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SOME ATTEMPTS AT A DIRECT REDUCTION OF THE INFINITE TO THE (LARGE) FINITE

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Abstract
I survey some endeavors which have been made to attain a sort of direct reduction of the usual notion of countable infinity to some reasonable notion of finiteness, in terms of non-standard arithmetic, feasibility, pseudo-models of derivations, Ehrenfeucht *models, etc. I maintain that although many interesting results have been obtained in these attempts, they ultimately show that (at least by the means considered here) no satisfactory reduction is possible.

1. *Introduction*

The aim of this paper is to survey and evaluate various attempts that have been made, by more or less non-traditional means, to explore the very possibility of giving a *direct* reduction of countable infinity to finiteness. The kind of attempted reductions in which we are interested have a straightforward character, they are made by brute force, namely by simply trying to 'squeeze' infinity into a finite, discrete object, typically a very big integer. Great efforts have been devoted by many authors to this apparently desperate enterprise — indeed, if anything ever deserved to be called 'a blanket too short', this is the 'finite/infinite' envisaged object — and a lot of very interesting results have been obtained along the way. We shall be able, in what follows, to look only at some samples of this vast and scattered literature, which ranges from little classics, such as Parikh (1971), to very controversial work, such as Esenin-Vol'pin's papers (one of which, however, was the object of an interesting exchange between Gödel and Bernays, which will be reported and briefly commented upon below, section 5). I chose on the basis of admittedly debatable preferences, trying to convey some significant ideas and to give only the gist of the arguments, without technicalities. We shall see that the efforts were indeed great; the achievement of the original goal, of course, is another matter. My conclusion, after a rather tortuous journey through quite exotic logical landscapes, will be (perhaps unsurprisingly) in the negative.

Before we start, some caveats are necessary. First, in this paper I will deliberately ignore some very important attempts at an *indirect* reduction of infinity to the finite: Hilbert's program and nominalist programs (e.g., those of Carnap, Tarski, Goodman, Quine, Field, Chihara, etc.). Secondly, it should be clear at the outset that in this work I am not interested in the arguments in favor or against strict finitism (the refusal of the idealization involved in the arbitrarily large finite, considered, among others, by Borel, Mannoury, Van Dantzig and Wittgenstein), although some of the authors whose ideas I discuss below are strict finitists (see Welti 1986 for a wide-ranging historical study of strict finitism). I am interested in a different problem, namely the possibility of 'modeling' (in some very non-traditional sense and not necessarily with eliminative aims) the infinite in the finite.

A further caveat is in order: it is well-known that a very special role in this area is played by *paraconsistency*, so that the reader might wonder why I will not deal, in spite of their interest, with programs based on paraconsistent logics (see, e.g., Van Bendegem 1994 and Priest 1997). At first sight, one could try to justify this choice by arguing that although some of the attempts which will be examined below produce systems which reveal 'a posteriori' highly non-traditional features, this is a sort of by-product, in the sense that they do not adopt from the start a system of logic admitting contradictions (in fact, proofs of consistency or 'quasi-consistency' are the target). If one compares the formal systems below with the usual systems of paraconsistent logic, one might argue that the former are syntactically and semantically 'closer' to classical logic (in terms of axioms and/or rules, interpretations, etc.), at least in their treatment of inconsistency, although (e.g.) restricting the length of proofs or pseudo-modeling single derivations has deeply non-classical effects. It is clear, though, that the distinction between classical systems with non-classical notions of proof or model (in a sense to be specified case by case) and non-classical systems is conceptually a weak distinction (and its weakness will become more and more visible in what follows). So, I simply decided to put paraconsistent systems on one side 'for the time being', concentrating on other approaches, less fashionable at present. The (in)competence of the author, the space limitations of a survey like this and the existence of excellent recent surveys on the paraconsistent approach (see, e.g., Priest 2003) will be (meager) justifications for my choice. Of course, one might argue that the failure of the attempts examined here is a further argument for the necessity of paraconsistency in this context.

The motivations of these attempts at reduction have different sources. Some of them are largely analogous to those of the nominalist programs, some analogous to those of strict finitism. But there could also be simple motivations of ontological nature (ontological economy, etc.), or of epistemological character: to explain our use of the infinite on the basis of an extrapolation from the indefinitely large, or simply the *very* large; to have a sort of 'conceptual' reduction of infinity to finiteness; to refuse the idealizations — already underscored by someone above all suspicion in this connection, namely Paul Bernays (Bernays 1935, 265) — involved in dealing with very large numbers and in the very idea of potential infinity, etc. There is, finally, the (unreasonable and unmentionable) desire to bypass the limitations of Gödel's theorem on consistency proofs, and to try to give shape to our vague feeling that 'we do set theory because we feel we have an informal consistency proof for it. This feeling is based on the fact that in any given case we are only speaking about specific sets defined by properties and by tracing back a contradiction we could eventually reduce it to the integers' (Cohen 1971, 14). Of course, Gödel's results taught us that what we can do in any given case can be very far from what we can do *in general* (see below, end of section 6), but the feeling remains. However, I shall not discuss these motivations here (they would require at least another paper), but only look at some examples of (tentative) mathematical fulfilment of the dream.

2. *A statement of the problem*

A set which is finite inside a model of Zermelo-Fraenkel set theory can be seen to be infinite 'from the outside'; but the converse cannot happen. This is the basic problem that I take as a starting point. The predicate 'x has n elements', for a fixed integer n, is absolute between models of ZF whatever. The *general* notion of finiteness, on the other hand, is absolute only between transitive models, and this engenders a certain degree of relativity. It seems, though, that the infinite remains in any case irreducible to the finite: we can realize that something which looks finite is in fact infinite, but not vice versa. On the other hand, one could object that non-standard natural numbers are infinite objects which behave exactly like finite ones, since they obey all the laws of first-order arithmetic. But even if one would be disposed to consider a non-standard model of arithmetic as the 'true' world, in which some supposedly infinite numbers reveal themselves to be finite, this is possible only in view of the relativity of the general notion of finiteness; the infinite natural numbers of any non-standard model satisfy all formulas $\neg(x = 0), \neg(x = 1)$, etc. In any case, it seems that no 'true' natural number, say 10^{12}, can be made infinite in any sense in any model. Seen 'from above', this phenomenon looks utterly devoid of mystery: ω is inaccessible, in the obvious sense that it is not the union of finitely many finite ordinals, and that if n is finite, 2^n is finite; while any natural number is, in the same sense, obviously accessible. Thus, we should look for some notion of pseudo-inaccessibility, perhaps in terms of unfeasibility (section 4), or of a vague, unapproachable boundary (a 'horizon', in Vopěnka's terms, section 8), in order to see as infinite what is really finite; or, alternatively, we should take a pseudo-finite number and then show that (almost) nothing is lost in treating it as finite but unfeasible, and modeling feasible numbers as the standard numbers, according to Kreisel's motto: feasible is to standard as standard is to non-standard (section 3). In sum, the point is the following: it is apparently obvious that no true integer can be made infinite in any way; is there a way to circumvent this impossibility, either by altering the notion of infinity (or of inaccessibility), or the notion of finiteness, or the notion of proof, or semantics (sections 6 and 7), or whatever? This is the subject of the following sections.

3. *Non-standard methods*

Non-standard analysis (see, e.g., Robinson 1974, and Robinson 1963 for applications to syntax) could apparently offer a twofold way to reduce directly the infinite to the finite. The first is the existence of hyperfinite natural numbers, objects which are indistinguishable from the true natural numbers, although they are in fact 'infinite' (there is no formal way, at first order, to isolate the standard numbers among the objects of any non-standard model). The second is the existence of non-zero infinitesimals, so that (e.g.) the set $\{c,\ c+c,\ c+c+c, \cdots\}$, where $c > 0$ is an infinitesimal, has no least upper bound (half a finite number remains an upper bound and twice an infinitesimal is greater than the infinitesimal). One could in some way use this phenomenon to generate a 'model' of the natural numbers in an arbitrarily small interval (a proposal in this spirit was perhaps first made by Rashevskii 1973). But both ways seem very problematic if one views them as attempts at a reduction of the infinite to the finite. In the first case, one has in fact a very complex structure, of order type $\omega + (\omega^* + \omega)\theta$, where ω^* is the reverse order of ω, and θ is a dense linear order without endpoints. All the objects there look like finite numbers, but the structure they live in cannot be reduced to the finite in any sense (it contains two typical aspects which allow or even require infinity, namely the non-well-foundedness of the order of the elements and the density of the order of the 'galaxies'). In the second case, we need the structure of the non-standard continuum, which is even more complicated. In this case, we would have a 'reduction' of the basic infinite structure to a much more complex infinite one. Even if we use the standard continuum we can well have (by means of trivial examples of limit processes) bounded structures in which we can 'embed' the denumerable infinite, but this is due precisely to the features of that continuum. From a certain viewpoint, non-standard analysis allows a sort of 'discretization' of the continuum, but for this advantage we pay the price of a much more complicated structure. But it seems that in no case we have any kind of reduction of the denumerably infinite to the finite.

Non-standard analysis perhaps also provides a possible interpretation of some aspects of the controversial proof-theoretic investigations on the consistency of ZF made by Esenin-Vol'pin within his 'ultra-intuitionistic' program, which could be better called 'ultra-finitist' (see Esenin-Vol'pin 1961, 1970, 1981; note that the name has been transliterated also 'Yessenin-Volpin' and 'Ésénine-Volpine'). This program remains very problematic, but it has the greatest importance with respect to our topic. The basic idea can be explained as follows (see Geiser 1974; the details, to our knowledge, have never been published, and cannot be easily reconstructed, so one has to be very cautious). The aim is a proof of consistency for ZF. We consider a structure T of all the hereditarily finite sets over a set of urelements, which is a model of ZF^{-} (ZF without extensionality) without the infinity axiom. We want to assert (as an axiom) that the designatum of a certain term t, which in fact is finite, is infinite. We get, of course, an inconsistent system, but all the same we try to define a notion of proof such that the length of the shortest proof of a contradiction is an increasing function of the size of t; this will allow a sort of (non-traditional) consistency proof for our system (see Geiser 1974). The basic idea of Esenin-Vol'pin in this connection was to use two different series of natural numbers: the first is longer, its length depends on the size of t, and is used to build the structure T; the second is shorter, though itself 'infinite', and (this is the key point) 'sees' t as infinite. Now, the role of non-standard analysis is to give a precise meaning to these vague intuitions: $\mathbb{N}$ (the ordinary natural number series) is used as the shorter series, ${}^*\mathbb{N}$ (the non-standard natural number series) is used as the longer one, and we give a pseudofinite (but in fact infinite) size to t. In this way Geiser (1974) obtains a consistency proof for a weak system of set theory with a Dedekind infinity axiom with respect to the non-traditional notion of proof employed. More precisely, he considers a notion of proof G_n for the hereditarily finite sets over a domain of n urelements, with a sort of ω-rule (from the set of formulas $\{A(t_i)\}$, t_i any closed term, infer $\forall x A(x)$), and then he extends the system to the case of a pseudo-finite integer n_0. Finally, he isolates G_{EV}, a subsystem of G_{n_0}, to formulate Esenin-Vol'pin's notion of proof, by considering a subset of the constant terms as the feasible ones (singled out in the metatheory by an indirect restriction to the standard integers, see ibid.), and putting a deduction tree T in G_{EV} if and only if only feasible terms occur in the subtree T* obtained from T by restricting the ω-rule to premises in which all t are feasible (this is nontrivial, since, in proving existential formulas, we may introduce new terms). On this basis Geiser defines a non-classical notion of proof G (see ibid.), which is consistent, closed under modus ponens, in which the law of the excluded middle does not hold in general, and which proves pairing, union, powerset, infinity, weak forms of comprehension and replacement, and all Σ^1_1 true sentences of second-order arithmetic.

We can now briefly explain the controversial 'consistency proof' for ZF originally proposed by Esenin-Vol'pin. Although the details are obscure, and sometimes lacking, the basic idea could perhaps be explained as follows (see Esenin-Vol'pin 1961, 1970 and Geiser 1974). The overall strategy is to translate the formal system S, whose consistency we want to prove, into another system S$'$, whose proofs are such that it is a priori certain that they will yield no contradiction (this is similar to what happens with Gentzen-style consistency proofs). We choose an integer k, and we consider ZF proofs of length at most k. Then we introduce two different natural number series, of different length (in the sense explained above), which can be represented respectively by the standard natural number sequence (employed as the shorter series) and the sequence of (finite and pseudo-finite) numbers less than or equal to a non-standard integer n_k (employed as the longer sequence). This allows the 'semantical interpretation' (in a non-traditional sense) of a corresponding suitable non-classical 'formal system' (see ibid.), the study of which yields the following result: no ZF proof of length at most k is a proof of a contradiction. Recall that k is an arbitrary integer, in the sense of an arbitrary element of a basic natural number series N which is chosen before the consistency proof and which constitutes the frame of reference for the general notion of finiteness adopted (both syntactic and semantical).

These results are interesting, but it is not at all clear whether we do really have a consistency proof for ZF, even on the basis of a system whose strength cannot be easily determined (and which of course has to be stronger than ZF, if we really prove consistency). We have the following dilemma. The first possibility is that we consider proofs of arbitrary finite length, and then we could well have a consistency proof, but in this case we have no reduction of the infinite to the finite, but only something which could point (but this is not clear) at the possibility of a hitherto unknown consistency proof for ZF by means of some (stronger) non-standard version of set theory (something similar happens, in a different context, with Hrbáček's non-standard set theory $\mathcal{NS}_3$, which proves the consistency of ZFC; see Hrbáček 1978, 6–7). The point is that, even in the case that we eventually had such a proof, the basic philosophical problem remains: the object satisfying the infinity axiom in these systems is in fact *infinite*, though pseudofinite, and this is a decisive obstacle to any reductionist interpretation of the results. The second possibility is that we consider proofs of length at most k, where k is a standard, finite integer, but then we no longer have a consistency proof, but only a proof of 'almost consistency', in the sense that the shortest possible proof of a contradiction will be non-feasibly long, and in this case the result could well be proved in a rather weak theory.

The second alternative will be the subject of the next section. As regards the first one, we recall that any formalization of non-standard analysis which has in fact been extracted (this is a very difficult task; it was attempted by Kreisel 1967, Ehrenfeucht-Kreisel 1967 and Geiser 1974) as a possible background theory of Esenin-Vol'pin's work gives a *conservative* extension of standard analysis (it is well-known that, in general, this is not always the case with formalizations of non-standard analysis), and this casts doubt on his results, since it seems that his principles are too weak for his purposes. It is true, though, that non-standard analysis provides only a very partial formalization of his ideas: the very notion of many natural number series, of different length, all of which are in some sense 'unending', and yet 'contained' (in some sense) in finite sets (finite from a classical point of view) is certainly very unorthodox, and it is dubious that non-standard analysis could explain it faithfully. Dummett (1975) gives a discussion of strict finitism which tries to make sense of such a notion, taking it seriously and connecting it with the Sorites paradox, vagueness phenomena and observational predicates. Dummett's conclusion is in the negative: he argues that the use of suitable predicates corresponding to the postulated sequences is intrinsically inconsistent, and this is sufficient for him to rule out strict finitism as a tenable position at all (for a thorough critique of Dummett's arguments, see Wright 1982).

A final remark on the use of non-standard methods in this context is in order. I will not deal here with some very important attempts at a *lato sensu* 'finitistic' foundation of non-standard analysis, the oldest of which is perhaps Nelson's 'internal set theory' (see, e.g., Nelson 1977). Such attempts could be relevant to our problem if one could make a reduction of infinity to hyper-finiteness, followed by a reduction of non-standard arithmetic to a finitistic basis. But nothing in Nelson's work shows that a true reduction could go through in this setting, despite his insistence on his purely syntactic view of the distinction between standard and non-standard objects, and on the fact that we simply have standard and non-standard elements inside the old classical set of the natural numbers (see ibid.). What we really have is hyperfinite (and not strictly finite) numbers, together with an axiomatic foundation of non-standard analysis based on non-finitistic principles. Nelson's work certainly provides a very interesting foundation of non-standard analysis, but not a reduction of infinity to finiteness. Similar remarks apply to other approaches, in spite of their intrinsic interest and their deep differences, at least as far as our problem of 'strict' reduction is concerned. To take only a few examples, with respect to our present viewpoint we would not consider as true reductions of infinity neither Baratella and Ferro's 'non-standard regular finite set theory' (see Baratella-Ferro 1995), based on a non-Cantorian notion of infinity (according to which a set may be considered as infinite by an observer if she is not able to recall its entire construction process), nor Andreev and Gordon's 'theory of hyperfinite sets' (see Andreev-Gordon 2006), admitting proper subclasses of large finite sets (see section 8 for this notion), nor, finally, a possible approach based on Vopěnka's 'alternative set theory' (Vopěnka 1979), which will be briefly considered below (section 8).

4. *Feasibility*

Another possible interpretation of ultra-finitism, a more straightforward one, is the one in terms of feasibility. As Kreisel remarks in his review of Esenin-Vol'pin 1961 (Kreisel 1967; see also Ehrenfeucht-Kreisel 1967), there are a priori various possibilities for those who seek a consistency proof for ZF on the basis of a notion of feasibility. The first possibility is to treat feasibility as a primitive notion and to adopt an informal approach by means of which we consider only feasible formulas and proofs of ZF. A more ambitious program 'would be to find a structure defined in terms of the new notion which satisfies all theorems of ZF' (Kreisel 1967, 9); this is not implausible, provided the envisaged notion of feasibility is sufficiently unfamiliar to transcend ZF set theory (in terms of definability or provability). One should find axioms and rules for the notion of feasibility, building up a formal system for it in the classical sense, and then give a finitist relative consistency proof of ZF with respect to the established formal system. There is also a third possibility: to consider a formal system for the notion of feasibility in which axioms and rules respect in their turn some feasibility restrictions, and to give a consistency proof of ZF (or a proof of consistency limited to feasible proofs in ZF) relative to it.

An explicit mathematical interpretation of ultra-finitism in terms of feasibility was first proposed by Parikh, in a paper (Parikh 1971) whose main interest was to introduce feasibility itself and to explore an 'anthropomorphic' view of mathematics, rather than to give reductionist arguments on the infinite. It is well-known that the very notion of feasibility is a priori problematic, in view of Sorites-like situations: 0 is feasible; for all n, if n is feasible so is $n+1$; thus, by induction ... This is the basic problem. Parikh's basic countermove is simple: we take a formal system for arithmetic in which very large numbers are dealt with (axiomatically) as though they were infinite (*without* redefining the basic arithmetical operations; see Burgin 1977 for an attempt in which this is done), but we prove that, nonetheless, as far as proofs of reasonable length are concerned, the formal system yields the correct results. It is not at all trivial to prove that this is the case, and Parikh was the first to do that, thus showing that feasibility could be treated as a respectable mathematical notion (his results were then generalized, e.g. by Dragalin 1985 and Sazonov 1995). In this way we could have systems (which Parikh calls 'almost consistent theories') which in the realm of finitist formulas differ from the traditional ones, and yet no contradiction in them would ever be met in our actual mathematical practice (see Pudlák 1996 for a survey of subsequent research on the length of proofs of statements asserting that there is no short proof of a contradiction, for various formal systems). Parikh makes a crucial point:

> Suppose we are given two sets, N_1 and N_2, which contain 0, are 'closed under successor', and are 'well-ordered' and such that N_1 is a proper subset of N_2. One feels at once that N_2 cannot be well-ordered [...] and that structures like N_2 arise because 'well-ordered' is not a first-order notion. What we would like to point out is that neither is the notion 'closed'. Clearly, having a notion of 'closed' presupposes that one already has a set of natural numbers and it cannot be used to define one. Thus it is quite possible that from the point of view of N_1, N_2 is not well-ordered and from the point of view of N_2, N_1 is either not closed or else not a proper set. (E.g. not 'internal' in the sense of A. Robinson) (Parikh 1971, 507).

In his own work Parikh provides an example of what happens when one takes the point of view which identifies the 'longer' sequence N_2 with the standard natural number sequence $\mathbb{N}$ (including its non-feasible members) and the 'shorter' N_1 with the non-closed 'sequence' of feasible numbers; while those who adopt non-standard methods (see the preceding section) take the point of view which identifies the 'shorter' N_1 with the standard sequence $\mathbb{N}$ and the 'longer' N_2 with a non-standard one (which, of course, cannot be in fact well-founded). But there is also the possibility to refuse to consider N_1 a 'proper set' (as Parikh remarks): this points not only at the 'internal/external' distinction, familiar in non-standard analysis (where, intuitively, internal sets are the only ones about which the formal language 'has information': e.g, $\mathbb{N}$ is not internal in $^*\mathbb{N}$), but also at Vopěnka's approach in terms of his 'alternative set theory' (see below, section 8).

Aiming at a more radical formulation of feasibility, Sazonov considers a variant of arithmetic, $PA_\Box$, with the largest natural number $\Box$ (see Sazonov 1995 and 1997; Esenin-Vol'pin was perhaps the first to study such theories, though they were suggested before, e.g. by Carnap, see Mancosu 2005). It is a version of Peano Arithmetic in the language for partial recursive functions and functionals, with corresponding axioms (i.e., recursive equations, in which second-order function variables can occur, but as free variables only; see Sazonov 1997 for the details), with a new constant, $\Box$, for the largest natural number and suitably modified successor axioms. $PA_\Box^\infty$ is the same theory with added the following axioms: $\neg(\Box = 0)$, $\neg(\Box = 1)$, etc. The basic idea is to consider a notion of proof such that a formula A is a theorem if and only if there is a *normal* classical natural deduction proof of A (i.e., intuitively, a proof without 'vicious circles' in the occurrences of introduction and elimination rules) such that each occurrence of any term in the proof has only a feasible number of symbols (in an intuitive sense of 'feasible', which can be made precise, see below). Even $PA^{\infty}_{\Box}$ is consistent with respect to this notion of proof, since any normal proof of a contradiction in it needs the occurrence of a term which cannot be physically inscribed or stored, simply because it is too long. In Sazonov's own words:

> $PA_{\Box}$ is actually a $\Box$-bounded arithmetic of $\Box$-recursive, or equivalently polynomial time computable global functions (relative to some second-order parameters) over the segment $U_{\Box} = \{0, 1, \cdots, \Box - 1, \Box\}$ with $\Box$ any indefinite, formally finite and possibly non-standard natural number [...]. We may take $\Box$ as some 'non-feasible' and 'concrete' (from the point of view of classical or intuitionistic mathematics) natural number, say $\Box = 2^{1000}$ [...]. In some non-traditional, though very natural sense, this axiom is consistent even with $PA^{\infty}_{\Box}$, if we assume that all (formulas in) formal proofs should be 'physically' written on a sheet of paper or in a computer memory. We can even imagine very informally a (second-order) model $U_{\Box}$ for the resulting theory, even though no universe for ZF has such a model as a legal object (Sazonov 1997, 100).

Here Sazonov refers to polynomial time computability over $U_{\Box}$ (see ibid. for the details). In this connection, it is right to recall an important historical fact: in the last few decades, the main interpretations of the very notion of feasibility have been given precisely in terms of computational complexity (polynomial-time computability, etc.) and corresponding formal systems (see Buss 1986).

The theories of Parikh and Sazonov could give examples of a possible foundation of the infinite upon the large finite (they deal with arithmetic and not with ZF, but they open the way to a possible 'finitization' of the infinite), but the price we have to pay is very high: one has to change the very notion of proof. One could maintain that nevertheless the *logic — stricto sensu* — is not changed, since both syntactically and semantically the formal systems considered remain classical; but this is a delicate matter, since in the end we do have inconsistent systems, in which disaster is prevented simply by considering suitably short proofs (instead of making other, more 'logical' prohibitions). In any case, if we interpret 'finite' as 'obtained in a feasible number of steps' and 'infinite' as 'obtained in a non-feasible number of steps' (though in fact finite), we are back to the case of proofs of bounded length (with fixed bound n), in which we simply apply the rules of formation and deduction only feasibly many times. This seems the only reasonable reductionist interpretation in terms of feasibility. Perhaps one will also need a somehow 'layered' notion of feasibility (see, e.g., Dragalin 1985), if one wants to represent in the finite all the objects dealt with in feasible ZF proofs. E.g., one has to accommodate various 'feasible' principles of construction of infinite cardinals currently available in ZF, and it is not at all obvious how to 'mimic' them in the finite, without too much distortion and without allowing disastrous shortenings in the proofs of contradictions (we have to take into account the enormous strength of the replacement axiom in this context — while the case without replacement seems more tractable at first sight, in spite of other well-known difficulties; however, to my knowledge, there is nothing on this topic in the literature). On the other hand, apparently no indefinite growth is needed: any imaginable iteration is bounded, and I do not see any compelling reason why one could not restrict oneself to a finite number of well-behaved rules of construction to be iterated, since (by hypothesis) one is content with modeling objects *feasibly* obtainable in ZF.

In any case, it seems that the notions of feasibility employed so far are not 'new' enough to permit a real foundation of the infinite on the large finite (a similar remark was made by Ehrenfeucht and Kreisel (1967) on Esenin-Vol'pin's alleged consistency proof). We have so far no really irreducible notion of feasibility, a notion whose properties are so 'strange' that we can hope to construct a structure defined by means of it in which *all* theorems of ZF hold, not only the feasibly provable ones. On the other hand (as Sazonov remarks), a notion of feasibility (once properly mathematized *without* reducing it to polynomial-time computability etc.) could be unfamiliar enough to be relevant in the future (in a way that we do not imagine at present) for the 'P vs. NP' question and other problems in complexity theory.

Another model of feasibility was proposed, independently of Parikh, by Engeler (1981, written in 1971), in order to give a reasonable formulation of strict finitism in 'algorithmic' terms, and also to explain, among other things, 'why, and in what fashion, finite minds can perceive infinite totalities' (Engeler 1981, 347). Engeler considers flowchart programs which generate hereditarily finite sets and verify their properties, with the crucial restriction that, given a program P, we consider only programs P^i, i an integer, in which each loop cannot be run through more than i times; in this sense Engeler speaks of 'the i-th mathematician'. A sentence is strict finitistically true if and only if for all sufficiently large i the i-th mathematician accepts the sentence, where acceptance is defined inductively in a reasonable way (see ibid. for the details) taking into account the limitations imposed on the programs P^i (e.g., a universally quantified formula is accepted if after i loops of the program composed of a generating program followed by a test program for the relevant subformula, all the tests, limited in number, have given positive results). The case in which we are interested is of course the one of the axiom of infinity. The basic idea is simple: since the i-th mathematician cannot repeat her tests more than i times, we can simply let her accept the 'infinity' of each suitably generated set of at least i elements. Note that the sentence (the conjunction) asserting that there is an infinite set (according to the usual ZF definition) and that no set is infinite (in the same sense), though having classically the form of a contradiction, belongs to the set of strict finitistically true sentences (thus intuitionistic logic is too strong for them, and Engeler concludes that his system is not relevant for the foundations of mathematics but only as a theory of feasibility in computer science). This formulation is interesting for its straightforward algorithmic character, though apparently it does not add anything conceptually new to the usual interpretations of strict finitism in terms of feasibility (recall, however, that Engeler originally formulated his proposal at the same time and independently of Parikh's work).

5. *Intermezzo: Gödel-Bernays exchange on feasibility and the work of Esenin-Vol'pin*

We find a brief but very interesting exchange on the work of Esenin-Vol'pin in Gödel's correspondence with Bernays in the years 1962–1963 (Gödel 2003, 204–233; see also Feferman's introduction, ibid., 57–59). I shall simply report this exchange here in its entirety, without detailed commentary, only to show the reactions of these eminent logicians in front of the first attempts which were made to develop the ideas we are discussing.

First, Gödel shows a skeptical attitude:

> As to Vol'pin's idea, I would very much like to see some, even just halfway plausible axioms about the concept of 'accessible number' [= feasible number] which imply the consistency at least of analysis. Are you aware of any such thing? It would also be really surprising if one could base mathematics (including number theory) on the insight that the concept of natural number is nonsensical (Gödel 2003, 209).

Bernays' reply is more charitable to Esenin-Vol'pin, but points at a crucial difficulty:

> In the applications of these considerations [the ones involved in Henkin's completeness proof] to set theory it is evident how far removed one remains from the characterization of an 'absolute'. The Skolem paradox asserts itself fully. This circumstance probably also

> brought Esenin-Vol'pin to his thoughts on sharpening the constructive standpoint. If it is possible — the thinking may have proceeded — to eliminate the impropriety in the axiomatic treatment of high infinities from proof-theoretic considerations, shouldn't it then also be possible to avoid the number-theoretic infinity in proof-theory? Here one need not at all have the view that 'the concept of natural number is nonsensical', any more than the proof-theorist needs to reject the concept of the uncountable as nonsensical. The foundational problem, however, is this: every way of making deduction precise apparently introduces already the number-theoretic infinity. Vol'pin account does not show, so far as I see, how this difficulty can be overcome. He introduces the postulate *Trad* (p. 205 [of Esenin-Vol'pin 1961]), which demands the existence of a 'natural sequence' N with the property that the operation 2^n is everywhere defined in this sequence, and this presupposition is used, if I understand correctly, in the sense that if n belongs to N, then 2^n also belongs to N (cf. p. 206, lines 23–24 [ibid.]). The difficulty that lies therein Vol'pin has of course also noted. Thus on p. 221 [ibid.] he says that the ultra-intuitionistic program requires above all the justification of the postulate *Trad*. It seems to me, though, that the same difficulty exists already in the requirement of the existence of the successor for each element of a 'natural sequence' (cf. p. 203, chapter [in the German original *Absatz* = paragraph] 3 [ibid.]). A kind of unsharpness, analogous to the Brouwerian unsharpness with respect to tertium non datur, would probably have to be introduced. It is hard to see how that is to be carried out theoretically, even though in actual fact such unsharpness exists when differentiating concrete sequences (e.g., those of acoustically perceptible degrees) (Ibid., 213–215).

Note that the (impossible) elimination of the number-theoretic infinite from proof theory is just the idea underlying subsequent work on pseudo-models of derivations (see below, section 6). Moreover, Bernays realizes that Esenin-Vol'pin's view per se neither presupposes nor implies the idea that the notion of natural number is nonsensical. Finally, Bernays acutely perceives the connection between the notion of feasibility and Sorites-like phenomena (this point was developed and discussed by Dummett, see above, section 3; it is also, independently, the key feature of Vopěnka's approach, see below, section 8), and the apparently overwhelming difficulties in making sense of the involved 'unsharpness'.

Gödel replies underscoring the differences of ultra-finitism with respect to ordinary finitism (whereas Bernays had noticed the similarities):

> The relation of Vol'pin to classical mathematics seems to me to be quite different from [the relation] of finitism to it. For Vol'pin assumes axioms about the concept 'accessible' that according to classical mathematics are false for every (even imprecise) concept, unless one gives up the concept of truth and speaks only of degrees of truth. But that seems to me incompatible with Vol'pin's idea of a consistency proof for classical mathematics [...]. P.S. With regard to the fact that for the consistency proof [of classical mathematics — G.'s footnote] Vol'pin nonetheless wants to use his axioms about accessibility only in connection with number theory (or combinatorics), I hold the following to be highly probable: if one somehow weakens the meaning of his axioms (e.g., also in the way you indicated) so that they become compatible with classical mathematics, then the existence of such a concept of accessibility (or its consistency with number theory) becomes provable in classical mathematics, which makes a consistency proof for it [viz., classical mathematics] impossible (Ibid., 223).

Here Gödel initially points at the radical 'strangeness' of the notion of feasibility with respect to the classical setting, and envisages (without endorsing it) a way out in terms of a non-traditional, 'graduated' notion of truth (which reminds us of Ehrenfeucht's nonclassical *semantics, see below, section 7). Then he makes a decisive point, which was (independently) also made by Kreisel (see above): a *tame* concept of feasibility might well be consistent with classical mathematics, but then it becomes too weak and completely useless for the original purpose.

Bernays concludes the exchange with a balanced remark, denying that Esenin-Vol'pin's assumptions are contradictory with respect to classical mathematics (they are only very restrictive — the proposal is a radical sharpening of the constructive viewpoint), remarking that the goal is a proof of '*almost* consistency' or 'feasible consistency' (see above), not of consistency *tout court*, and stating as an open problem the possibility of such a proof, a question that opens the way to the mathematical work that Parikh would have done a few years later (see above). Bernays writes:

> With respect to the investigations of Esenin-Vol'pin, you speak of the axioms that he assumes about the concept 'accessible' and that are false in the sense of classical mathematics. But in the version of his deliberations that is published in the Warsaw Congress volume

> [Esenin-Vol'pin 1961] I find no axioms at all that directly contradict classical mathematics; rather, only the rejection of many familiar assumptions of classical mathematics. Furthermore, there is a distinction from the usual proof-theoretic consideration, that it is not *consistency* per se that is to be proved, but only this: that a contradiction can only arise with a proof of a certain minimal length, which then no longer is viewed as a concrete one. Whether a consistency proof in this weakened sense is also excluded by your underivability theorem I have not yet pondered sufficiently (Ibid., 231).

6. *Pseudo-models of derivations*

We could also try to reconstruct Esenin-Vol'pin's idea by considering pseudo-models of derivations (Ehrenfeucht-Kreisel 1967), rather than the classical models of sets of formulas (with all their infinitely many logical consequences, or the infinitely many derivations from them). The idea is, as usual, to interpret terms defining infinite sets by means of large hereditarily finite sets. Given a certain derivation, its pseudo-model is required to 'pseudo-satisfy' only the formulas which actually occur in the derivation (as opposed to satisfying all the logical consequences of the formulas). There is an analogy (the extent of which is not clear) with Hilbert's ε-substitution method (Hilbert-Bernays 1939). Let us imagine to give a formulation of set theory in the ε-calculus, so that we have only propositional combinations of atomic formulas of the form $t = t'$ or $t \in t'$, where t and t' are ε-terms. Now, we can well have in any case an assignment of hereditarily finite sets to the terms occurring in any given derivation, in such a way that all formulas in the derivation come out true after evaluating atomic formulas. We simply replace distinct terms with distinct variables, take the conjunction of all the formulas occurring in the derivation, and consider the existential closure of that conjunction: by construction, it will be true in the universe of sets, and since any purely existential formula in the language of set theory which is true in a well-founded model is already satisfied by hereditarily finite sets, we can simply find by trial and error the required hereditarily finite sets, and obtain the pseudo-model we were looking for. The problem, in view of the envisaged reduction of the infinite, is not the existence of these pseudo-models, but the fact that it is not clear what is needed to prove their existence: Esenin-Vol'pin appeals to feasibility, but all the axioms on this notion which can be extracted from his informal treatment give, according to Ehrenfeucht and Kreisel (see ibid.), only conservative extensions of elementary arithmetic, and therefore are certainly insufficient for a consistency proof for ZF; we would need much stronger principles on the notion. If, on the other hand, we are satisfied with pseudo-modeling single derivations, then we succeed, but we give up the original quest for a real consistency proof.

A reconstruction of Esenin-Vol'pin's consistency proof in terms of pseudo-models of derivations was also given by Gandy (1982), in the following way. We take a concrete proof in ZF, and we assign to the bound variables in the proof finite (term) domains, by finding suitable witnesses, so that all steps in the proof come out true. We have a finite ordinal, W, to which all the designata of those witnesses which are *number* terms can be assumed to belong, so that it is sufficient to take a finite ordinal larger than all the elements of W as the designatum of the witness of the axiom of infinity (this finite ordinal will act as the substitute of ω), and hence suitable hereditarily finite sets as designata of witnesses in general. In this way, by assigning hereditarily finite sets as ranges to the bound variables, each step in the proof turns out to be true. But this procedure involves, in general, non-feasible computations.

> What Esenin-Vol'pin does is to introduce a notion ('F') of feasible number (and feasible hereditarily finite set). The idea is that the members of W will all be feasible, while ω will be represented by some non-feasible set. The feasibility of the members of W is to be secured by postulating that W (although finite) is closed under various functions (which will cover the relevant operations of witness formation). The plausibility of such postulates of feasibility is illustrated by an example: there were less than 10^{10} heartbeats in my childhood, but every heartbeat in my childhood was followed by another heartbeat in my childhood (Gandy 1982, 140).

Note that the latter example (originally given by Esenin-Vol'pin 1961 and discussed, e.g., by Dummett 1975, 313–317) is a classical one in the debates on strict finitism: it shows the possible connections between issues arising in these debates and those concerning *vagueness*. Is it *really* true that every heartbeat in my childhood was followed by another heartbeat in my childhood? Compare this example, on the one hand, with the case of birthdays in place of heartbeats, and on the other, with the case of shades of color — and, finally, why should all this be *mathematically* relevant?

However, Gandy's point is that if we simply take Esenin-Vol'pin's idea at face value, we would need a notion of feasible number closed at least under all the computable functions which are provably computable in ZF in a *feasible* number of steps, and this reference to feasible provability in ZF would introduce a possible vicious circularity. So, it is not immediately clear how to delimit the notion of feasibility in this approach. In general, the point is that the collection of 'concretely definable' numbers is an essentially *open* totality, somewhat similar (in this respect) to Cantor's 'absolute' (but one is reminded also of Brouwer's theory of the creative subject), since it contains 'numbers which will one day be given as yet quite unforeseeable concrete descriptions' (Gandy 1982, 134). However, Gandy concludes: 'the claim is that the highly elaborate theory of concrete mathematical activity which Esenin-Vol'pin has developed is both plausible and sufficiently powerful to prove the concrete (or feasible) consistency of ZF' (ibid.), thus having a chance to carry out one of the 'programs' based on feasibility hypothesized by Kreisel (see above, section 4), against Kreisel's own opinion.

The idea of pseudo-models of derivations was further developed by Isles in his work (see, e.g., Isles 1992; we are not interested here in the skeptical aims of his arguments against a univocal notion of natural number, but only in the positive side of his program; see also Cardone 1995). We build up non-traditional models, which must satisfy only the occurrences of formulas in a certain single derivation, disregarding their logical consequences. The interpretation of a formula depends even on its position in the derivation (this idea can perhaps be traced back to the medieval logician John Buridan), and we allow the assignment of suitable different ranges to the variables occurring there, in order to preserve consistency. If this is done in a suitable way, we can obtain in this non-traditional semantics finite pseudo-models for any set-theoretic theorem. But it seems that this cannot provide any reduction of infinity to finiteness, since in this way in fact we model (in a non-traditional sense) finite objects (the set of occurrences of formulas in a derivation) in finite (pseudo) structures, deliberately overlooking the potential infinity of consequences (both syntactic and semantical) 'implicitly contained' in a single formula, and so its full meaning (just think of the infinity axiom of ZF). If we want the generality required for a real relative consistency proof for ZF, we would need a universal construction of pseudo-models, which could be applied uniformly to all derivations, and this poses the problem of the irreducibility of these constructions to a single pattern, unless we have a theory stronger than ZF, which is just what one wants to avoid in this setting.

The latter problem can be illustrated with a simple example in an utterly classical context. Consider the following argument: (1) ZF proves the consistency of any finite subtheory of ZF; (2) ZF proves the compactness theorem for first-order theories; (3) hence, ZF proves its own consistency, and thus ZF is inconsistent. Of course, this is incorrect. And, of course, the point is to evaluate the exact meaning of (1). What cannot be done within ZF is to realize that in fact we have a uniform procedure to prove, given a finite subtheory of ZF, the consistency of that subtheory (by means of the local reflection principle). Given a specific subtheory, we can apply our procedure inside ZF; but we can realize the universal applicability of that procedure only outside ZF.

7. *Ehrenfeucht *models*

A very interesting proposal, explicitly traced back to Esenin-Vol'pin by its author, is the one which was originally given by Ehrenfeucht with his *models (Ehrenfeucht 1974), and then generalized by Parikh and others (see, e.g., Parikh 1999 and the references there). I shall give only a basic example. Consider the following set of structures: $R^* = \{R_1, R_2, R_3\}$. The domain of every R_i is $\{0, 1, 2, 3\}$, we have in each R_i identity, a designated element 0, and a single binary relation, whose graph is (respectively) the following: in R_1 the graph of the only relation is $\{(0,1),(1,2),(2,3)\}$, in R_2 it is $\{(0,3),(3,1),(1,2)\}$ and in R_3 it is $\{(0,2),(2,3),(3,1)\}$. Given a suitable definition of *truth (obviously unorthodox; see below), we have that in R^* the following sentences are simultaneously *true: 'every element has a successor', 'successor and predecessor are unique', 'there is exactly one element without predecessors', and 'there are exactly four elements'. Moreover, it can be proved that if S is a finite, (classically) consistent set of sentences, then S has a finite *model. The notion of *truth can be explained as follows (see Ehrenfeucht 1974 for the details). *Truth is defined in each case with respect to a set of structures, R^*, which is called a *structure. R^* must have suitable properties: the structures in R^* have common domain; they have certain designated elements (the same number in each); they are isomorphic, but the isomorphisms are not required to preserve the designated elements; the structures must have the same designated elements; finally, the same relations must hold among the latter. The notion of *truth is inductively defined, by means of assignments, in a substantially classical way, except for atomic formulas (an atomic formula is *true in R^* if and only if it is true in at least one structure R in R^*), and for the existential quantifier. This is the key point: an existential formula is *true in R^* under an assignment v if and only if there is R_1 in R^*, and an element e in the (common) domain of the structures in R^*, such that the relevant subformula is *true in R_1^*, under the assignment v_1 (which is exactly like v, but further assigns e to the quantified variable), where R_1^* is the *structure whose elements are the structures in R^* which are isomorphic to R_1 with an isomorphism which is the *identity* as far as the designated elements *and* the elements of the assignment v_1 are concerned.

Where is the infinite in this setting? Is this a true reduction? Recall that this proposal is not aimed at 'modeling' the set-theoretic axiom of infinity — which is the object of essentially all the approaches examined here — but rather assertions that imply the existence of infinitely many (finite) objects. By the way, note that the fact that the other approaches need *arbitrarily large* finite objects in order to represent arbitrarily large infinite ones is a non-issue for the reductionist: it would be decidedly too much to ask for something more than a reduction of the essentially *open* multiplicity of infinite sets to an analogously open multiplicity of finite ones. If we want to somehow ‘mock’ the set-theoretic infinities by means of finite objects, we should at least be allowed to use all the finite sets that we shall need (in the course of a process which cannot be imagined in advance), provided only we invent in any case suitable ways to describe them (recall Gandy’s comparison above, between concretely definable numbers and members of Cantor’s ‘absolute’).

Coming back to *semantics, note that it is *true that the *structure *satisfying the above propositions to the effect that there are infinitely many objects has *four* elements, so the semantics is highly unusual. E.g., in this semantics, when one verifies whether a certain sentence is true in a structure, the only elements which are really ‘visible’ during the verification are the designated elements. The two basic features of this proposal (explicitly recognized by its author) are the following: first, *non-iteration* — which reminds us of Sorites-like situations, in the sense that the trouble there arises from iteration of a locally innocuous step; secondly, *locality* — while there is a sense in which infinity is ‘non-local’: in this sense, the *structure above reminds us of those two-dimensional representations of ‘impossible’ geometric solids which are only locally, but not globally, geometrically and perceptively coherent (as cohomological considerations show). We could emphasize a third, decisive feature: (partial) circularity in the graph of the basic relation, which points at non-well-foundedness, apparently the true ‘way in’ of infinity in this context.

8. *Other approaches*

I shall briefly consider a few other approaches, perhaps even farther removed from the ordinary setting than the preceding ones. First, let us explain the notion of local finiteness. A theory is *locally finite* if and only if every finite set of its theorems has a finite model (see Lavine 1994; the original results were obtained by Mycielski, see e.g. Mycielski 1986). It can be shown that for every consistent first order theory T there is a locally finite theory Fin(T) such that provability and consistency are preserved in a suitable translation from one theory to the other. Moreover, it can be shown that the consistency of any first-order theory T is equivalent to the local finiteness of a corresponding Fin(T). In the case of ZF, which is obviously not locally finite, the idea is to translate the axiom of infinity by axioms in which the quantifiers are relativized to an infinite sequence of finite domains, Ω_0, Ω_1, etc. E.g., the axiom of infinity, formulated as follows:

$$(\exists x)(\emptyset \in x \wedge (\forall y)(y \in x \rightarrow \{y, \{y\}\} \in x)),$$

becomes, in the simplest case:

$$(\exists x \in \Omega_0)(\emptyset \in x \wedge (\forall y \in \Omega_1)(y \in x \rightarrow \{y, \{y\}\} \in x)).$$

Thus, x is closed under successor as far as Ω_1 knows, but the latter might not be closed under successor, and x may well be finite in reality. Mycielski (ibid.) claimed to have developed finite intuitions of the local finiteness of Fin(ZF) (from which consistency of ZF follows), but there is the same old problem of the generality of the proof, in this case involving local finiteness. One has to show that every finite set of theorems of Fin(ZF) has a finite model, and there is no way to do this uniformly, not even in ZF. As regards the conceptual reduction of the infinite to the finite, which is explicitly the purpose of Lavine, one should remember that the basic theory Fin (which is in a sense the common basis of all nontrivial Fin(T)) cannot have well-founded models, because of the 'potential infinity' of every postulated Ω_p, though every finite set of its axioms has a finite model of the form $\{0, 1, \cdots, n\}$.

Another attempt at the reduction of the infinite to the finite could be based on Vopěnka's Alternative Set Theory (AST, Vopěnka 1979). The idea is to develop a theory of classes in which all sets are in fact finite, but there are sets that include subclasses that are not subsets (proper subclasses or 'semisets'). Thus, proper classes are not necessarily larger than sets. Infinite sets are those sets which are not finite classes, where in general a class is finite if and only if all its subclasses are sets. This allows a straightforward treatment of problematic properties such as feasibility. E.g., if $F(x)$ means 'x is feasible' and n is not feasible, the set of feasible numbers less than or equal to n has classically the contradictory properties of containing 0, being closed under successor and not containing n, but in AST the separation axiom (a subclass of a set is a set) fails, so that the above defined alleged set is not a set. The idea was introduced mainly to deal with vague predicates, and it is founded philosophically on Vopěnka's notion of 'natural infinity', based in its turn on the concept of 'horizon' (see ibid.). As a reduction of the infinite to the finite an attempt based on AST seems promising at first sight, though no 'true' natural number can be made infinite in this way. But it is not easy to evaluate the meaning of the class/set distinction on the problem of reduction. One has the suspect that classes are introduced here as a means to treat uncertain boundaries, rather than infinities; in fact, even in the classical setting, proper classes can be viewed as inconsistent multiplicities, rather than the largest infinities (although they are always larger than sets, while this is not true in AST). Moreover, the most natural interpretations of AST are in terms of non-standard models of arithmetic, so that one is strongly tempted to reduce this approach conceptually to some non-standard background, although Vopěnka would reply that this would reverse the conceptual priority, since non-standard analysis is for him a (rather artificial) 'Cantorian' way to treat 'natural' infinity.

In intuitionist mathematics, the notion of finiteness is basically the following: a species is finite if and only if there is a constructive bijection between the species and an initial segment of the sequence of the natural numbers (see, e.g., Troelstra-Van Dalen 1988, 14). Moreover, a species is finitely indexed if it is the image under a constructive mapping of a finite species; and it is subfinite if it is a subspecies of a finite species. Note that subfiniteness and being finitely indexed are independent properties. Thus, there are classically finite sets whose finiteness is intuitionistically an open problem. Could this give another possibility for a reduction of the infinite to the finite (though it is well-known that this reduction is not at all a purpose of intuitionism)? I am strongly inclined to answer in the negative. First, no natural number could in any sense be considered infinite according to the above definition of finiteness. Even for other sets, whose status is still open (but one is usually able to prove intuitionistically at least that they are not infinite), we have no real gain, since once the set is shown (constructively) to be finite we have no longer the possibility to consider it 'infinite' from another point of view (intuitionistic logic and mathematics do not leave room for any kind of relativity phenomena in the sense of the various models of classical theories). On the other hand, until we (constructively) prove its finiteness, we do not have knowledge of its status, and certainly we cannot act as though it were known to be infinite (we have not a further point of view, but so to speak only the point of view of our maybe temporary ignorance). So it seems that intuitionistic mathematics does not provide examples of infinities which can be recognized, from another point of view, to be in fact finite.

As an aside, I recall that ordinal notations provide a way to represent effectively a segment of the second number class by means of natural numbers. In this way, ω (and in fact much larger ordinals) can be represented by finite numbers. E.g., in Kleene's system O (see, e.g., Rogers 1967) we have: $1 <^O 2 <^O 2^2 <^O 2^{2^2} <^O \cdots <^O 3 \cdot 5^{y_1} <^O \cdots$, where the first infinite sequence of terms gives notations for the natural numbers, in their order (of notations) $<^O$, and the following term is a notation for ω (among infinitely many possible notations), in which y_1 is the index of a partial recursive function which enumerates (in order) the notations of an infinite increasing sequence of natural numbers. But this is utterly useless in view of the reduction of the infinite to the finite. There is a sequence of notations which is infinite *in actu* below the finite notation for ω; we have here a way to 'reduce' a segment of the countable ordinals to the natural numbers, i.e. a 'higher' infinity to a 'lower' one, rather than the infinite to the finite. Similar remarks apply to Takeuti's 'discussion of ordinals from a finitist standpoint' (Takeuti 1987, 86–100), or any similar finitary treatment of a segment of the countable ordinals; however, these treatments have no strict reductionist aim.

Finally, it is well-known that the notion of Dedekind-finiteness does not necessarily coincide with finiteness in models in which the axiom of choice does not hold: there can be sets which are Dedekind-finite, but not really finite, there. But this is another unpromising attempt at a reduction of the infinite, since the envisaged Dedekind-finite sets are in fact infinite in every respect, except for the fact that they lack any bijections with proper subsets of themselves. That the latter feature can be considered as showing, after all, their finiteness (which is what we would need for a reduction) is, to say the least, dubious. In this connection, it seems that Mayberry's important (but rather idiosyncratic) work in the foundations of set theory, specifically his elaboration of a 'Euclidean finitism' (see Mayberry 2000), in which one assumes that every set is Dedekind-finite, is not directly relevant to our topic, although it allows the construction of different simply infinite systems (and hence different natural number sequences). In any case, it is not Mayberry's aim to reduce infinity to finiteness, but (among other things) to explore the possibility of developing a theory of infinite systems without axioms of infinity.

9. *Concluding remarks*

We have seen that when we consider the two main alternatives (among those we have discussed) which apparently allow one to make sense of a sort of 'modeling' of countable infinity in the finite, namely non-standard methods and feasibility, we face a dilemma. If we take into account proofs of arbitrary finite length, we might have consistency proofs, perhaps even a relative consistency proof of ZF with respect to some stronger non-standard version of set theory (though this is dubious, as we have seen), but we do not obtain any reduction of the infinite to the finite. On the other hand, if we consider proofs of length at most k, with k a standard integer, we have only proofs (possibly in relatively weak theories) of 'almost consistency', and we do not obtain real consistency proofs: we can only show that the length of the shortest proof of a contradiction is a non-feasible number. It seems that in any case we have to choose: either there is no finite upper bound on the length of the proofs we consider, and then we might have an interpretation in terms of non-standard models; or the maximal length of the proofs is in fact finite, and then we might have an interpretation in terms of feasibility. What is forbidden is to treat the length of proofs ambiguously, considering it as arbitrarily finite for the consistency proof, and then as determinately finite in our ultra-finitist metatheory. But this ambiguous treatment seems precisely what is needed to carry out a thoroughly reductionist program.

On the other hand, as regards the other attempts explained above, they do not seem to offer better prospects of success: pseudo-models of derivations seem to simply circumvent the original problem; Ehrenfeucht *models are based on a highly unusual semantics (perhaps just *too* unusual with respect to our present concerns); it is not clear to me how to make sense of the ideas of local finiteness and of proper subclasses of finite sets without recourse to non-standard models; finally, intuitionism is ultimately far from any reductionist aim. I conclude that none of the attempts that have been discussed here give any clue to the envisaged reduction of infinity to finiteness.

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