \quad \& \quad c_n = \begin{cases} x_n \text{ if } \forall k \leq n(\gamma(k) = 0) \\ x_k \text{ if } k \leq n \;\&\; \gamma(k) = 1 \end{cases}$$

(c_n) converges, say to c. As $0 < 2^{-k}$, we get:

$f(c) < 2^{-k} \vee f(c) > 0$.

If $f(c) < 2^{-k}$, then $f(c) = 0$, so $\forall p(\gamma(p) = 0)$, which is impossible. So $f(c) > 0$, and therefore $r \in \mathbb{Q} \vee r \notin \mathbb{Q}$. As before we see that this yields a non-trivial decomposition of the continuum. Contradiction.

This result establishes an equivalence between a certain characteristic of a function and the nature of its domains. Results of this kind are familiar from recursion theory and descriptive set theory.

In our description of Brouwer's universe we have discussed a few basic principles which have unusual consequences in practical mathematics. One of the challenges of constructive mathematics, is to find new principles that embody certain specific phenomena that shed new and unexpected light on the universe. Markov's principle is one of those principles, but unfortunately, one cannot justify it on the basis of a strong notion of 'constructive'. Kripke's schema is a good candidate. What we need is more experience with its applications, furthermore it would be desirable to find a realistic mathematical principle equivalent to KS, in the tradition of reverse mathematics.

References

[van Atten – van Dalen 2002] M. van Atten, D. van Dalen. Arguments for the Continuity Principle. *Bull. Symb. Logic*, 8, 329–347.

[Brouwer 1927] L.E.J. Brouwer. Über Definitionsbereiche von Funktionen. *Mathematische Annalen*, 97, 60–75.

[Brouwer 1949a] L.E.J. Brouwer. Consciousness, Philosophy and Mathematics. *Proceedings of the 10th International Congress of Philosophy, Amsterdam 1948*, 3, 1235–1249.

[Brouwer 1949b] L.E.J. Brouwer. De non-aequivalentie van de constructieve en negatieve orderelatie in het continuum. *Indagationes Mathematicae*, 11, 37–39. Transl. "The non-equivalence of the constructive and the negative order relation in the continuum" in CW 1, 495–496.

[van Dalen 1975] D. van Dalen. The use of Kripke's schema as a reduction principle. *Journal of Symbolic Logic*, 42, 238–240.

[van Dalen 1978] D. van Dalen. An interpretation of intuitionistic analysis. *Ann Math Log*, 13, 1–43.

[van Dalen 1999] D. van Dalen. From Brouwerian Counter Examples to the Creating Subject. *Studia Logica*, 62, 305–314.

[van Dalen 2001] D. van Dalen. Intuitionistic Logic. In L. Gobble, editor, *Philosophical Logic*, Oxford, Blackwell, 224–257.

[van Dalen–Troelstra 1988a] D. van Dalen, A.S. Troelstra. *Constructivism in Mathematics. Volume 1.* Amsterdam, North-Holland.

[van Dalen–Troelstra 1988b] D. van Dalen, A.S. Troelstra. *Constructivism in Mathematics. Volume 2.* Amsterdam, North-Holland.

[Kreisel 1967] G. Kreisel. Informal rigour and completeness proofs. In A. M̃usgrave, I. L̃akatos, editors, *International Colloquium of Philosophy of Science*, Amsterdam, North-Holland, 138–186.

[Myhill 1966] J. Myhill. Notes towards an axiomatization of intuitionistic analysis. *Logique et Analyse*, 9, 280–297.

Variations of Frege's Grundgesetze

J. W. Degen

Abstract

We retain the Grundgesetz V, but replace impredicative comprehension with certain ramified comprehension principles.

1 Introduction

If f is a unary function we write $x.f(x)$ for its Werthverlauf, which is a Gegenstand (object). In this notation, Frege's Grundgesetz V is

$$(GG\ V): \ x.f(x) = y.g(y) \leftrightarrow \forall z\,(\,f(z) = g(z)\,)$$

The operation $f \mapsto x.f(x)$ is according to the left-to-right direction ($GG\ V \ \rightarrow$) of the Grundgesetz ($GG\ V$) an injection from the class of functions into the class of objects. Therefore, if we have at least two objects and enough functions in the domain of function quantifiers, we get a contradiction, as was shown by Cantor around 1874. Frege (perhaps) learned that Cantorian cause of the inconsistency of his *Grundgesetze der Arithmetik*

(GGA) [5] from Russell in 1902, although the Nachwort to [5] 1903 indicates that Frege did not completely get the point.

Thus the question arises: what should be done with Frege's *GGA*? The best advice is to throw *GGA* out of the window since all what Frege intended to do with *GGA*, can surely be done by other systems which are more elegant and probably consistent. More precisely, the kind of mathematics Frege wanted to capture in *GGA*, or *begründen* (as the Germans like to say), does not transcend a certain version of PA^3, i.e. third-order Peano Arithmetic.

However, there remain several things to say about the relation $x.f(x) = a$ itself which was operative in forming Frege's ideas about the mathematical universe, ideas which are, at least according to standard conceptions, quite incorrect; the rest of *GGA* is just a two-sorted quantification theory.

The relation $x.f(x) = a$ may be read as: the object a *represents* the function f, or as: the function f *reduces or is contracted to* the object a. That is to say, a thing of a higher type, like the function f is contracted to a thing of a lower type, viz. the object a. We shall use the term *contraction*, because it is vague and unspecific enough. For Frege, functions and especially his *Begriffe*, seemed to belong to his famous Realm of the Objective Unreal, whereas the Werthverlauf $x.f(x)$ of the function f was perhaps imagined by Frege, at least in a case like $\lambda x.x^2 : [0,1] \to [0,1]$, as the graph of f which can be written down on the blackboard as an object consisting of chalk particles and should thus belongs to the Realm of the Objective Real.

With hindsight we can say that both Frege and Russell had a *type theory*; both wanted to found mathematics (or at least large parts of it) on a so-called *logic of types*. While Frege had just two types, objects and functions, his logic of types aimed to create mathematical strength by contracting functions from objects to objects — to objects, in some reversible or reconstructible, perhaps injective way. That's precisely the task of $(GG\ V)$.

Russell, on the other hand, tried to obtain mathematical strength by introducing more and more types, e.g. after the type 0, the type of objects, for each type n the type $n+1$ of predicates of things of type n. Because of some type-theoretic *horror infiniti*, he did not want to have infinite ordinals ω, $\omega+1$, $\omega^2, \ldots$ as types.

This is all that I want to say about the *difference* between Fregean and Russellian logicism. Observe, that the types 0,1,2,3 are more than enough for ordinary mathematics *provided* one has an axiom of infinity and some not too weak principles of comprehension. There are at least two possibilities, viz. in my papers [3] and [4], respectively, to give something like a *purely logical proof* of the axiom of infinity.

Therefore, I think that Russell's logicism can be and has been vindicated.

We shall now go back to Frege's logicism and explore possible vindications of it. Perhaps Frege's logicism is already vindicated by replacing $(GG\ V)$ with Hume's Principle for cardinalities. Here too we have a kind of contraction relation: a concept F is contracted to its cardinality $\#_x F(x)$, i.e. $\#_x F(x) = c$, where c is an object.

$$(\textit{hume card}): \quad \#_x F(x) = \#_y F(y) \leftrightarrow F \textit{ equipollent to } G$$

The consistency, mathematical strength, and elegance of Hume's Principle for cardinalities are admirable facts which should be elaborated further. For instance, one should work out subsystems, e.g. predicative ones, of the system with (*hume* *card*). By the way, $\forall y \exists F : \#_x F(x) = y$ can be consistently added. What about third-order extensions[1], etc? Moreover, one should consider versions of Hume's Principle for cardinals in the context of set theory, e.g.

$$(set\ hume[A]) :$$
$$\exists H : P(A)\backslash\{\emptyset\} \to A \, \forall X, Y \; [\; H(X) = H(Y) \leftrightarrow X \, equip. \, Y \;]$$

For instance, $ZFC \vdash \forall Z \, (set\ hume[Z])$, but on the other hand $ZF \nvdash \forall Z \, (set\ hume[Z])$ (consider amorphous sets).

I have played around with a Hume's Principle for ordinal numbers, without any success.

Observe, that Frege defined the humean contraction relation $\#_x F(x) = c$ in terms of his primitive and fundamental contraction relation $x.f(x) = a$. Thus we go back to $x.f(x) = a$.

First we generalize (or weaken) the idea inherent in $x.f(x) = a$ as follows. Our formal language will be syntactically simpler than Frege's but of equal expressiveness.

We let capital letters $A, B, \ldots$ (*free*) and $X, Y, \ldots$ (*bound*) range over *predicates* (or notions or classes or properties or whatever you may call these things); and small letters $a, b, \ldots$, $x, y, \ldots$ range over (so-called) individuals, or *objects*. Our primitive relations are (1) $B(a)$ [*predication*] with the usual intended meaning, and $A \searrow b$ [*contraction*] whose meaning is yet to be found. We only know that $A \searrow b$ should mean that the predicate A reduces or is contracted to the object b. Although equality between individuals could be defined (e.g. by $a = b \; := \; \forall X(X(a) \to X(b))$, we adopt also the primitive relation $a = b$.

It is a sad fact that we have no clear ideas at all about

[1]Here there is a "plausible" version which implies $\neg CH$

possible axioms for the relation $\searrow$. We have to make experiments and look for the results. Let me illustrate our situation by considering the following axiom

$$(sub) :: \quad \exists x : A \searrow x \wedge \forall z(B(z) \to A(z)) \to \exists y : B \searrow y$$

Is (sub) a plausible axiom? Suppose that A is a recursive set of natural numbers, and A can be contracted to a natural number x (which codes a definition of A). But if the subset B of A is not even hyperarithmetic, why should B be contractible to any number y at all? Moreover, if B contracts to the number y, then not necessarily by the same mechanism that contracted A to the number x.

Two further question. (1) If not every predicate can be contracted, then which predicates can? (2) How much of the logical (internal) structure of a predicate is permitted to vanish during contraction?

2 On the real nature of Russell's paradox in *GGA*

I will now turn to an account of Russell's paradox in *GGA* which may be not wholly faithful to what really happened when Russell derived his paradox from *GGA*. But I will show that it should have been the way I shall describe. In particular, I sacrifice historic faithfulness in order to better discuss both the *contraction* relation $A \searrow b$ and the issue of *predicativity*.

Frege's $(GG\ V)$ reads in our notation:

$$(gg\,5) \quad \exists x\,(\,A \searrow x \wedge B \searrow x\,) \leftrightarrow \forall x\,(\,A(x) \leftrightarrow B(x)\,)$$

Together with (*gg* 5), the following *impredicative* case of comprehension engenders a contradiction, using in addition only pure logic.

$$(russell)\quad \exists Y \forall x\,(\,Y(x) \leftrightarrow \neg\exists Z\,(\,Z \searrow x \wedge Z(x)\,)\,)$$

Now, we can see that (*gg* 5) and (*russell*) are consistent separately. To repeat: as an isolated formula of our Fregelike language, Frege's (*GG V*) is consistent; and, what is perhaps also unknown, Russell's paradoxical formula, again as the isolated formula (*russell*) of our Fregelike language, is equally consistent: interpret it simply as

$$\exists y \forall x\,(\,x \in y \leftrightarrow \neg\exists z\,(\,x \in z \wedge x \in z\,)\,)$$

Of course, the set y is the empty set $\emptyset$.

Thus, the formula (*russell*) *is definitely not* the (trivially inconsistent) formula $\exists y \forall x\,(\,x \in y \leftrightarrow \neg x \in x\,)$.

And the formula (*gg* 5) transforms into the satisfiable formula $\exists x(a = x = b) \leftrightarrow \forall x(a = x \leftrightarrow b = x)$.

Digression. A not wholly trivial but purely logical derivation of a contradiction from (*gg* 5) and (*russell*) runs as follows; it uses only intuitionistic laws [2]:

Using (*russell*), let A be such that

(1) $\forall x\,(\,A(x) \leftrightarrow \forall Z \neg(\,Z \searrow x \wedge Z(x)\,)\,)$.

But $\forall x\,(\,A(x) \leftrightarrow A(x)\,)$ is logically true; by (*gg* 5) there is an a such that $A \searrow a$. Using (1) we have:

[2] This is of course no heroic deed, since if $\neg(\varphi \wedge \psi)$ is classically provable, then also intuitionistically.

$$A(a) \to \forall Z \neg (\, Z \searrow a \land Z(a)\,) \to \neg (\, A \searrow a \land A(a)\,).$$
But we have also $A(a) \to (\, A \searrow a \land A(a))$. Hence $\neg A(a)$. Using (gg 5) we get $A \searrow a \land B \searrow a \land B(a). \to .A(a)$. Hence by pure logic we get also $\neg A(a) \land A \searrow a. \to .\forall Z \neg (\, Z \searrow a \land Z(a)\,)$.

Applying (1) once more we have $\neg A(a) \land A \searrow a. \to .A(a)$. Then $A(a)$ follows by cuts. Thus we have derived both $\neg A(a)$ and $A(a)$.

We add the remark that we have not used the fact that $\searrow$ is total in the sense that $\forall X\, \exists z : X \searrow z$, whereas $\forall f\, \exists z : x.f(x) = z$ follows logically from $x.f(x) = x.f(x)$.

The main steps in our argument are buried in the Frege's book GGA vol. I, up to p. 75; these steps are used there to derive the general comprehension principle $a \in \{x : F(x)\} \leftrightarrow F(a)$, which Russell could use immediately for his paradox. It was thus Frege himself who did the lion's share in the derivation of Russell's paradox in GGA via the just given proof.

End of Digression.

In 1983 I defined in [1] a system called *praeKid* which has (besides some equality axioms) the axioms $\forall X\, \exists z : X \searrow z$, and ($gg$ 5), and the *strictly predicative* comprehension schema:

$$(praecomp) \quad : \quad \exists Y \forall x\, (\, Y(x) \leftrightarrow \mathcal{F}[x]\,)$$
$\mathcal{F}$ *has no bound predicate variables*

In *praeKid*, one can deduce besides some simple set-combinatorial laws eight of the nine axioms of Hailperin for Quine's NF, e.g. by using the definition

$$[Def\ \in] \quad a \in b := \exists X\, (\, X \searrow b \land X(a)\,)$$

for the (type-homogeneous) membership relation *which makes* *the individuals to sets*; this is incidentally Frege's own definition of the membership relation. Recall that the formula-part $\neg\exists Z\,(\,Z \searrow x \wedge Z(x)\,)$ in (*russell*) is really nothing else but $\neg x \in x$.

Another definition of the membership relation would be

$$[DEF\ \in] \quad A \in B \ := \ \exists x\,(\,A \searrow x \wedge B(x)\,)$$

which tries to *make the predicates into sets*. One gets the same theorems in *praeKid* under both definitions of the membership relation.

Unfortunately, I could not prove the consistency of *praeKid* yet; but I believe that I am missing just a trivial trick. Since *praeKid* proves

$$\forall u\,(\,\neg\exists Z(Z \searrow u \wedge Z(u)) \ \leftrightarrow\ \exists Z(Z \searrow u \wedge \neg Z(u))),$$

not even Δ^1_1-comprehension is consistent. This phenomenon is important with respect to the (to my mind) not yet fully elucidated status of predicativity. In the usual second-order arithmetic, Δ^1_1 is the limit of predicativity, although such comprehensions are already slightly impredicative. In *praeKid* the limit of predicativity is much lower, viz. Δ^0_∞-comprehension. Regarding first-order arithmetic under the standard definitions of $0, S, +, \times$, one gets in *praeKid* certain rich extensions of Robinson Arithmetic; the amount of induction we can get I have not yet determined.

Suppose that the principles of *praeKid* are *sound and even true*, then we have sound and true principles *that are contradictory with rather weak forms of impredicativity.* Within second-order arithmetic there are no sound principles which would, e.g., contradict Π^1_2-comprehension. Therefore, to make consistency proofs easier (more constructive) seems to be at present the only advantage of predicativity.

Notes. (1) The precise form of *praeKid* can be recaptured from the system $GGA[\Delta]$ below. — (2) By the way, if one *defines* a type-homogeneous (!) membership relation by making reference to more than one type instead of taking it as primitive, what has been done in the definitions [$Def\ in$] and [$DEF\ \in$], then it is an interesting philosophical problem *to elucidate those notions through which the membership relation is defined.*

We want now to give a more refined version of predicativity, viz. through *ramification by levels.* Levels are nothing else but ordinals.

Let Δ be a limit ordinal, like $\omega, \omega^2, \omega^\omega, \omega^{(\omega^\omega)} \ldots$, but not much larger than ϵ_0. Then we define a system $GGA_0[\Delta]$ with levels $< \Delta$ in the next section.

3 The system $GGA_0[\Delta]$ and its extensions

The letters $\alpha, \beta, \xi, \eta, \ldots$ range over ordinals $< \Delta$; these ordinals are called *levels.* We define the language $\mathcal{L}_\Delta$ as follows.

Object variables $a, b, \ldots x, y \ldots$ have no levels. Predicate variables of level ξ are denoted by $A^\xi, \ldots, Y^\xi, \ldots$. Next we assign levels to formulae and to more complicated predicate terms. We use λ as a symbol for predicate-abstraction, not for function-abstraction.

Every expression $a = b$ is a formula with level 0.

The formula $T^\alpha(a)$ and the formula $T^\alpha \searrow b$ have level α, where T^α is a predicate-term of level α.

Let $\mathcal{F}[a]$ be a formula of level α such that the bound variable

x does not occur in $\mathcal{F}[a]$. Then the predicate-term $\lambda x\mathcal{F}[x]$ has also level α; And the formulae $\forall x\mathcal{F}[x]$ and $\exists x\mathcal{F}[x]$ have both also the level α.

Applying $\neg$ does not change the level. In the case of $\wedge, \vee, \rightarrow$ we take the maximum as the new level.

If in the formula $\mathcal{F}[A^{\xi}]$ of level α the bound predicate-variable X^{ξ} does not occur, then the formulae $\forall X^{\xi}\mathcal{F}[X^{\xi}]$ and $\exists X^{\xi}\mathcal{F}[X^{\xi}]$ have both the level $\rho = max\{\xi + 1, \alpha\}$.

Besides the free object-variable $a, b, \ldots$ we may also introduce some constants, $k, l, k_1, \ldots$ for objects, and some n-ary function constants (for functions from objects to objects). We denote the arising object-terms by $s, t, \ldots$

We define now $GGA_0[\Delta]$ as a classical sequent calculus over the language $\mathcal{L}_\Delta$. We may also consider, of course, its intuitionistic subsystem.

(1) Initial sequents $\varphi \Longrightarrow \varphi$.

(2) The usual structural inference rules, including the cut rule.

(3) The usual rules for $\neg, \wedge, \vee, \rightarrow$ and for $\forall$ and $\exists$ *over objects.*

(4) The rules for λ introduction, viz.

$$(\lambda \Longrightarrow) \quad :: \quad \frac{\mathcal{F}[t], \Phi \Longrightarrow \Psi}{\lambda x\mathcal{F}[x](t), \Phi \Longrightarrow \Psi}$$

The right rule $(\Longrightarrow \lambda)$ is defined analogously.

(5) The rules for predicate-quantifiers.

$$(\forall pred \Longrightarrow) \quad \frac{\mathcal{F}[T^\alpha], \Phi \Longrightarrow \Psi}{\forall X^\beta \mathcal{F}[X^\beta], \Phi \Longrightarrow \Psi}; \quad \alpha \leq \beta$$

$$(\Longrightarrow \forall pred) \quad \frac{\Phi \Longrightarrow \Psi, \mathcal{F}[A^\beta]}{\Phi \Longrightarrow \Psi, \forall X^\alpha \mathcal{F}[X^\alpha]}; \quad \alpha \leq \beta$$

The rules for $\exists pred$ are dual. In $(\Longrightarrow \forall pred)$ the eigenvariable A^β is not allowed in the conclusion.

The *levels* are used *cumulatively* in the quantifier inferences because there are upward and downward shifts involved; but we have only two types: predicates and objects. [If we have more than two types; and if the *types* are taken *cumulatively* in the quantifiers inferences, then there is no (good) cut-elimination theorem, in contrast to Theorem 1 below. See e.g. [4] for this phaenomenon.]

The inference $(\Longrightarrow \exists pred)$, equivalently $(\forall pred \Longrightarrow)$ in classical logic, may be called a *ramified comprehension principle*; with its help and the help of some other rules we can derive

$$(ram\ comp) \quad : \quad \Longrightarrow \exists X^\beta \forall y\, (\, X^\beta(y) \leftrightarrow \mathcal{F}[y]\,)$$

when $\lambda y \mathcal{F}[y]$ has level α with $\alpha \leq \beta$.

Observe that we have in $GGA_0[\Delta]$ no rules for $=$, although we admit the symbol $=$ in the formulae of the language $\mathcal{L}_\Delta$.

Theorem 1. *If* $GGA_0[\Delta] \vdash S$, *then* $GGA_0[\Delta] \setminus \{cut\} \vdash S$

The proof of Theorem 1 is constructive and more or less as usual. If follows that $GGA_0[\Delta]$ is consistent.

How is $GGA_0[\Delta]$ related to Frege's *GGA*? If one takes away (*GG V*), and replaces the impredicative comprehension law with something like the inference ($\Longrightarrow \exists pred$) or the principle (*ram comp*), then one gets almost precisely the system $GGA_0[\Delta]$.

The system $GGA_0[\Delta]$ is just a logic within which one may formalize possibly nonlogical theories. Of course, some of these theories may also be called logics; we do not discuss in the present paper the distinction between *a logic* and *a theory formalized in a logic.*

To apply a sequent calculus to formulate a theory T one may state the axioms of T by sequents of the form $\Longrightarrow \varphi$, where φ contains not free variables. We now list a set of such axioms, called the *contraction laws.* We write simply φ for the sequent $\Longrightarrow \varphi$.

$(=): \;\; \forall x : x = x \;\wedge\; \forall x, y, z(x = z \wedge y = z \rightarrow x = y)$

$(cong()\,\alpha): \;\; \forall Z^\alpha, x, y\,(\,Z^\alpha(x) \wedge x = y \rightarrow Z^\alpha(y)\,) \quad \alpha < \Delta$

$(cong() \searrow \alpha) : \forall X^\alpha, Y^\alpha, z, u\,(\,X^\alpha \approx Y^\alpha \;\wedge\; X^\alpha \searrow z \;\wedge$
$Y^\alpha \searrow u \rightarrow z = u\,) \quad \alpha < \Delta$
where $A^\alpha \approx B^\alpha \;:=\; \forall x\,(\,A^\alpha(x) \;\leftrightarrow\; B^\alpha(x)\,)$

$(cong \searrow \alpha): \;\; \Longrightarrow \forall Z^\alpha, x, y\,(\,Z^\alpha \searrow x \wedge x = y \rightarrow Z^\alpha \searrow y\,)$
$\alpha < \Delta$

$(updown): \;\; \Longrightarrow \forall z\, \exists X^\alpha : X^\alpha \searrow z \;\wedge$
$\forall X^\alpha\, \exists z : X^\alpha \searrow z \quad \alpha < \Delta$

$(\searrow fun): \;\; \forall X^\alpha, y, z\,(\,X^\alpha \searrow y \;\wedge\; X^\alpha \searrow z \;\rightarrow\; y = z\,)$
$\alpha < \Delta$

$(rev \searrow fun): \forall x, Y^\alpha, Z^\alpha\, (Y^\alpha \searrow x \land$
$Z^\alpha \searrow x \to Y^\alpha \approx Z^\alpha)\ \alpha < \Delta$

And finally a version of $(GG\ V)$:

$(gg5\ \alpha)$:
$\forall X^\alpha, Y^\alpha\, [\exists z\, (X^\alpha \searrow z \land Y^\alpha \searrow z) \leftrightarrow X^\alpha(u) \approx Y^\alpha]\ \alpha < \Delta$

What these (infinitely many) axioms say seems to be very dangerous. They say that given a level α, the predicates of level α stand in an "isomorphic" relation with the objects. But the danger is only apparent. Consider these axiom as naked and isolated formulae of our language $\mathcal{L}_\Delta$ and interpret the contraction relation $\searrow$ and the predication relation ..(..) both as equality $=$, and all variables as ranging over a one-element domain. In particular, the most dangerous axiom $(gg5\ \alpha)$ simply becomes

$$\forall x, y\, [\exists z\, (x = z = y) \leftrightarrow \forall u\, (x = u \leftrightarrow y = u)]$$

We call the just described system $GGA[\Delta]$; it contains the previously mentioned system *praeKid*, when we use only predicates of level 0: every formula $\mathcal{F}[a]$ without predicate quantifiers has also level 0; therefore, $\lambda x \mathcal{F}[x]$ has also level 0, and we can prove (*praecomp*) in the form $\exists Y^0 \forall x\, (Y^0(x) \leftrightarrow \mathcal{F}[x])$. However, if we arrange our assignment of levels more strictly, e.g. by letting an object-quantifier $\forall x$ raise the level by 1, then *praeKid* seems to be at least not directly contained in the resulting system.

For getting intermediate systems between $GGA_0[\Delta]$ and $GGA[\Delta]$ we have to choose judiciously certain subsets of the contraction laws; here a wide field of experimentation opens before our eyes — and we may play with our intuition for the contraction relation $\searrow$.

Let me add some scattered remark about possible developments within $GGA[\Delta]$.

We define a *leveled membership relation* on the objects

$$[Def\ \in_\alpha] \quad a \in_{(\alpha)} b := \exists X^\alpha (X^\alpha(a) \wedge X^\alpha \searrow b)$$

Proposition 2. *If $\lambda x \mathcal{F}[x]$ has level α, then we can prove in $GGA[\Delta]$: $\lambda x \mathcal{F}[x] \searrow b \implies (a \in_{(\alpha)} b \leftrightarrow \mathcal{F}[a])$.*

Proof. Let $\lambda x \mathcal{F}[x] \searrow b$, and let B^α be such that $B^\alpha(a)$ and $B^\alpha \searrow b$. Then by $(gg5\ \alpha)$ and $(\forall pred \implies)$ we have $\forall z (\lambda x \mathcal{F}[x](z) \leftrightarrow B^\alpha(z))$. Applying the λ-rule we get, in view of $B^\alpha(a)$, the formula $\mathcal{F}[a]$.

The other way around. From $\mathcal{F}[a]$ we get $\lambda x \mathcal{F}[x](a)$. And then by $(\implies \exists pred)$ the formula $a \in_{(\alpha)} b$. QED

What can we do with our leveled membership relations? I do not yet know; I have yet to carry out some more experiments with them.

Using Proposition 2 we have also a leveled powerset schema.

$[powerset\ \alpha]$:
$$\forall u \exists y \forall x (x \in_{(\alpha+1)} y \leftrightarrow \forall z (z \in_{(\alpha)} x \rightarrow z \in_{(\alpha)} u))$$

Again by Proposition 2 we get things like

$$\bigwedge_{1 \le i < j \le n} \neg a_i = a_j \implies \exists y \forall x (x \in_{(0)} y \leftrightarrow \bigvee_{1 \le i \le n} x = a_i)$$

We finish this section with the derivation of a nontrival law which contains the relation $a \in b$, or equivalently $a \in_{(0)} b$, on both sides of an $\leftrightarrow$.

$$[single] \;:\; \exists y \forall x\,(\,x \in y \leftrightarrow \exists z \forall u\,(\,u \in x \leftrightarrow u = z\,)\,)$$

The sentence [*single*] says that the set of all singletons exists; this is wildly false in Zermelo-like set theories.

Proof. Consider the predicate-term
$T := \lambda x \exists z\,[\,(\lambda v : v = z) \searrow x\,]$.
The term T has level 0. In view of Proposition 2, to prove [*single*], it is sufficient to show the equivalence

$$\exists z\,[\,(\lambda v : v = z) \searrow a\,] \;\leftrightarrow\; \exists z \forall u\,(\,u \in a \leftrightarrow u = z\,)$$

The direction $\rightarrow$. Let c be such that $(\lambda v : v = c) \searrow a$. Our aim is to show, that the right-hand z can be taken as c. So let $u \in a$, i.e. there is an A such that $A \searrow a$ and $A(u)$. By $(gg5\;0)$ [equivalently by $(gg\;5)$] the terms $\lambda v : v = c$ and A have the same elements. Hence $u = c$. If $u = c$, then $A(u)$. Hence $u \in a$.

Now for the direction $\leftarrow$. Let c such that (1) $\forall u\,(\,u \in a \leftrightarrow u = c\,)$. We have to show that $(\lambda v : v = c) \searrow a$. From (1) we have $c \in a$. Hence there is an A with $A \searrow a$ and $A(c)$. But $(\lambda v : v = c) \approx A$. By the axiom (*updown*) there is a d such that $(\lambda v : v = c) \searrow d$. Finally, by $(cong() \searrow 0)$ we get $(\lambda v : v = c) \searrow a$. QED

4 Conclusion

We have replaced the impredicative comprehension principle of *GGA* by the ramified principle (*ram comp*), and have strengthened Frege's (*GG V*) to the infinitely many contraction laws.

Appendix on Logicism versus Ontologicism

Logicism is not sharply determined by the statement that (Standard) Mathematics is "part of" Logic, or even "the same as" Logic. There are too many precise versions of the possible relationship between mathematics and logic.

However, let us (for the sake of discussion) assume that the formal system PM is a good vindication of logicism. Here PM is the unramified version of *Principia Mathematica* together with an axiom of infinity and some choice principles. The formal system PM is quite logical, it can be interpreted in systems which are even more logical, see [3] and [4].

The adjective *logical* is intended (by me) to denote *non-ontological*, or *ontologically non-committal*, or *ontologically thin and neutral*. Seen from the point of view of this distinction, Frege's *GGA* does definitely not belong to logicism, but rather to ontologicism, despite Frege's own terminology (and imagination). For Frege's objects (Gegenstände) are ontologically very heavy, even ontologically overloaded. Let us make this entirely clear. If f is the Fregean concept of being a real number, then its Werthverlauf $x.f(x)$ is an object, and this object is identical with the set $\mathbb{R}$ of real numbers, i.e. $\mathbb{R} = x.f(x)$, or $f \searrow \mathbb{R}$ in our notation. Also, the set of all functions from the reals to the reals is an Fregean object.

In this way, all entities are either objects or a contracted to objects. Therefore, ontologicism is a better term for what

GGA tried to implement. The effective implementation of the ideas behind *GGA* amounts to an ontological or rather *physical* theory of the contraction relation $A \searrow b$.

Suppose now we had a theory T of the contraction relation such that T is more or less mathematically equivalent to PM. Does then PM cease to belong to logicism? One may say NO since the infinitely many types of PM could delute somehow the ontological substance and heaviness of the objects in T.

Some of the ideas in this Appendix were inspired by [6].

References

[1] J. W. Degen : *Systeme der kumulativen Logik*, Philosophia Verlag, 1983

[2] J. W. Degen : *Two formal vindications of logicism*, in: Philosophy of Mathematics, Proceed. 15th Intern. Wittgenstein Symp. ed. J. Czermak, Wien 1993, 243–250

[3] J. W. Degen : *Complete infinitary type logics*, Studia Logica 63 (1999) 85–119

[4] J. W. Degen and Jan Johannsen : *Cumulative higher-order logic as a foundation for set theory*, Math. Log. Quart. 46 (2000) 2, 147–170

[5] G. Frege : *Grundgesetze der Arithmetik* Jena (1893, 1903).

[6] H. T. Hodes : *Where do sets come from?* J.S.L. 56 (1991) 1, 150–175

What is the Purpose of Neo-Logicism?

Philip A. Ebert & Marcus Rossberg

Introduction

This paper introduces and evaluates two contemporary approaches of *neo-logicism*. Our aim is to highlight the differences between these two neo-logicist programmes and clarify what each projects attempts to achieve. To this end, we first introduce the programme of the *Scottish* school – as defended by Bob Hale and Crispin Wright[1] which we believe to be a

We would like to thank Bernard Linsky and Edward Zalta for many interesting discussion and helpful comments on earlier drafts of this paper. Earlier versions of this paper were presented at the British Logic Colloquium meeting in Bristol, 2005, the conference of the British Society for the Philosophy of Science in Southampton, 2006, and in the *Arché* philosophy of mathematics research seminar in St Andrews; we are indebted to the audiences. We would also like to thank Patrick Greenough, Hannes Leitgeb, Øystein Linnebo, Paul McCallion, Stewart Shapiro, and Crispin Wright for helpful comments and criticisms, and to Lindsay Duffield for literary advice. We are grateful for the funding of our research by the AHRC and the Leverhulme Trust.

[1]See (Wright, 1983), (Hale, 1987), (Hale and Wright, 2001); see also (MacBride, 2003).

form of epistemic foundationalism in which logic is intended to play a foundational rôle in resolving specific epistemic challenges, such as our knowledge of arithmetic and analysis. We contrast this with what we call the *Stanford/Edmonton* school whose project is to put forth and defended by Bernard Linsky and Edward N. Zalta.[2] This latter approach is a form of axiomatic metaphysics, which, if successful, achieves a different aim. Having offered an outline of the general outlook of these two schools we discuss what Frege took to be the *purpose* of his logicism. In the light of this discussion we aim to highlight why we think that the *Scottish* school is not only closer to Frege's own project but also draw attention to some inherent shortcomings of what can be achieved if one pursues the programme of the *Stanford/Edmonton* school.

1 Neo-Logicism: Two Schools

In this section we outline two schools of thought in the philosophy of mathematics, both of which claim logicist roots and consider themselves *neo-logicist* programmes. We focus on how, in general, the two schools attempt to recover arithmetic, analysis and set theory, or even the whole of mathematics. We first discuss the *Scottish* school, which is commonly known as the *Neo-Fregean* programme or *Abstractionism.*

1.1 The Scottish School

The neo-logicism of the Scottish school is to be considered a form of epistemic foundationalism. It aims to explain knowledge of arithmetic and possibly the whole of classical mathe-

[2]See (Linsky and Zalta, 1995), (Zalta, 2000), (Linsky, 2005), and (Linsky and Zalta, 2006).

matics by appeal to what is called the context principle, certain basic principles – so-called *abstraction principles* – and standard second-order logic. It is with this trinity that they aim to resolve Benacerraf's well-known dilemma concerning mathematical knowledge by offering a platonist route to mathematical knowledge.

Roughly speaking, the function of the context principle is to guarantee that mathematical singular terms indeed refer, and so refer to abstract objects. The theory of abstraction principles aims firstly, to introduce mathematical singular terms and secondly, to offer an "epistemically tractable" way of how a subject can come to know basic mathematical principles. Lastly, second-order logic is adopted in order to generate the theorems of mathematics.

In this brief exposition we focus on the second and third component – abstraction principles and second-order logic – underlying the approach of the *Scottish* school. Generally speaking, this approach is a piecemeal approach to mathematics. That is, it is concerned with specific abstraction principles – in the case of arithmetic *Hume's Principle* – and evaluates whether these abstraction principles qualify as "epistemically tractable" principles that can found knowledge of arithmetic. In order to extend mathematical knowledge to other parts of mathematics, say, analysis or set theory, a similar investigation has to take place concerning other abstraction principles that introduce the notion of a real number or set. Let us briefly explain how, by using *Hume's Principle*, the Neo-Fregean story is meant to go for arithmetic. *Hume's Principle* (HP) can be formulated as follows:

$$\text{(HP)} \qquad \forall F \forall G(Nx : Fx = Nx : Gx \equiv F \approx G)$$

where '$Nx : Fx$' stands for 'the (cardinal) number of the Fs' and '$\approx$' expresses a one-to-one correspondence.[3] Thus, the principle claims that the cardinal number belonging to the concept F is identical to the cardinal number belonging to the concept G if, and only if, there is a one-to-one correspondence between the objects falling under F and those falling under G.

This abstraction principle, so the Neo-Fregean claims, can be put forth as an implicit definition. That is, the intention is to stipulate this principle as true[4] which thereby introduces a new expression: '$Nx: x$'. Assuming for the moment that this idea is legitimate and such stipulations are knowledge-conferring, the question arises how we can acquire knowledge of the right-hand side. This is exactly where the third claim gets its grip, and it is also why Neo-Fregeans consider themselves neo-*logicists*. For the claim is that it is a matter of logic that there are true instances of the right-hand side of HP. The Neo-Fregean argues that in order to see this one just has to note that it is a logical truth that the instances of the concept *being non-self-identical* can trivially be put into a one-to-one correlation with themselves. This true instance of the right-hand side of HP, suffices – assuming that HP is true – to yield a true identity statement about numbers on the left hand side. More formally this can be expressed as follows:

---

[3]The claim for the existence of a one-to-one correspondence can be formulated in purely (second-order) logical vocabulary. In full detail Hume's Principle is the following statement:

$$\forall F \forall G[Nx : Fx = Nx : Gx \equiv \exists R(\forall x[Fx \supset \exists y(Gy \wedge Rxy \wedge \forall z(Gz \wedge Rxz \supset z = y))] \wedge \forall y[Gy \supset \exists x(Fx \wedge Rxy \wedge \forall z(Fz \wedge Rzy \supset z = x))])]$$

[4]The Neo-Fregean also grants that not every stipulation is successful – more on this below.

Step 1

$$(Nx : x \neq x = Nx : x \neq x) \equiv (x \neq x) \approx (x \neq x)$$

The right-hand side of this statement is a logical truth. Assuming the truth of HP we can discharge the right-hand side and derive:

Step 2

$$(Nx : x \neq x = Nx : x \neq x)$$

Assuming that number-terms are singular terms we can, by adopting the context principle, infer the claim that there is an object to which the singular term refers. We can thus existentially quantify into this formula (reading the existential quantifier objectually and ontologically committing):

Step 3

$$\exists y(y = Nx : x \neq x)$$

In addition, having the formal result in place that the second-order version of the Peano-Dedekind axioms for arithmetic can be deduced in second-order logic from Hume's Principle – a result which is called *Frege's Theorem*[5] – the Neo-Fregean can

[5]This theorem was first explicitly noted by Parsons, in his (Parsons, 1965) and later independently rediscovered in (Wright, 1983), pp. 158–169. More recent presentations of the proof can be found in (Boolos, 1987) (discursive), (Boolos, 1990b) (rigourous), (Boolos, 1995), and (Boolos, 1996). Note that even a weaker version of Hume's Principle – *Finite Hume* – suffices for this derivation; see (Heck, 1997). Second-order logic is required for this proof. A relatively moderate portion of second-order logic suffices, however: Π^1_1 comprehension is enough. (Linnebo, 2004) has shown that Frege's Theorem cannot be proven in predicative second-order logic. (Heck, 2006) provides a proof that ramified second-order logic suffices. For formulations of the respective fragments of second-order logic see (Church, 1956), §58, and (Shapiro, 1991), chapter 3. For a general overview of the technical details of Fregean arithmetic, see (Burgess, 2005).

justifiably claim that knowledge of logic leads, merely through the stipulation of HP, to knowledge about numbers as objects and to knowledge of arithmetic. Since the proponents of the Scottish school regard knowledge of arithmetic as a priori, they also embrace the additional claim that basic mathematical principles can be known a priori and that reasoning within second-order logic (which is needed to establish Frege's Theorem) preserves the epistemic status of the a priori knowable abstraction principle.

According to the Scottish school, some, but not all, abstraction principles have the status of being meaning-constitutive of the expression it is meant to introduce. It is claimed that this rôle as a meaning-constitutive principle endows them with an epistemic dimension: namely, in the best case, it provides part of the justification for holding these abstraction principles as true.[6] However, the Neo-Fregeans do not regard abstraction principles *per se* as having this special epistemic status, and also do not insist that they are *logical* principles, but merely that they are *analytically* true, since they are meaning-constituting.[7]

In this respect they depart from Frege's logicism. Frege, at least initially, regarded the ill-fated Basic Law V – which would be construed as an abstraction principle in the Neo-Fregean programme – as logical.

Nevertheless, logic does play an important epistemic rôle

[6]The account is more complicated than this. Hale and Wright have also defended the idea that Hume's Principle (and presumably other abstraction principles) do not have *direct* ontological commitments which makes them particularly suitable for direct stipulations; see (Hale and Wright, 2000). For criticism of their approach using meaning-constituting principles see (Ebert, 2005).

[7]Exactly, what makes an abstraction principle a meaning-constituting principle which can underwrite knowledge is a notoriously hard questions and finding the right criteria is an ongoing research project.

within the Scottish school. Since it is knowledge of logic that is needed to justifiably discharge the right-hand side of Hume's Principle, the Neo-Fregean believes that logical knowledge, plus knowledge of certain abstraction principles, suffices to account for mathematical knowledge.

1.2 The Stanford/Edmonton School

Bernard Linsky and Edward Zalta's approach to neo-logicism is based on so-called *Object Theory* (OT), a theory that was first introduced in (Zalta, 1983). The higher-order modal[8] theory contains some interesting features. One of them is that it does not have one, but two modes of predication. The first is the ordinary form of predication, which is referred to as *exemplification*. This predication is formalised as, for example, 'Fa' and read 'a exemplifies F'. The second mode of predication is called '*encoding*'. To distinguish the two, the order of the predicate letter and the name is switched around: 'aF', read 'a encodes F'.

While any object whatsoever can exemplify properties, only *abstract* objects encode properties. Moreover, encoding a property does not entail exemplifying it: '$xF \nvdash Fx$', but encoding properties entails being abstract. A predicate '$A!$' stands for 'is abstract'. It is not taken as primitive, however, but defined with the help of the primitive predicate 'is concrete', '$E!$':

$$A!x \ =_{df} \ \neg\Diamond E!x$$

[8]Linsky and Zalta typically claim that their approach to neo-logicism is non-modal, see e.g. (Linsky and Zalta, 2006, p. 88). This is not entirely correct, however: the possibility operator '$\Diamond$' occurs in the definition of the abstractness predicate, and the necessity operator '$\Box$' occurs in the definition of identity between abstracta – see below.

Being an ordinary object, '$O!$' is also defined:

$$O!x \ =_{df} \ \Diamond E!x$$

Abstract objects are those, that are not possibly concrete; and ordinary objects are those that are possibly concrete. The notion of an ordinary object allows Zalta in other projects to propose a theory of merely possible and also of fictional objects.[9] This, however, will be of no concern here.

Abstract objects enter OT via a comprehension schema for abstract objects (OC):

(OC) $\exists x(A!x \wedge \forall F(xF \equiv \varphi))$, where '$x$' is not free in φ

This axiom schema asserts that for any formula φ (minding the restriction on free variables), there exists an abstract object that encodes all and only those properties F that satisfy φ; or, expressed in a more sloppy way, for any collection of properties, there is an abstract object encoding them.

OC guarantees that any (abstract) object that is described by an expression of the form '$\imath x(A!x \wedge \forall F(xF \equiv \varphi))$' exists (where there is no free 'x' in φ). So, there is, for example, an abstract object that encodes the property of *being Zalta* (or *being identical to Zalta*):[10]

$$\imath x(A!x \wedge \forall F(xF \equiv \forall y(Fy \equiv y = \text{Zalta})))$$

[9] OT was originally developed as a formal theory of fictional, abstract, and intensional objects inspired by the work of Meinong's student Ernst Mally: see (Zalta, 1983).

[10] All of the following examples are, of course, dependent on the English names and predicates entering the formal language in some way. How this is done for mathematical terms is described below. Moreover, identity is a defined notion in OT. So, strictly speaking, one would have to specify that the identity relation referred to in our examples is identity between concrete, rather than abstract, objects.

An abstract object that encodes *being either Linsky or Zalta*:

$$\imath x(A!x \wedge \forall F(xF \equiv \forall y(Fy \equiv (y = \text{Linsky} \vee y = \text{Zalta}))))$$

An abstract object that encodes all the properties Zalta has:

$$\imath x(A!x \wedge \forall F(xF \equiv F(\text{Zalta})))$$

Note that Zalta himself is not identical to any of these objects (since he is concrete and not abstract). He exemplifies, rather than encodes the respective properties. Sherlock Holmes, on the other hand, is an abstract object, viz. the abstract object that encodes all the properties that (the fictional character) Sherlock Holmes has according to the stories by Arthur Conan Doyle. (The devise for fomalising this will be introduced below in the discussion of mathematical theories.) There is also an abstract object that encodes being a square circle:

$$\imath x(A!x \wedge \forall F(xF \equiv \forall y(Fy \equiv (y \text{ is a circle} \wedge y \text{ is square}))))$$

Moreover, there is an abstract object that encodes being a set that contains all and only those sets that do not contain themselves. In order to avoid inconsistency, the second-order comprehension schema for predicates:[11]

$$\exists X \forall x(Xx \equiv \varphi(x)), \text{ where } X \text{ is not free in } \varphi$$

[11]For simplicity's sake we only give the comprehension schema for monadic second-order variables. The restrictions apply in the same way for the general formulation for polyadic variables. We here use the common formulation of second-order logic introduced in (Church, 1956); the current bible of second-order logic is (Shapiro, 1991). Linsky and Zalta use an equivalent formulation that employs λ-conversion, which requires an analogous restriction.

(and likewise the third- and higher-order comprehension schemata) has to be restricted. It has to be demanded of the standardly unrestricted second-order comprehension schema that φ does not contain any descriptions or "encoding subformulae". So, the fully explicit formulation of φ must not contain subformulae of the form $\ulcorner xY \urcorner$, i.e. subformulae containing the encoding mode of predication.[12]

Identity between abstracta, '$=_A$', is a defined relation. Two abstract objects are identical if, and only if, they necessarily encode the same properties:

$$x =_A y \ =_{df} \ A!x \wedge A!y \wedge \Box\forall F(xF \equiv yF)$$

With this criterion for identity at hand, we can see that the abstract object introduced above which encodes *being Zalta* is distinct from the object encoding all of Zalta's properties: the latter encodes *using a Mac* while the former does not.

So much for the formal background. Linsky and Zalta now suggest that mathematical theories can be identified as those abstract objects, that encode all the mathematical propositions that are true according to them.[13] This needs some unpacking. First, encoding was introduced as a mode of predication, i.e. a second-level relation that holds between an object and a property. In order for mathematical theories to be able to encode *propositions*, they are handled as zero-place properties. Any proposition p thus gives rise to a property *being such that* p; using the notation of λ-conversion, this can be expressed as: '$[\lambda y\, p]$'.

[12]One might complain that object comprehension, OC, is suspect on the grounds that with the introduction of OC the well established second-order comprehension schema, considered logical by many, needs to be restricted to avoid inconsistency. We will not follow this criticism here.

[13]See (Linsky and Zalta, 1995), pp. 538–539, and (Linsky and Zalta, 2006), pp. 89–90.

Being true according to a mathematical theory T can then be characterised using the resources of OT: it is simply defined as T encoding that particular truth:

$$\text{T} \models p \ =_{df} \ \text{T}[\lambda y\, p]$$

Note that '$\models$' does not denote a semantic consequence relation here, but merely abbreviates the encoding formulae on the right-hand side of the definition. There is, however, a *rule of closure* that guarantees that mathematical theories are deductively closed. Whenever a proposition is a proof-theoretic consequence[14] of some propositions that are true according to the theory – i.e. that are encoded by the theory, which means in particular the axioms – then the theory also encodes this proposition:

Rule of Closure

If $p_1, ..., p_n \vdash q$ and $\text{T} \models p_1$ and ... and $\text{T} \models p_n$, then $\text{T} \models q$.

The theoretical terms of a mathematical theory can also be imported into OT in the following way: Take a term κ of the mathematical theory T in question and index it with the name of the theory. OC will then guarantee that there is a corresponding abstract object:

$$\kappa_{\text{T}} \ =_{df} \ \imath x(A!x \wedge \forall F(xF \equiv \text{T} \models F\kappa_{\text{T}}))$$

[14]One might quibble whether this should mean a consequence according to the proof theory of the mathematical theory in question, or according to whatever metatheory Linsky and Zalta use for OT. (Zalta, 2000), p. 232, suggests that it is the proof-theoretic consequence relation of OT; it is unlikely that constructivist theories are faithfully represented in this way, not to speak of paraconsistent mathematics (see, for instance, (Priest, 1994)).

Generalising OC in the obvious way to also yield objects of higher types, i.e. properties and relations, we can import in an analogous way the properties and relations Π of mathematical theories into OT (the bold-face letters stand for third-order predicates and variables):

$$\Pi_{\mathrm{T}} =_{df} \imath R(\mathbf{A}!R \wedge \forall \mathbf{F}(R\mathbf{F} \equiv \mathrm{T} \models \mathbf{F}\Pi_{\mathrm{T}}))$$

Membership in Zermelo-Fraenkel set theory (ZF), for example, can thus be defined in OT as:

$$\in_{\mathrm{ZF}} =_{df} \imath R(\mathbf{A}!R \wedge \forall \mathbf{F}(R\mathbf{F} \equiv \mathrm{ZF} \models \mathbf{F}\in_{\mathrm{ZF}}))$$

With these items available, all propositions of the mathematical theory in question can be added to OT as (arguably) *analytic truths*, 'In theory T, p', in this way:

> Add to OT sentences of the form $\ulcorner \mathrm{T} \models \varphi^* \urcorner$, where φ is an axiom of the T and φ^* is arrived at by indexing all well-defined terms and predicates of theory T as belonging to T.

The rule of closure will then take care of all the theorems of the mathematical theory in question. To use ZF as an example again, the existence of a set without members according to ZF can be expressed in OT as:

$$\mathrm{ZF} \models \exists x \neg \exists y\ y \in_{\mathrm{ZF}} x$$

Linsky and Zalta take all the resulting sentences to be analytic, since all these sentences say, once they are imported into OT, is that a given mathematical theory affirms this-and-that. Moreover, since the theoretical terms of the mathematical theories are imported into OT too and OC guarantees a corresponding object, so to speak, OT also directly delivers the ontology to satisfy the *imported* sentences.[15]

In this way, *any possible* mathematical theory can be imported into OT. Linsky and Zalta write:

> [O]ur program ... takes as data any arbitrary mathematical theory that mathematicians may formulate, and provides a more general explanation and analysis as a whole. ((Linsky and Zalta, 2006), p. 89)

This, according to Linsky and Zalta, "constitutes a form of neologicism" since it is a weakening of the logicist claim that mathematics is reducible to logic alone. Being a weakening of this logicist claim is what Linsky and Zalta identify as a hallmark of neo-logicism. They write:

> Our claim is:
>
> Third-order object theory is a neologicism because it reduces (in the sense just described) all of mathematics to 'third-order' logic[16] and some analytic truths. ((Linsky and Zalta, 2006), p. 91)

[15]It cannot, however, deliver the ontology that the original theories are intended to be about. The intended model of real analysis, for example, has an uncountable domain, but the technique described above will only ever deliver countably many objects, since the language is countable.

[16]Linsky and Zalta comment on this: "By quoting the phrase 'third-order', we are calling attention to the fact that the theory is weaker than full third-order logic. Though our theory is most naturally formulated using third-order syntax its logical strength is no greater than multi-sorted first-order logic." (*ibid.*) The "analytic truth" not only include the mathematical statements that are to be imported into OT, but also the comprehension schema for abstract objects, OC, along with the notion of encoding. It should also be noted that modal operators figure in some of the definitions; see footnote 8 above.

Let us briefly come back to the importing of mathematical theories into OT. Are we not going in a circle here? Mathematical theories are supposed to enter OT as those abstract objects that encode all propositions true according to them, but importing these propositions into OT involves mentioning the mathematical theory. The crux is that it is not the imported mathematical statements that identify the mathematical theory in OT. The definition, rather, *quantifies* over propositions, and it is outside of OT that we are to decide what propositions are true according to a given mathematical theory. In other words, mathematicians, or mathematical practice will tell us.

Formally, Linsky and Zalta can thus state that if T is a mathematical theory, then it is that abstract object that encodes all its theorems:[17]

$$\begin{aligned}&\mathit{MathTheory}(\mathrm{T}) \supset\\ &\mathrm{T} = \imath x(A!x \wedge \forall F(xF \equiv \exists p(\mathrm{T} \models p \wedge F = [\lambda y\, p])))\end{aligned}$$

This, however, leaves open what the antecedent actually says. The answer is to be found in (Zalta, 2000, pp. 229–230). First, take '*Math*(p)' as a primitive notion, with the intended meaning

[17]Identity between properties (for all types or orders) is defined as necessary co-encoding:

$$F = G \ =_{df} \ \Box\forall x(xF \equiv xG)$$

This is another place where the modal operator comes in; see footnote 8. Similarly, identity between propositions is defined as:

$$p = q \ =_{df} \ [\lambda y\, p] = [\lambda y\, q]$$

which unpacks as:

$$p = q \ =_{df} \ \Box\forall x(x[\lambda y\, p] \equiv x[\lambda y\, q])$$

See (Zalta, 2000), p. 224, fn. 9.

'is a purely mathematical proposition'. Zalta asks us to rely upon the "pretty good pretheoretic grasp" we have to decide this predicate. Then, add another primitive predicate, namely 'Axy' for 'x authored y', or, equivalently, 'y is an author of x'.[18] Now '$MathTheory(x)$' can be defined:

$$MathTheory(x) \ =_{df}$$

$$\forall F(xF \supset \exists p(Math(p) \wedge F = [\lambda y p])) \wedge \exists y(E!y \wedge Ayx)$$

Thus, mathematical theories are a particular kind of story, akin to fiction in many ways. Mathematical theories have to be authored: there are no mathematical theories (yet) that have not been written or authored in some other way (yet).[19] This definition also seems to put mathematical theory ontologically on a par with pieces of fiction: for example, Peano Arithmetic is ontological on one level with the Brothers Grimm's *Rumpelstielzchen*, metaphysically speaking. We will not further dwell on potential problems of the resulting ontology here.[20] Instead, let us briefly consider a short story (which, as such, should be taken with a grain of salt). It is meant to highlight the differences between the two schools and will be used to support our final conclusions.

---

[18]We omit the type specification here for simplicity's sake. For a typed version see (Zalta, 2000), pp. 228ff.

[19]In (Zalta, 2000), p. 230, anticipates possible criticisms regarding this point. He suggests that possible authorship might be sufficient. Formally, this would be to introduce a possibility operator, '$\Diamond$', in front of the second existential quantifier.

[20]We are, however, discussing these and other further issues in our (Ebert and Rossberg, 2006).

2 Interlude: The Travels of Hero and Hera

The twins Hero and Hera had always been inseparable. After school they both went to Ohio State University, and together they discovered higher-order logic there. Each excelled at their new favourite subject, passing the final exams with flying colours. Before long, both Hero and Hera had applications for graduate programmes winging away in the post. Hero was awarded AHRC funding to go to St Andrews, whilst Hera gained a scholarship to study in Stanford.[21] And so, the time came for the twins to part company, as each budding young philosopher embarked on a PhD in logic at different universities, in different countries, and under different supervisors. Of course, being the diligent and obedient students they inevitably were, both Hero and Hera unquestioningly accepted every principle their respective supervisors (Crispin Wright and Edward Zalta) presented them with.

Hero's first week at St Andrews was a good one. In addition to testing the water (and a few other choice beverages) he learnt all about Hume's Principle, the abstraction principle which states that the number of the Fs equals the number of Gs if, and only if, there is a bijection between them. By his second week, Hero learnt the abstraction principle for real numbers. And, by the third week Hero had really begun to settle into the St Andrews lifestyle, to learn New V and Newer V, two consistent restrictions of the inconsistent Basic Law V.

By this point, by way of deduction in second-order logic, Hero had acquired arithmetic from Hume's Principle and real

[21] Hera briefly considered going to Edmonton instead, but the cold winters put her off.

analysis from the abstraction principle for the reals. Admittedly he didn't get much set theory from New V or Newer V, but towards the end of his final year Hero was lucky enough to learn that Julius Caesar is not a number, so all was well. He was able to deduce a set theory that interprets (most of) ZF from Hume's Principle, New V, and Newer V, along with the solution to the Caesar Problem.[22]

Meanwhile, across the pond, life was different for Hera. In her first week she learnt all about a new (primitive) form of predication, called 'encoding', and that abstract objects *encode* properties much in the same way that concrete objects *have* properties. In her second week, Hera learnt where these abstract objects come from, viz. from *Object Comprehension*. By the third week, Hera's supervisor had explained to her that mathematical theories are just examples of some of those abstract objects whose existence is given by Object Comprehension, and also that one can add any statement of the form 'In theory T, p' to the system – which she learnt is (analytically) true if p is a theorem of T. She also learnt that all the entities that these mathematical theories talk about exist as well: they are also given by Object Comprehension.

However, all that was only the beginning for Hera. She was quickly packed off to the mathematics department to learn all

[22] A careful study of (Cook, 2003) revealed to Hero that the following principles are enough to prove all axioms of ZF except Foundation. These are, New V, Newer V, the Size-Restricted Ordinal Abstraction Principle (SOAP) and the existence of infinitely many non-sets. From footnote 30 of (Cook, 2003) Hero learnt that, strictly speaking, SOAP can be dispensed with if slightly reformulated versions of New V and Newer V are adopted. Hero then realised (on his own) that the existence of infinitely many non-sets follows from Hume's Principle provided that the Caesar Problem is resolved in such a way as to yield that numbers are not sets, i.e. the problems raised in (Cook and Ebert, 2005) are resolved.

possible mathematical theories whose existence is guaranteed by Object Comprehension, and whose analytic truths ('In T, *p*') can be added to the system. Amidst all that, Hera was reminded that

> Indeed, a unique feature of our program is that it yields no proper mathematics on its own, and so makes no judgments about which parts of mathematics are philosophically justified! Instead, it takes as data any arbitrary mathematical theory that mathematicians may formulate. ((Linsky and Zalta, 2006), p. 30)

As one might expect from the amount of studying involved, Hero was going to graduate sooner, because Hera's course took longer to complete – after all she did have all possible mathematical theories to learn. (Hera certainly realised that she was not, strictly speaking, required to go and learn *all* possible mathematical theories – this is not part of the programme. Since she set out to acquire mathematical knowledge, however, she decided to make full use of the possibilities of Object Theory.) All Hero had to learn, on the other hand, was four abstraction principles (and that Julius Caesar is not a number).

Despite the physical and philosophical separation of Hero and Hera since their undergrad years, a remarkable coincidence conspired to unite them again in a bizarre way as each approached the ultimate end of their studies. In their final examinations, both Hero and Hera were confronted with the same questions:

1. Proof that $2 + 2 = 4$!

2. How do you know that it is true?

   Allowed materials: The principles your supervisor taught you.

Hero tackled the challenge in the following elegant way: He swiftly derived the Peano-Dedekind axioms of arithmetic from Hume's Principle in second-order logic, and used them to prove that $2 + 2 = 4$. For the second question, Hero simply claimed that he was allowed to take Hume's Principle as (analytically) true, since it is a meaning-constituting principle, and also since pure (second-order) logic can be used to derive the statement in question, he comes to know it simply by way of deduction from Hume's Principle. Although, his external examiners were not entirely convinced that the mere meaning-constituting character of an abstraction-principle will be enough to secure its truth and so account for his knowledge of Hume's Principle, and also had some misgivings about the adoption of second-order logic, they were sufficiently impressed by his story (and the presentation thereof) to award him a PhD. Yet they hoped he would – in the near future – say why exactly some abstraction principle succeed in founding knowledge while others fail.

For Hera, though, the challenge was considerably more daunting. After a little initial hesitation, she quickly proved that $2 + 2 = 4$ in Peano Arithmetic, then in Robinson Arithmetic, and then in real as well as complex analysis, followed by a proof in the system of *Principia Mathematica* and then in ZF (plus suitable definitions). Her examiners stopped her just before she started the proof in Aczel's anti-foundational set theory.[23]

[23] A consistent set theory that dispenses with the Axiom of Foundation, and allows sets to contain themselves; see (Aczel, 1988).

"But you didn't tell me what '2' and '4' and '+' and '=' you were talking about," she protested. "So I just started with some common theories. I can also prove it in Priest's paraconsistent arithmetic if that's better."[24] Looking a bit sheepish as she said it, Hera added that she also knew a few disproofs, if the examiners would like. The examiners assured her that they wouldn't like that, and asked her to move on to the second question. Alas, Hera was stumped on that one. Eventually, she reluctantly said that she knows it is a theorem of various mathematical theories. She knew, for example, that in PA, $2+2=4$, but without clarifying which '2' and '4' and '+' and '=' is meant, she wasn't sure what she was meant to show.[25]

Her examiners were intrigued about her responses and awarded her the degree for her stimulating views in the philosophy of mathematics, her sophisticated axiomatic metaphysics, and her heroic attempt to account for *any* possible mathematical theory.

[24] See (Priest, 1994).

[25] (Andersen and Zalta, 2004) present a different neo-logicist programme in the framework of second-order modal OT which allows for the derivation of '$2 + 2 = 4$' as a categorical statement, since this approach allows the derivation of some modest parts of mathematics as non-hedged statements from some additional assumption; see also (Zalta, 1999), and the discussion in (Linsky and Zalta, 2006), §4.2. It is argued in (Linsky and Zalta, 2006), §5, that the approach presented here is to be preferred to the Andersen and Zalta project.

3 Axiomatic Metaphysics vs Epistemic Foundationalism: the Purpose of Neo-Logicism?

The idea in this section is not to pinpoint specific problematic issues that threaten the tenability of either of the programmes. Rather, assuming that each project is tenable and internally consistent, we aim to tackle the question what the purpose is of pursuing either of these two projects. That is, what is the philosophical payback from pursuing either the Scottish school, i.e. epistemic foundationalism, or the Stanford/Edmonton school, i.e. axiomatic metaphysics. We hope that the story highlighting the achievements of Hero and Hera will help to identify the differences between the two approaches. By appealing to what Frege thought what the aim of logicism is, we hope to show that the purpose of pursuing the Stanford/Edmonton school does not fit the bill.

The original logicist programme was clearly epistemological in spirit, as Frege writes:

> The problem becomes, in fact, that of finding the proof of the sentence, and of following it up right back to the *primitive truths*. If in carrying out this process, one comes only to *general logical laws* and definitions, then the truth is an analytic one. [...] [If the] proof can be derived exclusively from general laws, which themselves neither need nor admit proof, then the truth is a priori. ((Frege, 1884), p. 4, our italics and translation)

and later in the Grundgesetze he writes the following:

> In virtue of the gaplessness of the chain of inferences it is achieved that each axiom, each presupposition, hypotheses, or however else one might want to call that which a proof rests upon, is brought to light; and thus one gains a *foundation for the assessment of the epistemological nature* of the proven law. ((Frege, 1893), p. XXVI, our italics and transl.)

These quotations provide a good indication that Frege's logicist project was foundationalist in nature. He aimed to identify a few select general logical laws, or basic laws, that were needed to provide an epistemic foundation: namely, mathematical knowledge was meant to "flow" from those basic principles and (what is now called) second-order logic. In addition, Frege also thought of the basic principles as providing an ontological foundation. Basic Law V was meant to identify the logical objects (extensions) by means of which numbers could then be defined. For Frege, logic was the most general of all sciences and concerned with the laws of thought. He considered it to be objectively valid independent of any thinker. Moreover, mathematical statements derived from these logical principles using second-order logic were also considered objective (and so independent of anyone "authoring" them) and the underlying objects were considered to exist mind-independently. There could not be two different yet equally acceptable logics, and there could not be different and incompatible theories of numbers.

The Scottish school is squarely in line with this epistemic foundationalist approach of Frege's. The aim is to select a few principles (which are, however, not regarded purely logical) and then to explain how Hero can, by means of grasping these principle come to know mathematics. The resulting theory explains (assuming it works) how mathematical knowledge can flow from basic principles and knowledge of second-order logic. In addition, the objects these principles are purportedly about are considered to exist, and exist mind-independently. Mathematics and logic are considered objective and not as a mere game or fiction: the statements Hero knows are categorical statements involving a distinct ontology. Thus, we think, the Scottish school neatly fits the general methodology and the aims of Frege's logicist project and should be labelled *neo-logicist*.

In contrast, the Stanford/Edmonton school is an enterprise in axiomatic metaphysics. It aims to select a few metaphysical principles and then provides the tools for Hera to re-interpret any mathematical theory within the new metaphysical framework. Her mathematical knowledge does not flow from some basic mathematical or logical principles. Rather she knows how any mathematical statement, or any mathematical theory for that matter, can be re-interpreted within object-theory.

This is a difference worth emphasising: while the Stanford/Edmonton school wants to account for any mathematical theory, the Neo-Fregean, like Frege, believes that there are mathematical principles, and so mathematical theories, that are better than others.[26]

So, for Hera, every mathematical statements will be true provided it is bound by the respective 'In theory T'-operator. Her mathematical knowledge thus reduces to knowledge of these hedged statements within the new metaphysical framework and, hence, is not categorical.[27] Also, since mathematical theories

[26]The Neo-Fregean does not aim at an epistemic foundation of inconsistent theories, for example. We expand on this in our (Ebert and Rossberg, 2006).

[27]There is a way of simulating categorical statements in OT (and there

and with them mathematical objects depend (in an ontological sense) on authorship[28], they cannot be regarded to exist mind- or subject-independently either.

is, as mentioned in footnote 25 above, also the approach presented in (Andersen and Zalta, 2004)). The statement that expresses that, according to ZF, the empty set does not have any members, is represented as a hedged OT sentence like this:

$$\text{ZF} \models \neg\exists x(x \in_{\text{ZF}} \emptyset_{\text{ZF}})$$

Since for this we already have to import the terminology of ZF into OT, using the technique described in section 1.2 above, one can now also directly express a related statement: while it is not provable in OT that the (ZF) empty set has no (ZF) member, in the sense of it *exemplifying* the property of having no (ZF) members, there is a sentence that is a theorem of OT which asserts that the (ZF) empty set *encodes* having no (ZF) members; formally, that looks something like this:

$$\emptyset_{\text{ZF}}[\lambda y(\neg\exists x(x \in_{\text{ZF}} y))]$$

The trouble is that as soon as the OT-defined terms, like '$\emptyset_{\text{ZF}}$' or '$\in_{\text{ZF}}$', are unpacked, the hedged statements appear again; recall, for instance, the OT definition of ZF-membership:

$$\in_{\text{ZF}} =_{df} \imath R(\mathbf{A}!R \wedge \forall \mathbf{F}(R\mathbf{F} \equiv \text{ZF} \models \mathbf{F}\in_{\text{ZF}}))$$

Intuitively, while the sentences about what properties are encoded by these mathematical objects appear to be categorical (in OT), the identification of the mathematical objects and theories goes via the hedged sentences again, i.e. via statements about what is true according to this-or-that mathematical theory. Thus, also these "categorical" sentences express no more than what is the case according to a certain theory, and, hence, should not count as properly categorical in our opinion.

[28] Or *possible* authorship. Perhaps adopting the modal strategy mentioned in footnote 19 above addresses this concern: if a case can be made that dependence on merely possible authorship is consistent with the relevant notion of (mind-)independent existence. How attractive this approach is, however, requires further discussion: for example, since the Barcan Formula, '$\Diamond\exists x\varphi \supset \exists x\Diamond\varphi$', is a theorem of OT (see (Linsky and Zalta, 1995), p. 543, fn. 24), mathematical theories that are not actually authored commit us to the existence of authors who are bare possibilia.

Hence both, the methodology involved in positing an axiomatic metaphysics and the aims of the Stanford/Edmonton school, are very distinct from Frege's original logicist project. The Stanford/Edmonton programme aims to account for all possible mathematical theories, while the attention of the Neo-Fregean is restricted to classical number theory and set theory. Thus, once Hero has learnt the right abstraction principles (however hard these might be to identify), all his mathematical knowledge in these areas can be arrived at by way of inferring it from these principles using second-order logic. While, strictly speaking, Hera did not have to go and learn all possible mathematical theories, she nevertheless had to go and study mathematical theories to get mathematical knowledge: no mathematical knowledge is provided by the Stanford/Edmonton programme on its own. Although it surely has its own intellectual merits and interest, we believe that it fails to fulfil the purpose of logicism and so should not be regarded a form of neo-logicism.

While it might not be considered a major blow that according to these considerations Linsky and Zalta's proposal should be denied the (largely honorific) label 'neo-logicism', we nevertheless want to maintain that their claim that this project "*constitutes an epistemic foundation*, in the sense that it shows how we can have knowledge of mathematical claims"[29] cannot be upheld; and we also have to disagree with their conclusion that their project "best addresses the underlying motives of the early logicists."[30] The "principal driving force of the early logicists", Linsky and Zalta suggest (correctly, as we think), were "epistemological concerns about how we can have knowledge of

[29](Linsky and Zalta, 2006), p. 61, our italics.
[30](Linsky and Zalta, 2006), p. 95.

mathematics" (*ibid.*). We argued that the epistemic concerns of logicism are not addressed by this programme.[31]

References

Aczel, P. (1988). *Non-Well-Founded Sets.* CSLI, Stanford.

Andersen, D. J. and Zalta, E. N. (2004). Frege, Boolos, and Logical Objects. *Journal of Philosophical Logic*, 33:1–36.

Black, M., editor (1965). *Philosophy in America.* Cornell University Press, Ithaca, NY.

Boghossian, P. and Peacocke, C., editors (2000). *New Essays on the A Priori.* Clarendon Press, Oxford.

Boolos, G. (1987). The Consistency of Frege's Foundation of Arithmetic. In (Thomson, 1987), pages 3–20. Reprinted in (Boolos, 1998), pages 183–201.

Boolos, G., editor (1990a). *Meaning and Method: Essays in Honour of Hilary Putnam.* Cambridge University Press, Cambridge.

Boolos, G. (1990b). The Standard of Equality of Numbers. In (Boolos, 1990a), pages 261–277. Reprinted in (Boolos, 1998), pages 202–219.

Boolos, G. (1995). Frege's Theorem and the Peano Postulates. *The Bulletin of Symbolic Logic*, 1:317–326. Reprinted in (Boolos, 1998), pages 291–300.

[31] Further worries concerning the resulting ontology and other epistemological issues are discussed in (Ebert and Rossberg, 2006).

Boolos, G. (1996). On the Proof of Frege's Theorem. In (Morton and Stich, 1996), pages 143–159. Reprinted in (Boolos, 1998), pages 275–290.

Boolos, G. (1998). *Logic, Logic, and Logic.* Harvard University Press, Cambridge, MA.

Burgess, J. P. (2005). *Fixing Frege.* Princeton University Press, Princeton.

Church, A. (1956). *Introduction to Mathematical Logic.* Princeton University Press, Princeton.

Cook, R. T. (2003). Iteration one more time. *Notre Dame Journal of Formal Logic*, 44(2):63–92.

Cook, R. T. and Ebert, P. A. (2005). Abstraction and Identity. *Dialectica* [special issue on the *Julius Caesar Problem*], 59(2):1–19.

Ebert, P. A. (2005). Transmission of Warrant-Failure and the Notion of Epistemic Analyticity. *Australsian Journal of Philosophy*, 83(4):505–521.

Ebert, P. A. and Rossberg, M. (2006). What Neo-Logicism Could Not Be. In preparation.

Frege, G. (1884). *Die Grundlagen der Arithmetik. Eine logisch mathematische Untersuchung über den Begriff der Zahl.* Wilhelm Koebner, Breslau. English translation: (Frege, 1950).

Frege, G. (1893). *Grundgesetze der Arithmetik: Begriffsschriftlich abgeleitet.* I. Band. Verlag H. Pohle, Jena. Partial English translation in (Frege, 1964).

Frege, G. (1950). *The Foundations of Arithmetic.* Translated by J. L. Austin. Blackwell, Oxford.

Frege, G. (1964). *The Basic Laws of Arithmetic: Exposition of the System.* Translated and edited by M. Furth. University of California Press, Berkeley.

Hale, B. (1987). *Abstract Objects.* Basil Blackwell, Oxford.

Hale, B. and Wright, C. (2000). Implicit Definition and the A Priori. In (Boghossian and Peacocke, 2000), pages 286–319. Reprinted in (Hale and Wright, 2001), pages 117–150.

Hale, B. and Wright, C. (2001). *The Reason's Proper Study: Essays towards a Neo-Fregean Philosophy of Mathematics.* Clarendon Press, Oxford.

Heck, R. G. (1997). Finitude and Hume's Principle. *Journal of Philosophical Logic*, 26:589–617.

Heck, R. G. (2006). Ramified Frege Arithmetic. *Journal of Philosophical Logic.* Forthcoming; available online: <http://phonline.org/paper.php?keynum=522>.

Linnebo, Ø. (2004). Predicative Fragments of Frege Arithmetic. *Bulletin of Symbolic Logic*, 10:153–174.

Linsky, B. (2005). Remarks on Platonized Naturalism. *Croatian Journal of Philosophy*, 5:3–15.

Linsky, B. and Zalta, E. N. (1995). Naturalized Platonism vs. Platonized Naturalism. *Journal of Philosophy*, 92:525–555.

Linsky, B. and Zalta, E. N. (2006). What is Neo-Logicism? *Bulletin of Symbolic Logic*, 12:60–99.

MacBride, F. (2003). Speaking with Shadows: A Study of Neo-Fregeanism. *British Journal for the Philosophy of Science*, 54:103–163.

Morton, A. and Stich, S., editors (1996). *Benacerraf and his Critics.* Blackwell, Oxford.

Parsons, C. (1965). Frege's Theory of Numbers. In (Black, 1965), pages 180–203.

Priest, G. (1994). Is Arithmetic Inconsistent? *Mind*, 103:337–349.

Shapiro, S. (1991). *Foundations Without Foundationalism: A Case For Second-Order Logic.* Oxford University Press, Oxford.

Thomson, J. J., editor (1987). *On Being and Saying: Essays for Richard Cartwright.* MIT Press, Cambridge, MA.

Wright, C. (1983). *Frege's Conception of Numbers as Objects.* Aberdeen University Press, Aberdeen.

Zalta, E. N. (1983). *Abstract Objects: An Introduction to Axiomatic Metaphysics.* Reidel, Dordrecht.

Zalta, E. N. (1999). Natural Number And Natural Cardinals As Abstract Objects: A Partial Reconstruction Of Frege's *Grundgesetze* In Object Theory. *Journal of Philosophical Logic*, 28:619–660.

Zalta, E. N. (2000). Neo-Logicism? An Ontological Reduction of Mathematics to Metaphysics. *Erkenntnis*, 53:219–226.

Abstraction and Nominalization in Leśniewski's Ontology

Nadine Gessler

1. Introduction

In this paper I intend to examine certain features that characterize the logicist construction that can be performed within the categorial and expansive framework provided by Leśniewski's Ontology, by putting these features in relation with the question of procedures of abstraction and nominalization. The latter will be placed in the problematic framework of classical logicism, relative to which the treatment of this question acquires all its relevance, given the fully effective resolution that Ontology makes possible.

As we have shown, Ontology, an extensional calculus of names of higher order, allows constructing Peano arithmetic, while conceding no more than an axiom of infinity to pure logic, and the development of this construction is not impeded by the difficulties met by the classical logicist approach[1]. Elaborated in the 1920s, Ontology historically responded to the problem of logical foundations of Mereology, the theory of collective classes that Leśniewski conceived following his discovery of Russell's antinomy in 1911. Starting from that date he concentrated all his efforts on a resolution *strictu sensu* of the contradiction. His work resulted in the development of three

[1] For a detailed presentation of the construction, see Gessler, Joray, Degrange (2005); cf. also Canty (1967) and Joray's paper on this volume.

theories: Mereology, Ontology and Protothetic – this last system being the propositional calculus on which Ontology is based[2]. The structural order of these theories is contrary to the chronological order of their conception. Mereology was conceived the first, in direct reaction to Russell's antinomy. Rejecting the usual definitions of class (or set) that he judged as not being consistent with the ordinary intuition of the concrete character of classes and of the individuals that these represent, Leśniewski introduced a collective definition of the class and showed that it is not subject to any antinomy similar in spirit to Russell's.

I need to emphasize through the evocation of the Leśniewskian program founded on the antinomy that without doubt, one of his major successes is having managed to reconcile the point of view of the class as collection and that of the class as extension of concept, conciliation that was inconceivable in the theoretical constructions elaborated by the tenants of classical logicism. Let us listen to Frege expressing his opinion on this subject, in his beautiful 1895 paper, directed against Schröder's algebra of logic, in which he reproaches the latter the fact of having privileged a purely extensional conception of classes, i.e. classes as collections of objects. This conception is not acceptable for Frege, both for its technical insufficiencies – as it does not allow seizing naturally the concept of empty class, and leads to the assimilation of an individual to the class that is composed only by him – and for its theoretical insufficiencies – as the concepts take logical precedence of their extension [Frege 1895: 456]. He writes:

> The complete difference, and indeed incompatibility, between *these conceptions of classes* is concealed ad first. Thus there arises a cruder conception of classes and extensions, side by side with a subtler one, the only that can be used in logic. (Frege 1895: 452-53)

The difference that Frege indicates, by qualifying it as incompatibility, expresses however fully the Leśniewskian program: starting from the adoption of a collective conception of classes, motivated by Leśniewski as possessing an intuitive legitimacy, as opposed to the

[2] For a detailed presentation of these three theories, see Miéville (2001, 2004) ; Gessler (2005).

concept of class (or set) as an abstract object, makes this one compatible with a language of pure logic, and in the end, clarifies the very reasons of the antinomy by actually solving it.

Before entering the heart of the subject, I add some words on Ontology. This theory, pinned above under the name of extensional calculus of names of higher order, constitutes a free, universal logic, and ontologically neutral. As I implied, its historical mission was to provide a foundational language for Mereology. This language was intended to meet two requirements: to be free from any antinomy of the Russell type and to avoid what is precisely not acceptable with Mereology, that is, introducing classes as abstract entities; in other words, it had to offer a treatment of the extensional dimension without introducing classes. This program will be realized on the basis of what is called the theory of semantic categories, theory that gives the general structure of the language in which the system of Ontology is formulated. The expressions of Ontology belong to distinct categories, which distinguish themselves through the fact that an expression belonging to a certain category cannot substitute for an expression of another category without losing the well formed character of the starting element where the substitution takes place. Formally, the stake of the theory of semantic categories is the same as for a theory of types, with the major difference that the tools function without an *ad hoc* installation. Besides, these categories are semantical in the sense that Ontology is an interpreted system, in which the formalism is under complete dependence of the semantic interpretation, the latter being anchored in the intuitions claimed by Leśniewski.

There are two categories recognized in the language as basic categories: that of propositions *S* and that of names *N*; the two systems of Protothetic and Ontology developed in parallel from this distinction. The other categories are functorial categories or derivated from the two basic categories. I will specify hereafter how any category can be introduced in the system. For the moment, I would like to expand on three major points that configure the paradigm of Ontology and that constitute the keys of the success of the logicist construction that can be performed starting from them.

2. The keys of success

The first point concerns the analysis of the singular proposition. The elementary proposition of Ontology has the form "*a* is *b*", formally "$a \varepsilon b$". The terms *a* and *b* belong to the semantic category of names, being thus either singular, or empty, or plural. The copula is thus analyzed as a formator functor of a proposition with two nominal arguments, which can be represented categorially as *S/NN*.

A proposition of the form "$a \varepsilon b$" is true if and only if the name *a* is a singular name and if the object designated by it belongs to the extension of objects indicated by the name *b*. These are the truth conditions that are formalized in the unique axiom of Ontology.

$$\lfloor ab \rfloor \lceil a\varepsilon b \equiv \lfloor \exists c \rfloor \lceil c\varepsilon a \rceil \wedge \lfloor cd \rfloor \lceil c\varepsilon a \wedge d\varepsilon a \supset c\varepsilon d \rceil \wedge \lfloor c \rfloor \lceil c\varepsilon a \supset c\varepsilon b \rceil \rceil$$

The axiom is a general sentence of the form $\lfloor \ldots \rfloor \lceil \ldots \rceil$, the characters $\lfloor$, $\rfloor$, $\lceil$ and $\rceil$ marking the quantifiers. Quantification, as I will specify hereafter, is of a nature different from that which operates in standard logic. This axiom is read:

Whatever the names *a* and *b*, a is *b* if and only if:

1) there is at least one *c* that is *a*
   (the name *a* denotes)
2) for all *c* and *d*, if *c* is *a* and *d* is *a*, then *c* is *d*
   (the name *a* denotes at most one individual)
3) for all *c*, if *c* is *a* then *c* is *b*
   (any object denoted by the name *a* is also denoted by the name *b*)

In other words, the name *a* is neither an empty, nor a plural name – i.e. a singular name – and the object that it denotes belongs to the extension of the name *b*.

It is crucial to understand here that it is by no means possible, when referring to the extension of a name, to associate it an object that would be the distributive class of the objects that it denotes. This logical treatment of names allows disqualifying the concept of class by limiting the concept of extension to its pure extensional dimension, be it empty, singular or multiple. That is the manner in which is neutralized, at this level of primitive Ontology, the antinomian feature that in the classical theories affects at the same time the question of the relation between extension as a multiplicity and extension as a unit. This question does simply not need to be considered any longer.

We will see hereafter that the fact that one can provide for the nominalization of the extension of a name is due to the categorial analysis and to the formal treatment of higher order. We will next measure all the importance of the analysis of the proposition that departs form the Fregean tradition in function/argument, and allows thus to escape all the thorny problems that accompanied the doctrines of functional symbols as incomplete symbols.

The second point refers to the ontological neutrality of Ontology. The source of this neutrality lies in the methods of interpretation of the quantification. Neither referential, nor substitutional, the quantify-cation is of categorial nature. It applies to variables of any semantic category, be it propositional, nominal or functorial, while its inter-pretation eludes the question of ontological commitment regarding the existence of objects constituting the possible significances of the related variables. Where there is the question of semantic categories, there is by no means a question of ontological categories. To each category is associated a quantification domain, which must be under-stood as the possibilities of extensional significances falling under the category in question. The forms $\lfloor v \rfloor \lceil A(v) \rceil$ and $\lfloor \exists v \rfloor \lceil A(v) \rceil$ must be read thus: "whatever the extensional significance allotted to the variable v – respectively for any extensional significance allotted to the variable v – it is the case that A(v)", *given the semantic category of variable v.* Without going into details, let us retain that Ontology achieves the purpose of a universal language, escaping the constraint of the adoption of a material of abstract entities, and being free both of any commitment, as of any ontological implication[3].

The third point refers to the constructive dimension of Ontology. This one is given by an internal adjustment to language of the defini-tion procedure. This adjustment arises under the form of an inferential

[3] For more details concerning quantification in Ontology, see Simons (1985), Miéville (1999), Joray (1999, 2005).

rule of definition allowing the treatment of explicit definitions, not as metalinguistic abbreviations, but as theses, by inserting them in the system through their equivalential formulation, expressed by using the biconditional operator '≡'. This explains why the unique primitive functor of the propositional calculus on which Ontology is based – Protothetic – is the biconditional, of category *S/SS*. The definition procedure ensures the language a categorial expressivity that is potentially infinite. It makes indeed possible, on the basis of the primitive significances contained in the axioms, the introduction of functors of any category built from the basic categories *S* and *N*.

The definition procedure rests on two distinct inferential directives: one of proposition type (inherited from Protothetic), the other of ontological or nominal type (necessitated with the introduction of the category of names and related to the primitive functor epsilon 'ε'). I give below a schematic presentation of both types, using each time two different notations (a conventional one and a "contextual" one, which was Leśniewski's original):

- Definition of propositional type (Dfs)

$\lfloor v_1 \ldots v_n \rfloor \lceil f(\!(v_1 \ldots)\ldots[\![\ldots v_n]\!] \equiv F_{v1\ldots vn} \rceil$ *conventional*

$\lfloor v_1 \ldots v_n \rfloor \lceil \equiv(f(\!(v_1 \ldots)\ldots [\![\ldots v_n]\!]\ F_{v1\ldots vn}) \rceil$ *contextual*

- Definition of nominal type (Dfn)[4]

$\lfloor v_1 \ldots v_n a \rfloor \lceil a\ \varepsilon\ g(\!(v_1 \ldots)\ldots[\![\ldots v_n]\!] \equiv .\ a \varepsilon a \wedge E_{av1\ldots vn} \rceil$ *conventional*

$\lfloor v_1 \ldots v_n a \rfloor \lceil \equiv(\varepsilon\{a\ g(\!(v_1 \ldots)\ldots[\![\ldots v_n]\!]\} \wedge (\varepsilon\{aa\}E_{av1\ldots vn}) \rceil$ *contextual*

Some remarks on these schemas are necessary. First, why two different writings? Being an expansive system, allowing the introduction

[4] In this type, the left hand formula is of form "*a* ε *definiendum*". As a singular sentence, it requires the subject term *a* to be a singular one. This explains the surprising conjunctive form of the right hand formula "a ε a ∧ *definiens*": the first conjunct of this formula "a ε a" only expresses the condition of singularity concerning the term *a*, the second conjunct being the *definiens* strictly speaking.

of new symbols by definitions, Ontology cannot be grounded on a given list of symbols for its variables and constants, which would be presemantically determined. The recognition of the different categories of the signs occurring in formulae is governed by a contextual approach of syntax: a prefixed writing using distinct parenthesizings that mark the various categories. These parenthesizings, qualified by the word "contexts", are formally characterized by the shape of the brackets and the number of argument places they delimit. In what concerns the defining diagrams, '(--)' and '{--}' are the primitive contexts. The first was associated to the category *S/SS* of the biconditional in the axiomatic basis of Protothetic, the second one to the category *S/NN* of epsilon, in the axiom of Ontology.

The other brackets in the defining diagrams are in dotted lines. This is to signal that their shape will depend on the number and respective categories of the arguments on which the defined functor will operate. If this functor is designed to belong to a category already present in the system, then it will have to be followed by the context corresponding to this category. If, by contrast, it is designed to belong to a new category, then a new context will need to be chosen.

As for the arguments of the functor to be defined, they can be divided in one or more contexts. In the first case (where the functor is followed by just one context), the definition is called *regular*, in the second case (where the functor is followed by more than one context), it is called a *parametric* definition. The functor introduced by a parametric definition is a *multi-link*-functor, i.e. a functor forming functor. At last, the functor to be defined can also not have any context; in this case the definition is said to be an *absolute* one. Here you have some examples. I only specify the contextual writing for the first two.

Df1: ⌊a⌋ ⌈ !{a} ≡ ⌊∃b⌋ ⌈b ε a⌉ ⌉ *Dfs*, !, *S/N*

⌊a⌋ ⌈≡(!{a}⌊∃b⌋ ⌈ε{b a}⌉ ⌉

!{a} is read: "the name *a* denotes".

Df2: ⌊ab⌋ ⌈⊂{ab} ≡ ⌊c⌋ ⌈cεa ⊃ cεb⌉ ⌉ *Dfs*, ⊂, *S/NN*

⌊ab⌋ ⌈≡(⊂{ab} ⌊c⌋ ⌈⊃ (ε {ca} ε {cb})⌉) ⌉

This is the definition of inclusion for names; ⊂{ab} is read: "the name *a* is included in the name *b*", that is, "the extension of the name *a* is included in the name *b*".

Df3: ⌊ab⌋ ⌈≈{ab} ≡ ⌊c⌋ ⌈cεa ≡ cεb⌉ ⌉ *Dfs*, ≈, *S/NN*

≈ represents the *extensional nominal identity*; ≈{ab} is read: "the names *a* and *b* have the same extension".

Df4: ⌊ab⌋ ⌈≈⟨a⟩{b} ≡ a ≈ b ⌉ *Dfs*, ≈, *(S/N)/N*

This definition is the parametric version of the functor defined above. The element ≈⟨a⟩, with *a* as parameter, followed by the context {-}, whose argument is of category *N*, is of category *S/N*; whereas the functor ≈, associated to the context ⟨-⟩, whose argument is of category *N*, is of category *(S/N)/N*.

The choice of these last two definitions is not innocuous. We will actually find them next, during our discussion on the act of abstraction and nominalization, and we will then be able to judge the major rôle that the procedure of parameterization plays in Ontology.

Df5: ⌊a⌋ ⌈a ε Λ ≡. aεa ∧ ~(aεa) ⌉ *Dfn*, Λ, *N*

Definition Df5 introduces the empty or contradictory name.

Df6: ⌊abc⌋⌈a ε ∩⌈bc⌉ ≡. aεa ∧ aεb ∧ aεc⌉ *Dfn*, ∩, *N/NN*

Df7: ⌊aαβ⌋⌈∩\αβ/{a} ≡. α{a} ∧ β{a}⌉ *Dfs*, ∩, *(S/N)/(S/N)(S/N)*

These definitions are two analogues of logical product: nominal intersection and predicative intersection. The last definition is equally parametric. The functor ∩\αβ/ is of category *S/N*; the defined functor ∩, associated to the context \--/, is of category *(S/N)/(S/N)(S/N)*. We will read thus ∩\αβ/{a}: "the name *a* forms/is the intersection of predicates α and β". ∩\αβ/ represents thus the intersection of predicates α and β.

The following three definitions are analogous to the first three, for the category *S/N*:

Df8: ⌊α⌋ ⌈![α] ≡ ⌊∃a⌋ ⌈α{a}⌉ ⌉ *Dfs*, !, *S/(S/N)*

Df9: ⌊αβ⌋ ⌈⊂[αβ] ≡ ⌊a⌋ ⌈α{a} ⊃ β{a}⌉ ⌉ *Dfs*, ⊂, *S/(S/N)(S/N)*

Df10: $\lfloor\alpha\beta\rfloor \lceil \approx[\alpha\beta] \equiv \lfloor a\rfloor \lceil \alpha\{a\} \equiv \beta\{a\}\rceil \rceil$ *Dfs*, ≈, *S/(S/N)(S/N)*

Notice that the choice of the same symbols as in the precedent definitions is not accompanied by any ambiguity, since the contexts allow distinguishing one constant from the other. It is the same with the parametric definitions Df4 and Df7.

3. The elements of the logicist construction

Let us now focus on the determinations and positive incidences on the logicist construction of the above described three characteristic features of Ontology (the analysis of proposition, the categorial nature of quantification and the definition procedure). I have selected four issues, the list being of course not exhaustive.

1) Peano arithmetic is built without resorting to the concepts of class or set, not even as linguistic conveniences, like in the *Principia Mathematica*. The cardinal number is defined as the property of a name. It is then of category *S/N* and the definition to which we arrived is the following:

$$\lfloor\alpha\rfloor \lceil Cn[\alpha] \equiv \lfloor\exists a\rfloor \lceil \alpha \approx \infty\langle a\rangle\rceil \rceil$$

I leave aside for the moment the comments related to the formal expression of this thesis, and provide only an intuitive reading:

> α is a cardinal number if and only if there is a name *a* such that α expresses *being equinumeric to* a.

2) The definition procedure is limited to the use of explicit definitions adjusted within the language, as we have emphasized above. Logicism is thus equipped with a really constructive dimension. It is a construction, an Ontology among the other possible ones, and its development is entirely regulated by the formal tools of the adopted logic.

3) Except for the presence of an axiom of infinity, this logicism is ontologically neutral. The analysis of the concept of number is in this way released from the yoke of abstract entities and from the need of dealing with the question of the nature of numbers.

4) The stratification of language is managed without making recourse to the concept of systematic ambiguity, on which rests type theory in the *Principia Mathematica*. If, as in *Principia*, there is an arithmetic at "each floor", the modes of formalization allow apprehending the question on the hierarchy of types in its generating movement, and finding isomorphism not by the external requisite that it is seen and it arises from a systematic analogy, but by the fact that it proves itself and it functions at the interior of the language, under the cut of the formal modes of this language. This stratification is managed by the possibility of reproducing the axiom of Ontology at each floor, by previously defining the so-called *higher-order epsilons*. For example, on the basis of the functor of extensional identity $\approx$ defined above (Df3), the following definition of a higher epsilon can be written, of category *S/(S/N)(S/N)*:

$$\mathrm{Df}(\varepsilon_{\approx})\colon \lfloor\alpha\beta\rfloor \lceil \varepsilon[\alpha\beta] \equiv. \lfloor\exists a\rfloor \lceil \alpha\{a\} \wedge \beta\{a\}\rceil \wedge \lfloor ab\rfloor \lceil \alpha\{a\} \wedge \alpha\{b\} .\supset \approx\{ab\}\rceil\rceil$$

This definition can be read: for any functor α and β of category *S/N*, α is β if and only if for some name *a*, α and β are verified by *a* and, for any name *a* and *b*, if α is verified by *a* and by *b*, then *a* and *b* have the same extension.

Afterwards we can derive the structural equivalent of the axiom of Ontology, in which variables of category *S/N* replace the nominal variables, and the defined superior epsilon of category *S/(S/N)(S/N)* replaces the primitive epsilon of category *S/NN*. Let the following thesis:

$$\mathrm{Ax}(\varepsilon_{\approx})\colon\ \lfloor\alpha\beta\rfloor \lceil \varepsilon[\alpha\beta] \equiv$$

$$\lfloor\exists\gamma\rfloor \lceil \varepsilon[\gamma\alpha]\rceil \wedge \lfloor\gamma\rfloor \lceil \varepsilon[\gamma\alpha] \supset \varepsilon[\gamma\beta]\rceil \wedge \lfloor\gamma\delta\rfloor \lceil \varepsilon[\gamma\alpha] \wedge \varepsilon[\delta\alpha] .\supset \varepsilon[\gamma\delta]\rceil\rceil$$

We can consequently dispose of the structural analogue of all the theses susceptible to be registered in the primitive Ontology. For example, for the definition Df1: $\lfloor a\rfloor \lceil !\{a\} \equiv \lfloor\exists b\rfloor \lceil b\varepsilon a\rceil\rceil$, we can derive, on the basis of superior functor ! of category *S/N* introduced with the definition Df6, the following thesis: $\lfloor\alpha\rfloor\lceil ![\alpha] \equiv \lfloor\exists\beta\rfloor \lceil\beta\varepsilon\alpha\rceil\rceil$.

What do we retain from here? The theory allows the treatment of higher entities as pseudo-names, without reification, and without returning them to a statute of logical fictions, as is in the *Principia Mathematica*. As each language layer managed by a higher epsilon imitates by its isomorphism the nominal layer managed by the primitive epsilon, the process of nominalization of the superior entities is thus validated by their pseudo-nominal representation.

Having expressed here the accomplishment and the success of Ontology in what regards nominalization, I rephrase the considerations by anchoring them in the difficulties met on this subject by classical logicism, to plunge them next in the theoretical framework of Ontology, and to examine them from a structural point of view.

4. Where we escape from the misfortunes of classical logicism

Russell writes in the *Principles*, aiming to describe the relationship between extension as a multiplicity and extension as a unit:

> [...] without a single object to represent an extension, mathematics crumbles. (1903: 489)

This quotation alone expresses the threshold at which hit – and even broke – classical logicism. It is the concept of class as one – or Frege's *Werthverlauf* – that is found in the heart of the antinomy, and that led Russell, for want of anything better, to the theory of logical fictions, carried by the technique of contextual definitions. It is with such a technique, attesting that a certain type of expression functioning seemingly as a unit of significance is in fact an incomplete symbol, that Russell faced the crucial problem of nominalization of the class as many. While classes are treated as linguistic conveniences, as fictitious objects, there is nothing that resists nominalization. The difficulty in cause is thus eliminated. Here I only evoke, without developing it, this crucial problem that prevented classical logicism from accomplishing in the way its founders had dreamed.

Let us now consider the issue within Ontology. How is the process of nominalization regulated? How can an extension or a number represent? I will approach the answer to these questions through two theses of Ontology. The first is the ontological expression of Frege's

Law V, which admits the logical equivalence between the identity of extensions and the formal equivalence of concepts. Carrying in germ the contradiction that was brought to daylight by the discovery of Russell's antinomy, this axiom can be expressed in the following manner, where I make use of the usual symbol for the class abstractor:

Law V: $\hat{x}F(x) = \hat{x}G(x) \equiv (\forall x)(F(x) \equiv G(x))$

The corresponding thesis in Ontology is[5]:

Th1: $\lfloor ab \rfloor \lceil \approx\langle a\rangle \approx \approx\langle b\rangle .\equiv. a \approx b \rceil$

I leave for later the comments concerning the formal expression of this thesis that makes use of the functors defined in section 2, and for now I give its following reading:

> Two names determine identical *expressions of extension* if and only if they are coextensive.

The second thesis is the ontological version of Hume's Principle, expressing the identity criterion for numbers. Considering the principle as an implicit definition of "the number of", neo-Fregeans showed that the essence of Frege's construction could be rephrased, without the famous Law V, on the unique basis of classical second-order logic expanded with Hume's Principle.

Hume's Principle: $(\forall FG)(\text{Number}(F) = \text{Number}(G) .\equiv. F \infty G)$

The ontological analogue of this principle is the following thesis:

Th2: $\lfloor ab \rfloor \lceil \infty\langle a\rangle \approx \infty\langle b\rangle .\equiv. a \infty b \rceil$

As before, I restrain for now to a reading, which will however allow shedding some light on the definition given above for the cardinal number:

> The cardinal number of *a* is identical to the cardinal number of *b* if and only if *a* is equinumeric to *b*.

[5] This thesis highlights the three available significances relative to the symbol $\approx$ (Df3, Df4, Df10). To be sure that this triple use is legitimate and that all confusion is removed, it suffices to rewrite the thesis in full contextual notation: $\lfloor ab \rfloor \lceil \equiv(\approx[\approx\langle a\rangle \approx\langle b\rangle] \approx\{ab\}) \rceil$.

For better understanding of the proposed readings, let us consider more in detail the functors that come into play in these theses.

• Th1: $\lfloor ab \rfloor \lceil \approx\langle a\rangle \approx \approx\langle b\rangle .\equiv. a \approx b \rceil$

i) $\approx$, occurring in "a $\approx$ b", is the functor of nominal extensional identity. Being of category *S/NN*, it was introduced into the preceding section, by definition Df3: $\lfloor ab \rfloor \lceil a \approx b .\equiv. \lfloor c \rfloor \lceil c\ \varepsilon\ a \equiv c\ \varepsilon\ b \rceil$. It is a reflexive, symmetrical and transitive relation.

ii) $\approx\langle - \rangle$ is the parametric version of the functor $\approx$, introduced by definition Df4: $\lfloor ab \rfloor \lceil \approx\langle a\rangle\{b\} .\equiv. a \approx b \rceil$. Let's take a look at this definition. Taking into account the significance of the *definiens* of this definition "a $\approx$ b", we can read the *definiendum* "$\approx\langle a\rangle\{b\}$" as "the *b* form/are the extension of *a*", i.e. "the names *a* and *b* have the same extension". Consequently $\approx\langle a\rangle$, of category *S/N*, can be assimilated to *being the extension of the name a*. And we can, under the authority of this categorial analysis, call $\approx\langle a\rangle$, *the extension of the name a*.

iii) $\approx$, occurring between the elements of the left hand expression "$\approx\langle a\rangle \approx \approx\langle b\rangle$" of the thesis is the functor of extensional identity between predicates of category *S/N*. It was defined with Df10: $\lfloor \alpha\beta \rfloor \lceil \approx[\alpha\beta] \equiv \lfloor a \rfloor \lceil \alpha\{a\} \equiv \beta\{a\} \rceil\rceil$.

• Th2: $\lfloor ab \rfloor \lceil \infty\langle a\rangle \approx \infty\langle b\rangle .\equiv. a \infty b \rceil$

i) the symbol ∞ appearing in "a ∞ b" is that of the relation of equinumericity between names. This relation is defined in a similar way with what is done within a classical framework between classes or sets: two names *a* and *b* are equinumeric if and only if there is a one-one relation between them, *a* represents the domain of the relation, and *b* its co-domain. In the language of Ontology and our arithmetic construction, one have[6]:

$$\lfloor ab \rfloor \lceil a \infty b \equiv \lfloor \exists R \rfloor \lceil \mathit{OneOne}(R) \wedge \mathit{Dom}(R)\{a\} \wedge \mathit{Cdom}(R)\{b\} \rceil \rceil$$

The functor ∞ has the properties of reflexivity, symmetry and transitivity.

ii) $\infty\langle a \rangle$: it is the parametric version of the relation of nominal equinumericity ∞. We introduce it with this definition:

$$\lfloor ab \rfloor \lceil \infty\langle a \rangle\{b\} .\equiv. a \infty b \rceil$$

According to a categorial reading similar with the one done on the parametric version of the extensional identity, $\infty\langle a \rangle$, of category *S/N*, expresses the cardinal number of *a*, in the categorial sense of *being the cardinal number of a*.

iii) The relation $\approx$, in the first member of the thesis "$\infty\langle a \rangle \approx \infty\langle b \rangle$", is – like in Th1 – that of extensional identity between elements of category *S/N*.

Let us subject now the preceding considerations to a more precise examination. First of all, a remark must be done concerning the identity expressed with the functor $\approx$, of category *S/(S/N)(S/N)*, and introduced with definition Df10. We notice that the logical paradigm of Ontology allows avoiding the absolute conception of identity as relating exclusively to objects. In the expression "$\alpha \approx \beta$", $\approx$ is not a referential identity, but an extensional identity between predicates of category *S/N*. It is by no means an identity expressing a relationship between abstract objects starting from the functors α and β, i.e., in Fregean language, the extensions of concepts. As we know, such writing is rejected by Frege as it does not reflect the character of

[6] Taking into account the following definitions:
- being a one-one relation between singular names:

$$\lfloor R \rfloor \lceil \mathit{OneOne}(R) \equiv \lfloor abc \rfloor \lceil R\{ac\} \wedge R\{bc\} .\vee. R\{ca\} \wedge R\{cb\} :\supset a \,\varepsilon\, b \rceil \rceil$$

- being the domaine of a relation of category *S/NN*:

$$\lfloor Ra \rfloor \lceil \mathit{Dom}(R)\{a\} \equiv \lfloor b \rfloor \lceil \lfloor \exists c \rfloor \lceil R\{bc\} \equiv b \,\varepsilon\, a \rceil \rceil \rceil$$

- being the co-domaine of a relation of category *S/NN*:

$$\lfloor Ra \rfloor \lceil \mathit{Cdom}(R)\{a\} \equiv \lfloor b \rfloor \lceil \lfloor \exists c \rfloor \lceil R\{cb\} \equiv b \,\varepsilon\, a \rceil \rceil \rceil$$

non-saturation of the functions. According to his analysis, when mathematicians use this – incorrect – notation, they use in an implicit way the fundamental Law V and the possibility of passing from the assertion of identity between the values taken by two functions on the arguments, to the assertion of identity between suits of values. In Ontology, such an expression will be understood as expressing "*the* functor α and *the* functor β have the same extension", in the same way in which will be read, for example, ![α] (*cf.* Df8), "*the* functor α denotes", although α and β do not have the statute of a name in either expression. There is no impossibility here of naming the functions, while eluding any problem of reification. In addition, as I previously emphasized, the nominalization of superior entities is completely legitimated in Ontology by the possibility of raising the axiom to any higher category. It is thus possible to reproduce by the definition approach the arithmetic of a given category towards superior categories. This fact shows, in addition, that the constructed arithmetic is indifferent to the nature of entities to which the numbers apply.

It is equally necessary to insist on the fact that ontological Th2 does not say the same thing as Hume's Principle, which is an implicit definition. In Ontology abstraction does not define, but it nominalizes through the procedure of parameterization that authorizes the disconnection from a certain linguistic element, categorically autonomous. Moreover, by contrast with Hume's Principle, cardinal numbers are not objects to be counted as elements of the universe.

In the light of the previous affirmations, let us now turn towards the definition of cardinal number (given in section 3):

$$\lfloor\alpha\rfloor\lceil \mathrm{Cn}[\alpha] \equiv \lfloor\exists a\rfloor\lceil\alpha \approx \infty\langle a\rangle\rceil\rceil$$

This definition introduces the functor Cn, *being a cardinal number*, of category *S/(S/N)*, a cardinal number being of category *S/N*. We can introduce a similar thesis to define *being an extension*. Say:

$$\lfloor\alpha\rfloor\lceil \mathrm{Ext}[\alpha] \equiv \lfloor\exists a\rfloor\lceil\alpha \approx \approx\langle a\rangle\rceil\rceil$$

This definition is read: "α is an extension if and only if there is a name *a* such that α expresses being the extension of *a*".

Let us consider now, in a parallel manner, some theses that are associated with these parametric functors expressing the extension of a name and the cardinal number of a name.

$$\lfloor a \rfloor \lceil \infty\langle a\rangle\{a\}\rceil \qquad \lfloor a \rfloor \lceil \approx\langle a\rangle\{a\}\rceil$$

$$\lfloor a \rfloor \lceil\ !\ [\infty\langle a\rangle]\rceil \qquad \lfloor a \rfloor \lceil\ !\ [\approx\langle a\rangle]\rceil$$

$$\lfloor a \rfloor \lceil \lfloor \exists\alpha \rfloor \lceil \alpha \approx \infty\langle a\rangle\rceil\rceil \qquad \lfloor a \rfloor \lceil \lfloor \exists\alpha \rfloor \lceil \alpha \approx \approx\langle a\rangle\rceil\rceil$$

These theses reveal that to any name, be it empty, singular or plural, is associated a function, of category *S/N*, that expresses the cardinal number of *a*. Similarly, it can be associated a function that expresses the extension of *a*. This result is corroborated to the following theses, using the defined functors for *being a cardinal number* (Cn) and for *being an extension* (Ext).

$$\lfloor a \rfloor \lceil \mathrm{Cn}\ [\infty\langle a\rangle]\rceil \qquad \lfloor a \rfloor \lceil \mathrm{Ext}\ [\approx\langle a\rangle]\rceil$$

They are read: "any name – whether it denotes or not – determines a cardinal number", and "any name – whether it denotes or not – determines an expression of extension". Let us notice that the latter thesis fulfils the first requisite of Russell's theory of classes i.e. each propositional function with an argument must determine a class which can be regarded as the collection of all the arguments satisfying the property in question[7].

In conclusion, the parametric functors $\infty\langle a\rangle$ and $\approx\langle a\rangle$ are thus *façons de parler* the extension and the number of a name. Both of category *S/N*, these functors provide the nominalization of the cardinal number, respectively, the extension of the name *a*. In what regards the question of knowing how to legitimate this process of nominalization, I have already answered. This legitimating concerns epsilons of a higher order, qualified thus for being functors of category *S/CC*, where $C \neq N$, and which, allowing to derive the equivalent of the axiom for their category, are also paraphrasable by "is", as proposition forming functors for pseudo-names. As each layer of language managed by a superior epsilon imitates, so to speak, the nominal layer

[7] See (Russell 1919: 184f) and (Whitehead & Russell 1927: 76f).

managed by the primitive epsilon, the process of nominalization of superior entities is therefore validated by their pseudo-nominal representation.

We will also retain the enlargement of the concept of name that Ontology calls. This concept is thus able to characterize any expression designating a linguistic entity of the language, either being intended to designate names in the strict sense (of category *N*, empty, singular or plural), or "names" of functors, functors of functors, etc., whatever the degree of categorial complexity of the functors in question.

5. Conclusion

That is how Ontology unties inherent difficulties within the classical conceptual framework and relative to the procedure of abstraction and of nominalization. Thanks to its analysis of proposition, Ontology, as we saw, reconciles the concept of name and that of function by a formal and categorial adjustment of the linguistic process of nominalization, without having the inconvenient of a complication of writing to which Frege was subject in his object/concept distinction and analysis of the language that marks this distinction. It also shows that its categorial and expansive tools refute Frege, who exploited the natural language to show, without possible compromise, that numbers are objects. Moreover, it has the merit, while allowing the functions to be named in the same manner as any entity of language, of advantages such as naturalness and simplicity, if one contemplates the practice of mathematics and an enlarged interpretation of the concept of name in adequacy with the linguistic process of objectivation. And finally, it shows that the logicist thought can find rest while developing outside of any dogmatic realism or razor of Occam that multiply the logical fictions.

References

AJDUKIEWICZ K. (1935). Die syntaktische Konnexität. *Studia Philosophica* 1. 1-27. [Engl. transl. in McCall S. *Polish Logic 1920-1939*. Oxford Univ. Press. 1967. 207-231].

BAR-HILLEL Y. (1953). A quasi-arithmetical notation for syntactic description. *Language* 29. 47-58.

BOOLOS G. (1987). The consistency of Frege's *Foundations of Arithmetic*. In Thomson J. (ed.). *On Being and Saying: Essays in Honnor of Richard Cartwright*. Cambridge Mass: MIT Press. 3-20.

BOOLOS G. (1998). *Logic, Logic and Logic*. Cambridge Mass: Harvard Univ. Press.

CANTY J. T. (1967). *Leśniewski's Ontology and Gödel Incompleteness Theorem*. PhD. Thesis. Univ. of Notre Dame.

CANTY J. T. (1969). The numerical epsilon. *Notre Dame Journal of Formal Logic* 10.1. 47-63.

FREGE G. (1893). *Grundgesetze der Arithmetik*. Jena. [Reprint Hildesheim: Olms].

FREGE G. (1893). Kritische Beleuchtung einiger Punkte in E. Schröder's Vorlesungen über die Algebra der Logik, *Archiv für systematische Philosophie* 1, 433-456. [Engl. trans. in *Translations from the Philosophical Writings of Gottlob Frege*, ed. P. Geach & M. Black, 2nd ed., 1960, Oxford, 86-106].

GESSLER N., JORAY P., DEGRANGE C (2005). *Le logicisme catégoriel. Travaux de logique*, n° 16. Université de Neuchâtel.

GESSLER N. (2005). *Introduction à l'œuvre logique de S. Leśniewski. III. La Méréologie. Travaux de logique du CdRS*. Université de Neuchâtel.

HALE B., WRIGHT C. (2001). *The Reason's Proper Study. Essays towards a Neo-Fregean Philosophy of Mathematics*. Oxford: Clarendon.

HIŻ H. (1977). Descriptions in Russell's Theory and in Ontology. *Studia Logica* 36.4. 271-283.

HUSSERL E. (1891). *Philosophie der Arithmetik*. Halle: CEM Pfeffer.

HUSSERL E. (1900). *Logische Untersuchungen*. Halle: M. Niemeyer.

JORAY P. (1999). Domaines de quantification et catégories syntaxico-sémantiques. *Rôle et enjeux de la notion de catégorie en logique. Travaux de logique du CdRS* 13. Université de Neuchâtel. 43-62.

JORAY P. (2002). Logicism in Leśniewski's Ontology. *Logica Trianguli* (Łódź, Nantes, Santiago de Compostella) 6. 3-20.

JORAY P. (2005). La quantification catégorielle. In Joray P. (éd.). *La quantification dans la logique moderne*. Paris: L'Harmattan. 233-260.

JORAY P. (2006). La définition dans les systèmes logiques de Łukasiewicz, Leśniewski et Tarski. In Pouivet R., Rebuschi M. (éds). *La philosophie en Pologne 1918-1939*. Paris: Vrin. 203-222.

LEJEWSKI C. (1954). Logic and Existence. *British Journal for the Philosophy of Science* 5. 104-119.

LEJEWSKI C. (1985). Accomodating the informal notion of class within the framework of Leśniewski's Ontology. *Dialectica* 39. 217-241.

LEŚNIEWSKI S. (1989). *Sur les fondements de la mathématique. Fragments (discussions préalables, méréologie, ontologie)*. Trad. de Kalinowski G., Préface de Miéville D., Paris: Hermès.

LEŚNIEWSKI S. (1992). *Collected Works* (2 vol.). Surma S. J., Srzednicki J. T., Barnett D. I. (eds). Warszawa: PWN / Dordrecht: Kluwer.

LUSCHEI E. C. (1962). *The Logical System of Leśniewski*. Amsterdam: North Holland.

MIÉVILLE D. (1984). *Un développement des systèmes logiques de Stanisław Leśniewski. Protothétique, Ontologie, Méréologie*. Berne: Peter Lang.

MIÉVILLE D. (2001-04). *Introduction à l'œuvre logique de S. Leśniewski. I. La Protothétique, II. L'Ontologie. Travaux de logique du CdRS*. Neuchâtel: Université. [Fasc. III. *La Méréologie*, Gessler (2005)].

RUSSELL B. (1903). *The Principles of Mathematics*. London: Allen & Unwin.

RUSSELL B. (1919). *Introduction to Mathematical Philosophy*. London: Allen & Unwin.

SIMONS P. M. (1995). Leśniewski and Ontological Commitment. In Miéville D., Vernant D. (éds). *Stanisław Leśniewski Aujourd'hui.* Grenoble: Univ. Mendès France / Neuchâtel: CdRS. 103-119.

SŁUPECKI J. (1955). S. Leśniewski's Calculus of Names. *Studia Logica* 3. 7-70.

SOBOCIŃSKI B. (1949). L'analyse de l'antinomie russellienne par Leśniewski. *Methodos* 1. 94-107, 220-228, 308-316 and *Methodos* 2 (1950). 237-257.

SRZEDNICKI J. T. J., RICKEY V. F. (eds). (1984). *Leśniewski's Systems: Ontology and Mereology.* Boston, The Hague: Nijhoff / Wrocław: Ossolineum.

SRZEDNICKI J. T. J., STACHNIAK Z. (eds). (1988). *Leśniewski's Systems: Protothetic.* Dordrecht: Kluwer.

WHITEHEAD A. N., RUSSELL B. (1927). *Principia Mathematica.* 2nd ed. Cambridge Univ. Press. [1st ed. 1910].

WRIGHT C. (1983). *Frege's Conception of Numbers as Objects.* Aberdeen Univ. Press.

Logicisme combinatoire et théorie de la définition

Jean-Pierre Ginisti

Le logicisme auquel on s'adressera pour le questionner sera compris comme la thèse suivante: on peut n'utiliser que des termes logiques et des procédures logiques de preuve pour reformuler l'ensemble des mathématiques. Bien sûr, un point central porte sur ce qu'il faut entendre par «logique».

Par commodité d'expression, on dira «mathématiques» pour désigner les mathématiques extralogiques, c'est-à-dire tout ce qui en mathématiques n'appartient pas à ce qu'on nomme «la logique mathématique».

Le problème posé par le logicisme doit être distingué de ce qui semble être un faux problème: il ne s'agit pas de se demander si dans les mathématiques, élaborées ou lues, on ne trouve directement que des lois ou des règles logiques (des détachements, des contrapositions de l'implication, etc.). Il est trop évident que ce n'est pas le cas.

Soit, par exemple, à démontrer que $\mathbb{Z}$ et $\mathbb{N}$ ont la même puissance. On effectue un certain va-et-vient entre les éléments de $\mathbb{Z}$ et les éléments de $\mathbb{N}$ qui les apparie. Le logicisme n'entend pas nécessairement remplacer le va-et-vient par des procédures logiques; il entend après reconstruction de la théorie en système logique dépasser le va-et-vient par les moyens logiques d'avoir le théorème «$\mathbb{Z}$ et $\mathbb{N}$ ont la même puissance». Il en irait de même pour des procédés anciens comme le crible d'Ératosthène qui obtient la suite des nombres premiers en écrivant la suite des entiers et en y barrant successivement tous les multiples de 2, de 3, de 5, etc.

Ainsi ne peut-on pas dire que dans toute preuve mathématique une partie des moyens mis en oeuvre peut être logique (comme la portion par l'absurde de la procédure diagonale, par exemple, pour établir que l'intervalle [0, 1] de ℝ n'a pas la puissance de ℕ), mais que la partie décisive et spécifique échappe à la logique (ici le type de parcours effectué selon la diagonale, justement). Le logicisme ne soutient pas que toutes les procédures qui interviennent *de facto* dans une démonstration mathématique seraient à valider par une traduction juxtalinéaire qui en donnerait des correspondants logiques. Il veut refondre la théorie. Il veut transcrire toute opération portant sur des objets (fussent ils les éléments de ℤ et de ℕ) et qui permet d'établir sur eux une certaine propriété formulée par un énoncé B, en opérations logiques portant sur un énoncé A et en obtenant le même énoncé B. Jean-Louis Gardies observe que derrière les procédures et les constructions, ou derrière les mots qui «se réfèrent à une activité humaine» (mener une parallèle, prolonger un segment), «se dissimulent de simples reconnaissances d'existence» (il existe une parallèle), ou des actes qui s'évitent, par exemple en changeant de primitifs (non plus le segment de droite à prolonger, mais la droite illimitée). On le voit clairement dans la reprise des procédures chez Hilbert et Bernays «sur le mode théorique» et qui suffit à constituer les démonstrations des théorèmes [3].

Autrement dit, le va-et-vient est remplacé par «il existe une bijection entre ℤ et ℕ» qu'on donne et qui établit le théorème sur l'équipotence de ℤ et ℕ, sans qu'on ait rien d'autre à mentionner. L'heuristique est essentielle, mais à d'autres égards.

Même l'intuitionnisme pourrait accepter un tel logicisme. Il exigerait seulement que la reprise théorique soit conforme à des procédures d'un type agréé (exemplification d'un cas, notamment). Cette condition supplémentaire, judicieuse ou non, n'interdit pas le logicisme, mais lui donne une certaine forme.

«Presque tout le dissentiment [...] au sujet du logicisme [...], remarque Dominique Dubarle, tient à ce fait que, dans le sujet qui fait des mathématiques, l'activité vivante et créatrice domine et déborde inévitablement l'activité dite et fixée dans l'instrumentation du langage» [2]. C'est alors une mauvaise querelle car un logicisme n'entend pas reproduire toutes les activités humaines déployées dans la preuve originale. Il aurait, sinon, ce défaut rédhibitoire de laisser croire qu'il ne faut pas deviner, expérimenter, avant de prouver, mais à tout moment respecter des canons. Le logicisme serait inadéquat en logique elle-même s'il soutenait qu'on ne peut y chercher un résultat qu'en appliquant des règles logiques disponibles, ou de nouvelles règles logiques. Church par exemple indique quelles lignes effacer dans la table d'un certain foncteur ternaire pour obtenir plusieurs définissants [1].

Si le logicisme ne retrouve pas un résultat qui semblait acquis ce n'est pas toujours que la logique n'a pas réussi à rejoindre la pensée vivante, comme on le dit souvent un peu vite, notamment quand l'infini intervient. Cela peut être aussi parce que la pensée vivante s'est leurrée. Il convient de rester circonspect contre la tendance actuelle qui jauge une entreprise formelle à sa proximité de la pensée vivante.

Il arrive qu'on objecte au logicisme que les erreurs dans les démonstrations mathématiques sont rarement des paralogismes, mais proviennent d'une supposition implicite fausse, comme c'est le cas dans la démonstration erronée par Schröder du théorème dit de Cantor-Bernstein [6]. Mais justement, c'est l'obligation de traquer l'implicite en cherchant à donner une *demonstratio ad oculos*, comme on le dit parfois – une exactitude qui saute aux yeux – qui fait l'un des prix du logicisme.

Au demeurant, celui-ci pose d'ailleurs deux grands sous-problèmes: 1) Peut-on définir (ou plutôt redéfinir) les termes mathématiques en primitifs logiques? 2) Peut-on démontrer (ou plutôt re-démontrer) les théorèmes mathématiques à partir des seules règles logiques et sans utiliser de prémisses extralogiques?

La définissabilité des termes mathématiques en primitifs logiques est une condition nécessaire, sans doute, mais non suffisante du logicisme. Pourtant, on ne s'intéressera à peu près qu'à elle ici puisqu'un problème crucial du logicisme est de savoir ce que sont les termes logiques qui peuvent constituer des définissants des termes mathématiques. On ne dira que quelques mots du deuxième sous-problème.

Peut-on se passer de prémisses extralogiques, proprement mathématiques? Ce point est d'autant plus important que le logicisme a tendance à réduire les primitifs, même s'il faut des axiomes plus nombreux ou plus complexes, ce que la réduction des primitifs entraîne souvent. La substance mathématique peut donc s'y réfugier mieux encore. Cela a constitué une objection historiquement importante, par exemple à l'égard de l'axiome «il existe au moins un ensemble infini» qui n'est pas une loi logique. Mais il y a une parade logiciste: Soit A_1, ..., A_n les axiomes logiques d'un système et B un axiome propre extralogique, alors pour tout théorème C tel que $A_1, \ldots, A_n, B \vdash C$, on a $A_1, \ldots, A_n \vdash (B \supset C)$, par le métathéorème de la déduction, dès qu'on donne au système les moyens logiques d'obtenir celui-ci. On n'a plus à affirmer la vérité factuelle de B. Rappelons que Bourbaki, malgré son hostilité à la logique, fait usage sans réticence de ce type de preuve.

Une autre opposition au logicisme vient des résultats gödeliens sur l'indécidabilité. Ils ne condamneraient le logicisme, pourtant, que si quelque autre approche, et bien sûr *à niveau égal d'exigences*, évitait l'indécidable. Or, ce n'est pas le cas. Soit HC l'hypothèse du continu. La vérité que HC est indécidable dans le système Zermelo-Fraenkel est beaucoup plus solide que la prétendue vérité de HC dans une théorie intuitive qu'on lui opposerait dérisoirement et qui triomphe à la Pyrrhus. Quant au fait qu'un système suffisamment fort ne peut pas établir sa propre consistance, la situation ne serait pas meilleure s'il pouvait l'établir car le système serait juge et partie.

Nous ne poserons le problème de la définissabilité qui va être le nôtre que dans les termes classiques (en bref, une définition est un énoncé métalangagier, non créateur et où le défini abrège un définissant, même s'il y a des raisons inexprimées et importantes de s'y intéresser). La définition au sens de Leśniewski ne sera pas considérée, car la logique combinatoire qui va intervenir devrait être adaptée à cette conception, ce qui supposerait antérieurement une autre investigation.

D'une façon sommaire, les termes logiques (comme 'non', 'donc') sont ceux qui peuvent constituer l'armature d'une inférence, ceux *par* lesquels on raisonne, les termes non logiques (comme 'cosinus') ceux *sur* lesquels on raisonne. Certes, on peut raisonner aussi sur un terme logique mais c'est alors «en seconde intention». Il est naturel que le vocabulaire logique se manifeste d'abord par son antériorité historique d'emploi dans le langage courant.

Beaucoup d'auteurs s'accordent néanmoins pour dire que le lexique des termes logiques ne peut pas être donné sans quelque arbitraire. Certains y admettront par exemple les opérateurs modaux, d'autres les excluront. Certains admettront seulement pour logique le premier ordre, etc. Il pourrait donc sembler que le problème de savoir si la logique peut réexprimer les mathématiques va demeurer indéterminé. Pourtant, s'il y a des notions suffisantes pour récrire les mathématiques qui sont admises largement comme étant logiques, le problème retrouve un sens, même s'il manque un vrai critère pour effectuer un tri dans beaucoup d'autres cas.

Au moins est-il généralement reconnu que les foncteurs et les quantificateurs sont des termes logiques, puisqu'ils ont un rôle stratégique dans l'identification d'une inférence.

Toutefois, si on voulait utiliser la logique naïve pour reformuler les mathématiques, l'entreprise serait perdue d'avance. Au moins faut-il une logique très affinée. Or, on brouille inévitablement le problème soulevé par le logicisme en étant contraint d'employer des concepts qui étaient logiques mais qui doivent désormais «appartenir à la logique», comme 'cosinus' appartient à la trigonométrie. Il n'est pas possible de dire que la distinction se maintient entre logique et trigonométrie quelle que soit la formalisation pour l'une et l'autre: qu'une notion formelle recouvre exactement ce qui constituait la notion informelle d'appel n'a pas de sens. Il n'est pas possible non plus de dire qu'un traitement très affiné d'un terme qui fut logique le fait passer de son lieu naturel vers les mathématiques, ne traite plus que d'un homonyme, que le traitement plurivalent de la négation par exemple n'est plus qu'une algèbre, car il va être difficile de dire à partir de quel niveau de finesse les notions logiques deviennent des transfuges.

Si par reprise logique des mathématiques on veut dire par des concepts restés naïvement logiques, on ne laisse aucune chance au logicisme. Si, inversement, on a recours à la logique d'aujourd'hui, par exemple pour valider la récurrence, et à supposer qu'on y parvienne, le logicisme risque fort de tomber sous l'objection suivante telle que la formule Dubarle: le calcul des propositions lui-même n'est qu'une algèbre à structure d'anneau et il ne devient une logique que par l'interprétation. «La mathématique [...] fait de l'une de ses théories particulières une canonique à portée générale de sa propre activité pensante» [2].

Bref, réduire les mathématiques à la logique ancienne ou commune est une cause perdue, et réduire les mathématiques à la logique mathématique est un faux-semblant.

Pourtant, peut-on dire que la structure d'anneau ne dépend en rien de la logique? En ce qu'elle doit employer, notamment, les quantificateurs, elle poursuit évidemment le «tout B est A», «A appartient à tout B» des traités logiques d'Aristote. On ne peut pas parler, cependant, d'importation, mais d'interaction, car il est bien connu que les quantificateurs modernes ont bénéficié de leur analogie avec les notions arithmétiques de somme et de produit, comme cela est resté longtemps apparent dans leurs symboles Σ, Π. Les disciplines s'hybrident, et utilement, au cours de leur histoire.

Notre investigation partira d'une situation bien connue: les *foncteurs propositionnels*, les *quantificateurs* et l'*appartenance* peuvent «remplacer, comme dit Quine, toutes les notions de l'arithmétique, de l'algèbre, du calcul différentiel et intégral, ainsi que des branches des mathématiques dérivées» [8]. Nous ne chercherons pas comment cette construction est effectuée, mais seulement s'il s'agit de notions logiques. Les procédures de définition y sont directement en cause, mais il n'est pas si facile de voir ce qu'on peut attendre de manière générale d'une définition.

Peut-on considérer que les définitions sont inutiles ou impossibles, comme l'ont dit les Sceptiques, inutiles si on connaît le défini puisqu'il est disponible et impossibles si on l'ignore puisqu'on ne peut pas alors lui donner un définissant adéquat [10]? On peut répondre que celui qui donne la définition doit évidemment connaître le défini et le définissant, mais que c'est à un tiers que la définition est utile, et pour lui enseigner comment comprendre un terme, qui va être le défini, et qu'il pourra rencontrer par ailleurs, alors qu'il ne connaît que les termes du définissant. «Les définitions sont des règles pour la traduction d'une langue dans une autre», dit Wittgenstein [11]. Certes, les Sceptiques objectent aussi à l'usage didactique des définitions: on peut apprendre sans définition, disent-ils, puisque le premier individu qui a eu accès à un terme l'a compris sans définition, ce qui est vrai mais ne retire pas à la définition la rapidité d'accès et la conceptualisation qu'elle est seule à donner. Ils objectent d'autre part que s'il faut remplacer le défini par son définissant pour le comprendre, la phrase devient au contraire inintelligible. On répondra qu'on n'a pas à remplacer, par exemple, tous les '$\supset$' d'un énoncé par un '$\mid$', selon la définition de '$\supset$' en système '$\mid$', $(p \supset q) =_{df} (p \mid (q \mid q))$, et que '$\supset$' est disponible dès la première traduction qu'on en donne puisqu'elle le fait comprendre une fois pour toutes. Pourrait-on même dire (ce que ne font pas les Sceptiques) que les termes d'un définissant peuvent être compris sans que le définissant, s'il est complexe, le soit? Non, car on est censé comprendre toutes les formules qui sont composées d'éléments individuellement compris. Dans le cas contraire, en effet, il en irait de même pour une formule complexe n'employant que des termes directement compris: $((p \supset (q \supset m)) \supset q) \supset n$ ne serait pas non plus intelligible, malgré l'accès supposé direct à '$\supset$'. Le problème ici porte sur la longueur et la complexité d'une expression et non sur la composition d'un définissant en primitifs, et ce problème est résolu partout de la même façon, à savoir en comprenant souvent pas à pas les sous-formules plutôt que la formule globale. Peano déclare lui-même, pour un autre langage, que ses trois «concetti primitivi» (0, nombre, successeur de) sont «posés comme connus» («posti come noti») [7].

Ainsi analysées, les définitions servent à faire intervenir les termes qui ont le moins besoin d'être définis pour être employés et donc constituent les meilleurs primitifs, dans une optique donnée, parce que cette optique leur accorde la plus grande intelligibilité.

Or, Bolzano, notamment, refuse l'idée que les primitifs seraient choisis pour leur intelligibilité (ou leur plus grande intelligibilité), exactement comme il refuse l'idée que les axiomes seraient choisis pour leur clarté intrinsèque. Les primitifs sont seulement des éléments qui ne sont pas définis dans le système, comme les axiomes sont seulement des vérités non déduites dans le système. Assurément, un ensemble de primitifs ne peut pas être choisi parce qu'il correspondrait à la manière dont tout le monde pense – et même si c'est en pensant le vrai – car ce serait là un psychologisme, mais chacun des ensembles complet de primitifs correspond pourtant à un type possible d'intelligibilité (un système '|' est censé compris par un certain esprit à existence théorique). Il n'y a rien à reprocher à une logique qui déploie toutes les manières possibles dans un cas donné, au moins *ceteris paribus*, d'accéder à la vérité.

L'analogie avec les axiomes est d'ailleurs à poursuivre puisque Bolzano a contesté aussi l'idée que le choix des axiomes ne serait déterminé que par leur fécondité déductive. Pour lui, la partition des axiomes et des théorèmes devrait correspondre à la manière dont le domaine des propositions vraies se structure (si difficile soit-il de la concevoir). L'ensemble des objets d'un domaine peut donc aussi se structurer mieux par tels primitifs que par tels autres, si on ajoute qu'un jugement humain se trouve toujours en jeu dans l'appréciation d'une structuration dite objective. On ne voit pas, en effet, comment expliquer quelque chose pourrait différer de *s'expliquer* cette chose, se trouver satisfait par un traitement, même s'il faut reculer souvent ce qui serait jugé éclairant à trop courte vue. L'objectivité atteinte par une analyse est toujours relative à l'objectif de l'analyste, à une certaine marge humaine de manoeuvre. Quine a insisté avec raison sur la sous-détermination des théories par les données objectives.

Si on devait renoncer en logique à la catégorie (psychologique, si l'on veut) de l'intelligible, on devrait renoncer aussi à tout logicisme puisque celui-ci accorde un primat à un certain type de notions, dites «logiques».

Il n'en demeure pas moins que le choix des primitifs ne répond pas seulement à l'intelligibilité immédiate qu'on leur attribue. Church [1] souligne l'intérêt de disposer par exemple d'un système autodual de primitifs indépendants, mais aussi de renoncer à telles ou telles propriétés, et justement selon ce qu'on veut rendre intelligible encore à d'autres égards. Un primitif unique et suffisant comme '|' (jugé souvent peu naturel) applique au moins d'une manière drastique le principe intelligible de parcimonie, et même s'il y a des conflits de normes dans la vie mathématique comme il y en a dans la vie morale.

Ajoutons qu'on ne peut obtenir, sans doute, que des systèmes sans *liste* des primitifs (employés dans les formules), et non des système sans primitifs, au sens où il y a des systèmes sans axiomes. On a au plus des systèmes sans primitifs redondants (par une méthode venue de Padoa).

Il arrive qu'on définisse le logicisme, comme Lalande, par le rejet de toute «intervention de la psychologie», ce qui trouve évidemment un écho dans les textes où Frege condamne l'analyse du nombre comme image mentale. Cependant, sans la catégorie de l'intelligible, le recours à des termes logiques pour élucider les termes mathématiques reste arbitraire. Chacun des ensembles complets de primitifs est convoqué tacitement à titre d'un esprit fictif qui comprendrait mieux cet ensemble de primitifs que tel autre (ou ne comprendrait que lui), et donc au nom d'un psychologisme généralisé et ainsi désamorcé. Une partie des vues contemporaines sur le logicisme peut dépendre de la manière dont on a su récemment réévaluer ce qui a été nommé «psychologisme». L'idée de base du logicisme, dès lors, est que les notions logiques forment le meilleur fondement, car le plus intelligible (par exemple que l'on comprend mieux «nier une proposition» que «dériver une fonction»). En ce sens, il constitue une certaine hypothèse sur des données psychologiques associables et non seulement une thèse sur les mathématiques. Sans un minimum de renvoi vraisemblable ou supposé au caractère intelligible ou plus intelligible d'une certaine construction, il n'y a pas de logicisme possible.

Or, (en réservant pour le moment le cas de l'appartenance), bien qu'on ait admis le caractère logique des foncteurs et des quantificateurs, on peut contester qu'ils soient les termes logiques les plus basaux. Nous sommes conduits ainsi à la partie la plus ambitieuse du programme de la logique combinatoire: remplacer toutes les constantes (implication, addition, cosinus, etc.) par des combinateurs, c'est-à-dire les constantes propres à sa théorie. Ce programme a connu une réussite: il existe déjà plusieurs versions combinatoires de l'arithmétique dans lesquelles les nombres et les opérations sont des combinateurs.

Notons d'abord que les combinateurs expriment des opérations logiques (ou prélogiques) très élémentaires. On peut le voir sur l'ensemble standard des cinq combinateurs primitifs **I**, **K** **W**, **C**, **B**, caractérisés par des règles (transitives) dites *de réduction* qui indiquent l'effet de chacun sur une certaine suite (dite *suite initiale*):

$$\mathbf{I}x_1 \rightarrow x_1$$
$$\mathbf{K}x_1x_2 \rightarrow x_1$$
$$\mathbf{W}x_1x_2 \rightarrow x_1x_2x_2$$
$$\mathbf{C}x_1x_2x_3 \rightarrow x_1x_3x_2$$
$$\mathbf{B}x_1x_2x_3 \rightarrow x_1(x_2x_3)$$

Les métavariables x_1, x_2, x_3 désignent n'importe quel élément différent de '→' (on dira *objet*) placé dans l'alphabet du langage (une variable, une constante quelconque, logique ou non, et même un combinateur), ou une molécule acceptée selon la règle de formation: si x_1, x_2 sont des objets, (x_1x_2) est un objet. $x_1 \rightarrow x_2$ peut se lire «il suffit d'avoir x_1 pour avoir x_2» (x_2 est dit *résultante*). Les parenthèses groupent deux ou *n* objets pour en former un seul.

Les transformations effectuées sont simples: **K** élimine le second de deux objets, **W** le répète, **C** permute les deux derniers de trois objets, **B** les regroupe en un seul. **I** reproduit un objet en fac-similé.

Les combinateurs expriment des opérations très précoces, mises en oeuvre dans les jeux d'enfant, notamment, et ne présupposant même pas le langage: éliminer un caillou d'une rangée, y permuter deux cailloux, grouper deux cailloux en les espaçant par exemple des autres cailloux, répéter le bruit fait par un caillou donné qu'on frappe sur une boîte, etc.

La raison principale du privilège accordé aux combinateurs vient de ce que beaucoup des constantes usuelles (comme l'implication) n'ont pas la teneur directement intelligible des combinateurs.

Plus généralement, un combinateur (propre) **X** est une constante telle que

$$\mathbf{X}x_1x_2 \ldots x_n \rightarrow Q$$

où Q ne diffère de $x_1x_2 \ldots x_n$ que par l'ordre, le nombre d'occurrences ou le groupement (par des parenthèses) de ses éléments, ou reproduit $x_1x_2 \ldots x_n$ à l'identique. Combiner, en effet, c'est modifier sans ajouter de nouveaux objets (même l'objet devenu unique (x_2x_3) reste bien distinct d'un objet seul nouveau qui serait x_4). On admet:

- si **X**, **Y** sont des combinateurs (**XY**) est un combinateur.
- une expression comme $\mathbf{K}x_1x_2x_3$ (où x_3 est surnuméraire) se réduit à x_1x_3.
- une expression comme $x_1(\mathbf{C}x_2x_3x_4)$ se réduit à $x_1(x_2x_4x_3)$, mais $x_1\mathbf{C}x_2x_3x_4$ ne se réduit pas.

L'objectif le plus apparent de la logique combinatoire est d'éliminer les variables liées dans tous les formalismes. En effet, les propriétés remarquables (comme la commutativité) concernent les constantes et non les variables, même si on emploie des variables pour les énoncer. La possibilité repose sur la proposition suivante (dont la preuve est beaucoup trop longue pour être donnée ici):

P_1: a) Pour toute formule F d'un langage formel, il existe un combinateur **X** du système standard tel que

$$\mathbf{X}c_1c_2 \ldots c_nv_1v_2 \ldots v_m \rightarrow \mathrm{F}$$

où $c_1, c_2, \ldots, c_n, v_1, v_2, \ldots, v_m$ sont, en une seule occurrence, respectivement les constantes (dans un ordre quelconque) et les variables de F, et tel que **X** exige exactement un nombre $n + m$ d'éléments pour s'effectuer;

b) $\mathbf{X}c_1c_2 \ldots c_n$ exprime alors F sans variable.

Soit à exprimer D*pp* sans variable (où D est '|' en notation préfixée). Il vient selon a) **WD***p* → D*pp*, puis de là **WD**, selon b).

On doit distinguer une expression comme $\mathbf{K}x_1$ qui, telle qu'elle est formulée, ne se réduit pas, et cette expression pourvue de x_2 qui s'effectue mais qui paraît complétée par un sujet humain. En réalité, celui-ci n'est que l'exécutant d'une mesure mathématique. Après **K**, x_1x_2 sont déjà présents, comme une parallèle est présente dans un cas où on dit la mener. C'est pourquoi:

> P_2: Un combinateur **X** qui exige exactement n objets pour qu'on puisse l'effectuer introduit les n objets qu'il lui faut, ou pour une expression $\mathbf{X}x_1x_2 \ldots x_k$ $n - k$ objets.

Soit $\mathbf{X}^n$ le combinateur **X** qui appliqué à une suite (suffisante) obtient une résultante à laquelle on applique à nouveau **X**, et cela n fois. On établit notamment que $\mathbf{B}^2\mathbf{I}$ exige (et donc introduit) $x_1x_2x_3$, et donc $\mathbf{B}^2\mathbf{I}$D exige et introduit x_1x_2 (qui, étant des objets quelconques, peuvent être choisis comme p et q). $\mathbf{B}^2\mathbf{I}$D exprime Dpq (sa résultante) sans variables. Bien sûr, on sous-entend les variables, mais parce qu'on a réussi à mettre les expressions dans une forme où cela est devenu possible.

Notons qu'on peut éliminer toutes les variables qui sont en fin de suite initiale ou seulement la dernière ou les deux dernières, etc. Dpq exprimé sans q mais avec p serait $\mathbf{B}^2\mathbf{I}(\mathbf{B}^2\mathbf{I})Dp$, qui exige q et obtient Dpq.

La logique combinatoire, plutôt que de supprimer (au sens strict) les variables, veut éviter qu'elles aient le poids des constantes. Elle exige donc que les variables soient assignées dans leur nombre et leur place par des constantes dont ce soit le rôle, à savoir les combinateurs.

Montrons qu'on peut exprimer les foncteurs propositionnels en combinateurs. Il suffit de transcrire l'implication (notée ici de manière préfixée par P) et la négation (notée N) puisque P, N suffisent à définir tous les autres foncteurs[1].

[1] Nous avons déjà donné le résultat sur les foncteurs dans [5]. Un prolongement sur les quantificateurs sera présenté ici pour la première fois.

Or, les valeurs de vérité, 1, 0, sont souvent utilisées pour sélectionner l'un des termes, x_1 ou x_2 d'une alternative, notamment dans les langages de programmation, par une consigne comme

si p est vrai faire x_1,

si p est faux faire x_2.

On a x_1x_2 et le vrai sélectionne x_1, le faux sélectionne x_2, de là:

$\mathbf{1}x_1x_2 \rightarrow x_1$,

$\mathbf{0}x_1x_2 \rightarrow x_2$.

On est très loin d'exprimer ce que disent «vrai» et «faux», mais il suffit ici de s'adresser aux seules propriétés qui nous sont utiles.

Considérons le combinateur **H** tel que $\mathbf{H}x_1x_2x_3 \rightarrow x_2x_3x_1$ et les combinateurs **1**, **0**. P sera **H1** puisqu'on a

$\mathbf{H1}pq \rightarrow pq\mathbf{1}$ et donc

pour $v(p) = 1$, $v(q) = 1$: $\mathbf{H111} \rightarrow \mathbf{111} \rightarrow \mathbf{1}$

pour $v(p) = 1$, $v(q) = 0$: $\mathbf{H110} \rightarrow \mathbf{101} \rightarrow \mathbf{0}$

pour $v(p) = 0$, $v(q) = 1$: $\mathbf{H101} \rightarrow \mathbf{011} \rightarrow \mathbf{1}$

pour $v(p) = 0$, $v(q) = 0$: $\mathbf{H100} \rightarrow \mathbf{001} \rightarrow \mathbf{1}$

D'autre part, on peut définir la négation. Comme N$p =_{df}$ Pp0, il vient: $\mathbf{H1}p\mathbf{0}$. En effet:

$\mathbf{H1}p\mathbf{0} \rightarrow p\mathbf{01}$

pour $v(p) = 1$: $\mathbf{H110} \rightarrow \mathbf{101} \rightarrow \mathbf{0}$

pour $v(p) = 0$: $\mathbf{H100} \rightarrow \mathbf{001} \rightarrow \mathbf{1}$

Tous les foncteurs sont donc exprimables par les trois combinateurs **H**, **1**, **0**. Dpq, notamment, qui est définissable comme $p \supset \sim q$, c'est-à-dire Pp(Pq0) sera $\mathbf{H1}p(\mathbf{H1}q\mathbf{0})$, si du moins on remplace chaque occurrence de P par **H1** et en gardant les variables, comme nous le ferons par commodité, alors qu'il faudrait se donner le combinateur **X** (très complexe) qui sur **H10** exigerait deux variables *pq* et tel que **XH10**pq → **H1**p(**H1**q**0**).

– Les combinateurs **H** , **1** , **0** sont respectivement en système standard **CC**, **K** et **KI**. On a en effet:
CC$x_1x_2x_3$ → **C**$x_2x_1x_3$ → $x_2x_3x_1$,
Kx_1x_2 → x_1,
KIx_1x_2 → **I**x_2 → x_2.

Montrons en esquisse qu'on peut aussi remplacer les quantificateurs par des combinateurs (ou plus exactement éviter les premiers au moyen des seconds). La méthode reprend des idées de Schönfinkel, mais s'en écarte significativement (principalement en ce qu'il n'utilisait pas exclusivement des combinateurs) [9].

– Seuls ∀et | (ou D) seront considérés, puisqu'ils forment un système symboliquement complet (en logique classique).

– Dans une formule donnée, on utilise des symboles différents pour les occurrences libres et les occurrences liées des variables.

– Récrivons avec Schönfinkel l'expression ∀x (fx | gx), qui va servir de modèle, comme fx $|^x$ gx (elle signifie: pour tous les objets notés x qui sont en cause dans l'incompatibilité). Notons T cette transformation.

P_3: Si on obtient une expression de forme '... $|^x$...' (au nom près de x) telle que les '...' ne comportent pas x, le x indicé devient vide et donc s'élimine. Notons V cette transformation.

Une ligne introduisant un combinateur doit reformuler une expression avec seulement en moins la variable qu'on va supprimer (s'il y a lieu) et que le combinateur doit supposer.

Exemple 1

1. $\forall x\,(fx \mid fx)$	
2. $fx \mid^x fx$	T
3. $\mathbf{BI}f \mid^x \mathbf{BI}f$	P_1
4. $\mathbf{BI}f \mid \mathbf{BI}f$	V
5. $D(\mathbf{BI}f)(\mathbf{BI}f)$	en D
6. $\mathbf{H1}(\mathbf{BI}f)(\mathbf{H1}(\mathbf{BI}f)\mathbf{0})$	selon **D**

En 3., fx devient $\mathbf{BI}f$ car $\mathbf{BI}f$ exige x et $\mathbf{BI}fx \rightarrow \mathbf{I}(fx) \rightarrow fx$. En 6., D est traduit en combinateur **D** en remplaçant les p, q de la formule $\mathbf{H1}p(\mathbf{H1}q\mathbf{0})$ par le premier argument de D et par le deuxième respectivement.

Exemple 2

1. $\forall x \forall y\,(rxy \mid syx)$	
2. $rxy \mid^y syx$	T pour $\forall y$
3. $\mathbf{B}^2\mathbf{I}rx \mid^y \mathbf{C}sx$	P_1
4. $\mathbf{B}^2\mathbf{I}rx \mid \mathbf{C}sx$	V
5. $\forall x\,(\mathbf{B}^2\mathbf{I}rx \mid \mathbf{C}sx)$	intervention de $\forall x$ resté en tête
6. $\mathbf{B}^2\mathbf{I}rx \mid^x \mathbf{C}sx$	T
7. $\mathbf{B}^2\mathbf{I}(\mathbf{B}^2\mathbf{I})r \mid^x \mathbf{B}^2\mathbf{IC}s$	P_1
8. $\mathbf{B}^2\mathbf{I}(\mathbf{B}^2\mathbf{I})r \mid \mathbf{B}^2\mathbf{IC}s$	V
9. $D(\mathbf{B}^2\mathbf{I}(\mathbf{B}^2\mathbf{I})r)(\mathbf{B}^2\mathbf{IC}s)$	en D
10. $\mathbf{H1}(\mathbf{B}^2\mathbf{I}(\mathbf{B}^2\mathbf{I})r)(\mathbf{H1}(\mathbf{B}^2\mathbf{IC}s)\mathbf{0})$	selon **D**

En 3., $\mathbf{B}^2\mathbf{I}rx$ exprime rxy car $\mathbf{B}^2\mathbf{I}rx$ exige y et $\mathbf{B}^2\mathbf{I}rxy \rightarrow rxy$. $\mathbf{C}sx$ exprime syx car $\mathbf{C}sx$ exige y et $\mathbf{C}sxy \rightarrow syx$. Semblablement pour 7. et 6.

Exemple 3 $\forall x\,(fx \mid (fx \mid fx))$

On a $fx \mid^{x} (fx \mid fx)$, mais non $fx \mid^{x} (fx \mid^{x} fx)$ car '$\forall$' n'est pas distribuable; de là, en reprenant une idée de Schönfinkel:

1. $\forall x\,(fx \mid \forall y\,(fx \mid fx))$ — introd. d'un quantificateur vide
2. $fx \mid^{x} (fx \mid^{y} fx)$ — T
3. $fx \mid^{x} (\mathbf{BK}fxy \mid^{y} \mathbf{BK}fxy)$ — exprimé avec y. $\mathbf{BK}fxy \rightarrow \mathbf{K}(fx)y \rightarrow fx$
4. $fx \mid^{x} (\mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx \mid^{y} \mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx)$ — P_1 à droite
5. $fx \mid^{x} (\mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx \mid \mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx)$ — V
6. $fx \mid^{x} (\mathrm{D}(\mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx)(\mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx))$ — en D à droite
7. $(\mathbf{BI}f) \mid^{x} (\mathbf{X}\mathrm{D}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})f)$ — P_1 sur les deux membres

   X est le combinateur complexe (que nous ne donnerons pas)
   tel que $\mathrm{XD}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})f$ exige x
   et tel que $\mathrm{XD}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})fx \rightarrow \mathrm{D}(\mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx)(\mathbf{B}^{3}\mathbf{I}(\mathbf{BK})fx)$.

8. $(\mathbf{BI}f) \mid (\mathbf{X}\mathrm{D}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})f)$ — V
9. $\mathrm{D}(\mathbf{BI}f)(\mathrm{XD}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})f)$ — en D
10. $\mathbf{D}(\mathbf{BI}f)(\mathbf{XD}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})f)$ — en **D**
11. $\mathbf{H1}(\mathbf{BI}f)(\mathbf{H1}(\mathbf{XD}(\mathbf{B}^{3}\mathbf{I})(\mathbf{BK})f)\mathbf{0})$ — selon **D** à gauche

Notons qu'un combinateur peut être en situation d'opérateur (comme dans 10. le premier **D**) ou en situation d'opérande (comme le deuxième **D** sur lequel porte **X**).

Exemple 4 $\forall x\, fx$

On procède ainsi, selon une idée de Schönfinkel: $\forall x\,(fx \vee fx)$ par idempotence, puis $\forall x\,((fx \mid fx) \mid (fx \mid fx))$ par la définition de '$\vee$' en système '$\mid$', puis $(fx \mid^{y} fx) \mid^{x} (fx \mid^{y} fx)$, et on est ramené à une expression qu'on sait traiter.

Reste la question du statut de l'*appartenance*. Il s'agit de savoir, en somme, à qui appartient l'appartenance, ce qui met du piquant à la problématique générale: y a-t-il des notions primitives qui appartiennent au logicien et qui suffisent à récrire les mathématiques avec bénéfice? Y a-t-il des notions primitives qui appartiennent au mathématicien et qui suffisent à écrire les mathématiques sans médiation? Bloqué dans ces formulations, le débat reflète les normes économiques de sociétés marquées par le régime de la propriété privée. On a essayé de montrer que la recherche de l'intelligibilité l'emporte sur la défense des territoires.

Par sa présence dès les travaux logiques d'Aristote, où la notion d'appartenance est rapportée à la forme des syllogismes, cette notion a l'ancienneté et la généralité des notions logiques. Certes, «A appartient à B», chez Aristote n'est pas notre '∈', mais 'ὑπάρχει' est son ancêtre. Ce qui relève de la logique n'a pas à être immuable. Quine objecte que le logicisme considère '∈' comme une notion logique et donc «la théorie des ensembles comme de la logique», mais sans renforcer les mathématiques «car la théorie des ensembles est moins bien assise» que les mathématiques générales [8]. Bien sûr, admettre sans restriction $x \in F =_{df} fx$, où F est l'ensemble des individus qui satisfont f, conduit à des antinomies, et la rigueur logique ne doit pas en comporter. Mais Quine reconnaît qu'il y a par ailleurs un usage innocent de l'appartenance et que, pour les autres, il y a différents moyens d'éviter les antinomies (axiome de séparation, etc.). Or, même si le meilleur traitement reste en discussion (réduire au minimum les exceptions et les rendre nécessaires), on ne voit pas pourquoi un concept échapperait à la logique, dès qu'il reste à élucider. Que la théorie des ensembles emploie l'appartenance n'entraîne pas qu'elle soit une notion ensembliste ni que la logique doive inclure la théorie des ensembles, mais seulement qu'il y a un usage ensembliste de l'appartenance, et en effet risqué. Déclarer «logique» l'appartenance, ce que nous ferons, c'est surtout l'estimer plus intelligible que beaucoup des notions mathématiques qu'elle va contribuer à définir.

Ce qui vient d'être décrit comme un logicisme combinatoire a notamment pour intérêt de constituer une provocation à l'égard des courants les plus opposés à l'intervention de la logique en mathématiques, puisque supprimer les variables, retranscrire les constantes en combinateurs peut passer pour non productif et pour un programme d'esthète. À ce titre au moins, les positions peuvent être clairement repérées, grâce à la surenchère combinatoire.

Pourtant, que le logicisme soit stérile ou fécond en mathématiques est loin de constituer le tout du problème. L'intérêt des mathématiques n'est pas limité aux services qu'elles rendent dans leurs applications, mais l'intérêt de la logique en mathématiques n'est pas limité non plus aux services qu'elle devrait y rendre. Par une reprise logiciste – combinatoire ou non, complète ou partielle – on peut apprendre sur l'intelligible et le rationnel, plus que sur les nombres et les figures, mais d'une manière sans doute irremplaçable.

BIBLIOGRAPHIE

[1] CHURCH A. (1956). *Introduction to mathematical logic*. Princeton University Press (*cf.* § 24).

[2] DUBARLE D. (1967). «Critique du réductionnisme», *Logique et connaissance scientifique*, dir. J. Piaget, Paris: Gallimard, La Pléiade (*cf.* p. 339-341).

[3] GARDIES J.-L. (2004). *Du mode d'existence des objets de la mathématique*. Paris: Vrin (*cf.* p. 121 *sq*).

[4] GINISTI J.-P. (1997). *La logique combinatoire*. Paris: PUF, coll. Que sais-je? n°3205.

[5] GINISTI J.-P. (2003). «La logique combinatoire: logique de l'objet quelconque ou logique de l'opérateur?», *Mathématiques et Sciences Humaines* 162 (*cf.* p. 67-71).

[6] KORSELT A. (1911). «Über einen Beweis des Äquivalenzsatzes», *Mathematischen Annalen* 70. 294-296.

[7] PEANO G. (1902). *Aritmetica generale e algebra elementare.* Torino: Paravia (*cf.* p. 8).

[8] QUINE W. V. O. (1972). *Logique élémentaire*, (trad). Paris: A. Colin (*cf.* p. 191-193).

[9] SCHÖNFINKEL M. (1924). «Sur les éléments de construction de la logique mathématique», trad. G. Vandevelde, analyses et notes par J.-P. Ginisti, *Mathématiques, Informatique et Sciences Humaines* 112 (1990).
[10] SEXTUS EMPIRICUS. *Esquisses pyrrhoniennes*, éd. P. Pellegrin. Paris: Seuil, 1997 (*cf.* Livre II, 16).
[11] WITTGENSTEIN L. (1921). *Tractatus logico-philosophicus* (*cf.* 3.343).

Kit Fine on *The Limits of Abstraction*

Bob Hale

Kit Fine's book is a study of abstraction in a quite precise sense which derives from Frege. In his *Grundlagen*[1], Frege contemplates defining the concept of *number* by means of what has come to be called Hume's Principle – the principle that the number of *F*s is the same as the number of *G*s just in case there is a one-to-one correspondence between the *F*s and the *G*s. Frege's discussion is largely conducted in terms of another, similar but in some respects simpler, proposal – to define the concept of *direction* by laying down the principle (the Direction Equivalence) that the direction of a line *a* is the same as that of line *b* if and only if lines *a* and *b* are parallel. Such principles have come to be known as abstraction principles. More generally, abstraction principles are ones of the shape:

$$\forall\alpha\forall\beta\ (\S(\alpha) = \S(\beta) \leftrightarrow \alpha \approx \beta)$$

where $\approx$ is an equivalence relation on entities of the type over which α and β vary and, if the principle is acceptable, § is a function from entities of that type to objects. Since 'the direction of' is intended to stand for a function from *objects* of a certain sort (viz. straight lines) to other objects (their directions), the Direction Equivalence is a first-order, or in Fine's terminology *objectual*, abstraction; since 'the number of' is intended to stand for a function from *concepts* (or properties) to objects, Hume's Principle is a higher, or as Fine says *conceptual*, abstraction. As is well-known, Frege himself abandoned the idea of defining number implicitly or contextually by means of

[1] Frege (1884). The relevant sections are §§ 62-67.

Hume's Principle – adopting instead an explicit definition of the number of *F*s as the extension of the concept *concept equinumerous to the concept F* – because he thought that an adequate definition should settle the question whether Julius Caesar, for example, is or is not the number of some concept, but that the proposed implicit definition cannot do so. But interest in abstraction principles has revived in the last couple of decades, largely as a result of Crispin Wright's attempt[2] to show that in spite of the many difficulties confronting it – including the notorious Julius Caesar problem – Hume's Principle can after all serve as a foundation for arithmetic.

Although Kit Fine allows that a theory of abstraction can be developed in which some fundamental mathematical theories may be reconstructed, the overriding message of his book, as its title suggests, is largely negative. In particular, he is quite out of sympathy with the idea that abstraction principles (e.g. Hume's Principle) might play any significant rôle in providing philosophical foundations for mathematics – even for those parts of it which can be obtained within the formal theory of abstraction developed in parts III and IV of his book. I suspect that, given the assumptions underlying and shaping Fine's approach to the subject, this negative assessment is more or less inevitable. Here, I want to explore what I believe to be some of the more important of those assumptions, and suggest some reasons why we might not find them irresistable.

We can begin with a point of agreement: if abstraction principles are to play a foundational rôle, it must be possible to view suitable such principles as serving somehow to *explain* or *define* fundamental mathematical concepts, such as cardinal number and real number.

What, at bottom, we disagree about is whether they can be so viewed. Fine believes not, and most of parts I and II of *Limits* is devoted to arguing his case. Since he denies that abstraction principles can serve in a definitional capacity, he does not ask: Which abstraction principles are *acceptable as definitions*? Instead, his

[2] Wright (1983). The possibility of basing arithmetic on Hume's Principle was first noted, I believe, by Charles Parsons (1965). A good deal of the subsequent work in the neo-Fregean programme Wright initiated is collected in Hale & Wright (2001).

questions are: Under what conditions is an abstraction principle *true*? Which are the true abstraction principles?

I believe that Fine's negative assessment of the prospects for using abstraction principles in a definitional, and thence foundational, rôle has its source, ultimately, in his adoption of what could be called an *externalist*[3] standpoint with regard to abstraction. Centrally, this involves thinking that what objects there are is largely independent of abstraction. The acceptability of abstraction principles is then a matter of their being true of a certain domain – the universe of all objects whatever – of (largely) independently constituted composition and cardinality. Under a suitable assumption about the size of this all-inclusive domain of objects – that the cardinality of the domain is unsurpassable[4] – the acceptable abstraction principles can be characterised as those which are non-inflationary[5] and predominantly logical[6]. But abstraction principles themselves cannot be used to give us any assurance of the truth of such an assumption, which must be justified independently, if it can be justified at all.

Clearly if Fine's claim that abstraction principles can play no useful definitional rôle is correct, then such principles can enjoy no philosophical advantage and can bring no epistemological or ontological gains. Since that runs directly counter to what neo-Fregeans have claimed for them, I will concentrate on Fine's critique of abstraction principles as definitions. After I have explained where and why I differ from him on this issue, I will go on to venture some more

[3] Fine uses the term in a somewhat different sense – in which a characterization of a position (e.g. that of the uncompromising abstractionist) is externalist if it can only be regarded as correct, or even as intelligible, by one who does not hold the position. Note that rejecting externalism in my sense does not require adoption of uncompromising abstractionism – one can allow that there are abstract objects which cannot be seen as introduced by abstraction, so long as there aren't too many of them – so many that those which can be so viewed constitute only an insignificant proportion.

[4] A cardinal **c** is unsurpassable if $2^{\mathbf{d}} \leq \mathbf{c}$, where **d** is the number of cardinals less than **c**. See Fine p. 7.

[5] That is, where $\approx$ is the abstraction's equivalence relation, the number of $\approx$-equivalence classes must not exceed the number of objects. See Fine p. 4.

[6] That is, the identity criterion given by the abstraction's equivalence relation 'involves only an 'exponentially small' number of objects in relation to the number of objects in the universe as a whole' See Fine p. 7, where the notion of exponential smallness is further explained.

speculative and tentative thoughts about the influence of the externalist perspective on his thinking about abstraction.

Fine contrasts what he calls ‘orthodox’ or ‘classical’ definitions with ‘unorthodox’ or ‘non-classical’ ones. I am not sure how he means to draw this distinction in general, but in the case of definitions which he takes to be intended to result in an assignment of objects to the defined terms, the idea seems to be that orthodox definitions presuppose the existence of the objects to be assigned as referents, whereas unorthodox definitions do not.

Prominent among the *unorthodox* kinds of definition Fine discusses are what he terms ‘definition by reconceptualization’ and contextual or implicit definitions of the sort he calls ‘creative’. Crispin Wright and I hold that Hume’s Principle can be used to implicitly define the number operator, and claim that the idea of reconceptualization can play a key rôle in explaining how we may acquire warranted beliefs about abstract objects. Since we could scarcely maintain the latter claim, if we agreed that the use of Hume’s Principle as a definition presupposes the existence of the objects which are to be the values of the number operation, we cannot view the proposed definition as orthodox (at least, not in Fine’s sense). So it may seem that our disagreement with Fine must largely concern whether it can function as a definition by reconceptualization or as a creative implicit definition. As we shall soon see, matters are not quite this straightforward. But it is certainly true that our main[7] points of disagreement with him concern what we may as well agree to call unorthodox definition, and I shall concentrate on these.

[7] I have some disagreements with Fine about definition in general, and in particular with his claim that definitions always presuppose a domain of discourse. Although they are, in my view, important, I don’t have space to go into them here.

Reconceptualization and implicit definition

(i) Reconceptualization

Fine discusses two 'non-classical' ('unorthodox') approaches to defining number by means of Hume's Principle. In the first of them, the leading idea is that the definition works by reconceptualization; in the second, that it involves an application of the context principle. Although both ideas derive from Frege, Fine is not concerned with faithful interpretation of Frege's thought – as he says, his interest is in 'whether the ideas themselves can be sustained'. His own view is that they cannot. While I do, and shall, disagree with him about that, I share the same primary focus of interest – in the ideas rather than Fregean exegesis. So I am especially interested in whether this broad agreement between us extends to agreement over what 'the ideas themselves' *are* which may or may not be sustainable. I regret that it seems to me not extend very far at all, with the unfortunate result that Fine largely fails to engage with the ideas in what I believe to be their best shape.

Fine treats reconceptualization and contextual definition as two alternative and entirely separate approaches. He does so because he takes 'definition by reconceptualization' (his term, not mine) to rest 'on the idea that new senses may emerge from the reanalysis of a given *sense'*, whereas 'the context principle ... rests upon the idea that certain truths may be used to fix the *reference* of the terms that they contain. Thus whereas the first-mentioned approach rests on the adoption of an unorthodox mechanism for the *determination of sense*, the second rests on an unorthodox mechanism for the *determination of reference*' (p. 35). Definition of the number operator by reconceptualization is supposed to proceed in two stages – 'first, the formula on the left is taken to have the same sense as the formula on the right; second, this sense is subject to reanalysis in such a way as to provide a sense for the number operator' (p. 37). The formulae on left and right should be taken, Fine holds, to be open sentences – thus in the case of Hume's Principle, they will be 'the number of Fs = the number of Gs' and 'the Fs are equinumerous with the Gs', where F and G are concept variables. The process, he suggests, can be seen as combining the methods of explicit and implicit definition. I think he means that the left hand formula is *explicitly* defined to have *the same sense* as the right hand formula, and the number operator is then *implicitly* defined as having the sense it must have, if the left and right hand formulae are to be synonymous. What he says is that 'The idea behind reconceptualization is to get the implicitly defined sense, not directly from the principle, but indirectly from the explicitly defined sense' (p. 38). It is anyway clear enough that, on the view he is discussing, reconceptualization involves treating the left and right hand formulae having the same sense and taking this to result, somehow, in a unique sense for the operator embedded in the left hand formula.

Fine does not deny that definition by reconceptualization – as *he* understands it – can ever succeed in determining a unique sense. He allows that examples such as one given in Frege's *Begriffsschrift* – whereby the one-place predicate 'commits suicide' is defined from the two-place predicate 'kills' by, in effect, identifying the argument-places – might be seen as successful applications of reconceptualization. But he denies that one can accept Hume's Principle, or any other abstraction principle, as a definition by reconceptualization, on the ground that doing so would force us to claim sameness of sense in cases where it is clearly incorrect to do so. Thus in his main example, we are to consider the statements:

(A) $\forall x(Fx \leftrightarrow Gx)$

(B) $\S xFx = \S xGx$

(C) $\forall x(x{=}\S xFx \leftrightarrow x{=}\S xGx)$

(D) $\S x(x{=}\S xFx) = \S x(x{=}\S xGx)$

where § is the class abstraction operator, and we are to prescind from the inconsistency of Frege's Basic Law V. Fine argues that:

> ... (B) should have the same sense as (A) and (D) the same sense as (C). Now presumably (C) should also have the same sense as (B). Indeed, it is hard to see how a criterion of synonymy could be so tolerant as to allow the synonymy of (B) to (A) (or of (D) to (C)) and yet not tolerant enough to allow the synonymy of (C) to (B). If this is

> right, it follows that (A) has the same sense as (D). But then if the sense of the operator given through reconceptualization is unique, the sense of §*xFx* will be the same as the sense of §*x*(*x*=§*xFx*), which is presumably not so (p. 40 – with minor correction).

It is, he concludes, 'difficult to see what general principles governing identity of sense might permit its reconceptualization and yet prevent its proliferation'.

I think Fine is right to resist any attempt to assimilate definition by means of abstraction principles to the kind of example given in the *Begriffsschrift*, but my reason for so thinking is probably not one on which we agree. Frege's claim there is that, in many cases, one and the same sentence can be analysed into function and argument(s) in different ways. He emphasizes that this does not affect the 'conceptual content', which remains the same under different decompositions of the sentence into function and argument(s). In such cases, it is then very natural to think of a single complex sense, expressed by the whole sentence, as analysable into constituent senses in different ways. In particular, one can then plausibly view the concept of committing suicide as obtained by 'reanalysing' the complex sense expressed by, say, 'Cato killed Cato', by taking 'Cato' as a single argument replaceable at both occurrences. But it is essential to the plausibility of this view that we have a *single* (unambiguous) sentence analysable in different ways. This is precisely what we don't have, in the case in which Frege speaks of carving up the content in a new way – in the passage in *Grundlagen* which has inspired talk of reconceptualization. For precisely this reason, I do not think we can view Frege's discussion in *Begriffsschrift* as supplying the model for his later talk of carving up content in new way. I believe we should think of reconceptualization – not as a process of supplying a new analysis of some single given sense – but in a quite different way. I shall say more of this later. Fine, however, makes no distinction between reconceptualization and reanalysis, which he construes in a way that suits the *Begriffsschrift* example – i.e. as a process by which a single sense is subjected to different decompositions. It is then hardly surprising that he finds that abstraction principles cannot be viewed as involving reconceptualization.

I think Fine is probably right, too, to be sceptical about the possibility of framing general criteria for identity of sense which would allow us to treat the left and right sides of Hume's Principle, for example, as having the same sense, without running into the problem of proliferation. So if it were the case that using Hume's Principle (or any other abstraction) as a definition required us to hold that its left hand side re-expresses the same sense as its right – as it would if it was intended to function as a definition by reconceptualization in Fine's sense – this would constitute a serious problem for us [neo-Fregeans]. But it seems to me – now, anyway[8] – that it doesn't. Neither the claim that Hume's Principle can function as an implicit definition of the number operator, nor the claim that, when the number operator is so defined, statements of numerical identity can be taken as reconceptualizing the same states of affairs as corresponding statements about one-one correlation between concepts – neither claim need involve taking its left and right sides as having the same sense.

In contrast with Fine, I do not view ideas about reconceptualization and the context principle as belonging to two entirely distinct approaches to definition. I do not – and never did – regard reconceptualization as a *type of definition* or mechanism (unorthodox or otherwise) for determining senses. When we use an abstraction principle to fix or explain the sense of the embedded abstraction operator, what we are giving is a kind of implicit definition[9]. In general, an implicit definition proceeds by stipulating the truth of a suitable sentence incorporating the definiendum, with the intention

[8] Some of my own earlier published work on this issue (especially Hale 1997) suggests that there needs to be a relation of sameness of (weak) sense between the left and right sides of an abstraction principle. Various authors besides Fine, including Michael Potter and Timothy Smiley, and William Stirton, have pointed out serious difficulties in the way of characterizing a suitable relation of sameness of sense. In the text below I explain why I think any such requirement is misguided.

[9] Thus for us, implicit or contextual definition is primarily a means of fixing *sense*, not – as Fine supposes – a mechanism for determining *reference*. So we are in sharp disagreement both with his view of reconceptualization and with his view of contextual definition.

that it is to mean precisely what it needs to mean – given the already fixed meanings of the expressions composing the sentential context in which the definiendum is embedded – for that sentence to express a truth. It is thus an implicitly definitional stipulation of the truth of a sentence which is the 'mechanism' by which, if all goes well, the sense of the definiendum is determined. That the LHS of an abstraction principle (more precisely, instances of the LHS) may then be regarded as reconceptualizing the states of affairs describable by (corresponding instances of) its RHS is *not* the *means* whereby the sense of the abstraction operator is fixed – it is rather a *result* of the operator's being defined in this particular way.

Let me say a little by way of explanation and defence of these last two claims, starting with my claim about implicit definition. In an *explicit* definition, there clearly *is* a claim or stipulation[10] about identity of sense – what is claimed or stipulated is that the definiendum has (or is to have) the same sense as the definiens. It is, in general, obvious that implicit definition by contrast involves *no* claim or stipulation about identity of sense – the definiendum is normally a single word or phrase, whereas the definiens is a complete sentence containing it, and there is no suggestion that part of the sentence has the same sense as the whole! The idea rather is – at least on the account of the matter I favour – that by embedding the definiendum in a suitable sentence, otherwise comprising only expressions whose meanings are already fixed, we can, by stipulating that the sentence is to be true, confer a meaning on the definiendum; it is to mean just what it needs to mean – no more, no less – for the definiens sentence to be true. Of course, there are important constraints on the enterprise – conditions for its success – but these need not concern us just now. The case is no different when the vehicle of implicit definition is a biconditional or perhaps a generalized biconditional, as in the case of an abstraction principle. There is still no claim or stipulation of sameness of sense. In particular, it is not being claimed or stipulated that the left and right sides of

[10] A claim if the definition aims to capture pre-existing usage, a stipulation otherwise.

(instances of) the biconditional are synoynmous. The stipulation is only that the (generalized) biconditional is to be true. If the definition is successful, the sense of the definiendum is fixed as what it needs to be, for that (generalized) biconditional to be *true*, and hence for (instances of) its left and right sides to have the *same truth-value* – it is *not* fixed as what it needs to be for them to have the *same sense*[11].

Turning now to the second claim, about reconceptualization, there are three points I want to make.

(i) The first simply expands on, and I hope explains, my claim that reconceptualization is not the *means* by which sense – the sense of the number operator, say – is determined, but is rather the *result* of its being determined in a quite different way. If, like Fine, we understand the claim that one sentence – e.g. 'the number of knives = the number of forks' – reconceptualizes what is expressed by another – e.g. 'the knives correspond one-one with the forks' – as requiring the two sentences to be synonymous, then my contrast between reconceptualization as a means of fixing sense and reconceptualization as a result of sense being fixed in some other way will be beside the point, as far as his objection (the problem of proliferation) is concerned. It wouldn't matter whether the claim of synonymy is the means whereby the sense of the number operator, say, is fixed or rather a result of its being fixed in some other way – for it is the claim itself that is objectionable. But as I have explained, there is no need to view the process of implicitly defining the number operator by means of Hume's Principle as involving any claim or stipulation about the synonymy of instances

[11] A possible source of confusion on this point lies in the fact – on which Fine remarks – that an explicit definition can normally be recast as a (generalized) biconditional. For example, instead of giving the definition " 'vixen' means 'female fox' ", we may define: $\forall x$ (x is a vixen $\leftrightarrow$ x is a female fox). Fine suggests that for this reason we can always treat explicit definition as a special case of implicit definition. In one way, this may be right – one can cast an explicit definition in the form of a stipulation of the truth of a sentence expressing a condition involving the definiendum – but in another, more important way, I think it is wrong or at least very misleading, because it obscures a fundamental difference between definitions of the two types, only one of which presents two expressions as having the same sense. Even if it is agreed that explicit definitions can be given as generalized biconditionals, in which the left and right sides have the same sense, it would of course be a gross error to infer that *any* definition which proceeds by stipulating the truth of a biconditional presents its left and right sides as synonymous. I am not, of course, suggesting that Fine makes any such inference.

of its left and right sides. And once that is accepted, there is every point in my contrast.

(ii) The second concerns the *object* of reconceptualization. As we have seen, Fine takes the objects of reconceptualization to be *senses*. By contrast, I think of conceptualizing as bringing *what we talk and think about* under concepts. Senses are not – or not usually, anyway – what we talk and think about, but are rather the means by which we talk and think about other things. Accordingly, I think of reconceptualization as bringing what we are already accustomed to talk and think about in one way under (new) concepts in a new way. Thus what gets reconceptualized, when we introduce the concept of direction by means of the relevant abstraction principle, is a certain type of states of affairs – the state of affairs consisting in line *a* being parallel to line *b* is reconceptualized or redescribed in terms of identity, or thought of instead as consisting in the direction of line *a* being identical to the direction of line *b*. And precisely because reconceptualization involves the introduction and deployment of new concepts, there can be no question of the sentences which articulate the reconception have the same sense as those which conceptualize things in the old way.

(iii) The third concerns (what can be said about) the *conditions* under which it is appropriate to think of one sentence as reconceptualizing what is conceptualized, in a different way, by another.

In my view, there is no need for a *separate* account of the conditions under which one sentence can be regarded as reconceptualizing the type of state of affairs depicted by another – i.e. in addition to an account of the constraints on implicit definition in general. Whenever the vehicle of implicit definition is a biconditional (or universal closure of such), then – provided the definition conforms to the appropriate general constraints – one can regard the left hand side as reconceptualizing the state of affairs depicted by the right hand side.

It is a nice question whether this condition is ever satisfied in any cases other than ones which are covered by the obvious generalization of what Frege says in *Grundlagen* §64. When Frege himself presents his example, what he claims is that we carve up the content of the symbol for parallelism in a new way – 'we replace the symbol // by the more generic symbol =, distributing the special content of the former between *a* and *b*'. What is especially interesting here is that it is the content of the *symbol* for parallelism which Frege thinks of as being carved up in a new way – i.e. as opposed to the content of the whole sentence. I now think this may be more important than I formerly supposed[12], and that it is primarily at the sub-sentential level of relational expressions, rather than with complete sentences, that recarving or reconceptualization should be viewed as taking place. Clearly the salient feature of Frege's example is that we have an equivalence relation between entities of a certain sort. When *x* and *y* stand in a certain equivalence relation, they are the same in a certain respect. If sameness in that respect is of interest or importance to us, we may introduce a word (an abstract noun) for the respect in question – we can then say that *x* and *y* are the same in length, shape, volume, etc., or that they have the same length, etc. The state of affairs consisting in an equivalence relation's holding between *x* and *y* is redescribed – or reconceived – as an identity. So a sufficient condition for a kind of state of affairs depicted by sentences of a certain type, S, to be reconceptualizable using sentences of a different type, R, is that R-sentences assert identities, S-sentences assert that some other equivalence to hold, and R-identities are true just in case suitably related S-equivalences hold. Perhaps there are other sufficient conditions, but perhaps there aren't – I don't see that there has to be a more general story about when one can think of the same 'content' as 'carved' in a new way.

(ii) Implicit definition

As we've seen, Fine contrasts *traditional* implicit definitions with *creative* ones, as he calls them. Where what are defined are terms for objects, traditional implicit definitions, he holds (p. 56), presuppose the existence (and uniqueness) of objects for the newly defined terms to refer to, and we are therefore obliged, in giving such a definition, to

[12] in, for example, Hale (1997).

justify these existential presuppositions. So one can get no epistemological advantage from implicit definitions traditionally understood. By contrast, 'creative' implicit definitions are supposed not to presuppose the existence of the relevant objects:

> The purpose of the definition may indeed be to assign objects to the terms. But these objects are not selected from a previously given domain. Rather the objects are introduced into the discourse simultaneously with their assignment to the terms. (*ibid.*)

One might now suppose – as I think Fine does – that a neo-Fregean must take abstraction principles to function as creative implicit definitions, rather than orthodox or traditional ones. But this is not so. It is of course true that the neo-Fregean cannot agree that abstraction principles presuppose the existence of objects to be the referents of terms formed with the abstraction operator, and so cannot regard them as *traditional* implicit definitions *as Fine characterizes them* – i.e. as carrying such a presupposition. But it would be a gross error to infer from this that the neo-Fregean must take abstraction principles to be creative definitions in any clear or significant sense.

It is in fact quite unclear just what creative definitions, as Fine describes them, are supposed to be. One might think that, if they are to avoid presupposing the existence of the objects which are to be assigned to the defined terms, these objects must be supposed to be somehow produced by the definition. But Fine denies this: the relevant objects are not to be thought of as literally brought into existence by the act of definition – rather, he tells us, the objects are to be viewed as existing 'independently of our practice of referring to them; but it will help to create or constitute our reference to those items. The practice will be constitutive of our reference to the objects, if not of the objects themselves'. I find this rather obscure. For one thing, the claim that the objects 'exist independently of our practice of referring to them' does not seem to square with the earlier claim that the objects are 'not selected from a previously given domain' but 'introduced into the discourse simultaneously with their assignment to the terms'.[13] Nor is it clear how there can be such a thing as 'our reference' [to certain objects] unless the objects in question exist, in which case our practice can 'constitute our reference' [to them] only if either (i) the existence of the relevant objects *is* after all presupposed or (ii) the practice *does* – contrary to what Fine says – constitute the objects themselves.

However, the most important point for present purposes is that there seems to be ample space for a third view about how implicit definitions by abstraction work, and the conditions for their proper use, distinct from both the traditional and creative views as Fine presents them.

On this third view – the view Wright and I have tried to articulate – we do *not*, in laying down an abstraction principle as an implicit definition, *presuppose the existence of objects* to which the new singular terms are to refer. The case is entirely different from that in which we propose to introduce terms for objects of some already recognised kind – e.g. we propose to introduce a term to denote a particular number, or heavenly body, or person. Before we can properly define: 'Let t be the largest prime p such that $p+2$ is also prime', we have to show that there is a prime meeting the stated condition. Strictly, before we can define 'Vulcan is the planet, orbiting the Sun, whose presence is the primary cause of such and such irregularities in the orbit of Mercury', we have to show that there is such a planet – or at least provide good, if not decisive, reasons to believe that there is one. And so in other such cases. But while such a requirement is entirely appropriate in these cases, where what is being introduced is a term purporting reference to an object of some already recognised kind, it can scarcely be meaningfully applied in cases of the sort which concern us – which are precisely distinguished by the fact that we are in the business of simultaneously defining a new

[13] Of course, the two claims don't directly contradict one another – saying that the objects are introduced into the discourse *could* be construed so as to be consistent with the idea that they already exist, independently of their introduction to the discourse, waiting in the wings as it were. But then the claim that they are introduced into the discourse simultaneously with their assignment to the terms would be a flat tautology. And what has become of the contrast with being selected from a given domain?

range of singular terms and setting up a corresponding sortal concept under which their referents (if they have referents) will fall.

Nor, of course, do we somehow *create such objects* in or by giving the definition. What is stipulated, when an abstraction principle is advanced as an implicit definition, is *not* the *existence* of certain objects – referents for the terms featured on its left hand side – but the *truth* of (indefinitely many) biconditionals co-ordinating identity-statements linking such terms with statements involving the relevant equivalence relation over the underlying domain. The truth of any given one of those identity statements, and hence the existence of objects to which its ingredient terms refer, is not stipulated, but follows only given the truth of the co-ordinated statement to the effect that the abstraction's equivalence relation holds among the relevant objects (or concepts, in case of a higher-order abstraction such as Hume's Principle). And the truth of that latter statement will be always a matter of independently constituted fact (about parallelism of certain lines, or one-one correspondence between certain concepts, etc.). What is brought into existence by the stipulation – if anything is – is not objects, but a certain sortal *concept*[14]. What objects, if any, fall under it is – as I've said – entirely dependent upon the truth of instances of the abstractive biconditional's right hand side.

What we do create – if all goes well, at least – is not (new) objects but a new *concept*. Thus our view of implicit definition contrasts sharply with Fine's. As we have seen, he takes implicit definition to be 'an unorthodox mechanism for determination of *reference*', whereas for us, it is a means of fixing *sense*. Even when, as in the case of implicit definition by abstraction, the definition serves to introduce a new range of singular terms, it does so by fixing the conditions for those terms to have reference – it fixes their sense, not their reference. What reference – if any – the new terms possess depends, as I have

[14] Here and in the next paragraph, I use 'concept' as roughly equivalent to 'sense' or 'meaning', not in Frege's post-1892 sense in which a concept is the reference of a grammatical predicate (i.e. function from objects to truth-values or, roughly, property). By contrast, the concepts in the range of the second-order quantifiers in a conceptual abstraction are not senses or meanings but properties. It is a further question whether these properties should be individuated extensionally or intensionally.

explained, on matters entirely independent of the definition. In so far as that involves an application of the context principle, what is appealed to is simply the idea that it suffices for an expression to refer to an object that there be suitable true statements in which it functions as a singular term.

None of this means that anything goes, or that there aren't constraints to which proper implicit definition ought to conform. We can create, or introduce, a concept only when there is logical or conceptual room for one. To put it another way, any concept we can form must – already and independently of any choice or decision of ours – be a *possible* concept. Hence the need for the various constraints we have proposed – such as consistency, harmony, conservativeness. Now, if there is an issue here about presuppositions, it concerns – not the requirement that we justify, prior to giving an implicit definition of a type of term, the belief that there exist objects for terms of that type to refer to – rather whether we are obliged to show (in advance, as it were) that there is a possible concept, i.e. that our proposed definition conforms to such constraints. Fine is very firmly of the opinion that we are thus obliged:

> ... it is highly implausible that we might altogether avoid the epistemic cost involved in adopting Hume's Law as a classical implicit definition of number by adopting one of the less orthodox forms of definition in its place. For whatever the mechanism by which the sense or reference of the defined terms is secured, we surely require assurance that the form of words by which the definition is given can be asserted without danger of contradiction. (*Limits*, 35).

And in a connected footnote, he adds:

> It is for this reason that I do not think we can simply pass from the stipulation of Hume's Law as a definition to its assertion as a truth, without the need for further justification. Hale and Wright ... recognize the need for the definition to be consistent if the transition is to be safe, but fail to acknowledge that the definition-monger is himself under an obligation to show that the transition is safe.

Since, on our view, we implicitly define the number operator by stipulating the *truth* of Hume's Principle, there can be no question of a transition from *stipulating* Hume's Principle *as a definition* to *asserting* it *as a truth*, and so no question of justifying such a transition. But there remains, evidently, a substantial issue over which we and Fine disagree. A proper discussion of this issue requires a paper to itself – here I can only briefly and somewhat dogmatically indicate how I see the matter.

A belief or set of beliefs cannot be true, and so cannot amount knowledge, unless it is consistent. Must we know or be able to show – if we are to be entitled to the belief (or set of beliefs) – that it is consistent? Not only does this not follow – it does not seem that it can be a perfectly general requirement, since it would be viciously regressive[15]. If that is right, then we can be entitled to hold beliefs without prior or independent entitlement to believe them consistent. I claim that something similar holds with regard to stipulations involved in implicit definition. If an expression e is to be implicitly defined by stipulation of the truth of a sentence S containing e, but otherwise composed of expressions whose meanings are already fixed, S must be consistent, i.e. it must be possible for S to express a truth. But it does

[15] For simplicity, take the case of a single belief, that p. If it is a universal requirement of entitlement to belief, that one should know or be able to show that the belief is consistent, then I must know, and so be entitled to believe, that my belief that p is consistent. But then I must know, and so be entitled to believe, that my belief that p is consistent is itself consistent, and so on. It may be claimed that since the belief that p is consistent is weaker than the belief that p, the regress is not vicious. I don't think this is correct. It is true, of course, that the claim that p is consistent is weaker than the claim that p (or that p is true), from which it follows. But it is still a significant claim. Clearly what is important is consistency in the semantic sense – possible truth – rather than just syntactic or proof-theoretic consistency, which is at most a necessary condition for semantic consistency. Of course, if one takes the relevant notion of possibility to be governed by the S4 principle, the claim that the claim that p is consistent is itself consistent (i.e. that it is possibly true that it is possibly true that p) is logically equivalent to the claim that p is consistent (i.e. that it is possibly true that p). But it would be a mistake to think, for this reason, that there is no regress – that we grind to a halt after one step. In the case with which we are concerned, I have to be entitled to believe that it is possibly true that p *independently* of any entitlement to believe that p – i.e. we must have some reason, other than my belief that p, to believe that it is possibly true that p. Let this be q. Then by the universal requirement, I must be entitled to believe that q is consistent, i.e. that it is possibly true that q. Now, either this entitlement to believe that it is possibly true that q derives from my prior entitlement to believe that q – in which case the latter entitlement does not depend on the former, and the alleged universal requirement is not universal after all – or it rests upon some other belief, r, to which I must be independently entitled, so that the regress continues.

not follow that, to be entitled to make such a stipulation, we must be able to prove this, or have some prior and independent entitlement to believe it. And in a similar way, it seems to me, a completely general requirement that any means of conferring sense on an expression must be known or shown not to lead to inconsistency, before we can be entitled to avail ourselves of it, would be equally unreasonable, because again regressive. Roughly, discharging the alleged obligation in any given case would require using some expressions whose meanings are taken as already settled – but if our entitlement to take *their* meanings as already settled depended upon a prior entitlement to believe that the means whereby that was, apparently, accomplished does not lead to inconsistency, then we would be saddled with a further obligation of the same kind, and so on. If that is right, then as in the case of beliefs, we can be entitled to proceed – in this case, employ means of fixing meanings – without prior or independent entitlement to believe in their consistency.

This line of argument does not settle a further important question. This is whether, in the case of entitlement to believe, we should accept that an entitlement to believe in consistency is *normally*, but not invariably, required – so that *absence* of an obligation to justify belief in consistency is the special case that needs to be explained or justified; or whether, rather, an entitlement to believe in consistency is *not* normally required, so that it is the *presence* of such an obligation which is the special case needing explanation or justification. And there will be a similar question regarding definitions and explanations of meaning more generally. But even on the more exacting view – that absence of an obligation to justify belief in consistency is the special case – given that we don't always have to justify belief in the consistency of our means of fixing meanings, it is plausible that when it comes to introducing/explaining the fundamental terms of a theory, we're entitled to adopt definitions without providing proofs of their consistency. If we do so, we have to acknowledge the possibility that our proposed definition will miscarry – will, perhaps in conjunction with other things we accept and are unwilling to give up, lead to inconsistency[16]. Our not having a proof of consistency, and the (consequent) possibility that our definition will turn out bad because inconsistent, non-conservative, or otherwise defective doesn't necessarily mean we can't have knowledge – even *a priori* knowledge – based on the definition. Perhaps it would rule that out, if one supposed that certainty or infallibility is required, at least for *a priori* knowledge. But it seems to me that we should accept no such requirement, any more than we should do so in the case of empirical or *a posteriori* knowledge. What makes knowledge *a priori*, on the view I favour, is not guaranteed absence of all possibility of error, but absence of the possibility of a certain kind of error – absence of the possibility of empirical refutation.

Abstraction and inflation

I have suggested that Fine's thinking about abstraction is shaped by a certain kind of externalism, which I roughly characterized as the idea that what objects there are is – at least to a very large extent – independent of abstraction, so that the acceptability of abstraction principles is then to be viewed as a matter of their being true of a certain domain – the universe of all objects whatever – of independently constituted composition and cardinality. In this final section, I would like to air some doubts about this idea. These can most conveniently be raised in connection with one of Fine's necessary conditions for the truth of an abstraction principle. An abstraction principle always provides an identity-criterion, given by means of an equivalence relation, for the abstracts it 'generates'. The necessary condition I want to discuss is that:

> … the identity-criterion should not be inflationary, the number of equivalence classes must not outstrip the number of objects. There must, that is to say, be a one-one correspondence between all the

[16] As indicated above, there are other constraints to which acceptable definitions should conform, besides consistency, including a form of conservativeness and more generally, harmony. I would extend my claims about consistency to these other constraints.

> equivalence classes, or their representatives, one the one hand, and some or all of the objects, on the other hand. (*Limits*, 4).

When Fine says that the number of equivalence classes (and so the number of abstracts) must not exceed the number of objects, what he means is that the number of equivalence classes (abstracts) must not exceed the number of all objects whatever. It is not difficult to see why one might take this to be a necessary condition for the truth of an abstraction principle. Let us suppose we are dealing with a conceptual abstraction – since it is only in this case that there is any danger of inflation. Then our abstraction operation will be a function from concepts to objects – that is, the domain of the function will comprise certain concepts[17], and its values will be objects. Which concepts are in the domain of the function will depend – in Fine's view, anyway, as we have seen – on an underlying domain of objects. They will be the concepts 'over' this underlying domain of objects. Now, if the underlying domain of objects includes all the objects there are, then assuming our abstraction principle is true, the abstracts it generates must be included in the underlying object domain. So there cannot be more of them than there are objects altogether – necessarily, the number of abstracts is no larger than the number of (all) objects. Thus if an abstraction principle is such that were it true, the number of its abstracts would exceed the number of objects, it can't be true. How could an abstraction principle fail in this way? The obvious answer, it may seem, is simple: suppose the domain of all objects whatever has a certainly cardinality, k. Then the number of concepts on this domain will far exceed this number. Taking concepts as individuated extensionally, there will be 2^k of them. So if our abstraction principle yields a distinct abstract for each concept – as happens if its equivalence relation is co-extensiveness – or even a distinct abstract for most concepts, there will be too many abstracts.

All this may seem quite straightforward. Let me try to explain why I don't think it is. On the face of it, the non-inflationariness condition assumes that it makes good sense to speak of the totality of all objects

[17] Concepts here in the sense of properties, rather than senses or meanings.

whatever, that there is such a thing, and that it has a number. I think this assumption – which one might think of as characteristically externalist[18] assumption – is very much open to question.

On the face of it, it makes good sense to think of one concept *F* as having as many instances as, or fewer instances than, another concept *G* only if *F* and *G* are both sortal concepts – that is, roughly, concepts with which are associated both criteria of application and criteria of identity. Thus on the widely accepted assumption that *brown* is a merely adjectival, non-sortal, concept, it makes no sense to speak of the number of brown objects, or of there being as many brown objects as there are *F*s, for any bona fide sortal *F*. My worry about talk of the number of objects stems, initially, from a doubt on this score. *Object* isn't – or at least, isn't obviously – a sortal concept. It doesn't seem to make much sense – except in special cases, where some relevant sort can be gleaned from the context[19] – to ask how many objects there are.

It might be – and I believe often is – thought that there is no difficulty here, since we can easily supply a suitable concept, i.e. one that is both sortal and applies to each and every object whatever. The usual candidate here is self-identity – that is, we can just take the number of all objects whatever to be Nx: $x = x$, the number of self-identicals. This particular proposal is problematic, because the status of *self-identical* as a genuine sortal is open to question. Certainly it cannot be a genuine sort if – as I think – the relation of identity is itself sortally dependent, in the sense that an identity statement $x = y$ can be true only if there is some sortal concept *F* such that x is the

[18] Although I did not have Hilary Putnam particularly in mind when I introduced the term 'externalism' in this context, externalism as I conceive it has obvious affinities with what he calls 'external' or 'metaphysical realism'. Recall in particular that it is characteristic of Putnam's metaphysical realist to think of 'the world' as having 'a fixed totality of mind-independent objects'.

[19] For example, we can ask how many objects were removed from the accused pockets, and be given the answer: 'Five – a box of matches, three coins and a handkerchief'. It isn't easy to say precisely what is required here for something to count as one object – but it is clear that in normal circumstance, undetached parts of the handkerchief, or the images of HM the Queen impressed on the coins, would not count as distinct objects.

same *F* as *y* (briefly $x =_F y$).[20] An independent argument due to Wright seems to show that *self-identical* is not sortal. It seems undeniable that if *F* is any sortal concept, then so will be its restriction by any other concept *G*, irrespective of whether *G* is sortal or merely adjectival. For example, given that *horse* is sortal, *brown horse*, for example, must likewise be sortal, even though *brown* (or *brown thing)* is itself no sortal. But now if *self-identical* were sortal, *brown self-identical* would likewise have to be so. But since every object is necessarily self-identical, *brown self-identical* is equivalent to *brown* simpliciter – necessarily an object is brown and self-identical just in case it is brown. Since *brown* is not sortal, neither is *brown self-indentical* nor, therefore, *self-identical*. If this is right, then the seemingly good question: *How many self-identicals are there?* has no determinate answer, and 'N*x*: $x = x$' has no determinate reference. There is no universal number. There is also space, I think, for a further doubt, about whether the contexts 'There are just as many *F*s as *G*s' and 'There are fewer *F*s than *G*s' are well-defined, or have determine truth-conditions, when one or both of *F* and *G* is non-sortal, and hence whether *self-identical* can be a suitable filler for *F* or *G* in those contexts.

An objector might concede that a concept *F* must be sortal for the *how many* question and talk of the number of *F*s to be in good order, and agree that *self-identical* is therefore, as it stands, unsuitable, but argue that we can get around this and re-instate the number of all objects, by defining it slightly differently. First note that if *F* is sortal, then so is *self-identical F* (i.e. the concept for which the predicate '*x* is the same *F* as *x*' – briefly '$x =_F x$' – stands). Of course, one can't get around Wright's difficulty just by picking some particular sortal concept *F* and using *self-identical F* in place of *self-identical*. More precisely, *self-identical F* will – though sortal – fail to apply to every object unless *F* itself does so; but if *F* itself is a universal sortal, then the detour through self-identity is a waste of time. We may, however,

[20] For a very clear explanation and defence of the thesis of the Sortal Dependence (as opposed to the Sortal Relativity) of Identity, see Wiggins (1980). For Wright's argument, see 'Is Hume's Principle Analytic?' in Hale & Wright (2001, 307-34). The argument is given on p. 315.

form the complex predicates '$\forall F\, x =_F y$' and '$\exists F\, x =_F y$'. And from these in turn we may form '$\forall F\, x =_F x$' and '$\exists F\, x =_F x$'. Presumably the first of these last two is true of no object whatever, and it would seem that every object whatever must satisfy the second. And this – or so it might be supposed – gives us a way out: just define the number of all objects as N*x*: $\exists F\, x =_F x$.[21]

Does that settle the matter? I don't think so. A concept *F*'s being sortal is a *necessary* condition for the how many question to be in good order and for the corrresponding term 'N*x*: *Fx*' to have determinate reference. But it is arguably *not sufficient.* Indeed, it is fairly obviously insufficient, if there are – as there certainly seem to be – concepts which are sortal but indefinitely extensible in Dummett's sense (however precisely one thinks that difficult notion is best to be explicated[22]). The concepts of *ordinal number*, *cardinal number* and *set* all seem to be in this case. And, since the ordinals, cardinals and sets are among the objects that there are, it is plausible that any universal sortal concept must likewise be indefinitely extensible[23]. But in any case, there is a particular reason to doubt that 'N*x*: *Vx*' can have a determinate reference. For – given that our proposed definition of the universal concept *V* involves quantification over (sortal) concepts – it could do so only if it were already determinate what sortal concepts there are. It can scarcely be that there is a determinate answer to the question: *How many objects are there?* – where this is construed as: *For how many x do we have* $\exists F\, x =_F x$*?* – unless there is

[21] Since $x =_F x$ iff *Fx*, one could just as well use '$\exists F\, Fx$' in place of '$\exists F\, x =_F x$'. Of course, this way out is good only if the concept $\exists F\, x =_F x$ is itself a genuine sortal concept. The mere fact that '$\exists F\, x =_F x$' is true of every object is certainly not enough to make it a sortal predicate – any more than the fact that '*x* has mass' is true of every physical object is enough to make it a sortal predicate of physical objects. For *being, for some F, the same F as itself* to be a genuine sortal, there needs to be a criterion of identity for the objects falling under it. But so, it seems, there is. Let us abbreviate our predicate '$\exists F\, x =_F x$' by '*V*x'. Suppose *b* and *c* both satisfy '*Vx*'. What condition is both necessary and sufficient for *b* and *c* to be one and the same *V*? Well, the obvious answer is that *b* and *c* are one just in case for some single *F*, $b =_F c$. '*V*' has thus both a criterion of application – *Vx* iff for some *F*, $x =_F x$ – and a criterion of identity – $x =_V y$ iff for some *F*, $x =_F y$.

[22] For a rough explanation, see the below, six paragraphs on.

[23] This will be so, if we can assume that if *F* is indefinitely extensible and $\forall x(Fx \rightarrow Gx)$, then *G* is likewise indefinitely extensible.

a determinate answer to the question: *What sortal concepts are there?* It is at least not obvious that there can be a determinate answer to *that* question.

Someone might protest: "There is no difficulty over that. For any given domain of objects, the corresponding domain of concepts is fixed. For each and every way of dividing the domain of objects, there is a concept, and those are all the concepts. If the domain of objects comprises k objects, there are thus 2^k concepts." But we have been here before – to assume a domain of objects 'given' is simply to fail to engage with the problem, or to assume it somehow solved. We cannot both assume a given domain of objects as a means of fixing the range of the quantifier 'For some *F*', and at the same time use that quantifier to define the supposedly sortal predicate '*V*' (i.e. '*x* is an object').

If a domain of objects is already somehow fixed as comprising k objects, then it is, of course, quite right that there are 2^k concepts on the domain – at least provided that concepts are individuated extensionally. But whilst there is no general objection to treating concepts extensionally – as, in effect, determined simply by what objects fall under them – it is questionable whether they are appropriately so treated in the present context, for at least two reasons.

The first, more general, reason is that it makes sense to think of concepts extensionally only if it is already determinate what objects belong to the domain on which they are to be thought of as defined. That condition may well be met in a particular case – it will be met if, for example, we are considering the domain comprising exactly the natural numbers. But it clearly cannot be assumed met in the present case.

Secondly, and more specifically, the whole point of insisting that an identity-statement $x = y$ has to be understood as true if and only if x and y are one and the same F, for some appropriate sortal F, is lost, if the covering sortal F is thought of as determined purely extensionally. The point is – at least in part – that objects cannot be individuated save as instances of some sortal concept or other, so that unless some appropriate sortal is specified or understood from the context, it is simply not determinate what is being asserted, when it is said that

$x = y$. If objects could be individuated simply as objects, there would be no justification for insisting that '$x = y$' must be understood so that it can be true only if $x =_F y$ for some specific sortal F, such as *horse*, *person*, *number* or the like – any identity-statement x = y could be understood as claiming simply that x is the same *object* as y.

If what I've said is right, the universal concept *self-identical under F, for some sortal F* exhibits something akin to the property of indefinite extensibility. I'm not sure that it *is* indefinitely extensible in the usual sense, which requires, for a concept G to be indefinitely extensible, that given any definite collection of Gs, there is an object satisfying the requirements for being G which cannot, on pain of contradiction, be one of that collection. I shall say that the universal concept is *sortally indeterminate*, leaving open the question of the relations between sortal indeterminacy and indefinite extensibility. Sortal indeterminacy differs from – and is in a way more radical than – that more familiar kind of indeterminacy we usually call vagueness. A concept is vague – at least on some widely accepted views of vagueness – when there are objects of a sort to which the concept applies, in regard to which it is indeterminate whether the concept applies or not. Some sortal concepts are vague (e.g. *novel*, *large bird*) and naturally to these no definite number can be assigned – but at least in some cases we may still be able to assign lower and/or upper bounds on their number, and so claim truly or falsely that there are more/fewer of their instances than there are Fs, for some other (possibly also vague) sortal (e.g. there are more large birds than there are small opera houses). But the sortal indeterminacy of the concept of object captured by $\exists F\, x =_F x$ is evidently not a matter of their being *objects* – or entities of any kind – with regard to which it is indeterminate whether or not *they* fall under the concept. The indeterminacy results from its not being determinate what sortal concepts there are. Of course, there may be a determinate collection of sortal concepts expressed, or expressible, in some given language. And maybe any sortal concept has to be expressible in some possible language. But is there a determinate collection of all sortal concepts? This seems to me doubtful. And for this reason, it seems to me doubtful that one can – as one can with vague concepts – sensibly talk of there being more concepts than objects.

If these doubts are well-founded, it seems to me that one cannot satisfactorily formulate a necessary condition for the truth, or acceptability, of an abstraction principle in terms of its generating no more abstracts than there are objects altogether. Whether there is a way of reformulating Fine's requirement which captures the basic idea that good abstractions should be non-inflationary is a good question, but I do not know the answer to it. It seems clear that there is no evident reason to ban abstractions which inflate on definite domains of determinate cardinality. Fine himself is, as I understand it, happy enough with abstractions which, like Hume's Principle, inflate on domains up to a certain cardinal size and stabilize thereafter. And I cannot myself see any reason to object to abstractions which inflate on all cardinally definite domains – such abstractions would generate a universe of abstracts which, like the universe of sets as standardly conceived, is not closed off, i.e. has itself no cardinal size. I am not sure that it is possible to characterize an objectionable kind of inflationariness without recourse to the idea that it is the kind of inflationariness that leads to contradiction. But if that's the best one can do, the requirement seems to me to boil down to the requirement of consistency, which we must have anyway.[24]

[24] I am very grateful to Kit Fine for extensive and very helpful discussion of our points of disagreement. I should also like to thank the organisers and other participants in the Neuchâtel Logicism conference for stimulating discussion of a somewhat shorter version of this material.

References

FINE K. (2002). *The Limits of Abstraction*. Oxford: Clarendon.

FREGE G. (1879). *Begriffsschrift*. Halle: L. Nebert. [Engl. trans. in van Heijenoort J. (ed). *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*. Harvard University Press. 1967].

FREGE G. (1884). *Die Grundlagen der Arithmetik*. Breslau: W. Kœbner. [Reprinted with Engl. trans. by Austin J. L. *The Foundations of Arithmetic*. Oxford: Blackwell. 1950].

HALE B. (1997). *Grundlagen* §64. *Proceedings of the Aristotelian Society* XCVII. 243-261.

HALE B. & WRIGHT C. (2001). *The Reason's Proper Study*. Oxford University Press.

PARSONS C. (1965). Frege's Theory of Number. In Black M. (ed). *Philosophy in America*. Ithaca: Cornell University Press. 180-203.

WIGGINS D. (1980). *Sameness and Substance*. Oxford: Blackwell.

WRIGHT C. (1983). *Frege's Conception of Numbers as Objects*. Aberdeen University Press.

On some Consequences of the Definitional Unprovability of Hume's Principle

Luca Incurvati

1. Neo-logicism, Hero and the bad company objection

In *Grundlagen* Frege proposes[1] to define the concept of number by laying down what, following a suggestion of Boolos'[2], is nowadays known as *Hume's Principle*:

(HP) The number of *F*s = The number of *G*s if and only if there is a one-one correlation between the *F*s and the *G*s.

However, he rejects[3] the proposal because of the so-called *Caesar problem*, that is, the problem that (HP) succeeds in settling the truth-value of statements of the form 'The number of *F*s = The number of *G*s', but not that of statements of the form 'The number of *F*s = *q*', where '*q*' is an arbitrary singular term. Thus, Frege shifts to an explicit definition in terms of extensions of concepts and later, in *Grundgesetze*, supplies a theory which is intended to show the purely logical nature of the concept of extension. This theory is embodied in the well-known and unfortunate *Axiom V*:

(V) The extension of *F*s = The extension of *G*s if and only if the *F*s and the *G*s are coextensive.

[1] Frege 1884, § 63.

[2] Boolos 1986-87, 171.

[3] Frege 1884, § 66.

Then, he proceeds with the proof of the basic laws of arithmetic from the definition of numbers in terms of extensions through second-order logic. He is therefore seen as having subscribed to *logicism*, the thesis that arithmetic is derivable from logic and suitable definitions. This thesis, on Frege's definition of analyticity, amounts to the thesis that arithmetical truths are analytic. Unfortunately, Russell's Paradox is derivable from (V) through simple logical rules of inference: (V), far from being a logical truth, turns out to be inconsistent.

Crispin Wright in his *Frege's Conception of Numbers as Objects*[4] claims that Frege's choice is premature: it is possible to solve the Caesar problem without turning to the definition of numbers in terms of extensions and therefore without making use of Axiom V. For Frege, while sketching in *Grundlagen* the derivation of the Peano Axioms from logic and definitions, makes use of the notion of extension only when he identifies numbers with extensions. This allows him to prove the truth of (HP)[5]. The notion of extension does not play any further role in the derivation of the basic laws of arithmetic: the Peano Axioms are theorems of *Frege Arithmetic*[6] (henceforward FA), the system resulting from the adjoining of (HP) to a suitable axiomatization of second-order logic. This result is now known as *Frege's Theorem*[7]. Moreover, FA is equiconsistent with mathematical analysis and, therefore, presumably consistent[8].

Wright ascribes to Frege's Theorem a deep philosophical significance, claiming that it can be the basis for the recovery of the logicist program: *neo-logicism*[9] is the thesis that arithmetical truths are

[4] Wright 1983.

[5] Frege 1884, § 73.

[6] Boolos so labels the system in Boolos 1987, 183-201.

[7] Boolos 1990b, 209.

[8] See Boolos 1987, 187-191.

[9] The term 'neo-logicism' is sometimes used to describe the entire program in the philosophy of mathematics. The program is also called 'neo-Fregean'. On the one hand, this term strikes me as more apt to refer to the two main theses (logicism and platonism) which distinguish the program and which Frege may be reasonably taken as having held. On the other hand, it raises some exegetical worries, since it may be doubted that the spirit of the program is completely Fregean (see Dummett 1991; Ruffino 2003). In this paper I will feel free to use the two terms interchangeably, since I will be mainly concerned with the epistemological component of the program.

analytic, the Peano Axioms being logical consequences of an analytic truth, namely (HP). Although Frege was wrong in thinking that arithmetic is reducible to logic, the philosophical essence of logicism, Wright argues, can be saved: arithmetical truths are epistemically innocent, since they inherit this status from (HP) through logical consequence.

In recent writings[10], Wright seeks to clarify the epistemological value of Frege's Theorem by showing how Hero, a subject endowed with mastery of second-order logic, could come to acquire knowledge of arithmetical truths on receipt of Hume's Principle. The idea is that Hero stipulates Hume's Principle and, in so doing, comes to know that it is true. Then, he can gain knowledge of arithmetic simply by reflecting upon its logical consequences.

Since the publication of Wright's book, many objections have been raised to the analyticity of (HP). Let me focus on one of them, the well-known *bad company* objection. (HP) is an instance of a kind of principles, known as *abstractions principles*, which give necessary and sufficient conditions for the identity of objects of some kind in terms of the obtaining of an equivalence relation between entities of another kind. The idea is to introduce a concept by giving identity criteria for the objects which fall under it. Call this method of concept-introduction the *method of abstraction*. The troubles begin as soon as we realize that (V), as well as (HP), has the form of an abstraction principle. We introduce the concept of extension by stipulating that the truth-conditions of identity statements between extensions are to be given in terms of coextensivity. But how can Hume's Principle be thought of as analytic if it belongs to a genre of principles which has inconsistent instances? However, this line of thought is, according to Wright, too simple-minded. Single failures of a method of concept-introduction do not show that the whole method is defective. What they show, rather, is that the method needs further refinement. Just lay down consistency as a constraint on the acceptability of an abstraction

[10] See Wright 1998b, 247-255 and Wright 1998c, 263-271.

principle and you will get what you need. But, as Boolos has shown[11], this reply will not do, since there are consistent abstraction principles which are inconsistent with (HP). In order to overcome this new difficulty, Wright has proposed further restrictions, such as conservativeness[12] and modesty[13]. On the other hand, further counterexamples have been put forward by the critics of neo-logicism[14]. The debate is still open and alive. However, I will not enter into it here. What I want to do, rather, is to explore some consequences of the introduction of restrictions.

2. The definitional unprovability of Hume's Principle and Success by Default

First of all, let us inquire after the epistemological status of restrictions. Consider the most intuitive one: consistency. Let Con(FA) be a sentence in the language of FA which expresses FA's consistency. Gödel's second incompleteness theorem tells us that, if FA is consistent, then Con(FA) is not provable in FA. We need other resources, resources which cannot be formalized in FA. Can we attribute such resources to Hero? *Ex hypothesi*, the only resources at his disposal are second-order logic and (HP). So there seems to be no way for him to prove the consistency of FA, even in principle. At this stage someone might point out that Boolos' proof of the equiconsistency of FA and analysis gives us strong reasons to believe that FA is consistent. Indeed, from our point of view, this is a very important result. Hero, however, cannot rely on Boolos' proof for two reasons. First, as Boolos himself emphasized, doubts may be raised about its actually delivering knowledge of consistency, since it leaves open the possibility that FA and analysis are both inconsistent[15]. Secondly and

[11] Boolos 1990b, 214-215.

[12] Wright 1997, 295-297.

[13] Wright 1998a, 324-330.

[14] Shapiro & Weir 1999 and Weir, forthcoming.

[15] Boolos 1997, 313.

crucially, Hero cannot be credited with knowledge of analysis, on pain of giving up the foundational significance of Frege's Theorem. So there can be no proof through which Hero can go in order to make sure of the consistency of Hume's Principle. What about the consistency of others abstraction principles? As Heck has shown[16], the consistency of second-order abstraction principles (that is, those abstraction principles whose equivalence relation is a relation on concepts) is in general undecidable.

The introduction of restrictions, then, forces the neo-Fregean to endorse the following thesis (**Definitional Unprovability**):

> (**DU**) For most abstraction principles AP (including (HP)), Hero cannot prove that AP is good.

Since Hero's story is intended to show the foundational significance of Frege's Theorem, endorsement of **DU** amounts to accepting that, when inquiring after the epistemological status of (HP), we cannot presuppose a demonstrative warrant for its truth. Nonetheless, the neo-Fregean claims that if (HP) is true, then Hero knows that it is true. On this view, Hero seems to be credited with something like a default justification for the truth of (HP). This point, already noted by Fraser MacBride[17], has been recently emphasized in a somehow different way by Augustín Rayo[18]. He claims that the neo-Fregean program rests on a hidden assumption, which he labels *Success by Default*. According to *Success by Default*, we are justified in thinking that a stipulation is successful (i.e. bestows meaning on the expressions which introduces in such a way that the stipulated sentence turns out to be true) unless there are reasons to believe otherwise. Rayo goes on to argue that since *Success by Default* has not yet received a proper justification, there is a serious gap in the neo-logicist program. What I will argue in the next section is that, given **DU**, even if we grant the correctness of *Success by Default*, it does not follow that Hero knows that (HP) is true.

[16] Heck 1992, 492-493.
[17] MacBride 2003.
[18] Rayo 2003.

3. Definitional unprovability and knowledge

In order to fully appreciate the situation, let us approach the matter by recalling a well-known example in the theory of knowledge. The example is Goldman's, on behalf of the theory of relevant alternatives[19]. You are asked to imagine a man, Henry, who, while driving a car in the countryside, identifies various objects on the landscape for his son: cows, tractors, and the like. Pointing at the objects, he says: 'That's a cow', 'That's a tractor', and so on. After a while, a barn comes into his view. As usual, he says: 'That's a barn'. Almost everyone would agree that Henry *knows* that the object he indicated is a barn. Now imagine that, unknown to Henry, the district across which he is driving is full of papier-mâché facsimiles of barns. However, the barn to which Henry has drawn his son's attention is an actual one. Even though Henry truly believes that there is a barn and seems to be perfectly justified in holding this belief, we would not ascribe knowledge to him in such situation. The reason, it is often said, is that the truth of what he said largely depends on lucky circumstances. But I think we can distinguish two ingredients which, taken together, lead our intuitions not to count the example as knowledge: (i) Henry cannot distinguish between the actual barn and the fake barns; (ii) the number of fake barns, compared to the number of actual barns, is remarkable. A single fake barn in a district full of actual barns would not deprive his belief of the status of knowledge.

Now consider Hero. He lays down Hume's Principle and believes it to be true. Moreover, Hume's Principle is successful and therefore true. Finally, he is justified in holding it to be so, provided *Success by Default* is correct. However, this is not enough for his belief to count as knowledge. In fact, there seems to be striking similarities between Henry's and Hero's epistemic situations. **DU** assures us that (i*) Hero cannot distinguish between (HP) and the inconsistent examples. When deciding whether to accept (HP), all he can do is check whether it has the form of an abstraction principle. Besides being satisfied by (HP),

[19] Goldman 1976.

however, this requirement is also satisfied by the inconsistent examples of the method of abstraction. To be sure, had he accepted an inconsistent principle, he could have scrutinized whether the usual lines to contradiction succeed in that case. But this would have only worked for few cases. So we have a perfect analogy with point (i) in the barn example. I guess it is (ii) which, to neo-Fregean eyes, reveals the real disanalogy between the two cases. While trying to show that failures of a method of concept-introduction are not usually taken as questioning the possibility of using such a method, Wright sometimes considers the method of fixing the sense of a sentence by stipulating its truth-conditions. Since no one would think that such method is mistaken simply on the basis of its having troublesome instances, why should we have a different attitude towards the method of abstraction? So Wright makes it sound as if he thinks that this method and the method of abstraction are perfectly analogous. The *rationale* seems to be that in both cases we are concerned with forms of definitions and introduction of concepts and expressions in our language. This analogy, however, is of no help here. What matters in the present situation is not the aim of the method, but whether its failures are isolated cases. But even if this were the case as regards the above-mentioned method of sense-fixing, it would not follow that it is the case as far as the method of abstraction is concerned. At this stage, someone might object that this kind of worry is ill-founded, pointing out that the method of abstraction is widely entrenched in mathematical practice. But this does not really tell us very much. For it still remains to be ascertained whether over the years mathematics has been emended in such a way as to remove the bad companions from it, thanks to considerations which Hero cannot take into account. In any event, the method does fail quite often, and we do register frequent historical failures. This sort of concern can get the support of some passages taken from some articles of Boolos. He expresses the following misgiving:

> It is clear that an account of logical truth that attempts to distinguish Hume's Principle as a logical truth will have the hard task of explaining why Hume's Principle is a logical truth even though two

> other similar-looking principles are not. [...] They read: Extensions of concepts are identical if and only if those concepts are coextensive; and: Relation numbers of relations are identical if and only if those relations are isomorphic. Russell showed the former inconsistent; Harold Hodes has astutely observed that the latter leads to the Burali-Forti paradox.[20]

Returning to the bad company objection at a distance of years, he writes more explicitly:

> What I think I was doing was illustrating that what is called [...] "contextual definition" is not, *in general*, a permissible way of introducing a concept. [...] I cited Hodes' splendid observation that the relation-number principle [...] leads to the Burali-Forti paradox in order to point out that Basic Law V was not an isolated case.[21]

I guess what Boolos wanted to stress is that failures of a method of concept-introduction throw an unfavourable light on the legitimacy of the whole method. As we have seen, Wright is not sympathetic at all towards this kind of reasoning. In Boolos' passage, however, there is another important point, which might be taken as showing that a perfect analogy with (ii) applies to Hero's situation: (ii*) the number of inconsistent examples, compared to the number of consistent examples, is remarkable.

Hero's story astonishingly resembles Henry's. You have a subject who masters second-order logic. This subject stipulates (HP) and comes to believe it. If *Success by Default* holds, his belief is justified, since there are no reasons to think that such stipulation is not successful. Since this stipulation *is* successful, it is true. However, there are many other inconsistent principles which he could have stipulated and which he has no means to distinguish from (HP). How can we ascribe knowledge to him? To stress: I am not claiming that the analogy conclusively establishes that Hero and Henry are on the same epistemic foot. What I am claiming, rather, is that even granted the correctness of *Success by Default*, the neo-Fregean still owes us an argument which shows that the bad companions, despite their frequent

[20] Boolos 1990b, 214.

[21] Boolos 1997, 311.

occurrence, really are isolated cases, if she wants to claim that the stipulation of (HP) results in knowledge useful for foundational purposes.

4. Definitional unprovability, bad definitions and use

In this section, I will argue that, given **DU**, the neo-Fregean seems to have troubles in explaining our practices and our behaviour towards definitions. In doing this, I will be outlining a different conception of how I think we should regard abstraction principles.

The neo-Fregean completely subscribes to what she calls the *traditional connection*, i.e. the thesis that some important kinds of a priori knowledge, in particular logical and mathematical knowledge, are grounded in the stipulation of implicit definitions. On this conception, the path from implicit definitions to a priori knowledge proceeds as follows. We stipulate the truth of a certain sentence '#*f*' – where '#_' is a matrix whose content is known and '*f*' is a previously contentless expression whose syntactic category is such as to make it suitable to fill the gap in '#_'. The stipulation, then, confers meaning on '*f*' in such a way that (i) '#*f*' can be understood and (ii) the meaning of '*f*' is such as to render '#*f*' true. This puts us in a position to recognize that the sentence is true, since stipulated to be so. This, in turn, results in a priori knowledge of that very sentence.

Prima facie, what appears surprising is the idea that the truth of a sentence depends upon a decision of ours. To be plausible, this idea cannot be held unrestrictedly, without further qualification. To this end, the neo-Fregean specifies two conditions which, when satisfied, guarantee that we can indeed stipulate the truth of '#*f*' and that the meaning so bestowed on '*f*' will be such as to make '#*f*' true.

The first condition requires that some stipulations should not be *arrogant*, where a stipulation is arrogant when its truth "cannot justifiably be affirmed without collateral (a posteriori) epistemic work"[22]. This condition is needed to characterize a class of sentences

[22] Hale & Wright 2000, 128.

whose truth can be legitimately stipulated without going outside of the domain of the a priori. Sometimes, to say the least, a justification for the truth of a sentence demands cooperation from the world.

The second condition requires that the stipulation of '#*f*' determine a meaning for '*f*'. This condition is needed to deal with the obvious objection that there are cases in which the stipulated sentence, although not arrogant, far from being true, proves to be inconsistent. For someone might say that these cases show that when we stipulate a sentence we do not establish its truth. The neo-Fregean replies that when we stipulate a sentence we do establish its truth, but that there are cases in which the stipulation fails to determine a genuine meaning for the *definiendum*. The distinction between these cases and the good ones can be drawn thanks to some constraints. The neo-Fregean is therefore committed to the following thesis:

> (T) The stipulation of an abstraction principle which does not meet the admissibility constraints fails to provide the *definiendum* with a genuine meaning.

(T), however, is very problematic once **DU** is accepted. For **DU** opens up the possibility of a scenario where Hero comes to accept an abstraction principle which is, by neo-Fregean criteria, bad. It seems that Hero can go on to use the concept that the bad definition serves to introduce. What account are we to give of Hero's use? The only viable strategy for the neo-Fregean is to claim that Hero's use does not count as a *proper* use. However, this seems to clash with the way we conceive of our practices. We usually think that good definitions and bad definitions, although differing as regards features such as consistency, deliver concepts which can be perfectly understood and used. None of us would say that the concept of phlogiston does not have a genuine meaning. The neo-Fregean, for her part, would stress the difference between scientific theoretical terms and logical and mathematical expression. But are we really willing to say that, for example, when using naïve set theory, mathematicians did not genuinely mean what they were talking about?

Moreover, what account are we to give of Hero's attitude towards an abstraction principle if he cannot have a demonstrative warrant for its truth? From Hero's point of view the stipulation of an abstraction principle seems to be only an *attempt*. Hence, it cannot be right to describe him as establishing the *truth* of the abstraction principle. When he stipulates an abstraction principle, his attitude seems rather to be that of *trying* to capture a truth. If we adopt this stance, we also become free to drop (**T**). On this view, in fact, the meaning-constituting role is not played by the *truth* of the principle, but by our *regarding* it *as* true. The stipulation bestows genuine meaning on the *definiendum* independently of its success.

To be sure, there is a familiar move which would put us in a position to deal with the two above-mentioned problems while saving the standard account that a stipulation consists in establishing the truth of a sentence. On a long-standing tradition about the definition of scientific theoretical terms[23], we cannot stipulate the truth of the theory itself, since we have to allow for the possibility that the theory could be false. Nevertheless, we can stipulate the truth of some other sentence that suffices to bestow the intended meaning on the *definiendum* while remaining true if the theory gets disconfirmed by experience. On this conception, the theory is factorized into two components:

(R) $\exists x\,(\#x)$

and

(C) $\exists x\,(\#x) \rightarrow \#f.$

The first component, known as the *Ramsey sentence*, captures the empirical content of the theory, whereas the second component, usually called the *Carnap conditional*, serves to confer meaning on the *definiendum*. Empirical disconfirmation of the theory affects the truth of (R) but not that of (C), whose truth we are therefore free to stipulate. Analogously, we would be free to stipulate the truth of the Carnap conditional of an abstraction principle, even though this might be, for all that Hero knows, inconsistent. This strategy would also enable us to drop (**T**), since on this conception the meaning-constituting role is not played by the theory but by its Carnap conditional.

[23] See, for example, Carnap 1928 and Ramsey 1931.

Wright and Hale claim that even if this kind of account is correct as far as scientific theoretical terms are concerned, it does not follow that the same account must be given as regards the introduction of mathematical and logical expression. However, **DU** implies that Hero cannot rule out the possibility that the theory which (HP) embodies is false. So if we really want to hold on the idea that implicit definitions consist in the stipulation of the truth of certain sentences, what should be taken as being stipulated is not (HP) but the following principle:

> (HP*) If there is a function % from properties to objects such that %Fs = %Gs if and only if there is a one-one correlation between the *F*s and the *G*s, then The number of *F*s = The number of *G*s if and only if there is a one-one correlation between the *F*s and the *G*s.

What I am suggesting, then, is that, given **DU**, we have two options. Either we hold on the thesis that what is stipulated is the truth of a sentence, in which case we have to allow for the possibility that the theory is false, falling back on its Carnap conditional, or we accept that a stipulation is only an attempt, in which case we have to give up the idea that what we do when we stipulate a sentence is to establish its truth. Either way, Hume's Principle loses its special role in the neo-Fregean program. The former option is useless for the neo-Fregean's purposes, since (HP*) is not strong enough to provide for the derivation of the basic laws of arithmetic in second-order logic. The latter option deprives (HP) of its special epistemological value, since an attempt, even if successful, can be hardly said to result by itself in knowledge.

5. Extending our results and a bit of holism

I now want to show how the considerations of the previous section could have a wider range of application and extend from Hero's situation to ours. Let S be the set of constraints on the acceptability of an abstraction principle which have been laid down so far (consistency, conservativeness, etc.) and let Q be a complete set of constraints suitable to separate the good companions from the bad ones. Even if we consider ourselves able to know that (HP) meets the constraints in S, we know that it is successful only if:

(i) $Q \subseteq S$.

I said at the outset that I would not enter into the debate concerning the bad company objection. Let me say, however, that there are serious reasons to doubt that all the constraints in Q have been found[24]: Q seem to be a proper extension of S, contrary to (i). Thus, we seem to be in the same situation as Hero: our stipulation is an attempt because an abstraction principle might be bad owing to a constraint which is in Q but not in S. So if we want our stipulation to be something more than an attempt, at least two further conditions need to be satisfied:

(ii) we have succeeded in giving a complete list of the members of Q

(iii) we know that (HP) meets all the constraints in Q.

(ii), however, can be satisfied only if there is a set such as Q. But what guarantee do we have that there is such a set? While working on some analogies between the method of abstraction and other methods of concept-formation, Wright writes:

> [W]e should not, presumably, conclude that the whole idea that properties can be defined by the stipulation of satisfaction condition is misbegotten. Rather, some kind of restriction is wanted. Nor, crucially, pending such a restriction, should we suspend judgement about what appear to be perfectly innocent examples of such procedure. The sensible response is rather that there is a distinction to be

[24] See Weir, forthcoming.

> drawn *which we are, perhaps, not clear how exactly to draw* but which, once drawn, will safeguard the vast majority of cases where we fix a property by stipulating satisfaction conditions [*my emphasis*].[25]

In this passage, Wright himself seems to acknowledge that it is not completely clear how exactly to draw the distinction between the good and the bad instances of a method of concept introduction. But, then, why should we take it for granted that such distinction can be drawn? In a later article, Wright and Hale say, more cautiously, that they see "no reason for pessimism that such a complete set of constraints can be given"[26]. But the growing number of counterexamples might just be taken as casting doubts on the possibility of characterizing the required set. Moreover, any kind of consideration seems to be potentially relevant in this context. We could be led to modify or reject a concept for many different reasons. So many that it can be doubted that a complete list of constraints can actually be given. So our being in the same situation as Hero might be something more than a temporary situation. Relatedly, in the absence of a set such as Q, how can we say that the considerations we will have to call into account in order to separate the good companions from the bad ones will not jeopardize its analyticity? A posteriori threatens.

The picture which has emerged, then, is the following. We introduce a concept. This introduction, however, is only an attempt to capture a truth. We do not establish that a principle is true even when the principle *is* true. The discovery of inconsistencies or other problems will lead us to modify the concept. If the concept cannot be modified in such a way as to make it acceptable from the new perspective, the concept will be dropped. Let me end, in this connection, with an historical remark which seems to shed light on how this picture seems to fit well with our practices. In 1893, Frege introduced the concept of extension by laying down Axiom V in the first volume of *Grundgesetze*. Russell's discovery of the paradox in 1902 forced him to try to emend the concept of extension in such a way as to make it

[25] Wright 1997, 288.

[26] Wright & Hale 2000, 137.

consistent but powerful enough for his purposes. In 1906, he realized that this could not be done and decided to drop the concept. His behaviour closely resembles that of a scientist who, when presented with contrary empirical evidence, decides to modify and eventually comes to reject the theory. The difference between scientific and mathematical enterprises, though important, should not be exaggerated.

References

BOGHOSSIAN P. & PEACOCKE C. (eds). (2000). *New Essays on the A Priori*. Oxford: Clarendon.

BOOLOS G. (1986-87). Saving Frege from Contradiction. *Proceedings of the Aristotelian Society* 87. 137-151. [Boolos 1998, 171-182].

BOOLOS G. (1987) The Consistency of Frege's *Foundations of Arithmetic*. In Thomson 1987, 3-20. [Boolos 1998, 183-201].

BOOLOS G. (ed). (1990a). *Meaning and Method: Essays in Honor of Hilary Putnam*. Cambridge University Press.

BOOLOS G. (1990b). The Standard of Equality of Numbers. In Boolos, 1990a, 261-278. [Boolos 1998, 202-219].

BOOLOS G. (1997). Is Hume's Principle Analytic? In Heck 1997, 245-262. [Boolos 1998, 301-314].

BOOLOS G. (1998). *Logic, Logic, and Logic*. Cambridge Mass.: Harvard University Press.

CARNAP R. (1928). *Der Logische Aufbau der Welt*. Berlin: Schlachtensee Weltkreis-Verlag.

DUMMETT M. (1991). *Frege. Philosophy of Mathematics*. London: Duckworth.

FREGE G. (1884). *Die Grundlagen der Arithmetik*. Breslau: Koebner.

FREGE G. (1893, 1903). *Grundgesetze der Arithmetik, begriffsschriftlich abgeleitet*, 2 vol., Jena: Pohle.

GOLDMAN A. I. (1976). Discrimination and Perceptual Knowledge. *Journal of Philosophy* 73. 771-91.

HALE B. & WRIGHT C. (2000). Implicit Definition and the A Priori. In Boghossian & Peacocke 2000, 286-319. [Hale & Wright 2001, 117-150].

HALE B. & WRIGHT C. (2001). *The Reason's Proper Study. Essays Towards a Neo-Fregean Philosophy of Mathematics*. Oxford: Clarendon.

HECK JR. R. (1992). On the Consistency of Second-Order Contextual Definitions. *Noûs* 64. 491-494.

HECK JR. R. (ed). (1997). *Language, Thought and Logic: Essays in Honour of Michael Dummett*. Oxford University Press.

MACBRIDE F. (2003). Speaking with Shadows: A Study of Neo-Logicism. *British Journal for the Philosophy of Science* 54/1. 103-163.

RAMSEY F. P. (1931). Theories. In his *The Foundations of Mathematics and Other Logical Essays*. London: Routledge & Kegan Paul. 212-237.

RAYO A. (2003). Success by Default? *Philosophia Mathematica* 11/3. 305-322.

RUFFINO M. (2003). Why Frege Would Not Be a Neo-Fregean. *Mind* 112. 51-78.

SHAPIRO S. & WEIR A. (1999). New V, ZF and Abstraction. *Philosophia Mathematica* 7/3. 293-321.

SCHIRN M. (ed). (1998). *The Philosophy of Mathematics Today*. Oxford: Clarendon.

THOMSON J. (ed). (1987). *On Being and Saying: Essays in Honor of Richard Cartwright*. Cambridge Mass.: MIT Press.

WEIR A. (Forthcoming). Neo-Fregeanism: An Embarassment of Riches. *Notre Dame Journal of Formal Logic*.

WRIGHT C. (1983). *Frege's Conception of Numbers as Objects*. Aberdeen University Press.

WRIGHT C. (1997). On the Philosophical Significance of Frege's Theorem. In Heck 1997, 201-244. [Hale & Wright 2001, 272-306].

WRIGHT C. (1998a). Is Hume's Principle Analytic? *Notre Dame Journal of Formal Logic* 40/1. 307-332. [Hale & Wright 2001, 307-32].

WRIGHT C. (1998b). On the (Harmless) Impredicativity of Hume's Principle. In Schirn 1998. 339-68. [Hale & Wright 2001. 229-255].

WRIGHT C. (1998c). Response to Dummett. In Schirn 1998. 389-405. [Hale & Wright 2001. 256-271].

A New Path to the Logicist Construction of Numbers

Pierre Joray

> Any theory can always be applied to infinitely many systems of basic elements. David Hilbert

1. A reductionist programme?

Since Frege, Whitehead and Russell, logicism has been widely described as a programme for the *reduction* of mathematics – centrally arithmetic – to logic. If the goal seems clear and can be summerized by the short claim that arithmetic is nothing but logic, it is nevertheless far from being easy to understand accurately what such a reduction is and what is its significance. Basically, the reduction of a theory to another is a technical notion: a theory S_1 is said to be reducible to a theory S_2, when the axioms of S_1 can be derived in S_2 by means of explicit definitions of the primitive terms of S_1 in the language of S_2. This of course does not mean that S_1 is nothing but S_2, but only that S_1 can be *interpreted* in S_2. However, the significance of such a technical reduction is not inconsiderable. It shows first that if S_2 is consistent, then S_1 is also consistent. It also shows that certain entities we can construct in S_2 can *play the rôle* of the objects of S_1, even if it is not a guarantee that these two groups of objects are simply identical.

The kind of reduction logicists usualy have in mind is something much stronger than this technical kind. In the case of arithmetic, the definition of "number" is supposed to grasp the very notion of number. So the definition must be *materially adequate* (in the sense Tarski uses these words in his papers on the concept of truth). In classical logicism, the definition is thus preceded by a philosophical investigation about the nature of numbers. But as Russell pointed out in 1903, logicist definitions are quite paradoxical: on the one hand they "are nothing but statements of symbolic abbreviations", but on the other hand "in the development of a subject, they always require a very large amount of thought, and often embody some of the greatest achievements of analysis" (1903: 63). Even if theoretically insignificant, introducing only a convenient abbreviation, the definition of the term "number" is supposed to have the value of an *explanation* of what a number is, that is to say the value of a real definition of *number*. Now, in order to show that statements of arithmetic are not synthetic, as Kant claimed, but purely analytical ones, the theory in which the definition is stated must be a purely analytic one. The aim of the reduction was actually for original logicists to provide arithmetic with an epistemic foundation: our mathematical knowledge would be secured if numbers can be shown to be logical entities, the properties of which only depend on basic logical laws.

Nevertheless, this form of logicism was a failure: the authors of the *Principia Mathematica* were forced to enlarge their logical basis with three non-logical axioms and Frege faced with contradiction. The only way for logicism was then the search of the weakest addition to pure logic allowing the reduction of Peano-Dedekind arithmetic (PA), while preserving the epistemic component of the original programme. We know today, first from C. Parsons (1965), but also from C. Wright (1983) and G. Boolos (1987), that Russell's paradox was not the death sentence of the whole of Frege's foundation of mathematics: what is now called Frege's Theorem – the proof that the fundamental laws of arithmetic can be derived from second-order logic through the (explicit) definition of three terms and the addition of a single proper axiom, namely Hume's Principle (HP)[1] – is considered as an extremly interesting result for the philosophy of mathematics. But, C. Wright's and B. Hale's claim that Frege's Theorem is still a form of *logicism* is highly controversial. HP is a *proper* axiom and there is no way to use an argument such as Russell's one concerning his axiom of infinity – i.e. that it can be considered as an hypothesis we must assume when entering the field of mathematics – for HP introduces into the system a (non-logical?) proper term (*the cardinal number of*). Though not purely logical truths, according to Wright, the laws of arithmetic are still shown to be analytic or purely conceptual by Frege's Theorem. The reason being put forward by neo-Fregeans is that HP is (or more precisely, involves) an explanation in logical terms of our concept of (cardinal) number. HP is then considered by these authors as an *implicit definition* of that concept, stated into the language of second-order logic. HP is considered by neo-Fregeans as an analytic truth, for – as they say – when stipulating HP as true[2], the process is meaning-constituting[3]. What I understand is that the addition of HP to a first-order logic axiomatic system provides an implicit definition giving the new proper term a *definite* meaning, the logical analysis of which allows to show that cardinal (and then natural) numbers exist as objects with the attended properties.

But the problems with implicit definitions abound. Their addition – as with any proper axiom – modify the whole system and can even lead to contradiction. Relative to a certain goal, they can be too strong, too weak, or even both. Contrary to Frege's Basic Law V (which is exactly of the same shape), HP is certainly consistent with second-

[1] HP can be formulated as $(\forall F)(\forall G)(\mathrm{N}x{:}Fx = \mathrm{N}x{:}Gx \equiv F \infty G)$, where '$\mathrm{N}x{:}Fx$' expresses "the cardinal number of *F*" and '∞' the relation of equinumerosity i.e. the existence of a one-one correspondance between the objects falling under *F* and those falling under *G*.

[2] I must confess I cannot understand what "stipulating HP *as* true" really means, for I see only two possibilities regarding HP: either it is a proposition or an open formula with 'N(-)' as a free variable. In the former case, 'N(-)' already has a meaning in the language in which HP is considered and then the truth value of HP does not depend at all on our *stipulation*. In the latter case (open formula), it cannot have (or receive) any truth value. The only way I can understand "I stipulate HP as true" is: "let me *consider* 'N(-)' with one of *these* meanings (if any!) which are adequate for HP to be understood as a true proposition".

[3] Cf. for example Ebert & Rossberg in this volume.

order logic. But, as a *definition* of a *single* proper term, it is too strong, for it also modifies the logical constants with which the term to be defined is explained. Due to its impredicative character, HP excludes interpretations in a finite domain of individuals. In other terms it involves an axiom of infinity. From a proof-theoretical point of view, expressions of the form

$$(\exists x_1)(\exists x_2)\ldots(\exists x_n)(x_1 \neq x_2 \wedge x_1 \neq x_3 \wedge \ldots \wedge x_1 \neq x_n \wedge x_2 \neq x_3 \wedge x_2 \neq x_4 \wedge \ldots \wedge x_2 \neq x_n \wedge \ldots \wedge x_{n-1} \neq x_n) \quad \text{[with } n \geq 2\text{]}$$

which are not theorems of logic and do not contain the new term, are logical consequences of the "definition"[4].

On the other hand, HP is also too weak for being a *logicist* definition of cardinal number, for it does not warrant that only one kind of entity fall under the concept. In Hilbertian terms, HP does not warrant the existence of only one "system of objects" satisfying the "definition". The so-called Ceasar problem is a consequence of this weakness. Saying, with neo-Fregeans, that the open sentences of the form "$y = \mathrm{N}x{:}Fx$" are only satisfied by objects falling under the identity conditions expressed by HP is not enough, for we cannot exclude that there is no (non-standard) "system of objects" including Julius Ceasar and satisfying HP however (even if Julius Ceasar clearly does not fall under the identity conditions which follows from *our* intended interpretation of 'N-:-').

At the end, in spite of its elegance and impressive economical character (just a single short and intuitive proper axiom with only one primitive term, the others being introduced by explicit definitions), it

[4] In Łukasiewicz's vocabulary, such a definition is said to be *creative*, for it allows the derivation of expressions which are independant from the axioms and do not include any occurrence of the defined term (Łukasiewicz: 1928). Important and difficult questions remain open concerning the power of creative definitions and the way to fix the limite between definitions and expressions which are too strong to be considered simply as definitions. As an obvious example of the latter case, consider the propositional expression *E*: $(\sim p \supset \sim q) \supset ((\sim p \supset q) \supset p)$. In spite of apparences, *E* cannot be used as an implicit definition of negation in a system which only contains the intuitionist version of conditional. Of course *E explains* the new symbol '~' (negation), but it also strongly modify the meaning of the conditional '⊃', for it allows the derivation of Peirce Law $((p \supset q) \supset p) \supset p$, which only contains '⊃' and is not a theorem of intuitionist logic. On creative definitions, see (Joray: 2005, 2006).

is not obvious whether Frege Arithmetic (FA = second-order logic + HP) is more *logicist* than Peano-Dedekind Arithmetic (PA). After all, Peano's axioms also constitute (taken together) an implicit definition of "zero", "number" and "successor", explaining the related concepts in terms of pure logic. Moreover, the unsolved Ceasar problem shows that FA also invites the criticism Russell opposed to PA:

> We want our numbers to be such as can be used for counting common objects, and this requires that our numbers should have a *definite* meaning, not merely that they should have certain formal properties. (Russell 1921: 10).

Following R. Heck (1997, 2000), I will say that the reduction of a theory to another one – for example PA to FA – does not signify that the reduced theory *itself* is derivable in the other one, but only that the former is *interpretable* in the latter. The very object of an axiomatic mathematical theory is what is called a structure by mathematicians. Of course, every axiomatisation is *guided* by a certain intended or pre-interpretation. For example, Greek geometry was certainly guided by the consideration of the construction of concrete figures[5] and arithmetic by the common practice of counting concrete objects.

Strictly speaking, the very mathematical notion of number cannot be defined from logic or from any other theory, for it would make numbers having properties we are not ready to recognize as arithmetical ones[6]. Mathematicians' natural numbers cannot *be* such things as extensions or other logical objects abstracted from concepts; they cannot *be* classes of classes, or certain kind of ZF sets, neither – as suggested in (Simons: to appear) – properties of multitudes. In all these cases, numbers would have mathematicaly irrelevant properties, expressed by propositions which do not belong to arithmetic. What I

[5] As an example, the implicit presence of such a concrete interpretation becomes apparent since the demonstration of the first proposition of Euclide's *Elements*, where the existence of a point of intersection between two circles can only be infered from the observation of the diagram associated with the reasonning. The relation to common practice is also visible in the first three "demands", the expression of which is obviously inspired by the use of the ruler and the pair of compasses.

[6] So is, for example, in FA, the property of zero expressed by the formula "$(\exists F)(\mathrm{N}x{:}Fx = 0)$", or in ZF: "$(\forall E)(0 \subsetneq E)$".

am ready to call "natural number" in mathematics is actually only one of these abstract and general entities which stricly satisfy all the theorems of arithmetic *and no other*.

In this perspective, what a logicist approach to a mathematical theory can provide is only what I will call a *picture* of this theory, that is to say a specific interpretation in which purely logical objects or constructions can play the rôle of mathematical notions (numbers, in particular). Of course, the very existence of such a *purely logical picture* is far from being pointless for the philosophy of mathematics. As a (technical) reduction, it gives of course a relative proof of consistency. But it especially provides an *objective and conceptual path* to arithmetical knowledge. A logicist picture gives such a secured epistemic justification for it allows to replace by a conceptual construction the intuitive content and naïve notions which, in the development of their practice, lead mathematicians step by step to the axiomatic caracterisation of their theory – in Russell's terms, the picture provides a *logical analysis* of the intuitive notions. The route which is thus constructed is epistemicaly secured, for it consists in propositions the truth of which depends only on logic.

The possibility of reinterpretations is today widely recognised by logicians and mathematicians as an essential advantage of axiomatic theories and Russell's above mentionned criticism was clearly overtaken by further developments of formal sciences. Nonetheless, his requierments – that our numbers can be used for counting common objects and that they have a definite meaning – are perfectly relevant relative to the *picture* of arithmetic logicists want to elaborate. In order to provide the kind of justification I have just described, the picture must be *materialy adequate* – it must present an adequate analysis of the naïve notion of number we use when counting concrete objects. On the other hand, it must also be *definite in meaning*. This requiers the meaning of the defined terms of the construction to be *fully determined* by the logical constants, excluding reinterpretations. For this reason, the use of any *implicit* definition should be prohibited in this conception of logicism.

Frege's requierment that only logical constants occure in his Basic Laws is not followed by neo-Fregeans. FA is undoubtly a very nice theory to which arithmetic can be reduced. Nevertheless, it is neither arithmetic *itself* (as PA is), nor it is a good logicist *picture* of arithmetic, for it does not exclude reinterpretations of the proper term introduced by HP. For the latter condition to be satisfied, only explicit definitions must be used in the construction.

In the following pages, I am going to show that a valuable logicist picture of arithmetic can be constructed from logic using only explicit definitions. This will be done without introducing extensions of concepts or classes – even as incomplete symbols or way of speaking – but on the ground of Stanisław Leśniewki's logical notion of name, which unable to express in the logical language pluralities of things – like plural terms do in natural language.

2. A logic of names

When we assert a numerical statement like "there are five continents", according to Frege, we are ascribing a certain property to the concept *being a continent*. For Russell and Whitehead, it is to the class of continents that the property is ascribed. But which property? Certainely not that of *being five*. Of course, neither the concept, nor the class can simply be said to be five. Before being analysed by means of the logical relation of equinumerosity, the property in question can only be described as *having five objects falling under it* (for the concept), or *being a member of it* (for the class). On the other hand, *being five* is obviously not a property of the objects themselves: the continents *are* five, but none of them *is* five. According to P. Simons (to appear), *being five* is a property of the "multitude" of continents, a notion he says to be akin to Husserl's "Vielheit" or Russell's "class as many". But where is the expected solution? Like with "the concept of continent" or "the class of continents", "the multitude of continents" is obviously a singular term. The ordinary fact that we can use a single word or a single expression in order to refer to several objects seems to be mysterious for logicians as long as we do not postulate the existence of a single intermediate entity which has the (still mysterious?) virtue to gather together the things in question. In this direction, set theory is the most achieved solution and I am sceptical about the possibility to do better with the introduction of an other kind of abstract "multiple" entity for the definition of numbers.

The idea underlying the logical picture of numbers to be presented hereafter is much more unsophisticated. Without trying to *explain* the one-many link between expressions and objects, one just *observes* that ordinary language involves expressions or words which are used to refer sometimes to a single thing (for example, "Cairo" or "the capital of Egypt"), sometimes to several things ("The African capitals", "horses") and also sometimes to none of them ("the capital of Africa", "Ulysses", "round circles"). We all know – at least when speaking – that our words or expressions exist. What we do not know so surely is whether these words or expressions really refer to objects as we expect; pure logic cannot inform us about the existence of these references[7]. In the logical picture, the idea is to interprete numbers as *certain semantic properties of names*. So to say, zero will be depicted as the property of a name to be empty, one as the property of a name to be singular and three as the property of a name to refer to three things.

Primitive functor epsilon

Leśniewski's calculus of names[8] – called "Ontology" – is grounded on such basic observations. It is constructed as an expansion of a quantified propositional calculus – called "Protothetic" – through the addition of a single axiom. This axiom introduces variables for names

[7] Strictly speaking, it's a defect of standard predicate logic that it allows to show there exist at least one object in the universe.

[8] For a systematic presentation of Leśniewski's logic, see Miéville (1984, 2001-04) and the papers in Srzednicki & Rickey (1984).

(I will use here the first small latin letters *a*, *b*, *c*, ...), and a copula: a binary nominal relator called *epsilon*. The axiom is the following[9]:

AxOnto:
$\lfloor ab \rfloor \lceil a\varepsilon b .\equiv. \lfloor \exists c \rfloor \lceil c\varepsilon a \rceil \wedge \lfloor cd \rfloor \lceil (c\varepsilon a \wedge d\varepsilon a) \supset c\varepsilon d \rceil \wedge \lfloor c \rfloor \lceil c\varepsilon a \supset c\varepsilon b \rceil \rceil$

The left hand side of the biconditional of this universal expression '$a\varepsilon b$' is the general form of a singular proposition. It can be read as "*a* is *b*", or more precisely "the only object denoted by '*a*' is also denoted by '*b*'". In so doing, I am considering the right hand side as expressing the truth conditions of the singular proposition in the following way:

'$a\varepsilon b$' is true iff

1. something is denoted by '*a*';
2. '*a*' does not denote more than a single object;
3. what is denoted by '*a*' is also denoted by '*b*'.

In other words, '$a\varepsilon b$' is truly asserted iff '*a*' stands for a singular name (not empty and not plural) and '*b*' for a singular or plural name which denotes (possibly among others) the object denoted by '*a*'.

Among the inference rules, there are of course rules governing the use of quantifiers, which are subject to the standard principles. It has nevertheless to be noticed that the usual objectual or referential interpretation of quantifiers is not adequate. As a name can be singular, plural, but also empty, it is possible to express with a quantifier that there is a name which is empty.

Definition rules

As a very important peculiarity, the logic of names also includes rules for stating explicit definitions of two kinds. Instead of stating definitions in the metalanguage – like in the *Principia*, using the unspecified symbol '$=_{df}$' and introducing only convenient abbreviations – Leśniewski uses his primitive logical constants for expressing the equivalence relation between the *definiendum* (*Dum*)

[9] Where the universal and particular quantifiers are expressed by the respective forms "$\lfloor v \rfloor \lceil$ E $\rceil$" and "$\lfloor \exists v \rfloor \lceil$ E $\rceil$".

and the *definiens* (*Diens*). The first rule allows the introduction of propositional constants or functors, the second one, the introduction of nominal constants or functors. The logical equivalence is thus expressed by one of the two forms:

$\lfloor v_1\ v_2\ v_3 \ldots \rfloor \lceil\ Dum \ \equiv\ Diens\ \rceil$ Def$_S$ (propositional rule)

$\lfloor a\ v_1\ v_2\ v_3 \ldots \rfloor \lceil\ a\varepsilon Dum \ \equiv\ Diens\ \rceil$ Def$_N$ (nominal rule)

where 1. the left and right hand sides of the biconditional involve the same (free) variables; 2. *Diens* is a formula with only primitive or already defined symbols; 3. in the case of Def$_N$, *Diens* must be such that the name '*a*' is expressed to be a singular term[10] and 4. *Dum* is of the following form, where # is the symbol to be defined and no symbol occure more than once:

Dum: $\#\,(v_1\ v_2 \ldots)(\ v_i\ v_{i+1} \ldots) \ldots (\ v_j\ v_{j+1} \ldots\ v_n)$

As we will see below, the general form of *Dum* relates to three possiblities. First, there can be no variable in *Dum*. The defined symbol is then either a constant proposition (with Def$_S$), or a constant name (with Def$_N$), like in the following examples:

D1. $\top \equiv \lfloor p \rfloor \lceil\ p \equiv p \rceil$ Def$_S$ ($\top$: constant *true*)

D2. $\lfloor a \rfloor \lceil\ a\varepsilon\Lambda \equiv\ (a\varepsilon a \wedge \sim (a\varepsilon a))\ \rceil$ Def$_N$ (Λ : empty name)

D3. $\lfloor a \rfloor \lceil\ a\varepsilon \mathrm{V} \equiv\ a\varepsilon a\ \rceil$ Def$_N$ (V : universal name)

In the second case, the variables of *Dum* occure in a single pair of parentheses. The defined symbol is then a functor:

D4. $\lfloor ab \rfloor \lceil\ \doteq\{ab\} \equiv (a\varepsilon b \wedge b\varepsilon a)\ \rceil$ Def$_S$
($\doteq\{ab\}$: *a* is the same object as *b*)

D5. $\lfloor ab \rfloor \lceil\ =\{ab\} \equiv \lfloor c \rfloor \lceil\ c\varepsilon a \equiv c\varepsilon b\ \rceil\ \rceil$ Def$_S$
($=\{ab\}$: *a* and *b* are identical or have the same reference(s))

D6. $\lfloor a \rfloor \lceil\ 0\{a\} \equiv \sim \lfloor \exists b \rfloor \lceil b\varepsilon a \rceil\ \rceil$ Def$_S$ ($0\{a\}$: *a* is empty)

[10] In fact, this inelegant condition becomes superfluous if we impose the nominal definitions to be stated in the following form: $\lfloor a\ v_1\ v_2\ v_3 \ldots \rfloor \lceil\ a\varepsilon Dum \ \equiv\ (a\varepsilon a \wedge Diens)\ \rceil$.

D7. $\lfloor a \rfloor \lceil 1\{a\} \equiv a\varepsilon a \rceil$ Def$_S$ ($1\{a\}$: a is singular)

D8. $\lfloor \varphi\psi \rfloor \lceil \approx[\varphi\psi] \equiv \lfloor a \rfloor \lceil \varphi\{a\} \equiv \psi\{a\} \rceil \rceil$ Def$_S$
($\approx[\varphi\psi]$: φ and ψ are coextensive or satisfied by the same name(s))

D9. $\lfloor abc \rfloor \lceil a\varepsilon(b.c) \equiv (a\varepsilon b \wedge a\varepsilon c) \rceil$ Def$_N$ (. : nom. intersection)

D10. $\lfloor abc \rfloor \lceil a\varepsilon(b+c) \equiv (a\varepsilon b \vee a\varepsilon c) \rceil$ Def$_N$ (+ : nominal union)

D11. $\lfloor abc \rfloor \lceil a\varepsilon(b-c) \equiv (a\varepsilon b \wedge \sim(a\varepsilon c)) \rceil$ Def$_N$ (– : nom. complement)

In the last case, the variables of *Dum* split up into more than one pair of parentheses. The symbol to be defined is thus a multi-link or parametric functor, i.e. a functor forming functor:

D12. $\lfloor ab \rfloor \lceil =\langle a \rangle \{b\} \equiv =\{ab\} \rceil$ Def$_S$
(parametric nominal identity; $=\langle a \rangle$: denoting like a)

D13. $\lfloor ab \rfloor \lceil \varepsilon\langle b \rangle \{a\} \equiv a\varepsilon b \rceil$ Def$_S$
(parametric *epsilon*; $\varepsilon\langle b \rangle$: being one of the b's)

I will not go here into the proofs of the formal properties of the above introduced symbols. Let me just underline the aspects of the definition rules which are central for the understanding of the definition of numbers in the next section.

First, it has to be noticed that the definition rules allow to introduce symbols of categories which are not previously available in the language. This is particulary obvious with the introduction of multi-link or parametric functors. In D12, for example, the parametric nominal identity is introduced on the basis of the usual identity binary relation. According to D5, '$=\{ab\}$' means "the names 'a' and 'b' denote the same things (if any)". D12 introduces an other linguistic possibility to express the same content: first the symbol for the parametric identity is applied to 'a' and the result '$=\langle a \rangle$' expresses the nominal property "denoting-(exactly)-the-a's"; this property can be applied to a name 'b', obtaining thus '$=\langle a \rangle\{b\}$' which expresses that 'b' denotes (exactly) the a's. This is very akin to a λ-abstraction and '$=\langle a \rangle$' is in Leśniewski's language what in λ-notation would be expressed by '$\lambda b.[=\{ab\}]$'.

Secondly, Leśniewski's system is such that the definition of a symbol of a new category allows the use of variables of that category and also the binding of these variables by quantifiers. See for example the use of the bind nominal-property-variables φ and ψ in D8, which depends on the introduction of the nominal-property-category in D6 (definition of '0{-}', the nominal property of emptiness).

This power of definition rules makes Ontology a strong analytical tool, but it has important consequences that I cannot present here in detail. As constants of any semantic category can be defined step by step, the system cannot be said to be of a determined order. Only specific definitional developpements of the axiomatic basis can be said to be of such or such order. On the other hand, it is clear that the formation of expressions cannot be specified as usual, by a previously defined set of well formed formulae. One of Leśniewski's main achievments, in the field of formal languages, was his ability to elaborate completly explicit semantic and syntactic constraints in order to impose extensionality at each level and to avoid ambiguity in the potentially infinite process of definition[11]. This paper cannot present and even make use of this full apparatus. Local conventions like differences in the kind of letters and the use of different sorts of parentheses are sufficient for its purpose.

4. The definition of numbers[12]

In the present logical construction, natural numbers are going to be depicted as cardinal properties of finite names (names which denote only a finite quantity of objects). Before going into the definition of the general notion of natural number, let me consider how any particular natural number can be defined. Zero and one have already been introduced by definitions D6 and D7:

[11] More on this issue in Gessler's paper in this volume p. 68*ff* and, for a full presentation, see Miéville (1984 or 2001-04).

[12] For the full presentation of the following logicist construction, with proofs and technical details, see Gessler, Joray, Degrange (2005: 73-137). The construction is partially inspired from Canty (1967).

D6. $\lfloor a \rfloor \lceil 0\{a\} \equiv \sim \lfloor \exists b \rfloor \lceil b \varepsilon a \rceil \rceil$ $\qquad$ Def_S (zero)

D7. $\lfloor a \rfloor \lceil 1\{a\} \equiv a \varepsilon a \rceil$ $\qquad$ Def_S (one)

Now, two and three can be defined in the following way:

D14. $\lfloor a \rfloor \lceil 2\{a\} \equiv \lfloor \exists b \rfloor \lceil b \varepsilon a \wedge 1\{a - b\} \rceil \rceil$ $\qquad$ Def_S (two)

D15. $\lfloor a \rfloor \lceil 3\{a\} \equiv \lfloor \exists b \rfloor \lceil b \varepsilon a \wedge 2\{a - b\} \rceil \rceil$ $\qquad$ Def_S (three)

The idea is very simple: in order to define the successor n' of a previously defined natural number n, one have to state that a name a has the number n' iff a name which denotes exactly the a's exepted one of them has the number n. This gives rise to the general definition of the *successor* of a nominal property:

D16. $\lfloor \varphi\, a \rfloor \lceil \mathrm{S}\backslash \varphi / \{a\} \equiv \lfloor \exists b \rfloor \lceil b \varepsilon a \wedge \varphi\{a - b\} \rceil \rceil$ $\qquad$ Def_S (successor)

From D16, it is obvious that a symbol $\underline{n}$ for any natural number $n > 0$ can be introduced with a definition of the following form, where '$\mathrm{S}\backslash - /$' is iterarted n times:

$\lfloor a \rfloor \lceil \underline{n}\{a\} \equiv \mathrm{S}\backslash\, \mathrm{S}\backslash \ldots \mathrm{S}\backslash 0 / \ldots // \{a\} \rceil$ $\qquad$ Def_S

From these definitions of particular numbers, the point is now to generalize. As in other logicist programmes this is done with the relation of equinumerosity. This relation obtains between two names when their references are in a one-one correspondance. More precisely, we need first the definitions of one-one relations, and of the domain and counter-domain of a relation:

D17. $\lfloor R \rfloor \lceil \mathrm{OneOne}[R] \equiv$
$\lfloor abc \rfloor \lceil ((R\{ac\} \wedge R\{bc\}) \vee (R\{ca\} \wedge R\{cb\})) \supset \doteq\{ab\} \rceil \rceil$ Def_S

D18. $\lfloor aR \rfloor \lceil a\ \varepsilon\ \mathrm{Dom}\langle R \rangle \equiv (a \varepsilon a \wedge \lfloor \exists b \rfloor \lceil R\{ab\} \rceil) \rceil$ $\qquad$ Def_N

D19. $\lfloor aR \rfloor \lceil a\ \varepsilon\ \mathrm{Cdom}\langle R \rangle \equiv (a \varepsilon a \wedge \lfloor \exists b \rfloor \lceil R\{ba\} \rceil) \rceil$ $\qquad$ Def_N

Notice that the use of '$\doteq\{ab\}$' (a and b denote the same object) instead of '$=\{ab\}$' (a and b have the same references) at the end of D17 restricts the notion of one-one relation to relations which obtain only between *singular* names. Then a one-one relation expresses always a correspondance between objects[13]. Now comes the definition of the nominal relation of *equinumerosity*:

$$\text{D20.}\ \lfloor ab \rfloor \lceil a \infty b \equiv \lfloor \exists R \rfloor \lceil (\text{OneOne}[R] \wedge \text{Dom}\langle R \rangle = a \wedge \text{Cdom}\langle R \rangle = b \rceil \rceil \quad \text{Def}_S$$

The cardinality of a name is thus very simply expressed by the introduction of the parametric version of '∞':

$$\text{D21.}\ \lfloor ab \rfloor \lceil \infty\langle a \rangle \{b\} \equiv a \infty b \rceil \qquad \text{Def}_S$$

By the abstraction of '*b*' in '*a* ∞ *b*', one obtains the complex functor '$\infty\langle a \rangle$', which expresses the nominal property "denoting as many objects as *a*" or "having the cardinality of *a*". '$\infty\langle - \rangle$' is then a multi-link functor which gives the cardinal property of the name to which it is applied. As numbers in this construction are properties of names, it is natural to read '$\infty\langle a \rangle$' as "the cardinal number of *a*"[14] and the following theorem, which is easy to derive from D21, as the Leśniewskian version of Hume's Principle:

$$\lfloor ab \rfloor \lceil \infty\langle a \rangle \approx \infty\langle b \rangle \ \equiv\ a \infty b \rceil$$

Contrary to the Fregean version, the left hand side does not express an identity between singular names, but an identity between nominal functors. Leśniewskian versions of Fregean "abstraction principles" are strictly predicative – the value of the arguments of the identity sign in the left hand side is not among the possible values of the variables in the right hand side. This has important consequences on the present construction. First, Leśniewskian versions never lead to contradiction. In particular, the analogue of Frege's Basic Law V is perfectly harmless and can be easily infered from D12:

$$\lfloor ab \rfloor \lceil =\langle a \rangle \approx =\langle b \rangle \ \equiv\ a = b \rceil$$

13 Of course a relation can also obtain between plural and even empty names, but when it only obtains between singular names, it corresponds to a relation between the objects denoted by the names in question. On this issue, very specific to Ontology, see Joray (1999: 187-190).

14 This is of course only a *façon de parler*, for '$\infty\langle a \rangle$' is not the name of an object, but a symbol for a function. In natural languages, nominalization is a very usefull way to state dependant or incomplete meanings as objects of the discourse. In the present context, it remains harmless as long as it does not come with a reification of the dependant meaning. On this issue, see N. Gessler's paper in this volume.

Secondly, the fact that abstraction's results are not designated as objects preserves the ontological neutrality of logic. Theorems of Leśniewski's calculus are logicaly true in the sense they are true in all domains, included the empty one. As we will see further, a consequence of this is that there will be no way to avoid the addition of an axiom of infinity for the derivation of all Peano's propositions.

From D21, the general definition of *cardinal number* can be stated as:

D22. $\lfloor \varphi \rfloor \lceil \mathrm{Cn}[\varphi] \equiv \lfloor \exists a \rfloor \lceil \infty\langle a \rangle \approx \varphi \rceil \rceil$ $\quad \mathrm{Def}_S$

Now, in order to specify which cardinal numbers are natural numbers the definition of finite names is required. Like in Frege's *Grundlagen*, this will be done using the notion of inductivity: a name is said to be *finite* or *inductive* if it has all the properties of the empty name that are preserved by the addition of a single denotation:

D23. $\lfloor a \rfloor \lceil \mathrm{Ind}\{a\} \equiv$
$\lfloor \varphi \rfloor \lceil (\varphi\{\Lambda\} \wedge \lfloor bc \rfloor \lceil (\varphi\{c\} \wedge 1\{b\}) \supset \varphi\{c+b\} \rceil) \supset \varphi\{a\} \rceil \rceil$

From this, *natural numbers* can be caracterised as the cardinal numbers of finite names:

D24. $\lfloor \varphi \rfloor \lceil \mathrm{Nn}[\varphi] \equiv (\mathrm{Cn}[\varphi] \wedge \lfloor a \rfloor \lceil \varphi\{a\} \supset \mathrm{Ind}\{a\} \rceil) \rceil$ $\quad \mathrm{Def}_S$

D6, D16 and D24 are the respective definitions in Leśniewski's Ontology of Peano's primitive terms *zero*, *successor* and *number*. It has been demonstrated that Peano's propositions I, IV and V are derivable from these definitions in pure Ontology

P_{I} $\quad \mathrm{Nn}[0]$
(zero is a number)

P_{IV} $\quad \lfloor \varphi \rfloor \lceil \mathrm{Nn}[\varphi] \supset \mathrm{S}\backslash\varphi/ \neq 0 \rceil$
(zero is not the successor of a number)

P_{V} $\quad \lfloor P \rfloor \lceil (P[0] \wedge \lfloor \varphi \rfloor \lceil (\mathrm{Nn}[\varphi] \wedge P[\varphi]) \supset P[\mathrm{S}\backslash\varphi/] \rceil) \supset$
$\lfloor \psi \rfloor \lceil \mathrm{Nn}[\psi] \supset P[\psi] \rceil \rceil$
(mathematical induction)

the remaining two propositions being derivable in infinite Ontology:

P_{II} $\lfloor\varphi\rfloor \lceil \mathrm{Nn}[\varphi] \supset \mathrm{Nn}[S\backslash\varphi/] \rceil$
(the successor of a number is a number)

P_{III} $\lfloor\varphi\psi\rfloor \lceil (\mathrm{Nn}[\varphi] \wedge \mathrm{Nn}[\psi]) \supset (S\backslash\varphi/ = S\backslash\psi/ \supset \varphi = \psi) \rceil$
(different numbers have different successors)

It would be inappropriate, in such a presentation, to give the proofs which are long and have already been published with all the technical details in (Gessler, Joray, Degrange 2005: 75-137).

In a way quite similar to what is done in the *Principia Mathematica*, it is now possible to explicitly define addition and multiplication[15]. A full picture of Peano Arithmetic is thus constructed in a third-order development of infinite Ontology: a system of pure logic with the addition of an axiom of infinity[16].

Just notice that the dependance of Peano propositions vis-à-vis the single non-logical axiom is not exactly like in the *Principia*, for not only P_{III}, but also P_{II} (the successor of a number is a number) requires the existence of infinitely many objects. This is due to the fact that P_{II} cannot be read here as "ambiguous as to type", avoiding the very artificial meaning of P_{II} in the *Principia*: *for every number* n, *there is a type* t *in which the successor of* n *(in fact the analogue of* n *for* t*) is a number*.

As it has been shown by Nadine Gessler[17], type (or categorial) ambiguity is not needed to warrant the unity of all the higher-degree arithmetics which can be developed in Ontology. Anyhow, since what is to be constructed is not arithmetic *itself*, but a logical *picture* of it – an interpretation of general arithmetic in a system of certain definite

[15] Cf. Joray (2002).

[16] As Dedekind's finitude and inductivity are only equivalent with the principle of choice, the axiom of infinity in the 2005 publication (using Dedekind's notion) is too strong and can be replaced by a formula expressing there exists a name which is not inductive:
$\lfloor\exists a\rfloor \lceil \sim \mathrm{Ind}\{a\}\rceil$
which is an abbreviation of the official axiom which must be stated without any defined term:
$\lfloor\exists\varphi\rfloor \lceil \lfloor a\rfloor \lceil \sim \lfloor\exists b\rfloor \lceil b\varepsilon a\rceil \supset \varphi\{a\}\rceil \wedge \lfloor abc\rfloor \lceil (a\varepsilon a \wedge \varphi\{b\} \wedge \lfloor d\rfloor \lceil d\varepsilon c \equiv (d\varepsilon b \vee d\varepsilon a)\rceil) \supset \varphi\{c\}\rceil \wedge$
$\sim \lfloor a\rfloor \lceil \varphi\{a\}\rceil \rceil$.

[17] Cf. Gessler, Joray, Degrange (2005: 9-36).

logical entities – the classical question of the unity of the type hierarchy of arithmetics becomes almost superfluous.

5. Conclusion

Even if the above presented construction constitutes an interpretation of Peano's general arithmetic and a way to reduce it to Leśniewski's logical system, the mathematical theory remains independant from logic. Such an approach does not provide any argument for the claim that arithmetic would be a part of logic or for an answer to ontological questions about the nature or the essence of numbers.

It is nevertheless a logicist approach for it shows the possibility to reach arithmetical knowledge in the realm of logical entities and logical laws. Each natural number is depicted through the explicit definition of a purely logical constant. On the other hand, Peano's propositions can be obtained under the assumption that the universe is not finite. This is not arithmetic itself, which is more general, but a picture of it, where arithmetic is applied to definite logical constants. But the definition of these constants is not ad hoc, for it provides a logical analysis of the naïve notions involved in the act of counting concrete objects.

Neither in common counting, nor in any application of pure arithmetic, the assumption that there will always be enough available objects for the successor of a given number to be different from the number in question implies any ontological commitment concerning the nature of the real world. The axiom of infinity is not an empirical statement concerning the world, but an hypothesis specifying the kind of idealization through which we apply arithmetic to specific concrete situations.

The given logical picture does not inform us about the ontology of abstract numbers. Neither it explains in which sense arithmetical sentences can be said to be true. Nevertheless, providing a conceptual content which guides us to Peano's axioms, it gives an analytic justification for the adoption of these axioms as forming the basis for the coherent and applicable theory of pure mathematics we know.

References

BOOLOS G. (1987). The consistency of Forage's *Foundations of Arithmetic*. In Thomson J. (ed.). *On Being and Saying: Essays in Honor of Richard Cartwright*. MIT Press. 3-20. [reprinted in Boolos 1998: 183-201].

BOOLOS G. (1998). *Logic, Logic and Logic*. Cambridge (Mass.): Harvard Univ. Press.

CANTY J. T. (1967). *Leśniewski's Ontology and Gödel Incompleteness Theorem*. PhD. Thesis. Univ. of Notre Dame.

FREGE G. (1884). *Die Grundlagen der Arithmetik*. Breslau: Koebner.

FREGE G. (1893). *Grundgesetze der Arithmetik*. Jena: Pohle Verlag.

GESSLER N., JORAY P., DEGRANGE C. (2005). *Le logicisme catégoriel. Travaux de logique* 16. Université de Neuchâtel.

HALE B., WRIGHT C. (2001). *The Reason's Proper Study. Essays towards a Neo-Fregean Philosophy of Mathematics*. Oxford: Clarendon.

HECK R. G. (1997). Finitude and Hume's Principle. *Journal of Philosophical Logic* 26. 589-617.

HECK R. G. (2000). Cardinality, Counting and Equinumerosity. *Notre Dame Journal of Formal Logic* 41.3. 187-209.

JORAY P. (1999). *La subordination logique. Une étude du nom complexe dans l'Ontologie de S. Leśniewski*. Bern: Peter Lang.

JORAY P. (2002). Logicism in Leśniewski's Ontology. *Logica Trianguli* (Łódź, Nantes, Santiago de Compostella) 6. 3-20.

JORAY P. (2005). Should Definitions be Internal? In Bilkova M., Behounek L. (eds). *The Logica Yearbook 2004*. Praha: Filosofia. 189-199.

JORAY P. (2006). La définition dans les systèmes logiques de Łukasiewicz, Leśniewski et Tarski. In Pouivet R., Rebuschi M. (éds). *La philosophie en Pologne 1918-1939*. Paris: Vrin. 203-222.

LEŚNIEWSKI S. (1992). *Collected Works* (2 vol.). Surma S. J., Srzednicki J. T., Barnett D. I. (eds). Warszawa: PWN / Dordrecht: Kluwer.

ŁUKASIEWICZ J. (1928). O definicyach w teoryi dedukcyi (On definitions in deductive theories) & Rola definicyjn systemach dedukcyjnych (The rôle of definitions in deductive systems). *Ruch Filozoficzny* 11. 164 and 177-178. [French transl. by Błaszczyk M. in Joray 2006].

MIÉVILLE D. (1984). *Un développement des systèmes logiques de Stanisław Leśniewski. Protothétique, Ontologie, Méréologie.* Berne: Peter Lang.

MIÉVILLE D. (2001-04). *Introduction à l'œuvre logique de S. Leśniewski. I. La Protothétique, II. L'Ontologie. Travaux de logique du CdRS.* Neuchâtel: Université.

PARSONS C. (1965). Frege's Theory of Number. In Black M. (ed). *Philosophy in America.* New York: Cornell Univ. Press. 180-203.

RUSSELL B. (1903). *The Principles of Mathematics.* London: Allen & Unwin.

RUSSELL B. (1919). Introduction to Mathematical Philosophy. London: Allen & Unwin. [cited in 1971 ed. New York: Simon and Shuster].

SIMONS P. M. (to appear). What Numbers Really Are. In Auxier R. E. (ed). *The Philosophy of Michael Dummett.* La Salle: Open Court.

SRZEDNICKI J. T. J., RICKEY V. F. (eds). (1984). *Leśniewski's Systems: Ontology and Mereology.* Boston, The Hague: Nijhoff / Wrocław: Ossolineum.

WHITEHEAD A. N., RUSSELL B. (1927). *Principia Mathematica.* 2nd ed. Cambridge Univ. Press. [1st ed. 1910].

WRIGHT C. (1983). *Frege's Conception of Numbers as Objects.* Aberdeen Univ. Press.

Interpreting Arithmetic: Russell on Applicability and Wittgenstein on Surveyability

Mathieu Marion

> What Russell and Frege do is to make connexions between English and German words "all", "or", "and", etc. and numerical statements. This clears up a few points. But that we should actually then say, "3,000,000 + 4,000,000 = 7,000,000", does not follow from this.
>
> *Ludwig Wittgenstein*

Our perceptual powers are limited. Indeed, most people can't tell at a glance, without counting, if there are 15 or 16 strokes in this figure:

||||||||||||||||

This fact accounts for the various numerical notations introduced throughout history, as even stroke notations never go further than 3 or 4 (for example in Roman notation, I, II, III, with IV eventually replacing IIII). Arguments that rely on this fact, I shall dub 'surveyability' arguments. Such arguments are generally considered irrelevant to foundations of mathematics. They are on occasion deployed, because of the appeal to human epistemic limitations, in order to argue for 'strict finitism', a foundational standpoint which is usually considered as extremely narrow and unable to issue in an account of any substantial part of mathematics. One finds 'surveyability' arguments in Wittgenstein's ill-understood *Remarks on the*

Foundations of Mathematics (Wittgenstein 1983) and, unsurprisingly, Wittgenstein's philosophy of mathematics has been described as a form of 'strict finitism'. In this paper, I shall not be concerned at all with these issues. I shall only make two claims: first, that 'surveyability' arguments have *bona fide* uses that have nothing to do with such 'foundational' issues and, secondly, that Wittgenstein's use of 'surveyability' against a particular pair of claims by Russell – see (6) and (7) below – should be counted among those *bona fide* uses. It thus undermines two theses that were originally perceived as part and parcel of logicism.

*

That there are *bona fide* uses of 'surveyability' arguments can only be argued by showing one such use and one can find it in Alan Turing's celebrated 1936 paper 'On Computable Numbers, with an Application to the *Entscheidungsproblem*' (Turing 1936), in which he provided a negative answer to the decision problem for the predicate calculus, one of the great limitation results of the 1930s. In order to see where the 'surveyability' argument is invoked and why it has importance, I must recall some basic steps leading to his proof.[1] The decision problem was to find or to show that one cannot find an 'effective' method by which one can decide, for any formula expressible in the predicate calculus, if it is provable or not in it. The problematic notion in this formulation of the problem is that of 'effective method', as it is not 'formal'. Therefore, the first step towards a solution of the problem is to replace it by a formal notion that can then be used to formulate a mathematical proof. So Turing's strategy consisted first in providing in its stead a formal predicate, which he called simply 'computable'; nowadays, we speak of 'computability by a Turing machine'. This is known as Turing's Thesis (TT), which is not Church's Thesis but close to it: 'every effective method for computing the values of a number-theoretic function can be computed by a Turing machine'.

[1] My account owes a lot to careful studies of Turing's 1936 paper by Robin Gandy and Wilfried Sieg (Gandy 1988) (Sieg 1994, 2001).

What Turing showed was not a formal equivalence between the two notions. Indeed, since the notion of 'effective' method is not formal, its identification with a formal predicate such as 'computable by a Turing machine' cannot rely on formal arguments. Incidentally, this shows the non-eliminability of philosophical arguments in these matters, a fact recognized by Turing:

> All arguments which can be given are bound to be, fundamentally, appeals to intuition, and for this reason rather unsatisfactory mathematically. (Turing 1936, 249)

The surveyability argument occurs at this stage, when Turing sets up his predicate 'computable' simply by describing what he called 'computing machines' (Turing 1936, 231sq.), i.e., Turing machines. One should note that, since our modern digital computing machines did not exist in 1936, Turing could not have referred to them when using the word 'computer'; in fact he clearly referred to a human being, albeit an idealized one. In order to avoid confusion, I shall use the term 'computor' to refer to the human being and retain 'computer' for machines.[2] So Turing's 'computer' is in fact an idealized human being, a 'computor'.[3]

Turing provides three (types of) arguments in support of TT but only the first one is of interest to us (Turing 1936, 249-252). Here, Turing analyses what happens when a computor applies an 'effective method', breaking down the steps in their most elementary form, in order to arrive at " 'simple operations' which are so elementary that it is not easy to imagine them further divided" (Turing 1936, 250). In his analysis, Turing spoke of a (finite) set of 'states of mind' of the computor (working on a tape divided in squares), on which he imposed (strict) finiteness conditions, on the number of symbols and squares observed at any one moment. The 'simple operations' of the computor are (roughly defined) as follows: the computor can change at most one symbol at a time in an observed square; it can move to another square

[2] In this, I merely follow a convention initiated in (Gandy 1988).

[3] Incidentally, when Wittgenstein wrote that Turing machines are "humans who calculate" (Wittgenstein 1980, § 1096), he was thus right.

but only within a bounded distance. Furthermore, any 'simple operation' depends only on the current state of mind of the computor and the symbol observed in the square, and Turing further required that from any given state and observed symbol, there is at most one operation that can be performed. Once these 'simple operations' of the computor are clearly defined, Turing could carry on with his strategy to solve the *Entscheidungsproblem* by arguing that one could construct a computer (a Turing machine) "to do the work of this comput[o]r" (Turing 1936, 215). In particular, this is done by identifying the 'states of mind' of the computor with 'configurations' of the computer.

It is in one of the above steps, i.e., when imposing finiteness conditions, that Turing uses a 'surveyability' argument concerning the number of symbols printed on the squares of the tape:

> The differences from our point of view between the single and compound symbols is that the compound symbols, if they are too lengthy, cannot be observed at one glance. This is in accordance with experience. We cannot tell at a glance whether 9999999999999999 and 999999999999999 are the same. [...] We may suppose that there is a bound *B* to the number of symbols or squares which the computer can observe at one moment. If he wishes to observe more, he must use successive observations. We will also suppose that the number of states of mind which need be taken into account is finite. The reasons for this are of the same character as those which restrict the number of symbols. If we admitted an infinity of states of mind, some of them will be 'arbitrarily close' and will be confused. (Turing 1936, 250)

Now, I have never heard of any rejection of Turing's analyses or of this use of a 'surveyability' argument, *et pour cause*... One should note that the point Turing is making has nothing to do with arguing for 'strict finitism' or some such thing. And this was the point of telling my story: to see that 'surveyability' arguments can have *bona fide* use without presupposing some presumably unpalatable views about mathematics.

*

As I pointed out, one finds uses of 'surveyability' arguments in Wittgenstein, e.g., in Part III of the posthumously published *Remarks on the Foundations of Mathematics*. Here is one example:

> ||||||||||||||||||||||||||| ||||||||||||||||
>
> Is this pattern a proof of 27 + 16 = 43, because one reaches '27' if one counts the strokes on the left-hand side, '16' on the right-hand side, and '43' when one counts the whole row?
> Where is the queerness of calling the pattern the proof of this proposition? It lies in the kind of way this proof is to be reproduced or known again; in its not having any characteristic visual shape? (Wittgenstein 1983, III, § 11)

Embedded here is a 'surveyability' argument, since Wittgenstein uses the fact that one cannot tell the equivalence of

(S) ||||||||||||||||||||||||||| + |||||||||||||||| = |||||||||||||||||||||||||||||||||||||||||||

and

(A) 27 + 16 = 43

merely by looking; one cannot even tell what the result in (S) is without counting the strokes.

Part III of the *Remarks on the Foundations of Mathematics* was written in 1939-40, roughly a year after Wittgenstein met Turing but one finds 'surveyability' arguments invoked as early as 1929, i.e., the earliest surviving material dating from Wittgenstein return to philosophy. There is thus no reason to think here of an influence of Turing on Wittgenstein or vice-versa. Incidentally, a 'surveyability' argument occurs during Wittgenstein's 1939 *Lectures on the Foundations of Mathematics* that were attended by Turing (Wittgenstein 1976, 258-259); I shall come back to it.

So 'surveyability' arguments are ubiquitous in Wittgenstein's writings and one should beware of lumping them all together, as well as one should try and avoid too simplistic an account of Wittgenstein's multi-faceted discussion of these issues, e.g., under the heading 'strict finitism'. For example, the occurrence in 1929 is used against the claim, usually but falsely attributed to his own *Tractatus Logico-Philosophicus*, that arithmetical propositions are tautologies:

> The correctness of an arithmetical proposition is never expressed by a proposition's being a tautology. In the Russellian way of expressing it, the proposition 3 + 4 = 7 for example can be represented in the following manner:
>
> $$(E3x)\varphi x\ .\ (E4x)\psi x\ .\ \sim(Ex)\varphi x\ .\ \psi x :\rightarrow: (E7x).\ \varphi x \vee \psi x$$
>
> Now one might think that the proof of this equation consisted in this: that the proposition written down was a tautology. But in order to be able to write down this proposition, I have to *know* that 3 + 4 = 7. The whole tautology is an application and not a proof of arithmetic. (Wittgenstein 1979, 35)

Note here that the argument deployed is slightly more complicated. It is implied here that the Russellian equivalent of 27 + 16 = 43 is the following formula, involving numerical quantifiers:

$$\text{(R)}\quad [\exists!_{27}x\,(Gx) \wedge \exists!_{16}x\,(Hx) \wedge \forall x \neg(Gx \wedge Hx)\,] \rightarrow \exists!_{43}x\,(Gx \vee Hx)$$

Wittgenstein rejects here the foundational principle that '(A) because of (R)' because the only way to know that (R) is precisely by applying (A), i.e., counting the number of variables in the unabbreviated version of (R), i.e., in something such as (S) which is poorly hidden by the mechanism of numerical quantifiers; here one may run a 'surveyability' argument. As Hao Wang put it, we are able to see that (R) is a theorem of logic

> [...] only because we are able to see that a corresponding arithmetic proposition is true, not the other way round. (Wang 1961, 335)

How this use of a 'surveyability' argument is to work against the claim that arithmetical propositions are tautologies is well worth further investigation but I would like to focus here on one particular use of this argument, against a peculiar pair of theses, (6)-(7) below, first propounded by Russell. The attack against Russell occurs in this passage:

> The *application* of the calculation must take care of itself. And that is what is correct about 'formalism'.
> The reduction of arithmetic to symbolic logic is supposed to shew the point of application of arithmetic, as it were the attachment by means of which it is plugged in to its application. As if someone were shewn, first a trumpet without a mouthpiece – and then the mouthpiece, which shows how a trumpet is used, brought into contact with the human body. But the attachment which Russell gives us is on the one hand too narrow, on the other hand too wide; too general and too special. The calculation takes care of its own application.
> (Wittgenstein 1983 III, § 4)

I shall first explain what is the thesis that Wittgenstein attacked and then argue that Wittgenstein's use of a 'surveyability' argument should be listed among the *bona fide* cases.

*

If one is to ask about prospects for logicism today, one must ask first what is it that one wishes to revive or, at least, ponder about. My claim will be that Wittgenstein's argument demolishes for good one part of logicism, namely Russell's theses that I have alluded too. Of course, it may be that these theses were farfetched to begin with or that the logicist project remains essentially untouched as a result of this particular critique. But these are questions into which I shall not enter here; one cannot deal with every aspect of this question within the narrow compass of a single paper.

The heart of logicism is the following theses:

(1) The concepts of arithmetic can be derived from logical concepts through explicit definitions.

(2) The theorems of arithmetic can be derived from logical axioms through purely logical deduction.

(I limit myself, for the sake of simplicity, to the arithmetical case.) These can be found in, e.g., Carnap's contribution to the Königsberg symposium on the foundations of mathematics (1931), from which they were lifted (Carnap 1983, 41). Taken together, they amount to the following:

> (3) There exists some formal system of logic such that arithmetic can be generated from it.

It is not immediately clear what would be gained from a vindication of (3). Frege spoke of 'fruitful definitions' (Frege 1980, § 88) but it is debatable to which extent (3) is *arithmetically* 'fruitful'. One could also draw like Frege some inferences about the nature of arithmetical propositions, e.g., that there are not 'synthetic a priori', etc. But these are purely philosophical conclusions. Russell spoke instead of the 'meaning and justification' of arithmetic (Russell 1919, 194), i.e., of providing foundations or providing grounds for the acceptance of arithmetic. As a matter of fact, when Wittgenstein presented in his 1939 lectures a 'surveyability' argument in order to deny the idea that arithmetic is based on logic, Turing picked it up and, in the course of the discussion, made exactly that point:

> *Turing*: Russell's definitions show us the *point* of having these ideas of addition and finite cardinals and so on.
> *Wittgenstein*: Yes – and it is just that that I want to deny. (Wittgenstein 1976, 262)

For that reason, logicism cannot be limited to (3). It must also have, at least in Russell's mind, an 'epistemological' side, which can be given by the following pair of theses, as formulated by Mark Steiner in *Mathematical Knowledge* (Steiner 1975, 25):

> (4) It is sufficient to understand proofs written in this system in order to know all the truths of arithmetic that we know.
>
> (5) It is possible for us actually to come to know arithmetical truths in the way suggested in (4) by constructing logical proofs of them.

In his book, Steiner discusses Wittgenstein's use of a 'surveyability' argument, inasmuch as it can be brought to bear upon these theses; I shall not go over the points he raised.[4] I would like instead to point out that this is not the end of the story. Russell framed another pair of claims in the process of arguing for logicism:

(6) Interpretation within the logical system, in the way suggested by (1), provide a definite meaning to the basic arithmetical concepts.

(7) This interpretation allows for the applications of arithmetic.

These claims are seldom discussed. I shall first explain what they amount to in Russell's mind and then, after some brief remarks about their historical importance, proceed to show how Wittgenstein's use of 'surveyability' undermines them.

After listing Peano's axioms, in the first chapter of *Introduction to Mathematical Philosophy*, Russell pointed out that the primitive concepts they contain, '0', 'number', and 'successor' are "capable of an infinite number of different interpretations, all of which will satisfy the five primitive propositions" (Russell 1919, 7). Given one such interpretation, one obtains a series which is called by Russell a "progression" and which will, like the series of natural numbers, satisfy these axioms:

> In fact, given any series
>
> $x_0, x_1, x_2, x_3, \ldots, x_n, \ldots$
>
> which is endless, contains no repetitions, has a beginning, and has no terms that cannot be reached from the beginning in a finite number of steps, we have a set of terms verifying Peano's axioms.
> (Russell 1919, 7-8)

There are indeed an infinity of such progressions, since it suffices for example to pick any natural number other than 0 as the beginning, e.g., 99, to form one. Now, Russell's argument is that "there is nothing" in Peano Arithmetic "to enable us to distinguish between [...] different interpretations of his primitive ideas" but

> This point, that "0" and "number" and "successor" cannot be defined by means of Peano's five axioms, but must be independently understood, is important. We want our numbers not merely to verify

[4] See (Steiner 1975, 41-54) and (Marion 1998, 228-236).

> mathematical formulae, but to apply in the right way to common objects. We want to have ten fingers and two eyes and one nose. A system in which "1" meant 100, and "2" meant "101", and so on, might be all right for pure mathematics, but would not suit daily life. We want "0" and "number" and "successor" to have meanings which will give us the right allowance of fingers and eyes and nose. We have already some knowledge (though not sufficiently articulate or analytic) of what we mean by "1" and "2" and so on, and our use of numbers in arithmetic must conform to this knowledge.
> (Russell 1919, 9)

A little further, this point is reiterated:

> (...) we want our numbers to be such as can be used for counting common objects, and this requires that our numbers should have a *definite* meaning, not merely that they should have certain formal properties. This definite meaning is defined by the logical theory of arithmetic. (Russell 1919, 10)

These are the passages where Russell expressed (6) and (7). They indicate clearly the very purpose of the system of *Principia Mathematica*: it was set up as an *interpretation* of Peano Arithmetic which, in Turing's terms, shows us "the *point* of having these ideas of addition and finite cardinals and so on" or, more precisely, to provide a definite meaning to its primitive terms, such that one could recover ordinary applications of arithmetic. And it is at these views of Russell that Wittgenstein takes aim in the passage from Part III, § 4 of the *Remarks on the Foundations of Mathematics* that I quoted earlier.

*

Before moving on to Wittgenstein, I would like to point out, with some brief historical remarks about Frege and Carnap, that applicability is a recurring theme within logicism: logicists saw it as a proof of the superiority of their approach that it accounts for applications, while other approaches – especially formalism – fail. It is well-known, for example, that Frege claimed in Part III of his *Grundegesetze der Arithmetik* that "it is applicability alone which elevates arithmetic from a game to the rank of a science" (Frege 1998, § 91). According to him, formalists could not account for the applicability of mathematics because they held that mathematical formulas express no thoughts:

> [...] for an arithmetic with no thought as its content will also be without possibility of application. Why can no application be made of a configuration of chess pieces? Obviously, because it expresses no thought. If it did so and every chess move conforming to the rules corresponded to a transition from one thought to another, applications of chess would also be conceivable. Why can arithmetical equations be applied? Only because they express thoughts. How could we possibly apply an equation which expressed nothing and was nothing more than a group of figures, to be transformed into another group of figures in accordance to certain rules? (Frege 1998, § 91)

Frege's point is admittedly different from Russell's and it involves ideas that are typically his. Indeed, Frege thought it necessary for a mathematical formula to be applied that it expresses a thought, because he believed the application of a mathematical theorem to be an instance of a deductive inference and that such inferences are only possible from (true) thoughts: it is thus senseless to speak of inferring to the truth of a conclusion from something that was not a thought.

I shall make only one brief point in relation to Frege's critique of formalism. It has been answered by Friedrich Waismann in his 1936 *Einführung in das mathematische Denken*:

> What, then, has to be added, in order for a mathematical equation to express a thought-content? Application, and nothing more. It is mathematics when the equation is used for the transition from one proposition to another; otherwise it is game. To say that a move in chess expresses no thought is hasty; for it wholly depends on us. [...] 'Because a chess move expresses no thought, one cannot apply it'. Would it not be correct to say that because we have not provided an application for it, the chess move does not express a thought? (Waismann 1951, 240)

This critique has been in turn criticized by Sir Michael Dummett (Dummett 1991 259-261), who puts the following gloss on it:

> Waismann denies that we first confer a sense on the proposition, and then, in the light of that sense, make various applications of it: rather, we make the application, and *thereby* give it a sense – a truly Wittgensteinian idea. (Dummett 1991, 260)

I do not wish to discuss here Dummett's argument against Waismann; I simply wish to point out that, although Waismann's position may be described with some justice as 'Wittgensteinian', he does not make any appeal to a 'surveyability' argument and Wittgenstein's use of the latter does not presuppose or imply that it is the application that gives to a proposition its sense.

With the current fascination for his neo-Kantian background[5], one is likely to forget the fact that Carnap was, early on, a staunch logicist and that he was influenced by Russell on key points[6]. Indeed, Carnap's *Abriß der Logistik* (Carnap 1929) is probably the last major logicist work, incorporating advances made by Ramsey in his 1925 masterpiece on 'The Foundations of Mathematics' (Ramsey 1990, 164-224). The recent publication of manuscripts from the late 1920s, *Untersuchungen zur allgemeinen Axiomatik* (Carnap 2000) allows us better to understand his early philosophy.[7] I would like simply to point out that Russell's (6) and (7) are everywhere in evidence in Carnap's work during that period. Already in 'Eigentliche und uneigentliche Begriffe' (Carnap 1927), he had argued that formal concepts such as Peano's '0', 'number', and 'successor' are *uneigentliche* and in need of explicit definitions of the kind provided by Russell. The very project of *The Logical Structure of the World* was but a generalization of (6) and (7) to the whole "conceptual system of unified science". Indeed, it was meant to supplement the work of axiomatization by providing an interpretation of the primitives (ultimately into a domain of 'basic objects') through a 'constructional system':

> A theory is *axiomatized* when all statements of the theory are arranged in the form of a deductive system whose basis is formed by the axioms, and when all concepts of the theory are arranged in the form of a constructional system whose basis is formed by the fundamental concepts. So far, much attention has been paid to the first task, namely, the deduction of statements from axioms, than to the

[5] For example (Richardson 1998) or (Awodey & Klein 2004).

[6] Of course, Frege's lectures in 1910-1914 played a fundamental role. Carnap's student notes are now published (Reck & Awodey 2004). But my point here is simply to emphasize the role of (6) and (7) in Carnap's thinking.

[7] See the excellent study (Awodey & Carus 2001).

> methodology of the systematic construction of concepts. The latter is to be our present concern and is to be applied to the conceptual system of unified science. (Carnap 1967, § 2)

During the discussion at Königsberg, Carnap expressed theses (6) and (7) in so many words:

> 1. For all mathematical signs there are one or more interpretation, as a matter of fact these are purely logical.
> 2. If an axiomatic system is consistent, then we replace in each mathematical formula its mathematical signs by the logical interpretation thus found (more precisely, one among many different interpretations), and it becomes a tautology (a sentence with general validity).
> 3. If an axiomatic system is complete (in the sense given by Hilbert: no formula which is not derivable can be added without contradiction), then the logical interpretation is unique; each sign has exactly one interpretation, and the formalist construction is transformed in a logical construction. (Hahn *et al.* 1931, 141)

These results depended, in Carnap's mind, on a proof that he did not (and could not) possess, that of his *Gabelbarkeitssatz* (Carnap 2000, 133). This theorem would have shown that any consistent axiomatic system is complete if and only if it is categorical (in Carnap's words, 'monomorphic'). With this result, Carnap had hoped, during these years, to provide the standard interpretation to Peano Arithmetic and thus to integrate it into his constructional project. (Gödel's results showed that categoricity could not be proved at first-order level, thus ruining Carnap's plans.)

*

Wittgenstein was thus reacting against what was perceived at the time as an essential part of the logicist doctrine. How does his argument, as above, work against it? We may first note that it does not address the manner by which the system of *Principia Mathematica* is supposed to select the series of natural numbers among an infinity of progressions, e.g., how one cleverly defines in that book '0', '1', '2', and so on. Wittgenstein does not criticize the system of *Principia Mathematica*, which is left standing, untouched, but rather some claims made on its behalf by Russell, (6) and (7)[8]. We should rather note that, in expressing these, Russell admits that we "have already some knowledge (though not sufficiently articulate or analytic) of what we mean by "1" and "2" and so on, and our use of numbers in arithmetic must conform to this knowledge" (Russell 1919, 9). So his position seems to be that we have first knowledge of some arithmetical truths, e.g., 27 + 16 = 43 or (A), and that we then 'go formal', so to speak, and learn that (A) is a 'consequence' of some other formal sentence. Here, the claim would be that, thanks to the interpretation of Peano Arithmetic effected in *Principia Mathematica*, we are now in a position to claim that (R) is the 'ground' for (A), alternatively, that (A) is the 'consequence' of (R). In other words, an 'explicativist' epistemology is presupposed by Russell[9]. So we have a 'why' claim of the form:

(8) (A), (A) because of (R)

However, this won't do. One may simply appeal here to Aristotle's discussion in *Posterior Analytics*, Book I, section 13, of the demonstration that 'the planets are near'. As Aristotle points out, one can demonstrate this from the facts that 'the planets do not twinkle' and that 'what does not twinkle is near'. But, as he points out,

> (...) this deduction is not of the reason why but of the fact: for it is not because they do not twinkle that they are near, but because they are near that they do not twinkle. (Barnes 1995, vol. 1, 127)

In other words, according to Aristotle one has merely asserted here that the planets are near and not that they do not twinkle *because* they are near, for the simple reason that it is because they do not twinkle

8 This claim is repeated in many places, e.g., "If Russell's calculus is to be merely an *auxiliary* calculus, dealing with 'if''s and "then"s, etc., – then it is all right. But that is not what it is meant to be" ; "The Russellian method is just one method, like many of these other methods"; "We might say that Russell's method is perfectly all right, but neither is more *fundamental*" (Wittgenstein 1976, 261, 262 & 263).

9 About this explicativist epistemology, see (Mancosu 2000) and for a critique of it, for which this passage is deeply indebted, (Dubucs & Lapointe 2003).

that we *know* that they are near. Now Wittgenstein's argument as expounded above is to the effect that:

> (9) We know that (R) because we already know that (A)

This is so, not because of the fact that we happen to know (A) before discovering that it is a 'consequence' of (R) but because it is impossible to know (R) without already knowing that (A). It is not just that (R) is less easily cognizable than (A) – a prerequisite here –, it is not even cognizable at all without it. And the reason for this is but another use of the 'surveyability' argument, as we saw. To paraphrase Aristotle, Wittgenstein's argument is thus that Russell can merely assert that (R) and not that '(A) because of (R)', for the simple reason that it is because of (A) that we *know* that (R). It is an application of (A) that allows us to *know* that (R) and, if anything it is because of (A) that (R); it is thus to delude oneself to assume (8) in order to claim (6) and (7).

Textual evidence for this is easy to find in Wittgenstein's corpus, for example in the midst of his discussion with Turing in 1939 lectures:

> The idea that there is a science, namely logic, on which mathematics *rests*. I want to say it in no way rests on logic. And the fact that you can make logical formulae agree with it, in no way shows that it rests on logic. [...] We have normal ways of finding whether the numbers on both sides of the implication sign are the same [Wittgenstein is referring here to an example similar to (R) in this paper]. And this does not depend at all on Russell's principles; on the contrary, they depend on it. If we didn't have such ways of comparing the different sides, we shouldn't know what to call a tautology.
> (Wittgenstein 1976, 260)

I should emphasize again that this use of a 'surveyability' argument does not imply an overall view of mathematics founded on epistemic limitations such as strict finitism (an easy way out for those wishing to dismiss Wittgenstein's critique), it is, like Turing's use that I presented at the beginning of this paper, an appeal to an undeniable fact, it is *bona fide*. As a matter of fact, in his discussion with Wittgenstein, Turing never denies the plausibility of his use of the 'surveyability' argument, although he is sceptical about the claims Wittgenstein derives from it, because he obviously adheres to the 'explicativism' on which Russell's position relies. But 'explicativism' is itself based on a misunderstanding, as I just pointed out. That this is so is quite another matter but the mere expression of one's prejudices (in favour of 'explicativism') is not a refutation, an argument is needed. One will have noticed from the last quotation that, at least in Wittgenstein's mind, his rejection of (8) implies, through a rejection of (6) and (7), a rejection of (3), i.e., logicism is refuted. This may be too brash a claim, as much as Russell was, to begin with, too brash to claim (6) and (7). But this is quite another matter that cannot be dealt with in this paper. One thing seems more certain. It is that logicism, if it is to make sense for us, should be dissociated from claims such as (6) or (7).

Bibliography

AWODEY S. & CARUS A. W. (2001). Carnap, Completeness, and Categoricity: The *Gabelbarkeitssatz* of 1928. *Erkenntnis* 54, 145-172.

AWODEY S. & KLEIN C. (eds). (2004). *Carnap Brought Home. The View from Jena*, Chicago & LaSalle IL: Open Court.

BARNES J. (ed). (1995) *The Complete Works of Aristotle. The Revised Oxford Translation*, sixth printing with corrections, Princeton University Press.

CARNAP R. (1927). Eigentliche und Uneigentliche Begriffe. *Symposion* 1, 355-374.

CARNAP R. (1929). *Abriß der Logistik.* Vienna: Springer.

CARNAP R. (1967). *The Logical Structure of the World.* Los Angeles CA: University of Califronia Press

CARNAP R. (1983). The Logicist Foundations of Mathematics. In Benacerraf P. & Putnam H. (eds). *Philosophy of Mathematics. Selected Readings*, 2nd edition, Cambridge University Press, 41-52.

CARNAP R. (2000). *Untersuchungen zur allgemeinen Axiomatik*, Bonk T. and Mosterin J. (eds). Darmstadt: Wissenschaftliche Buchgesellschaft.

DUBUCS J. & LAPOINTE S. (2003). Preuves par excellence. *Philosophiques* 30/1, 219-234. [Engl. Trans. 'On Bolzano's Explicativism', to appear in *Synthese*].

DUMMETT M. (1991). *Frege. Philosophy of Mathematics*. London: Duckworth.

FREGE G. (1980). *The Foundations of Arithmetic*. Oxford: Blackwell.

FREGE G. (1998). *Grundgesetze der Arithmetik I/II*. Hildesheim: Olms.

GANDY R. (1988). The Confluence of Ideas in 1936. In Herken R. (ed). *The Universal Turing Machine. A half-Century Survey*. Oxford University Press. 55-111.

HAHN H., CARNAP R., GÖDEL K., HEYTING A., REIDEMEISTER K., SCHOLZ A. & VON NEUMANN J. (1931). Diskussion zur Grundlegung der Mathematik. *Erkenntnis* 2. 135-151.

MANCOSU P. (2000). On Mathematical Explanation. In Grosholz E. & Breger H. (eds). *The Growth of Mathematical Knowledge*. Dordrecht: Kluwer. 103-119.

MARION M. (1998). *Wittgenstein, Finitism, and the Foundations of Mathematics*. Oxford: Clarendon.

RAMSEY F. P. (1990). *Philosophical Papers*. Mellor D. H. (ed). Cambridge University Press.

RECK E. & AWODEY S. (eds). (2004) *Frege's Letures on Logic. Carnap's Student Notes, 1910-1914*. Chicago & LaSalle IL: Open Court.

RICHARDSON A. W. (1998). *Carnap's Construction of the World*. Cambridge University Press.

RUSSELL B. (1919). *Introduction to Mathematical Logic*. London: Allen & Unwin.

SIEG W. (1994). Mechanical Procedures and Mathematical Experience. In George A. (ed). *Mathematics and the Mind*. Oxford University Press. 71-117.

SIEG W. (2001). Calculations by Man and Machine: Conceptual Analysis. In Sieg W., Sommer R. & Talcott C. (eds). *Reflections on the Foundations of Mathematics. Essays in honor of Solomon Feferman*. ASL, Lecture Notes in Logic 15. 387-406.

STEINER M. (1975). *Mathematical Knowledge*. Ithaca NY: Cornell University Press.

TURING A. M. (1936). On Computable Numbers with an Application to the Entscheidungsproblem. *Proceedings of the London Mathematical Society*, 2nd series, vol. 41. 230-65.

WAISMANN F. (1951). *Introduction to Mathematical Thinking*. London: Hafner.

WITTGENSTEIN L. (1976). *Wittgenstein's Lectures on the Foundations of Mathematics. Cambridge 1939*. Diamond C. (ed). University of Chicago Press.

WITTGENSTEIN L. (1979). *Ludwig Wittgenstein and the Vienna Circle. Conversations recorded by Friedrich Waismann*. McGuinness B. (ed). Oxford: Blackwell.

WITTGENSTEIN L. (1980). *Remarks on the Philosophy of Psychology. Volume 1*. Oxford: Blackwell.

WITTGENSTEIN L. (1983). *Remarks on the Foundations of Mathematics*. von Wright G. H., Rhess R. & Anscombe E. (eds). revised edition. Oxford: Blackwell.

Logic and Arithmetic

Hartley Slater

Since there are non-sortal predicates Frege's attempt to derive Arithmetic from Logic stumbles at its very first step. There are properties without a number, so the contingency of that condition shows Frege's definition of zero is not obtainable from Logic. But Frege made a crucial mistake about concepts more generally which must be remedied, before we can be clear about those specific concepts which are numbers.

1

Is the concept of being a prime number a function which takes the value True when applied to the argument 5? Frege thought the predicate 'is a prime number' was a denoting phrase, and denoted a concept, i.e. something which is 'unsaturated'. But while the predicate is incomplete, and so unsaturated – simply because it is just a part of a sentence – it does not denote anything at all. The concept of being a prime number is denoted not by the predicate but by its nominalisation, and so it is 'saturated' and cannot be a function – it is an object. Cocchiarella wrote the referring phrase 'λxFx', and the associated predicate 'λxFx( )' (Cocchiarella 1987, 83); in Kneale's terminology (Kneale & Kneale 1962, 602), if the predicate is 'F' then the referential '§xFx' refers to the concept. Refined discriminations between referring phrases to concepts and open predicates thus enable us to oppose Frege's thought that concepts are categorically distinct from objects. When we are talking about concepts we are nominalising the associated predicates, and it is those open predicates which are distinct from referring phrases to objects.

Frege was plainly not too clear about some of these discriminations. For he believed that concepts were not objects, and specifically that numbers were not concepts but objects. He believed that numbers were not concepts but objects, even though he could formulate no clear way of separating the objects which were numbers from other objects, like Julius Caesar. In fact, Julius Caesar, the concept horse, the number of the planets, etc. are all objects, but they are objects of different orders, and only the first may be presented independently of predications. It was Frege's identification of numbers with objects rather than concepts, however, which supported the specific reasoning which led him to separate objects from concepts, since he thought of objects as 'saturated' while concepts were not. So we must inspect his reasons for that identification. Frege said (Geach & Black 1952, 24-5):

> The two parts into which a mathematical expression is thus split up, the sign of the argument, and the expression of the function, are dissimilar; for the argument is a number, a whole complete in itself, as the function is not... For instance, if I say 'the function $2.x^3 + x$', x must not be considered as belonging to the function; this letter only serves to indicate the kind of supplementation that is needed; it enables one to recognise the places where the sign for the argument must go in.

But, by Frege's own grammatical criterion, the expression 'the function $2.x^3 + x$', being a definite description, ought to denote an object, even though, in his representation of it, it contains some gaps, and so is unsaturated. Moreover, although what might fill the gaps, namely numbers like the number 7, arise in arithmetical statements such as '$7 + 5 = 12$', this merely uses '7' as a substantive, and Frege elsewhere recognised that the numerals, in their adjectival use, were parts of second-order predicates, so that these terms also can form parts of incomplete expressions. The phrase 'there are (exactly) seven', for instance, needs a substantive added to it, such as 'horses', to make a complete thought. What Frege did not fully appreciate, therefore, was that corresponding to the referential and descriptive uses of numerals, there are complete and incomplete expressions with all predicates.

Thus, following Cocchiarella, there is the functional expression '$\lambda x(2.x^3 + x)(\ )$', which is not a referential phrase, and so does not denote any object at all, and there is the definite description 'the operation of doubling the cube of a number and adding it to that number', i.e. '$\lambda x(2.x^3 + x)$', which contains no gaps, and therefore refers to an object – a mathematical operation, which is one kind of abstract object. Being 7 in number ($\lambda Q(7x)Qx$) is another abstract object: it is that property of discrete and distinctive things of having a correlation with the non-zero numerals up to 'seven', while in the predication 'The Ps are 7 in number', i.e. '(7x)Px', or its equivalent '$\lambda Q(7x)Qx(P)$', the same property is not referred to but expressed. The natural numbers themselves therefore satisfy Hume's Principle, i.e.

$$Nx{:}Fx=Nx{:}Gx \equiv (\exists R)(R \text{ is } 1\text{-}1.R(F, G)).$$

2

We can now proceed to look more closely at one central consequence of the above definition of number – the fact that things with a number must be discrete. The point about discreteness is crucially involved in, amongst other things, the proof that Julius Caesar is not a number.

According to Wright (Wright 1983, 11), there were three specific considerations which were involved in Frege's judgement that numbers were objects. One was the use of definite descriptions like 'the number of the planets'. Another was the currency of numerical identities, like '5 + 7 = 12'. The remaining consideration Frege appealed to was the contrast between, for instance, 'the number of planets is 9' (Nx:Px = 9) and 'there are exactly 9 planets' ((9x)Px). Only the former represents '9' as a singular referential term, and so Frege took it to be the basis of his formal analysis of Arithmetic. But, as we shall now see, it is the predicative form which has priority, and it is that fact which also shows that numbers, while still objects, are categorically distinct from objects like Julius Caesar, since they then cannot be known independently of predications. By contrast, one does not need to know someone is Julius Caesar before one can be acquainted with him.

The priority of the predicative form arises because the foundation for the theory of number is to be found in appropriate definitions of quantificational expressions like '(nx)Fx' (i.e. 'there are exactly n Fs'), from which expressions like 'εm(mx)Fx=n' (i.e. the number of Fs is n) follow quite straightforwardly. For '(nx)Fx' entails '(∃m)(mx)Fx', by existential generalisation, and so '([εm(mx)Fx]y)Fy', by the epsilon definition of the existential quantifier, which equates '(∃x)Px' with 'PεxPx'. One can then derive 'εm(mx)Fx=n' because of the uniqueness of the exact numerical quantifier. The reverse entailment crucially does not hold, however, because of the numerical indeterminacy of non-sortals: one can have 'εm(mx)Fx=n' without the epsilon term numbering the Fs, i.e. without '([εm(mx)Fx]y)Fy', since there may not be any Fs, but merely some F. The numerical identity then can still arise, but only through the arbitrary specification of a value for the epsilon term, in a case where ¬(∃m)(mx)Fx, i.e. where the predicate 'F' is not count, and so does not discriminate discrete things. Such a case is when 'F' is a mass term, and there is only an amount of stuff, in which case 'the number of Fs' must be non-attributive. Thus we do not have the epsilon equivalence '(nx)Fx ≡ εm(mx)Fx=n', but merely a one-way implication '(nx)Fx ⊃ εm(mx)Fx=n'. Certainly one can have the iota equivalence '(nx)Fx ≡ ιm(mx)Fx=n', but this does not involve an individual term on the right hand side, since it is the same as '(nx)Fx ≡ (∃m)((mx)Fx.m=n)'.

The crucial difference between epsilon terms and iota terms is that epsilon terms are complete terms for individuals, unlike iota terms, which are incomplete terms, as this last point shows. That means epsilon terms may be non-descriptive, and so can formalise Millian names; in fact they are the logically proper names Russell hypothesised, but did not have a symbolism for. The epsilon definition of the existential quantifier means that '¬(∃x)Px' equates with '¬PεxPx', so in the present case, even if there is no number of Fs

($\neg(\exists m)(mx)Fx$), still '$\varepsilon m(mx)Fx$' will refer, although then, like 'The Morning Star', for instance, its reference will be given accidentally. Thus, just as Venus is not a star, although 'The Morning Star' conventionally refers to it, so 'the number of F' when 'F' denotes some stuff, does not refer to a number which numbers discrete things. One specific consequence of the possibility of such deceptive, Millian 'number names', which dramatises the matter, is that their arbitrary reference might well be, on occasion, a physical object – for instance even Julius Caesar.

3

How can the basis for the theory of number lie in numerical quantification? In fact David Bostock essayed a deduction of Arithmetic from Logic in this quantificational style (see also, more recently, Agustin Rayo 2002). Bostock defined the numerical quantifiers in the Fregean fashion, and used a generalised theory of quantification, applicable to the numerical place in such expressions as '$(nx)Fx$', to deduce Peano's Postulates, with certain further assumptions.

Bostock was much more appreciative of the difference between numbers and amounts than other logicians (see Bostock 1974, and Bostock 1979, respectively, as a whole). But he nevertheless did not appreciate the above points about the differences between count and mass terms. For, right at the start, he tried to define a weak numerical quantifier with '$(\exists 1x)Fx \equiv (\exists x)Fx$', and a strong numerical quantifier with '$(0x)Fx \equiv \neg(\exists x)Fx$', (Bostock 1974, 9-10). So the given foundation for Bostock's deduction was unsafe – as unsafe, as we shall see, as Frege's. If 'F' is a mass term, then '$(\exists x)Fx$' and '$\neg(\exists x)Fx$' simply read 'there is some F' and 'there is no F', and even '$(\exists x)(Fx.(\exists y)(Fy.y\neq x))$' merely reads 'there is some F, and some more F'. In none of these cases, therefore, do numbers or pluralisation enter the content. There are guaranteed to be Fs, however, if there are two or more Fs, so there is no difficulty with the strong numerical quantifiers above 1, and a construction of Arithmetic in Bostock's style remains possible. If '$\underline{n}$' ranges from 2 upwards then F is count iff $(\exists\underline{n})(\underline{n}x)Fx$ or $(\exists x)(y)(Fy \equiv y{=}x) . M(\exists\underline{n})(\underline{n}x)Fx$ or $\neg(\exists x)Fx.M(\exists\underline{n})(\underline{n}x)Fx$, and abbreviating the latter disjuncts to '$(1x)Fx$', and '$(0x)Fx$', respectively gives us the simplified definition that 'F' is count iff $(\exists n)(nx)Fx$, where 'n' ranges from 0 upwards. That means that Bostock's definitions above will hold only on the supposition that $(\exists n)(nx)Fx$. The restriction in the case of the number 1 is required not just because of the possibility of mass terms, but also proper names, since 'is Peter', for instance, will hold without 'is one Peter' holding. The restriction with the number zero is required because for non-sortals there may be no F without there being zero Fs.

Why are there guaranteed to be Fs if not just $(\exists x)(Fx.(\exists y)(Fy.y{\neq}x))$, but $(\exists x)(Fx.(\exists y)(Fy.y{\neq}x.(z)(Fz \supset z{=}x \vee z{=}y)))$? Consider two rings of gold (or two atoms of water, say). Since these are both gold (water) there is clearly a third object which is also gold (water), namely the mereological sum of the previous two objects. So if there are <u>just</u> two ($(2x)Fx$), then the predicate must be count. The case of 1, as it is standardly formalised ($(\exists x)(y)(Fy \equiv y{=}x)$), still allows the predicate to be a mass term, since if there is just one atom of water, then while portions of that atom are not water themselves, and so only the whole atom is water, that whole atom is still 'some water' and not 'one water'. In a somewhat similar manner, although 2 is the only even prime it is still 'an' even prime, not 'one' even prime. So always the possibility of there being two items is required before we can start to count with a term. The point even holds when there can be nothing of the kind in question. For if we could say there were no round squares we could rightly say there were zero round squares. But in fact there is merely no round square, from which it does not follow there is zero round square.

4

We can now finally see how the restricted definition of zero, which emerges from such considerations about plurality, undermines entirely all Fregean, and Neo-Fregean attempts to derive Arithmetic from Logic. In the above terms, Frege presumed that '(∃n)(nx)Fx' held for all predicates, and the leading Neo-Fregean Crispin Wright is notable for being amongst the first to publicise the fact that this is not so. But this criticism has more radical consequences than Wright realised for the development of Arithmetic using Hume's Principle.

Boolos and Wright, with others, have demonstrated how most of Frege's development of Arithmetic can be obtained from Hume's Principle, starting from Frege's definition of zero as the number of things which are not self-identical (Nx:x≠x). But in this extensive, and now very elaborate discussion, no question has been raised about whether '¬(∃x)(x≠x)', entails 'Nx:x≠x = 0'. If the negative existence statement entails the numerical statement, then Nx:x≠x must be determinate, and that is contingent on 'x≠x' being a sortal predicate, as Wright has admitted. But what unit is determined by non-self-identity? No argument for there being one has been given, either by Wright, or by any one else within this tradition. Indeed, at one time it was simply presumed that all predicates were sortal. But Wright has recently given a proof that self-identity is not a sortal concept (Hale & Wright 2001, 315). As a result (as Wright himself explicitly realised earlier, see Wright 1983, 187), argument is needed to show that non-self-identity is a sortal concept. On the above definition it clearly is not. Much ink has been spilled debating whether Hume's Principle is analytic, and so whether the Arithmetic taken to be derivable from it can, or cannot, be properly described as a part of Logic. But if Logic does not discriminate between sortal and non-sortal concepts, then there is no way to get from it the other crucial element in Frege's generation of the number series: its starting point.

We say 'the number of Fs is n' and can do so whether n is 0, 1, 2, or more; but only count nouns pluralise in the appropriate way. Mass terms sometimes appear pluralised, but not in the same sense: in 'there are several champagnes', for instance, we are speaking about glasses of champagne, maybe, or varieties of champagne. In English we can say 'it is F' rather than 'it is an F', but nothing corresponding to this is to be found in Frege's language. Maybe there is no martini in a glass. Does that mean the number of martini in the glass is zero? It does not. There is no such thing as a number, i.e. a number of Fs, in this case. There might have been some F, rather than no F, and in both cases as much F as G, but the required plural case, and so the possibility of a number, and the same number, just does not arise. Could we not simply introduce a count noun, and talk about the number of Cs which are F instead? Certainly if there is no gold then the number of ingots which are gold is zero. Wright has discussed this matter more than most, and he has admitted: 'to number the instances of some non-sortal concept is intelligible only if relativised to a sortal' (Wright 1983, 3; see also Hale & Wright 2001, 315, 387). But the necessary distinction between substantives and adjectives is just what is lacking. Maybe '$\neg(\exists x)Fx$', is equivalent to '$(C)(Nx: (Cx.Fx) = 0)$', where 'C' ranges over count terms, but one cannot say this in Frege's concept-script.

References

BOSTOCK D. (1974). *Logic and Arithmetic I*. Oxford: O.U.P.

BOSTOCK D. (1979). *Logic and Arithmetic II*. Oxford: O.U.P.

COCCHIARELLA N. B. (1987). *Logical Studies in Early Analytic Philosophy*. Columbus: Ohio State University Press.

FREGE G. (1968). *The Foundations of Arithmetic*. Oxford: Blackwell.

HALE B. & WRIGHT C. (2001). *The Reason's Proper Study*. Oxford: Clarendon.

KNEALE W. & KNEALE M. (1962). *The Development of Logic*. Oxford: Clarendon.

RAYO A. (2002). Frege's Unofficial Arithmetic. *Journal of Symbolic Logic* 67/4. 1623-1638.

WRIGHT C. (1983). *Frege's Conception of Numbers as Objects*. Aberdeen University Press.

Ajdukiewicz and Kotarbiński on Names: a Pretext for Ontological Games

Anna Zielinska

Leśniewski's Ontology was one of the most inspiring aspects of Polish philosophy in the 20th century. I would like to reveal two original ways of thinking about names present in Polish pre-war philosophy and inspired by Leśniewski's ideas, *i.e.* Kotarbiński's reism and Ajdukiewicz's criticism of the latter. It seems obvious, at least in texts of the philosophers quoted above, that the question of names was hiding much deeper quarrels. Although Kotarbiński's and Ajdukiewicz's positions were not in radical opposition, several disagreements between them were very fruitful regarding their respective works. This paper gives an overview of their respective contributions to logic presented against the background of their philosophical positions

All along his philosophical life, Ajdukiewicz nourished his writings by criticisms addressed to theses contained in Tadeusz Kotarbiński's *Elementy* (Kotarbiński 1929). This significant volume was prepared for university students but appeared much too rich and much too innovative to serve this purpose. One year after, Ajdukiewicz published a famous review of the *Elementy* (Ajdukiewicz 1930), which is not only a precious criticism of Kotarbiński's book, but also an excellent account of his own views. He continued the dialogue with Kotarbiński through many other papers, and – as we will see – he worked upon his attitude toward Kotarbiński's writings for years.

One of the major issues of *Elementy* was what Kotarbiński called "reism"[1], ontological and semantic theory. Materialism represents its ontological side, and its semantic one is a radical nominalism. When Ajdukiewicz starts to criticize reism, he admits that he finds himself unable to formulate the main thesis of that theory. Even if he clearly sees the project of reducing of all Aristotelian categories to the one of things, he notices a hesitation between ontological and semantic formulations.

*

Ajdukiewicz quotes what is supposed to be the main thesis of reism: "any name, which is not a name of a thing, is considered as an apparent name". He is convinced that this is a mere tautology, based on arbitrary definitions. He proposes then an interpretation of reism that he finds more plausible than what is explicitly present in Kotarbiński's text: "names denoting things constitute a closed semantic category". This means, quite trivially: in a meaningful sentence, if we replace the name of a thing by an expression which is not such a name and if the meaning of the other expressions remains unchanged, then what we obtain is a nonsense. On the other hand, if we replace this name of a thing by another name of a thing, then we get a sentence, false or true, but certainly meaningful.

If we concede this interpretation, says Ajdukiewicz, we should define the language we are talking about, even though Kotarbiński seems to think that this should be valid for any language. While the very notion of semantic category was introduced to prevent us from antinomies, we are confronted here to a quite radical norm that forbids, for example, putting together words like "table" and "pain" into the same semantic category. Is this restriction justified? Ajdukiewicz does not see any danger of antinomy in considering as meaningful [*sensowne*] sentences like "Something is a pain" and "Something is a table".

[1] Known also as pansomatism or concretism – the modification of signification comes with the discovery of Brentanian version of dualistic reism and with Ajdukiewicz's criticisms.

A reist is confronted to a fundamental problem: the negation of the existence of apparent names. If his ontology admits only genuine names, how is he able to say, for example, that "events do not exist"? The interpretation of genuine names as a distinct category implies that this proposition is senseless, whereas in a metaphorical language this proposition is simply false. All what a reist can tell is – as it has already been said – that every object is a thing, because the non-existence of literal relations or qualities cannot be consistently expressed in a reistic language. Ajdukiewicz thought that Kotarbiński's project, as for its ontological ambitions, might have hidden a conviction of our ability to conceive our world in a "direct" way, without any conceptual apparatus. The critic accused the reist to be close to Kantian research of noumenons.

*

The nature of mathematics as presented by reism faces some very similar criticisms to those encountered by a nominalist in mathematics. Ajdukiewicz criticizes Kotarbiński's conception of numbers: the author of *Elementy* maintained – at least at the time of that book – that numbers do not exist in themselves. According to him, they are a sort of apparent names and constitute a useful convention. They are obviously not the object of mathematics, because mathematics talks about things, as natural sciences do; the only difference between them is what they say about things. Then the arithmetic expression:

$$2 + 3 = 5$$

is an abbreviation, whose proper and detailed formulation would be the following:

For every x: $3x + 2x = 5x$
(where x is a variable and takes as values names of things or persons)

On the contrary, Ajdukiewicz maintains there is nothing more than numbers he has to think about when counting. Reading "2 + 3 = 5", he does not think of it as of a metaphor, hiding some particular objects. What, by the way, should be understood by the expression: "what mathematics talks us about"? "If what it talks about are referents of names (values of variables of names), thus mathematics talks about nothing, since there are no names in its theorems. But if what it talks about are objective correlates of functors occurring in these theorems, which have no arguments, then it talks about numbers; it is so because – in the arithmetical sentence "2 + 3 = 5" – numbers, and nothing else, are the final arguments." (Ajdukiewicz 1930, fragm. not translated in Eng.) One can evoke Gardies stating that classical algebra is a language that abandoned the primitive purpose of communication to the one of creation (Gardies 1975, 57). This idea seems to be very present in the conflict between Ajdukiewicz and Kotarbiński.

*

Ajdukiewicz recognizes the instrumental utility to the postulates of semantic reism: he concedes that replacing apparent names by genuine ones helps to avoid a whole range of philosophical problems, appearing as false. Even if in his grammatical research Ajdukiewicz did not seem to be interested in historical semantics, he formulated some remarks on this topic in his famous essay "The Scientific World-Perspective". According to the Polish philosopher, the modifications of paradigms proper to the history of science perturbed our primitive use of language which matched, "in a simple and naïve manner" (Ajdukiewicz 1934a), what we experienced and what we lived. The fact that we are sometimes unable to distinguish the rules of one's language means only that this person hesitates between different languages, says Ajdukiewicz. The question to ask here would be the following: what language-game are you playing?

In any case, Ajdukiewicz still did not see any fundamental danger hidden behind the use of apparent names. Subsequently, he said that the second way of considering reism – this time as an ontological theory – seems trivial to him. This triviality seems extremely interesting because, though imperfectly formulated, reism was the most radical version of nominalism and of materialistic monism known at that time. Ajdukiewicz approaches the problem formally: the thesis saying that every object is a thing, "All *A* is *B*", means "For every *x*, if *x* is *A*, *x* is *B*, and some *x* are *A*" (i. e. *A* is not an empty name); "For every *x*, if *x* is an object, *x* is a thing, and some *x* are objects". Then it remains trivial, says Ajdukiewicz, to maintain that objects exist.

*

This criticism of reism, both ontological and semantic, was not innocent from the metaphysical point of view, since Ajdukiewicz was, in the early thirties, quite a radical conventionalist. Classical conventionalism underlines the difference between report sentences (purely describing ones) and sentences having an interpretation. The first ones belong to the primitive part of our language – the one we have learnt in our childhood – whereas the second need an interpretation with a set of more or less sophisticated rules. "These additional meaning-rules are called 'conventions', 'coordinative definitions', etc." (Ajdukiewicz 1934; 1978, 78). Ajdukiewicz's contribution to this position can be summarised by the following lines:

> Our point of view is significantly more radical than that of the conventionalism just discussed. We see no essential difference between a report sentence and an interpretation-sentence. [...] We can avoid accepting such sentences [...] if we are willing to choose a conceptual apparatus in which their meaning does not occur. Thus, and it seems with some justification, we designate our point of view as that of a ***radical conventionalism***. (Ajdukiewicz 1934; 1978, 79).

The main difference between this version of conventionalism and the classical one relies on the attitude towards the concept of truth. While Le Roy or Poincaré used to talk about "commodity" of scientific theories, Ajdukiewicz, on the contrary, insisted on the use of the notion of truth when dealing with rules and interpretations. The assertability of a sentence is fully determined by the mastery of the language in which it is formulated. Ajdukiewicz's conception of meaning with its semantic rules did not focus on the "sense" of propositions, but on their acceptability. What interests him is not whether an expression is meaningful, but whether it fits to one's conceptual apparatus. A linguistic world-picture was then given by the set of all the theses of a given language, i.e. the sentences which are to be accepted in virtue of the rules of this language. This shows how opposed he was to the very idea of universal rules for every language, as those stated by reism. As he said in 1960,

> I have concentrated all my efforts to demonstrate that, for every language, there exist the rules for accepting sentences; [...] if someone violates them, he will not speak the given language anymore. (Ajdukiewicz 1960, VII)

There are many possible conceptual apparatus with specific languages, and most of them are able to provide a set of sentences truly describing world's nature. Ajdukiewicz was then convinced that reism has its own language. This language could have been criticized by Ajdukiewicz's one, but it was also judged as competent to provide a true and coherent conception of the world. There is a temptation to state that Ajdukiewicz interpreted reism not at all as contradictory to some other, more elaborated, ontology, but only as formulated in an alternative language. This is quite a surprising suggestion coming from someone defining himself as opposed to anti-irrationalism. Nevertheless, the peculiarity of his conventionalism was to underline that many paths can lead to truth. In this context, he should admit that a theory like Platonism can be paraphrased in a way to obtain, I keep on with our example, a reistic account of reality. And this idea is still waiting to be proved.

*

Just before publishing "Die syntaktische Konnexität" (Ajdukiewicz 1935) Ajdukiewicz made public a small but essential article in Polish, "On the Problem of Universals" (Ajdukiewicz 1935a). He introduced there the formalisation of semantic categories. A proposition [*zdanie*] in its logical sense, says Ajdukiewicz, *i. e.* an expression which is either true or false, plays a crucial role in the definition of semantic category:

> [E]xpressions *A* and *B*, taken in sense *a* and *b* respectively, belong to the same semantic category if and only if every sentence S_A containing expression *A* in sense *a* upon replacement of *A* by *B* taken in sense *b* (the meaning of all other expression sand their interconnections remaining unaltered) is transformed into an expression which is also a sentence, and if vice versa: every sentence S_B upon replacement *B* by *A* (with analogous qualifications) is also transformed into sentence. (Ajdukiewicz 1935a; 1978, 95)

"Socrates" and "Plato" belong thus obviously to the same semantic category, whereas "Socrates" and "walks" do not seem to do: when we replace the word "walks" by "Socrates" in a proposition "Socrates walks", we obtain "Socrates Socrates", which is not, at least at first glance, a sentence. This definition of semantic category is quite identical to the one advanced in the famous "Die syntaktische Konnexität". And even if there is a controversy very present among logicians of whether general and individual names form a unique semantic category, Ajdukiewicz, within this context, is not particularly outraged by a sentence like "Every Socrates is mortal", which is the result of the replacement of a general term by a singular one in the sentence "Every man is mortal".

Ajdukiewicz has found the idea of semantic categories in Leśniewski's struggles against Russell's paradoxe and his theory of types. According to Jan Woleński (Woleński 1987, 141) and as it has also been underlined by Leśniewski himself[2], the conceptual bases of semantic categories are to be found in Aristotelian categories and in Husserl's semantic categories (*Bedeutungskategorie*), as a natural continuation of grammatical repartition of expressions in sentences. Ajdukiewicz was convinced, unlike Leśniewski, that what Husserl really meant by his semantic categories was closer to our understanding of syntactic categories. His use of the notion of category seems to be disconnected from Leśniewski's original idea of constructional nominalism, and adopted as a convenient tool to measure the syntactic connection of sentences. Leśniewski distinguished three categories: sentences (the basic one), names and different fun-

[2] Leśniewski (1929, 14).

ctors. Ajdukiewicz's conception was very close, although he thought that names and sentences were basic categories, whereas functors was a derived one. Besides, number of other categories may be constructed with the help of those three ones. With his conventionalist background, Ajdukiewicz remained convinced that there can be no unique answer, and the choice of semantic categorisation is arbitrary. In consequence, his acceptation, in 1935, of Leśniewski's categorisation does not seem to be in contradiction with his doubts concerning putting names of things and general names into the same semantic category.

*

In the above quoted article on universals, Ajdukiewicz compared the classification introduced by Leśniewski and continued by Kotarbiński to the one we find in *Categories*. Aristotle's semantics also has a category for functors and one for sentences, but it divides names into at least two different basic categories: the names which can be predicates, and names which can only be subjects. Ajdukiewicz thought that the meaning of the term "universal" must be different in two different conceptual apparatus, like Kotarbiński's and Aristotle's semantics. Parenthetically, Georges Kalinowski proposed a convincing demonstration showing that Leśniewski's struggle against universals does not affect moderate realism, as the one we meet in Aristotle's writings (Kalinowski 1995).

Ajdukiewicz proposed (Ajdukiewicz 1935a; 1960, 102-104) then a formalisation of Aristotelian categories, and gave an interpretation of one and the same sentence according to the two points of view:

Aristotle	Kotarbiński
i – individuals' names	n – names
g – general names	s – sentences
s – sentences	
"is" : $\frac{s}{ig}$ or $\frac{s}{gg}$	"is" : $\frac{s}{nn}$
Socrates is a man: $i\frac{s}{ig}g$	Socrates is a man: $n\frac{s}{nn}n$
Dogs are mammals: $g\frac{s}{gg}g$	Dogs are mammals: $n\frac{s}{nn}n$

The method of verification of the syntactic connexion remains the same, and a sentence is connected if a unique letter or a fraction is the result of the simplification. In these considerations the verb "to exist", quite problematic in Kotarbiński's writings, may have two different forms in Aristotle, and within his language none of them leads to contradiction. By the way, in "Die syntaktische Konnexität", Ajdukiewicz does not quote Aristotle, but he still underlines that natural language distinguishes two parts in the basic category of names, analogous to those mentioned above.

This article, as we said, shows the multiplicity of possible categories, but it also goes against Kotarbiński's idea that a proper name is an expression fitting the role of predicate in a proposition of type "*A* is *B*" with a fundamental meaning of the verb "is". Kotarbiński, as Leśniewski did, accepted a very special sense of the word "is", proper to Polish or Latin language, but inexistent in French or English because of the presence of articles before nouns. He needed it to underline the ontological challenges of the expression "being something". This is a fundamental, as he wants it, use of the word, like in the sentence "Sokrates jest czlowiekiem" or "Socrates est homo", and not as in sentences "There is Justice" or "Every man is mortal". As far, says Ajdukiewicz already in 1930, as we did not make a statistical research on how do people use their "is" in this kind of sentences, there is no way to determine what they do mean by that.

*

In 1960, Ajdukiewicz commented Kotarbiński's reaction to his critics from 1930: "This dispute incited – at least this is the impression I have – a weakening of the first form of the doctrine: from an ontological thesis it has been transformed into a thesis proposing a program of language construction" (Ajdukiewicz 1960, VI). It is obvious that his paper deeply influenced Kotarbiński, but it seems that his interpretation of this influence is almost the exact opposite of what had really happened. Kotarbiński accepted a huge part of formal remarks concerning his thesis, but he did not abandon his materialist and monistic world-view. By the way, merely semantic reism (with no ontological claims) would have no reason to exist. Czesław Lejewski formulated this idea with a particular lucidity:

> There is not much point in avoiding abstract noun-expressions in disciplines of lesser generality. Elimination of onomatoids from final pronouncements is of paramount importance only if these final pronouncements are meant to be used in ontological arguments. (Lejewski 1979).

"Concretism" or "pansomatism" – as others names of his conception – almost lost its concern for language as itself, because the ordinary language is far too complex to be governed by simple rules. This movement is analogous to Wittgenstein's evolution from the *Tractatus* to the *Philosophical Investigations*: some primitive presumptions remained the same, but as they appeared far from being exhaustive, the whole philosophical framework needed to be modified.

Kotarbiński ended up asking his readers just to have in mind the idea that whatever they say, they should be conscious that the only genuinely existing objects are things, singular and spatio-temporal. Unlike Wittgenstein, he tried to keep some semantic classifications present in his early papers, since we can still read, in a posterior article (Kotarbiński 1949), that there are 3 kinds of concrete names: singular, general (predicates) and empty ones (Santa Claus), and we should always be able to bring back all we are talking of to them[3]. Anyway, his disinterest in issues of philosophy of language and of logic came from his commitment to practical philosophy, to what he called praxiology and autonomous (independent) ethics. Since logic and philosophy are to be *organon* – one should not forget the strong Aristotelian tradition of the Lvov-Warsaw School – Kotarbiński did not see any fundamental reason to waist his time in more and more abstract formal debates. The formulation of his ontology turned out, quite surprisingly, to be either trivial and tautological or nonsensical. Subsequently, he concentrated on how things should function rather on what they are.

We have then seen how Ajdukiewicz, at least in the thirties, tried to give the impression of someone quite uninterested by ontological issues. He was reluctant to give any opinion on the nature of the world – he concentrated his attention mainly on formal correctness of languages susceptible to express true or false propositions. This attitude can be illustrated by the way he dealt with the problem of identity of mental and physical phenomena and with the issue of extensionalism in logic (Ajdukiewicz 1934b). Ajdukiewicz was, already in 1934, conscious of the limitations of possible applications of formal logical rules to the events of the world. He noted that even the validity of a concrete syllogism – "If all man is mortal, and Socrates is a man, then Socrates is mortal" – cannot have pure logical principles as its only justification. Ajdukiewicz opened a perspective for a possible creation of a logical language, formed as paraphrases of general propositions of logic, with new, even if sometimes arbitrary and restricted meanings for a number of expressions. These restrictions, according to him, would be quite a decent price for the exactness we would obtain.

*

[3] Kotarbiński's comment to many criticics of reism: "il s'est produit un déplacement de ce que les théoriciens des litiges appellent *onus probandi*. Auquel des partis plaidant en justice la tâche de fournir les raisons valables qui convaincraient le tribunal de se ranger à ses revendications? En général, on n'exige pas aujourd'hui des adversaires du réisme d'expliquer pourquoi il n'est pas possible d'éliminer de toute proposition tous les noms apparents; on demande au contraire aux adeptes du réisme de prouver que ceci est toujours possible à exécuter." (1966, 476).

Kotarbiński's radical nominalism and materialistic monism as ontological postulates remained in deep conflict with Ajdukiewicz's whole methodological approach – the latter was, incidentally, relentlessly reducing the field of application of his ideas. It seems that the most perturbing element in this conflict is the problem of the limits of what can be said and of the limits of logic as *organon*. Kotarbiński conceded that many of his formulations contained some obvious mistakes and changed his language, but he did not abandon any of his world-attitudes. It would be almost trivial to recall the ideas of a few last propositions of Wittgenstein's *Tractatus*. Philosophy limits the thinkable and helps in finding the way to express the things clearly (4. 113 – 4. 116). Finally, all what can be said about the world, can be expressed only in the language of natural sciences. We find, in Kotarbiński's later writings, a temptation to insist on dealing with tiny things, temptation accompanied by a particularitst method, but no attempt to make explicit the universal structure of the world.

References

AJDUKIEWICZ K. (1930). Elementy teorii poznania Tadeusza Kotarbińskiego. *Przeglad Filozoficzny* 33, 1/2; [Engl. trans. in Woleński 1990].

AJDUKIEWICZ K. (1934). Das Weltbild und die Begriffsapparatur. *Erkenntnis* 4, 259-287. [Engl. trans. (The World-Pictures and the Conceptual Apparatus) in Ajdukiewicz 1978, 67-89].

AJDUKIEWICZ K. (1934a). Die wissenschaftliche Weltperspektive, *Erkenntnis*, V; [Engl. trans. (The Scientific World-Perspective) in Feigl & Sellars 1949, 182-188 and in Ajdukiewicz 1978, 111-117].

AJDUKIEWICZ K. (1934b) O stosowalnosci czystej logiki do zagadnien filozoficznych. *Przeglad Filozoficzny*, 38, 323-327. [Germ. version (Über die Anwendbarkeit der reinen Logik auf philosophische Probleme) *Actes du VIII Congrès International de Philosophie*, Prague, 1936, 170-174; Engl. trans. (On the Applicability of Pure Logic to Philosophical Problems) in Ajdukiewicz 1978, 90-94].

AJDUKIEWICZ K. (1935). Die syntaktische Konnexität. *Studia Philosophica* I; [Engl. trans. (Syntactic Connection) in McCall S. 1967. 207-231].

AJDUKIEWICZ K. (1935a) W sprawie 'uniwersaliow'. *Przeglad Filozoficzny* 37, 219-234. [Engl. trans. (On the Problem of Universals) in Ajdukiewicz 1978, 95-110].

AJDUKIEWICZ K. (1960). *Jezyk i poznanie I*, Warszawa: PWN.

AJDUKIEWICZ K. (1965). *Jezyk i poznanie II*, Warszawa: PWN.

AJDUKIEWICZ K. (1978). *The Scientific World-Perspective and other Essays (1931-1963)*, Giedymin J. (ed). Dordrecht: Reidel.

FEIGL H. & SELLARS W. (eds). (1949) *Readings in Philosophical Analysis*. New York: Appleton-Century-Crofts.

GARDIES J.-L. (1975). *Esquisse d'une grammaire pure*. Paris: Vrin.

KALINOWSKI G. (1995) Les démonstrations de la non-existence des objets généraux chez Leśniewski. *Recherches sur la philosophie et le langage* 16, 121-146.

KOTARBIŃSKI T. (1929). *Elementy teorji poznania, logiki formalnej i metodologji nauk*. Lwow: Wydawnictwo Zakladu Narodowego Imienia Ossolinskich.

KOTARBIŃSKI T. (1949). O postawie reistycznej czyli konkretystycznej. *Mysl wspolczesna*, 10/41.

KOTARBIŃSKI T. (1966). Franz Brentano comme réiste. *Revue Internationale de Philosophie*. 78/20, 459-476.

LEJEWSKI C. (1979). On the Dramatic Stage in the Development of Kotarbiński's Pansomatism. In Woleński (1990).

LEŚNIEWSKI S. (1929). Grundzüge eines neuen Systems der Grundlagen der Mathematik. *Fundamenta Mathematicae* 14. 1-81.

MCCALL S. (ed). (1967). *Polish Logic 1920-1939*. Oxford: Clarendon.

WITTGENSTEIN L. (1922). *Tractatus logico-philosophicus*, London: Routledge and Kegan Paul.

WOLEŃSKI J. (1987). Filozoficzna szkola lwowsko-warszawska, Warszawa: PWN.

WOLEŃSKI J. (ed). (1990). *Kotarbiński, Logic, Semantics and Ontology*. Dordrecht, Boston, London: Nijhoff / Kluwer (Philosophy Series vol. 40).

List of Authors

DIRK VAN DALEN, Professor Dr., Department of Philosophy, Utrecht University. (dirk.vandalen@phil.uu.nl).

J. WOLFGANG DEGEN, Dr., System simulation, Department of Computer Science, Erlangen University. (degen@informatik.uni-erlangen.de)

PHILIP A. EBERT, PostDoctoral Research Fellow, *Arché*, AHRC Research Center for Philosophy of Logic, Language, Mathematics and Mind, University of St. Andrews. (pae1@st-andrews.ac.uk)

NADINE GESSLER, Dr., Chargée de cours, Institut de logique, Université de Neuchâtel. (nadine.gessler@unine.ch)

JEAN-PIERRE GINISTI, Professeur à l'Université Lyon 3 Jean Moulin.

BOB HALE, Professor, Department of Philosophy, University of Sheffield. (r.hale@sheffield.ac.uk)

LUCA INCURVATI, University of Cambridge, St. John's College and Faculty of Philosophy. (li216@cam.ac.uk)

PIERRE JORAY, Professeur à l'UFR de Philosophie de l'Université de Rennes 1. (pierre.joray@univ-rennes1.fr)

MATHIEU MARION, Chaire de recherche du Canada en philosophie de la logique et des mathématiques, Département de philosophie, Université du Québec à Montréal. (marion.mathieu@uqam.ca)

MARCUS ROSSBERG, PostDoctoral Research Fellow, *Arché*, AHRC Research Center for Philosophy of Logic, Language, Mathematics and Mind, University of St. Andrews. (mr30@st-andrews.ac.uk)

BARRY HARTLEY SLATER, Honorary Senior Research Fellow, Department of Philosophy, University of Western Australia. (slaterbh@cyllene.uwa.edu.au)

ANNA ZIELINSKA, Equipe de Recherche Philosophie, Langages et Cognition, Département de Philosophie, Université de Grenoble 2. (anna.zielinska@wanadoo.fr)

Travaux de logique
Liste des numéros parus

1. Denis Miéville: Introduction à la théorie des systèmes formels. Première partie. Septembre 1985 (épuisé).
2. Denis Miéville: Introduction à la théorie des systèmes formels. Deuxième partie. Janvier 1987 (épuisé).
3. James Gasser: La syllogistique d'Aristote à nos jours. Juin 1987.
4. Denis Miéville: Introduction à la théorie des systèmes formels. Première partie. Avril 1991 (réédition du n° 1; épuisé).
5. Denis Miéville Introduction à la théorie des systèmes formels. Deuxième partie. Avril 1991 (réédition du n° 2; épuisé).
6. Denis Miéville: La négation, une étude logique. Mai 1991 (épuisé).
7. Denis Miéville (éd.): Kurt Gödel. Actes du colloque, Neuchâtel, 13 et 14 juin 1991. Septembre 1992.
8. James Gasser: Introduction à la logique des relations de C.S. Peirce. Novembre 1993.
9. D. Miéville, P. Joray, D. Stauffer, N. Gessler: Études logiques. Port-Royal: une logique des idées. L'avènement de la théorie sémantique de la vérité de Tarski. George Boole et l'algèbre de la logique. Décembre 1994.
10. D. Bourquin, P. Joray, N. Gessler, D. Miéville: Analyse catégorielle et logique. Octobre 1996.
11. D. Miéville (éd.): Introduction aux logiques non classiques. Octobre 1997.
12. F. Vuissoz: La conception sémantique de la vérité. Logique et philosophie chez Alfred Tarski. Décembre 1998.
13. D. Miéville, P. Joray, F. Nef, M. Bourdeau, D. Bourquin, A. Lecomte, J. Lambek, B. Godart-Wendling: Rôle et enjeux de la notion de catégorie en logique. Actes du colloque organisé à Neuchâtel, les 16 et 17 octobre 1998. Septembre 1999.
14. F. Nef, C. Hughes, P. Giaretta, A. Bottani, N. Gessler, F. Correia. P. Simons, A. Varzi: Méréologie et modalités. Aspects critiques et développements. Actes du colloque, Neuchâtel, 20-21 octobre 2000. Août 2001.

⌧ D. Miéville, Introduction à l'œuvre de S. Lesniewski. Fasc. I: La protothétique. Novembre 2001.

15 A. Facchini, «Maison Hilbert»: un très joli édifice sans toit ni sol. Analyse model-théorétique d'un échec. Octobre 2003.

⌧ D. Miéville, Introduction à l'œuvre de S. Lesniewski. Fasc. II: L'ontologie. Novembre 2004.

16. N. Gessler, P. Joray, C. Degrange, Le logicisme catégoriel. Janvier 2005.

17. J.-Y. Béziau, A. Costa Leite, A. Facchini (eds), Aspects of Universal Logic. Décembre 2004.

⌧ N. Gessler, Introduction à l'œuvre de S. Lesniewski. Fasc. III: La Méréologie. Août 2005.

⌧ M. Peeters, Introduction à l'œuvre de S. Lesniewski. Fasc. IV: L'œuvre de jeunesse. Janvier 2006.

18. M. Joray (ed.), Contemporary Perspective on Logicism and the Foundation of Mathematics, Janvier 2007.

Ces publications peuvent être obtenues auprès du Centre de Recherches Sémiologiques au prix de **Fr.s. 15.-; Fr. 20.-** dès le n° 14 (TVA comprise).

Travaux du Centre de Recherches Sémiologiques
Liste des numéros parus

*1. G. Vignaux: La nouvelle rhétorique. Revue critique et perspectives d'application. 1969-70.
*2. G. Vignaux: L'argumentation antique: Aristote. Janvier 1970.
*3. M.-J. Borel: Pour définir l'argumentation. 1969-70.
*4. F. Bugniet: Remarques sur les notions d'assertion linguistiques et de proposition logique. Septembre 1970.
*5. M.-J. Borel, G. Vignaux: L'étude de l'argumentation. Séminaire 1969-70.
*6. G. Vignaux: L'argumentation: bibliographie sélective. Janvier 1971.
*7. J.-B. Grize: Logique de l'argumentation et discours argumentatif. Mai 1971.
*8. J.-L. Galay: La rhétorique du discours de philosophie systématique. Essais d'analyse. Mars 1971.
*9. C. Morier: Charles Sanders Peirce et la sémiotique. Mars 1971.
*10. G. Vignaux: L'argumentation et le résumé. Mars 1971.
*11. C. Gillièron, C. Bonnet: Peut-on définir l'argumentation? Avril 1971.
*12. J.-B. Grize: Notes sur l'ontologie et la méréologie de S. Lesniewski. Mars 1972.
*13. M. Hirsbrunner, P. Fiala: Les limites d'une théorie saussurienne du discours et leurs effets dans la recherche sur l'argumentation. Avril 1972.
*14. C. Gillièron, A.-M. Badonnel, J.-P. Iacazzi: Les recherches psychologiques et psycholinguistiques sur la négation et les relations d'opposition. Mai 1972.
*15. J.-L. Galay: Esquisse pour une théorie figurale du discours. Septembre 1972.
*16. Y. Oppel: Sémiotique littéraire, à propos de la coordination, répétition et opposition dans un texte littéraire. Mai 1973.
*17. P. Fiala, C. Ridoux: Essai de pratique sémiotique. Juin 1973.
*18. M. Hirsbrunner: Pour une critique de la sémiotique de Roland Barthes. Juillet 1973.
*19. Y. Oppel: Colloque sur l'analyse du discours «Divergences et convergences». Février 1974.
*20. (Collectif): Logique, argumentation, discours (LAD). Recherche. Septembre 1974.

*21. (Collectif): Logique, argumentation, discours (LAD). Recherche. Septembre 1974.
*22. A.-F. Schmid: Philosophie et sciences chez Henri Poincaré: lecture philosophique. Octobre 1974.
*23. M.-J. Borel: Schématisation discursive et énonciation. Arguments théoriques et approche descriptive (LAD I). Octobre 1975.
*24. A. Licitra: Les relations interpropositionnelles. Huit types d'après R. Longacre (LAD I). Octobre 1975.
*25. (Collectif): Discours et structures sociales. Janvier 1977.
*26. M. Ebel: Langage, histoire, action: les recherches de Jean Pierre Faye. Septembre 1975.
*27. M. Ebel, P. Fiala: Recherches sur les discours xénophobes I. Juillet 1977.
*28. M. Ebel, P. Fiala: Recherches sur les discours xénophobes II. Juillet 1977.
*29. J.-B. Grize: Matériaux pour une logique naturelle (LAD I). Mai 1976.
*30. D. Miéville, M.-J. Borel, A. Licitra: Discours et analogie (LAD II). Mai 1977.
*31. J. Moeschler: Contribution linguistique à une sémiotique du cinéma. Mai 1977.
*32. A. Lecomte: Paraphrase et thématisation. Essais d'analyse logique. Décembre 1978.
*33. (Collectif): Langue et discours I. Colloque Besançon-Neuchâtel, 2-4 octobre 1978. Mars 1978.
*34. (Collectif): Langue et discours II. Colloque Besançon-Neuchâtel, 2-4 octobre 1978. Mars 1978.
*35. P. Baldi, J. Moeschler: Comment contrôler le discours: interaction et réfutation dans le débat Giscard-Mitterrand (1974). Juillet 1979.
*36. (Collectif): Quelques réflexions sur l'explication. Février 1980.
37. M. Sanchez-Mazas: Traduction arithmétique des graphes et des relations binaires et applications logiques et informatiques. Juin 1981.
*38. (Collectif): Le discours explicatif I. Septembre 1981.
*39. (Collectif): Le discours explicatif II. Septembre 1981.
40. C. Wülser: Actes de langage explicatifs. Février 1982.
*41. (Collectif): Logique naturelle du raisonnement. Avril 1982.
*42. (Collectif): Linguistique et sémiologie I. Colloque Besançon-Neuchâtel, 5-6 octobre 1981. Juillet 1982.
43. (Collectif): Linguistique et sémiologie II. Colloque Besançon-Neuchâtel, 5-6 octobre 1981. Juillet 1982.
*44. (Collectif): Raisonnements et raisons. Avril 1983.

45. F. Albera: Problèmes de l'énonciation au cinéma. Février 1984.
46. (Collectif): Construction et transformations des objets du discours I. Colloque Besançon-Neuchâtel, 3-4 octobre 1983. Mars 1984.
47. (Collectif): Construction et transformations des objets du discours II. Colloque Besançon-Neuchâtel, 3-4 octobre 1983. Mars 1984.
*48. (Collectif): Analyse de texte assistée par ordinateur. Utilisation du logiciel DEREDEC. Janvier 1985.
*49. (Collectif): Problèmes et méthodes d'une analyse de texte articulant organisation cognitive, argumentation et représentations sociales. Juin 1985
50. (Collectif): Actes du colloque «Dialogisme et Polyphonie», 27/28 septembre 1985. Avril 1986.
*51. (Collectif): Le discours descriptif. Du texte aux objets de connaissance I. Juillet 1986.
*52. (Collectif): Le discours descriptif. Du texte aux objets de connaissance II. Juillet 1986.
*53. (Collectif): La référence. Points de vue linguistique et logique. Mars 1987.
54. D. Apothéloz, J.-B. Grize: Langage, processus cognitif et genèse de la communication. Septembre 1987.
*55. (Collectif): La schématisation descriptive. Types textuels, formes et fonctions discursives. Janvier 1988.
56. D. Miéville, R. Martin, A. Culioli, G.G. Granger, C. Gillièron, G. Seel, J. Molino, L. Frey, J.-B. Grize: La négation sous divers aspects. Actes du colloque, Neuchâtel 22-23 octobre 1987. Septembre 1988.
*57. D. Miéville, D. Apothéloz, P.-Y. Brandt, G. Quiroz, J.-B. Grize: La négation. Contre-argumentation et contradiction. Septembre 1989.
*58. M. Charolles: De l'art de nager et des différentes manières d'en parler. Septembre 1990.
*59. D. Miéville, M.-J. Borel, J.-P. Desclés, J. Gasser, P.-Y. Brandt; D. Apothéloz, J. Moeschler, J. Jayez, M.F. Blès: La négation. Le rôle de la négation dans l'argumentation et le raisonnement. Actes du colloque, Neuchâtel 11-12 octobre 1990. Septembre 1991.
60. D. Miéville, D. Apothéloz, P.-Y. Brandt: Les organisations raisonnées. Analyse de l'articulation de séquences discursives. Juin 1992.
61. D. Miéville, M. Chavaz, E. Gattico: Relations formelles et non formelles. Septembre 1993.
62. D. Miéville, C. Tiercelin, G. Heinzmann, G. Deledalle, J. Gasser, N. Everaert-Desmedt, J. Réthoré, M. Balat, J.-P. Kaminker: Charles Sanders Peirce. Apports récents et perspectives en épistémologie,

sémiologie, logique. Actes du colloque, Neuchâtel, 16-17 avril 1993. Avril 1994.

63. D. Miéville, J.-P. Desclés, P. Engel, J.-L. Gardies, J.-C. Gardin, J. Gasser, J.-B. Grize, F. Nef: Raisonnement et calcul. Actes du colloque, Neuchâtel, 24-25 juin 1994. Septembre 1995.
64. D. Apothéloz, U. Bähler, M. Schulz (éds), Analyser le musée. Actes du colloque international organisé par l'Association Suisse de Sémiotique (ASS/SGS), Lausanne 21-22 avril 1995. Août 1996.
65. D. Miéville, J.-L. Gardies, J.-B. Grize, O. Houdé, J.-P. Bronckart: Temps, logique et langage. Actes du Symposium tenu lors du colloque international «Penser le temps», Neuchâtel, 8-10 septembre 1996. Avril 1997.
00. A. Roulet Juan: Benno Besson en mouvement. Notes sur une mise en scène de «Lapin Lapin», comédie de Coline Serreau. Numéro spécial septembre 1998.
66. C. Salavastru: Identité et altérité. Les avatars de la rhétorique contemporaine. Novembre 1998.
67. D. Miéville, P. Joray, N. Gessler, B. Godart-Wendling, A. Bottani: Essais sur le nom et la nominalisation. Novembre 2000.

Les titres précédés d'un astérisque sont épuisés.
Les publications disponibles peuvent être obtenues auprès du Centre de Recherches Sémiologiques au **Fr.s. 15.-** , dès le n° 67 **Fr.s. 20.-** (TVA comprise).