Tennenbaum's Theorem and Non-Classical Arithmetic

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written by

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Abstract

This works aims to address the significance of Tennenbaum's Theorem for the philosophy of model theory, from the perspective of non-classical inconsistent models of arithmetic. Several authors have recently argued that Tennenbaum's Theorem, when coupled with the claim that intended addition is computable, is capable of isolating the intended models of Peano Arithmetic up to a single isomorphism type. Such argument, which we will call *the argument from Tennenbaum's Theorem*, is particularly welcoming for a class of views in the foundations of mathematics that reject great epistemic access to mathematical objects. By focusing on a specific class of paraconsistent models of PA, as well as their features regarding cardinality and computability issues, we will argue that when pursued to its last consequences the argument from Tennenbaum's Theorem leads to very unintuitive results. In fact, we will show that the insistence on the computability of the intended addition function leads to placing inconsistent models in the class of intended ones. We discuss how this is an unwanted result for the advocate of the argument from Tennenbaum's Theorem and possible ways to block it. As we will see, the unexpected consequences are not so easily dismissed. We conclude that the argument from Tennenbaum's Theorem is too weak to establish its conclusion.

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Finally to my family. This has been my first experience studying abroad and I would not have done it without all their help. Here, in the attempt of avoiding to incur on the cliché of using a latin quote in the Acknowledgments Section, I do instead incur on the other also popular cliché of repeating previous dedications which the author likes. It is in this sense that:

To my family 'sem os quais não'.

[H]uman practice, actual and potential, extends only finitely far. Even if we say we can, we cannot 'go on counting forever'. If there are *possible divergent extensions of our practice, then there are possible divergent interpretations of even the natural number sequence* – our practice, our mental representations, etc., do not single out a unique 'standard model' of the natural number sequence. We are tempted to think they do because we easily shift from 'we could go on counting' to 'an ideal machine could go on counting' (or, 'an ideal *mind* could go on counting'); but talk of ideal machines (or minds) is very different from talk of *actual* machines and persons. Talk of what an ideal machine could do is talk *within* mathematics, it cannot fix the interpretation *of* mathematics.

Hilary Putnam, *Reason, Truth and History.*

[N]ot only my actual performance, but also the totality of my dispositions, is finite. It is not true, for example, that if queried about the sum of any two numbers, no matter how large, I will reply with their actual sum, for some pairs of numbers are simply too large for my mind – or my brain – to grasp. When given such sums, I may shrug my shoulders for lack of comprehension; I may even, if the numbers involved are large enough, die of old age before the questioner completes his question.

Saul Kripke, *Wittgenstein on Rules and Private Language.*

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Chapter 1

Introduction

Model is a notion that is easy to define. *Intended model* not so much. When attempting at a definition we might stress that a model $\mathcal{M}$ is intended of a theory Γ if it adequately captures our 'intuitions' about Γ. This much seems uncontentious. Focusing on the case of the natural numbers, we wish to say that the structure 0, 1, 2, ..., omnipresent in our everyday practices involving counting and computing, is indeed intended of arithmetic. That the standard model of classical Peano Arithmetic $\mathcal{N}$ or, perhaps more loosely, any structure isomorphic to $\mathcal{N}$ reflects our intuitions about the natural numbers. As a consequence, any non-standard model is unintended; the extra non-standard numbers are not constitutive of our normal understanding of *natural number*. Hence, we have the following thesis:

> **Thesis:** The intended model of arithmetic (PA) is just the standard model $\mathcal{N}$, up to isomorphism.

Even though intuitive, we see that when pressed for elaboration the claim is very hard to justify. Worse, *prima facie*, the Thesis is not able of mathematical proof. The reason why is indeed quite simply. Terms such as 'intuition', inevitably constitutive of the meaning of *intended model*, belong to vague ordinary language rather than to the language of mathematics. Of course this does not mean that there can be no (almost) conclusive evidence that decides the Thesis. For instance, many would state that the Church-Turing Thesis is equally informal (in the sense that notions as 'effective computability' do not belong to the language of mathematics), and yet also many mathematicians and philosophers of mathematics would agree that it is true. Not only 'intuition' is an informal notion, but, as we will see, it is very hard to accommodate the Thesis holding at the same time some very commonsensical views regarding realism and epistemology.

Despite the extensive philosophical scope and interpretative issues facing the set of papers nowadays included under the term *Putnam's Model-Theoretic Arguments*, one rather natural way to read Putnam's Arguments is arguing precisely against the ability of determining the intended model of arithmetic within a (epistemicaly) *moderate* and *realist* view. What Putnam shows is that a moderate realist is not able to explain why should the standard model (up to isomorphism) be considered intended, rendering the view highly unattractive. Chapter 2 will cover these issues setting the stage for the discussion to follow. We will carefully analyse the earliest versions of Putnam's Model-Theoretic Arguments (as they are presented in Putnam(1977) and Putnam(1980)), highlighting first the historical connections with earlier work already developed by

Skolem, and second the challenge that Skolem-Putnam's work presents to set theory and specially arithmetic. Here, we will also see how a shift to categorical theories of arithmetic and a focus on *categoricity* in general can help the moderate realist address the Skolem-Putnam's challenge.

Chapter 3 starts with the proof of Tennenbaum's Theorem. The result will be of central importance in that chapter and echoed throughout the work. Essentially what we will call *the argument from Tennenbaum's Theorem* assumes that in intended models of arithmetic the addition (and multiplication) function should be computable; this latter fact together with Tennenbaum's Theorem isolates the intended models up to isomorphism. According to the argument the intended model is just the isomorphism class that contains the standard model. However, and even though the literature on the topic is still very small, many have already criticised the argument due to the use of purportedly vague and circular notions like 'recursivity'. The argument makes use of concepts interdefinable with the 'natural numbers', bearing the charge of circularity; i.e. of assuming a determinate understanding of the natural numbers when trying to define what the natural numbers actually are.

In Chapter 4 we will present a new critique built from the point of view of paraconsistent arithmetic, using the logic LP. We will cover the main techniques behind the construction of finite inconsistent LP-models of PA. After covering the difficult topic of how to exactly define isomorphism between LP-models, we will motivate two main results: first, that there are finite LP-models without the same structure as (and, therefore, not isomorphic to) the standard model; second, that the addition function defined on these finite LP-models is effectively computable. Such results will be used to put forth what we will call *the LP-argument* leading to the claim that there are inconsistent but nevertheless *intended* models of arithmetic. The conclusions to take from the LP-argument are multiple and varied: two possible conclusions reintroduce the Skolem-Putnam challenge for the moderate realist, whereas the third option, called *Supplementation*, tries to save Tennenbaum's Argument by placing extra constraints on the class of intended models so to rule out the inconsistent ones. It is this third conclusion that will solely occupy us in Chapter 6 – there we analyse several ways in which Supplementation may be pursued and argue why they are not forthcoming. Before this, in Chapter 5 we quickly consider the notion of 'paraconsistent computation'; that is, a computational procedure not recognisable as such by non-paraconsistent logics. We will motivate the existence of these computable functions and explain away the strangeness of such a procedure by showing that this odd concept is actually a natural by-product stemming from paraconsistent arithmetic.

We will end this work with Chapter 7 where we repeat the main conclusions from our discussion and present some questions hopefully for future research.

Chapter 2

Skolem-Putnam's Model-Theoretic Arguments

2.1 Putnam's Model-Theoretic Arguments

Putnam's Model-Theoretic Arguments are essentially an *embarrassment of riches* problem against realist views: for any model $\mathcal{M}_0$ of a theory T that a realist might be thought as intended (of sets, natural numbers, cats, ...), Putnam employs fairly standard model-theoretic constructions to build a model $\mathcal{M}_1$ of T apparently unintended and yet satisfying those same relevant sentences or constraints. His is not a single argument but a collection of different techniques yielding non-standard interpretations. Following Bays(2001 : 335) *verbatim*, we stress that there is an underlying common structure shared by all the different model-theoretic arguments, captured by:

1. Premise: Theoretical and operational constraints do not fix a unique 'intended interpretation' for the language.
2. Premise: Nothing other than theoretical and operational constraints could fix a unique 'intended interpretation' for the language.
3. Conclusion: There is no unique 'intended interpretation' for the language.

It will be the aim of this chapter to explain the role of Putnam's arguments in set theory and arithmetic, what the targeted views are and how categorical theories may help address them. The *loci classici* and basis of our discussion will be Putnam(1977) and Putnam(1980).

We will pay most attention to Putnam's Skolemization Technique: pedagogically we find that Skolemization best illustrates the general structure of the model-theoretic arguments, having a long history whose details serve to shed light on Putnam's overall dialectic; further, even though its original target is the language of set theory, the techniques can be easily applied in arithmetic which will be the main focus of our work.[1]

[1] Even though we will try to maintain the technical terminology as standard as possible, the reader is invited to consult the Appendix for details on definitions and symbolism.

2.1.1 Skolemization

Putnam(1980 : 464) opens with a telling indication of the main inspiration for the model-theoretic arguments. There we can read 'In 1922 Skolem delivered an address before the Fifth Congress of Scandinavian Mathematicians in which he pointed out what he called a "relativity of set-theoretic notions".' The quote refers to Skolem(1922) where *Skolem's Paradox* is first presented together with the implication that '[...] set-theoretic notions are relative.' (Skolem, 1922 : 300)

Skolem's Paradox makes essential use of the Löwenheim-Skolem Theorem:

> **Löwenheim-Skolem Theorem:** Consider an infinite $\mathcal{L}$-structure $\mathcal{M}$. Then:
>
> 1. For any $A \subseteq M$, there is an $\mathcal{L}$-structure $\mathcal{H} \prec \mathcal{M}$, and $A \subseteq H$ with $\mid H \mid \leq \max(\mid A \mid, \mid \mathcal{L} \mid, \aleph_0)$;
> 2. For any cardinal $k \geq \max(\mid M \mid, \mid \mathcal{L} \mid)$, there is an $\mathcal{L}$-structure $\mathcal{N}$ such that $\mathcal{M} \prec \mathcal{N}$ and $\mid N \mid = k$.

The paradox is given by the fact that a standard first-order axiomatization of set theory, say, ZFC, on the assumption that it is consistent (and, therefore, that it has a (infinite) model), admits by the Löwenheim-Skolem Theorem a countable model $\mathcal{B}$. Since $\mathcal{B}$ is a model of ZFC it follows by Cantor's Theorem: $\mathcal{B} \models \exists x$ 'x is uncountable'. Then there is a $b \in B$ for which $\mathcal{B} \models$ 'b is uncountable'.[2] But since $\mathcal{B}$ is countable we also have that there are only countably-many $a \in B$ with $\mathcal{B} \models a \in b$; this leads us to expect that '$\neg$ (b is uncountable)' also holds. The appearance of contradiction quickly fades by noting that b is *really* countable, though the relevant surjective function lies outside the domain of $\mathcal{B}$ and so outside what $\exists^{\mathcal{B}}$ can 'see'. That is, the 'paradox' confuses two different levels of interpretation: on one interpretation of '$\exists x$ 'x is uncountable'' the quantifier ranges over the small domain B of $\mathcal{B}$ such that *within* the model $\exists^{\mathcal{B}}$ does not 'see' any surjection f from ω onto b; on another interpretation where the quantifiers are allowed to range over the entire set-theoretic universe, the quantifiers range over the relevant surjection. 'In a slogan: to be uncountable-according-to-$\mathcal{B}$ is not to be uncountable *simpliciter*. Paradox dissolved.' (Button & Walsh, 2018 : 177). Skolem's paradox hinges then on ambiguity: on the one hand, the claim that '$\exists x$ 'x is uncountable'' is made *within* the model where no such surjection is available, and '$\neg\exists x$ 'x is uncountable'' *outside*.[3]

Though the appearance of paradox is normally taken to be merely mathematically ill-founded,[4] Skolem proposes weighty consequences claiming:

[2] By 'x is uncountable' we mean a suitable formalization $\phi(x)$ in the language of set theory capturing the fact that there is no surjective function f from ω onto x. So $\phi(x)$ is an open formula with a free variable, and $\phi(b)$ the result of interpreting the formula in $\mathcal{B}$ and replacing x with b.

[3] We note that with the respect to Skolem's paradox obtained only through the Downward Löwenheim-Skolem Theorem, the ambiguity may also hinge on a deviant understanding of the membership relation interpreted within $\mathcal{B}$. A more subtle derivation of the paradox that locates the problem solely in the interpretation of the quantifiers involved, as explained in the main text, may be obtained by a strengthened version of the Downward Löwenheim-Skolem Theorem, the *Transitive Submodel Theorem*. See Bays, 2014 : § 1 for details.

[4] And Skolem is aware of this point: '[...] there is no contradiction at all if a set M of the domain B is nondenumerable in the sense of the axiomatization; for this means merely that *within* B there occurs no one-to-one mapping Φ of M onto Z_0 (Zermelo's number sequence). Nevertheless there exists the possibility of numbering all objects in B, and therefore also the elements of M, by means of the positive integers; of course, such an enumeration too is a collection of certain pairs, but this collection is not a "set" (that is, it does not occur in the domain B).' (Skolem, 1922 : 295)

> [...] axiomatizing set theory leads to a relativity of set-theoretic notions, and this relativity is inseparably bound up with every thoroughgoing axiomatization. (Skolem, 1922 : 296).

It is important to be clear on Skolem's understanding of 'axiomatization'. From Skolem's critique of Zermelo's own (conception of) axiomatization (see Skolem, 1922 : 295-296), something as an *algebraic* conception must be what he has in mind. This reading is further supported by the influence exerted on Skolem by the Schröder's algebraic school of logic[5] and, second, the overall intellectual *milieu* found in the 20s when an algebraic understanding of a theory's axioms was common-place (see Bays, 2014 : § 3.1). On the algebraic conception, set-theoretic notions are captured model-theoretically through the structures satisfying the axiomatization; the axioms serve to characterize (or even define) the notions they include: for example, a 'set' is just an element of the domain of a model and 'membership' a relation defined on the model. As a consequence, the axioms of set theory do not aim at pinning down a previously given 'intended' model; on the contrary, all those structures that satisfy the axioms can, in a sense, be counted as intended.

The crucial point is that, on the algebraic conception, basic notions are captured by the model theory of the axiomatization: if a formula has a *fixed* interpretation then it has an *absolute* meaning; *relative* otherwise. We should be careful in spelling out what we mean by *fixing the interpretation* of a formula or notion, on pain of triviality. For, in a sense, algebraic axiomatizations do make all set-theoretic notions relative: the membership relation in one model may be different than the membership relation in another; some object may stand for the empty-set in one model and for a singleton in another. Now, if by *fixing the interpretation* we just mean that a notion or formula must have the same denotation or value across different models, then (almost) every notion will turn out to be relative. Skolem's understanding of 'relativity' is more fine-grained than this. Granted that in a way 'empty-set' is relative: in the formula 'x is the empty-set', the denotation of x may change across models. However, if x is *really* the empty-set, then x will have to satisfy $\forall y(y \notin x)$ across *all* models. This gives us a sense in which we may fix and capture *absolutely*, within an algebraic axiomatization, what is for something to be the empty-set: x is the empty-set iff x satisfies $\forall y(y \notin x)$. What Skolem demonstrates is then that, even with this more fine-grained understanding of 'relativity', 'uncountability' is (still) relative: no matter the first-order axiomatization we use to capture the set-theoretic notion of 'uncountability' or the meaning of 'x is uncountable', the Löwenheim Theorem will always yield a countable structure. If, as the algebraic conception has it, what an element is is captured by its role *within* the model, there is a sense in which that x is *really* countable. As long as set-theoretic notions are characterized by looking at the model theory of first-order axioms, some notions like 'uncountability' will turn out to be relative. And Skolem repeats this conclusion almost *ad nauseam*:

> [...] on an axiomatic basis higher infinities exist only in a relative sense. (Skolem, 1922 : 296)
>
> Tous les concepts de la théorie des ensembles et par conséquent de la mathématique tout entière se trouvent ainsi *relativisés*. Le sens de ces concepts n'est pas absolu; il se rapporte au champ axiomatique basique. (Skolem, 1942 : 467-468)[6]

[5]For a quick introduction to Schröder's 'algebra of logic' see Peckhaus, 2009 : § 3.

[6]Every concept of set theory and as a consequence of mathematics as a whole are in this way

> More than 30 years ago I proved by use of a theorem of Löwenheim that a theory based on axioms formulated in the lower predicate calculus could always be satisfied in a denumerable infinite domain of objects. [...] As I emphasized this leads to a relativisation of set theoretic notions. (Skolem, 1955 : 587)

But even if set-theoretic notions are relative modulo the algebraic conception, Skolem leaves open the question if they are relative *tout court*, modulo every conception of 'axiomatization'. And here we may distinguish three readings of what Skolem is trying to do when presenting his paradox (see Bays, 2014 : § 3.1 for details and references therein):

1. On one traditional reading Skolem(1922) is a critique of set theory and set-theoretic notions in general. Skolem argues that the best way to define and understand basic set-theoretic notions is axiomatically, given that a mere 'naive' insight is not feasible due to Russell-like paradoxes. It is assumed that the only legitimate axiomatization is algebraic. Löwenheim-reasoning shows the relativity of set-theoretic notions. Conclusion: set theory is relative. This reading emphasizes Skolem's attitude towards set theory as an improper foundation for mathematics:

   > The most important result above is that set-theoretic notions are relative. [...] I believe that it was so clear that axiomatization in terms of sets was not a satisfactory ultimate foundation of mathematics [...] (Skolem, 1922 : 300-301)

   Further, in a remarkable passage worth quoting in full in the original french, Skolem even equates 'sets' more to fictions than to concrete or absolute notions:

   > Comme les raisonnements d'après toute axiomatique des ensembles ou d'après un système logico-formel se font de manière que l' absolu non-dénombrable n'existe pas, l'affirmation de l'existence des ensembles non-dénombrable ne doit être considérée que comme un jeu de mots, cet absolu non-dénombrable n'est donc qu'une fiction. La véritable portée du théorème de Löwenheim est justement cette critique du non-dénombrable absolu. Bref : cette critique ne réduit pas les infinis supérieurs de la théorie simple des ensembles *ad absurdum*, elle les réduit à des non-objects. (Skolem, 1942 : 468)[7]

2. A more toned down reading sees Skolem's Paradox has playing a modest role in the overall argument against set theory as a foundation. Skolem's Paradox does show the problems of the algebraic conception, but is silent on the role played by the Löwenheim Theorem regarding other conceptions of 'axiomatization'.

rendered relative. The sense of these concepts is not absolute; they are related with the basic axiomatique field. (Our translation)

[7]Since every reasoning concerning every axiomatic of set theory or one logical-formal system is made in a way such that the absolute nondenumerable does not exist, the affirmation of the existence of nondenumerable sets should only be considered as a play-on-words, this absolute nondenumerable is nothing but a fiction. The real consequence/meaning of Löwenheim's Theorem is precisely this critique of the absolute nondenumerable. In brief : this critique does not reduce the higher infinities of elementary set theory to *ad absurdum*, it reduces them to non-objects. (Our translation) Recall that by 'absolute' Skolem means a non-relative notion so that '*l' absolu non-dénombrable n'existe pas*' is meant as saying that uncountability is essentially relative.

3. A third way to understand the role of Skolem's Paradox is seeing it as highlighting the particular character of the notion of 'uncountable set'. Like the first option, it places the paradox in the larger project of attacking set theory as a foundation; and, like the second, it gives it only modest importance within Skolem's wide goals. Accordingly, the Paradox does not lead to the conclusion that set theory is completely relative but, on the contrary, only that there is no legitimate (i.e. non-relative) way to introduce the notion of 'uncountability' in mathematics. Skolem's Paradox is taken to establish that, in an algebraic understanding of the set-theoretic axioms, we do not need to accept uncountable sets since we may always choose to interpret the axioms within a countable model.

The main aim of Putnam(1980) is to extend the overall consequences of Skolem's Paradox by producing a genuine antinomy within philosophy of language (or of mathematics or of model theory), even though the Paradox itself might not be a genuine antinomy in mathematical logic. Putnam's *Skolemization Argument* defends that a theory whose *prima facie* intended interpretation is uncountable has by the Downward Löwenheim-Skolem Theorem a countable unintended interpretation. First, he shares the commonly acknowledged conclusion of Skolem's Paradox: that non-standard models highlight the fact that an 'intended' interpretation or 'intuitive notion of set' is not captured by the formal system of set theory, i.e. its axiomatization. Second, he argues that, by the same token, the collection of all our 'theoretical constraints' – understood as a collection of sentences which constitute our best theory of the physical world plus our best theory of set theory, cannot rule out unintended interpretations. Quite simply, on the assumption that the class of theoretical constraints can be first-order regimented, it will by the Downward Löwenheim-Skolem Theorem have a countable model. And given that we wish to say that the intended interpretation of the theoretical constraints must be uncountable (for they include the language of set theory whose intended model is, by assumption, uncountable), the countable interpretation is unintended:

> [...] even a *formalization of total science* (if one could construct such a thing) [...] could not rule out unintended interpretations, and, *a fortiori*, such a formalization could not rule out *unintended* interpretations. (Putnam, 1980 : 466).

Such a collection of sentences has a *countable* unintended model (assuming again that the intended interpretation is uncountable). By parity of reasoning, neither can 'operational constraints' – understood as the collection of all physical measurements we might come to make, fix the intended interpretation: 'we can find a countable submodel of the 'standard' model (if there is such a thing) [...] of our *entire body of belief* which meets all operational constraints.' (Putnam, 1980 : 466).

This gives us the first premise of the model-theoretic arguments:

> Premise: Theoretical and operational constraints do not fix a unique 'intended interpretation' for the language.

Now, Putnam argues that even the addition of extra-theoretical-cum-operational constraints cannot fix the intended model. Here, Bays(2001 : 341) remarks 'The heart of Putnam's defense [...] is the observation that the phrase "theoretical constraints" is broad enough to encompass *philosophy* as well as mathematics and natural science.' That is, any constraint on how set theory gets its intended interpretation can be seen as a new theoretical constraint up for reinterpretation through non-standard models. This means that no new attempts can fix the interpretation for, by the same techniques, we can find models that satisfy the original constraints together with the new requirement. Hence, any attempt to fix the reference of our mathematical vocabulary through the addition of extra-mathematical or empirical constraints on our mathematical language is bounded to form a broader characterization of our mathematical practices and, consequently, admits a (first-order) formalization interpreted with non-standard models. In the literature, this widely criticized strategy (for example, Bays(2008)) is known as the 'just-more-theory manoeuvre': attempts to fix the intended interpretation of our language by the addition of new requirements are seen as 'just-more-theory', i.e. just more first-order regimented sentences addable to the language and capable of being interpreted in a non-standard fashion.

This gives us the second premise of the model-theoretic arguments:

> Premise: Nothing other than theoretical and operational constraints could fix a unique 'intended interpretation' for the language.

The above strategy has motivated a form of model-theoretic scepticism that threatens our supposed ability to determine the intended models of our theories and whose original debt to Skolem is recognized with terms as 'Skolemite' or imaginary sceptical scenarios proposed by a certain 'Thoralf'. Normally, given a suitable mathematical notion, say, *set*, defined through a formal system T, the sceptic is fond of pointing out the existence of deviant non-standard interpretations of T that yield incompatible (i.e. non-isomorphic) extensions of *set*. If this relativity is unavoidable, germane to the axiomatization, then there is no principled way to discern one particular model as intended of the relevant notion. That is, if the theory which defines *set* does not determine a particular structure, no particular structure satisfying the theory can be counted as best capturing what is for something to be a set. We then obtain Putnam's desired conclusion:

> Conclusion: There is no unique 'intended interpretation' for the language.

Hamkins and the Skolemite

The set-theoretic relativity defended by Skolem and Putnam has attracted contemporary supporters. They illustrate how the consequences of the model-theoretic arguments affect matters of ontology, truth-value and the question of adding 'new axioms' in set theory capable of deciding independent statements.

Set-theoretically, skolemite scepticism is problematic for a universe view. The universe view admits a unique background conception of *set* whose intended extension corresponds to a single universe V with sets accumulating transfinitely, and where all statements (including CH) have a determinate truth-value. In this sense, independence phenomena are taken to be just a curious feature about provability – about the weakness of the specific theories in finding the truths holding in the universe – rather than the truths themselves that hold there. In a single universe, for good or for worse, CH will be either true or false, and independence results are seen, if not as a distraction, then at most as a hint of the needed supplementation of ZFC with stronger axioms capable of deciding the independent statements.[8] Forcing extensions of the universe or V-generic filters are considered merely illusory, a *façon de parler*

[8]An example of such a position would be Gödel(1947).

somehow paraphrased away, since V is already everything there exists. Skolemite reasoning however breaks with the pretension of characterizing V (up to isomorphism) as the intended model or the determinate extension of *set*: different models of *set* define, *prima facie*, equally legitimate extensions of the concept so that V is not to be prefered from a countable model $\mathcal{B}$. Recently, Hamkins(2012) has developed a view that noticeably respects the skolemite spirit. For Hamkins and his multiverse view there are distinct concepts of *set*[9], each instantiated in a corresponding set-theoretic universe within which there are different set-theoretic truths. He argues that the use of forcing extensions, inner models and other model-theoretic techniques made the models of set theory the principal objects studied in the discipline, in such a way that set-theorists have gained a 'robust experience' (Hamkins, 2012 : 418) of the alternative universes rendering difficult (if not completely *ad hoc*) to explain away these constructions as the universe view intends. The multiverse is a a view of (what is dubbed) higher-order realism: each universe exists in the very same Platonic sense that proponents of the universe view take V to exist.

Shapiro introduces a very helpful distinction between *algebraic* and *non-algebraic theories* (see Shapiro, 1997 : chap. 2). We say that a theory is non-algebraic if it aims at describing a particular structure or class of structures; a theory is algebraic if it is not non-algebraic. Model-theoretically, the distinction can be better sharpened thus:

> The model-theoretic framework allows a relatively neat distinction between algebraic and nonalgebraic branches of mathematics. A field is nonalgebraic if it has a single "intended" interpretation among its possible models or, more precisely, if all of its "intended" models are isomorphic (or at least equivalent). [...] A field is algebraic if it has a broad class of (nonequivalent) models. (Shapiro, 1997 : 50)

Examples of algebraic theories would then be, for example, group theory, or graph theory, not aiming at picking out a particular structure but rather at being applied to many different theories. The traditional view would classify arithmetic or set theory has non-algebraic in spirit; after all, talk of '*the* natural numbers' or '*the* model of set theory' carries the implicit commitment in one intended structure that *really* or *correctly* characterizes the natural numbers.[10]

Hamkins(2012) is normally taken to show that set theory is algebraic, in the above sense of the term. There is no intended model of the theory, but rather a collection of structures that stand on equal ground in terms of being *the* model of set theory. If Hamkins is here correct in inferring an algebraic view of set theory might be a controversial matter. However it is not our interest to critically address the burgeoning literature on the multiverse conception (for a comprehensive critique see Koellner, 2013), but only to illustrate how it can be seen to be an extension of Skolem's own aims and methods. With this latter goal then in mind, we note that the multiverse takes forcing extensions at face value (Hamkins, 2012 : 425): V-generic filters G do exist as well as well as the corresponding universes $V[G]$. The multiverse is a natural

[9] Hamkins notion of conception of *set* is too algebraic, seeing the notion has captured by the model theory of the relevant axiomatization: 'I shall simply identify a set concept with the model theory of set theory to which it gives rise.' Hamkins, 2012 : 417.

[10] Before we have also used the term 'algebraic' when referring to Skolem's conception of axiomatization. The two are easily associated – a theory with an algebraic axiomatization (in Skolem's sense) and an algebraic theory (in Shapiro's sense) are both a theory without an intended interpretation.

counterpart to the philosophical consequences of Skolem's Paradox: imagine $\mathcal{M}$ to be a model living somewhere in the multiverse and a some set in M; then, $\mathcal{M}$ has a forcing extension $\mathcal{M}[G]$ where a is countable. This gives substance to the idea that from a certain perspective or conception of *set* (recall, that a conception of *set* is equated with a model or class of isomorphic models of the theory) every set can be seen as countable, similar to Skolem's original idea that 'uncountability' is relative and model-dependent. When providing a set of principles for the multiverse, Hamkins then proposes:

> **Countability Principle:** Every universe V is countable from the perspective of another universe W.

Also, like Skolem, within Hamkin's proposal set-theoretic questions are rendered relative to the concept or universe one chooses to work with. The truth-value of set-theoretic questions – is CH true/false? – is parametrized to the background model. As a result it is not expected for set-theoretic statements to have a determinate truth-value either. In some models CH will be true and in others false. '[...] the answer to CH consists of the expansive, detailed knowledge set theorists have gained about the extent to which it holds and fails in the multiverse [...]' (Hamkins, 2012 : 429) Finally, Hamkins also agrees that set-theoretic relativity spreads up to other notions defined set-theoretically, in particular, arithmetic and arithmetical concepts. There is no prior reason as to why different set conceptions should agree on the concept of natural number. (Hamkins, 2012 : 427 - 428) Further, an observation to which we will return to below at some length, Hamkins recalls the familiar fact that Dedekind's Categoricity Proof for arithmetic is a full second-order claim dependent on a fixed concept of arbitrary subsets of $\mathbb{N}$ (in the meta-theory) and that for this reason any claim regarding a unique structure or determinacy of the natural number sequence should be made with respect to this more relative and dubious grasp of $\wp(\mathbb{N})$.

Despite all the similarities, Hamkins motivates his multiverse (and consequent set-theoretic relativity) not from paradoxical reasoning, but from actual mathematical practice. First, there is a sense in which the multiverse respects or makes justice to the set-theorist's 'robust experience' or belief in those other universes. Second, a universe view worried about 'simulating' universe extensions within V 'may miss out on insights that could arise from the simpler philosophical attitude taking them as fully real' (Hamkins, 2012 : 426) – forcing techniques are no longer served only for relative consistency proofs, but to study objects which are interesting by themselves; a universe view that dismisses them *tout court* may loose important mathematical insight. Third, a universe view imposes (arbitrary) constraints on the mathematician's work placing limitations on which kinds of universes might there exist. As Bays sees it, 'Hamkins does not argue that, because forcing extensions are *possible*, we are *stuck* with set-theoretic relativity; rather, he argues that, because forcing extensions are *natural*, we should *embrace* set-theoretic relativity.' (Bays, 2014 : § 3.3) Hamkins(2012) can then be seen as a natural development and illustration of Skolem's set-theoretic relativity.

Arithmetic and the Skolemite

It is an easy task to transfer Skolem's or Putnam's traditional model-theoretic scepticism about *sets* to *natural numbers*. Arguably, the most common axiomatization of the natural numbers is the axiomatics of first-order Peano Arithmetic (PA) being the one that we will be mostly interested throughout the rest of this work. Within the signature $\mathcal{L}_{\text{PA}} = \{0, S, <, +, \times\}$, PA describes the basic algebraic properties of addition and multiplication with an axiom schema for the validity of (first-order) induction:

Definition (Robinson's Arithmetic) Robinson's Arithmetic Q is the theory obtained by deductive closure of the axioms:
$\neg\exists y(S(y) = 0)$
$\forall x(x \neq 0 \rightarrow \exists y(x = S(y))$
$\forall x\forall y(S(x) = S(y) \rightarrow x = y)$
$\forall x(x + 0 = x)$
$\forall x\forall y(x + S(y) = S(x + y))$
$\forall x(x \times 0 = 0)$
$\forall x\forall y(x \times S(y) = (x \times y) + x)$
$\forall x\forall y(x \leq y \leftrightarrow \exists z(x + z = y))$

Definiton (Peano's Arithmetic) Peano's Arithmetic PA is the theory obtained by deductive closure of Q and the Induction Schema:

$$Ind(\varphi) := (\varphi(0) \wedge \forall x(\varphi(x) \rightarrow \varphi(S(x)))) \rightarrow \forall y\varphi(y)$$

where φ is an $\mathcal{L}_{\text{PA}}$-sentence.

$\mathcal{N} = \langle \mathbb{N}, 0, S, <, +, \times \rangle$ is the standard model of PA. It is an elementary result that PA admits models non-isomorphic to the standard model. The existence of non-standard models is proven with the Compactness Property for first-order logic.

Definition (Non-Standard Model) A non-standard model $\mathcal{M}$ of PA is a model of PA such that $\mathcal{M} \ncong \mathcal{N}$.

Definition (Categoricity) A theory T is k-categorical iff for any two models $\mathcal{A}$ and $\mathcal{B}$ of cardinality k, $\mathcal{A} \cong \mathcal{B}$.

Now, we quickly recall some classical theorems for first-order logic:

Completeness Theorem (Gödel 1930): Let T be an L-theory (a theory in the language of classical first-order logic) and φ an L-sentence. Then $T \models \varphi$ iff $T \vdash \varphi$.

From Gödel's Completeness it is easy to derive:

Lemma: T is consistent iff it is satisfiable.

Compactness Theorem: An L-theory T is satisfiable iff every finite subset of T is satisfiable.

With this little background in place we are able to show that PA admits non-standard models and, as a consequence, that it is not categorical.

Theorem (Skolem 1934): There exist non-standard models of PA.

Proof. We want to show that there is a model of PA non-isomorphic to the standard model $\mathcal{N}$. Augment the signature of PA, $\mathcal{L}_{\text{PA}}$, by recursively defining a collection of terms $\overline{n} \in \mathcal{L}_{\text{PA}}$ such that $\overline{0} = 0$ and $\overline{n+1} = n + 1$ (for $n \in \mathbb{N}$), and let c be a fresh constant symbol. Now, consider in the language of $\mathcal{L}_{\text{PA}} \cup \{c\}$ the theory

$$T = \{\text{Axioms of PA}\} \cup \{\neg(\overline{n} = c) \mid n \in \mathbb{N}\}$$

It is clear that every finite subset $T_0 \subset T$ is satisfiable. For since T_0 is finite it contains finitely many sentences of the form $\neg(\overline{n} = c)$. So, by letting m be the largest $\overline{n}$ such that $\neg(\overline{n} = c) \in T_0$, we can consider a signature-expansion $\mathcal{N}^+$ of $\mathcal{N}$ with $c^{\mathcal{N}^+} = m+1$. It follows that $\mathcal{N}^+ \models T_0$. Since T_0 was arbitrary, every finite subtheory of T is satisfiable, and by the Compactness Theorem T itself is satisfiable. Hence, there is a model $\mathcal{M}$ of T. Hence, $\mathcal{M} \models$ PA.

Now, it is left to show that $\mathcal{N}$ is non-isomorphic to $\mathcal{M}$. This is easily seen because in $\mathcal{M}$ the interpretation of c differs from the interpretation of each $\overline{n}$. So this element $c^{\mathcal{M}}$ must be a non-standard number since it is larger than any number picked out by a (standard) numeral. We conclude that $\mathcal{N} \ncong \mathcal{M}$, and $\mathcal{M}$ is non-standard. □

Countable models of PA have exactly two order types, since:

Theorem: If $\mathcal{M}$ is a non-standard model of PA, then, as an ordered set, $\mathcal{M} \cong \mathbb{N} + \mathbb{Z} \times \eta$, where $\eta \neq 0$ is a dense linear order without end-points.

Proof. See Kaye, 1991 : 6.2. □

From Cantor's back-forth construction we know that $(\mathbb{Q}, <)$ is the unique countable linear order without end-points. Hence, in the countable case, there are only two possibilities for η: either $\eta = 0$ and $\mathcal{M}$ has order type $\mathbb{N}$ as in the case of the standard model or $\eta = \mathbb{Q}$ and $\mathcal{M}$ has order type $\mathbb{N} + \mathbb{Z} \times \mathbb{Q}$ as in the case of the non-standard models. This is sometimes expressed by saying that PA has two countable models. Of course, by a theorem from Vaught no first-order theory can have *exactly* two isomorphism types, for a given cardinality. What is true is rather that the theory has exactly two countable order types. In fact, we can show that there are $2^{\aleph_0}$-many such countable models.

Theorem (Gödel-Rosser Theorem) Let T be a recursively axiomatizable L-theory extending Q. Then, there is a Π_1-sentence φ such that $T \nvdash \varphi$ and $T \nvdash \neg\varphi$.

With the Gödel-Rosser Theorem we can prove our desired statement.

Theorem: There are $2^{\aleph_0}$-many countable models of PA.

Proof. Let φ be the Gödel-Rosser sentence of PA. PA leaves φ undecided: PA $\nvdash \varphi$ and PA $\nvdash \neg\varphi$. Then, both PA $+ \varphi$ and PA $+ \neg\varphi$ are consistent and, by the Completness Theorem, they both have a model. So consider $\mathcal{M}_0 \models$ PA $+ \varphi$ and $\mathcal{M}_1 \models$ PA $+ \neg\varphi$. By the Downward Löwenheim-Skolem Theorem we can consider both models countable. We note that $\mathcal{M}_0 \not\equiv \mathcal{M}_1$ and, therefore, $\mathcal{M}_0 \ncong \mathcal{M}_1$. We repeat this process and consider the Gödel-Rosser sentences of the theories PA $+\varphi$ and PA $+\neg\varphi$. Call them φ_0 and φ_1, respectively. Again, there exist (countable) models $\mathcal{M}_{0,0} \models$ PA $+ \varphi + \varphi_0$ and $\mathcal{M}_{0,1} \models$ PA $+ \varphi + \neg\varphi_0$. Similarly, $\mathcal{M}_{1,0} \models$ PA $+ \neg\varphi + \varphi_1$ and $\mathcal{M}_{1,1} \models$ PA $+ \neg\varphi + \neg\varphi_1$.

We have $\mathcal{N} \models$ PA and, therefore, $\mathcal{N} \prec \mathcal{M}_0$ and $\mathcal{N} \prec \mathcal{M}_1$. Similarly, $\mathcal{M}_0 \prec \mathcal{M}_{0,0}$ and $\mathcal{M}_0 \prec \mathcal{M}_{0,1}$, and $\mathcal{M}_1 \prec \mathcal{M}_{1,0}$ and $\mathcal{M}_1 \prec \mathcal{M}_{1,1}$. We continue this process by letting x equal 0 in $\mathcal{M}_{a,....,b,x}$ when $\mathcal{M}_{a,....,b} \models \Gamma$ is an elementary substructure of $\mathcal{M}_{a,....,b,x} \models \Gamma + \varphi$, and x equal 1 when $\mathcal{M}_{a,....,b} \models \Gamma$ is an elementary substructure $\mathcal{M}_{a,....,b,x} \models \Gamma + \neg\varphi$.

Now, the cardinality of the set of functions from $\mathbb{N}$ to $\{0,1\}$ is $2^{\aleph_0}$. Then, given that we can associate each function $f \in \{0,1\}^{\mathbb{N}}$ to the index of a model $\mathcal{M}_n$ whose n is a sequence of 0's and 1's, it is clear that there are $2^{\aleph_0}$ extensions of PA.

Consider a model $\mathcal{A}$ for which $\mathcal{A} \prec ... \prec \mathcal{M}_i$ and $\mathcal{A} \prec ... \prec \mathcal{M}_j$. Then, since $i \neq j$, there is a formula for which $\mathcal{M}_i$ and $\mathcal{M}_j$ disagree. Then, $\mathcal{M}_i \not\equiv \mathcal{M}_j$ and $\mathcal{M}_i \not\cong \mathcal{M}_j$. Hence, $\mathcal{M}_k \not\cong \mathcal{N}$, for each $\mathcal{M}_k$ that extends $\mathcal{N}$. Since, for any k, $\mathcal{M}_k \models$ PA, there are $2^{\aleph_0}$-many countable non-standard models of PA. □

The existence of non-standard models provides the necessary resources for a full-blown model-theoretic scepticism. Intuitively, we wish to say that $\mathcal{N}$ (perhaps even up to isomorphism) is the intended model of PA and to rule out other structures from being constitutive of our concept of *natural number*. But this is precisely what the skolemite sceptic prevents us to do. If $\mathcal{M} \models$ PA for non-standard $\mathcal{M}$, then the model is as good a candidate to determine the interpretation of *natural number*. The challenge is then to explain which, if any, is the *intended* model of arithmetic.

A point seems uncontentious. Skolem takes the relativity of set-theoretic concepts as the infectious root of the relativity of mathematical concepts defined through the former, including arithmetic. And similarly the construction of non-standard models also leads Putnam to embrace the relativity of the natural number sequence.

> [...] if one desires to develop arithmetic as a part of set theory, a definition of the natural number series is needed and can be set up as for example done by Zermelo. However, this definition cannot be conceived as having an absolute meaning, because the notion set and particularly the notion subset in the case of infinite sets can only be asserted to exist in a relative sense. (Skolem, 1955 : 587)

> [...] le caractère vague de la notion d'ensemble. [...] Il est évident que ce caractère douteux de la notion d'ensemble rend aussi d'autres notions douteuses. Par exemple, la définition sémantique de la vérité mathématique proposée par A. Tarski et d'autres logiciens présuppose la notion générale d'ensemble. (Skolem, 1958 : 633)[11]

> If there are possible divergent extensions of our practice, then there are *possible divergent interpretations of even the natural number sequence* – our practice, or our mental representations, etc, do not single out a unique 'standard model' of the natural number sequence. (Putnam 1981 : 67)

2.1.2 Constructivization

By Skolem's Paradox the meaning of basic notions as 'x is uncountable' or 'x is finite' is not fixed by the model theory of first-order set theory, so that those expressions

[11] [...] the vague character of the notion of set. [...] It is evident that this dubious character of the notion of set also renders other notions dubious. For example, the semantic definition of mathematical truth proposed by A. Tarski and other logicians presupposes the general notion of set. (Our translation)

are rendered (semantically) 'relative' in the sense which we have been elaborating. If the theory cannot rule out non-standard interpretations then it cannot by itself fix or pin down the intended interpretation of its first-order language. From these by now over-repeated comments, Putnam infers the indeterminacy of truth-value of set-theoretic statements, focusing on the case of independent sentences. He writes: '[...] sentences which are independent of the axioms which we will arrive at in the limit of set-theoretic inquiry really have *no* determinate truth-value; they are just true in some intended models and false in others.' (Putnam, 1980 : 467)

For a given theory or domain of discourse T we wish to say that a statement φ is *really* true if the statement is satisfied by the intended model of the theory. On this view, unintended models where φ is falsified can be easily dismissed since they do not capture the theory itself correctly. However, if there is no principled way to determine which model is intended, then it seems that given two models of T that disagree on the truth-value of φ there is no principled way to decide if φ is true or false. This gives us the indeterminacy of truth-value in relation with φ.

Putnam's *Constructivisation Argument* attacks the determinacy of truth-value regarding set-theoretic statements. Gödel's 1938 result shows that $V = L$ is independent of the axioms of ZFC: on the assumption that ZFC is consistent, ZFC$+V = L$ and ZFC $+ V \neq L$ are equally consistent. Now, if what is needed to be the intended model is just to satisfy the axioms of the theory, then there is an intended model where $V = L$ is true and another where it is false, so that $V = L$ comes out indeterminate. This illustrates how non-standard models pose challenges to the determinacy of truth-value. Curiously, Putnam is (very) generous to the advocate of, what we can call, 'truth-value determinacy' and entertains the thought that empirical measurements might decide on $V = L$. For example, suppose we define a set $s \subseteq \mathbb{N}$ of an infinite sequence of tosses of a (idealized) random coin; to this end we say that $n \in s$ iff at the n^{th} toss the coin lands heads. Given that s is 'built' by a random process, there is no reason to suppose that s is a *definable* subset of $\mathbb{N}$, in which case s would be a non-constructible set yielding the falsehood of $V = L$. 'In this case, it might seem like *nature itself* manages to falsify the hypothesis that $V = L$.' (Bays, 2001 : 333) Now for a small theorem:

> **Theorem:** ZF$+V = L$ has an ω-model which contains any given countable set of real numbers.
>
> *Proof.* See Putnam, 1980 : 468.[12] □

Let OP be a *countable* collection of real numbers which codes up all the measurements human beings will ever make, including s (it is sensible to assume that human beings will make at most countably-many measurements). By the above theorem there is a model which contains OP (or, at least, a formal analogue of it). The model satisfies both ZFC $+ V = L$ and our measurement s. This means that even if s is

[12] However, as noted by Bays(2001), Putnam's proof contains a mistake. There is a step where Putnam needs to apply the Downward Löwenheim-Skolem Theorem to L producing a countable structure elementary equivalent to L and containing the respective set of real numbers. But this is an illegitimate move for the Theorem only applies to structures with sets for their domains, which is not the case for L. There are easy ways to fix the prove by moving to stronger theories; for instance, if Putnam admits the existence of inaccessible cardinals k, he can go on to say that L_k is a model of ZF$+V = L$ and apply the Downward Löwenheim-Skolem Theorem to L_k instead and prove his claim. For philosophical and mathematical elaborations on this issue see Bays(2001) and, more recently, Button(2011).

nonconstructible 'in reality', there can be a model accounting for s which satisfies at the same time the claim that 'everything is constructible'.

> [...] suppose we formalize *the entire language of science* within the set theory ZF plus $V = L$. Any model for ZF which contains an abstract set isomorphism to OP can be extended to a model for this formalized language of science which is *standard with respect to* OP – hence, even if OP is nonconstructible in "reality", we can find a model *for the entire language of science* which satisfies *everything is constructible* [...] (Putnam, 1980 : 468)

Again, the conclusion is similar as before. If only theoretical-cum-operational constraints fix the 'intended interpretation', there will be intended models with $V = L$. On the other hand, Putnam also assumes that there are interpretations of set theory (also compatible with any empirical measurements we may ever come to make) with $V \neq L$. Putnam concludes that the original skolemite relativity of set-theoretic notions extends to the relativity of truth-value of $V = L$ (and he also admits by similar arguments the truth-value relativity of the axiom of choice and CH).

The Constructivization Argument can also be applied in arithmetic. Recall that Constructivization consists in extending Skolem's relativity of mathematical notions to the relativity of truth-value of mathematical statements. Now, by Gödel's Second Incompleteness and Completeness Theorems, there are models $\mathcal{M}_1 \models \text{PA} + Con(\text{PA})$ and $\mathcal{M}_2 \models \text{PA} + \neg Con(\text{PA})$. If there is no intended model, model-theoretic scepticism will entail that the truth-value of $Con(\text{PA})$ is indeterminate: both $\mathcal{M}_1$ and $\mathcal{M}_2$ are two equally good descriptions of the natural numbers that decide on the question of consistency differently. A second comment, perhaps not related with truth-value but still worth making, remarked by Dean(2002 : 4), concerns the $\mathcal{L}_{\text{PA}}$-terms $Sent_{\mathcal{L}_{\text{PA}}}(x)$ and $Proof_{\mathcal{L}_{\text{PA}}}(x, y)$ expressing, respectively, 'x is the Gödel number of an $\mathcal{L}_{\text{PA}}$-sentence' and 'x is the Gödel code of an $\mathcal{L}_{\text{PA}}$-sentence with Gödel number y'. They are satisfied by arbitrary large standard numbers. Appealing to the Overspill Principle:

> **Overspill Principle:** Let $\varphi(x)$ be an $\mathcal{L}_{\text{PA}}$ formula. Let $\mathcal{M}$ be a non-standard model. If $\mathcal{M} \models \varphi(n)$ for all $n \in \mathbb{N}$, then there is a non-standard number a such that $\mathcal{M} \models \varphi(a)$
>
> *Proof.* Assume $\mathcal{M} \models \varphi(n)$ for all $n \in \mathbb{N}$ and let a be a non-standard number. If $\mathcal{M} \models \varphi(a)$ it's done. Otherwise we have $\mathcal{M} \not\models \varphi(a)$. Since $\mathcal{M}$ is a model of PA it satisfies the least-number-principle, so there is a least b that satisfies $\neg\varphi(x)$. By assumption b must be non-standard. Since $b \neq 0$ we have that there is a c such that $\mathcal{M} \models (S(c) = b)$. Then c is also non-standard. Since $c < b$ and b is the least $\neg\varphi(x)$, we have $\mathcal{M} \models \varphi(c)$. $\square$

The terms $Sent_{\mathcal{L}_{\text{PA}}}(x)$ and $Proof_{\mathcal{L}_{\text{PA}}}(x, y)$ will also apply to non-standard numbers in a non-standard model of PA and in this way they will have a non-standard interpretation. As a consequence, the model-theoretic sceptic leads us to conclude that the interpretation of these terms is equally indeterminate.

2.1.3 Permutation

Putnam(1977)'s *Permutation Argument* is based on the fact that isomorphic structures are very simple to come by. Given any structure that assumes the role of, say, *the natural numbers* and any bijection whose underlying domain is the domain of that structure, we may build an isomorphic copy of that initial structure:

> **Push-Through Construction:** Let $\mathcal{L}$ be a signature and $\mathcal{M}$ an $\mathcal{L}$-structure with domain M. Consider a bijection $\pi : M \to N$. The function π induces an $\mathcal{L}$-structure $\mathcal{N}$ with domain N by 'pushing' the assignments in $\mathcal{M}$ through π. That is, for every symbol s in the signature, we define $s^{\mathcal{N}} = \pi(s^{\mathcal{M}})$. More precisely:
>
> - For $\mathcal{L}$-constant symbol c, $c^{\mathcal{N}} = \pi(c^{\mathcal{M}})$
> - For n-ary $\mathcal{L}$-function symbol f, $f^{\mathcal{N}}(\pi(t_1^{\mathcal{M}}), ..., \pi(t_n^{\mathcal{M}})) = \pi(f^{\mathcal{M}}(t_1^{\mathcal{M}}, ..., t_n^{\mathcal{M}}))$
> - For n-ary $\mathcal{L}$-relation symbol R, $R^{\mathcal{N}} = \{(\pi(t_1^{\mathcal{M}}), ..., \pi(t_n^{\mathcal{M}}) \mid (t_1^{\mathcal{M}}, ..., t_n^{\mathcal{M}}) \in R^{\mathcal{M}}\}$
>
> By construction π defines an isomorphism between $\mathcal{M}$ and $\mathcal{N}$, so that $\mathcal{M} \cong \mathcal{N}$.

The Push-Through Construction then shows that, given a suitable bijection, isomorphic structures are easy to build. Perhaps more interesting, it gives us the following theorem:

> **Permutation Theorem:** Let $\mathcal{L}$ be a signature and $\mathcal{M}$ a *non-trivial* $\mathcal{L}$-structure with domain M. That is, $\mathcal{M}$ is such that:
>
> - $\mid M \mid \geq 1$; and
>   - there is an object denoted by a constant in the signature; or
>   - there is a non-empty non-universal relation defined on the model: that is, for $a_1, ..., a_n, b_1, ..., b_n \in M$ (with $a_i = a_j$ iff $b_i = b_j$) it is the case that $(a_1, ..., a_n) \in R^{\mathcal{M}}$ and $(b_1, ..., b_n) \notin R^{\mathcal{M}}$; or
>   - there is a non-empty non-universal function defined on the model: that is, for $a_1, ..., a_n, b_1, ..., b_n \in M$ (with $a_i = a_j$ iff $b_i = b_j$) it is the case that $a_n = f^{\mathcal{M}}(a_1, ..., a_{n-1})$ and $b_n \neq f^{\mathcal{M}}(b_1, ..., b_{n-1})$.
>
> Then, there is an $\mathcal{L}$-structure $\mathcal{N}$ with domain N such that $\mathcal{M} \cong \mathcal{N}$ and $M = N$ but $\mathcal{M} \neq \mathcal{N}$.
>
> *Proof.* Consider a non-trivial $\mathcal{L}$-structure $\mathcal{M}$. Define a bijection $\pi : M \to M$ such that either (a) for some constant c denoting an object in M, $\pi(c^{\mathcal{M}}) \neq c^{\mathcal{M}}$, or (b) for some non-empty non-universal relation R as defined above, $\pi(a_i) = b_i$ (for $1 \leq i \leq n$), or (c) for some non-empty non-universal relation f as defined above, $\pi(a_i) = b_i$. Let π induce an $\mathcal{L}$-structure $\mathcal{N}$ by a Push-Through Construction. By construction, $\mathcal{M} \cong \mathcal{N}$. Further, since the bijection is a permutation, the domain of $\mathcal{N}$ is just M. Finally, by the constraint imposed on π, $\mathcal{N}$ disagrees with $\mathcal{M}$ with respect to the interpretation of (at least) a constant or relation or function symbol. So $\mathcal{M} \neq \mathcal{N}$. $\square$

The Permutation Theorem assures that a (first-order) theory with a non-trivial model has many different isomorphic models sharing the same domain. It is then easy to see how given a model of arithmetic, many isomorphic copies can be produced. Since the copies are isomorphic, they will be elementarily equivalent and satisfy the same arithmetical formulas, making it harder to isolate one particular model as the correct one. Now, since the models are distinct they will disagree on the interpretation, i.e. the reference of some symbol. This raises the philosophical challenge of deciding which model better explains or describes the reference relation between word-numbers and actual numbers. We can take the word-number '3' to refer to the third successor of the natural number sequence or, by a permutation argument, to the seventh. So when we consider the term '3' there is no single object to which the term (necessarily) refers; '3' can be made to refer to any natural number. So what can ever fix the (intended) referential relations?

Benacerraf and Push-Through

Benacerraf uses the Push-Through Construction and, more generally, the various isomorphic or elementarily equivalent models of arithmetic to show that we should be wary in identifying natural numbers with particular (set-theoretic) constructs rather than isomorphism types. His analyses is the first cornerstone in a possible response to the model-theoretic arguments.

Benacerraf(1965)'s main aim is to attack an 'object-reductionism' view from natural numbers to sets. Without aiming at exhaustiveness, object-reductionism from a domain of objects A to B consists in successful translation of talk about A-objects – a formula $\varphi_A(x_{a_1}, ..., x_{a_n})$, to talk about B-objects – $\varphi_B(x_{b_1}, ..., x_{b_n})$, showing that reference over the first kind can be seen as, or reduced to, reference over the second. In the case of set theory, the canonical view would reduce natural numbers to set-theoretic entities. So, if numbers just are von Neumann ordinals, a numerical expression exhibiting explicit reference to numbers as in the case of '$0<1$' is paraphrased away through its set-theoretic counterpart '$\emptyset \in \{\emptyset\}$'; or '$S(0) = 1$' through '$\emptyset \cup \{\emptyset\} = \{\emptyset\}$'. Now, Benacerraf focuses on two prominent reductions of the natural number sequence: sure, numbers may be identified with von Neumann ordinals with $0, 1, 2, ...$ being translated by $\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}$; still, the reduction to Zermelo ordinals $\emptyset, \{\emptyset\}, \{\{\emptyset\}\}$ is equally justified.[13] It is hard to discern that which may make the first translation better that the second, for once $+$ and $\times$ are defined recursively in the usual way, both reductions will agree on which arithmetical sentences are true. It is only in contrast with extra-arithmetical questions like $2 \in 4$ that the two definitions stop being co-extensional: in the von Neumann reduction $2 \in 4$, in the Zermelo $2 \notin 4$. Now, it cannot be the case that both reductions are correct. Assuming, classically, that identity is transitive, if we identify 2 with an object a it cannot be the case that 2 is also equal to an object b different from a. But it is also true that both reductions seem equally legitimate in such a way that no identification is to be preferred. 'So we are left without an answer to the question of whether 2 is really a member of 4 or not. Will the real 2 please stand up?' (Shapiro, 1997 : 5)

At this point, one position that the 'object-reductionist' might take is to claim that there is indeed a *correct* reduction – natural numbers *really* are *this* kind of set, say, von Neumann ordinals, but no argument can establish that natural numbers are von Neumann ordinals rather than Zermelo's. But it is argued that this move just pushes the notion of *correctness* too far: 'The notion of "correct account" is breaking loose from its moorings if we admit of the possible existence of unjustifiable but correct answers to questions such as this [i.e. the reduction of numbers].' (Benacerraf, 1965 :

[13]For finite von Neumann ordinals the successor function maps each object to the union of itself and its singleton: $s(n) \mapsto n \cup \{n\}$; for finite Zermelo ordinals the function maps each object to its singleton: $s(n) \mapsto \{n\}$.

58) If the notion of ‘correctness’ of an identity statement is to make sense, there has to be some ground on which to judge that pretension to correctness; there must be arguments that support it, otherwise, if has Benacerraf shows nothing can pick out the correct account from other candidates satisfying the relevant correctness criteria then talk of a ‘correct’ interpretation stops making sense.[14]

To repeat, if the reduction from numbers to sets is to be successful, or even intelligible, then (a) numbers must be identified with only one particular kind of set (again, we cannot have $3 = \{\{\{\emptyset\}\}\}$ and at the same time $3 = \{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$ on pain of violating transitivity of identity), and (b) it must be possible to provide a reason as to why numbers must be identified with *that* particular kind of set. The latter requirement follows from the idea that talk of unprovable or unknowable ‘correct’ equalities is just unsatisfactory. But there are different possible reductions of the natural numbers satisfying our criteria for correctness. As a consequence, there is no cogent reason to prefer one reduction over another and, hence, requirement (b) comes out unsatisfied. If the above follows, we have that the identification between 3 and a von Neumann ordinal is bound to be merely arbitrary, and, more generally, any identification or reduction between 3 and a particular set misguided.

Benacerraf’s solution consists in maintaining that numbers are not sets[15]; in fact, by the same token, numbers are not objects for there does not seem to be any reason to identify a number with a particular object rather than with another isomorphic copy. On the contrary, what is particular to the number 3 is its role in the natural number sequence, i.e. that of being the third element of the sequence. So there is no *particular* object that can be thought of as 3. In fact we saw by the Push-Through Construction that any object can be the third element of a model of arithmetic and therefore can be made to refer to ‘3’. This all suggests that what is important about natural numbers is their general role in the natural number sequence. As Benacerraf argues, arithmetic is not about a particular collection of objects, but rather about a particular collection of *system of relations* determining a class of structures:

> Arithmetic is therefore the science that elaborates the abstract structure that all progressions have in common merely in virtue of being progressions. It is not a science concerned with particular objects – the numbers. The search for which independently identifiable particular objects the numbers really are [...] is a misguided one. (Benacerraf, 1965 : 70)

Against this background and recovering Shapiro(1991)’s terminology, Benacerraf(1965) can be read as pushing the transition from a non-algebraic view of arithmetic to an algebraic one – to insist on the obvious, Benacerraf shows that arithmetic is not about a single structure but a class of isomorphic *adequate* structures.

[14]On this precise point, Paseau has recently argued *pace* Benacerraf that it is preferable to arbitrarily choose one between two (or more) equally good possible set-theoretic reductions than to refuse to perform the reduction at all. His claim is due, among several reasons, to the economy that such a reduction from numbers to sets would afford: ontological economy – reductionism is a less ontologically committing view of mathematical entities; ideological economy – reductionism reduces the amount of primitive terms presupposed by a theory; axiomatic economy – reductionism reduces the amount of basic principles. See Paseau(2009).

[15][...] there is little to conclude except that any feature of an account that identifies 3 with a set is a superfluous one and that therefore 3, and its fellow numbers, could not be sets at all. (Benacerraf, 1965 : 62)

2.2 Categoricity and the Skolemite

2.2.1 The Moderate Realist and Categoricity

According to Putnam, the model-theoretic arguments are not, by themselves, meant to be problematic for *every* view on the foundations of set theory/arithmetic. Putnam(1980) provides a special classification of foundational positions based on different views regarding reference and truth: first, the extreme Platonist position 'posits nonnatural mental powers of directly "grasping" forms (it is characteristic of this position that "understanding" or "grasping" is itself an irreducible and unexplicated notion)'; second, 'the verificationist position which replaces the classical notion of truth with the notion of verification or proof'; third, 'the moderate realist position which seeks to preserve the centrality of the classical notions of truth and reference without postulating nonnatural mental powers'. (Putnam, 1980 : 464)

Suppose we run through the model-theoretic arguments to a philosopher of mathematics. If she is what we called an extreme Platonist, she might appeal to a magical nonnatural inherent "grasp" of the natural numbers to determine which model is intended. Problem solved! If she is prone to some sort of verificationism (i.e. in identifying truth with verifiability, broadly construed) then she will just say that our understanding of 'The real numbers are nondenumerable' consists in our knowing what does constitute a proof of the claim, and not in the 'grasp' of a 'model'. Problem solved! In the rest of the paper, however, we will be interested in analysing the model-theoretic arguments through the eyes of the *moderate realist* since it is for her that the arguments pose a greater challenge.

The moderate realist is first of all two things: moderate and realist. As a realist, she believes in genuine abstract mathematical objects, independent of the mathematician, taking mathematical statements at face-value. She does not wish to paraphrase away the implicit existential commitment present in mathematical vocabulary. When she hears 'There are infinitely many reals between 0 and 1' she understands that there really are infinitely many reals between 0 and 1. Further, she is epistemically moderate: she rejects any notion of *faculty of mathematical intuition* or *intuitive grasp of mathematical objects*.[16] It is easy to see why, at first sight, the moderate realist view is attractive. First, unlike the verificationist, it takes mathematical statements at face-value preserving the classical accounts of truth. Second, unlike the extreme Platonist, it fully dispenses with appeal to mysterious faculties that seem 'both unhelpful as epistemology and unpersuasive as science. What neural process, after all, could be described as the perception of a mathematical object?'. (Putnam, 1980 : 471) Still it is she who falls prey to Putnam's challenge for she faces the following dilemma: on the one hand, she believes that mathematical entities are real abstract entities and 'since they are abstract, she accepts we cannot fix reference to mathematical entities by seeing them, pointing to them [...]' (Button & Walsh 2018 : 43); on the other hand, non-standard models are *prima facie* not intended models and, therefore, she will need a principle capable of ruling out these structures, without incurring on talk about (causal) epistemic access to the natural numbers. The moderate realist may insist that, say, the standard model is *preferable* in our formalization of PA; but given precisely her moderation she cannot justify such preference as the extreme Platonist

[16]Though this may be a very general characterization of the view it is doubtful that a more sharp definition is available, for *moderate realism* is not a position ever actually elaborated or defended in full detail but the general basis of a framework in contemporary philosophy of mathematics that may be pursued or fine-grained in various ways.

does by postulating an 'intuitive grasp' of the natural number sequence, being very hard to see how 'preferability' could be spelled-out otherwise.

Now, we saw how Putnam's challenge and Benacerraf's arguments may push a change from a non-algebraic to an algebraic view of arithmetic. This suggested that the true objects of arithmetic are the structures satisfying our preferred axiomatization of the natural numbers. Of course not all structures are intended and it is hard to defend the addition of extra-constraints capable of fixing the interpretation from a moderate realist perspective. Yet, if we let the mathematical theory itself to fix the meaning and truth-value of its own mathematical vocabulary and statements, Putnam's challenge may be addressed in a way that respects moderation. And we can give substance to this idea through the notion of a *categorical theory.*

A theory is categorical iff all of its models are isomorphic. If we think of a theory as picking out its models, then a categorical theory will pick out a single isomorphism type.[17] Now, if the theory is categorical, its intended class of models will be determined by the theory itself: to the extent that the theory only has an isomorphism type, there is only one single class of structures that can be counted as intended of the theory. Given the natural identification between mathematical objects and isomorphism types *from an algebraic perspective of arithmetic*, the moderate realist will then be able to defend that the intended models of the theory, what those mathematical objects *really* are, is captured by the structure up to isomorphism satisfying it. Hence, in the case of a categorical theory of arithmetic, she will be able to discover or identify the true natural numbers without great assumptions on our epistemic access to numbers: as long as we are able to understand the theory itself (its axiomatization) and the fact that it is categorical, we know what *really* counts as an intended natural number sequence. As a consequence, there will be no alien non-standard models since every model will have the same structure – this takes care of Skolemization. We will see in a moment how sameness of truth-value is also achieved, tackling Constructivization. For now we note that this view does not completely address the Permutation Argument: assume that we think that there is an isomorphism type that corresponds to *the* natural numbers. Regardless, no object can be thought as, say, the element 27 of the sequence – by a push-through construction every object can be the 27^{th} element of some (standard) model of arithmetic, such that the term '27' does not pick out a single object. 'Indeed, on this view, if '27' refers at all, then it surely refers to all of 'the 27s' of all of the isomorphic models equally, i.e. it refers to *every* object equally. And this is just to say that our arithmetical vocabulary is radically referentially indeterminate.' (Button & Walsh, 2018 : 39) Hence, we still have referential indeterminacy. Still, following Horsten, perhaps this is just the best we can do: 'Many philosophers of mathematics believe nowadays that this remaining indeterminacy resulting from the fact that for logical and mathematical purposes, any two isomorphic structures serve equally well, is simply a fact of life that we have to learn to live with. Mathematical structures just are not determined sharper than up to isomorphism. In the sequel, I will assume that this is essentially correct.' (Horsten, 2001 : § 4.2.1) And so will we.

An important fact is that a categorical theory is also semantically complete:

> **Theorem** (Vaught's Test) Let T be a consistent $\mathcal{L}$-theory with no finite models, that is also k-categorical for some infinite cardinal $k \geq\mid \mathcal{L} \mid$. Then T is complete.

[17] This of course on the assumption that the theory has a model.

Proof. By contraposition. Suppose T is not complete. Then there is an L-formula φ such that $T \not\models \varphi$ and $T \not\models \neg\varphi$. Then there are models $\mathcal{M}$ and $\mathcal{N}$ of T such that $\mathcal{M} \models \varphi$ and $\mathcal{N} \models \neg\varphi$. Since $k \geq \mid \mathcal{L} \mid$ we can use the Upward and Downward Löwenheim-Skolem Theorem to take $\mathcal{M}$ and $\mathcal{N}$ to be of the same cardinality k. This means that T is not k-categorical. □

Now, say that a formula φ is determinately true or false if it is respectively true or false in all the models of the theory:

- φ is determinately true iff every model of T satisfies φ
- φ is determinately false iff every model of T falsifies φ
- φ is indeterminate otherwise.

This means that, assuming bivalence, a semantically complete theory assures that every sentence has a determinate truth-value. Hence, categoricity provides a way to challenge Putnam's Constructivization from a moderate realist perspective; the categoricity of a theory also explains how truth-value comes out determined:

> [...] we learn the theory, and the theory singles out a particular class of models having the same structure – an isomorphism class. As a result, every sentence in the language of the theory must have a determinate truth-value. (Incurvati, 2016 : 369)

2.2.2 Benacerraf's Way Out: ω-models and recursivity

An example of a shift to categoricity in order to determine the intended model of arithmetic is Benacerraf himself. We argued that Benacerraf(1965) can be read as insisting that arithmetic should not be seen as concerning a single structure but a class of isomorphic *adequate* structures. The crucial point is what *adequate* means here. Even though admitting that arithmetic is about structures (broadly construed) there is a sense in which some of these are more 'adequate' than others to fill the role of *the* natural numbers. There is a sense in which non-standard numbers should be seen more as a by-product of the model-theoretic apparatus than as genuine (though *sui generis*) natural numbers. In order to rule them out Benacerraf(1965) proposes two main constraints on the class of intended models of PA yielding a categorical theory:

1. be an ω-sequence;
2. 'the "<" relation over the numbers must be recursive'. (Benacerraf, 1965 : 53.)

Let us start by the second requirement – the 'less-than' relation (<), should be recursive. It is important that we are clear on Benacerraf's motivations for recursivity given that they will play a special role in the subsequent chapters to follow. His original stress on the recursivity of < is mainly due to the fact 'that we expect that if we know which numbers two expressions designate, we are able to calculate in a finite number of steps which is the "greater" [...] I am just making explicit what almost everyone takes for granted.' (Benacerraf, 1965 : 52-53). Besides insisting on the strong way in which <-recursivity is entrenched in our normal number-theoretic practice and intuitive understanding of *natural number*, it is further argued that a non-recursive <-relation is useless in counting tasks, which is one of the main uses of *the* natural numbers. This is illustrated in the following way. Consider a progression $A = a_1, a_2, a_3, ...$ built as follows. First, we generate two sequences: we take a sequence $B = b_1, b_2, b_3, ...$ of integers that are the Gödel-codes of valid formulas of classical first-order logic, and $C = c_1, c_2, c_3, ...$ of integers that are not the Gödel-codes of valid formulas. Second, we let $a_{2n+1} = b_n$ and $a_{2n} = c_n$. Now, is A a good candidate for the natural numbers? Since first-order consequence is only semi-decidable, there is no recursive procedure to enumerate all the members of A. As a consequence, we could not list its elements in order of magnitude for, not being a recursive set, we could not know what those magnitudes should be.[18] Then we could not even have a ready means to tell which of the elements of the progression is the n^{th}-successor nor could we know, starting from a given number, which number comes after. But if, when determining the size of a finite n-membered set, one needs to actually be able to build a suitable correspondence between the elements of the set and the collection of numbers smaller than n, it seems that A does not serve well our counting needs. In a more recent paper, Benacerraf(1996) recanted the above argument and the demand that the intended natural numbers should form a recursive progression, since any computable or non-computable progression can be enumerated with a recursive set of entities. Further, since the standard model clearly is of order-type ω, the first-requirement suffices to place $\mathcal{N}$ in the class of intended models: 'any old ω-sequence would do after all.' (Benacerraf 1996 : 189)

Benacerraf's first requirement – 'be an ω-sequence', faces a greater challenge however. No particular motivation or justification is given for its postulation, and in a way it clearly begs the question. The requirement seems to demand that an intended model should be isomorphic to the standard-model; but wasn't precisely the privileged role given to the standard model that was challenged by the model-theoretic arguments? Another worry is that we may well doubt if the notion of ω-sequence can be pin down in a non-circular way. For instance, Benacerraf cannot say that an ω-sequence is an order with the same order as the natural numbers, on pain of assuming what it tries to prove. But also the notion of ω-sequence is defined set-theoretically: an ω-sequence is something isomorphic to the von Neumann ordinals. However, the von Neumann ordinals are also defined in set theory; and there are non-standard models of set theory that give non-standard interpretations of the von Neumann ordinals. For example, there are models where the finite ordinals are not well-ordered 'from outside the model' but are well-ordered 'from inside'. This means that to make sure that the notion of ω-sequence captures a well-ordering, the model of set theory employed should be intended. But as we saw skolemite scepticism applies equally well to set theory and the problem of deciding what the intended numbers are reappears at the level of set theory. In this sense it is in no way obvious that Benacerraf's account is feasible and does not beg the question against the sceptic.

2.2.3 Shapiro's Way Out: PA^2

Shapiro(1991) maintains that the categoricity of PA^2 makes it more adequate for our formalization of *natural number*. PA^2 is the theory obtained by deductive closure of

$$\neg\exists y(S(y) = 0)$$
$$\forall x(x \neq 0 \rightarrow \exists y(x = S(y))$$
$$\forall x\forall y(S(x) = S(y) \rightarrow x = y)$$

[18]This, of course, on the assumption that human computation powers are at best those of a Turing machine.

together with the second-order Induction Axiom. Where X is an $\mathcal{L}_{\text{PA}^2}$-sentence:

$$Ind^2(\varphi) := \forall X(X(0) \wedge \forall x(X(x) \to X(S(x)))) \to \forall y X(y))$$

together with second-order Comprehension Schema. Where $\varphi(x_1, ..., x_n)$ is a formula not containing X^n:

$$\exists X^n \forall x_1, ..., \forall x_n(\varphi(x_1, ..., x_n) \leftrightarrow X^n(x_1, ..., x_n))$$

Notice that $\mathcal{L}_{\text{PA}^2} = \{0, S\}$ whereas $\mathcal{L}_{\text{PA}} = \{0, S, <, +, \times\}$. This is because order, addition and multiplication are definable in the signature of PA^2. For example, the graph of the addition function is the ternary relation obtained by the union of all ternary relations satisfying the condition:

$$Add(B) := \forall x B(x, 0, x) \wedge \forall x \forall y \forall w(B(x, S(y), w) \to \exists z(w = S(z) \wedge B(x, y, z)))$$

By second-Order Comprehension there is a relation A such that $A(x, y, w)$ iff $\exists B(Add(B) \wedge B(x, y, w))$. So by letting $x + y = w$ be abbreviated by $A(x, y, w)$ we can show, by induction, that the theory satisfies $\forall x(x + 0 = x)$ and $\forall x \forall y(x + S(y) = S(x + y))$ (analogous results for multiplication and order).

The interesting property about PA^2 is that, in *full* semantics, the theory is categorical. *Full* second-order semantics allows the second-order variables to range over what is essentially the full-power set of the domain of the structure; if X is an n-ary relation variable then the relevant domain of quantification in $\forall X\varphi$ will be $\wp(M^n)$ (for a model with domain M). Now, for the important theorem:

Dedekind's Categoricity Theorem: The theory of full second-order PA is categorical:

Proof. Let $\mathcal{M}$ be an arbitrary full model of PA^2. Define the set

$$X := \{m \in M \mid \exists n \in \mathbb{N}, \mathcal{M} \models m = S^n(0)\}$$

Where $S^0(a) = a$ and $S^{n+1}(a) = S(S^n(a))$. Informally, X is the set of all elements in M finitely far from 0. Since $\mathcal{M}$ is a full model of PA and since, as a consequence, it satisfies the second-order Induction Axiom, X falls within the range of the Axiom's second-order quantifier so that:

$$\mathcal{M} \models (X(0) \wedge \forall x(X(x) \to X(S(x)))) \to \forall y X(y)$$

Clearly, $\mathcal{M} \models X(0)$. Also, if $\mathcal{M} \models a = S^n(0)$, then $\mathcal{M} \models S(a) = S^{n+1}(0)$ – this means that $\mathcal{M} \models \forall x(X(x) \to X(S(x)))$. Hence, the antecedent of the above instance of the Induction Axiom is satisfied. From this fact we infer $\mathcal{M} \models \forall y X(y)$. This means that $M \subseteq X$. From this latter result and from the fact that $X \subseteq M$, we obtain $M = X$.

We now want to show that $\mathcal{M}$ is isomorphic to $\mathcal{N}$. For this we define an isomorphism $\pi : \mathcal{N} \to \mathcal{M}$ in the following way:

$$\pi(n) = a \text{ iff } \mathcal{M} \models a = S^n(0)$$

We are left to check that π is a bijective function that preserves the successor operation. Surjectivity follows from the fact that $M = X$. For injectivity we suppose that $\pi(n) = \pi(m)$; then, $\mathcal{M} \models S^n(0) = S^m(0)$. Since in $\textsc{pa}^2$ the successor operation is injective, it follows $n = m$. Now, by definition $\pi(0) = 0^{\mathcal{M}}$ because $\mathcal{M} \models 0 = S^0(0)$. Finally, we note that the function preserves succession: if $\pi(n) = a$ then $\mathcal{M} \models a = S^n(0)$ from where we obtain $\mathcal{M} \models S(a) = S^{n+1}(0)$; this means that $\pi(n+1) = S^{\mathcal{M}}(a) = S^{\mathcal{M}}(\pi(n))$. Hence ϕ defines an isomorphism.

We then have that $\mathcal{M} \cong \mathcal{N}$. Since $\mathcal{M}$ was arbitrary, every full model of $\textsc{pa}^2$ is isomorphic to $\mathcal{N}$. $\square$

Dedekind's Theorem provides a way for the moderate realist to pin down the natural number sequence. If the correct formalization of the natural numbers is $\textsc{pa}^2$, she may appeal to Dedekid's Proof and identify the natural numbers with the only isomorphism type satisfying the theory. This is then yet another example of how a change to a higher-logic capable of giving categorical theories, may help to determine the theory's intended model.

The change to a second-order setting is not without its critics. By far the most noticeable objection is that full second-order semantics presupposes quantification over the full power-set of the domain's structure. As a consequence, worries about the determinacy and grasp of the natural number sequence will be transferred to the notion of 'power-set' employed in the meta-theory. If the meta-theory is indeed standard ZFC, complications arise. First, it is know that the notion of power-set is not absolute; there are countable transitive models $\mathcal{B} \models \textsc{zfc}$ where $\wp(\mathbb{N})^{\mathcal{B}}$ defines a countable set. In this case, by Skolem-like reasoning we would be lead to the claim that $\wp(\mathbb{N})$ does not fix a particular notion, similar to 'uncountability'. Second, some authors such as Feferman have argued that 'the continuum itself, or equivalently the power set of the natural numbers, is *not* a definite mathematical object.' (Feferman, 2000 : 405): it is argued that the working set-theorists does not have any sharp conception of $\wp(\mathbb{N})$, but merely a vague intuition of what is the totality of arbitrary subsets of the natural numbers; further, even though this intuition is sharp enough to allow giving many evident properties to that object, it may never be sharpened completely as to determine that object itself. To be sure, Feferman admits that we may come to make sense of $\wp(\mathbb{N})$ given that the notion may be geometrically represented as the sets of infinite branches of a tree of height $2^{<\omega}$. Regardless, this same geometrical aid is not available for $\wp(\mathbb{R})$ and so the notion of arbitrary subsets of the real numbers remains vague (see Feferman, 2000 : 410 - 411). In fact, it is not easy to see how a non-circular sharpening of $\wp(\mathbb{R})$ may look like: on one hand, if we require that all the subsets of the reals must be found in L or $L(\mathbb{R})$, then Feferman claims we violate the spirit of arbitrariness in 'arbitrary sets of reals'; on the other, it is not clear how to specify how large must $\wp(\mathbb{R})$ be.[19] This shows that there may be doubts about the determinacy of 'power-set' in such a way that the change to a higher-order setting just succeeds in extending our concerns regarding arithmetical indeterminacy to the language of set theory.

[19]Feferman quite famously defends that 'the Continuum Hypothesis is what I have called an "inherently vague" statement [...]'. And this relativity or vagueness spreads quickly: '[...] it follows that the conception of the whole cumulative hierarchy [...] is even more so inherently vague, and that one cannot in general speak of what is a fact of the matter under that conception.' (Feferman, 2000 : 405).

Another way in which PA^2 can be seen to go wrong is with regards to the second-order semantics used. Sure, we may formalize PA^2 with a second-order semantic, but there are other options available. To see this we introduce the notion of Henkin-structure:

> **Definition** (Henkin-structure) For a signature $\mathcal{L}$, an $\mathcal{L}$-Henkin-structure $\mathcal{M}$ consists of:
>
> 1. a non-empty set of elements M called the domain of $\mathcal{M}$;
> 2. a set $M_n^{rel} \subseteq \wp(M^n)$, for each $n<\omega$;
> 3. a set $M_n^{fun} \subseteq \{g \in \wp(M^{n+1}) \mid g \text{ is a function } M^n \to M\}$, for each $n<\omega$;[20]
> 4. for each constant symbol c in the signature, an object $c^{\mathcal{M}} \in M$.
> 5. for each n-ary function symbol f in the signature, a function $f^{\mathcal{M}} : M^n \to M$;
> 6. for each n-ary relation symbol R in the signature, an ordered tuple $R^{\mathcal{M}} \subseteq M^n$.

In Henkin-semantics M_n^{rel} and M_n^{fun} serve as the domain of quantification of the relation and function symbols. So, for example, if X is an n-ary relation variable then the relevant domain of quantification in $\forall X\varphi$ will be M_n^{rel} (for a model with domain M) instead of $\wp(M^n)$. It can be shown (see Shapiro, 1991 : 88-96) that in *faithful* Henkin semantics[21] we can prove a Löwenheim-Skolem Theorem, meaning that in faithful Henkin semantics PA^2 is not categorical. Hence, if Shapiro's argument is supposed to work it must be shown why full semantics is a better formalization of the natural number sequence than Henkin semantics, and it is not obvious how this can be done in a way that respects the moderate realist's moderation. The problem of grasping the intended natural numbers just shifts to the problem of grasping the intended semantics. Putnam is particularly clear here:

> Some have proposed that *second-order* formalizations are the solution, at least for mathematics; but the "intended" interpretation of the second-order formalism is not fixed by the use of the formalism (the formalism itself admits so-called "Henkin models", i.e., models in which second-order variables fail to range over the *full* power set of the universe of individuals), and it becomes necessary to attribute to the mind special powers of "grasping second-order notions". (Putnam, 1980 : 481).

2.3 Summary

In this chapter we covered the main techniques from Putnam's Model-Theoretic Arguments, with a special focus on Skolemization. From here we have motivated a (skolemite) sceptical problem that challenges our supposed knowledge of the *intended* interpretations of both set theory and arithmetic. The moderate realist is by far the most affected by such a problem. Still, by shifting to an algebraic conception of arithmetic, focused more on structures rather than particular set-like objects, we saw how

[20] A function $g : M^n \to M$ is defined through its set-theoretical graph. That is, $g \in \wp(M^{n+1})$ such that if $(m_1, ..., m_n, a) \in g$ and $(m_1, ..., m_n, b) \in g$ then $a = b$, and for every $(m_1, ..., m_n) \in M^n$ there is an $n \in M$ such that $(m_1, ..., m_n, n) \in g$.

[21] A faithful Henkin structure is a Henkin structure where the Comprehension Schema and Choice Schema hold. See for example Button & Walsh, 2018 : 24f.

the moderate realist may address the sceptical challenge: find suitable constraints that yield categorical theories and isolate the intended models as an isomorphism type. However, such logical constraints are hard to come by without begging the question against the sceptic. In what follows we will see a recent strategy which we will call *the Argument from Tennenbaum's Theorem* addressing these issues.

Chapter 3

The Argument From Tennenbaum's Theorem

3.1 Introduction

In the last chapter we covered the main results from Skolem(1922) and Putnam(1980). It was argued that the elementary exploration of basic limitative results from first-order logic raise concerns regarding our grasp of arithmetic's intended models. Moreover, we explained how this problem is particularly pressing for a cluster of views in the philosophy of mathematics, both realist about mathematical entities and modest as to their epistemic powers towards numbers. The *moderate realist* may hope to address Skolem-Putnam's sceptical challenge by focusing on logical constraints capable of yielding categorical theories of arithmetic, or by isolating the intended interpretations up to a single isomorphism type. This, in turn, is dependent on an algebraic view of arithmetic *à la* Benacerraf(1965) that equates numbers more with structures (broadly construed) rather than set-theoretic objects. However, it is rather hard to see how such isomorphism types may be isolated or the constraints on categorical theories warranted without begging the question against the sceptic.

In this chapter we will discuss one of the most recent (and most technically interesting) approaches in answering the sceptic, stemming originally from Horsten(2001). This argument – which we will call *the argument from Tennenbaum's Theorem*, tries to determine an intended isomorphism type by focusing on the computational properties of the functions defined on intended models. A strong case can yet be made for the circular character of the argument; as we will see, it can be shown that the argument itself depends on a determinate grasp of notions whose determinacy attempts to establish, bearing the charge of assuming what it tries to prove. Nonetheless, such considerations are dependent on the way the argument is used, there being some disagreement on what the argument from Tennenbaum's Theorem is an argument *for*. By clarifying its several uses we aim also to explain how different authors think the argument might be more or less successfully pursued in each case. We will conclude with some general reflections on the overall viability of the skolemite challenge itself and how it relates with the work in the chapters to follow.

3.2 Tennenbaum's Theorem

The result known as *Tennenbaum's Theorem* was given by Stanley Tennenbaum in 1959 and appeared as a one-page abstract under Tennenbaum(1959). The result sharply contrasts the computational properties of arithmetical operations defined in standard and non-standard models of PA:

> **Theorem:** (Tennenbaum, 1959) If $\mathcal{M}$ is a countable model of PA such that $\mathcal{M} \ncong \mathcal{N}$, then $\mathcal{M}$ is not recursive.

The standard model $\mathcal{N}$ where order, addition and multiplication are interpreted standardly is recursive. Tennenbaum shows that it is the *only* recursive model, up to isomorphism. The proof of the above statement will mostly follow Kaye(2011). The strategy is normally to assume that there is a recursive non-standard model of PA and show this leads to contradiction; we do this by building a non-recursive set and prove, under the assumption that such recursive non-standard model exists, that the set is recursive. First, we define:

> **Definition:** We call an $\mathcal{L}_{\text{PA}}$-structure $\mathcal{M}$ recursive iff there are recursive functions $S : \mathbb{N} \to \mathbb{N}$, $+ : \mathbb{N}^2 \to \mathbb{N}$ and $\times : \mathbb{N}^2 \to \mathbb{N}$, a binary recursive relation $< \subseteq \mathbb{N}^2$ and $0 \in \mathbb{N}$ such that $\mathcal{M} \cong \langle M, 0, S, +, \times \rangle$.

The proof of the Theorem may be separated into two subproblems: first, define when a set $A \subseteq \mathbb{N}$ is *coded* in a model, and, second, the implications of non-recursive coded sets. The coding of a non-recursive set in the domain of a model can be done by a prime number technique where a set of numbers n is defined by an element $c \in \mathcal{M}$ and a two-place formula φ such that $\mathcal{M} \models \varphi(n, c) \leftrightarrow \exists k(c = k \times p_n)$ where p_n is the n^{th} prime number. Then we can define the *standard system of sets coded in a model* as the set of all sets coded by an element c in the model.

> **Definition:** (SSy($\mathcal{M}$)) Given a non-standard model $\mathcal{M}$ of PA we call the standard systems of sets in $\mathcal{M}$, SSy($\mathcal{M}$), the set defined as:
>
> $$SSy(\mathcal{M}) = \{A \subseteq \mathbb{N} \mid \exists c \in \mathcal{M} : A = \{n \in \mathbb{N} \mid \mathcal{M} \models \varphi(n, c)\}\}$$

The Theorem is also related with the following classical results from recursion theory:

> **Definition:** (Recursively Inseparable) We say that two disjoint sets $A, B \subseteq \mathbb{N}$ are *recursively inseparable* iff there is no recursive set $C \subseteq \mathbb{N}$ such that $A \subseteq C$ and $C \cap B = \emptyset$.
>
> **Lemma:** There exist recursively enumerable recursively inseparable sets.

The traditional approach to Tennenbaum's Theorem now proves the existence of a non-recursive set coded in a non-standard model. For this, we recall the principle:

> **(Overspill Principle)** Let $\varphi(x)$ be a $\mathcal{L}_{PA}$ formula. Let $\mathcal{M}$ be a non-standard model. If $\mathcal{M} \models \varphi(n)$ for all $n \in \mathbb{N}$, then there is a non-standard number a such that $\mathcal{M} \models \varphi(a)$.

Now, we have:

> **Theorem:** Let $\mathcal{M}$ be a non-standard model of PA. Then SSy($\mathcal{M}$) contains a non-recursive set.

Proof. Consider $A, B \subseteq \mathbb{N}$ recursively enumerable recursively inseparable sets, as given by the above Lemma. Consider a, b the μ-recursive functions that enumerate them. Given that every μ-recursive function is Σ_1-representable in PA, there are Σ_1-formulas $\exists y \alpha(x, y)$ and $\exists z \beta(x, z)$ that define A and B, respectively, where α and β are Δ_0. We regard $\mathcal{N}$ as an initial segment of any non-standard model. Since $\mathcal{N} \prec \mathcal{M}$, there is an embedding $\pi : \mathbb{N} \to M$ that preserves the Σ_1 formulas. That is, since the interpretations of a and b are preserved upwards from $\mathcal{N}$ to its extension (i.e. $a(\pi(x)) = \pi(a(x))$ and $b(\pi(x)) = \pi(b(x))$), the same Σ_1 formulas express A and B in $\mathcal{M}$. Then, from this latter fact and from the disjointness of A, B it follows that for any $k \in \mathbb{N}$:

$$\mathcal{M} \models \forall x{<}k, \forall y{<}k, \forall z{<}k \ \neg(\alpha(x, y) \wedge \beta(x, z))$$

By applying the Overspill Principle, there is a non-standard $c \in M$ such that:

$$\mathcal{M} \models \forall x{<}c, \forall y{<}c, \forall z{<}c \ \neg(\alpha(x, y) \wedge \beta(x, z))$$

Define the set $C \subseteq \mathbb{N}$ with $C = \{n \in \mathbb{N} \mid \mathcal{M} \models \exists y{<}c \ (\alpha(n, y))\}$. Then, by preservation of Σ_1-fromulas and since c is non-standard, we have that $A \subseteq C$ and that $C \cap B = \emptyset$. Hence, since A and B are recursively inseparable, C is a non-recursive set. $\square$

From the above we can prove:

Tennenbaum's Theorem:

Proof. Let $\mathcal{M}$ be a non-standard model of PA. By the above Theorem there is a set $C \in SSy(\mathcal{M})$ such that C is non-recursive. Now, there is a $c \in M$ such that

$$C = \{n \in \mathbb{N} \mid \mathcal{M} \models \varphi(n, c)\}$$

where $\varphi(n, c) = \exists k(c = k \times p_n)$ uniquely codes C with c. We want to show that if $+^{\mathcal{M}}$ is recursive, then C is recursive, against assumption. Assume then, for contradiction, that $+^{\mathcal{M}} : \mathbb{N}^2 \to \mathbb{N}$ is recursive. Define:

$$\psi(n, c) = (c = \underbrace{k + ... + k}_{p_n \text{ times}}) \vee (c = \underbrace{k + ... + k}_{p_n \text{ times}} + 1) \vee (c = \underbrace{k + ... + k}_{p_n \text{ times}} + \underbrace{1 + ... + 1}_{p_n - 1 \text{ times}})$$

We now note that PA $\vdash$ Euclidean Division. Then, since $\mathcal{M} \models$ PA, we have that $\mathcal{M}$ also proves Euclidean Division. Hence, $\exists ! p_n^{\mathcal{M}} \exists ! r$ such that $c = (k \times p_n) + r$ for $0 \leq r < p_n$. Now, for any input n, we may compute p_n and search for a $k \in M$ and $r < p_n$ such that $(\underbrace{k + ... + k}_{p_n \text{ times}}) + r = \varphi(n, c)$. This search is bounded to terminate since Euclidean division is computable. Now, if $r = 0$, then $c = \underbrace{k + ... + k}_{p_n \text{ times}}$ is the case and $\psi(n, c)$ is true; in this case $\varphi(n, c)$ is true and $n \in C$. If $r \neq 0$, then one of the other disjuncts of $\psi(n, c)$ is true; in which case $\varphi(n, c)$ is false and $n \notin C$. This shows that it is decidable in $\mathcal{M}$ if $n \in C$ or $n \notin C$. Hence, C is recursive against assumption. Since we derive a contradiction, we conclude that $+^{\mathcal{M}}$ is not recursive. $\square$

An interesting question is how far can the argument be extended to theories weaker than PA. The proof above only requires overspill for Δ_0-relations and $I\Delta_0$ is strong enough to prove enough properties concerning Euclidean Divison and primes for the above argument to go through. In fact, McAlloon(1982) showed that if we replace PA with the weaker subsystem $I\Delta_0$, with induction restricted to bounded quantifiers, an analogue to Tennenbaum's Theorem also holds: addition and multiplication will be non-recursive in any non-standard model of $I\Delta_0$. Also, from Wilmers(1985) we know the same result holds for the subtheory IE_1 of $I\Delta_0$, with induction restricted to bounded existential quantifiers.

3.3 Halbach & Horsten's Argument

The argument from Tennenbaum's Theorem tries to determine which model of PA should be counted as the intended interpretation of our arithmetical vocabulary, by appealing to the notion of 'recursivity' and by making essential use of Tennenbaum's Theorem. At least to the knowledge of the present author, Horsten(2001) seems to have been the first to put the argument forward. To appreciate the original motivations of the argument, crucial in the evaluation of its strength when facing the sceptic, it is worth recalling the full power of Putnam's just-more-theory manoeuvre. For this we will use Shapiro(1991) as illustration.

As explained in the previous chapter, Shapiro(1991) pushes for a formalization of *natural number* within a higher-order logic capable of yielding categoricity. In doing so it is argued, not without controversy, that Shapiro makes himself vulnerable to Putnam's manoeuvre. By trying to determine the correct model of PA through the addition of constraints that go beyond what can be reasonably found in arithmetical practice, Shapiro shifts the problem of the relativity of the original arithmetical vocabulary to that of the relativity of the supplementary vocabulary that is supposed to fix the interpretation of the former. The addition of new constraints is subject to first-order regimentation and, consequently, reinterpretation in a non-standard fashion. That is,

> These constraints succeed in determining the structure of the natural numbers only modulo the determinacy of the language of the constraints by which they propose to supplement PA. And it should be clear [...] that Putnam's model theoretic argument may be iterated against the individual proposals [Shapiro's proposal] to construct models of supplemented theories in which the extension of the term "natural number" is non-standard. (Dean 2002 : 6)

What this suggests is that new constraints should come from reasons already found *within* the actual ordinary arithmetical practice instead from extraordinary (mathematical) considerations such as the expressibility of continuum-many relations over the natural numbers as in the (full) second-order case[1], on pain of Putnam's ma-

[1]This is not a totally uncontroversial matter, however. For instance, Kreisel(1967) argued, quite famously, that the acceptance of the second-order induction axiom is prior to the acceptance of the first-order schema:

> A moment's reflection shows that the evidence of the first order axiom schema derives from the second order schema: the difference is that when one puts down the first order schema one is supposed to have convinced oneself that the specific formulae used (in particular, the logical operations) are well defined in any structure that one considers [...]. (Kreisel, 1967 : 148)

noeuvre. And Benacerraf(1965) respects this *credo* entirely. We stressed that his insistence on the recursivity of the $<$ - relation is mainly because, as he sees it, this is a property which naturally springs out from our intuitive understanding of *natural number* and from our normal operations with numbers. To repeat, 'we expect that if we know which numbers two expressions designate, we are able to calculate in a finite number of steps which is the "greater" [...] I am just making explicit what almost everyone takes for granted' (*op. cit.).* It then seems natural, if not necessary, to demand for the intended model of arithmetic to have a recursive $<$ - relation. To the sceptical charge that there might be non-intended reinterpretations of our practice – for instance, where $<$ is not recursive but yet the sentence '$<$ is recursive' is satisfied within a non-standard model, we might well reply that the *re*interpretation is indeed a *mis*interpretation; i.e. the non-standard model does not respect nor capture the way we *use* (i.e. compute with) numbers. By itself, the recursivity of $<$ is not enough to fix the standard model as the intended model of PA, up to isomorphism: there are countable non-standard models $\mathcal{M}$ (where addition is not recursive) with a recursive $< \subseteq \mathbb{N}^2$ such that $\mathcal{M} \restriction < \cong (\mathbb{N}, <^{\mathcal{N}})$.[2] Nonetheless, the insistence on the 'well-entrenchment' of the additional constraints in regards to our ordinary arithmetical practice provides a possible avenue to face the skolemite challenge.

Halbach & Horsten(2005) propose to take up Benacerraf's recursivity requirement; their goal is to (within a structuralist approach)[3] pin down the interpretation of arithmetic by restraining the computational properties of the functions defined on the domains of intended models. The first step of their argument is to extend Benacerraf's requirement: in the same way as Benacerraf argued that the intended interpretation of $<$ should be recursive, now it is claimed the same for $+$ and $\times$. When learning primary-school arithmetic, we notice the fundamental role of numbers in addition and multiplication: given two numbers we expect to be able to compute their sum and product. This much seems uncontentious. If the argument for the recursivity of the $<$ - relation is valid, then, by the same token, it also seems sensible to insist on the recursivity of $+$ and $\times$. Hence, Halbach & Horsten demand for the intended interpretation of $+$ and $\times$ to be decidable. If $+$ and $\times$ are taken to represent the functions we actually compute with in practice, then non-computable denotations cannot be a faithful representation of our everyday use of numbers.[4] 'Numbers are something we can calculate with; if we cannot calculate with objects, then they are not numbers.' (Halbach & Horsten, 2005 : 177) This gives us their first restriction on intended models:

> **REC1:** In an intended model the relation $<$ and the operations of addition and multiplication are recursive. (Halbach & Horsten, 2012 : 177)

The above restriction coupled with Tennenbaum's Theorem shrinks the class of intended models to the isomorphism type containing the standard model of PA. We recall the essential theorem (stated in the contrapositive):

> **Tennenbaum's Theorem:** If $\mathcal{M}$ is a countable model of PA where $+^{\mathcal{M}}$ (and $\times^{\mathcal{M}}$) is recursive, then $\mathcal{M} \cong \mathcal{N}$.

[2] See Kaye, 1991 : 157; exercise 11.10.

[3] This is made in the spirit of what we have been calling an *algebraic* view of arithmetic: arithmetic is about a collection of (intended) structures and not a collection of (intended) objects that would form the domain of a certain structure.

[4] In fact, for their project to go through it is enough for them to focus on the recursivity of addition. Hence, we will omit the case for $\times$, bearing in mind that whatever holds for addition is also meant to hold for multiplication. On this point see Halbach & Horsten, 2005 : 177.

Tennenbaum's Theorem guarantees that only the standard model, up to isomorphism, has recursive addition, and together with **REC1** suffices to rule out non-standard models. What this shows is that only the standard model, again up to isomorphism, is able to correctly represent our computational practices involving addition (and multiplication), and is intended of the natural number sequence.[5] A curious result is that now Benacerraf's ω-requirement appears as conclusion and not as premise: ω order type is not a requirement to be an intended model, but a result of the argument from Tennenbaum's Theorem.

We should pause here. The careful reader might have already noticed some ambiguity in our talk of recursive operations, that we should better clarify. When talking about the recursivity of $<$ or $+$ we have reflected on our (i.e. human) effective computational powers. Hence, we talked about computable functions in a rather informal non-mathematically defined sense. But the two, recursive functions and 'effectively-computable' functions need not necessarily coincide. So, unless argued for, the passage from one (this function if effectively-computable) to the other (this function is recursive) is unwarranted. What is implicit in Halbach & Horsten(2005)'s project is a commitment to the Church-Turing Thesis. To clarify some terminology, by 'effectively-computable' (or informally computable) we refer to a class of functions defined on some fixed space that take a finite amount of input and whose output can be algorithmically determined in a finite number of steps. The notion of 'recursivity' is defined in terms of some mathematical model of computation; we will say that a function is recursive (or formally computable) if it is computable by, say, a Turing machine. Now, by assuming the Church-Turing Thesis:

> **Church-Turing Thesis** A function is effectively-computable iff it is recursive.

The move from effective computability to recursivity is now justified. By reflecting on our practice we see that intended addition is effectively-computable. By the Church-Turing Thesis, the intended interpretation of $+$ is recursive. And now by Tennenbaum's Theorem the intended interpretation of arithmetic, PA, is only the standard model up to isomorphism.

Halbach & Horsten's main argument (**REC1**) can be properly called *the argument from Tennenbaum's Theorem.* In one way or another it is this argument that is addressed in the current literature as such. Different authors use the argument in different ways. If we assume that arithmetic is about a single (intended) structure, three questions immediately come to mind:

1. How do we know that arithmetic is about a single intended structure? And how do we know how that intended structure is like?
2. How is that structure like?
3. How do we manage to refer to that structure?

(1) is a question about epistemology; (2) is (in lack of a better name) 'metaphysical' and (3) linguistic. We do not wish to say that the argument may satisfactorily address all the different problems above; different questions may demand different answers. Here, we are only interested in seeing how the argument plays out as an answer

[5] As noted above, Tennenbaum's Theorem holds for weaker subsystems of PA. Thus, *prima facie*, the above strategy may still be pursued with a weaker induction schema.

to (1). That is, we take the argument from Tennenbaum's Theorem as a possible solution to the *epistemic* problem of determining the intended model of arithmetic. This must be carefully distinguished from the *reference-fixing* project of using the argument to explain how we refer to the intended model. In the literature, we find these question often conflated. For instance, as Horsten(2012 : 275) himself notes, Halbach & Horsten(2005) do not properly distinguish which of the questions (1) and (3) they are addressing. We find the same problem in Button & Smith(2012). Now, Horsten(2012), Dean(2002) and Dean(2013) are clear that they use the argument to tackle (3), not (1). It is our view that both questions can be closely associated, and seeing how the argument plays out when answering one sheds light on the other.[6] But, to be sure, we will for the rest of this work be mainly interested in the argument when applied to (1).

With these desiderata in place we can continue with Halbach & Horsten(2005). They propose important minute modifications to their original argument and to **REC1** that will be helpful in understanding the discussion to follow. We said that their proposal is put forward in the same spirit as that shared by an algebraic conception of arithmetic. However, the notion of recursivity is normally defined for functions on *natural numbers* there being no general notion of recursivity that applies to functions on arbitrary objects whether mathematical, or concrete, or ... But, within an algebraic spirit, we would like **REC1** to apply to models that do not necessarily have as domains subsets of sets of the natural numbers. To solve this issue they consider the notion of *coding*. Given a model of PA we may try to code the domain of the model by standard natural numbers and check if the relevant operations are indeed recursive.[7] Hence, a slight adjustment to **REC1**:

> **REC2:** For every intended model there is a coding of the set of its elements such that the relation $<$ and the operations of addition and multiplication on the codes, as they are induced by the relations on the intended model, are *recursive*. (Halbach & Horsten, 2012 : 179)

REC2 also determines the standard models as the intended model up to isomorphism. First, for uncountable models obviously there is no such coding, excluding them automatically. Second, if a countable model is not isomorphic to $\mathcal{N}$, by Tennenbaum's Theorem, addition will not be recursive violating the requirement. However, how should we understand the notion of *recursive* procedure present in the requirement? Well, normally, the notion of recursivity is defined for functions over the *natural numbers*; we say that a Turing machine completes a task in a *finite* number of steps, or that a μ-recursive function takes *finite tuples of natural numbers* and returns a single *natural number*. But this makes clear that when giving **REC2** we explicitly introduce the problematic notion that we were trying to define with the requirement itself (i.e. the notion of *natural number*). A similar problem arises when talking about

[6] The association is natural. In fact, though we have opted to prefer the epistemic-side of Putnam's model-theoretic arguments ('Among many non-isomorphic models of PA, how do we know which is the right one?), another important aspect of his arguments relates to language and reference ('Among many non-isomorphic models of PA, how do we manage to refer to the right one?). In fact, one could argue, it is this latter variant the most important part of his challenge: '[...] I want to take up Skolem's arguments, not with the aim of refuting them but with the aim of extending them [...] It is not my claim that the "Löwenheim-Skolem Paradox" is an antinomy *in formal logic*; but I shall argue that it *is* an antinomy, or something close to it, in *philosophy of language*. (Putnam, 1980 : 464)

[7] In fact, this is how Tennenbeaum's Theorem is normally presented: if a model can be coded in the natural numbers so that the induced operation of addition is recursive, then the model is isomorphic to the standard model.

'*finite* number of steps' or '*finite* tuples'. Given that the notion of finite cardinal is often defined via the notion of natural number – a set is finite if it can be put in a one-to-one correspondence with an initial segment of the naturals, it appears equally circular to define natural number via finite cardinal.[8,9] This opens the possibility for the skolemite sceptic to claim that the argument does assume that which it tries to prove (i.e. a (determinate) grasp of the natural numbers). To avoid this difficulty, Halbach & Horsten(2005), following some intuitions of Dean(2002) (see below), propose to take as primitive not the notion of *theoretical* recursivity (mathematically defined through some computational model like Turing machines or μ-recursivity), but rather the notion of *practical* recursivity. They identify the practical notion with the idea of an 'effective procedure' and, as we saw, justify the shift from this notion to the formal notion used in Tennenbaum's Theorem with the Church-Turing Thesis. As they see it:

> [...] the notion of an *effective procedure* and thus of computability in the informal sense does not presuppose number theory or even set theory. Effective procedures do not apply to numbers, but also to other objects. [...] This *practical* notion of computability is distinguished from the *theoretical* notion of computability (and recursiveness). The theoretical notions of recursiveness is a purely mathematical notion [...] The practical notion, in contrast, is not defined in set theory and does not completely belong to theoretical mathematics. [...] In order to apply Tennenbaum's Theorem for ruling out nonstandard models, we have to assume that a practically recursive operation is also recursive in the formal sense. That is, we are appealing to Church's thesis. (Halbach & Horsten, 2005 : 180)

Now, we might be rather suspicious of the advantages brought with this shift. After all, the notion of effective procedure seems to equally presuposse the notion of (finite) natural number, for an algorithm also completes a task in a *finite* number of steps. We will qualify this judgement at great length in the next section, and for now it interests us only to see how this shift alters their **REC2** requirement.

Their idea is that even though it is the formal definition of recursivity that we rely on when applying Tennenbaum's Theorem, in *practice*, we 'see' that a function is recursive by informally demonstrating the existence of an algorithmic procedure. For example, we come to the idea that addition is recursive by first coming to terms with the *primitive recursive addition algorithm*:

PRaddition:

On inputs $S^n(0)$ and $S^m(0)$ use the rules

$$\forall x(x+0=x) \qquad \forall x\forall y(x+S(y)=S(x+y))$$

to compute the equalities $S^n(0)+S^m(0)=S(S^n(0)+S^{m-1}(0))=...=S^{n+m}(0)$ and output $S^{n+m}(0)$

[8] Further, the notion of finiteness is then relative to a model of set theory; but different models may have different (non-isomorphic) definitions of natural numbers, meaning that they may disagree on their notions of finiteness. To amend this issue the definition of finiteness may be restricted to the *intended* model of set theory. However, this introduces the perhaps more challenging question of determining the intended model of set theory.

[9] Similarly with *coding* in the sense that it is just a function from a collection of objects to the set of the *natural numbers*.

Now, it would be on the basis of the apprehension of an efficient procedure like PRaddition, that we would be justified in the claim that:

> The function denoted by + is computable.

To better explain what is meant by practical recursivity, or effective procedure, they propose then to define the former through the idea of employing an algorithm. They note that an important aspect is that algorithms compute on symbols – they are instructions on how to manipulate symbols, regardless if those symbols stand for numbers or not. It seems essential that symbols are intentional notations: if something is a symbol depends on whether it is used to convey some meaning. Further, symbols, unlike numbers qualified from a structuralist-cum-algebraic perspective, do have an internal structure. They are not just distinguished from their place in the structure. For example, when computing division the symbol '1' must be distinguished from '2', otherwise we may not follow the instructions of the algorithm. In this sense, standard numerals as symbols belonging to a notation system do have an internal structure, and this notation system is itself a structure of some kind. Hence, numerals differ from numbers when structurally conceived, for numbers do not have this internal structure. These quick considerations let us see that practical algorithms do not manipulate (compute) on natural numbers, but on objects (symbols) possessing an internal structure. When understood in this way, the 'recursiveness' present in **REC2** does not presuppose numbers any more, but only symbols of some sort. Similarly, a *coding* is now defined as a function from a collection of objects to symbols, instead to natural numbers. It should be noted that we still rely on the Church-Turing thesis to make the passage from the informal effective procedure to formal recursiveness. The intended model of arithmetic will be one where we have notations (symbols) for the elements of the model such that the operations of addition and multiplication are computable on the notations. (Halbach & Horsten : 182).[10]

In this sense, they see a coding as an assignment of standard numerals (which are symbols of some sort) to all and only the objects in the domain of a model. However, for **REC2** to work, we need to be able to determine when a certain assignment of symbols to objects is indeed a coding. That is, we need to be able to determine if *every* object in the domain of the model receives a symbol, and, therefore, to decide whether all objects are named by a standard numeral. But to do this we must be able to distinguish between standard and non-standard numerals, begging again the question against the sceptic. The sceptic is asked to already understand the notion of *standard natural number* to define it. This challenge leads Horsten & Halbach to their final requirement:

> **REC3:** Intended models are notation systems with recursive operations on them satisfying the Peano axioms. (Halbach & Horsten, 2005 : 183)

REC3 dispenses appeal to codings and to natural numbers in fixing intended models: 'There no longer is any need to see that all objects are named, for the objects in intended models all *are* names. Our proposal entails that in a fundamental sense, arithmetic is exclusively *about* notations.' (Halbach & Horsten, 2005 : 183) Tennenbaum's theorem will still ensure that the intended notation systems are isomorphic to the standard-model. **REC3** has as consequence that the elements of the natural number sequence do not have, in a algebraic-cum-structuralist spirit, any internal content. But the structure of arithmetic is obtained by abstraction on notation systems.

[10] Of course, for this, we need to assume we have infinitely many symbols, which they are disposed to.

We finish the section as we started. Using Shapiro(1991)'s proposal. Halbach & Horsten note that their proposal, what they call *Computational Structuralism*, scores better than a move to second-order logic since the former requires less of our mathematical knowledge and abilities. Even though, in a way, second-order structuralism may be thought as more basic by avoiding the long detour through Tennenbaum's Theorem, that detour is mathematical and not philosophical. When properly seen, computational structuralism only assumes that agents willing to engage on arithmetical practice are able to perform sums; in sharp contrast, the second-order structuralist must endow their agents with the power of (somehow) grasp second-order quantification which, they argue, is more demanding on the abilities of the agent doing arithmetic.

3.4 On Theoretical and Practical Recursivity

3.4.1 Theoretical Recusivity

We have now fully discussed Halbach & Horsten(2005)'s proposal, stemming from Tennenbaum's Theorem, addressing the worries on how we go about to determine the intended model of arithmetic. Button & Smith(2012) are themselves sceptical of the viability of the above solutions.[11] If there truly is a genuine problem about intended models[12] exposed by the model-theoretic results, then, they argue, it is not to be solved with more model-theory and, in particular, with Tennenbaum's Theorem. Their discussion starts from a fictional character, a certain 'Thoralf', rather concerned about our grasp of the intended model in the aftermath of the construction of non-standard structures. To be sure, it is assumed that Thoralf is happy in accepting abstracta and allowing some knowledge about them; still, he is rather abashed in the light of non-isomorphic models, asking himself what sense can there be of talk of a 'right' or 'intended' interpretation if our practice does not single out a unique PA-model. We see then that Thoralf's case is (humour aside) the same of what we have been calling the moderate realist position and the challenge is then how to ease Thoralf's (and, by association, the moderate realist's) doubts.

To illustrate how Tennenbaum's Theorem doesn't work in attending to Thoralf's predicament it is helpful to consider an argument with a similar structure as the one of Halbach & Horsten. It starts with the easy result:

> **Initial Segment Theorem:** If $\mathcal{M}$ is a model of PA and if for all $m \in M$ it is the case that $\mathcal{M} \models m = S^n(0)$ (for some $n \in \mathbb{N}$), then $\mathcal{M} \cong \mathcal{N}$.

Now the argument would go something like this: in learning primary-school arithmetic we learn to count backwards. Reflecting on our practice we realize that (under the standard order relation) any natural number is finitely far from zero or, what is equivalent, it has finitely many predecessors. Conclusion: by the Initial Segment Theorem, the intended model of PA (where each number is finitely far from zero) is just the standard model up to isomorphism. Still, it would be surprising if Thoralf was convinced by the above argument. Afterall, the argument makes essential use of the

[11] We vacillate in our reading of Button & Smith's paper. Throughout this section we will interpret the paper as addressing the epistemological problem of knowing the intended interpretation of arithmetic (and how Tennenbaum's Theorem plays out here) rather than as addressing the linguistic problem of explaining how we manage to refer to the intended structure.

[12] As we will see below, the assumption that skolemite cogitations pose a genuine problem for the determinacy of mathematical concepts is not uncontentious.

Initial Segment Theorem whose meaningfulness is dependent on a prior understanding of what is for something 'to be finitely far from zero'. But to understand the former we need to understand what counts has a finite cardinal number, and it is this precise notion which Thoralf thinks is open to multiple interpretations.

By way of analogy, Button & Smith(2012) argue that Tennenbaum's Theorem is subject to essentially the same problem as in the above case. Tennenbaum's Theorem contains expressions of the form '$+^{\mathcal{M}}$ is recursive', making indispensable use of the notion of recursive function. The skolemite sceptic, irritatingly sceptical as usual, will ask for an elaboration of the notion of 'recursive function'. We give a definition: a function is recursive just means that there is a Turing machine that can compute the output of the function (given a certain input). Suppose he urges for further elaboration. We reply: a function is computable by a Turing machine if (given this model of computation) the output of the function may be computed in a finite number of steps (given a certain input). But now the circularity is plainly evident for in our explanation of 'recursive function' we (explicitly) used the notion of 'finite number of steps' and, consequently, of 'finite number'. It then seems that to understand Tennenbaum's Theorem we have first to understand what is for something to be a finite (natural) number. And again this is precisely the disputed notion which Thoralf thinks is opened to multiple interpretations. 'So, if Thoralf genuinely doesn't understand how we grasp the standard model, the argument from Tennenbaum's Theorem plainly can't help him.' (Button & Smith, 2012 : 117)

Dean(2013) pushes the point even further. It is not *only* the case that, as Button & Smith(2012) want to argue, there are divergent non-intended interpretations of our computational practices. Worse, *every* non-intended interpretation (i.e. *every* non-standard model of PA) will satisfy the formal requirement defining the (theoretical) recursivity of addition:

> [...] our computational practices are sufficiently elementary that statements [...] by which computationalists hope to rule out such [divergent] interpretations will typically be provable mathematically. *There thus seems to be little room for the truth values of sentences by which we report positive attributions of computability to vary among the interpretations in question.* (Dean, 2013 : 152, our italic)

Recall that Halbach & Horsten(2005) wish to constrain the intended interpretation of PA by adding a requirement of the form:

(1) The function denoted by $+$ is recursive.

As we saw, recursivity is a formal notion definable with μ-recursivity, for example. Call a function $f(\vec{x})$ (with $\vec{x}$ of arity k) μ-recursive iff it is extensionally equivalent to a $\phi_i(x)$ μ-recursive function, for a certain i in an enumeration $\phi_1(x), \phi_2(x), ...$ of μ-recursive functions. Then, (1) is equivalent to

(2) The function denoted by $+$ is extensionally equivalent to $\phi_i(x)$.

By Kleene's Normal Form Theorem, there exists a primitively recursive function u, such that if ϕ_e is recursive, then

$$\phi_e(\vec{n}) = u(\mu x T_k(e, \vec{n}, x))$$

where μ is the minimization operator and T the Kleene-predicate defined by '$T_k(e, \vec{n}, x)$ iff x encodes the steps of a halting computation ϕ_e on input $\vec{n}$ '. Since T_k and u are primitively recursive they are representable in PA by Σ_1-formulas $t_k(i, \vec{w}, x)$ and $v(w, x)$ such that

$$\phi_e(\vec{n}) = y \text{ iff } \mathcal{N} \models \exists q((\forall r{<}q)\neg t_k(e, \vec{n}, r) \wedge t_k(e, \vec{n}, q) \wedge v(q, y))$$

Using the above biconditional, (1) and (2) are now equivalent to:

(3) $\exists e \forall x \forall y \forall z(x + y = z \leftrightarrow \exists q(t_2(e, x, y, q) \wedge v(q, z)))$

Dean(2013 : 149) now notes that we may, by formalizing the algorithm PRAddition, explicitly construct a μ-recursive definition corresponding to the index e and show that (3) is provable in PA. But since (3) is provable, it is also satisfiable in all models of PA (including non-standard models). This means that if we take (3) to express the recursivity of addition, then all models will satisfy the arithmetical definition for recursivity. Hence, formally, those models will have an operation of addition which (the model thinks) is recursive. The appeal to theoretical recursivity in ruling out non-standard models will drastically fail.

3.4.2 Practical Recursivity

It is then circular or, at least, problematic to fix the intended models and explain what the natural numbers are through a notion that presupposes the natural numbers. Dean(2002) is aware of the general problem and moves from a 'theoretical' to a 'practical' notion of recursivity. As he eloquently explains:

> [...] Tennenbaum's Theorem is merely the tip of the iceberg with respect to illuminating the anomalies which would arise were the reference of computational terms to be tied down no more firmly than the class of extensions which their first-order definitions allow. [...] If the meanings of complexity theoretic concepts were only fixed relative to these definitions, they too would inherit the indeterminacy of finiteness. *Complexity theoretic terms, however, are intended to express real world limitations on our abilities* [...] As a consequence, the plausibility of iterating the sceptical argument that our practices and intentions are insufficient to determine the reference of certain complexity theoretic terms becomes strained to the point of collapse. (Dean, 2002 : 11-12; our italic)

What is proposed here is to understand 'recursivity' in a practical way, i.e. not as a 'theoretical' notion focused on functions and relations over numbers, but as a notion that in fact precedes that of natural number.[13] Practical recursivity is then informally defined by considering what we could compute given our real (practical) limitations; by entreating Thoralf to reflect on what he could actually decide in polynomial time, it is expected that he may come to form a sense of what is for a function to be recursive in a way that does not presuppose numbers. Still, even with this amendment, the argument is subject to the same dialectic. To see why consider again, first, the argument from the Initial Segment Theorem. We saw how the problem here was the

[13] Also, Horsten(2012 : 278) talks about a 'pre-theoretical sense of computability' (though within a slightly different project, see below). In fact, it is a consequence of Putnam's just-more-theory that adding to the axioms of PA a first-order claim stating that addition is recursive, will admit non-standard models. What Horsten(2012 : 287) takes this to show is that the notion of computability cannot be fully formalized.

implicit circularity in demanding a prior understanding of what 'being finitely far from zero' means. Suppose we explain it by pointing to the *real world limitations on our abilities*. For this let us represent numbers as Hilbert-strokes and the notion of predecessor of a given number via substrings; then, 'numbers finitely far from zero' could be translated as 'strings that, given our real world limitations, can be written down'. But, again, given our world limitations, we will only be able to to write a maximum amount of such numbers.[14] It follows that what counts as a number finitely far from zero is somehow dependent on, say, what we can write down before the heat death of the universe – for certainly, given the *real world limitations on our abilities*, we will never be able to *actually* write down strings which take more time to write than the time remaining until the heat death of the universe. Of course, this is an absurd conclusion. And charity in interpretation requires us to assume that this cannot be what the author intends by practical recursivity. The problem is clear: what is missing is the possibility of talking about what we can write down *without an upper limit*, that is, what we can write down *in principle*. Now, Thoralf will ask what *in principle* means, and the only sensible response we may give is that it means *given time and world enough*, i.e. given an *arbitrary finite number* of stages to write down strokes. And now circularity is clear.

Similar in Tennenbaum's case. Our everyday computational practice shows that we are proficient with sums, or multiplication for tractably small numbers. But the *real world limitations on our abilities* reveals only that we are good at computing sums for *only* tractable small numbers. If we wish to talk about what we can compute without upper limit, we have to talk about what we can compute *in principle*, given an *arbitrary finite number* of stages in a computation. And we are back with assuming what it was asked of us to prove. (Button & Smith, 2012 : 117 -119)

Button & Smith(2012) then conclude that if there is a genuine problem regarding model-theoretic scepticism, it is not to be solved with Tennenbaum's Theorem. The Theorem itself makes use of purportedly vague or circular notions, shifting the problem of the indeterminacy of the *natural number sequence* to the problem of the indeterminacy of those notions instead. Further, a practical rendering of those mathematical notions is only successful through great amount of idealization. What the argument from Tennenbaum's Theorem is in the end taken to show is just a special case of a more general failed dialectic strategy: mathematical indeterminacy is not to be solved with more model-theory; Thoralf's problem about how we determine the intended interpretation of, say, PA, will reappear as a problem about how we determine the intended interpretation of the richer theory by which we intend to supplement the former.

3.5 Tennenbaum's Theorem as Reference-Fixing

In our presentation of Button & Smith(2012) we stressed the similarities between the argument from Tennenbaum's Theorem and the argument from the Initial Segment Theorem. In a paper of the same year, Horsten too discusses both arguments reaching yet different conclusions.

[14]We will ignore complications introduced by the possibility of performing an infinitely long computation within finite time (supertask), such as the hypercomputational scenario of agents or Turing machines travelling a region with an infinite time-long trajectory, in a Malament-Hogarth spacetime. For more on this point see Manchak & Roberts(2016) and the references therein.

According to Horsten(2012 : 275), Halbach & Horsten(2005)'s main weakness is having wrongly conflated two different questions – the epistemic and linguistic problems of intended models specified above. Our presentation of that paper was primarily done in an epistemic way: we read the paper as claiming 'how do we know which model of arithmetic is the right one? Well, just use Tennenbaum's Theorem.' However, Horsten(2012) sees the argument from Tennenbaum's Theorem not as solving an epistemic problem, but instead as a solution to the reference-fixing problem: 'how do we manage to single out (refer to) the right isomorphism type of arithmetic? Well, just use Tennenbaum's Theorem'. In fact, this much is clear from the way he chooses to start his paper; Horsten is well-aware of the problem of singling out the structure of the natural numbers given non-standard models built by compactness and the like; but, he says:

> Now we could, with Skolem or Putnam perhaps, acquiesce in this conclusion, and deny that arithmetic has an intended interpretation that is unique up to isomorphism. But I will do the opposite. I will presume that we can isolate the natural-number structure in our referential practice. The question addressed in this article is how we have managed to refer to the natural-number structure: *how has our reference to the natural-number structure come about?* (Horsten, 2012 : 278)

The rest of the story is by now a familiar one: the reference of our arithmetical vocabulary is determined by arithmetical practice, together with the essential fact that numbers are things that we can calculate it. By Tennenbaum's Theorem the candidates for the reference of our vocabulary that respect the practice is just the standard model, up to isomorphism. This reference-fixing role given to the argument from Tennenbaum's Theorem is best illustrated in its relation with the argument from the Initial Segment Theorem. (Recall, we are now interested in determining how we manage to refer to the intended model, and not how we know that the model is intended.) Horsten considers the following thesis:

> **Thesis:** The reference of our arithmetical vocabulary is determined by our principles of arithmetic together with our ability to count up to every natural number. (Horsten, 2012 : 282)

A quick moments reflection makes it clear that the above Thesis shares the same strategy as the one in the argument from the Initial Segment Theorem. First, we plausibly assume that there is a finite amount of time required to count each number. As a consequence, by reflecting on our practice, we realize that only numbers finitely far from zero can (in principle) be counted. If the practice is to determine the intended interpretations, then only models whose elements have finitely many predecessors will be warranted. The thesis singles out the standard model, up to isomorphism, as the intended referent of our arithmetical vocabulary.

But to what extent does arithmetical practice motivate the Thesis? Suppose we are teaching a child what the natural numbers are. It is true that, when learning primary-school arithmetic, we learn how to effectively count arbitrarily many numbers. We learn that the natural numbers are 0, 1, 2, ..., *and so on.* And of course the question is how to understand the 'and so on'. For the Thesis to work, for practice to secure the intended reference, the 'and so on' must not include non-standard numbers. But how are we sure that the practice of arithmetic excludes non-intended interpretations? Well, after teaching the child to count arbitrarily many natural numbers, we may add

'All the natural numbers may be counted in this way'. And, after surveying Button & Smith's discussion, it is clear why this will just not do. As Horsten explains, for the child to understand our claim he has to understand its content; and this just means that he has already to understand what is a standard natural number and that for every standard natural number there is a finite ordered sequence of moments such that at the last of these that number is counted out. Nonetheless, given that the child still doesn't know what the natural numbers are, the child does not yet have the conceptual machinery to understand this. Hence, 'Mastery of the enumeration procedure for counting out the natural numbers is not sufficient in the context of Peano arithmetic to single out the natural-number structure. It does not guarantee that the domain of discourse exhausts the standard natural numbers.' (Horsten, 2012 : 284)

Nonetheless, he thinks that, unlike the above case, arithmetical practice concerning computability is able to successfully single out the intended interpretation. Unlike the counting procedure, the mastery of the addition algorithm is both total and computable. If it is not total, the child has not mastered the algorithm completely. If it is not computable, the child has not mastered the algorithm at all. More importantly:

> The child need not have any reflective knowledge about her algorithmic addition powers. She just has to know what to do in response to the teacher's instruction (such as '28, 23:add!'). When the child as acquired the right disposition, the admissible interpretations of her natural-number talk are restricted to an isomorphism type. [...] the child has to master the algorithm [...] the child *can* do this without first having to come to understand somewhat sophisticated concepts (such as the concept of finiteness). (Horsten, 2012 : 284-285)

In order for the child to master the counting algorithm and, more precisely, for her enumeration not to contain any non-standard numbers, the child must *know* that the counting procedure cannot contain non-standard numbers. And this presupposes the circular knowledge that all the numbers she can (correctly) count must be *finitely* far away from zero. On the other hand, Horsten thinks that we may come to master the algorithm to compute addition without any big number-theoretic pre-requisites, including that of finiteness. In fact, there is no need of reflexive knowledge of the practice for it to constrain the reference of our vocabulary. To elaborate, besides our use of an algorithm for computing sums, we may come to *know* we are using an effective procedure. But 'Knowing that we have an algorithm for computing sums is not needed to fix on the structure of the natural numbers: it suffices to simply adopt the algorithm.' (Horsten, 2012 : 281) Knowing the algorithm presupposes the notion of finiteness; but using the algorithm does not. And if we use the algorithm correctly, Tennenbaum's Theorem will guarantee (even if we are not aware of it) that we only compute with standard numbers.

Of course, as a response to the sceptic the argument leaves much to desire. After all, when explaining how we manage to determine the intended model we need to use notions as algorithm and, consequently, finiteness. We need to state some recursive procedure when explaining that we compute sums and this just seems to add more number-theoretic theory falling in circularity. What this shows is that the argument from Tennenmaum's Theorem may not be the best approach when dealing with the epistemic challenge of explaining how we know which model is intended. By itself, this does not affect Horsten(2012) for his goal is not to address the epistemic problem, but rather the reference-fixing problem.[15] And his point is just that Tennenbaum's Theorem allows us to refer to the intended model.

To be sure, Carrara, *et al.*(2016) disagree. They think that the use of the addition algorithm to fix the reference of our mathematical vocabulary is again circular, since to understand the notion of algorithm we presuppose the notion of a procedure computable in *finitely many* steps, and the same dialectic reappears. However, we should note that we think that Carrara, *et al.*(2016)'s critique is misguided, since Horsten(2012) does not assume that we need to have knowledge on how we perform with the addition algorithm, but only that we perform according to it. So nothing as 'understanding the addition algorithm' is presupposed when explaining how the intended reference gets fixed. See Horsten(2012 : 284-285).[16] Since our interest is in the epistemic challenge posed by the sceptic, and not on the reference-fixing problem, we have nothing much to add to this discussion.

3.6 (Dis)solving Skolemite Scepticism

3.6.1 Solving

The common thread throughout the critique of Tennenbaum's Theorem is the insistence on circularity brought by the dependence or interdefinability between the natural number sequence and the notion of *finiteness*. Carrara *et. al*(2016) propose an absolute or primitive notion of finiteness, not captured by any axiomatic system (not even in a model of set theory) nor reducible to other mathematical notions (like natural number). Their goal is obvious: if such a case for a primitive notion of finiteness can be made, it would then be possible to determine the natural number sequence.[17] They note that to recognize the existence of non-standard models of arithmetic we must first recognize or understand the language of (first-order) arithmetic and what would count as a structure satisfying its axioms. But, a point to which we will return to below, to understand the syntax of a first-order logical language we must understand the notion of a *finite strings of symbols*. Carrara *et al.* propose to account for this notion of a *finite string* in a way inspired by similar reflections made by Hilbert and Parsons on the matter. In an often quoted passage, Hilbert writes:

> As a condition for the use of logical inferences and the performance of logical operations, something must already be given to our faculty of representation, certain extra-logical concrete objects that are intuitively present as immediate experience prior to all thought. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated, is immediately given intuitively, together with the objects, as something that neither can be reduced to anything else nor requires reduction. This is the basic philosophical position that I consider requisite for mathematics and, in general, for all scientific thinking, understanding and communication. (Hilbert, 1926 : 376)

[15]'[...] the reference-fixing question is what I set out to address, not sceptical challenges. The question was: *given that most of what we think to know about the natural numbers is correct*, how have we managed to fix the reference of our arithmetical vocabulary? To assume that this requires us to answer sceptical challenges would be a mistake.' (Horsten, 2012 : 287)

[16]Thanks to Leon Horsten here in helping to clarify his view.

[17]For example, by the argument from the Initial Segment Theorem.

Here, the intuition of such objects, extra-logical in nature, is for Hilbert a precondition for logical reasoning itself. Parsons(1990) adds that these objects are 'quasi-concrete' in the sense of being instantiated and intuited from their spatial representations. This kind of intuition is immediate, that is, not mediated by any other object, akin to ordinary perception. It is Carrara *et al.*'s claim that mathematics relies on *intuition* and that mathematical knowledge is knowledge that relies on appropriate intuitions of some sort. For example, the induction schema of PA must be grounded in a similar primitive notion of finiteness. The intuition that any number is reachable from *finitely* many steps from 0 is essential for the evidence that if the antecedent of any instance of the axiom schema is true then *all* the numbers inherit the relevant property.[18] In this sense they stipulate a primitive understanding of finiteness, prior to the grasp of the induction axiom, obtained by some sort of intuition. It is this intuited notion that allows them to pin down the intended model, without circularity. They then conclude:

> Our claim is that the standard model is characterized by the fact that every natural number has finitely many predecessors, where the notion of finiteness here involved is absolute and primitive. This means that it cannot be *defined* in terms of more elementary notions. (Carrara *et al.*, 2016 : 316)

Now, is this intuition again 'unhelpful as epistemology and unpersuasive as science.'?, (*op. cit.*) as Putnam(1980) said. We are not sure. For once, it seems that that such intuition is presupposed in understanding PA itself. Further, if it is quasi-concrete, it is abstracted from ordinary experience – which is a far cry than other much more mysterious kinds of mathematical intuition such as the Gödelian kind (see Gödel(1947)). Perhaps then a case can be made to accommodate this intuition within a moderate view. We do not wish to elaborate this matter further, but only point out the possibility of further improvement. If so, this strategy illustrates a further way to resist the sceptic. But is there a sceptical challenge to resist at all?

3.6.2 Dissolving

We made reference above to the fact that Button & Smith(2012) are themselves sceptical regarding the viability of model-theoretic scepticism. They 'do not suppose that Thoralf's problem *is* a genuine problem'. (Button & Smith, 2012 : 119). This because in order for the sceptic to argue for the indeterminacy of mathematical notions she has to suppose their own determinacy. To clarify what is meant here, first, it is worth recalling that first-order logic lacks expressive power to capture the notion of *finiteness* or *recursive procedure*. Still, technically we do need to assume a notion of finiteness in order to define the syntax of first-order logic: we say that a first-order formula has an arbitrary but finite length, that it is a finite sequence of symbols arranged with certain rules. Similarly, well-formed formulas or a satisfaction relation are defined first by a base-clause for atomic formulas and then by recursive conditions for the others. Hence, it is clear that the use of first-order logic and first-order model theory presuppose a firm understanding of the relevant notions – *finiteness* and *recursive procedure*. As a consequence, in order for the skolemite sceptic to put forward his model-theoretic scepticism, he must make obvious use of first-order model theory and assume a grasp of the notions for which he argues there can be no such grasp. More generally, in using some model theory to argue for the indeterminacy of

[18] Of course this relies on the assumption that we do have a thorough understanding of the induction schema, which the sceptic may reject.

the concept of natural number, the sceptic must make use of conceptual tools that allow her (and us) to determine the natural numbers.[19]

To illustrate the latter point, consider again the argument from the Initial Segment Theorem. The argument pins down the standard model, up to isomorphism, as intended of our practice. But the sceptic is keen on pointing out that when using the Initial Segment Theorem we make implicit use of the (determinacy of the) notion of *finiteness*, interdefinable with *natural number*. Therefore she will argue that even when stating the argument we already assume what we wanted to prove: that *natural number* is already a determinate concept. Yet, in order for the sceptic to argue for the indeterminacy of the natural number sequence, she herself must implicitly assume the (determinacy of the) notion of *finiteness* when employing her model-theoretic paraphernalia. And this leads to the following dilemma: either (a) *finiteness* is not determinate, in which case the sceptical problem cannot even be posed; or (b) *finiteness* is determinate, in which case the sceptical problem can be easily solved[20].

> The point of all this is the following. In order to understand why model theory is supposed to push us towards sceptical concerns, we must possess certain model-theoretic concepts. However, possession of those model-theoretic concepts enables us to brush aside the sceptical concerns. Accordingly: insofar as we can understand the sceptical challenge, we can dismiss it. (Button & Walsh, 2018 : 208)

By itself, this does not mean that everything is fine with the moderate realist view. As we saw, Skolem-Putnam's challenge is a very natural problem to pose to this position: if we have only modest epistemic access to numbers, how can we know how they are like? What the above considerations show is rather that moderate realism is led to a deeply incoherent sceptical position, highlighting that something must be wrong not only with skolemite scepticism but with moderate realism itself too.[21] Further, this considerations still do not suffice to show how we come to acquire determinate mathematical beliefs or that we can acquire them; only that the sceptical challenge does not stand.

Hence, the dissolution claims that the model-theoretic challenges cannot even be posed in a non-self-refuting way; in contrast, the solution assumes that the model-theoretic challenges can be meaningfully posed, but also that they can be meaningfully answered given that we already possess a primitive notion of *finiteness*.

3.7 Summary

In this chapter we have presented the argument from Tennenbaum's Theorem and how it can be put to work against the sceptical challenge. After surveying its different uses

[19]See Bays, 2001 : § IV. 'This, then, is what I like to call the *stability objection* to Putnam's argument. The argument rests on the assumption that we cannot use semantically indeterminate language to describe "intended interpretation". But, by Putnam's own standards, the notions needed to formulate first-order model theory turn out to be semantically indeterminate. So, since Putnam's own techniques for obtaining intended interpretations involve first-order model theory, his position is logically unstable.' (Bays, 2001 : 346)

[20]For example, again, by the argument from the Initial Segment Theorem.

[21]The proper assessment of this issue is heavily dependent on the exegetics of Putnam's own work. Therefore we do not have much to add here except directing the reader to Button(2013) for an elaborate exposition.

and foibles, we concluded that perhaps the sceptical challenge is by itself incoherent there being no need for an argument from Tennenbaum's Theorem at all.

In what follows we wish to give new life to the sceptical challenge, showing that neither the solution nor the dissolution are enough to resist it. Further, we want to present a new argument (what we will call *the LP-argument*) against the argument from Tennenbaum's Theorem. We will see the unexpected consequences of our argument; if they can motivate skolemite scepticism depends on minute philosophical details that we postpone until the next chapter.

Chapter 4

Non-Classical Skolemite Scepticism

4.1 Introduction

We have just covered two main results: first, the argument from Tennenbaum's Theorem and, second, the sceptical solution/disolution. Let us quickly recap the former: the argument starts by assuming that in intended models the operation denoted by $+$ is a recursive function. In fact, putting it in this way is already assuming too much. When properly seen, the basic claim here is that in intended models the operation denoted by $+$ is an 'informally-computable' function. (Below we elaborate on this point, but for now it suffices to keep the above in mind.) Given this assumption, coupled with the Church-Turing Thesis and with Tennenbaum's Theorem, it follows that the intended model of arithmetic is restricted to a single isomorphism type, the class of models isomorphic to $\mathcal{N}$. Yet things are not so simple: the argument presupposes more than what makes explicit, being the purpose of this chapter to show exactly what the hidden assumptions amount to. As we will argue, for the argument to work as expected a 'classicality-constraint' must be assumed. We will show this by considering the non-classical (paraconsistent) semantics LP (see Priest, 1979) and the models of PA obtained with this logic. A by-product of our presentation will be the reintroduction of the sceptical challenge against the solution/dissolution proposals. In arguing against the argument from Tennenbaum's Theorem we will see how the sceptic is happy to embrace a determinate notion of finiteness without leading to a full-blown determinacy of arithmetic's interpretation nor contradicting his own sceptical qualms. It is the goal of this chapter to see how neither the strength of the argument from Tennenbaum's Theorem nor of the sceptical (dis)solution remain unaffected by considering non-classical models of arithmetic.

4.2 Inconsistent LP-Models of Arithmetic

4.2.1 LP-Language and Semantics

The logic LP is a strong-Kleene semantics proposed originally in Priest(1979) (and hinted before by Ansejo(1966)) in order to model contradictions (and logical paradoxes, in particular) without explosion. The language LP is the language of first-order logic, including function symbols and identity (with terms and formulae defined inductively in the usual way).

> **Definition** (LP-Structure) For a signature $\mathcal{L}$, an LP-$\mathcal{L}$-structure $\mathcal{M}$ consists of:
>
> 1. a non-empty set of elements M called the domain of $\mathcal{M}$;
> 2. for each constant symbol c in the signature, an object $c^{\mathcal{M}} \in M$.
> 3. for each n-ary function symbol f in the signature, a function $f^{\mathcal{M}}$: $M^n \to M$;
> 4. for each n-ary relation symbol R in the signature, an ordered tuple $R^{\mathcal{M}} = \langle R^{+^{\mathcal{M}}}, R^{-^{\mathcal{M}}} \rangle \subseteq M^n \times M^n$

A denotation function defined for an LP-structure is very much like the classical first-order structure with the exception of the relation symbols. Intuitively, $R^{+^{\mathcal{M}}}$ is the *extension* of the relation in the structure, that is, the set of objects or tuples true of R, and $R^{-^{\mathcal{M}}}$ its anti-extension. Clearly, in a classical structure we may associate a relation with its anti-extension by $R^{-^{\mathcal{M}}} = M^n \backslash R^{+^{\mathcal{M}}}$; i.e. classically, the anti-extension of a relation is the complement of its extension. In LP this does not need to hold: though we stipulate $R^{+^{\mathcal{M}}} \cup R^{-^{\mathcal{M}}} = M^n$ (and so we have excluded middle), it is *not* assumed $R^{+^{\mathcal{M}}} \cap R^{-^{\mathcal{M}}} \neq \emptyset$. This is why we have to explicitly define the anti-extension of a relation.

> **Definition** (LP-model) For a theory T in the signature of $\mathcal{L}$, the LP-$\mathcal{L}$-structure $\mathcal{M}$ is a model of T iff, for every formula $\varphi \in T$, $\mathcal{M} \models_{LP} \varphi$.

Since we are working with a non-classical semantics, logical consequence $\models_{LP}$ does not behave classically. For convenience, we first define a valuation v, relative to a structure $\mathcal{M}$'s interpretation function, as a function taking formulas to truth values such that $v(\varphi) \in \wp(\{1,0\}) - \emptyset$, where $\{1\}$ and $\{0,1\}$ are designated values.[1] Now, if φ is atomic and t_1 .., t_n are terms, then $1[0] \in v(R(t_1, ..., t_n))$ iff $\langle t_1^{\mathcal{M}}, ...t_n^{\mathcal{M}} \rangle \in R^{+[-]^{\mathcal{M}}}$.[2] The other cases are as follows:

- $1[0] \in v(\neg\varphi)$ iff $0[1] \in v(\varphi)$;
- $1[0] \in v(\varphi \wedge \psi)$ iff $1[0] \in v(\varphi)$ and[or] $1[0] \in v(\psi)$;
- $1[0] \in v(\forall x \varphi)$ iff $1[0] \in v(\varphi(x/d)))$ for all[some] $d \in D$, with $v(\varphi(x/d)))$ being the valuation that results in assigning to the variable x the element d.[3]

Disjunction, implication and existential quantification have their normal definitions through the dual of the other logical connectives, such that:

[1] Intuitively, '$v(\varphi) = \{1\}$' can be read as 'φ is *only* true, '$v(\varphi) = \{0\}$' as 'φ is *only* false', and '$v(\varphi) = \{1,0\}$' as 'φ is *both* true and false'. Despite intuitive this need *not* be the case. As Barrio & Da Ré(2018, 161-162) persuasively argue 'There is no intrinsically dialetheic value for LP. [...] [T]he intermediate value might be interpreted in a non-dialetheic fashion. In other words, to admit three-valued semantics does not compel us to interpret the intermediate semantic value, this pure value, in any particular way.'

[2] This is to be read $1 \in v(R(t_1, ..., t_n))$ iff $\langle t_1^{\mathcal{M}}, ...t_n^{\mathcal{M}} \rangle \in R^{+^{\mathcal{M}}}$, and $0 \in v(R(t_1, ..., t_n))$ iff $\langle t_1^{\mathcal{M}}, ...t_n^{\mathcal{M}} \rangle \in R^{-^{\mathcal{M}}}$. Throughout we will adopt this convention where 'x[y] iff z[w]' is to be read as the two clauses: 'x iff z' and 'y iff w'.

[3] For simplicity, we take the names of the elements of the domain as being the elements themselves, so that: if $d \in M$, then $d^{\mathcal{M}} = d$.

- $v(\varphi \vee \psi) = v(\neg(\neg\varphi \wedge \neg\psi))$;
- $v(\varphi \to \psi) = v(\neg\varphi \vee \psi)$;
- $v(\exists x\varphi) = v(\neg\forall x\neg\varphi)$.

We then say that a model satisfies a formula if it comes out at least true under the model (though it may be both true and false):

> **Definition** (LP-Satisfaction) Given an LP-structure $\mathcal{M}$ and formula φ, we say that φ is *satisfiable* in $\mathcal{M}$ and write $\mathcal{M} \models_{LP} \varphi$ iff the valuation function under the model's interpretation is such that $1 \in v(\varphi)$.

It is easy to see that every classical model is isomorphic to an LP-model in which all atoms (and, therefore, all formulas) take either the value $\{1\}$ or $\{0\}$. Hence,

> **Definition** (Classical/Consistent LP-Model) An LP-model $\mathcal{M}$ is a *classical* or *consistent* LP-model iff, for every atomic φ, $v(\varphi) \in \{\{1\}, \{0\}\}$.
>
> **Definition** (LP-Validity) Given a formula φ, we say that φ is *valid* and write $\models_{LP} \varphi$ iff $1 \in v(\varphi)$ for every model $\mathcal{M}$ and associated valuation v.
>
> **Theorem** For arbitrary φ, $\models_{LP} \varphi$ iff $\models_L \varphi$[4]
>
> *Proof.* We first show a small Lemma:
>
> > **Lemma** Let v be an arbitrary LP-valuation. For arbitrary n-ary relation R, we define
> >
> > $$v^*(R(t_1, ..., t_n)) = \begin{cases} v(R(t_1, ..., t_n)) & \text{iff } v(R(t_1, ..., t_n)) \in \{\{1\}, \{0\}\} \\ \{1\} & \text{otherwise} \end{cases}$$
> >
> > Then, for arbitrary φ, $v^*(\varphi) \subseteq v(\varphi)$.
> >
> > *Proof.* The proof is by induction on the complexity of φ. □
>
> **(⇒)** Suppose $\models_{LP} \varphi$. Then φ is (at least) true under every LP-valuation v. Since every classical valuation (that is, without formulas both true and false) is an LP-valuation, it follows that φ is true under every classical valuation. Hence, $\models_L \varphi$.
>
> **(⇐)** By contraposition. Suppose $\not\models_{LP} \varphi$. There is an LP-valuation v under which φ is only false. This means that $1 \notin v(\varphi)$.
>
> By the above Lemma, there is a valuation v^* such that $v^*(\varphi) \subseteq v(\varphi)$. From this latter fact and from $1 \notin v(\varphi)$, it follows $1 \notin v^*(\varphi)$. Since by construction v^* is a classical valuation that falsifies φ, we have $\not\models_L \varphi$. □

Now, for semantic consequence:

[4] Recall that an L-theory is a classical theory in the language of first-order logic.

Definition (LP-Consequence) Given a formula φ and set of formulas Γ, we say that φ is a *(semantic) consequence* of Γ and write $\Gamma \models_{LP} \varphi$ iff for every model $\mathcal{M}$ such that $\mathcal{M} \models_{LP} \gamma$ for all $\gamma \in \Gamma$ it is the case that $\mathcal{M} \models_{LP} \varphi$.

Theorem If $\Gamma \models_{LP} \varphi$, then $\Gamma \models_{L} \varphi$. The converse of the implication is false.

Proof. If $\Gamma \models_{LP} \varphi$ then all valuations are truth preserving. Hence, all classical valuations are truth-preserving. Therefore, $\Gamma \models_{L} \varphi$.

To see why the converse does not hold it suffices to give a counterexample. Consider an LP-valuation v with $v(\alpha) = \{1, 0\}$ and $v(\beta) = \{0\}$. Then, $v(\alpha \to \beta) = \{1, 0\}$, from where it follows: $\alpha, \alpha \to \beta \not\models_{LP} \beta$. $\square$

We end this section with some notable classical validities that do not hold in LP. It is easy to show that the there are LP-models where the following fail (Priest, 1979, § III.14.)[5]:

$$\varphi, \varphi \to \psi \models \psi$$
$$\varphi \to \psi, \neg\psi \models \neg\varphi$$
$$\varphi \to \psi, \psi \to \chi \models \varphi \to \chi$$

4.2.2 Collapsed models

Paraconsistent collapsed models of arithmetic are essentially structures obtained by quotienting classical models of arithmetic such that their elements are collapsed through an equivalence relation that is also a congruence relation on the operations of successor, addition and multiplication. The main difference here is that, unlike classical quotient algebras of $\mathbb{N}$, the elements in a collapsed model gain the non-identity of its members: if $[x] = \{x, y\}$ and $x \neq y$, then $[x] \neq [x]$. Though the techniques stem originally from the work of Meyer(1976), by far the most well-studied constructions have been elaborated by Priest(1997) and Priest(2000) using the logic LP.

For ease of symbolism, we let I denote the interpretation of the non-logical vocabulary in a model. Let $\sim$ be an equivalence relation on the domain M of a consistent LP-model $\mathcal{M}$ of PA, such that it is also a congruence relation on the interpretation of the function symbols; that is, for n-ary function symbol f and elements $d_1, ..., d_n, e_1, ..., e_n \in M$, if $d_i \sim e_i$ (for $1 \leq i \leq n$) then $I(f)(d_1, ..., d_n) \sim I(f)(e_1, ..., e_n)$. Given $\mathcal{M}$ and $\sim$, we call $\mathcal{M}/\sim$ the *collapsed model* of $\mathcal{M}$ under $\sim$. In order to construe a collapsed model, we will then have to define the domain of the new model and the denotation of the vocabulary through $I^{\sim}$.[6] For this, we let the domain $M/\sim$ of $\mathcal{M}/\sim$ to be the set of equivalence classes obtained by the partition defined on M under $\sim$. That is, $M/\sim \; = \; \{[m] \mid m \in M\}$.[7] We can define a collapsed interpretation $I^{\sim}$ as:

- For every constant c, $I^{\sim}(c) = [I(c)]$
- For every n-place function f, $I^{\sim}(f)([d_1], ..., [d_n]) = [I(f)(d_1, ..., d_n)]$

[5]Throughout we will mostly write $\models$ for $\models_{LP}$ letting context disambiguate.
[6]$I^{\sim}$ corresponds to the interpretation in the collapsed model.
[7]As usual, $[i] = \{x \mid x \sim i\}$.

- For every n-ary predicate P, $\langle [d_1], ..., [d_n] \rangle \in I^{\sim +[-]}(R)$ iff for some $e_1 \sim d_1$, ..., $e_n \sim d_n$ it follows $\langle e_1, ..., e_n \rangle \in I^{+[-]}(R)$
- $I^{\sim}([x] = [y]) = I^{\sim}(x \sim y)$
- $I^{\sim}([x] \neq [y]) = I(x \neq y)$

As a consequence:

- $I^{\sim}(0) = [I(0)]$
- $I^{\sim}(S([x])) = [I(S(x))]$
- $I^{\sim}([x] + [y]) = [I(x + y)]$
- $I^{\sim}([x] \times [y]) = [I(x \times y)]$

We quickly note that the collapsed interpretation for function symbols is well-defined since $\sim$ is by assumption a congruence relation. Otherwise, there would be no natural way to define the collapsed interpretation; for instance, without congruence, we could not let $I^{\sim}(f)([d]) = [I(f)([d])]$ (for u-nary f) since $d \sim e$ and $[d] = [e]$ need not imply $[I(f)([d])] = [I(f)([e])]$. Now, the important point is that $I^{\sim}$ identifies all members in the same equivalence class, producing a composite element (that is, the equivalence class itself) inheriting all the properties of its members (even if these properties are inconsistent). From the way the collapsed model is defined, the crucial Lemma follows:

> **Collapsing Lemma:** For any LP-interpretation I and arbitrary φ:
>
> $$v_I(\varphi) \subseteq v_{I^{\sim}}(\varphi)$$
>
> $v_I, v_{I^{\sim}}$ are valuation functions under their respective interpretations.
>
> *Proof.* The proof is by induction on the complexity of φ. See Priest(1991). $\square$

The Lemma guarantees that in collapsing a model no truths are lost. If in the original interpretation $1 \in v_I(\varphi)$ then $1 \in v_{I^{\sim}}(\varphi)$ (and similarly with 0), though there may be formulas ψ such that $\{1, 0\} \in v_{I^{\sim}}(\psi)$.

Linear Models

In order to better understand the significance of the above result we want to introduce a special class of collapsed models called *linear models*. The following Definition and Lemma from classical PA will prove themselves very useful.

> **Definition** For a classical model $\mathcal{M}$ of PA, we call $S \subseteq M$ a *slice* iff S is an initial section of $\mathcal{M}$ (i.e. if $x < y$ and $y \in S$, then $x \in S$) closed under successor, addition and multiplication. We say S is *proper* iff $S \neq \mathbb{N}$ and $S \neq M$.

> **Lemma** For every non-standard classical model $\mathcal{M}$ and slice $S \subset M$, there is a proper slice that extends S.

Proof. Consider a non-standard model $\mathcal{M}$ and let S be a slice such that $S \subset M$. We want to show there is a proper slice that extends S. First, we note that $\mathbb{N}$ is the smallest slice of $\mathcal{M}$ and so $\mathbb{N} \subseteq S$. Take $a \notin S$; then, a is non-standard. Define

$$a^{\mathbb{N}} = \{m \in M \mid \exists n \in \mathbb{N} : \mathcal{M} \models m < a^n\}$$

It is easy to see that $a^{\mathbb{N}}$ is closed under successor, addition and multiplication. For the successor case consider $x \in a^{\mathbb{N}}$; then, there is an $n \in \mathbb{N}$ such that $x < a^n$. Then, either (a) $x+1 < a^n$ in which case $x+1 \in a^{\mathbb{N}}$, or (b) $x+1 = a^n$ in which case $x+1 < a^{n+1}$ and $x+1 \in a^{\mathbb{N}}$.[8] Similar, for the other cases. So that $a^{\mathbb{N}}$ is a slice.

Define $\varphi(x,a) = \exists x(x < a)$. It is clear that for every $n \in \mathbb{N} : \mathcal{M} \models \varphi(n,a)$. From this latter fact and from Overspill, we know that there is a non-standard $c \in M$ with $\mathcal{M} \models \varphi(c,a)$ so that $c \in a^{\mathbb{N}}$. Hence, since $c \notin \mathbb{N}$ we have $\mathbb{N} \neq a^{\mathbb{N}}$.

Also, it is clear that $a^{\mathbb{N}} \subseteq M$. But, since for every $n \in \mathbb{N}$ we have $a^n < a^a$ it follows that $a^a \notin a^{\mathbb{N}}$ and $a^{\mathbb{N}} \neq M$. Hence, $a^{\mathbb{N}}$ is a proper slice that extends S. □

Consider a classical model $\mathcal{M} \models$ PA, standard or non-standard. Define for $\mathcal{M}$ a chain $\{S_i \mid i \leq \mu \text{ with } 0 \leq \mu \leq \omega\}$ of strictly initial segments of M such that: $S_\mu = M$ and S_j a slice (for $0 < j \leq \mu$). It is important to note that given the way it is defined S_0 is not necessarily a slice, but only an initial segment of the model. Now, for $0 < j \leq \mu$, define $C_j = S_j - S_{j-1}$. Also, for $0 < j \leq \mu$, we let p_1 be a non-zero (possibly non-standard) number with $p_1 \in S_1$ and if $j < k$, then p_j is a multiple of p_k. We define a relation $\sim$ such that:

Definition We say that $\sim$ is a *linear* relation if it is of the form:

$x \sim y$ iff
$(x, y \in S_0 \wedge x = y) \vee (\text{for some } i > 0 \text{ and } x, y \in C_i : x = y \pmod{p_i})$

Lemma $\sim$ is an equivalence relation and a congruence relation with respect to successor, addition and multiplication.

Proof. That $\sim$ is an equivalence relation follows easily from inspection of cases together with the fact that equality and congruence modulo n are themselves equivalence relations. For congruence:

- Successor: Assume $x \sim y$. Then, either (a) $x, y \in S_0$ or (b) for some $i > 0$, $x, y \in C_i$. Suppose (a). Then, $x = y$. Hence, $S(x) = S(y)$. If $S(x), S(y) \in S_0$, then $S(x) \sim S(y)$. If for some $i > 0$, $S(x), S(y) \in C_i$, then $S(x) = S(y) \pmod{p_i}$, and $S(x) \sim S(y)$. Suppose (b). Then, $x = y \pmod{p_i}$, from where it follows $S(x) = S(y) \pmod{p_i}$. Also, from the fact that C_i is closed under successor, it follows that $S(x), S(y) \in C_i$. Hence, $S(x) \sim S(y)$.
- Addition: Assume $x_1 \sim y_1$ and $x_2 \sim y_2$. Then, either (a) $x_1, x_2 \in S_0 \vee y_1, y_2 \in S_0$, or (b) for some $i, j > 0$ (not necessarily distinct),

[8] A better notion here for the successor of x would be $S(x)$. We choose it to write it as $x+1$ to avoid ambiguity with the slice S.

$x_1, x_2 \in C_i \wedge y_1, y_2 \in C_j$. Suppose (a). Assume, without loss of generality that the first disjunct holds; that is, $x_1, x_2 \in S_0$. Then, $x_1 = x_2$. Now, if $y_1, y_2 \in C_0$ then $y_1 = y_2$, and then $x_1 + y_1 = x_2 + y_2$, from where it follows, $x_1 + y_1 \sim x_2 + y_2$, irrespectively of the C_i or S_0 that has them as elements. If, for some $i > 0$, $y_1, y_2 \in C_i$, then $x_1 + y_1, x_2 + y_2 \in C_i$, for C_i is closed under addition. Then, since modulo operation is congruent on addition, it follows $x_1 + y_1 = x_2 + y_2 \pmod{p_i}$ and, therefore, $x_1 + y_1 \sim x_2 + y_2$. Suppose (b). Then $x_1 = x_2 \pmod{p_i}$ and $y_1 = y_2 \pmod{p_j}$. Assume, without loss of generality, $i < j$. Then, p_i is by construction a multiple of p_j. Hence, $x_1 = x_2 \pmod{p_j}$. Hence, $x_1 + y_1 = x_2 + y_2 \pmod{p_j}$. Also, since C_j is closed under addition, $x_1 + y_1, x_2 + y_2 \in C_j$. Hence, $x_1 + y_1 \sim x_2 + y_2$.

- Multiplication: (Same as Addition).

$\square$

Definition We call an *LP*-model *linear* if it is obtained by collapsing a classical model under a linear relation.

Linear models have a tail comprising an initial segment of the original model followed by μ-many cycles such that for each cycle C_i (where $i>0$) the period is p_i. For instance, consider $\mathcal{M}$ a classical non-standard model of PA and a chain of strictly initial segments $S_0 \subseteq S_1 \subseteq S_2$ with $S_0 = \{m \mid m < n\}$ (for some finite n), $S_1 = \mathbb{N}$ and $S_2 = M$. Letting p_1, p_2 stand for the (finite) period of the cycles (with p_1 multiple of p_2) and c non-standard, we consider the linear relation:

$x \sim y$ iff

- $(x, y < n \wedge x = y)$ or
- $(n \leq x, y < \omega \wedge x = y \pmod{p_1})$ or
- $(x, y > \omega \wedge x = y \pmod{p_2})$

The relation produces a collapsed model $\mathcal{M}/\sim$ with a tail isomorphic to an initial segment of $\mathbb{N}$ of length n, followed by two cycles of period p_1 and p_2. Letting the arrows represent the successor operation, the successor graph of the collapsed model is then[9]:

$$\begin{tikzcd}
{[0]} \arrow[r] & {[1]} \arrow[r] & {[2]} \arrow[r] & {[...]} \arrow[r] & {[n]} \arrow[r] & {[n+1]} \arrow[d] & {[c]} \arrow[r] & {[c+1]} \arrow[d] \\
 & & & & {[n+p_1-1]} \arrow[u] & {[...]} \arrow[l] & {[c+p_2-1]} \arrow[u] & {[...]} \arrow[l]
\end{tikzcd}$$

Since the model has finitely many elements it is obviously finite and with a different order type than the standard model. Since by assumption $\mathcal{M} \models$ PA by the Collapsing Lemma we have that $\mathcal{M}/\sim\ \models$ PA. We then have an example of a finite model of arithmetic.

[9] We assume, for illustration, that n is greater than 2. Of course, if $n = 2$ or $n = 1$, the tail would be shortened.

For the rest of the paper we will be mostly interested in linear structures obtained by collapsing the standard model $\mathcal{N}$. In building collapsed models of $\mathcal{N}$, μ must either be 0 or 1, for the standard model only has one slice (i.e. $\mathbb{N}$ itself). If $\mu = 0$ we consider a single strict initial segment of the model, corresponding to its entire domain $\mathbb{N}$ and build $N/\sim$ by collapsing the initial structure with $x \sim y$ iff $x, y \in \mathbb{N} : x = y$. The resulting structure will produce an isomorphic copy of $\mathbb{N}$, where each integer is raised to its type lift (that is, if $n \in \mathbb{N}$, then $[n] = \{n\} \in \mathbb{N}/\sim$). A more interesting construction is letting $\mu = 1$. Here, there are two possibilities. If $S_0 \neq \emptyset$, the collapsed model consists of a tail of finite length followed by a cycle also of finite period. The linear relation will be:

$$x \sim y \text{ iff } (x, y < n \wedge x = y) \vee (x, y \geqslant n \wedge x = y \pmod{p_1})$$

And the successor graph will be:

$$\begin{tikzcd}
[0] \arrow[r] & [1] \arrow[r] & [...] \arrow[r] & [n] \arrow[r] & [n+1] \arrow[d] \\
 & & & [n+p_1-1] \arrow[u] & [...] \arrow[l]
\end{tikzcd}$$

The relation puts each integer up to n in its own equivalence class, producing a cycle of period p_1. It is easy to see that the model is inconsistent for

$$\mathcal{N}/\sim \ \models [n] = [n+p_1] \wedge [n] \neq [n+p_1]$$

The first conjunct follows from $n \sim n+p_1$, whereas the second from $n \neq^{\mathcal{N}} n+p_1$. The collapsed model is non-trivial since it can be easily checked that $\mathcal{N}/\sim \ \not\models [0] = [1]$. But again by the Collapsing Lemma, $\mathcal{N}/\sim \ \models$ PA. We will call such models with finite tail followed by a cycle, a *heap* model.

If $S_0 = \emptyset$, the collapsed model will only be formed by a cycle also of finite period. Again as an example, we consider collapsing $\mathcal{N}$ under:

$$x \sim y \text{ iff } (x = y \pmod{p_1})$$

The successor graph of $\mathcal{N}/\sim$ may be depicted as:

$$\begin{tikzcd}
[0] \arrow[r] & [1] \arrow[d] \\
[p_1-1] \arrow[u] & [...] \arrow[l]
\end{tikzcd}$$

It is also straightforward to see how this new $\mathcal{N}/\sim$ is inconsistent but non-trivial. Further, again, $\mathcal{N}/\sim \ \models$ PA. We will call such models with a single cycle, a *cyclic* model.[10] In fact, heap and cyclic models are the only 'interesting' collapses of the standard model[11] – any interesting equivalence relation must identify two different numbers; supposing j to be the least inconsistent number (i.e. the least number identified with another number) and k the least number greater than j that is identical

[10]Interestingly, these were the first kind of inconsistent models studied in Meyer(1976).
[11]By 'interesting' we mean a model not-isomorphic to the standard model.

with it, we then have that $S_0 = \{m \mid m < j\}$ and $p_1 = k - j$. This means that the collapse produces a *linear* model.

We then have the following structure for finite linear LP-models. The general structure is a finite tail, followed by a cycle of standard numbers (or, better, their type lift) and a finite collection of cycles of non-standard numbers. Having surveyed some important kinds of inconsistent models and the underlying logic, we move to a problem in comparing them.

Isomorphic LP-Models

We think that there might be a small complication for the argument we will want to put forward later that we should better tackle with now. Recall that when defining an LP-structure we must associate not only an extension but also an anti-extension to each relation symbol R. However, when presenting a classical first-order structure we only associate an extension to R, for the anti-extension may be easily obtained by taking the complement. But then, if an LP-structure imposes different conditions on the interpretation of the relation symbols, how should we capture the notion of 'isomorphism' when paraconsistent structures are involved? To see why this question is not at all trivial let us consider the classical case for a bit.

Classicaly, for a function to establish an isomorphism between two models $\mathcal{M}$ and $\mathcal{N}$ it must be the case that[12]

- For n-ary $\mathcal{L}$-relation symbol R, $(m_1, ..., m_n) \in R^{\mathcal{M}} \Leftrightarrow (\pi(m_1), ..., \pi(m_n)) \in R^{\mathcal{N}}$ (for $m_1, ..., m_n \in M$)

The clause only concerns the extension of R so a better formulation would be:

- For n-ary $\mathcal{L}$-relation symbol R, $(m_1, ..., m_n) \in {R^+}^{\mathcal{M}} \Leftrightarrow (\pi(m_1), ..., \pi(m_n)) \in {R^+}^{\mathcal{N}}$ (for $m_1, ..., m_n \in M$)

Since the biconditional is equivalent to $(m_1, ..., m_n) \notin {R^+}^{\mathcal{M}} \Leftrightarrow (\pi(m_1), ..., \pi(m_n)) \notin {R^+}^{\mathcal{N}}$ and since (assuming consistency) $a \in {R^+}^{\mathcal{M}} \Leftrightarrow a \notin {R^-}^{\mathcal{M}}$, the anti-extensions will also agree. That is, in the classical case, an isomorphism also guarantees that $(m_1, ..., m_n) \in {R^-}^{\mathcal{M}} \Leftrightarrow (\pi(m_1), ..., \pi(m_n)) \in {R^-}^{\mathcal{N}}$. But this doesn't need to happen in the LP-case. Now, a classical consequence of there being isomorphic models is elementary equivalence: $\mathcal{M} \cong \mathcal{N} \Rightarrow \mathcal{M} \equiv \mathcal{N}$. But we find that with the classical notion of isomorphism in place, we are not guaranteed that two isomorphic models will satisfy the same formulas when we consider supposedly isomorphic inconsistent LP-structures.

To see why this is the case we propose a simple example. Consider the two-element models $\mathcal{A}$ and $\mathcal{B}$ with no function symbols: $A = \{a_1, a_2\}$ and $B = \{b_1, b_2\}$ such that $\langle a_1, a_2\rangle \in R^{+\mathcal{A}}, R^{-\mathcal{A}} = \emptyset$ but $\langle b_1, b_2\rangle \in R^{+\mathcal{B}} = R^{-\mathcal{B}}$. Define a bijection $\pi : \mathcal{A} \to \mathcal{B}$, with $\pi(a_1) = b_1$ and $\pi(a_2) = b_2$. It is easy to note that the function defines an isomorphism in the classical case. Note that the condition:

- $(m_1, ..., m_n) \in {R^+}^{\mathcal{A}} \Leftrightarrow (\pi(m_1), ..., \pi(m_n)) \in {R^+}^{\mathcal{B}}$ (for $m_1, ..., m_n \in A$)

[12]The reader may want to consult the Appendix.

It is also satisfied. Of course, we do not have

- $(m_1, ..., m_n) \in R^{-^{\mathcal{A}}} \Leftrightarrow (\pi(m_1), ..., \pi(m_n)) \in R^{-^{\mathcal{B}}}$
  (for $m_1, ..., m_n \in A$)

But since the classical definition of isomorphism only (explicitly) requires the agreement of the extension of the relation symbols, we do not need for this latter clause to come out true. So the function is indeed an elementary embedding. However, both models are not elementary equivalent. For:

$$\mathcal{A} \models R(a_1, a_2)$$
$$\mathcal{A} \not\models \neg R(a_1, a_2)$$
$$\mathcal{B} \models R(b_1, b_2)$$
$$\mathcal{B} \models \neg R(b_1, b_2)$$

The philosophical problem that we found is the following. We want to make comparisons between LP-models and to be able to say when two models are the same *up to isomorphism*. In fact, as noted before, the goal of this chapter is to show that the argument from Tennenbaum's Theorem does not isolate a single isomorphism type. And this because, as we will argue, there are inconsistent LP-models that are not *isomorphic* to the standard model. However, what is the notion of isomorphism here? We may think that one necessary condition for two models to be isomorphic is elementary equivalence. Hence, if we use a notion like that of 'classical isomorphism', that allows for not elementary equivalent albeit isomorphic models in the LP-case, it might be suggested that the notion is not suitable to capture that which we want to capture when establishing isomorphic relations between inconsistent models. And if the above follows, we must provide a new definition to establish when these models are isomorphic – one that also captures that which we want to capture with the classical notion of 'isomorphism'. Otherwise, when claiming, as we will below, that there are inconsistent intended LP-models not *isomorphic* to the standard intended models, it might be counter-argued that we are using 'isomorphism' with a different meaning; and in this way it might not be established that those inconsistent models are 'really' non-isomorphic to the standard model.

A bit of further elaboration on the topic of isomorphic LP-structures is required for a better understanding of these constructions and their differences or similarities with more common classical constructions. As it will be seen below, we will go around such problems, bearing in mind that there is more to be said on exactly how two LP-models can be isomorphic.

4.3 Changing the Logic for Tennenbaum's Theorem

It is more than fair to ask how the long digression on paraconsistent models relates with our main problem at hand. In fact, nowhere in the literature do we find reference to non-classical theories of PA as being of technical and philosophical significance for the intendedness of arithmetical models and for Putnam's model-theoretic arguments. This is evident in the classicality constraints with which the debate on Tennenbaum's Theorem has been conducted. For instance, Halbach & Horsten(2005)'s argument aims to show that any model of PA where addition is computable is intended of our practice. However, what they leave implicit is the classical background of their thesis; when properly seen, Halbach & Horsten must rather state that 'any *classical* model of PA where addition is computable is intended of our practice'. And this because, as we will show, their argument does not work without a 'classicality constraint'; that is, insisting on the recursivity of addition without the associated claim that the models must be classical (or consistent) does not single out a unique isomorphism type as intended of arithmetic.

4.3.1 Sameness of Structure in LP-Models

Consider now the non-trivial heap model $\mathcal{N}^*/\sim$ obtained by collapsing the standard model $\mathcal{N}$ under:

$$x \sim y \text{ iff } (x, y < 3 \wedge x = y) \vee (x, y \geqslant 3 \wedge x = y \pmod 4)$$

The successor graph of $\mathcal{N}^*/\sim$ may be depicted as:

$$\begin{tikzcd}
{[0]} \arrow[r] & {[1]} \arrow[r] & {[2]} \arrow[r] & {[3]} \arrow[r] & {[4]} \arrow[d] \\
 & & & {[6]} \arrow[u] & {[5]} \arrow[l]
\end{tikzcd}$$

$\mathcal{N}^*/\sim$ puts each number up to 2 in their own equivalence class and, from 3 on, produces a cycle of period 4. We know, from the Collapsing Lemma, that $\mathcal{N}^*/\sim\ \models$ PA since $\mathcal{N} \models$ PA. We want to argue that $\mathcal{N}^*/\sim$ is a model of PA not isomorphic to the standard model $\mathcal{N}$. Of course, as we just saw, defining isomorphism between LP-models is a big task being difficult to see in what sense the notion of 'isomorphism' when specified for LP-structures is equivalent to that specified for classical first-order structures. Be that as it may, we find uncontentious that the notion of 'sameness of structure' is prior to that of 'isomorphism': it is through this latter mathematically formalized concept that we try to define the informal former one. And the following also seems to hold without great deal of argumentation required:

> If two models (broadly construed) have the same structure then:
>
> 1. both models have the same cardinality;
> 2. if the first model is of order type α then the second model is also of order type α.[13]

[13] It is very important here to note that our intuitions about what is sameness of structure are motivated by the background thought that isomorphism is *really* meant to capture sameness of structure. Now, when giving an approximate rendering of what it is for two models to be the same, we think of those conditions that are necessarily captured by an isomorphism: an isomorphism guarantees that the models are of the same size and same order type. To better clarify, suppose that one thinks that bissimulation instead is what captures sameness of structure. Then, the definition of sameness of structure may change. For instance, consider two bisimular Kripke models $\mathcal{M} = \langle W, R, V\rangle$ where one is a single reflexive point and the second an infinite chain. In a way, if bissimulation is supposed to capture sameness of structure then both models will have the same structure – but we could hardly say they have the same cardinality, or order-type. So our working hypothesis will be that sameness of structure is captured (more or less) by the notion of isomorphism. Hence, since any two isomorphic models must satisfy the above clauses, any two models with the same structure should also satisfy the above clauses. However, elementary equivalence is more of a by-product of the fact that two (classical) models are isomorphic rather than a really constitutive feature of the meaning of isomorphism. Hence, we do not include a clause for elementary equivalence in our definition.

Note that the conditional above is not a biconditional. This because we wish to allow the possibility of there being a more fine-grained understanding of 'sameness of structure' requiring more conditions to be fulfilled. In this sense, the two clauses need not be exhaustive of the relevant concept, being merely a set of necessary though perhaps not sufficient conditions for sameness of structure. We will not argue further for this point; we find the clauses to be constitutive of the ordinary and technical meaning of the notion to the extent that if someone maintains that two models can have the same structure without fulfilling one (or both) of the required clauses, then either (a) she is plainly wrong, or (b) she is using a different notion than what we normally mean by 'sameness of structure'. Given the intuitive character of the two clauses above, the burden of proof for the unnecessity of either one or both, when defining sameness of structure, must rest on our opponent.

Under this understanding do the models $\mathcal{N}^*/\sim$ and $\mathcal{N}$ have the same structure? First, is clause (1) satisfied? Well, $\mathcal{N}$ is infinitely countable and, therefore, by definition has $\aleph_0$ as cardinal number. On the other hand, $\mathcal{N}^*/\sim$ is finite:

- $\mid \mathcal{N}^*/\sim\mid\ = 7$, for $\mid \mathbb{N}^*/\sim\mid\ =\ \mid\{[0],[1],[2],[3],[4],[5],[6]\}\mid\ = 7$.

Since both models have different cardinalities, they do not have the same structure. But we should be careful here since in a way $\mathcal{N}^*/\sim$ may be seen to be infinite too! To see why this might be the case it is worth recalling a fun thought-experiment remarked in Denyer(1995 : 572) to the extent that no two inconsistent models have the same cardinality.[14] Consider two classical isomorphic copies of the standard model $\mathcal{N}_1$ and $\mathcal{N}_2$ and produce their respective collapsed models under our familiar relation:

$$x \sim y \text{ iff } (x, y < 3 \wedge x = y) \vee (x, y \geqslant 3 \wedge x = y \pmod 4)^{15}$$

We will have two collapsed models with a tail up to $[2_1]$ or $[2_2]$ and a cycle of period 4. Both models will have size 7. However, curiously enough, the second model will have size 8; to see this just note that regarding the domain of the second model:

- $\mid \mathbb{N}_2/\sim\mid\ =\ \mid\{[0_2],[1_2],[2_2],[3_2],[4_2],[5_2],[6_2],[7_2]\}\mid\ = 8$.

Now, it might be objected that the model only has 7 elements because $[3_2] = [7_2]$; however, it is also the case that $[3_2] \neq [7_2]$. Hence, by the latter fact, when counting the number of elements not identical with other ones in $\mathbb{N}_2/\sim$ we reach the total of 8 elements. Hence, the models do not have the same cardinality. Of course by assumption they also have the same cardinality! By iterating this process infinitely many times we will have that the cardinality of $\mathcal{N}_2/\sim$ is (countably) infinite.

What the example shows is that from the moment we identify $[x] = [x + p]$, with $x \neq x + p$ in the original model, we may follow a reason like the above reaching the conclusion that the model is infinite after all; even though it is also finite. In reality, there is an easier way to reach the same point, as noted in Priest(1996). Even though 'finiteness' cannot be defined in the language of first-order logic, there is a sense in which a *finite* inconsistent model is indeed finite, for it satisfies a formula for finiteness:

- $\mathcal{N}/\sim\ \models \forall x(x = 0 \vee x = 1 \vee ... \vee m = 1)$

[14] His original argument concerns categoricity; we adapt it here for our purposes.

[15] '3' and '4' is just a sloppy notation for the objects in each model that are isomorphic to the standard numbers 3 and 4 in the standard model; call those objects 3_1 and 4_1 in the model $\mathcal{N}_1$ and 3_2 and 4_2 in the model $\mathcal{N}_2$.

The model then 'says of itself' that it has at most finitely-many $m+1$ elements. But it is also true that a collapsed model will satisfy an expression of infinitude (or, at least, non-finitude):

- $\mathcal{N}/\sim\ \models \forall x \exists y \forall z \leq x(y \neq z)$

Hence, 'As regards the content of M [a finite inconsistent heap model], there is a sense in which it is true that it claims that there are finitely many numbers, and a sense in which it denies this.' (Priest, 1996: 650) So, is clause (1) satisfied for $\mathcal{N}$ and $\mathcal{N}^*/\sim$? Well, ... on one hand *No!* because both models have different cardinality, ... on the other hand *Yes!* because both models have the same cardinality (they are both countably infinite). But can we sensibly think this is what we mean by sameness of structure? The intuitive appeal of clause (1) is due to our experience with cardinalities *only* in the classical case: clause (1) works in the classical case for it is implicitly excluded the possibility of a model with different cardinalities. But when working with LP-models this intuition breaks down. What is needed is a refinement of the first clause in order to account for the inconsistent case. *Prima facie*, it would appear that sameness of structure does not put any upper bound on the amount of sizes models may come to have: when we say that 'if two models are the same, then they agree on the size' we just mean they agree on the size *for every possible size the models may have.* The fact that this assumption is only implicit is that when reasoning in the classical case a model with two different sizes is absurd; but we can make sense of this in the paraconsistent case and the idea of requiring of two models to have the same structure only if they agree on *all* their cardinalities seems to respect the spirit encoded in the *same structure, same size* motto. Hence, we refine our original formulation of 'sameness of structure' such that:

> If two models (broadly construed) have the same structure then:
>
> 1. if the first model is of cardinalities $k_0, ..., k_n$ then the second model is also of cardinalities $k_0, ..., k_n$ (for $n \in \mathbb{N}$);
> 2. if the first model is of order type α then the second model is also of order type α.

Note that the new clause (1) allows for the possibility of a model to have different sizes; all it requires is that if it has two (or more) different sizes (at the same time), then a model with a same structure must also have those two (or more) different sizes. Under the new definition, our models will not have the same structure for the clause is immediately violated:

- $|\ \mathcal{N}^*/\sim| = 7 \not\Rightarrow |\ \mathcal{N}\ | = 7.$

Hence, $\mathcal{N}$ and $\mathcal{N}^*/\sim$ do not have the same structure. To push the point further, we now focus on clause (2). Obviously, the order type of the standard model is ω. But $\mathcal{N}^*/\sim$ does not define a linear order ($[6] \leq [7] = [3] \leq [6]$), so that clause (2) is unsatisfied; the collapsed model is not a linear order and then cannot be isomorphic to an ordinal. With this our claim is strengthened: the structure of the models is not the same.

4.3.2 The LP-Argument

We have now established a sensible case for the distinct structural character of our models. Given that sameness of structure is a necessary condition for isomorphism, the models are not isomorphic, regardless the details on how this latter notion is specified. Now, the main point of our discussion is that $\mathcal{N}^*/\sim$ is also a model with a recursive addition! The denotation of the addition function in collapsed models was defined as:

$$I^{\sim}([x]+[y]) = [I(x+y)]$$

This means that $+^{\mathcal{N}^*/\sim}$ can be defined in terms of $+^N$ which, in its turn, is expressed by a finite number of applications of the primitively recursive successor function. As a consequence, $\mathcal{N}^*/\sim$ is a model non-isomorphic to the standard model with recursive addition. It might be suggested that to call $+^{\mathcal{N}^*/\sim}$ recursive is an abuse of terminology for, after all, we expect Turing machines to be consistent. We will discuss this point at great length in the next chapter. For now it suffices to note that $+^{\mathcal{N}^*/\sim}$ is (at least) clearly informally computable. Here is an algorithm to determine the output of $[x] +^{\mathcal{N}^*/\sim} [y]$:

1. Compute $x \ +^{\mathcal{N}} y$ instead, which is computable since $+^{\mathcal{N}}$ is computable too (for example, by PRAddition);
2. Lift the result (say z) to its type-lift $[z]$.

Collapsed addition is therefore informally computable. And this has, we will now argue, important consequences for the argument from Tennenbaum's Theorem. The argument states that in intended models addition must be computable. Since the requirement is supposed to be justified from within our actual arithmetical practice, this notion of computability must be informally construed; i.e. we argue for the computability of intended addition by reflecting on what we (humans) can *actually* compute, as opposed to what an abstract model of computation can perform. In this sense, the expectation that operations defined on numbers should be computable must mean that we actually have an algorithm to perform the relevant computations. Again, we do not require from the start that intended addition is recursive, but instead reach this claim from the Church-Turing Thesis and the more basic assumption that addition is informally-computable. Of course, the move from an informal to a formal notion of recursivity is needed to make use of Tennenbaum's Theorem; still recursivity of addition is not the basic assumption. On the contrary, informal-computability of addition is. Suppose in fact that the Psychological Church-Turing Thesis holds but the Ontological Church-Turing Thesis does not.[16] This means that there are recursive functions not informally computable. In this case, what sense would then make to claim that arithmetical practice shows that intended addition is recursive (in a formal sense)? For all we may now, even if addition is recursive, it may not be effectively-humanly computable; but if this is the case, how can arithmetical practice and our insistence that *we* can always compute with numbers (regardless if an idealized model of computation can) justify the claim that addition is recursive but not necessarily effectively-humanly computable? The absurdity of such reasoning makes clear that the informal effective-human computability of addition must be the basic assumption

[16]Sometimes a distinction between an *ontological* and *psychological* Church-Turing Thesis can be made:

> **Ontological Church-Turing Thesis:** A function is effectively-computable iff it is recursive.

> **Psychological Church-Turing Thesis:** Any effectively-computable function *that humans can compute* is a recursive function.

of Tennenbaum's argument. We first stipulate, justified from arithmetical practice, that addition is computable *by us*, and then change to the formal notion of recursivity. Hence, we stress that the argument's basic requirement must be:

> **Requirement:** In an intended model the operation of addition is informally (effectively-humanly) computable.

But we just showed the informal computability of $+^{\mathcal{N}^*/\sim}$. Then what this means is that there are non-classical models of arithmetic non-isomorphic to the standard model and yet respecting our intuitions on computability and our ordinary computational practice with natural numbers. Therefore, if computability of addition (and, perhaps, also multiplication and $<$-relation) is all there is when determining the intended model of arithmetic, then it seems that a finite paraconsistent LP-model as the kind proposed above must count as intended.[17] Since $\mathcal{N}^*/\sim$ is intended and is not isomorphic to the standard model, Halbach & Horsten's constraint is insufficient to determine the intended model up to isomorphism.

Since we have covered a lot of material since our introduction of the aims of the moderate realist, it is worth quickly recapturing the main idea: the moderate realist wants to be able to say which model is intended of our practice without heavy assumptions on our epistemic grasp of mathematical objects, in a way that respects her moderate attitude. For this, a promising solution is to determine the intended model up to isomorphism by finding suitable logical constraints, motivated by our ordinary number-theoretic practice, capable of yielding categorical theories or singling out one intended isomorphism type. It is here where we find the argument from Tennenbaum's Theorem. In the last chapter we have covered several objections pertaining to establish the circular character of the argument by focusing on a 'theoretical' and 'practical' notion of recursivity. Now we have just shown that, besides the above counter-arguments, if no requirement besides computability is added, the argument from Tennenbaum's Theorem will not describe the intended models as an isomorphism type. In this way we reintroduce the skolemite challenge for the moderate realist. For the purpose of brevity, we will call this line of reasoning *the LP-argument*.

What The LP-argument Is

The LP-argument is a study of how the assumptions behind the argument from Tennenbaum's Theorem play out when placed in a non-classical setting. It shows that its conclusions should be extended to account for the paraconsistent phenomena. But what exactly this extension is can be a matter of dispute. Without aiming at exhaustiveness, we think there are at least three possible reactions to the LP-argument:

1. **Conclusion$_1$: Bite the bullet:** accept that the argument from Tennenbaum's Theorem is able to single out the *classical / consistent* intended models[18] up to isomorphism, but that there also are many other intended LP-structures non-isomorphic to the standard model. Despite this, the argument is successful

[17] In fact, there are at least countable-infinitely many such models. Too see this take the the standard model and produce, for each $n \in \mathbb{N}$, a finite collapsed model through the relation: $x \sim y$ iff $(x, y < n \wedge x = y) \vee (x, y \geqslant n \wedge x = y \pmod 4)$.

[18] Recall that by classical models we mean models without contradictions; not models whose underlying logic is classical.

in both (a) isolating the intended models as the models with an effectively-computable addition (regardless if they are consistent or inconsistent), and (b) isolating classical intended models as the class of isomorphic copies of the standard model;

2. **Conclusion$_2$: *Reductio*:** reject from the outset intended paraconsistent models on the charge of inconsistency. As a consequence, if the argument from Tennenbaum's Theorem contradicts that initial assumption, this only shows that there must be something very wrong with the initial argument;

3. **Conclusion$_3$: Supplementation:** accept that the argument from Tennenbaum's Theorem is on the right track. Nonetheless, the intended alien LP-constructions are indicative that supplementation of the computability requirement with stronger constraints is needed to fully determine the intended models. This position can be seen as a way to save the argument from *reductio* at the same time that refuses to introduce LP-models in the class of intended ones.

Regarding **Conclusion$_1$** there is not much to be discussed; the position can be seen as a soft reintroduction of skolemite scepticism. Placing finite LP-models in the class of intended models disallows the possibility of isolating the intended model up to isomorphism, making Skolem-Putnam's challenge of determining the intended models enter through the back door. About **Conclusion$_2$** we will merely point out three small remarks. First, this conclusion also reintroduces skolemite scepticism in the sense that refutes the argument that was supposed to solve the sceptical challenge in the first place – this does not mean that there are no other ways to argue against the sceptic, but Tennenbaum's Theorem is not one of those. Second, the initial assumption that inconsistent models are not intended cannot be introduced willy-nilly and must rather be argued for; and it is not obvious how the moderate realist can justify her claim from an epistemically moderate position. Third, and related with the previous point, the conclusion is dependent on the polemic status of paraconsistent logic for which there is already a great amount of literature. Obviously, the advocate of *reductio* must spell out exactly what is so great about consistency. We only note *en passant* that by Gödel's Second Incompleteness Theorem $\textsc{pa} \nvdash \mathrm{Con}(\textsc{pa})$ not being at all clear that arithmetic is consistent from the start; of course, $\textsc{pa}+\mathrm{Con}(\textsc{pa}) \vdash \mathrm{Con}(\textsc{pa})$, but then $\textsc{pa}+\mathrm{Con}(\textsc{pa}) \nvdash \mathrm{Con}(\textsc{pa}+\mathrm{Con}(\textsc{pa}))$ and the problem of proving the consistency of $\textsc{pa}+\mathrm{Con}(\textsc{pa})$ reappears. A further point is that the (dis)advantages of paraconsistent logics have been thoroughly studied as well as the status of consistency and the Law of Non-Contradiction (LNC); since it would be impossible to review here all the literature presented in favour of inconsistent mathematics, we limit ourselves to point the reader for further readings (see Priest(1998) and Priest(2016) as well as the references therein), indicating only that it is not clear why consistency should have such a privileged status. And even if it is agreed that LNC should be a theorem, paraconsistent logics are no less preferable since, for instance, $\models_{LP}$ LNC. And, perhaps, paraconsistency is motivated by mathematical practice; after all:

> [It] is at least plausible that scientists, when working with inconsistent theories, implicitly invoke a paraconsistent logic. Of course, most working scientists (even mathematicians) don't explicitly invoke a particular logic at all. The usual story that they all use classical logic is a rational (and heavily theory-laden) reconstruction of the practice. (Colyvan 2009 : 162)

Even though these comments are not decisive they illustrate the difficulties in advocating a privileged statues to classicality in detriment of intended finite inconsistent models and of inconsistent mathematics or paraconsistency in general.

Conclusion$_3$ is by far the most interesting and the one that will occupy us in the chapters to follow. An important point to bear in mind is that all the possible conclusions address only the LP-argument and are silent about the other counter-arguments we covered in the last chapter.

What The LP-argument Is Not

In the literature on paraconsistent logic there is the unfortunate tendency, perhaps more common among critics, to confuse paraconsistency with *dialetheism*, the view that there are true contradictions. As we see it, this is an instance of when a mathematical result is inflated with non-neutral philosophical import. Nonetheless, as Priest *et al.*(2018, § 1.1) make clear 'The view that a consequence relation should be paraconsistent does not entail the view that there are true contradictions. Paraconsistency is a property of a consequence relation whereas dialetheism is a view about truth.'[19] Hence, the fact that it is possible to model logical consequence without *ex contradictione quodlibet* does not, by itself, imply that there are true contradictions. As a consequence, the LP-argument is *not* an argument for dialetheism; in fact, our goal here is more didactic and overall neutral – we are simply interested in pursuing the argument from Tennenbaum's Theorem to its last consequences. Our detachment from any philosophically committing view is assumed in the different conclusions that we find to justifiably follow from the LP-argument; we stress that, for example, by **Conclusion**$_2$, we allow the possibility of there being extra reasons that go against intended inconsistent models of arithmetic immediately entailing *prima facie* the absence of any true arithmetical contradictions. It should then be clear that ours is not a dialetheistic view – by itself, the argument should not simply be taken as an endorsement of true contradictions in arithmetic. A similar remark is that the argument is neither a defence of paraconsistent logic in general; only that these yield interesting arithmetical structures philosophically weighty for the views we have been addressing.

Having said this it is also important to remark that the LP-argument easily lends itself to a defence of inconsistent mathematics and paraconsistent logic. This is evident in our **Conclusion**$_1$: the project of inconsistent mathematics is boosted by there being intended inconsistent LP-models of arithmetic and, consequently, if these models are to be coherent (i.e. without triviality) a paraconsistent logic must be preferred. This is a natural argument, but one that we will not make. As we said, we will only discuss **Conclusion**$_3$. It will be our goal to show that it is a rather difficult task that of specifying new constraints capable of both (a) being justified from *within* mathematical practice and (b) dismissing inconsistent LP-models. In this sense, we will show how the other strategies (or another position different than these remaining two conclusions) should be preferable, highlighting the complex ramifications of the argument from Tennenbaum's Theorem.

However, these conclusions need not follow if there is no argument from Tennenbaum's Theorem. And the only interest in the argument was due to the sceptical challenge that we argued to be self-refuting. If the solution or dissolution of the sceptical

[19]It should be noted that dialetheism is rather liberal regarding what 'truth' amounts too: 'In talking of true contradictions, no particular notion of truth is presupposed. Interpreters of the term 'dialetheia' may interpret the notion of truth concerned in their own preferred fashion. Perhaps surprisingly, debates over the nature of truth make relatively little difference to debates about dialetheism.' (Priest, 2007 : 131) For a comprehensive introduction to the differences and intersections, both technical and historical, between paraconsistency and dialetheism, see Priest, 2007.

case are right all along, there is no need to introduce Halbach & Horsten's argument responsible for generating intended inconsistent models.

4.4 Saving Skolemite Scepticism from the (dis)solution

We saw two possible ways to reintroduce skolemite scepticism: **Conclusion**$_1$ and **Conclusion**$_2$. However, in the previous chapter we mentioned two ways to immediately block the skolemite sceptic independently of the argument from Tennenbaum's Theorem: (a) mathematical knowledge makes evident the possession of a primitive notion of *finiteness* irreducible to other mathematical concepts and capable of determining the intended model up to isomorphism, or (b) the possibility of formulating the sceptical challenge presupposes resources, again a grasp of *finiteness*, which leads to its self-refutation. The idea being that if one has a determinate notion of finiteness, then by, say, invoking the argument from the Initial Segment Theorem the intended model is singled out up to isomorphism. It should be stressed that the Initial Segment Theorem is but one of many ways in which finiteness may be employed to solve the sceptical challenge, and we will discuss other options later. But for now let us concede that we have a determinate notion of *finiteness*. Does this immediately solve (or dissolve) the sceptical problem? Well, it need not to. Just check that every element of $\mathcal{N}^*/\sim$ is finitely far from 0 (or [0]) – in fact, any $[x] \in \mathbb{N}^*/\sim$ is such that $[x] = [0] + [y]$, for $0 \leq y \leq 6$.

Again we just need to repeat the early dialectic. First, we have the requirement that intended models of arithmetic are models in which every element is finitely far from zero. We concede that the notion of *finiteness* in the argument is somehow fixed and does not presuppose a grasp of that which tries to determine. Second, we know that every element of $\mathcal{N}^*/\sim$ is finitely far from [0]. From these two facts we conclude that $\mathcal{N}^*/\sim$ is intended of arithmetic. Now, since $\mathcal{N}^*/\sim$ is an intended model not isomorphic to the standard model we reintroduce the doubt regarding which types of structures are intended of arithmetic and in this sense we recover skolemite scepticism. And hence the requirement is insufficient to single out one intended isomorphism type. Again, we have similar conclusions forthcoming:

4. **Conclusion**$_4$**: Bite the bullet:** accept that the grasp of a determinate notion of finiteness is not able to single out the intended models up to isomorphism. If anything this view just reinforces skolemite scepticism;

5. **Conclusion**$_5$**: *Reductio*:** reject intended paraconsistent models on the charge of inconsistency. As a consequence, if the grasp of a determinate notion of finiteness contradicts our initial assumption, this only shows that there must be something very wrong with the initial argument. Of course, again, the initial assumption that inconsistent models are not intended cannot be introduced willy-nilly and must rather be argued for;

6. **Conclusion**$_6$**: Supplementation:** accept that the grasp of a determinate notion of finiteness is on the right track. Nonetheless, the intended alien LP-constructions are indicative that supplementation of the finiteness requirement with stronger constraints is needed to fully determine the intended models.

And similar comments apply as before.

4.5 Summary

We want to use this section to repeat some main points so to better clarify what we covered in this chapter and how it relates with the point from where we started our discussion. To repeat, Putnam's model-theoretic arguments show, or try to show, that a moderate realist position is not tenable. There are many models of arithmetic and the moderate realist is in no position to tell us which of them are intended, rendering the view highly unattractive. A possible way out, so to save the moderate realist, is to make use of the argument from Tennenbaum's Theorem. So far this is nothing new. Now, what we did in this chapter was to present the LP-argument. This stated that there are finite inconsistent LP-models whose intendedness is a by-product of Tennenbaum's argument; that is, if you buy the assumptions of the argument from Tennenbaum's Theorem, then these alien models are really intended. Simply put, by the argument from Tennenbaum's Theorem computable models are intended, and by the LP-argument there is a case to be made for computable finite inconsistent LP-models. At first sight the argument serves to reintroduce the sceptical problem: there are several non-isomorphic models (consistent and inconsistent) that could conceivably be counted as intended, so the moderate realist better give us an answer specifying which of these is really intended.

What to make of the above? The moderate realist can expectedly react in one of three different ways. One possible way out is for the moderate realist to add more constraints that rule out LP-models and preserve the standard intended model. Later on we will try to argue that this option cannot be suitably pursued without at the same time breaking with her epistemic moderate assumptions. Hence, let us for now suppose that this option is a no-go. From this latter fact we expect the moderate realist to fall back on one of the other two remaining options. She can say that LP-models are obviously non-intended (though we stress again that from our part we cannot imagine how she could justify this pretension without again breaking with her moderation). As a consequence, since Tennenbaum's Theorem leads us to the intendedness of the inconsistent LP-models, by *reductio*, the argument from Tennenbaum's Theorem is wrong. But sadly, this eliminates the moderate realist line of defence against Putnam's arguments, reintroducing Skolem-Putnam's challenge. Another option is simply to bite the bullet: accept that indeed the argument from Tennenbaum's Theorem makes us count LP-models as intended. But then this also means that the moderate realist cannot isolate intended models up to isomorphism, and so she hasn't addressed Skolem-Putnam's challenge to a suitable degree so as to make her position attractive. Bottom line, the argument from Tennenbaum's Theorem cannot help the moderate realist solve Skolem-Putnam's challenge.

More, the above is particularly problematic for the moderate realist since, by parity of reasoning, neither the solution nor dissolution of the sceptic problem fully work, as also shown by the finite LP-models. Before we address the Supplementation strategy and see why and how it is not a viable option, we would like to (quickly) address the peculiar nature of *inconsistent* effective procedures or functions and see exactly what sense can be made of them.

Chapter 5

Paraconsistent Computations

5.1 Introduction

This chapter aims to cover some aspects of inconsistent computations. By this we mean a procedure that verifies φ *and* verifies $\neg\varphi$, or a procedure that outputs a single solution x *and* a single solution y, with $x \neq y$. It will be divided in two goals: first, we aim to present some motivations for inconsistent procedures and how a function behaves in inconsistent arithmetic; second, we want to quickly think about the relation between finiteness and decidability in LP-models.

We should note the following. A quick survey of the literature on (the philosophy of) paraconsistent computations shows that the subject is still in its infancy. Though there is a growing case in favour of effective procedures which cannot be recognisable as such by non-paraconsistent logics, important comparisons between classical and non-classical computational terms are yet lacking. The purpose of this chapter then is not to motivate a thorough analysis of the computational aspects of inconsistent arithmetic substantiated by established results in the literature, but rather to make a collection of intuitive remarks that hopefully help to demystify the problematic notions.

5.2 Logic-Relative Computations

5.2.1 Motivation

In the elegant Copeland & Sylvan(2000) it is argued that the first recursion theorists made a mistake after the limitative results 'rising from the ashes of the first Hilbert program [...] [T]hat of assuming that they had built their unsolvability and limitation results *on an entirely absolute foundation*.' (Copeland & Sylvan, 2000 : 189-190, our italic) They go on to explain that such a pretension is now orthodox in the literature, found in the writings of Gödel, Church and Post. Here we give only the example of Gödel:

> [...] the great importance of the concept of general recursiveness (or Turing's computability) [...] is largely due to the fact that with this concept one has for the first time succeeded in giving *an absolute definition of an interesting epistemological notion, i.e., one not depending on the formalism chosen.* (Gödel, *Remarks before the Princeton bicentennial conference*

of problems in mathematics quoted in Copeland & Sylvan, 2000 : 191, our italic)

Part of this absoluteness consists in the defence of a unique model of computation (to be safe we should add, 'up to convergence theorems') whose conceptual and logical foundations are situated in a *classical* logic. The insistence on classical mathematics is strikingly evident in the consistency-requirement of a Turing-machine. For instance, call an *i-quadruple* (instruction-quadruple), an ordered set $\langle q_1, S, A, q_2 \rangle$ where:

- q_1 is a numeral other than '0';
- S is one of the symbols '0', '1' or 'B' (for Blank);
- A is one of the symbols '0', '1', 'B', 'R' (for Right) or 'L' (for Left);
- q_2 is a numeral, possibly '0'.

Now Smith goes on to define a Turing programme as follows:

> Our first shot at characterising a Turing program therefore comes to this: it is a set of i-quadruples. But we plainly don't want *inconsistent* sets which contain i-quadruples with the same label issue inconsistent instructions. So let's say more formally:
>
> > A set Π of i-quadruples is consistent if there's no pair of i-quadruples $\langle q_1, S, A, q_2 \rangle$, $\langle q_1, S, A', q_2' \rangle$ in Π such that $A \neq A$ or $q_2 \neq q_2'$.
>
> Which leads to the following sharpened official definition:
>
> > A *Turing program* is a finite consistent set of i-quadruples. (Smith, 2007 : 290)

It is then clear that a default to a classical logic immediately ends with the possibility of non-classical, say, inconsistent computational procedures.[1] However, the purpose of Copeland & Sylvan's paper is, as their title indicates, to motivate the opposite case: as they claim *computability is logic-relative*. And our purpose here is using (some of) their insights to make the case for paraconsistent computable functions; i.e. effective procedures not recognisable as such by a non-paraconsistent logic. Before this we make some general remarks to illustrate that logical-relative computable functions are very easy to come by. First, there are logical systems weaker than classical logic where *less* functions are computable: for instance, classical systems weaker than Q where not all the classical primitively recursive functions are definable. Also, for f defined on the set of natural numbers, the following function is classically computable but not constructively computable[2]:

$$f(n) = \begin{cases} 1 & \textsc{ch} \text{ is true} \\ 0 & \textsc{ch} \text{ is false} \end{cases}$$

The example comes from Bridges(1994). He explains that since 'most mathematicians are formalists on weekdays and Platonists on Sundays' (Bridges, 1004 : viii)[3],

[1]Thence, our warning in the last chapter about calling $+^{\mathcal{N}^*/\sim}$ *recursive*.

[2]By constructively computable we mean computable in constructive logics/systems.

[3]Funny enough, sometimes the saying appears inverted as 'most mathematicians are Platonists on weekdays and formalists on Sundays'.

(at least) on Sundays most mathematicians would accept that f is a well-defined function. That is, mathematicians *qua* Platonists would tend to accept that CH has a determinate truth-value (either true or false) regardless whether we know it or not.[4] Therefore, on a Platonist reading, the mapping will have an image for every argument. Now, classically, there will be an algorithm that computes the function: this algorithm is either one that when applied to every n outputs 1 or one that when applied to every n outputs 0. However, given the independence of CH from ZFC, we are not able to decide which of these two possible algorithms actually computes f. Bridges gives the following conclusion: 'With classical logic there seems to be no way to distinguish between functions that are computed by programs which we can pin down and those that are computable but for which there is no hope of our telling which of a range of programs actually performs the desired computation.' (Bridges, 1994 : ix). With a constructive (intuitionistic) logic behind the function is not computable simply because it is not well-defined: since we cannot decide the truth or falsehood of CH, constructively we would say that so far the question of the truth-value of CH is ill-posed; constructively it is not the case that CH is true nor the case that it is false, so that the mapping f is not properly defined.

Similar considerations happen when computability is defined with an underlying paraconsistent logic, where *more* rather than less functions are computable. One of the motivations for paraconsistent computations arises in the presence of diagonal functions leading to inconsistent results; similar to other cases where a paraconsistent logic is motivated by paradoxical reasoning regarding Truth or sets. Here we present the *paradox of algorithmic functions*. Consider functions that take natural numbers as inputs and map them to natural numbers. We say that a function is an *algorithmic function* if there is some algorithm that effectively computes it. Now, an algorithmic function can be formalized by a finite string of symbols containing finitely-many function symbols, variables, brackets, ... This means that any (finite) algorithmic function can be represented with a finite string of such symbols. Hence, it is decidable if a finite string of symbols does constitute an algorithmic function: an agent lists the algorithmic functions by first examining all strings of length 1, then 2, and so on ... Even though the list is infinite every member is reached after finitely many steps. Consider then the list/set S of algorithmic functions with just one argument. Define P_x as the $(x+1)^{th}$ element of S and p_x the corresponding function. Now, define the diagonal d by:

$$d(x) = p_x(x) + 1$$

$d(x)$ is effectively computable: the agent uses the list S to find P_x, computes $p_x(x)$ and adds 1. Hence, $d(x)$ is an algorithmic function and is a member of S. Therefore there is a z just that d is the ${P_z}^{th}$ element on the list such that $p_z = d$. Now consider:

$$p_z(z) = d(z) = p_z(z) + 1$$

By standard arithmetic we have $n = n + 1$, for some n. The also standard solution is incompleteness: consistency is secured by assuming that some functions are incomplete, i.e. undefined on some values (such as z). But there is a risk of *ad hocness* for this solution: '[A]s with dialetheic reaction to the other logical paradoxes, it looks like there is nothing to explain why some functions cannot take all inputs from the very domain they are meant to draw on – nothing except the inconsistency that results.'

[4]Granted that this is a very elementary take on the relation between Platonism and independent statements, but for our purposes it will suffice.

(Weber, 2016 : 208). Worse, retreating to partial functions does not end the problem; a consequence of incompleteness is that no algorithmic procedure can select those sets of instructions that yield total functions. This being the case, the distinction between total and partial functions, central in recursion theory, cannot be effectively drawn at the risk of contradiction. One possible way to introduce such a procedure to effectively select total functions is by allowing computations that yield inconsistent outputs by adopting an underlying paraconsistent logic. Again, following Copeland & Sylvan here:

> Effectively the claim is that diagonal functions cannot be computed (are not total computable functions), because the contrary assumption leads to contradiction. But now that we have more experience in working with inconsistent totalities, we may well be inclined to push on, rather than retreating (for example to partial functions) [...] For in fact we may know perfectly well how to compute certain diagonal functions. For example, if the instruction is to increase the diagonal by 1, well then add 1! Pushing on, a diagonal element is encountered such that $d = d+1 : d = d \ \& \ d \neq d$. This gives pause neither to the contradiction-tolerating theorist nor to a D-machine of type 1 [i.e. a machine programmed with a paraconsistent logic] that is computing the function. (Copeland & Sylvan, 2000 : 197-198)

5.2.2 LP-Addition Revisited

Even though there may be some motivations for inconsistent computations, one of the apparent main difficulties in making sense of inconsistent (models of) arithmetic is specifying what is for a procedure such as addition to be inconsistent; that is, to have elements x, y, z, w such that $+^{\mathcal{M}/\sim}(x, y) = z$ and $+^{\mathcal{M}/\sim}(x, y) = w$ with $z \neq^{\mathcal{M}/\sim} w$. Such a reluctance is often presented under the form of an 'incredulous stare' when faced with inconsistent procedures; for instance, take Shapiro(2002)'s remark concerning provable and non-provable statements in an inconsistent theory:

> On all accounts [...] we have that g is a code of a PA*-derivation of G^*. This can be verified with a painstaking, but completely effective check. How can the dialetheist go on to maintain that, in addition, g is *not* the code of a PA*-derivation of G^*? What does it mean to say this? [...] I must admit that I cannot make anything of this supposedly possibility. (Shapiro, 2002 : 828)

Shapiro's claim is made in the context of a recursively axiomatizable theory PA* containing a truth-predicate and capable of proving its own Gödel setence G^*. Since the details of the theory are somewhat under-specified and since the point of Shapiro's critique applies equally well to axiomatizable inconsistent arithmetics such as the ones we have been addressing, we follow Priest(2006 : 239)'s suggestion and concern ourselves with the LP-models we have sketched. Now, take a *finite* inconsistent theory of arithmetic (PA) closed under LP-consequence; call it Γ. Let us assume, for the purpose of argument, that finiteness implies decidability.[5] Then, Γ is trivially recursively axiomatizable. We know that since it is recursively axiomatizable, every set of formulas is recursive and, therefore, strongly representable in $Th(\mathcal{N})$; for $\varphi \in \Gamma$ and $Prov(y, x)$ the proof relation of Γ:

[5]Even though this implication is not obvious. See the next section.

- $\varphi \in \Gamma \Rightarrow \exists y Prov(y, \ulcorner\varphi\urcorner) \in Th(\mathcal{N})$
- $\varphi \notin \Gamma \Rightarrow \neg\exists y Prov(y, \ulcorner\varphi\urcorner) \in Th(\mathcal{N})$

By the Collapsing Lemma, it follows that every decidable set is representable by the same formula in every collapsed model. This means that since $Th(\mathcal{N}) \subseteq \Gamma$, we have

- $\varphi \in \Gamma \Rightarrow \exists y Prov(y, \ulcorner\varphi\urcorner) \in \Gamma$
- $\varphi \notin \Gamma \Rightarrow \neg\exists y Prov(y, \ulcorner\varphi\urcorner) \in \Gamma$

By standard fixed-point techniques, we build:

$$G \leftrightarrow \forall y \neg Proof(y, \ulcorner G\urcorner)$$

It is now straightforward to check $G \wedge \neg G \in \Gamma$, so that G and $\neg G$ are both provable in Γ. First, either $G \in \Gamma$ or $G \notin \Gamma$. If $G \in \Gamma$, then $\exists y Proof(y, \ulcorner G\urcorner) \in \Gamma$ (by the above clause), which means $\neg G \in \Gamma$. If $G \notin \Gamma$ we obtain, by completeness, $\neg G \in \Gamma$ and (by the above clause) $\forall y \neg Proof(y, \ulcorner G\urcorner) \in \Gamma$ and $G \in \Gamma$. In both cases $G \wedge \neg G \in \Gamma$. Hence:

$$(*) \;\; \exists y Proof(y, \ulcorner G\urcorner) \wedge \neg\exists y Proof(y, \ulcorner G\urcorner)$$

Now, consider the following biconditionals:

1. $Proof^{+}$: y is the code of a proof of a formula with code x iff $Proof(y, x) \in \Gamma$
2. $Proof^{-}$: y is not the code of a proof of a formula with code x iff $Proof(y, x) \notin \Gamma$

If the above holds, then given $(*)$ we are lead to accept that there is a number g that both is and is not a code of a proof and, equivalently, that something is and is not provable. The challenge is now to make sense of this. It is important to note that this is a different problem than what the paraconsistent (or dialetheist) has normally accustomed us to – it is standard to defend paraconsistency on the grounds that there are true contradictions; but these contradictions normally concern cases like the Liar where self-reference or some semantic notion akin to Truth is involved. What Shapiro seems to find particularly problematic is that $Proof(y, x)$ is a statement in the pure language of arithmetic (without semantic notions) so that contradictions even appear at this very basic level:

> Even if we concede, for the sake of argument, that contradictions are acceptable when dealing with semantic notions like truth, we see that PA* [and, in our case, the theory of finite LP-models of PA] entails contradictions concerning the natural numbers alone [...] (Shapiro, 2002 : 823)

One possible solution would be to reject $(*)$ on the grounds that statements in the language of pure arithmetic are consistent. If arithmetic truth is to be consistent, we cannot accept $Proof(g, \ulcorner G\urcorner)$ and $\neg Proof(g, \ulcorner G\urcorner)$ as both true. One way out is to reject the soundness of Γ; in this way, we may accept that, if g is indeed a code of a derivation of G, then we have $Proof(g, \ulcorner G\urcorner)$ but, by consistency, we may deny $\neg Proof(g, \ulcorner G\urcorner)$. But this goes against the spirit and the letter of paraconsistent arithmetic; if we want to take seriously the idea that paraconsistent models are intended models, then we must inevitably recognize inconsistencies at the basic level of pure arithmetic; and that even primitively recursive relations are inconsistent. Denying that primitively recursive functions are not inconsistent just because otherwise they would be inconsistent does not seem that becoming.

Another option, that maintains the soundness, is to reject the equivalences between 'being the code of a derivation' and the extension of the predicate $Proof(y, x)$, and, similarly, 'not being the code of a derivation' and the extension of the predicate $\neg Proof(y, x)$ expressed in the clauses (1) and (2). The idea being that $Proof(g, \ulcorner G \urcorner)$ and $\neg Proof(g, \ulcorner G \urcorner)$ are true of the natural numbers, but deny that, say, g is not the code of a derivation of G. However this would break the extensional equivalence between non-derivations and $\neg Proof(g, \ulcorner G \urcorner)$ and the accustomed isomorphism between natural numbers and strings of characters.

The final option Shapiro surveys is to accept $(*)$ and the consequential inconsistency of the theory at this basic level of pure arithmetic. But if this is the case, then Shapiro wonders, as we saw, what can it mean to verify that g is a code of the proof of G and it is not a code of the proof of G. To explain a bit more: the relations $Proof(g, \ulcorner G \urcorner)$ and $\neg Proof(g, \ulcorner G \urcorner)$ yield similar verification procedures. In the first case, we unpack g to see what sequence it codes and write down such sequence; then we only need to verify if each instance of the sequence is either an axiom or a derivation obtained by previous steps in the sequence. If all goes well, and if the last line is G, we may conclude that g is the code of G. Otherwise, we may conclude that g is not the code of G. But if both cases obtain there must be a line in the derivation where a contradiction obtains; that line both is and is not an axiom, or is and is not a legitimate derivation. And Shapiro's (rhetorical) qualms concern the absurdity of such an hypothesis:

> She claims that we do indeed successfully and accurately verify that g is the code of a derivation of G^* and we also successfully and accurately verify that g is not the code of a derivation of G^*, presumably by using the same procedure at the same time. It is not a matter of vagueness. Some *one* step in the procedure must yield contradictory results. [...] in the present case we seem to have no idea what it would be like to discover a contradiction concerning derivation – there is no analogue of the Escher drawings. (Shapiro, 2002 : 829)

A general point made by Weber worth recalling is that incredulous stares do not work with those of us a bit more credulous. Regarding truth, we say that all contradictions are false, but some like the Liar are also true. Regarding vagueness, we say all possible thresholds of the predicate 'heap' are arbitrary or nonsensical; even though one of those points must be correct. In set theory, the Mirimanoff collection of all well-ordered sets is and is not well-ordered. 'So qua dialetheism, there is nothing immediately special to say about Shapiro's objection.' (Weber, 2016 : 212). What might need some clarification is how a proof can in some sense be contradictory; how can we verify a sentence that expresses a recursive relation and at the same time its negation? And the answer is in fact quite straightforward. A proof of G is a collection of statements $\langle A_1, ..., A_n, G \rangle$ where $A_1, ..., A_n$ are axioms or legitimate derivations. To say that G is provable and not provable is to say that it follows and does not follow from $A_1, ..., A_n$. That there is a step in the proof which is a step from truth to falsity that is also a step from truth to truth. Sure, but Shapiro may still ask how such a proof *looks* like.

Now, here, we are unintentionally starting to get ourselves into the shaky waters of *conceivability* and *inconsistency.* We will have just some general remarks to point. First we should note that, in the quote above, Shapiro's reference to the inconceivability of a provable/unprovable statement is made in relation with an 'analogue of

Escher drawings'. It seems as if Shapiro equates conceivability with mental pictorial imagery similar to actual pictorial perception. However, not all kinds of conceivability need to be like this; mainstream cognitive psychology distinguishes two kinds of mental representations: pictorial representation akin to visual perception, and linguistic representation that is disconnected from the sensory modalities normally present in pictorial representation (i.e. mereological and quasi-spatial features).[6] Whereas pictorial representations are more common when conceiving situations that presuppose the imagining of spatial and temporal features, linguistic representations appear in abstract scenarios that do not involve those perceptual components.[7]

Given that provable sentences involve non-perceptual abstract concepts, it is reasonable to assume that the type of conceivability involved must be linguistic. At this moment let us assume the 'Parity Assumption: whatever content is representable by a natural language sentence, is also representable by some linguistic mental representation.' (Berto & Schoonen, 2018 : 2703) The motivation for the Assumption is mainly due to the fact that every content represented in a natural language should be mentally representable for *ex hypothesi* it is the latter, cognition, that grounds the learnability of natural language. If this is correct then the claim that we cannot conceive of a proof that is also not a proof essentially implies that the ordinary english sentence 'φ is provable and unprovable' or 'in a proof the step α both leads from truth to truth *and* from truth to falsity' is essentially meaningless.[8] But such a proposal seems plainly absurd. We offer two reasons for our claim: first, if P is meaningful and $\neg P$ is meaningful, then $P \wedge \neg P$ must be meaningful too – surely '$P \wedge \neg P$' is a far cry from Chomsky's 'colourless green ideas sleep furiously'. Second, if the above sentences were indeed meaningless, we couldn't possibly disagree with them: if we cannot in principle understand the content they express we cannot judge that content to be false. But Shapiro's seems, at least, to disagree with them.

Of course, to an extent we empathise with Shapiro's view – it indeed seems easier to concede the contradictory character of the Liar, when compared to the notion of *proof*. We ignore the reasons behind this folk intuition. However, to admit that there is no way to conceive or make sense of a provable and unprovable statement or of a proof that is not a proof seems to be just as ill-founded as the claim that talk of the Russell set is meaningless. And the implicit thought here is that we can indeed make some sense of the Russell set – at least to the extent that we find meaningful principles that restrict the construction of Russell-like sets.

We will not discuss this issue further. The topic is far to wide to attempt any level of detail in a few words. Moving on, to better clarify the notion of inconsistent proof in general, recall that in LP verifying α does not, by itself, determine $\neg\alpha$: α and $\neg\alpha$ are verified by different procedures. For example, $R(a, b)$ is true iff $\langle I(a), I(b)\rangle \in I^+(R)$; now, this not excludes the possibility of $\neg R(a, b)$ being true; to determine this we also need to check $\langle I(a), I(b)\rangle \in I^-(R)$. Recalling the last chapter, $[3] \neq [7]$ is true in $\mathcal{N}^*/\sim$; but there is the further question if $[3] = [7]$ is also true. A similar case applies to g: what does it mean to say that g is the code of a proof of G? It means that, let us suppose, $g = 200$. What does it mean to say that g is *not* the code of a proof of

[6] The data here is taken from Berto & Schoonen(2018). References to the literature can be found therein.

[7] If there is a substantial distinction or if all representations can be reduced to the linguistic type is currently a matter of dispute.

[8] Berto & Schoonen(2018)'s argument is more complex than the one we propose, but here we don't need to go into the details of their presentation.

G? It means that $g \neq 200$. And this may happen in LP, when $200 = 200 \wedge 200 \neq 200$ (which in fact happens about [200] in $\mathcal{N}^*/\sim$).

Similar comments apply to the provability of G. What does it mean to say that G is provable? Well, it means that $\exists y(y$ is the code of a proof of $G)$ which means that $\exists y(y = 200 \wedge y$ is the code of a proof of $G)$. What does it mean to say that G is *not* provable? Well, it means that $\forall y \neg(y$ is the code of a proof of $G)$ which means that '(1 is not the code of a proof of $G) \wedge ... \wedge$ (200 is not the code of a proof of $G) \wedge ...$'. In particular, this implies that $200 \neq 200$ which, again, can be the case.

> [...] to say that γ is not provable is to say that every number is distinct from a code of the proof of γ. This does not rule out there being a proof of γ. (In general, the truth of $\neg\alpha$ in a paraconsistent setting does not rule out the truth of α). In particular, it will hold if the proof is distinct from itself. And how can a proof be distinct from itself? In the same way that a number can. (Priest, 2006 : 242)

This gives us an easy way to understand the inconsistent addition function. In the previous chapter we gave an effective procedure to determine $x +^{\mathcal{N}^*/\sim} y$.[9] Given the way addition is defined, we may have cases where the function gives inconsistent outputs; for instance, we have:

1. $\mathcal{N}^*/\sim \models [3] + [1] = [4]$
2. $\mathcal{N}^*/\sim \models [3] + [1] = [8]$
3. $\mathcal{N}^*/\sim \models [4] \neq [8]$

The problem is now what to make of this case where all formulas are satisfied; or, more generally, when

$$x + y = z \wedge x + y = w \wedge z \neq w$$

The inconsistency of the underlying elements of the domain helps to explain the inconsistency of the functions defined on them. (1) just means that there is a procedure computed on [3] and [1] that yields [4]. Of course, paraconsistently, this does not rule out there being a procedure that yields a different value for the computation; and this is precisely stated by (2). Finally, (3) tells the trivial fact that there are two numbers that are different: $[4] \neq [8]$. All this conditions may happen in a model that satisfies $[4] = [8] \wedge [4] \neq [8]$ like the case of $\mathcal{N}^*/\sim$. Inconsistent procedures add no additional strangeness, besides the already strange character of the numbers (or elements) in the domain. It is in this sense, that 'the facts about computability and provability are simply read off from the arithmetic'. (Priest, 2006 : 243).

5.3 Computability on the cheap?

In the previous chapter we have argued for the effective computability of the addition function in (some) heap collapsed models. We argued for this by noting that $+^{\mathcal{M}/\sim}$ is defined in terms of $+^{\mathcal{M}}$; based on this fact, we may specify an easy algorithm to compute the collapsed addition in terms of the addition function defined in the original models (provided the original addition function is indeed computable). However, is this really surprising? Couldn't we have reached the same conclusion already through

[9] The procedure obviously generalizes to other models.

a much easier route. It is tempting to think so; after all *prima facie* it is easily seen that a theory $Th(\mathcal{M}/\sim)$ (closed under LP-consequence) is decidable: the values of atomic formulas may be computed using LP-matrices, and given that the model is finite, the truth-value of quantified formulas are computed using the following equivalences (where $\overline{n}$ is the numeral for n, and m is the greatest number in the model):

- $v(\exists x\varphi) = v(\varphi(x/\overline{0}) \vee ... \vee \varphi(x/\overline{m}))$
- $v(\forall x\varphi) = v(\varphi(x/\overline{0}) \wedge ... \wedge \varphi(x/\overline{m}))$

Hence, *a fortiori*, collapsed addition is computable. But things are not so simple. Normal intuitions break down in the inconsistent case, urging great cautiousness with swift conclusions.

Denyer(1995 : § 2) is rather wary of the strength of the argument. The decision procedure for a finite collapsed model works essentially by quantifier elimination: LP-matrices will give an effective procedure to calculate the value of any formula prefixed by any arbitrary finite number of quantifiers that can be eliminated through the above equivalences. But now let us suppose that we apply the procedure to a statement preceded by m many quantifiers. So, for definiteness, consider a statement: $\forall x_1, ..., \forall x_m\varphi$ (with $x_1, ..., x_m$ free in φ). Suppose further that m is an inconsistent number in the model; that is, $m = m + p$ (with $p \neq 0$). Say $p = 1$ so that $m = m + 1$. By this latter equality, the original statement preceded by m-many quantifiers is, after all, preceded my $m + 1$-many quantifiers so that the original statement is equal to $\forall x_1, ..., \forall x_{m+1}\varphi$. Now, we may apply the decision procedure to the latter statement and produce a new statement (an arbitrarily finitely long conjunction) preceded my m-many quantifiers. But then it is clear that the decision procedure didn't get us any far for there are still m (and $m + 1$) quantifiers to eliminate! A similar worry occurs when the statement is preceded even by only one quantifier. We reduce the statement to, suppose, a finite disjunction with $l+1$ disjuncts: $\varphi(x/\overline{0}) \vee ... \vee \varphi(x/\overline{l})$. Now, since l is the greatest number of the finite model, it will be inconsistent and therefore, say, $l = l + 1$; so that $\varphi(x/\overline{0}) \vee ... \vee \varphi(x/\overline{l+1})$. But then even when we decide the first of these disjuncts (i.e. $\varphi(x/\overline{0})$) we still have $l+1$ disjuncts to decide. Again, we haven't really advanced in our decision procedure. Such considerations lead Denyer to claim:

> Algorithms that require us to take a magic number of steps are, in short, no more use as decision procedures than algorithms that require us to take infinitely many. [...] But that simply means that the decidability [...] attache[d] to [...] paraconsistent arithmetic [i.e. LP-models] is not quite the decidability which, before the limitative results came along, people had hoped would attach to classical arithmetic. (Denyer, 1995 : 570)

The following arguments are not the final word on the matter. And this because, properly speaking, they are not arguments but mainly argument *sketches.* The problem is that Denyer's arguments merely gesture at the negation of the decidability of paraconsistent models. But for example, if the underlying logic is paraconsistent, it is not clear that Denyer's arguments follow: the idea that subtracting 1 from $m + 1$ equals m only holds for the classical material conditional; LP-conditionals appear only in a non-detachable form. And if we are dealing with non-classical paraconsistent models, we may doubt if our reasoning regarding meta-theoretical properties such as the computability of LP-consequence should be guided by classical logic. Either way, these comments should give us pause when entailing decidability from finiteness.

5.4 Summary

As we pointed out before there can be an initial strangeness about an inconsistent effective procedure. This small chapter aimed to show that there is nothing strange about it; or better, that an inconsistent computation is as strange as its underlying inconsistent arithmetic – if the underlying arithmetic admits inconsistent elements, then it is a natural consequence that the computational operations are inconsistent too. In fact, what is problematic is to try to keep a classical computational theory or meta-theory for provability in inconsistent arithmetical settings. Another independent but related problem that we addressed concerned decidability in finite paraconsistent models. If a model is finite, it would then seem that consequence-relation is trivially decidable. However, things are not so simple in the paraconsistent case, and there must be a wider story to be told about such an entailment.

Chapter 6

On Supplementation and Categoricity

6.1 Introduction

This chapter concerns Conclusion$_3$, what we dubbed 'Supplementation', as a possible answer to the LP-argument. To recall, the conclusion was:

> **Conclusion$_3$: Supplementation:** accept that the argument from Tennenbaum's Theorem is on the right track. Nonetheless, the intended alien LP-constructions are indicative that supplementation of the computability requirement with stronger constraints is needed to fully determine the intended models.

Our chapter will be divided in an inductive and deductive part. Regarding the inductive part, we will analyse four ways to supplement the computability requirement and see that they are not sufficient to rule out inconsistent LP-models from the class of intended models. Regarding the deductive part we will offer a sketch of a reasoning based mostly on the work of Dummett and McGee, to the extent that no possible criteria can fully rule out inconsistent (intended) numbers.

6.2 The Inductive Argument

The purpose of the inductive argument is to cover four possible strategies on how supplementation might be pursued and show why they fail with respect to their goal. Without aiming at exhaustiveness we have chosen these four options mainly due to their overall intuitiveness or due to the fact that they are commonly found in the literature on categoricity.

6.2.1 Counting and Infinity

The first option starts with the trivial fact that finite LP-models are finite. And even though we can make a sensible case for they being infinite too, they are finite. But ordinary classical models are countably infinite. In this sense a possible way to rule out the alien LP-constructions would be by adding the further requirement that the intended model of PA is (only) countably infinite. To show why this strategy is unsatisfactory we offer two arguments:

1. The intended natural numbers don't need to be infinite. Despite being a *sui generis* claim (to say the least!), we think we can reasonably argue for this. Throughout the text we have argued that, from a moderate realist perspective, the constraints imposed on the class of intended models should be determined by *actual* number theoretic practice. In this sense, let us imagine two different counting practices: person A starts to count from 0, adds 1, then 2, 3, and so on – A will not have any greatest number where the counting stops; in fact, if we stepped into a time machine and went to the future an arbitrarily large numbers of years from now, in principle, we would discover that A hasn't stop counting. Now, B will start very much like A: B starts with 0 then, 1, 2, 3, ... – however, for B there will be a certain greatest number n where she will stop the counting. For sake of argument 'Let us, henceforth, fix n as some incredibly large number, say, a number larger than the number of combinations of fundamental particles in the cosmos, larger than any number that could be sensible specified in a lifetime, so large that it has no physical meaning or psychological reality.' (Priest 1994a : 338) As a consequence, we are not to expect that even with 100 life-times B can reach this magic number n. We have now the following question: is it the case that we *actually* count like A; or is it the case that we *actually* count more like B? *Prima facie*, we would say that we count like A. The important thing to note is that in *actual* situations A and B seem to be counting according with the same rule. Recall that n is a number so great that is humanly impossible to reach (or even imagine). So, what makes us say that we count more like A is *only* the intuitions that we have about our counting practices in mere hypothetical (i.e. non-actual!) scenarios like those about reaching and counting after n; however our intuitions about what we would do or can do in hypothetical (non-actual) situations can be incredibly vague and unreliable and, more importantly, as Kripkenstein's 'quus' function has shown us[1], any rule-following ascriptions which are based in intuitions regarding non-actual situations are extremely underdetermined. Any determinacy in rule-following ascriptions can only be grounded in *actual* practices. A similar point is that A and B's behaviour starts to diverge only after reaching n; but since n is *ex hypothesis* 'humanly-unreachable' our intuitions about human rule-following dispositions after counting n concern intuitions about what is humanly-impossible – and there does not seem to make much sense in talking about what we are disposed to do in situations impossible to perform. Though not decisive, these considerations at least undermine the certainty in thinking that we count more like A rather than B.

2. Can we even state the requirement in a non-question-begging way? The familiar dialectic that we have been exploring for finiteness or computability reappears here. Let us recall the case for computability. We wanted to say that for a model to be intended it needed to be computable; however, the notion of computable model seems to presuppose, we could argue, a prior understanding of basic number-theoretic concepts. This implies that in order to determine what the natural numbers are we would already have to have a clear conception of what the natural numbers are, making circularity evident. Now, the same happens with infinity. For what can an infinite set be?; well, we would say that a set is infinite if it has no bijection with a finite set. But what can a finite set be?; well, a set is finite if it has a bijection with an initial segment of the natural numbers.

[1] As it is well-known 'Kripkenstein' is the colourful amalgamation denoting 'Wittgenstein's [rule-following] argument as it struck Kripke'. (Kripke, 1982 : 5) For the equally well-known 'quus' function see also Kripke, 1982 : 7f.

> Hence, similarly as in the computability case, the requirement presupposes what it tries to prove.

6.2.2 Practical Matters

Another option is given by the impression that finite inconsistent models do not accommodate our everyday use of natural numbers. Denyer is quite clear in this point so we will be excused if we quote him at length:

> If the magic number for your paraconsistent arithmetic is low enough, then there are simple ways to show that you mean what you say. If for example the magic number is ten, we can see if you are happy to accept a cheque for £10 in complete discharge of a debt of £20; we can see if, when it comes to this sort of crunch, you are still ready to accept that $10 = 20$. And such a readiness would persuade me at least of your bona fides [...] Priest however has made his magic number big enough for him to be insulated from financial tests of this or any other kind. His magic number is larger than any number that could be sensibly specified in a lifetime [...] and no financial implications either. We may wonder how Priest can be so sure that the magic number is so large. For certainly the fact that we all have conclusive reason for accepting that $10 \neq 20$ is not, by Priest's lights, conclusive reason against accepting also that $10 = 20$. (Denyer 1995 : 574)

There is an easy way out of the financial qualms: suppose again that we locate this magic number in a point with no physical or psychological reality such that it would never show up in our *actual* practice; then all our ordinary use of numbers would concern only the consistent tail of our finite inconsistent models – these kind of extensions for *natural number* would respect our practice, as in when we go to the Bank. The second point of Denyer's argument – that $10 \neq 20$ does not by itself (paraconsistently) preclude $10 = 20$, is rather weak. Even though something might be logically possible, it does mean it is actually true.[2] Mere logical possibility does not force us to consider models with $10 = 20$ as intended, for simply put there is no reason to do so; these models with $10 = 20$ would not respect the practice.

6.2.3 Induction

Argue that finite inconsistent models do not account for *bona fide* induction. We note that, as a matter of definition, any finite model of PA satisfies induction. But Quinon & Zdanowski claim:

> It seems that simply by definition there cannot be a model for arithmetic which do not satisfy induction. Nevertheless, [...] What is basic for us are the natural numbers and our computational experience with them. On this ground the principle of induction should be supported by some argument. [...] the algorithmic definition of the natural numbers is a basis for a justification of induction. Indeed, if natural numbers are exactly the following objects:
>
> — 0 is a natural number,
>
> – if a is a natural number then $a + 1$ is also,

[2]See Priest, 1996 : 657-658; Priest 1998 : 422.

then to show that a given property P is true about all natural numbers it suffices to show the premises of the induction axiom for P. (Quinon & Zdanowsi, 2007 : 315-316)

Now, it can be claimed that under finite models, what the natural numbers turn out to be is somehow independent of the induction principle. Consider our toy model $\mathcal{N}^*/\sim$: since this is a model of arithmetic with only seven distinct elements, it is very easy to determine if, for a given (first-order definable) property P, P holds for *all* the natural numbers (in the model) – since there are only seven numbers we may just directly check if for each number n it is the case that $P(n)$. However, this seems to render the Induction Schema rather dispensable, contradicting our intuitions regarding the indispensability of induction. Again, the solution is more of the same: by fixing the least inconsistent number in a point with no physical or psychological reality, the Axiom of Induction could be restored: since, in these models, we would never be able to *actually* test case-by-case if P holds for *all* the natural numbers, the only way to prove the claim would then have to be by induction.

6.2.4 (Again) Finiteness

We present two ways to characterize the intended models up to isomorphism via finiteness.

- **Case Study 1**

  Consider augmenting the signature of PA with the two-place Rescher's quantifer Q^R semantically defined by

  $$\mathcal{M} \models Q^R(x,y)(\phi(x),\psi(y)) \text{ iff } \mid \{d \in D : \phi(d)\} \mid < \mid \{d \in D : \psi(d)\} \mid$$

  Informally, Rescher's quantifier states that the cardinal number of ϕ's is strictly less than the cardinal number of ψ's. Härtig's quantifier Q^H – specifying that the cardinal number of ϕ's is equal to that of ψ's, can then (with Choice) be defined as[3]:

  $$\mathcal{M} \models Q^H(x,y)(\phi(x),\psi(y)) \text{ iff}$$
  $$\mathcal{M} \models \neg(Q^R(x,y)(\phi(x),\psi(y)) \vee Q^R(y,x)(\psi(y),\phi(x)))$$

  We can then express that if x equals y then they have the same (cardinal) number of predecessors

  $$(*) \quad \forall x \forall y(x = y \leftrightarrow Q^H(u,v)(u<x, v<y))$$

  Theorem: The theory of *Classical* PA + $(*)$ characterizes $\mathcal{N}$ categorically.[4]

[3] Q^H is definable from Q^R but not vice-versa. So a language with Q^H but not Q^R has a strictly weaker expressive power – because of this, we may want just to add Q^H directly to the signature of PA instead of defining it via Q^R.

[4] By Classical PA we mean PA with underlying first-order classical logic. This requirement is important for in the proof we make use of the fact that classical models have order-type $\mathbb{N} + \mathbb{Z} \times \eta$, which is not the case for many paraconsistent models.

Proof. Clearly $\mathcal{N} \models$ PA. It can also be checked that $\mathcal{N} \models (*)$ so that $\mathcal{N} \models$ PA$+(*)$. Now, we know that every classical model of PA has order-type $\mathbb{N}+\mathbb{Z}\times\eta$. We wish to show that if $\mathcal{M}$ is an arbitrary model of PA and if $\mathcal{M} \models (*)$, then $\mathcal{M}$ has order-type $\mathbb{N}$ – this means that only models with order-type $\mathbb{N}$ satisfy $(*)$. Suppose for contradiction that there is a model $\mathcal{M}$ of PA $+ (*)$ with order-type such that $\eta \neq 0$. Then, there are non-standard elements $a, b \in M$ such that $a = b+n$ for a natural number $n \neq 0$. Consider the set of predecessors of both a and b. Since the sets must have the same cardinal number and since $\mathcal{M} \models (*)$, it follows that $a = b$. Contradiction. Hence, $\eta = 0$. We conclude that every model $\mathcal{M} \models$ PA $+ (*)$ is of order-type $\mathbb{N}$. As a consequence, $\mathcal{M} \cong \mathcal{N}$. ☐

- **Case Study 2**

  Consider now moving to an infinitary logic $\mathcal{L}_{\omega_1\omega}$ that allows for infinitely-long disjunctions, in the signature of PA. That is,

  - $\bigwedge \varphi$ and $\bigvee \varphi$ are $\mathcal{L}_{\omega_1\omega}$-formulas, for any set of $\mathcal{L}_{\omega_1\omega}$-formulas φ of size $<\omega_1$
  - $\exists V \varphi$ and $\forall V \varphi$ are $\mathcal{L}_{\omega_1\omega}$-formulas, for any sets of variables V of size $<\omega$, and any $\mathcal{L}_{\omega_1\omega}$-formula φ such that if $\exists X$ or $\forall X$ occurs in φ then $X \cap V = \emptyset$

  We obtain categoricity by requiring for a model of PA to form a discretely ordered semi-ring (i.e. without additive inverse) such that every element is some n^{th} successor of the additive identity. We can express the fact that every number is found after finitely many iterations of the successor function:

  $$(**) \qquad \forall x \bigvee_{n<\omega} x = S^n(0)$$

  Similar to before, the theory of Classical PA $+ (**)$ characterizes $\mathcal{N}$ categorically. Another way to achieve the same result without sentences of infinite length is to formalize PA in a weak second-order logic instead. Weak second-order logic allows for quantification over second-order variables only over finite sets without the total power of full second-order. When evaluating a second-order formula $\forall X\varphi(X)$ or $\exists X\varphi(X)$, it can only be considered relations over the domain that hold of finitely many elements. It is straightforward to express that there are only finitely many predecessors for each element, characterizing $\mathcal{N}$ categorically:

  $$(***) \qquad \exists X \forall x \forall y (y<x \to X(y))$$

All these strategies have at some time been proposed to characterize intended models of classical PA up to isomorphism.[5] However, and again, they are based on purportedly circular notions. Case 1 requires understanding the semantics of Härtig's quantifer. But to understand its *intended* semantics we have to understand the as we saw problematic notion of infinite cardinality. Similarly, Case 2 requires either (a) previous understanding of *infinitary* disjunction (and therefore the again problematic infinite cardinality) in $(**)$ or (b) of quantification over arbitrary *finite* sets (i.e. of *finite* cardinalities) in $(***)$. In order to fix what the *natural numbers* are, we continue to appeal to an already fixed notion of the natural number sequence. Discussing similar examples, Read observes:

[5]For a short survey see Button & Walsh, 2018 : Chap. 7. For elaboration see Read, 1997.

> A similar objection can be levelled at $\mathcal{L}^{\omega 2}$ [weak second-order logic]. [...] It simply forces finiteness [...] by a metatheoretical constraint in the semantics that only finite sets belong to the range of the predicate variables of quantification. This does not provide the necessary insight to the character of $\mathcal{N}$ and of 'finite number'. (Read, 1997 : 92)

Not only that but all this cases amount no more to the claim that 'intended numbers in an intended model should be obtained by iterating the successor operation finitely many times'. And a finite LP-model will intuitively satisfy this claim and so be intended.

6.2.5 Conclusion of the Inductive Argument

The cornerstone of the above arguments essentially comes down to an insight already noted by Putnam and that serves as the epigraph to this work: '[H]uman practice, actual and potential, extends only finitely far. Even if we say we can, we cannot 'go on counting forever'.' (Putnam, 1981 : 67) Our number-theoretic practice is essentially finite; that is, it will at most only *actually* involve finitely-many numbers and finitely-many operations defined on them.[6] In this sense we may stipulate a certain number such that itself and all its successors will never appear in or be relevant to our practice. This is precisely what we did when supposing the least inconsistent number to be *so large that it has no physical meaning or psychological reality*, larger than the number of combinations of fundamental particles and larger than any number that could be specified in a lifetime. When we do this we are able to have inconsistent models where the inconsistencies start to appear at such a large point that all our actual computations will concern only the consistent tail of the model, where numbers behave 'normally'. That the inconsistencies must start at a very large point is already a significant requirement that rules out some (but not all!) of the finite inconsistent LP-models from the class of intended models. The requirement immediately dismisses *cyclic models*. We recall that these are models obtained by quotiening a (standard and consistent) model under the following relation:

$$x \sim y \text{ iff } x = y \pmod{m}$$

where m is a small, 'easily reachable' number. For instance, consider again the standard model $\mathcal{N}$ and the quotient $\mathcal{N}/\sim$ where

$$x \sim y \text{ iff } x = y \pmod{4}$$

Then the successor graph is:

$$\begin{tikzcd} {[0]} \arrow[r] & {[1]} \arrow[d] \\ {[3]} \arrow[u] & {[2]} \arrow[l] \end{tikzcd}$$

Cycle models as the above cannot be intended since they don't accommodate *actual* practice: if the reader owes 40 to the Bank, they will not cancel your debt with the excuse that $40 = 0$ (or more precisely, $[40] = [0]$). Now, are all finite cyclic models unintended? We hesitate here. For suppose $\mathcal{N}/\sim$ is built from $x \sim y$ iff $x =$

[6] Again, we exclude considerations involving hypercomputations inside black holes.

$y \pmod n$, where n is that 'very very large' number. This will have as a consequence that the natural number 1 (or [1]) will be inconsistent, because $[1] = [1+n]$ given $1 = 1+n \pmod n$. And again this model cannot be intended since it goes against the practice. However, n is our 'very very large' number; therefore, it would seem that scenarios where the equality $[1] = [1+n]$ would be noticeable in the practice would never appear, because, by construction, n is so large that *actual* situations that involve n do not exist. Still we think this argument may need further elaboration. Moving on, collapsed models can have a (consistent) tail before entering into a cycle; we called these *heap models*. We build a finite heap model quotiening a (standard and consistent) model under:

$$x \sim y \text{ iff } (x, y \ < \ m \wedge x = y) \vee (x, y \geqslant m \wedge x = y \pmod p)$$

In the same way as before, if the element m is very small, an 'easily reachable' number, the resulting model cannot be intended – the same financial worries can be easily imagined. However, if

$$x \sim y \text{ iff } (x, y \ < \ n \wedge x = y) \vee (x, y \geqslant n \wedge x = y \pmod p)$$

where, again, n is our 'very very large' number, all our *actual* arithmetical operations would involve numbers that would belong only to the consistent tail of the model. This means that no problem would arise regarding our *actual* practice with natural numbers if we take these models as intended.

Hence, we have our inductive argument. By considering a couple of constraints and how they still allow intended finite inconsistent LP-models, we have offered inductive support to the negation of the supplementation strategy. From this discussion, however, we were also able to reduce a bit more the class of intended finite LP-models – for these models to be intended and to respect our practices, they must (at least) be heap models where the cycle (and, therefore, the inconsistencies) start at a very very large number.

We end this section by noting that one option we have not covered is requiring for the underlying logic to be classical; but this is part of a much more general and longer discussion on the status of paraconsistent logic that here we could not possibly engage with.

6.3 The Deductive Argument

The purpose of this section is to better understand the assumptions behind the inductive argument(s). We will first consider a reply to intended finite LP-models presented by Tim Button and use it to present our own take on the viability of the supplementation strategy. Before we do this we introduce an argument proposed by Bays against Putnam's just-more-theory manoeuvre.

6.3.1 The Supermodel Argument

Bays(2008) notes that the key idea behind the 'just-more-theory' manoeuvre consists in the fact that any additional requirement imposed on a language's referential relations can be viewed as just a new sentential theoretical or operational constraint addable to the language itself and up for reinterpretation. Realist attempts to fix the intended interpretation of the language have been met with a first-order regimentation of their proposal later reinterpreted with Putnam's favourite model theory. Against this too liberal use of the just-more-theory manoeuvre, Bays(2008) gives a trivial semantics accountable by just-more-theory-like reasoning – his *Supermodel Argument.* The argument is used as a dismissal of Putnam's manoeuvre: either the supermodel is accounted by the manoeuvre working as a *reductio*, or the supermodel and the just-more-theory are refuted on the same grounds.

We first build Bays' *supermodel* G:

- Define a satisfaction relation for $\models_g$ that agrees with the classical first-order recursive clauses for all the logical operators except for negation, i.e. for any model $\mathcal{M}$ and variable assignment v, the clause for $\neg$ is defined as $\mathcal{M}, v \models_g \neg\varphi \Leftrightarrow M, v \models_g \varphi$.
- Define a model $G = \langle D, I \rangle$ such that $D = \{d\}$ and all relation symbols are interpreted maximally; i.e. for every n-ary relation R and n-tuples $\langle I(t_1), ...I(t_n)\rangle \in D^n : \langle I(t_1), ...I(t_n)\rangle \in I(R)$.

Claim: Under the satisfaction relation $\models_g$, G is a trivial model: for all φ, $G \models_g \varphi$.

Proof. The proof is by induction on the complexity of φ. Every atom is satisfiable since relation symbols have a maximal interpretation. For the negation case, we have by induction hypothesis $G \models_g \varphi$ which by the redundant negation clause implies $G \models_g \neg\varphi$. For conjunction, the truth of the conjuncts implies the truth of the conjunction. Finally, for quantifiers, since the domain of G has only one element, $G \models_g \forall x\varphi \Leftrightarrow G \models_g \exists x\varphi \Leftrightarrow G \models_g \varphi(x/d)$. □

If 'satisfaction' is understood as $\models_g$, then G will trivially satisfy every sentence. Consequently, it will satisfy all the theoretical-cum-operational constraints imposed on the language's referential relations. It then follows that theoretical-cum-operational constraints do not seem to commit to the existence of more than one object; the supermodel G is such an example of an interpretation satisfying our constraints in a one-element model. Of course we may add additional requirements relative to, say, cardinality or imposing a more 'natural' semantic clause for negation. But by the 'just-more-theory' manoeuvre the extra requirements may, after being first-order regimented, be added to the total collection of sentences in the language and, after, subject to multiple reinterpretations. Since G satisfies everything, it will also satisfy those new requirements. For example, we may add a *Multiplicity Constraint* of the form:

> **Multiplicity Constraint:** An intended interpretation must have more than one element.

Obviously, we may formalize the above claim by $\exists x\exists y(x \neq y)$, and, indeed, G will satisfy the requirement – after all, $G \models_g \exists x\exists y(x \neq y)$. Similar arguments may be rehearsed for other cases. In the end, Bays(2008) concludes that the new constraints do not dismiss G as unintended and, quite on the contrary, show that every theoretical-cum-operation constraint is trivially satisfiable in G .

The *Supermodel Argument* is meant as a *reductio* of Putnam's manoeuvre. But to appreciate exactly what the argument is supposed to be a reductio of we need to look more closely at Putnam's way to reinterpret additional theoretical constraints. Putnam's argument is dependent on the privileged semantics within which non-intended interpretations are to be formed – for exploring the basic limitative meta-properties of first-order logic, classical standard model theory is required. Still, to model a language's referential relations we may prefer to work with a stronger semantics, say, first-order model theory plus a Causality Constraint:

> **Causality Constraint:** An intended interpretation must respect all the intended causal relations between words and referents.

Since, presumably, we sometimes wish to use different names for different objects, the interpretation G that assigns to all names the same denotation d^G will fail to respect the causal links embedded in a speaker's referential practice. When specifying the semantic clauses for a satisfaction relation we must account for these relevant causal links. Such a requirement can then be seen as specifying which kind of model theory should be used when interpreting our theoretical constraints, which can be immediately recognized as a substantially different project than that of merely adding to the language new theoretical constraints reinterpreted in the standard classical model theory that Putnam chooses to use. Putnam's manoeuvre tends to confuse these distinct tasks, taking the Causality Constraint as an example of the latter and not of the former:

> [...] we can view Putnam's just-more-theory defence as an attempt to close the gap between the kinds of strong background semantics preferred by realists [e.g. classical first-order semantics plus a causality constraint] and the substantially weaker background semantics for the model-theoretic argument [...] by reducing the realist's strong semantics to the first-order semantics needed for Putnam's model theory. (Bays, 2008 : 202-203)

After Putnam has formalised the relevant constraints in first-order logic, there will be many possible interpretations of 'Causality fixes reference' (as there will be many interpretations of what 'Causality', 'fixes' and 'reference' denote), such that multiple deviant models will satisfy the requirement. The Supermodel Argument then shows that there is nothing unique about Putnam's reduction: just as we may reduce realist's strong semantics to Putnam's preferred first-order semantics, so we may reduce Putnam's semantics to $\models_g$, the satisfiability relation with redundant negation.

For the model-theoretic sceptic the challenge is then to provide a way to refute G that does not refute Putnam's manoeuvre at the same time. More precisely, to justify the reduction from the realist's strong semantics to standard first-order semantics without at the same time accounting for the supermodel semantics, or any other kind of reduction.

Tim Button(private correspondence) locates the problem with the Supermodel G in its redundant negation operator. For let $truth_g^G$ be the property relation that is formed by considering what G satisfies$_g$ (where satisfiability$_g$ is satisfiability $\models_g$). Now, Button claims[7] that 'Whatever truth is, it's not $truth_g^G$' After all: every sentence is $true_g^G$, but we *accept* some sentences and *reject* others'. If truth was $truth_g^G$ then the assertion of φ would imply the assertion of $\neg\varphi$. However, asserting φ implies (though not always) the rejection of $\neg\varphi$. Essentially, $truth_g^G$ just does not conform with our normal assertion and rejection practice. Hence, truth is not $truth_g^G$.

[7] Thanks to Tim here for permission to quote his comments.

6.3.2 Button's Argument

The *Supermodel Argument* provides important insight regarding LP-models of PA. First, consider $truth_L^{\mathcal{N}}$ (where L is first-order classical logic). Ideally, the moderate realist will want to characterize only the standard model, up to isomorphism, as the intended model of PA. This will in part require identifying arithmetical truth with $truth_L^{\mathcal{N}}$. However, the constraints defined on the class of intended models allow, *prima facie*, for the inclusion of LP-models. In this sense, arithmetical truth can just be interpreted as $truth_{LP}^{\mathcal{N}/\sim_1}$, where $\sim_1$ is an equivalence relation that produces a suitable non-trivial inconsistent collapsed model, and $truth_{LP}^{\mathcal{N}/\sim_1}$ the property relation formed by considering what $\mathcal{N}/\sim_1$ satisfies$_{LP}$ (where satisfiability$_{LP}$ is satisfiability $\models_{LP}$). $truth_{LP}^{\mathcal{N}/\sim_1}$ will equally conform with our ordinary mathematical practice. However, we may just like in the Supermodel Argument give a different interpretations of arithmetical truth. Collapsing $\mathcal{N}$ by a relation $\sim_2$ that places all the original numbers in one single equivalence class will produce a model much like Bays' G. And if $\mathcal{N}/\sim_2$ is to be intended, then the collapsing strategy will just seem plainly implausible. The challenge is then to justify the reduction from $truth_L^{\mathcal{N}}$ to $truth_{LP}^{\mathcal{N}/\sim_1}$ in such a way as to not also account for $truth_{LP}^{\mathcal{N}/\sim_2}$. In fact this is easily achieved: as we noted $truth_{LP}^{\mathcal{N}/\sim_2}$ does not conform with our ordinary mathematical practice; but $truth_{LP}^{\mathcal{N}/\sim_1}$ may. However, if *ordinary mathematical practice* is to include assertion/rejection practice, we find a challenge. For, as Button wants to argue, 'I suggest that our behaviour (concerning assertion and rejection) also allows us to rule out LP models'. After all, if we build an inconsistent LP-model, the equivalence relation will relate two numbers $x \sim y$ such that $x \neq^{\mathcal{N}} y$. As a consequence, we will have $[x] =^{\mathcal{N}/\sim} [y]$ and $[x] \neq^{\mathcal{N}/\sim} [y]$. It is then easy to see that $\mathcal{N}/\sim$ will satisfy$_{LP}$ the following formulas:

(1) $\neg\exists x\exists y(x = y \wedge x \neq y)$

(2) $\exists x\exists y(x = y \wedge x \neq y)$

Normally, we would assert (1) and reject (2). But if truth were $truth_{LP}^{\mathcal{N}/\sim_1}$ we would both assert (1) and (2). Whatever truth is, it's not $truth_{LP}^{\mathcal{N}/\sim_1}$.

This is then Button's argument against intended LP-models. We think however that this counter-argument is different than the G case: it raises different challenges in such a way that without a proper analysis of the relevant structural differences we risk mischaracterization of the assumptions involved. Button's argument against G, to repeat, that redundant negation contradicts everyday assertion/rejection practice, just seems an iteration of the old *exclusion problem.* The exclusion problem states that the obtaining of the negation of a statement should allow for the incompatibility of the obtaining of that statement: when we assert $\neg\varphi$ we may wish to express that φ does not obtain, that φ is excluded on logical grounds. Now, if *negation* is interpreted redundantly, the assertion of $\neg\varphi$ cannot *exclude* the assertion of φ: in this sense, redundant negation is unable to express genuine exclusion or incompatibility between different facts. This cannot be the same argument Button has in mind against LP-models. Firstly, the argument against G concerns a general account of how negation is or should be defined; when against LP, we are only concerned about negation in a unique case involving arithmetical identities and non-identities. Secondly, unlike G, there are non-trivial LP-models where the assertion of a formula is compatible with the rejection of its negation. Thirdly, and more importantly, if Button's argument against LP-models was just another case of the exclusion problem, then it would have little force. There is already a way to handle it in the paraconsistent case. Here, the crucial idea would be that negating a statement should be distinguished from the speech acts of acceptance/denial and an agent's cognitive state of assertion/rejection, wrongly conflated in the Frege-Geach Thesis:

$$\dashv_a \varphi \Leftrightarrow \vdash_a \neg\varphi^8$$

Instead, we may take the rejection of a statement as, first, a primitive act not reducible to the assertion of its negation (against the Frege-Geach Thesis) and as, second, the expression of the refusal in believing it. It follows that if an LP-theorist wishes to express incompatibility or exclusion, she only needs to incur in the pragmatic act of denying (rejecting) the statement (which may be done by uttering 'not' – context and pragmatics disambiguates if 'not' is a rejection or an assertion of negation).[9] Now, if Button's argument consists only in noting that LP does not allow for exclusion – that the assertion of (1) does not exclude the assertion of (2) and that nothing else can, then the argument is just confusing two distinct non-interdefinable speech acts. Of course the Frege-Geach Thesis is a polemic matter and the minute details of the debate do not need to concern us here; we aim only at noting that there are more interesting ways to look at Button's objection.

A more promising approach to Button's argument against LP-models consists in taking the argument as stating something particular about the arithmetical sentences involved. Button's argument just seems to be defending that we have an expectation about the natural numbers, actually present in our normal number-theoretic practice, yielding the truth of (1) and falsity of (2). *If numbers do not satisfy (1) they are not numbers. If numbers satisfy (2) then (stamp the foot, bang the table) they are not numbers!* Button's point is just that in any interesting LP-model, that is not isomorphic to the standard or other consistent models, we will have to identify two different numbers, and we are unwilling to do that. At this point the argument starts to look awfully like an incredulous stare.

6.3.3 Against Button's Argument

Still, it may strike us as rather odd how quickly the above argument solves the problem of the indeterminacy of the natural number sequence (regarding LP-models). The rebuttal just seems too easy! When giving the inductive argument we saw many cases where, *pace* Button, neither assertion/rejection practice nor expectations about the natural numbers should be taken at face value – for instance, we would assert that 'every number should be finitely far from zero' but its rather hard for a moderate realist to justify this. Not all assertions/rejections can be taken to be true without begging the question against the model-theoretic sceptic. Of course, the situation here is not entirely analogous with the examples we covered in the inductive argument (for example, in the Härtig's Quantifier case). When rejecting that 'no two different numbers are identical', we are not (so it seems) assuming, in the semantics, concepts that we were already trying to pin down; rather we are simply rejecting some sentences in the object-language. Yet, one could argue that denying inconsistent features (like identity) to *all* the natural numbers seems still to presuppose a lot of information about the natural numbers that is not available to the moderate realist. Before we

[8]'$\dashv_a \varphi$' is read as 'Agent a denies/rejects φ' and '$\vdash_a \varphi$' as 'Agent a accepts/asserts φ'.
[9]For details see Berto(2012).

elaborate on this point, let us just stress again that not any intuition can decide the correctness of a model. But are there intuitions that may decide on correctness? Well, those that are very well-entrenched in the practice can. That is, for instance, we would require that the 'true' number 1 should be finitely far from zero; a model that would require somehow infinitely many iterations of the successor function to reach the first element of the sequence (if such absurd scenario can even be envisioned) would not count as intended. However, there will be a point (numbers after the very very large n never present in our practice) for which these intuitions will not apply; simply because, the numbers do not appear *in* and therefore are not justified *by* the practice. Hence, it seems that the constraints that work are not quantified claims ranging over *all* the natural numbers, but rather those that concern very specific elements.

Generalizing, what the above suggests is that *variable-free* arithmetic formulas grounded in actual numerical practice impose legitimate justifiable constraints on the intended model. We can see a dim glimpse of such a proposal in Priest(1996), though, at least to the knowledge of the present author, such a proposal has yet to be elaborated with any sufficient detail:

> Doubtless, for example, most people are not disposed to assert the existence of a number greater than or equal to all numbers. But this simply reflects a belief that they have acquired during their education. To be constitutive of the truths of arithmetic, the dispositions must be of a more fundamental kind. [...] a natural position is that it is those dispositions that should count here. It is, after all, the practices of counting, adding, etc., that constitute learning arithmetic. Dispositions concerning *generalised* claims (i.e. those employing variables) are part of a *theory* about numbers that people come to acquire later. (Priest, 1996 : 657)

So there are intuitions that constitute justifiable constraints on the class of intended models: quantifier-variable-free formulas accounted for within actual arithmetical practice. But Button's argument, though concerns arithmetical practice, involves existentially quantified claims. Further, as we noted, besides assertions and intuitions, Button's claim is also equated with mathematical truth. That we *know* that (1) is (only) true and (2) is (only) false. Benacerraf's problem shows, however, that it is a rather difficult task that for the realist, or better, the moderate realist in our case to explain how exactly she is able to *know* that an arithmetical statement is true:

> [O]n a realist (i.e. standard) account of mathematical truth our explanation of how we know the basic postulates must be suitably connected with how we interpret the referential apparatus of the theory. [...] [But] what is missing is precisely [. . .] an account of the link between our cognitive faculties and the objects known. [. . .] We accept as knowledge only those beliefs which we can appropriately relate to our cognitive faculties. [S]omething must be said to bridge the chasm, created by [...] [a] realistic [...] interpretation of mathematical propositions [...] and the human knower. (Benacerraf, 1973 : 674)

Normally Benacerraf's problem is read in an epistemic way: the realist must account for her mathematical *knowledge* while recognizing the acausal character of mathematical entities. McGee(1993) gives a more fundamental account of the problem:

> [Benacerraf's] problem is sometimes posed as a problem in mathematical epistemology: How can we know anything about mathematical objects, since we don't have any causal contact with them? But to put it as a problem in epistemology is misleading. The problem is really a puzzle in mathematical doxology: Never mind knowledge, how can we even have mathematical beliefs? Mathematical beliefs are beliefs about mathematical objects. To have beliefs about mathematical objects, we have to refer to them; to refer to them, we have to pick them out; and there doesn't appear to be anything we can do to pick out referents of mathematical terms. (Mcgee, 1993 : 103)

What McGee's remark provides is a more complex challenge for the realist. From Benacerraf's original reading we noted that to the assertion of the truth of an arithmetical expression (regarding some (acausal) mathematical objects), we are entitled to demand a suitable epistemic account of the grasp of such truth; now, more, to a suitable epistemic account of the grasp of such truth, we are entitled to demand a suitable doxological[10] account of such objects. An arithmetical universally quantified formula $\varphi := \forall x \phi(x)$ (where x occurs free in ϕ) would then state that *all* the natural numbers are such that ϕ. Let us suppose, for the purposes of the argument, that φ is claimed to be true of the natural numbers. How should we understand this? Following Mcgee, knowledge of φ implies ability to refer to the *natural numbers*. Hence, Button's claim that (1) is true and (2) is false must be made relative to some background 'conception' of *natural number*. Now, here, we are purposely introducing the rather vague notion of 'concept'. The introduction is motivated on very dummettian grounds:

> [...] each domain for the individual variables will constitute the extension of some substantival general term (or at least the union of the extensions of a number of such substantival terms) [...] (Dummett, *Frege: Philosophy of Language* quoted in Rayo & Uzquiano, 2006 : 11)[11]

First, excluding particular matters of taste, it is common to associate 'concepts' with criteria of identity and of application: a concept C encodes criteria to decide what objects it applies to, and under what conditions two objects falling under it are identical. Secondly, there is a natural association between (mathematical) concepts and isomorphism types – the notion of, say, 'ω-sequence' provides a set of properties (those invariant under isomorphism) that determine which objects are an ω-sequence (those objects with a particular structure). This insight leads to the idea that (some)[12] mathematical concepts are as fine-grained as isomorphism types so that the former are explained via the latter. Since *natural numbers* are thought, at least in the wide context where our discussion is placed, as the intended isomorphism type of PA and since the intended isomorphism type provides criteria to determine what structures are the natural numbers, the isomorphism type just works as ersatz for the concept. It is in this sense that the concept of *natural number* is captured by the isomorphism type corresponding to the intended models.

Mcgee then shows that epistemological concerns are always second to doxological ones. Further, a suitable account of our capacity to refer to the objects to which we ascribe

[10] The term comes from (Button & Walsh, 2018 : chapter 6).

[11] Here, 'substantival term' can be though along side a concept.

[12] Note that not all isomorphism types need to determine a mathematical concept. For example, if we believe that concepts presuppose our understanding of them, we will ever be able to articulate only finitely many. So there will be infinitely many structures that do not correspond to concepts.

a certain property involves an understanding of the concept under which those objects fall, that is, a capacity to identify them. This much seems uncontentious – any theory of reference must acknowledge a prior possession of minimal identity criteria: for how would we ever be able to refer to cats and not cherries, unless we had a way to pick out (i.e. identify) the cats instead of the cherries? Continuing with the ω-sequence example: '[...] consider the epistemological question of how we could know that there actually are any ω-sequences. The very attempt to *ask* this epistemological question presupposes that we possess the concept ω-sequence: if we lacked that concept, then we could not even pose the question.' (Button & Walsh, 2018 : 149) It should be noted that even though a necessary condition, identity criteria is not sufficient for reference. Funny enough, the reasons why are again dummettian. Orthodoxy has it that 'The fact revealed by the set-theoretic paradoxes was the existence of indefinitely extensible concepts [...]' (Dummett, 1994 : 26). For suppose we are able to quantify over *all* sets; to place them in some (set-like) domain over which our variables are allowed to widely range. The domain, call it E, will correspond to the extension of the concept 'set'. Then by standard russellian reasoning we consider the set R of members of E that are not members of themselves and contradiction. Since, from the attempt to supply an extension for *set* we derive paradox, *set* is indefinitely extensible. Still, the Axiom of Extensionality provides clear identity criteria to determine when two objects falling under the concept *set* are identical. Hence, identity criteria is not a sufficient condition to refer to all elements/determine the extension of a concept.

One important distinction is in place. Classical quantification maps objects in a domain to truth values; for it to be justified it must be clear which mapping is being used, which demands clear understanding of which objects do belong to the domain. For Dummett this just consists in having identity and application criteria. As he writes:

> In order to confer upon a general term applying to concrete objects – the term "star," for example – a sense adequate for its use in existential statements and universal generalizations, we consider it enough that we have a sharp criterion of whether it applies to a given object, and a sharp criterion for what is to count as one such object – one star, say – and what as two distinct ones: a criterion of application and a criterion of identity. (Dummett, 1994 : 24 – 25)

However, such an example considers non-mathematical objects such as stars. For the mathematical case he immediately adds an extra proviso:

> It is otherwise, however, for such a mathematical term as "natural number" or "real number" which determines a domain of quantification. For a term of this sort we make a further demand: namely, that we should "grasp" the domain, that is, the totality of objects to which the term applies, in the sense, of being able to circumscribe it by saying what objects, in general, it comprises – what natural numbers, or what real numbers, there are. (Dummett, 1994 : 25)

It is not clear how to understand 'grasp' here. From Dummett's remark, we see that 'grasp' of an object is somehow equated with the epistemic claim of knowing that there is such an object and how it is like. Further, quantification over a mathematical domain (a domain comprised of mathematical objects) would demand a grasp of *all* the objects that comprise it – let us call this Dummett's Thesis. Let us assume this working hypothesis for the moment: quantification over the extension of a mathematical concept does require grasp of all the objects falling under it. How does this relate with Button's objection? Well, we need a slight small detour before we can join all the pieces.

Strict finitists defend that there is no infinity of natural numbers: the natural number sequence must end somewhere with a greatest number, call it, L. Now, how is L like? Is L prime? divisible by 3? does $L + 1$ exist? does L^2 exist?

> The key of dealing with objections like these, it seems to me, is given by wondering what are the presuppositions of one of the above questions, viz. "Is L different from $L + 1$?" One of the presuppositions is that I can rightfully speak of L and $L + 1$. But surely that is the problem to start with! To the extent that one can speak about a number $L + 1$, I should be able to make a representation of it, immediately implying that L is no longer the greatest number. As a consequence, to reply to the above question "I am sorry, but I cannot answer this question" is perfectly defensible. [...] with respect to the greatest number L, one can very well answer: it is that number about which no question whatsoever can be asked. (van Bendegem, 2012 : 144)

Essentially, the closer the strict finitist comes to a characterization of L the easier it is for critics to imagine numbers greater than L or questions that make L seem very arbitrary. The best approach is to secure L from our normal mathematical expectations regarding, say, the 'closeness of addition' or 'primality', by refusing to offer a characterization of L at all. We can see the similarities between L and the (by now well-known) requirement 'n has to be *so large that it has no physical meaning or psychological reality*'. Consequently, statements of the form 'for all numbers up to L' must be meaningless:

> As the greatest number itself is indeterminate, there is no sense in making statements about *all* numbers. A universal quantifier such as "For all n up to L" must be meaningless, for, [...] from the moment we make a representation of all numbers up to L [including L itself], L ceases to be the greatest number. (van Bendegem, 2012 : 145)

For suppose we have a grasp of L – then L ceases to be L. So we cannot have a grasp of L. But if we don't grasp it how can we know how L is like? How can quantification of the form "For all n up to (and including) L, n is ϕ" be ever justified if we do not (and cannot) know that L is ϕ?

Now, if we subscribe to Dummett's Thesis, Button's argument for (2) being false presupposes a conception of the natural numbers. Recall also that we have located (in the intended LP-models) the inconsistencies at a very large number n. If Button says that models, where 1 or 2 or other very small numbers are inconsistent, are not intended because those numbers are *not* inconsistent we will happily agree. But what happens with models where the inconsistencies start at a very large n? Those are the cases that should interest us the most because those were the models that we argued are intended. Well, again following Dummett's Thesis, if Button wishes to say those models are wrong because the very large n is not identical with a different number, then he has to have a clear grasp of how n is like. But, like van Bendegem's L, from the moment Button grasps n, n ceases to be n, for that object starts to have a physical meaning or psychological reality. In a word (or two), *quantification over n is indefinitely extensible*. Hence, Button cannot have a clear grasp of all the natural numbers for he cannot have a clear grasp of how n is like. And if he cannot grasp it, he cannot ascribe properties to it of the form 'n is so and so' or 'n is not identical with a number different from n'. Conclusion: Button cannot know that n is not inconsistent; more generally, Button's claim is unwarranted.

The above reasoning is not definite. And it is clear why. The argument is dependent on what we called *Dummett's Thesis*, which is polemic at best. We cannot elaborate much further on this point. The relations between quantifying and grasping the objects over which we are quantifying are complex enough to write a full book. We just note a problem with this view. The PA axioms also comprise universal quantifications over the natural numbers; and a similar reasoning as before might show that, say, the axiom $\forall x(x + 0 = x)$ seems equally illegitimate for very large numbers like our n. We can bite the bullet here and concede that we cannot know that natural numbers are such that they satisfy the PA-axioms but this seems very unappealing.

Either way, let us quickly try another strategy that does not use Dummett' Thesis. The point now is that to say that n is not inconsistent just seems to require a great deal of epistemic power not available to a moderate realist: how can she know that n is so and so if (by definition) we can not make any sense of n. Debates can sometimes be paralysed by the second-order debate about which side bears the burden of proof. And we find this to possibly be one of those cases. On one hand, given that we are trying to defend the very weird possibility that n might be inconsistent, it would seem that it is our side that has to justify this pretension – breaking common-sense can be seen as constitutive for bearing the burden. On the other hand, given the moderation of the moderate realist, it seems that her claim that 'n is so and so' is not that easily available to her knowledge. Let us focus on other properties rather than consistency so we might get a more clear picture. What we said about the consistency or inconsistency of n works for any other property. Arithmetical practice does not give much direction about the structure of the natural numbers after a large enough n. For example, let n be a number so large that it would take more time to say or write it than the time left until the heat death of the universe. Now, is such number prime? divisible by 3? multiple of 28? There does not seem to be any principled way for not God-like creatures such as us to ever determine that, say, n is prime – one wonders how Eratosthenes Sieve could ever be applied here! From a moderate realist view, it seems that the only thing which can justify her knowledge about numbers is arithmetical practice; but the examples in the inductive argument show that practice is silent on how the numbers behave after an inconceivably large point. This can lead us to think that, as it stands, the burden of proof lies on the side of the moderate realist: it just seems to require an extra amount of epistemic exertion *prima facie* not available to the moderate realist to determine if n has the properties that would make it consistent. And in fact, by definition of n, such level of epistemic exertion is not achievable. We can't know that n is or is not a consistent or inconsistent natural number without making n cease to be n.

The above considerations only have relative force and leave many important aspects open. Still, until they are sorted out, the deductive argument bears some initial strength against the Supplementation strategy. Coupled with the inductive argument we think we are entitled to assume that until moderate realists come up with a non-question-begging constraint or are able to explain how they know how n is like, the Supplementation strategy does not work.

6.4 Summary

We then end with the result that from a moderate realist perspective, the supplementation strategy is not at all appealing. Hence if the argument from Tennenbaum's Theorem is to be pursued, the moderate realist must go with Conclusion$_1$ or Conclusion$_2$. Either way the problems raised by Skolem and Putnam reappear.

Chapter 7

Conclusion

This work has been an exploration of the argument from Tennenbaum's Theorem and its relation with the philosophy of model theory and non-classical arithmetic. Regarding the philosophy of model theory we discussed how some authors have put the Theorem to good work in the foundations of mathematics using it when pinning down (the structure of) the natural numbers. This is of particular importance given that basic *limitative* logical results, such as the Löwenheim-Skolem Theorem and the construction of non-standard models of PA, have exposed, so its has been argued (Putnam, 1980), the inability to determine the intended models within a moderate realist view in mathematics. In this sense, it was thought that the moderate realist could find a safe haven, shielded from the protests of the skolemite sceptic, by following a two-step plan: first, point to our number-theoretic practice and stress that our everyday use of numbers justifies the idea that intended addition must be computable; second, employ the Theorem by Tennnenbaum and characterise the intended models categorically. As we saw, critics were fast to point that adding more model theory is unlikely to solve a problem already related with model-theoretic indeterminacy (i.e. indeterminacy of the intended interpretation of a theory). The argument presupposes computational terms such as 'recursivity' whose mathematical formalization is done by employing great chunks of arithmetic. Therefore, using computational notions interdefinable with the natural numbers in order to explain what the natural numbers are just plainly assumes that which it tries to prove; i.e. a firm understanding of the natural numbers. Further, a practical pre-theoretical rendering of 'recursivity' equally presupposes the same understanding of the relevant arithmetical notions.

This is then the present stage where we find the literature on the subject. Our goal was to make a new critique of the argument through the study of some computational properties of the functions defined over non-classical inconsistent models of PA. The counter-argument we gave was based on the fact that the same appeal to 'recursivity', found behind the original argument, equally justifies the inclusion of a subclass of *finite* inconsistent models of PA in the class of intended models. As a consequence the intended models do not define a single isomorphism type against the hopes of those that were expecting to solve skolemite scepticism by characterizing the intended structures categorically.

As we saw, three possible conclusions are taken from our *LP*-argument: *Bite the Bullet*, *Reductio* and *Supplementation*. Through a detailed analyses on how the Supplementation strategy could be pursued, we argued that the possibility of a legitimate and intuitive constraint, or set of constraints, capable of dismissing intended inconsistent models is not that promising. This means that, if the LP-argument is successful, the only viable conclusion must rest on one of the two remaining options. Regardless of the option chosen, both conclusions will reintroduce the epistemic challenge proposed by Skolem and Putnam which adepts of the argument from Tennenbaum's Theorem were hopeful to have solved. In this way our work has shown that Tennenbaum's Theorem does not help the moderate realist explain how we *know* that the intended model of arithmetic is the standard model, up to isomorphism.

There have been several points in our work where we have (perhaps without making it explicit) thought that the ideas discussed needed more elaboration. However since their proper development would take us too far from the main goals and arguments we wanted to present, for the purpose of rigour we couldn't possibly have discussed them in full. In this sense, we now list a set of future questions worth pursuing:

1. Our discussion revolved around *finite* LP-models. It remains to be seen if there is any prospect of having intended *infinite* inconsistent models and if these square better against the finite ones when it comes to our intuitions about the natural numbers. If this were to be the case, then it would seem that our argument would be reinforced adding more non-isomorphic models to the class of intended models.

2. We addressed the problem of formalizing sameness of structure between LP-models and found that their might be some complications when using the classical notion of isomorphism here. An interesting question would be to search for better more fine-grained methods to capture and compare the structure of LP-models so to be able to say when two models are structurally the same. A related matter would be to explore how those possible methods decide on the issue of comparing the structure between the standard model and an inconsistent one (like $\mathcal{N}^*/\sim$). In fact this is an important issue given that the success of the LP-argument is dependent on the argued assumption that $\mathcal{N}^*/\sim$ does not have the same structure of the standard model.

3. When giving the three possible conclusions to the LP-argument we stressed that we didn't aim at exhaustiveness. A possible question is if there are other reasonable reactions to the argument that we have not explored and how do they play out with the refutation of the argument from Tennenbaum's Theorem.

4. We stressed that we were interested in the argument from Tennenbaum's Theorem as used in the epistemic problem of explaining how we know what the intended model is. But we also noted that there is the parallel use of the argument with regards to the linguistic problem of explaining how we manage to refer to the intended interpretation of arithmetic. It remains to see what consequences our discussion can bear to this latter view.

5. Finally, van Bendegem(2012 : 142) sees the collapsed finite LP-models of PA as a way to develop a strict finitist view of arithmetic. It remains to see if the existence of intended finite models, that follows from accepting the *Bite the Bullet*-conclusion from the LP-argument, can boost the strict finitist project.

Appendix

The purpose of this Appendix is to quickly recall some basic technical preliminaries and notations pervasive in our main discussion. Otherwise indicated, the notion of semantic entailment used and the models defined will be those for first-order classical logic. We first define the notion of language:

> **Definition** ($\mathcal{L}$-signature) A signature $\mathcal{L}$ is a collection of constant symbols, and function and relation symbols for any given arity.
>
> **Definition** ($\mathcal{L}$-structure) For a signature $\mathcal{L}$, an $\mathcal{L}$-structure $\mathcal{M}$ consists of:
>
> 1. a non-empty set of elements M called the domain of $\mathcal{M}$;
> 2. for each constant symbol c in the signature, an object $c^{\mathcal{M}} \in M$.
> 3. for each n-ary function symbol f in the signature, a function $f^{\mathcal{M}} : M^n \to M$;
> 4. for each n-ary relation symbol R in the signature, an ordered tuple $R^{\mathcal{M}} \subseteq M^n$.
>
> **Definition** ($\mathcal{L}$-reduct) Consider two signatures $\mathcal{L}$ and $\mathcal{L}^+$ with $\mathcal{L} \subseteq \mathcal{L}^+$, and an $\mathcal{L}^+$-structure $\mathcal{M}$. We say that $\mathcal{N}$ is the $\mathcal{L}$-reduct of $\mathcal{M}$ iff $\mathcal{N}$ is the unique $\mathcal{L}$-structure with domain M and $s^{\mathcal{N}} = s^{\mathcal{M}}$ for all symbols $s \in \mathcal{L}$. We say that $\mathcal{M}$ is a signature expansion of $\mathcal{N}$.

Now, given a theory T, understood as a collection of sentences in the language closed under the rules of some deductive system, we introduce a model for the theory:

> **Definition** ($\mathcal{L}$-model) For a theory T in the signature of $\mathcal{L}$, the $\mathcal{L}$-structure $\mathcal{M}$ is a model of T iff, for every formula $\varphi \in T$, $\mathcal{M} \models \varphi$.

In order to understand the distinction between intended and unintended models we need, partly, to understand when do two different models capture a theory 'in the same way'. This can be given a precise formulation through the notion of a mapping preserving structure:

> **Definition** (Homomorphism) Let $\mathcal{M}$ and $\mathcal{N}$ be two $\mathcal{L}$ structures. An homomorphism $\pi : \mathcal{M} \to \mathcal{N}$ is a function $\pi : M \to N$ with:
>
> 1. For $\mathcal{L}$-constant symbol c, $c^{\mathcal{N}} = \pi(c^{\mathcal{M}})$;
> 2. For n-ary $\mathcal{L}$-function symbol f, $f^{\mathcal{N}}(\pi(m_1), ..., \pi(m_n)) = \pi(f^{\mathcal{M}}(m_1, ..., m_n))$ (for $m_1, ..., m_n \in M$);
> 3. For n-ary $\mathcal{L}$-relation symbol R, $(m_1, ..., m_n) \in R^{\mathcal{M}} \Rightarrow (\pi(m_1), ..., \pi(m_n)) \in R^{\mathcal{N}}$ (for $m_1, ..., m_n \in M$).

Definition (Embedding) An embedding of $\mathcal{M}$ into $\mathcal{N}$ is an homomorphism $\pi : \mathcal{M} \to \mathcal{N}$ such that:

1. π is injective;
2. For n-ary $\mathcal{L}$-relation symbol R, $(\pi(m_1), ..., \pi(m_n)) \in R^{\mathcal{N}} \Rightarrow (m_1, ..., m_n) \in R^{\mathcal{M}}$ (for $m_1, ..., m_n \in M$).

Definition We say two $\mathcal{L}$-structures $\mathcal{M}$ and $\mathcal{N}$ are isomorphic iff there is a surjective embedding $\pi : \mathcal{M} \to \mathcal{N}$. We write $\mathcal{M} \cong \mathcal{N}$.

Finally, the following theorem captures the relation between a model's structure and satisfiabilty:

Theorem Let $\mathcal{M}$ and $\mathcal{N}$ be two $\mathcal{L}$ structures and $\pi : M \to N$ a bijection. Then the following are equivalent:

1. π is a an isomorphism between $\mathcal{M}$ and $\mathcal{N}$;
2. $\mathcal{M} \models \varphi(m_1, ...m_n) \Leftrightarrow \mathcal{N} \models \varphi(\pi(m_1), ..., \pi(m_n))$, for $m_1, ..., m_n \in M$ and atomic $\mathcal{L}$-formula φ;
3. $\mathcal{M} \models \varphi(m_1, ...m_n) \Leftrightarrow \mathcal{N} \models \varphi(\pi(m_1), ..., \pi(m_n))$, for $m_1, ..., m_n \in M$ and first-order $\mathcal{L}$-formula φ;
4. $\mathcal{M} \models \varphi(m_1, ...m_n) \Leftrightarrow \mathcal{N} \models \varphi(\pi(m_1), ..., \pi(m_n))$, for $m_1, ..., m_n \in M$ and second-order $\mathcal{L}$-formula φ with semantic consequence defined via either full or Henkin second-order semantics.

Definition (Elementary Equivalence) We say two $\mathcal{L}$-structures $\mathcal{M}$ and $\mathcal{N}$ are elementarily equivalent iff they satisfy the same $\mathcal{L}$-formulas. We write $\mathcal{M} \equiv \mathcal{N}$.

Definition (Elementary Extension) For two $\mathcal{L}$-structures $\mathcal{M}$ and $\mathcal{N}$ with $N \subseteq M$ we say that $\mathcal{M}$ is an elementary extension of $\mathcal{N}$, and write $\mathcal{N} \prec \mathcal{M}$, iff for any elements $n_1, ..., n_n \in N$ and formula $\varphi(x_1, ..., x_n)$, $\mathcal{N} \models \varphi(n_1, ..., n_n)$ iff $\mathcal{M} \models \varphi(n_1, ..., n_n)$. In this case we also say that $\mathcal{N}$ is an elementary substructure of $\mathcal{M}$.

Theorem $$\mathcal{M} \cong \mathcal{N} \Rightarrow \mathcal{M} \equiv \mathcal{N}$$

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