SECOND-ORDER LOGIC: ONTOLOGICAL AND EPISTEMOLOGICAL PROBLEMS

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Second-Order Logic:

Ontological and Epistemological Problems

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Für meine Eltern, Irene und Wolfgang Rossberg,

meine Großmütter, Hedwig Grosche und Ilse Rossberg,

und gewidmet dem Andenken an meinen Großvater

Paul Rossberg

(1920 – 2004)

For my parents, Irene and Wolfgang Rossberg,

my grandmothers, Hedwig Grosche and Ilse Rossberg,

and dedictated to the memory of my grandfather

Paul Rossberg

(1920 – 2004)

Abstract

In this thesis I provide a survey over different approaches to second-order logic and its interpretation, and introduce a novel approach. Of special interest are the questions whether (a particular form of) second-order logic can count as logic in some (further to be specified) proper sense of logic, and what epistemic status it occupies. More specifically, second-order logic is sometimes taken to be mathematical, a mere notational variant of some fragment of set theory. If this is the case, it might be argued that it does not have the "epistemic innocence" which would be needed for, e.g., foundational programmes in (the philosophy of) mathematics for which second-order logic is sometimes used. I suggest a *Deductivist* conception of logic, that characterises logical consequence by means of inference rules, and argue that on this conception second-order logic should count as logic in the proper sense.

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This thesis is for them.

Notational Conventions

For the benefit of a consistent use of the logical symbols I have tacitly changed the symbols other authors use into the ones preferred here. Boolos' '$\rightarrow$' and '$\leftrightarrow$', for example, are replaced by '$\supset$' and '$\equiv$', respectively. Likewise, Quine's '(x)', for example, is replaced by '$\forall x$', and his notation using '**.**', '**:**', '**.:**', etc., as both symbols for conjunction and for scope distinctions is abandoned in favour of the nowadays more common use of '$\wedge$' (for conjunction) and the use of parentheses (for scope distincitons).

I use single quotations marks for mentioning an expression, and double quotations marks where the quoted expression is used; the latter case almost exclusively occurs for verbatim quotations from the literature and the use of a word in a non-literal sense or in a sense that I explicitly do not agree with. It should in all cases be clear from the context in which of these senses I uses the double quotation marks. I tacitly changed the quotations marks in verbatim quotations from the literature to accord to this rule where authors followed different conventions. (Quine and Boolos, for example, sometimes use double quotation marks for mentioning expressions.)

Where longer passages are quoted verbatim from the literature, the quotation is separated from the main text by an extra wide margin, and quotation marks are omitted.

Corner quotes, '$\ulcorner$' and '$\urcorner$', are used as devices for quasi-quotation, as introduced in (Quine, 1955), §6.

Contents

Chapter 1

Introduction

1.1 The Project

The argument of this thesis is for the claim that second-order logic is *proper* logic.

This is not merely a question of handing out honorifics, more or less arbitrarily, but a substantial claim about the nature of second-order logic, properly construed. Proper logic plays a crucial role in codifying correct inference. While there are, of course, various species of correct inferences, including mathematical ways of reasoning, the epistemic advantage of a proper logic is that the conclusion of an argument, that is valid in the proper logical way, is *ideally justified* on basis of its premises. This concept was introduced by Stephen Wagner in his (Wagner, 1987). Ideal justification is Wagner's way of spelling out a thought contained already in Gottlob Frege's writings that logic is not supposed to add anything to the inference. Everything that is needed to justify a claim is already contained in the premises; only then is an inference properly logical.

Wagner argues, however, that second-order logic does not satisfy this constraint, and thus cannot count as a proper logic. I will argue against this claim, and also against other allegations against second-order logic that have been put forward. Most commonly it is believed that second-order logic is really a mathematical theory, very much akin to set theory. This claim goes back to W.V. Quine's quip that second-order logic is "set theory in sheep's clothing".[1] The problem with this is not that set theory is *false*, but that it is a strong mathematical theory, and not logic. Set theory makes enough substantial mathematical claims that virtually all areas of mathematics can be represented in it. Ideal justification is not in general possible with such a system, since these substantial mathematical claims go into the argument from premises to conclusion, and we cannot in general be sure that the conclusion is indeed solely justified in basis of the premises, or if it partly rests on some of these substantial mathematical presupposition.

Since mathematical truths are, presumably, necessarily true, if true at all, why would that matter? It does not always matter. Mathematics is applicable in the sciences, for example, and presumably does not cause any problems there. Indeed, many think it is indispensable. There are areas, on the other hand, where no mathematics should be presupposed. The philosophy of mathematics is one such area, at least as construed by some research projects in this area. Hartry Field's nominalist programme proposed in his *Science Without Numbers*[2], for example, argues against the claim that mathematics is indispensable to science. His case study is Newtonian Mechanics, for which he produces a formal system that does not contain any mathematics. Formal systems are built on a formal logic, however, and if the alleged logic that is used would itself really be a mathematical theory, the

[1](Quine, 1970), p. 66.
[2](Field, 1980).

argument would break down. Field indeed considers the advantages a second-order formulation of his system would have, but opts for the first-order version – with the Quinean quip in mind.

Another research programme that should not presuppose any mathematics is logicism. In Frege's flawed masterpiece, the *Grundgesetze der Arithmetik*[3] [*Basic Laws of Arithmetic*], he uses the logic he developed earlier in his *Begriffsschrift*[4] to derive the axioms of arithmetic. If the logic he uses for this is inclusive of mathematics, this logicist reduction of arithmetic to this logic would not be of the epistemic merit that Frege envisioned for it. Logic is epistemically safe; to show that arithmetic is so too, a reduction of the axioms of arithmetic to basic logical truths and carried out by purely logical means would suffice. The system that Frege for the first time in the history of logic introduces in the *Begriffsschrift* is polyadic predicate logic, essentially the logic that we use today, *modulo* Frege's unusual and perhaps awkward seeming notation. It is also second-order.

Alas, Frege's "logical system" of the *Grundgesetze* included one additional "logical law" that is not present in the *Begriffsschrift*: the infamous Basic Law V. Bertrand Russell showed just before the publication of Frege's second volume of the *Grundgesetze*, that Frege's logic was not only inclusive of mathematics, but in fact inclusive of everything. A contradication, known as Russell's Paradox, is derivable from Basic Law V. Frege briefly attempted to fix the problem, but gave up quite quickly. The planned third volume of the *Grundgesetze*, for which the logicist foundation for real, and perhaps complex analysis was planned, never appeared.

It has more recently been observed that Frege only uses Basic Law V to derive one

[3](Frege, 1893) (Frege, 1903).
[4](Frege, 1879).

other principle, that already figures in his earlier philosophical outline of his logicist reduction, the *Grundlagen der Arithmetik*[5] [The Foundations of Arithmetic]: so-called "Hume's Principle". Crispin Wright shows in his (Wright, 1983) that Frege's project can indeed be carried out, if Hume's Principle is assumed. The Peano-Dedekind axioms of arithmetic are derivable from it. Thus a neo-logicist research programme of *Neo-Fregeanism* took wing.[6] The project is sometimes also called 'Abstractionism'; Hume's Principle, and other principles of the same form are called *abstraction princples*. These are conceived as implicit definitions that introduce on their left-hand side of a bi-conditional a mathematical concept, like 'natural number' in the case of Hume's Principle, and express on their right hand side an equivalence relation in purely logical terms, a bijection between the extension of two predicates in the case of Hume's Principle. The logic that Neo-Fregeanism uses is second-order logic, as it was for Frege. If second-order logic is indeed set theory, the project loses almost all its interest: a reduction of arithmetic and analysis to set theory is no news. In particular, it would not show that arithmetic inherits its epistemic status from Hume's Principle (which is arguably analytic). The ideal justification sought for the axioms of arithmetic, and with them all arithmetical truths, requires that the logic used is *proper* logic, and not set theory in disguise.

There are more projects in the philsophy of mathematics that also use second-order logic. Geoffrey Hellman's modal structuralism,[7] for example, is formulated in second-order logic with modal operators. Given the nominalist character that Hellman wants his approach to have, second-order logic had better not be mathematics

[5](Frege, 1884).

[6]See (Wright, 1983), (Hale, 1987), (Hale and Wright, 2001); see also (MacBride, 2003) for an excellent survey and critical discussion of the Neo-Fregean project.

[7](Hellman, 1989).

if the project is to have any chance to succeed. Stewart Shapiro's structuralism is also dependent on second-order logic.[8] As we will see, however, he does not believe that a sharp line between mathematics and logic can be drawn, and that the case of second-order logic especially shows this. My thesis, thus, not only has to argue against the enemies of second-order logic, but against at least some of its proponents, too.

1.2 Outline of the Thesis

After a short introduction to the formal system of second-order logic in chapter 2, my argument for the logicality of second-order logic begins, in chapter 3, with investigating Quine's reasons to claim that second-order logic is set theory. It will turn out that Quine argues from ontology. The second-order quantifiers have to have a range, and as they are quantifying into predicate position, they have to range over some kind of universals. The best case for second-order logic, therefore, is according to Quine that they range over sets, as these are the only universals (he sees sets as such) that he can accept.

In chapter 4 I discuss George Boolos' attempt to rebut the Quinean argument by providing a plural interpretation of the second-order quantifiers. Monadic second-order quantifiers can be interpreted as just ranging over the first-order domain, and thus not introducing any new ontology, if they are construed as quantifying plurally. Boolos provides a translation of the monadic second-order existential quantifier as 'there are some thing*s*', analogous to the first-order quantifier, 'there is something'. Boolos' claim is that this way, no new ontological commitment arises.

[8](Shapiro, 1997)

Since according to my criticism of Boolos' attempt to justify the second-order quantifiers in this way – and of the attempt of his followers to patch up his account where it is found wanting – leads me to reject this way of arguing against Quine, I analyse in chapter 5 Quine's criterion of ontological commitment in detail. The criterion is found deficient, even for the paradigm cases of formalised first-order theories that are the golden standard of all scientific and philosophical enterprise for Quine. I suggest a natural modification and precisification of Quine's criterion to make it indeed applicable to all first-order theories. The resulting new criterion, however, suggests that the introduction of second-order quantifiers does not bring about any ontological commitment that was not already contained in the first-order theory; in particular, no new commitment to sets arises.

The deductive system of second-order logic is incomplete with respect to its standard semantics, in the sense that there are conclusions that are declared to follow from some premises by the standard model-theoretic semantics, that are not deducible from them in the deductive system, and indeed not in *any* sound deductive system for this semantics. This is a corollary of Gödel's incompleteness theorem for arithmetic. On the grounds of the lack of a complete proof procedure for the semantical consequence relation, second-order logic is often denied the rank of a proper logic. Surely, so the claim goes, every proper logic must be complete. I argue in chapter 6 that there is no good reason to think so.

Chapter 7 deals with one of the main proponents of second-order logic today, Stewart Shapiro. In his (Shapiro, 1991) he makes a case for second-order logic on the basis of the claim that accepting second-order logic is the only way to make sense of mathematical practice. His argument is, roughly, that mathematical practice requires the anyway plausible claim that mathematicians can speak about specific infinite structures, like the structure of the natural numbers or the structure of the real numbers, and discern them. To do justice to this fact, Shapiro claims, one has to consider categorical axiom systems for the mathematical theories that are about these structures. A categorical axiom system is defined as one that has, up to isomorphism, only one interpretation: all its models are isomorphic to each other. It can be shown that no first-order axiomatisation of a theory over an infinite domain can be categorical. The resources of second-order logic with its standard (but not a Henkin) semantics allow us, however, to give categorical axiom systems of arithmetic and real analysis, for example.

Critics of Shapiro argue that the standard model theory of second-order logic makes strong mathematical presuppositions. While this presumably shows that second-order logic can be no proper logic, if the model-theoretic system is identified with second-order logic, this does not constitute an argument against Shapiro. He rejects, precisely on these grounds the sharp distinction between logic and mathematics. His critics further argue that the categoricity results do not show the determinacy that Shapiro requires for his argument, and some also suggest that a second-order standard model-theoretic treatment hampers mathematical practice rather than doing justice to it.

In chapter 8, finally, I introduce what I call the Deductivist Conception of Logic. The model-theoretic approach to account for logical consequence is rejected on the grounds that model theory is mathematical. As already argued above, however, a proper logic must not presuppose any substantial, mathematical content since it otherwise cannot fulfill his purpose to facilitate ideal justification. The Deductivist proposal is to characterise logical consequence by purely deductive means. I argue that on such a construal none of the objections against second-order logic discussed up until that point apply. Moreover, it appears that on a Deductivist conception second-order quantification is sufficiently similar to and indeed of a piece, in a sense, with first-order quantification that the former should count as properly logical if the latter does.

My conclusion is, as already suggested in the first sentence of this introduction, that second-order logic is proper logic, if it is construed in a Deductivist way. The remainder of chapter 8 discusses further objections against second-order logic that appear to be particularly pressing for the a Deductivist approach: an apparent inherent incompleteness of second-order logic, that is not relative to some some model-theoretic semantics, and the impredicativity of the second-order quantifiers.

An appendix discusses Etchemendy's arguments concerning the concept of logical consequence. Since the negative part of his project in which he criticises the Tarskian reductive analysis of logical consequence is not unlike mine in spirit and character, it seemed justified to include this appendix that discusses Etchemendy's arguments in some detail. Moreover, despite sharply critising the "interpretational" model-theoretic semantics, Etchemendy come to a conclusion that is diametrically opposed to the Deductivist account. He argues for what he calls "representational semantics" which still uses (more or less) the standard model theory. A comparison of the two approaches concludes the thesis.

Chapter 2

The Formal System of Second-Order Logic

2.1 Preliminaries

This chapter introduces the formal system of classical second-order predicate logic. Unless stated otherwise, the presentation follows chapters 3 and 4 of (Shapiro, 1991).

Standard systems of second-order logic can also be found in (Church, 1956) or (Mendelson, 1997).[1] A tree (or tableaux) system for second-order logic is introduced in (Jeffrey, 1967); (Bell et al., 2001) also contains such a system.

2.2 Language

The language of a standard first-order logic is presupposed; see, for example, (Mendelson, 1997) or (Boolos and Jeffrey, 1985). Only the material conditional '$\supset$', negation

[1]Note that only the 4th edition of Mendelson's book contains a presentation of second-order logic.

'$\neg$', and the universal quantifier '$\forall$' are taken as primitive, the other logical constants are defined in the usual way. The first-order existential quantifier, e.g., is defined as

$$\exists x\Phi =_{\mathrm{df}} \neg\forall x\neg\Phi$$

where 'Φ' is a schematic letter standing for an arbitrary formula of the system.

For the language of second-order logic we introduce second-order variables: n-place predicate variables, 'X^n', 'X_1^n', 'X_2^n', 'Y^n', 'Y_1^n', ..., that can stand in place of n-place predicate letters, and n-place function variables, 'f^n', 'f_1^n,''f_2^n', ..., that can stand in the place of n-place function letters. The superscript indicates the number n of argument places. Thus, 'X_4^1' is a one-place predicate variable, 'f_1^3' is a three-place function variable. In the following the superscripts indicating the number of argument places will usually be omitted. Counting the terms that follow the variable will disambiguate.

The language also contains second-order universal quantifiers that are formed by attaching the '$\forall$' to second-order variables: '$\forall X$', '$\forall f$', etc. The second-order existential quantifiers are defined as:

$$\exists X\Phi =_{\mathrm{df}} \neg\forall X\neg\Phi$$
$$\exists f\Phi =_{\mathrm{df}} \neg\forall f\neg\Phi$$

'$=$' is not taken as primitive but defined:

$$x = y =_{\mathrm{df}} \forall X(Xx \equiv Xy)$$

We can also define:

$$x \neq y =_{\mathrm{df}} \neg x = y$$
$$\exists! x\Phi(x) =_{\mathrm{df}} \exists x\forall y(\Phi(y) \equiv x = y)$$

The recursive formation rules that are added to those for the language of first-order logic are:

If 'f' is an n-place function variable and $\ulcorner\langle x\rangle_n\urcorner$ is a sequence of n terms, then $\ulcorner f\langle x\rangle_n\urcorner$ is a term.

If 'R' is an n-place predicate variable and $\ulcorner\langle x\rangle_n\urcorner$ is a sequence of n terms, then $\ulcorner R\langle x\rangle_n\urcorner$ is an atomic formula.

If 'f' is a function variable and Φ is a formula, then $\ulcorner\forall f(\Phi)\urcorner$ is a formula.

If 'R' is a predicate variable and Φ is a formula, then $\ulcorner\forall R(\Phi)\urcorner$ is a formula.

Convention: The parenthesis that inclose the formula and indicate the scope of the quantifier can be omitted in cases where there is no scope-ambiguity.

2.3 Deductive Systems

2.3.1 Axiomatic System

Let us define a standard *axiomatic* system for second-order logic first.

Let Γ be a set of sentences of the language and Φ a single sentence of the language. Define a *deduction of* Φ *from* Γ to be a finite sequence $\Phi_1, ..., \Phi_n$ such that Φ_n is Φ and, for each $i \leqslant n$, Φ_i is an axiom (see below), or Φ_i follows from previous sentences in the sequence by one of the rules of inference (see below). We can symbolise that there is a deduction of Φ from Γ as: $\Gamma \vdash \Phi$.

Let a *proof* of Φ be a deduction of Φ from the empty set. Call Φ a *theorem* if there is a proof for Φ; we can also write: $\vdash \Phi$.

The following are *axiom schemata*. Any formula obtained by substituting formulas for the schematic letters 'Φ', 'Ψ', and 'Ξ', is an *axiom* of the system.

$\Phi \supset (\Psi \supset \Phi)$

$(\Phi \supset (\Psi \supset \Xi)) \supset ((\Phi \supset \Psi) \supset (\Phi \supset \Xi))$

$(\neg\Phi \supset \neg\Psi) \supset (\Psi \supset \Phi)$

$\forall x \Phi(x) \supset \Phi(t)$

where 't' is a term free for 'x' in Φ

$\forall X^n \Phi(X^n) \supset \Phi(T)$

where 'T' is an n-place predicate letter free for 'X^n' in Φ

$\forall f^n \Phi(f^n) \supset \Phi(p)$

where 'p' is an n-place function letter free for 'f^n' in Φ

A term 't' is *free* for 'x' in Φ if no variable has an occurrence that is both free in 't' and bound in $\ulcorner \Phi(t) \urcorner$; analogously for predicate and function letters.

The rules of inference of the system are:

Modus ponens:

from Φ and $\ulcorner \Phi \supset \Psi \urcorner$ infer Ψ

Generalisation:

from $\ulcorner \Phi \supset \Psi(t) \urcorner$ infer $\ulcorner \Phi \supset \forall x \Psi(x) \urcorner$

provided 't' does not occur free in Φ or in any of the premises of the deduction

from $\ulcorner \Phi \supset \Psi(T) \urcorner$ infer $\ulcorner \Phi \supset \forall X \Psi(X) \urcorner$

provided 'T' does not occur free in Φ or in any of the premises of the deduction from $\ulcorner \Phi \supset \Psi(p) \urcorner$ infer $\ulcorner \Phi \supset \forall f \Psi(f) \urcorner$

provided 'f' does not occur free in Φ or in any of the premises of the deduction

The usual axioms and rules for the other sentential connectives and the existential quantifier, which are defined here and not taken primitive, can be derived from the rules and axioms given above.

We add to the system an *axiom schema of comprehension*:

$$\exists X^n \forall \langle x \rangle_n (X^n \langle x \rangle_n \equiv \Phi \langle x \rangle_n)$$

provided 'X^n' does not occur free in Φ; $\ulcorner \langle x \rangle_n \urcorner$ is a sequence of n first-order variables; $\ulcorner \forall \langle x \rangle_n \urcorner$ abbreviates a sequence of n quantifiers $\ulcorner \forall x_i \urcorner$, for $1 \leqslant i \leqslant n$.

The comprehension schema asserts that every open sentence of the language there a (possibly many-place) predicate with the same extension. If Φ contains no bound second-order variables, we call the corresponding instance of the comprehension schema *predicative*, and *impredicative* otherwise.

We also add an *axiom of comprehension for functions*:

$$\exists X^{n+1} (\forall \langle x \rangle_n \exists ! y X^{n+1} \langle x \rangle_n y \supset \exists f^n \forall \langle x \rangle_n X^{n+1} \langle x \rangle_n f \langle x \rangle_n)$$

Shapiro suggests to add instead of the comprehension for functions a stronger principle, an axiom of choice. He writes:

> The axiom of choice has a long and troubled history [...], but it is now essential to most branches of mathematics. In fact, a corresponding meta-theoretic principle is necessary for many of the theorems reported [in (Shapiro, 1991)]. Mathematical logic also thrives on the axiom of choice.[2]

[2](Shapiro, 1991), p. 67.

The *axiom of choice* is:

$$\exists X^{n+1}(\forall\langle x\rangle_n\exists yX^{n+1}\langle x\rangle_n y \supset \exists f^n\forall\langle x\rangle_n X^{n+1}\langle x\rangle_n f\langle x\rangle_n)$$

Note, that it does not have the uniqueness condition attached to the first existential quantifier that the axiom of comprehension for functions has. The antecedent of the axiom of choice asserts that for each sequence $\ulcorner\langle x\rangle_n\urcorner$ there is at least (*exactly*, for the weaker comprehension for function above) one 'y' such that the sequence $\ulcorner\langle x\rangle_n y\urcorner$ satisfies $\ulcorner X^{n+1}\urcorner$. The consequent asserts the existence of a function that "picks out" one such 'y' for each $\ulcorner\langle x\rangle_n\urcorner$.

The axiom of choice, which is often considered problematic, cannot be discussed here.

2.3.2 Natural Deduction

Equivalently, we can give a natural deduction system for second-order logic. (Shapiro, 1991) does not contain such a system; for a system similar to the one introduced here, see (Prawitz, 1965).

The classical introduction- and elimination-rules for the propositional fragment are presupposed (see for example (Prawitz, 1965)).

Observe, that function letters are dispensable. This is also the case for the axiomatic system, of course, but as I will, for simplicity's sake, use quantification into function-letter-position in some of the chapters below, it seemed advisable to introduce them into the language and axiomatic system. $(n+1)$-place predicate variables can serve as surrogates for n-place function variables, however. The clause $\ulcorner\forall\langle x\rangle_n\exists!yF^{n+1}\langle x\rangle_n y\urcorner$ indicates that $\ulcorner F^{n+1}\urcorner$ is in effect an n-place function.

For the natural deduction system we dispense with function letters, variables and quantifiers. The introduction (I) and elimination (E) rules for the first- ($\forall^1$) and second-order ($\forall^2$) universal quantifiers are:

$$\frac{\Phi(t)}{\forall x \Phi(x)}\ \forall^1\text{-I} \qquad\qquad \frac{\Phi(T)}{\forall X^n \Phi(X^n)}\ \forall^2\text{-I}$$

$$\frac{\forall x \Phi(x)}{\Phi(t)}\ \forall^1\text{-E} \qquad\qquad \frac{\forall X^n \Phi(X^n)}{\Phi(\Xi)}\ \forall^2\text{-E}$$

Φ is an open sentence matching the number of argument places of the expression it applies to, in the case of the second-order rules possibly just one term; 't' is a term of the language.

Restrictions:

$\forall^1$-I 't' does not occur free in any of the assumptions that $\ulcorner\Phi(t)\urcorner$ depends on.

$\forall^1$-E 't' is free for 'x' in Φ.

$\forall^2$-I 'T' is a n-place predicate letter and does not free in any of the assumptions that $\ulcorner\Phi(T)\urcorner$ depends on.

$\forall^2$-E Ξ is an open sentence with n argument places; no variable in Ξ is bound in $\Phi(\Xi)$ that are not already bound in Ξ.

2.4 Semantics

2.4.1 Semantics for First-Order Logic

The model-theoretic semantics for the first-order logic fragment of second-order logic is presupposed, and merely sketched here.

A model is an order pair $M = \langle d, I \rangle$, in which d is the *domain* of the model, a non-empty set, and I is an *interpretation function* that assigns objects in d and sets that are constructed from objects in d to the non-logical vocabulary of the language. If 'a' is a term of the language, for example, I('a') is a member of d; if 'R' is a two-place predicate letter, I('R') is a subset of $d \times d$ ('$\times$' standing for the Cartesian product). A *variable assignment* s is a function from the variables of the language to d.

For each model and variable assignment there is a *denotation* function that assigns an object in d to every term of the language. *Satisfaction* is defined in the usual way as a relation that holds between models, variable assignments, and formulae. Let us write '$M, s \vDash \Phi$' for 'M and s satisfy Φ'. If $M, s \vDash \Phi$ for every variable assignment s, we say that M is a *model* of Φ. If s and s' are two variable assignments that agree on all free variables of Φ, then $M, s \vDash \Phi$ if, and only if, $M, s' \vDash \Phi$. Since if Φ is a sentences, i.e. a formula with no free variables, the variable assignment makes no difference, we can just write $M \vDash \Phi$.

A formula Φ is *satisfiable* if, and only if, there is a model M and a variable assignment s on M such that $M, s \vDash \Phi$. A set of formulae Γ is satisfiable if, and only if, there is a model M and a variable assignment s on M such that $M, s \vDash \Phi$ for every $\Phi \in \Gamma$. A formula Φ is a *semantic consequence* of a set of formulae Γ, if the set that contains $\ulcorner \neg\Phi \urcorner$ and all the sentences contained in Γ is not satisfiable. We can also say that Φ is a semantic consequence of Γ if, and only if, for every model M and any variable assignment s on M, if $M, s \vDash \Psi$ for any $\Psi \in \Gamma$, then $M, s \vDash \Phi$. We can also write this as '$\Gamma \vDash \Phi$'.

A formula Φ is *valid* if, and only if, $M, s \vDash \Phi$ for all M and all s on M. We can also write this as '$\vDash \Phi$'.

2.4.2 Standard Semantics for Second-Order Logic

We can build a *standard semantics* for second-order logic on this basis. A *standard model* is still an ordered pair $\langle d, I\rangle$, as in first-order logic. A *variable assignment* is a function that assigns a member of d to each first-order variable, a subset of d^n to every n-place predicate variable, and a function from d^n to d to each n-place function variable. (d^n is d for $n = 1$, $d \times d$ for $n = 2$, $d \times d \times d$ for $n = 3$, etc.) The range of the one-place predicate letters is thus the *powerset* of the domain d; generally the powerset of d^n is the range of the n-place predicate letters.

The new clause for the *denotation function* which is to be added to those for first-order logic is:

> Let $M = \langle d, I\rangle$ be a model and s be a variable assignment on M. The denotation of $f^n\langle t\rangle_n$ in M, s is the value of the function $s(f^n)$ at the sequence of members of d denoted by $\langle t\rangle_n$.

The relation of *satisfaction* is also extended from first-order logic. The three new clauses are:

> If X^n is an n-place predicate variable and $\langle t\rangle_n$ is a sequence of n terms, then $M, s \vDash X^n\langle t\rangle_n$ if, and only if, the sequence of members of d denoted by $\langle t\rangle_n$ is a member of $s(X^n)$.

$M, s \vDash \forall X \Phi$ if, and only if, $M, s' \vDash \forall X \Phi$ for every s' on M that agrees with s at every variable except maximally X.

$M, s \vDash \forall f \Phi$ if, and only if, $M, s' \vDash \forall X \Phi$ for every s' on M that agrees with s at every variable except maximally f.

The definitions of satisfiability, semantic consequence and validity remain the way they are introduced for first-order logic.

2.4.3 Henkin Semantics

In a Henkin semantics (introduced by Leon Henkin in (Henkin, 1950)) it is not assumed that the n-place predicate variables range of the full powerset of d^n, but a separate domain is specified for them in each model, and also for the function variables. A *Henkin model* is a quadruple $M^H = \langle d, D, F, I \rangle$ in which d is the domain and I an interpretation function as above. D is a sequence of non-empty sets $D(n)$ that contain subsets of d^n for every n, to be assigned to the n-place predicate variables as we will see below. Likewise, F is a sequence of non-empty sets $F(n)$ of functions from d^n to d. Intuitively, the range of the one-place predicate variables, for example, is a fixed subset of the powerset of d for each model.

A *variable assignment* is a function that assigns a member of d to each first-order variable, a member of $D(n)$ to each n-place predicate variable, and a member of $F(n)$ to each n-place function variable. The remaining features of a Henkin semantics are analogous to those of the standard semantics.

The four new clauses, to be added to the semantics of first-order logic, are:

Let $M^H = \langle d, D, F, I \rangle$ be a Henkin model and s a variable assignment on M^H.

The denotation of $f^n \langle t \rangle_n$ in M^H, s is the value of the function $s(f^n)$ at the sequence of members of d denoted by $\langle t\rangle_n$.

If X^n is an n-place predicate variable and $\langle t\rangle_n$ is a sequence of n terms, then $M^H, s \vDash X^n\langle t\rangle_n$ if, and only if, the sequence of members of d denoted by $\langle t\rangle_n$ is a member of $s(X^n)$.

$M^H, s \vDash \forall X\Phi$ if, and only if, $M^H, s' \vDash \forall X\Phi$ for every s' on M^H that agrees with s at every variable except maximally X.

$M^H, s \vDash \forall f\Phi$ if, and only if, $M^H, s' \vDash \forall X\Phi$ for every s' on M^H that agrees with s at every variable except maximally f.

Again, the definitions of satisfiability, semantic consequence and validity are analogous to those introduced for first-order logic, only that they are with respect to Henkin models.

2.5 Some Meta-Theoretic Results

In this section the meta-theoretic results concerning first- and second-order logic that will be of interest for the philosophical discussion in the following chapters are stated. The results are discussed where they are mentioned in the later chapters. Their proofs are omitted. For first-order logic, they can be found, for example, in (Mendelson, 1997) or (Boolos and Jeffrey, 1985); for second-order logic, the proofs are sketched in (Shapiro, 1991).

Let us start with the relation between standard and Henkin semantics.

2.5.1 Standard and Henkin Semantics

A Henkin model in which all the $D(n)$ are the full powerset of d^n, and all the $F(n)$ the sets of all n-place functions from d^n to d is obviously equivalent to a standard model. Thus, if we restrict the range of Henkin model to such models, this restricted Henkin semantics will be equivalent to the standard semantics. It hence follows that:

> If Φ is valid according to Henkin semantics, then Φ is valid according to the standard semantics.
>
> If Φ is a semantic consequence of Γ according to Henkin semantics, then Φ is a semantic consequence of Γ according to the standard semantics.
>
> If Φ is satisfiable according to the standard semantics, then Φ is satisfiable according to Henkin semantics.

The converse does not hold in any of the cases.

2.5.2 First-Order Logic

The soundness and completeness theorems for first-order logic are well know. They are:

Soundness: Let Γ be a set of formulae and Φ a formula of the first-order language. If $\Phi \vdash \Gamma$ then $\Phi \vDash \Gamma$. *A fortiori*, if $\vdash \Gamma$ then $\vDash \Gamma$.

Completeness: Let Γ be a set of formulae and Φ a formula of the first-order language. If $\Phi \vDash \Gamma$ then $\Phi \vDash \Gamma$. *A fortiori*, if $\vDash \Gamma$ then $\vdash \Gamma$.

As Shapiro notes, the soundness proof

is straightforward. One checks each axiom and rule of inference. Virtually no substantial set-theoretical assumptions are needed. [...] [T]he completeness of first-order logic depends on a principle of infinity (in the metalanguage). If the model-theoretic semantics had no models with infinite domains, the completeness theorem would be false.[3]

Another standard meta-theoretical results is compactness:

Compactness: Let Γ be a set of formulae of the first-order language. If every finite subset of Γ is satisfiable, then Γ is satisfiable.

It follows that if an infinite set of first-order formulae is non satisfiable, it has a finite subset that is not satisfiable. The compactness theorem is a direct corollary of soundness and completeness. For the remaining two standard meta-theorems we should introduce more technical terminology. Let $M = \langle d, I\rangle$ and $M' = \langle d', I'\rangle$ be two models. We define that M' to be a *submodel* of M if, and only if, d' is a subset of d, I and I' give the same denotation to each individual constant, and the interpretation of each predicate and function letter under I' is the restriction to d' of the corresponding interpretation under I. If the theory contains function constants, then d' must be closed under these functions.

Löwenheim-Skolem theorem: If M is a model of a set Γ of first-order formulae, then M has a submodel M' whose domain is at most countable infinite, such that for each assignment s on M' and each formula Φ in Γ: $M, s \models \Phi$ if, and only if, $M', s \models \Phi$.

The axiom of choice is required in the meta-theory for the proof of the Löwenheim-Skolem theorem.

[3](Shapiro, 1991), p. 79.

Löwenheim-Skolem-Tarski theorem: Let Γ be a set of first-order formulae. If, for each $n \in \omega$, there is a model of Γ whose domain has at least n members, then for any *infinite* cardinal κ, there is a model of Γ whose domain has at least cardinality κ.

This entails that every first-order theory with a countably infinite model, e.g. Peano Arithmetic, has an *uncountable* model, too. By the Löwenheim-Skolem theorem, real analysis which has as the intended uncountable domain the real numbers, has a countable model.

2.5.3 Second-Order Logic with Standard Semantics

One, and only one, of these results for first-order logic carry over to second-order logic with standard semantics:

Soundess: Let Γ be a set of formulae and Φ a formula of the second-order language. If $\Phi \vdash \Gamma$ then $\Phi \vDash \Gamma$ according to the standard semantics. *A fortiori*, if $\vdash \Gamma$ then $\vDash \Gamma$ according to the standard semantics.

The proof involves the assumption that every formula in the meta-theory determines a set, and uses the principle of separation (*Aussonderung*). If we add the axiom of choice to the deductive system (as Shapiro suggests), then we need the axiom of choice in the meta-theory, too.

As mentioned above, first-order theories with countable models also have uncountable models, and first-order theories with uncountable models also have countable models. A way to paraphrase this is that first-order theories cannot determine that their domain has a certain infinite cardinality. Second-order theories, however, can have so-called *categorical* axiomatic systems for infinite structures. An axiomatic system is *categorical* if, and only if, all of its models are isomorphic. The theory of second-order arithmetic, for example, is categorical given these axioms:

$\forall x(sx \neq 0)$	(zero)
$\forall x \forall y(sx = sy \supset x = y)$	(successor)
$\forall X[(X0 \wedge \forall x(Xx \supset Xsx) \supset \forall x Xx]$	(induction)

'0' and 's' are non-logical constants for zero and the successor function, respectively. Addition and multiplication do not have to be mentioned in the axioms, as they can be defined in the second-order theory. Let AR be the conjunction of the three axioms above.

Categoricity of second-order arithmetic: Let $M1 = \langle d_1, I_1 \rangle$ and $M2 = \langle d_2, I_2 \rangle$ be two models of second-order arithmetic (with the axioms mentioned above). For $1 \leqslant i \leqslant 2$ let 0_i be the interpretation of zero in d_i, and let s_i be the interpretation of successor. If $M1 \vDash AR$ and $M2 \vDash AR$, then $M1$ and $M2$ are isomorphic: there is a bijection f, a one-to-one function from d_1 onto d_2, such that $f(0_1) = 0_2$, and for each $a \in d_1$, $f(s_1(a)) = s_2(f(a))$. That is, f preserves the structure of the models.

Since the intended interpretation, the natural numbers, is countably infinite and a model of AR, it follows from categoricity that all models of second-order arithmetic are countably infinite. The analogous result holds for real analysis in its second-order axiomatisation. All of its model are of the cardinality of the continuum, i.e. of the powerset of the natural numbers.

For second-order Zermelo-Fraenkel set theory a similar, but restricted result holds, often called *quasi-categoricity*. Intuitively, the quasi-categoricity of this theory says that if two model both satisfy the its axioms, then they are either isomorphic, or one is isomorphic to an "initial segment" of the other. All models of second-order Zermelo-Fraenkel set theory are isomorphic up to an inaccessible rank (the existence of inaccessible cardinals is independent of this theory). Thus, all models, in a sense, "agree on" the structure below the least inaccessible rank, or, as it is sometimes glossed, any two models are isomorphic up to the least inaccessible rank. The theory that we get from adding the claim that there are *no* inaccessible cardinals to second-order Zermelo-Fraenkel set theory is categorical.

It follows immediately from categoricity that both the **Löwenheim-Skolem** and **Löwenheim-Skolem-Tarski** theorems **fail** for second-order logic with standard semantics. Also **Compactness fails** as is shown to follow from the categoricity of second-order arithmetic in my chapter 7 below.

Moreover, it follows from Gödels incompleteness theorem for arithmetic that there is no sound deductive system that is complete with respect to the standard semantics. Thus, second-order logic with standard semantics is inherently **incomplete**. This is easy to see: Take the Gödel sentence of the second-order axiom system of arithmetic, call it G. By Gödel's proof, G is true in the theory, but not provable in the deductive system. $\ulcorner AR \supset G \urcorner$, then, is not provable either in the deductive system, but it is a validity of the standard semantics of second-order logic as follows from the categoricity result for arithmetic. This, however, holds for *any* deductive system.

2.5.4 Second-Order Logic with Henkin Semantics

Second-order logic with Henkin semantics has the same meta-logical properties as first-order logic, and thus cannot provide categorical axiomatic systems for any theory that has an infinite domain, because of the Löwenheim-Skolem, and Löwenheim-Skolem-Tarski theorems.

The range of Henkin models has to be restricted (in a straightforward way) in order to be able to prove that the meta-theorems hold. Define a Henkin model to be *faithful* to the deductive system of second-order logic if, and only if, it satisfies every instance of the comprehension schema (and the axiom of choice, if this is added).

Soundness: If $\Gamma \vdash \Phi$, then Φ is satisfied by every faithful Henkin model that satisfies every member of Γ. *A fortiori*, if $\vdash \Phi$, then Φ is satisfied by every faithful Henkin model.

Completeness: Let Γ be a set of formulae and Φ a formula. If $M, s \vDash \Phi$ for every faithful Henkin model M and variable assignment s on M that satisfies every member of Γ, then $\Gamma \vdash \Phi$.

Compactness: Let Γ be a set of formulae. If every finite subset of Γ is satisfiable in a faithful Henkin model, then Γ is satisfiable in a faithful Henkin model.

For the Löwenheim-Skolem theorem the notion of a submodel has to be extended. We also have to define a *correspondence function* between a submodel and a model. Intuitively, this function maps the sets that are assigned to the predicates and functions in the submodel to those in the model. Details are omitted here; they can be found in (Shapiro, 1991), pp. 92–94. It suffices to say here that the analogues for the **Löwenheim-Skolem** and **Löwenheim-Skolem-Tarski** theorems **hold** for second-order logic with a Henkin semantics restricted to faithful models.

Chapter 3

On Quine

3.1 Introduction

An intuitive way to think of second-order logic is to add upper case variables figuring in predicate position to the standard first-order logic and allow for the binding of these with the usual existential and universal quantifier. The inferential behaviour of the second-order quantifiers might be taken to be sufficiently analogous to that of the first-order quantifiers. Let us further take for granted that first-order logic is logic proper. Thus, if first-order logic *is* proper logic, and if what is added to it to get second-order logic is not fundamentally different from what we had before, it appears that second-order logic is proper logic, too.

The most famous critic of the claim that second-order logic is proper logic is probably W.V. Quine. He attacked second-order logic vigorously over decades on various grounds. In his writings he ascribed to it an air of incoherence, found it unintelligible, said it was dishonest, and famously claimed that it is “set theory in sheep's clothing".[1] Obviously, Quine cannot hold all these claims together. I will in this chapter reconstruct how rather one of these claims leads to the next, and present Quinean reasons as to why. In presenting different conceptions of what is happening when one quantifies into predicate position, I will argue, Quine ends up with stating that the best case that can be made for second-order logic is taking it to be some sort of class theory (or set theory: Quine uses 'class' and 'set' interchangeably[2]). This, however, attracts a charge akin to intellectual dishonesty. Being a class theory, second-order logic has an ontological commitment to classes, but this commitment is masked in the form of second-order quantification; the ontological commitment is not made explicit. Second-order logic, for Quine, is hence practically a Trojan Horse, and a gigantic one: in the first edition of his *Philosophy of Logic* Quine ascribes to second-order logic a commitment to the "staggering existential assumptions" of set theory.[3]

These claims, how they work together, and how a Quinean argument can be rationally reconstructed is the topic of this chapter. Quine's criterion for ontological commitment plays a major in various parts of the argument. For the purpose of this chapter, this criterion is granted. Chapters 4 and 5, though, discuss this assumption in detail, and the criterion is finally rejected in chapter 5.

Before going into the exposition and discussion of Quine's quarrels, it might be worth mentioning a possible psychological explanation as to why Quine believes that second-order logic is set theory. Quine's way to think about second-order logic is

[1]So the title of a section in (Quine, 1986a), pp. 64–66.

[2]Quine takes the use of the term 'set' rather than 'class' in mathematical circles to be almost entirely a matter of mere fashion. This does not mean, however, that he neglects the distinction between so-called *proper classes* (Quine prefers the term 'ultimate class') and such classes that can themselves be members of other classes. See (Quine, 1969b), p. 3.

[3](Quine, 1970), p. 68.

to conceive of it as a variant of the Simple Theory of Types. A quote from his *Set Theory and its Logic* makes this clear:

> [An] assimilation of set theory to logic is seen also in the terminology used by Hilbert and Ackermann and their followers for the fragmentary theories in which the types leave off after finitely many. Such a theory came to be called the predicate calculus (Church: functional calculus) of nth order [...], where n is how high the types go. Thus the theory of individuals and classes of individuals and relations of individuals was called the second-order predicate calculus, and seen simply as quantification theory with predicate letters admitted to quantifiers. Quantification theory proper came to be called the first-order predicate calculus.
>
> This is a regrettable trend. Along with obscuring the important cleavage between logic and "the theory of types" (meaning set theory with types), it fostered an exaggerated if foggy notion of the difference between the theory of types and "set theory" (meaning set theory without types) – as if the one did not involve outright assumptions of sets the way the other does.[4]

Also, as further evidence for my suggestion, in Quine's earlier papers where he raises his complaints about higher-order quantification, he indeed explicitly mentions Russell's Simple Theory of Types, e.g. in (Quine, 1947), rather than second-order predicate logic which is his target especially in his *Philosophy of Logic* (Quine, 1970). The criticism concerning one or the other shows continuity; only the terminology changes.

[4](Quine, 1969b), p. 258.

While there are interesting structural similarities between the Simple Theory of Types and higher-order logic, which can often usefully be exploited, it is still important to keep them apart. The Theory of Types Quine is concerned with is explicitly designed to be a theory of classes[5], with typed class variables, while higher-order logic allows quantification into predicate position. In particular, variables for many-place predicates (or relation symbols) and their binding with quantifiers do not figure in this type theoretical system. The latter are, however, an important ingredient in higher-order logic. Neglecting this can lead into trouble: many-place predicates will play an interesting and surprising role further down in this chapter, and also in chapters 4 and 5.

Running a Quinean conception of the Theory of Types and higher-order logic together would provide an excellent ground to claim that second-order logic is really a theory of classes, were it not fallacious to do so. The philosophical arguments for the claim that second-order logic is ontologically committed to sets are, of course, independent of these anecdotical remarks about Quine. Bearing in mind that this is Quine's viewpoint, however, sometimes helps understanding why Quine expresses things the way he does, especially in the earlier papers, which sometimes seems a bit awkward to the reader today who is more familiar with a conception of second-order logic that is quite independent of the Theory of Types.

3.2 Incompleteness and Branching Quantifiers

Before discussing the Quinean worries mentioned above, it is worth noting an objection that is almost unrelated to Quine's other complaints against second-order

[5]See his (Quine, 1969b).

logic: the common objection concerning its incompleteness. There are some places in his *Philosophy of Logic* where Quine suggests that the lack of a completeness proof indicates that the border to set theory, i.e. mathematics, has been crossed. Most notably this comes up in his discussion of branching quantifiers.[6] It appears, however, that the only place in Quine's writings where he mentions this worry in connection with second-order logic is in one of his replies in his volume of Schilpp's *Library of Living Philosophers*[7] – and even there the incompleteness of second-order logic is only mentioned in one sentence, and not discussed further. (My chapter 6 below contains a detailed discussion of the incompleteness objection.)

It might be that Quine takes it that what he offers against second-order logic is devastating enough so that there is no need for the additional objection. Another explanation would be that he does not think that the argument from incompleteness is a particularly strong one. Trying to reconstruct Quine's view on this, one notices that he is not particularly fond of the model theoretic approach to logic, as becomes clear from his discussion of it in chapter 4 of his *Philosophy of Logic*.[8] Quine's preferred way to characterise logical truth is substitutional. He defines a logical truth as a substitution instance of a valid logical schema, where a valid logical schema is one that has only true substitution instances.[9] Logical schemata, for Quine, are sentence-like well-formed formulae, constructed according to the syntactical rules of first-order logic. His definition of logical truth obviously is hostage to the identification of the logical vocabulary about which Quine says precious little.

Quine's worry about the model-theoretic approach to logical truth is that it

[6](Quine, 1986a), p. 90–91.
[7](Quine, 1986b), p. 646.
[8](Quine, 1986a), pp. 51–53.
[9](Quine, 1986a), p. 50.

requires a set-theoretical interpretation of the language,[10] and thus affords an ontological commitment to sets that his substitutional account apparently avoids. On grounds of ontological parsimony Quine's substitutional account is preferable to the model-theoretic one.[11] Quine concedes, however, that he does not take his substitutional account to be *wholly* independent of sets. It deals with sentences, and Quine takes sentences to be sets of their tokens. He also claims that sets are necessary for the construction of a syntax for a language, especially in order to be able to talk about arbitrarily long sentences even if some will never be written down.[12] Although Quine is prepared to commit himself to sets in this way, he takes it that the commitment is rather modest compared to the one that the model-theoretic account brings with it. The substitutional account, Quine claims, merely requires finite set theory:

> The way to look upon the retreat [from model theory to the substitutional account], then, is this: it renders the notions of validity and logical truth independent of all but a modest bit of set theory; independent of the higher flights.[13]

Quine does not say this explicitly, but one might take the underlying reasoning to

[10](Quine, 1986a), p. 51. That I am sympathetic to Quine's worries here, albeit for different reasons, will become clear in chapters 7 and 8.

[11](Quine, 1986a), pp. 55–56.

[12]For all of these concessions Quine makes concerning the commitment to sets, however, there seems to be logical space for resistance. Syntax and proof theory are to some, perhaps sufficient, extent available without recourse to sets, as Quine himself argued together with Nelson Goodman in an early joint paper: (Goodman and Quine, 1947). Also other strategies have been proposed.

[13](Quine, 1986a), p. 56. Quine's exposition of this claim makes the detour via the possibility of a Gödel coding and an arithmetisation of syntax. Quine does not give the details, and they need not concern us here either. In any case, he takes number theory "in effect equivalent still to a certain amount of set theory [...], [but] it is a modest part: the theory of finite sets." Quine takes set theory to be all one needs as a foundation of mathematics, as numbers, functions and relations are definable as certain sets of sets. See, for example, (Quine, 1947), p. 79 (reappears as (Quine, 1953), p. 122), (Quine, 1960), §54, or (Quine, 1986a), p. 66.

be that whatever formal language one uses, a certain modest amount of set theory will be needed. Model-theoretic approaches, however, need in addition substantially more set theory. Quine's substitutional account does not need this additional bit and is therefore to be preferred.

In section 3.4 below I will reconstruct and discuss Quine's argument for the claim that second-order logic is committed to the "staggering ontology" of set theory. If the interpretation I give of Quine's argument is right, set theory is not available in bits as far as Quinean ontological commitment is concerned. If Quine, therefore, wanted to uphold the claim that second-order logic is committed to the whole of the set theoretical hierarchy, then he cannot uphold his claim that the substitutional account fares better than the model-theoretic one on counts of parsimony: commitment to some sets always means commitment to the entire hierarchy.

Be that as it may, the standard completeness proof shows that a given syntactic system captures all the model-theoretic validities, and declares them to be theorems of the system – the logical truths in the case of a system of logic.[14] If, however, model theory is not assigned a special role, the significance of such a proof is doubtful.[15] Not only does Quine not assign any particularly special role to model theory, he is also rather wary of it because of its use of set theory, as mentioned above. The lack of a completeness proof for second-order logic is therefore something that Quine is in no position to put forward as a strong argument.

Indeed, also in his discussion of branching quantifiers the incompleteness objection appears as a kind of add-on to his main criticism. Quine observes that a

[14]At least this is the part of it that Quine focuses on. The standard completeness proof shows not only that the valid sentences are captured, but shows this generally for the consequences relation. Logical truths are merely a special case of this: consequences of an empty set of premises.

[15]See my chapter 6.

sentence like:

$$\begin{array}{l} \forall x \exists y \\ \forall z \exists w \end{array} F(x, y, z, w)$$

is not equivalent to any of its first-order linear versions like:

$$\forall x \exists y \forall z \exists w\, F(x, y, z, w)$$

or:

$$\forall z \exists w \forall x \exists y\, F(x, y, z, w)$$

Rather, we need to quantify over functions, to get it "back into line":[16]

$$\exists f \exists g \forall x \forall y\, F(x, f(x), y, g(y))$$

Since we are thus committed to functions (as we quantify over them), we have crossed the border to mathematics, Quine claims: "We leave logic and ascend into mathematics of functions, which can be reduced to set theory but not to pure logic."[17] Quantification over functions, whoever, is second-order quantification. Quine uses here second-order quantification to discredit (on ontological grounds) an alternative logic, while his line on second-order logic is that it is dishonest at best, and otherwise unintelligible, or even inconsistent.

This puts Quine into a strange position: of the two arguments he presents against branching quantifiers, one relies on second-order logic which he emphatically rejects, and the other one is from incompleteness which at least he cannot coherently consider to be a very strong argument. To be charitable, Quine would probably retreat to a rendering of the offendingly untidy branching formulae in linear first-order set theory.

[16](Quine, 1986a), p. 90.
[17](Quine, 1986a), p. 90.

The fact remains, however, that he *does not* do it that way, but rather recasts the branching quantifiers formulae in second-order logic to make his point. Presumably, he does so for didactical reasons: the connection between the branching, and the second-order sentence can readily be seen, while a rendering in set theoretical terms would be much more cumbersome. (Functions are defined as special sets of ordered pairs; and an ordered pair $\langle x, y\rangle$ is defined as a set $\{\{x\}, \{x, y\}\}$, following Wiener and Kuratowski.[18] If we have to use these sets and also spell out the conditions for functionality the result will be much more difficult to parse,[19] and its connection to the branching sentence will not be as obvious.) This didactic strategy, however, will not work if the utilised means is inconsistent or unintelligible. If Quine is right about second-order logic, it seems as if he, hence, would have to accuse himself of dishonesty. This certainly does not constitute a decisive counter-argument against Quine's position; the curious situation Quine got himself into nevertheless seems noteworthy.

As already mentioned, the incompleteness allegation against second-order logic is discussed in detail in chapter 6. In the rest of this chapter I attempt to unpack Quine's animadversions against second-order logic.

[18]Quine regards the Wiener-Kuratowski definition of an ordered pair as a paradigm case of a successful explication: (Quine, 1960), §53; see also (Quine, 1947), p. 79.

[19]In a different context, the Wiener-Kuratowski definition of an ordered pair is spelled out in detail and shown "in action" in section 3.4 below.

3.3 Incoherence and Unintelligibility

3.3.1 Russell's Paradox

In some of his earlier publications Quine alludes to Russell's Paradox in his discussion of higher-order quantification. Russell's Paradox arises in naïve set theory in the following way. Let us say that for any open sentence there is a set that contains all and only the objects that satisfy this open sentence. *Prima facie* this sounds like a reasonable suggestion, but this principle leads to Russell's famous antinomy. Take the predicate 'being a set that does not contain itself'. Call the set that contains all and only sets that do not contain themselves 'r' – the "Russell Set". Does r contain itself? It seems it cannot, since r contains only sets that do not contain themselves; for if it contained itself, it could not be amongst those. So it does not contain itself. In that case, however, it is one of those sets that do not contain themselves, and hence it has to be in the respective set, i.e. in itself. So, if r contains itself, it does not, and if it does not contain itself, it does; or formally, $r \in r \equiv \neg r \in r$: contradiction.

In *On Universals* (Quine, 1947) Quine suggests that an antinomy similar to Russell's for naïve set theory might occur if one takes second-order quantification seriously. If we allow binding predicate letters with quantifiers this means that they "acquire the status of variables", and that we are thus "granting [them] all privileges of 'x', 'y', etc.", i.e. of the first-order variables. This means allowing second-order variables to occur in name position, and hence allowing formulae like 'GH' (just as 'Gx') "seems a very natural way of proclaiming a realm of universals" to Quine.[20] He proceeds by giving a proof in this so characterised system of an analogue of

[20](Quine, 1947), p. 78. See also (Quine, 1969b), p. 257, and (Quine, 1941), pp. 19-20.

Russell's Paradox:[21]

(1)	$GH \equiv GH$	logical truth
(2)	$\forall H(GH \equiv GH)$	(1), universal generalisation
(3)	$\forall F \neg \forall H(FH \equiv GH) \supset \neg \forall H(GH \equiv GH)$	instance of the '$\forall$'-axiom[22]
(4)	$\neg \forall F \neg \forall H(FH \equiv GH)$	(2), (3), *modus tollens*
(5)	$\exists F \forall H(FH \equiv GH)$	(4), quantifier conversion
(6)	$\exists F \forall H(FH \equiv \neg HH)$	(5), subst. '$\neg HH$' for 'GH'
(7)	$\exists F(FF \equiv \neg FF)$	(6), "by a few easy steps"

Quine makes the detour through line (3) and (4) as the deductive system he proposes in this paper does not have any inference rules for the existential quantifier; he treats '$\exists x$' as a mere abbreviation of '$\neg \forall x \neg$'. Otherwise line (5) would follow immediately from line (2) by existential generalisation – if we ignore for the moment that none of the formulae is well-formed according to any standard higher-order logic or theory of types. (I will come back to the question of peculiar syntax below).

This, however, is not the only oddity about this derivation. The substitution step from line (5) to line (6) is invalid: it brings the first 'H' in the substituted expression '$\neg HH$' into the scope of the universal quantifier. Quine justifies this step by referring to two of the rules of inference that he introduced earlier. These are the already mentioned rule to allow the predicate letters all privileges of individual variables, and the rule for substitution of formulae: "Substitute any formulae for 'p', 'q', 'Fx', 'Fy',

[21]Compare (Quine, 1947), p. 78. The notation is changed to match the symbolisation used throughout this thesis. Moreover, Quine's commentary column is omitted in favour of my own annotations, as Quine's abbreviated comments would require rather extensive introduction. Only the annotation in line (7) is verbatim.

[22]The '$\forall$'-axiom is '$\forall x Fx \supset Fy$' in Quine's formalisation.

'Gx', 'Fxy', 'Gzw', etc. (subject to sundry provisos which need not be recounted here)".[23] The provisos that Quine skips so elegantly include that the substitution has to be uniform (so that the faulty inference from '$\forall x(Fx \supset Fx)$' to '$\forall x(Fx \supset Gx)$' is barred), and that variables must not come into the scope of a quantifier that they were not in before (to avoid, e.g., the inference from '$Gy \supset \forall x(Fx \supset Gy)$' to '$\neg Fx \supset \forall x(Fx \supset \neg Fx)$'). The former restriction is obeyed in Quines "proof"; the latter, alas, is not.

In a later paper, *Logic and the Reification of Universals*, which is partly based on *On Universals*, Quine still alludes to the paradox, but ommits its "derivation", and indeed any mention of the peculiar syntax that allows for expression like '$\neg HH$'.[24]

What drives Quine to suggest formulae like '$\neg HH$'? It seems that his conviction that if we allow quantification into predication position we ontologically commit ourselves to things that predicates now are meant to refer to (while at first-order we do not treat predicates as referring) is his reason to do so. It seems that for Quine the existential quantifier (of whatever order) shows us what we are ontologically committed to, and if we further assume that those entities that we are committed to are referred to by expressions that occur in name position, it is only a small step to put second-order variables into name position.

This way will indeed lead to inconsistency, whether Quine's "proof" as such succeeds or not. Russell first[25] formulated his Paradox in a letter that he wrote to Frege in response to Frege's *Grundgesetze der Arithmetik*[26] [*Basic Laws of Arithmetic*].[27]

[23] (Quine, 1947), p. 77.
[24] (Quine, 1953), p. 121.
[25] This was where *Russell* first formulated the paradox. Credit where credit is due, however: Ernst Zermelo had discovered "Russell's Paradox" a full year before Russell, see (Rang and Thomas, 1981).
[26] (Frege, 1893).
[27] (Russell, 1902).

Frege's system of the *Grundgesetze* famously contains Basic Law V which, roughly, assigns an object to every extension of a predicate. While, strictly speaking, the formulation Russell used was not well-formed in Frege's system, Frege saw immediately how the antinomy could be formulated properly and derived from Basic Law V.[28] Quine mirrors this strategy for his criticism of second-order logic. Quantifying into predicate position for him means treating predicates as referring to objects. In one way or other, a paradox analogous to Russell's should arise. We only have to define a predicate such that the object it refers to can somehow be proven both to fall and not to fall under this very predicate, i.e. something like a predicate that expresses non-self-reference has to be found. If the formulation of the system is careless enough, this should be possible.

Quine knows, of course, that there are consistent systems of second-order logic in which no contradiction can be proven. In his *Philosophy of Logic* he concedes that "there is no actual risk of paradox as long as the ranges of 'x' and 'G' are kept apart".[29] In *On Universals* he claims, however, that these consistent systems are arrived at by making "one or another aribtrary restriction [...]. The most familiar restriction is the simple theory of types, due to Russell."[30] Quine considers the restrictions that need to be made in order to avoid the paradox as wholly artificial.

It should be noted that Frege's system of the *Begriffsschrift*[31] neither allows for a derivation of Russell's Paradox, nor does it contain restrictions that appear artificial or arbitrary in any way. The distinction that blocks the derivation of an analogue to Russell's Paradox is that between name and predicate (that Quine wants to make,

[28](Frege, 1902).
[29](Quine, 1986a), p. 68.
[30](Quine, 1947), p. 78.
[31](Frege, 1879).

too). In Frege's *Begriffsschrift* corresponds to the distinction between object and function, that seems well motivated in Frege.[32]

Quine, however, takes it that the objects referred to by the predicates in second-order logic have to be classes, as in the theory of types which he explicitly mentions and which, at least for him, is a class theory. They are thus objects and belong in the (first-order) domain. A possible autobiographical reason for this was already mentioned in the introduction to this chapter. There are, however, also philosophical reasons for Quine.

3.3.2 No Entity Without Identity

Whilst Quine admits in a way that second-order quantification can, despite its air of incoherence, be construed in a consistent way, by way of imposing "arbitrary restrictions", he still thinks that in doing so the border from pure logic to mathematics is crosses as we are now quantifying over classes. Even granting for the moment (later chapters will discuss this assumption) that second-order quantifiers range over new kinds of entities, and thus bring about new ontological commitment, why should that be a commitment to classes? Quine reasons that as the new entities are referred to by predicates they have to be universals. The only universals he find acceptable, however, are classes. In *On the Individuation of Attributes* (Quine, 1975) Quine writes:

> Classes, down the years I have grudgingly admitted; attributes not. I have felt that if I must come to terms with Platonism, the least I can do is keep it extensional.[33]

[32] Compare also (Dummett, 1981), chapter 8, concerning this point.
[33] (Quine, 1975), p. 100.

His worry concerning the issue of extensionality has got to do with his slogan: “There is no entity without identity”.[34] Classes are extensional: their identity is determined by what members they have. There are no two different classes which have all and only the same members. If the classes A and B both have as their only members both Forth Rail Bridge and the Eiffel Tower, then ‘A’ and ‘B’ are really just two names for the same class; A and B are identical.

Not so with attributes. Attributes which have the same extension still need not be identical. Quine’s famous example are the attributes ‘creature with a heart’ and ‘creature with kidneys’.[35] The class H that contains all and only creatures with a heart is identical to the class K that contains all and only creatures with kidneys since H and K have the same members (or so we may at least suppose the sake of the argument). The attributes ‘creature with a heart’ and ‘creature with kidneys’, however, are *not* identical. The predicates are co-extensional, but the corresponding attributes are supposed to be distinct. Such intensional objects have, according to Quine, no precise identity conditions, and thus need to be rejected. While he “grudgingly admitted” abstract objects and universals[36] in form of classes, he never abandoned extensionality.

Intensional universals are obscure creatures for Quine. They have to be eliminated from scientific and philosophical discourse in favour of extensional notions. In particular, talk involving attributes is to be replaced by a language that makes use of classes. Attributes cannot be allowed into any ontology acceptable to Quine. They lack a good criterion of identity (if such a criterion can be specified at all), since it

[34](Quine, 1975), p. 102; compare also (Quine, 1948), p. 4, (Quine, 1960), §43, (Quine, 1969a), pp. 21–23, (Quine, 1992), p. 52, and other places.
[35](Quine, 1969a), p. 23, (Quine, 1975), p. 101.
[36]Quine takes classes to be abstract universals.

would have be intensional, and intensionality is mysterious.[37] Quine sees the likely best shot to give criteria of identity for attributes as being in terms of synonymy[38], maybe involving necessity claims, but certainly some sort of analyticity claim.[39] He, of course, rejects all these notions.[40] If the best attempt available thus has to rely on unacceptable notions or resources, it seems to Quine that any attempt is doomed to fail.

It appears therefore, that Quine's argument for his claim that second-order logic is "set theory in sheep's clothing" might run more or less as follows: (i) Quantifiers show us our ontological commitments. (ii) Second-order quantifiers, hence, commit us to some kind of universal. (iii) The only acceptable universals are classes – based on the considerations above, the *prima facie* main candidates *attributes* should be ruled out. (iv) Therefore, second-order logic commits us to sets. (In chapter 5 I will present a slightly different reconstruction, based on a more thorough analysis of Quine's criterion for ontological commitment.)

All three of (i)-(iii) have been attacked. (i) is denied by, amongst others, Meinongians who propose the being of non-existent objects. (iii) is denied by philosophers who argue in favour of real properties (which is, presumably, what Quine calls "attributes"). In chapter 5 I argue that "Quantifiers" in (i) should be read as "*First-order quantifiers*", and that (ii) does not follow for that reason. George Boolos accepts (i), and also accepts it for second-order quantifiers, but still resists the step to (ii), proposing a plural interpretation of the second-order quantifiers. Boolos proposes that second-order quantification is not the old kind of quantification over new

[37](Quine, 1975), pp. 101–105.
[38](Quine, 1986a), p. 67.
[39](Quine, 1975), p. 105.
[40]See (Quine, 1951b).

entities (as (ii) claims) but rather a new kind of quantification, i.e. plural quantification, over the same old entities, i.e. those that the first-order variables range over singularly.[41] My chapter 4 will discuss this proposal in detail.

3.4 Dishonesty: The Hidden Staggering Ontology

In the first edition of his *Philosophy of Logic* (Quine, 1970), Quine ascribes to second-order logic an ontological commitment to the set-theoretic hierarchy:

> Set theory's staggering existential assumptions are cunningly hidden now in the tacit shift from schematic predicate letter to quantifiable set variable.[42]

Having admitted that second-order logic can be formulated in a coherent way, and that a commitment to obscure entities like attributes can be avoided by construing the second-order variables as ranging over sets, we are led in this way to the remaining two Quinean objections against second-order logic: it is committed to the vast set theoretical ontology, and, moreover, disguises this commitment by a "cunning" deception. Both allegations will be discussed in chapter 5, and Boolos' second response to the first Quinean complaint, the suggestion of a plural interpretation of second-order logic, is the topic of my chapter 4.

Before Boolos proposed his plural interpretation, however, he reacted to Quine's "tacit set theory" charge in *On Second-Order Logic* (Boolos, 1975). Boolos first observes that it would be a mistake to just understand or translate second-order sentences so that predicates and predicate variables are replaced by names for sets

[41] First in (Boolos, 1984b); see chapter 4 for more references.

[42] (Quine, 1970), p. 68.

and set variables, as he understands Quine to be suggesting. Replacing expressions like, e.g., 'Ga' uniformly with expressions like '$a \in \gamma$' where 'γ' is now explicitly a name of a set will lead to a loss of validity, Boolos contends.[43] His main example is the second-order validity '$\exists F \forall x\, Fx$'. Translated along the lines Boolos suggests Quine demands, we end up with '$\exists \alpha \forall x\, x \in \alpha$'. This sentence, however, asserts the existence of an all-inclusive set which is not only not valid, but also inconsistent with Zermelo-Fraenkel set theory (ZF).[44]

In defense of Quine it has to be said, however, that Quine never proposed a translation into ZF as Boolos suggests. If Boolos is right at all in interpreting Quine as demanding a straightforward translation of second-order logic into set theory, it seems to be a more charitable interpretation that Quine intended a translation into one of his own set theories, the system of *New Foundations* (NF), for example. Strikingly, NF asserts the existence of the universal set, and so '$\exists \alpha \forall x\, x \in \alpha$' is valid in it.[45]

Quine builds NF containing a rule for set comprehension '$\exists x \forall y (y \in x \equiv \varphi)$' that attempts[46] to block Russell's Paradox by requiring that 'φ' is "stratified" and does not contain 'x'; 'stratified' here means that the variables occurring in the two places left and right of '$\in$' must be consistently typed throughout the formula such that if the type of the variable on the left is n, the type of the variable on the right is $n+1$, e.g. '$x^2 \in y^3$'. Expressions like '$\neg x \in x$', hence, are not stratified and thus

[43](Boolos, 1975), p. 40.

[44]Boolos' second example is a mystery: he states that '$x = z$' is implied by '$\forall F(Fx \supset Fz)$', but not by '$\forall \alpha(x \in \alpha \supset z \in \alpha)$'. If we can take '$\forall \alpha$' to range of all sets (which seems reasonable), and especially that the range includes the singleton of x, then the latter sentence certainly entails '$x = z$', for z could not be in the singleton of x without being identical to x, on pain of contradiction.

[45](Quine, 1937), p. 94.

[46]As of now there is no proof of consistency for NF.

banned.[47]

Quine obviously models NF along the lines of the Simple Theory of Types. Sets can contains sets that contain sets, but type bounderies cannot be jumped or reversed. '$x \in y \wedge y \in z$' is fine, as it is stratified which is made explicit like this: '$x^1 \in y^2 \wedge y^2 \in z^3$'. '$x \in y \wedge y \in z \wedge x \in z$', however, is not. If we spell out the types of the variables, '$x^1 \in y^2 \wedge y^2 \in z^3 \wedge x^1 \in z^3$', we see that the last conjunct violates the conditions for stratification: it jumps a type. Also, '$x \in y \wedge y \in z \wedge z \in x$', violates stratification: z would have to be both one type lower than x, and two types higher.

It seems, therefore, that a translation from second-order logic into NF works neatly: predicates are one type higher that names, and so the formulae coming out of the translation should always be stratified. One might think that a problem lurks when this procedure is applied to second-order set theory, rather than pure second-order logic. An example that springs to mind is that of the axiom of separation which is not valid in NF.[48] One has to be careful, however, not to beg the question against Quine: second-order ZF formulae, surely, cannot count as counterexamples: the inconsistencies with NF would come from ZF, rather than from the translation of the second-order variables. If we just take the second-order axiom of separation '$\forall F \forall x \exists y \forall z (z \in y \equiv (z \in x \wedge Fz))$' as such, the resulting translation turns out to be stratified: '$\forall \alpha \forall x \exists y \forall z (z^1 \in y^2 \equiv (z^1 \in x^2 \wedge z^1 \in \alpha^2))$'.

While Boolos' example does not work if we grant Quine NF (which seems only fair), and while the translation seems to work fine for monadic predicate variables, it can nevertheless be shown, albeit in a less elegant way, that it breaks down as soon as

[47](Quine, 1937), pp. 91–92.

[48](Quine, 1937), pp. 96–97. Quine calls the axiom by its German name, "*Aussonderung*", following Zermelo.

we consider two- and three-place predicates. As mentioned above, Quine champions the Wiener-Kuratowski definition of an ordered pair, and suggests treating many-place predicates (or relation symbols) as monadic predicates that take ordered pairs, triples, etc. as arguments.[49] Quine takes it for granted that by following this strategy everything will fall into place concerning many-place predicates. Concerning the "translation" of second-order logic into NF, however, this is not the case.[50]

Following Wiener and Kuratowski, an ordered pair $\langle a, b\rangle$ is defined as a set of sets, namely $\{\{a\}, \{a, b\}\}$. An order triple $\langle a, b, c\rangle$, is turn, is defined as an ordered pair $\langle a, \langle b, c\rangle\rangle$, i.e. as the set $\{\{a\}, \{a, \{\{b\}, \{b, c\}\}\}\}$. The curly set parentheses are mere notational abbreviations, of course, and need to be replaced by expression involving only variables, quantifiers, brackets, symbols for truth-functions, and '$\in$'. Once this unpacking is done, it can easily be checked whether a formula is stratified or not. The second-order valid sentence

$\exists R\, Rab$

for example, is translated and unpacked like this (done step by step):

$\exists R\, Rab$

$\exists R\, R\langle a, b\rangle$

$\exists r\, \langle a, b\rangle \in r$

$\exists r\, \{\{a\}, \{a, b\}\} \in r$

$\exists r \exists x\, (x \in r \wedge \{a\} \in x \wedge \{a, b\} \in x)$

$\exists r \exists x \exists y \exists z\, (x \in r \wedge y \in x \wedge z \in x \wedge a \in y \wedge a \in z \wedge b \in z)$

[49] For references see footnote 18 above.

[50] In chapter 5 we will see how Quine gets into trouble concerning many-place predicates again in a different context.

The resulting formula is thus stratified: 'a' and 'b' are of type 1, 'y' and 'z' are of type 2, 'x' is of type 3, and 'r', finally, is of type 4:

$$\exists r \exists x \exists y \exists z\,(x^3 \in r^4 \wedge y^2 \in x^3 \wedge z^2 \in x^3 \wedge a^1 \in y^2 \wedge a^1 \in z^2 \wedge b^1 \in z^2)$$

Similarly, a second-order validity featuring a three-place predicate can be translated and stratified:

$$\exists S\ Sabc$$
$$\exists S\ S\langle a, b, c\rangle$$
$$\exists s\ \langle a, b, c\rangle \in s$$
$$\exists s\ \langle a, \langle b, c\rangle\rangle \in s$$
$$\exists s\ \{\{a\}, \{a, \langle b, c\rangle\}\} \in s$$
$$\exists s\ \{\{a\}, \{a, \{\{b\}, \{b, c\}\}\}\} \in s$$
$$\exists s \exists x\,(x \in s \wedge \{a\} \in x \wedge \{a, \{\{b\}, \{b, c\}\}\} \in x)$$
$$\exists s \exists x \exists y \exists z\,(x \in s \wedge y \in x \wedge z \in x \wedge a \in y \wedge a \in z \wedge \{\{b\}, \{b, c\}\} \in z)$$
$$\begin{aligned}\exists s \exists x \exists y \exists z \exists v\,(&x \in s \wedge y \in x \wedge z \in x \wedge a \in y \wedge a \in z \wedge\\ &v \in z \wedge \{b\} \in v \wedge \{b, c\} \in v)\end{aligned}$$
$$\begin{aligned}\exists s \exists x \exists y \exists z \exists v \exists w \exists u\,(&x \in s \wedge y \in x \wedge z \in x \wedge a \in y \wedge a \in z \wedge\\ &v \in z \wedge w \in v \wedge u \in v \wedge b \in w \wedge b \in u \wedge c \in u)\end{aligned}$$
$$\begin{aligned}\exists s \exists x \exists y \exists z \exists v \exists w \exists u\,(&x^5 \in s^6 \wedge y^4 \in x^5 \wedge z^4 \in x^5 \wedge a^3 \in y^4 \wedge a^3 \in z^4 \wedge\\ &v^3 \in z^4 \wedge w^2 \in v^3 \wedge u^2 \in v^3 \wedge b^1 \in w^2 \wedge b^1 \in u^2 \wedge c^1 \in u^2)\end{aligned}$$

's' is of type 6 while 'r' above was of type 4, but this does not pose any problems; it just shows that three-place predicates are generally two types higher than two-place predicates, due to the Wiener-Kuratowski construction. Also that 'a' turns out to be of type 3 while 'b' and 'c' are of type 1, does not pose any problems *as such*.

The only thing that matters is that the formula is stratified, and this is the case. If we consider a second-order validity that contains both a three- and a two-place predicate, however, we end up with a non-startified sentence after the translation. Boolos' claimed loss of validity finally takes place. If we break down

$$\exists R \exists S\,(Rab \wedge Sabc)$$

in the same way as we did above, it is easy to see that the first conjunct require 'a' and 'b' of the same type, while the second requires 'a' to be of type $n+2$ where n is the type of 'b'. Hence, the resulting sentence is not a validity of NF, while the original one is valid in second-order logic.

There is no need, however, to interpret Quine at all as proposing a straight translation like the one Boolos envisions between second-order logic and any set theory. In the same book that Boolos' criticism is directed againt, *Philosophy of Logic*, Quine even states *explicitly* that assuming that all predicates (or open sentences) correspond to sets and *vice versa* leads into contradiction.[51] As Quine's remarks are interpreted here, what he presents is the best case for second-order logic as being some kind of class theory (taking it for granted that the second-order quantifiers commit us to new kinds of entities) – most likely something very similar to a theory of types with which it indeed has a lot of features in common. If the counterexamples to a translation are indeed counterexamples to this claim, so much the worse for second-order logic. It is not a problem for Quine.

It will certainly be possible, however, to construct some kind of translation or interpretation in set theory that assigns sets to the second-order variables and is hence a witness to the spirit of Quine's claim. One can be reasonably confident

[51](Quine, 1986a), p. 53.

that there could be such a translation or interpretation, since there actually is one: the standard model theory provides exactly that. As I point out in chapter 7, for proponent of the model-theoretic account to second-order like Shapiro, the set theoretical nature of this conception of second-order logic is not a last resort to make sense of the second-order quantifiers, as it is for Quine: it is a rather feature that is embraced from the start.

Boolos next investigates what becomes of Quine's claim concerning the staggering set-theoretical commitment of second-order logic if the standard model-theoretic interpretation is taken into account. On this interpretation, the second-order variables take as values subsets of the domain, and so Boolos admits a commitment to sets. He observes, however, the disanalogy between second-order logic and set theory that all standard set theories agree that there is at least one two-membered set, while the second-order sentence '$\exists X \exists x \exists y\, (Xx \wedge Xy \wedge x \neq y)$' is not valid: it is false in a model with a one-membered domain.[52] The only sentences valid in second-order logic that commit us to particular sets, thus, seem to be '$\exists X \forall x\, \neg Xx$' and '$\exists X \forall x\, Xx$' which appear to affirm the existence of the empty and the universal set, respectively.

Boolos concludes, however, that in the case of second-order logic

> the commitment is exceedingly modest; the null set is the only set to whose existence second-order logic can be said to be committed.[53]

Why only the null (or empty) set and not the universal one? The reason is simple: what set (on the set theoretical interpretation) '$\exists X \forall x\, Xx$' picks out is relative to

[52](Boolos, 1975), pp. 40 and 44; p. 44 contains a typo that turns the above sentence into the stronger one: '$\exists X \forall x \exists y\, (Xx \wedge Xy \wedge x \neq y)$'. This sentence entails the existence of the universal set, and hence cannot be what Boolos intended.

[53](Boolos, 1975), p. 48.

the first-order domain. It picks out the set that contains all and only the objects in the domain.[54] It therefore does not commit us to any particular set. It seems, however, that one should admit that some additional commitment occurs. Standard first- and second-order logic alike assume a non-empty domain; therefore, there is always at least one object and hence always at least one singleton set. What this set is varies with the domain, but our ontology gets pumped up.

Generally, as the second-order variables are interpreted as ranging over the subsets of the domain, we will end up with a "second-order ontology" of the size of the powerset of the first-order domain. If there are n objects in the domain, the second-order variables range over 2^n sets. Only one application of the powerset axiom is needed, though, which is not very much compared to the vastness of the set-theoretical hierarchy.[55] So, speaking in set theoretical terms, the commitments of second-order logic are still fairly modest, although not quite as modest as Boolos suggests.

Boolos does not seem to be willing to make that concession, as we cannot *definitely* say which sets those are; the model-theoretic interpretation of second-order logic does not determine any particular set independent of the domain, except for the empty one. For any given domain, however, it determines which sets the second-order variables range over, viz. the subsets of the domain – barring the continuum problem if the domain is infinite which is discussed in detail in chapter 7.

For second edition of his *Philosophy of Logic* (Quine, 1986a) Quine revised the passage quoted in the beginning of this section. The remark about the "staggering

[54](Boolos, 1975), p. 47. Obviously, this poses a problem in the case of second-order set theory where the domain is intended to include all sets, and thus does not form a set itself. This will be discussed in chapter 5.

[55]This point is made by (Shapiro, 1991), p. 21; but see also (Shapiro, 1999), p. 60.

existential assumptions" of set theory, which second-order logic is allegedly committed to, disappears and is replaced by the complaint that "still a fair bit of set theory has slipped in unheralded."[56] Since this second edition appeared after Boolos' *On Second-Order Logic* was published – a publication that Quine was without any doubt aware of – it does not seem too unlikely that Quine changed this passage in response to Boolos' arguments.

Whilst I argue in chapter 5 against Quine's claim that second-order logic brings about any special set-theoretical commitment, I still want to submit here that Quine should not have given in to Boolos. We can distinguish between two notions of ontological commitment: (i) a commitment to kinds of things, and (ii) a commitment to a certain number of things. (The commitment to individual sets, which seems to be the notion that Boolos is interested in, gets spelled out by way of a Russellian analysis of definite descriptions, and is thus subsumed under case (i). See chapter 5 for details.) The latter kind of commitment is most usefully though of in this context as determining the size of the first-order domain. If we, for example, have a commitment of type (i) to the natural numbers (the natural-number kind of entity), we will run into a commitment of type (ii) to an infinite domain, as there are infinitely many natural numbers. Single natural numbers do not come on their own, one has all of them or none. We could not just commit us to the existence of number 7, and deny that 6 and 8 existed, too. Or rather, if we were doing so, we would speak of things that are not the natural numbers.

The same should be true for set theory. If we are committed to the kind "set", we commit to what our favourite theory of them (be it ZF, NF, or whatever) says about them. If we favour ZF, for example, as the theory that tells us what sets are, and

[56](Quine, 1986a), p. 68.

what sets *there* are, we commit to the entire hierarchy of sets.[57] If this interpretation of Quine's criterion for ontological commitment is right, Quine should have stuck to his guns: If second-order quantification indeed commit us to the existence some sets, we *are* committed to the "staggering existential assumptions" of set theory. (As mentioned above, Quine's assumptions that lead to this conclusion will be argued against in the following two chapters of this thesis.)

If Quine wanted to follow this way, however, he might find himself in an uncomfortable situation. As mentioned in section 3.2 above, Quine requires some set theory for his substitutional account of validity for first-order logic. If so, Quine's account is also committed to the entire set theoretic hierarchy, just as the model theoretic approaches for both first- and second-order logic. The difference between first- and second-order logic concerning their set-theoretical commitment vanishes. The ontological commitment to the "staggering" ontology of set theory cannot be escaped at first-order either.

All this, however, depends on the Quinean claims about ontological commitment. The following two chapters will discuss this question. Chapter 4 contains a discussion of Boolos' suggestion of a plural interpretation of second-order logic. He accepts that second-order quantification expresses ontological commitment, but denies that it is any *extra* commitment. In chapter 5 I analyse in detail Quine's criterion of ontological commitment.

[57]A point similar to this is made in response to (Shapiro, 1991), chapter 1.3, by (Jané, 1993), pp. 75–76; compare also Shapiro's response in (Shapiro, 1999), p. 59–61. The subtle issues raised by Jané are discussed in detail in chapter 7.

Chapter 4

Plurals

4.1 Some Critics

After his first defense of second-order logic in *On Second-Order Logic* (Boolos, 1975), Boolos published a series of articles that propose and defend the now famous plural interpretation of monadic second-order logic.[1] Boolos' main interest seems to lie in an account of second-order quantification that does not construe variables in predicate position as ranging over the subsets of the domain, although he still assumes the latter in his (Boolos, 1975). For the case of second-order set theory, however, that cannot be the right construal. There is no set of all sets, so the first-order domain cannot be assumed to be a set, and the range of second-order variables cannot be the powerset of the domain. '$\exists X \forall y Xy$' is a theorem of second-order logic, and hence of second-order set theory. If the predicate variables are taken to refer to sets, however, this sentence asserts that there is a set that contains all sets which contradicts the standard Zermelo-Fraenkel set theory (ZF). Also the sentence

[1] See mainly (Boolos, 1984b), (Boolos, 1985a), (Boolos, 1985b), and (Boolos, 1994).

'$\exists X \forall y(Xy \equiv \neg y \in y)$' is a valid second-order sentence (it is an instance of the comprehension schema), but if we are interpreting the second-order variables to range over sets, this sentence asserts that there is a set that contains all and only the sets that do not contain themselves. This, however, is known to lead into Russell's Paradox (see chapter 3).[2]

It is worth noting that, in a way, this problem arises in first-order set theory already. We are still in need of specifying a domain, which usually is taken to be a set. This cannot be the case for set theory, though, on pain of contradiction. This problem is usually ignored, or if acknowledged, a flight to proper classes[3] is taken: the domain of a first-order set theory is the proper class that contains all the sets that the theory is about. Proper classes could also serve as the range of second-order variables where the (first-order) domain is too large to form a set, but this seems also unsatisfactory, at least to Boolos: "Set theory is supposed to be a theory about *all* set-like objects."[4] Boolos declares that he is a "settist" by which he means that he accepts no classes, collections, etc., that are not sets of ZF.[5] As a solution for this problem Boolos proposes to interpret second-order variables plurally. Before he unveils this, though, Boolos spends more than the first half of his (Boolos, 1984b) easing the reader into plural talk. He presents natural language examples of plural sentences that "*look* as if they ought to be symbolizable in first-order logic"[6] but

[2]See (Boolos, 1984b), p. 64. The Boolos of (Boolos, 1975) takes second-order quantification for this reason to be illegitimate when the first-order variables range over sets. Boolos rejects his earlier view as he proposes the plural interpretation that is the topic of this chapter; see (Boolos, 1984b), p. 65.

[3]Proper classes in this sense are classes that can contain, but cannot themselves be contained in classes. No set in the usual (i.e., ZF) sense is a proper class.

[4](Boolos, 1984b), p. 66. Boolos expresses the same thought more emphatically in (Boolos, 1974), p. 35: "Wait a minute! I thought that set theory was supposed to be a theory about all, 'absolutely' all, the collections that there were and that 'set' was synonymous with 'collection'."

[5](Boolos, 1974), p. 36; see also (Boolos, 1994), pp. 239–241.

[6](Boolos, 1984b), p. 57.

turn out not to be.

His first, and most famous, example is the so-called Geach-Kaplan Sentence:

(1) Some critics admire only one another.

Boolos specifies as the intended meaning of the sentence

> that there is a collection of critics, each of whose members admires no one not in the collection, and none of whose members admires himself.[7]

Taking the domain of discourse to be critics, and symbolising 'x admires y' by 'Axy', (1) can be formalised as the second-order sentence

(2) $$\exists X(\exists x Xx \wedge \forall x \forall y[(Xx \wedge Axy) \supset (x \neq y \wedge Xy)])$$

Boolos poaches this sentence from Quine who mentioned it in both his *Roots of Reference* and *Methods of Logic*.[8] Quine in turn credits Peter Geach with the discovery of this sentence, and David Kaplan with the proof that (1) cannot be adequately expressed in first-order logic, as (2) is not equivalent to any sentence of pure first-order logic.

To see that (2) has no first-order equivalent, substitute '$(x = 0 \vee x = y + 1)$' for 'Axy' in (2). The resulting sentence:

(3) $$\exists X(\exists x Xx \wedge \forall x \forall y[(Xx \wedge (x = 0 \vee x = y + 1)) \supset (x \neq y \wedge Xy)])$$

can be shown to be true in all non-standard models of first-order arithmetic, but

[7](Boolos, 1984b), p. 56.

[8](Quine, 1974), p. 111, and (Quine, 1982), p. 293, respectively.

false in the standard one.[9] There is a non-standard model, however, that satisfies any sentence of first-order arithmetic if, and only if, it is satisfied in the standard model.[10] Hence, there can be no first-order sentence that is equivalent to (2).

Boolos is a bit uneasy with the Geach-Kaplan sentence, however. He is not entirely content that (1) in fact is an "acceptable" sentence of English. He explains: "The 'only' seems to want to precede the 'admires' but the intended meaning forces it to stay put." He suggests that inessential changes in (1), i.e. exchanging the predicate without changing the logical form, improves the situation. "Some computers communicate only with one another" or "Some Bostonians speak only to one another" sound more natural, and are still formalised as in (2). Boolos states:

> I don't have any idea why replacing the transitive verb 'admire' by a verb or verb phrase taking an accompanying prepositional phrase helps matters, but it does seem to me to do so.[11]

He therefore presents another pair of sentences to provide better examples. The first of the two is readily formalisable in a first-order way:

(4) There is a horse that is faster than Zev and also faster than the sire of any horse that is slower than it.

[9] (3) is true in any non-standard model: Let X be the set of all non-standard elements of an arbitrary model. X does not contain zero, but is non-empty, hence it contains only successors and immediate predecessors of any of its members. (3) is false in the standard model: X is a (non-empty) set of natural numbers. If the least member of X, x, is 0, then let $y = 0$; and $x = y + 1$ for some y otherwise. Since x is the least member of X, y is not in X. Compare (Boolos, 1984b), p. 57, fn. 7.

[10] See (Shapiro, 1991), p. 123, for details; compare also p. 259, n. 4.

[11] (Boolos, 1984b), p. 57. Peter Simons exchanges 'admire' for 'praise' in the Geach-Kaplan sentence and thereby produces the perhaps aesthetically more pleasing 'Some critics praise only one another', (Simons, 1997), p. 260.

Boolos assumes a domain of horses, and symbolises ‘Zev’, ‘the sire of’, ‘is faster than’, and ‘is slower than’, by ‘0’, ‘s’, ‘$>$’, and ‘$<$’, respectively. (4) can then be symbolised in first-order logic as

(5) $$\exists x(x > 0 \wedge \forall y[y < x \supset x > s(y)])$$

If we, however, consider the very similar, but plural sentence:

(6) There are some horses that are faster than Zev and also faster than the sire of any horse that is slower than (all of) them.

we will find that a first-order formalisation cannot be achieved. Rather, the second-order sentence representing (6) is:

(7) $$\exists X(\exists x Xx \wedge \forall x(Xx \supset x > 0) \wedge \forall y[\forall x(Xx \supset y < x) \supset \forall x(Xx \supset x > s(y))])$$

If (7) is given the obvious arithmetical interpretation it can again be shown by an argument analogous to the one used above, that (7) has no first-order equivalent.[12]

What does Boolos aim to show with these examples, though? It is of course not true, *strictly* speaking, that (1), for example cannot be formalised in first-order logic. Bluntly, choosing ‘Cx’ for ‘x is one of the critics who admire only one another’, we get

(8) $$\exists x Cx$$

[12](Boolos, 1984b), p. 58; see also (Boolos, 1984a) – where it occurs first – for details. Boolos mentions this example again in (Boolos, 1985a), pp. 73–74.

as a first-order formalisation of (1). (8), of course, is not equivalent to (2) in any normal sense, just like the formalisation of (1) in propositional logic, 'p', is neither equivalent with (2), nor with (8). Boolos' claim, therefore, is best understood as stating that sentences like (1) and (6) cannot be formalised displaying their full logical structure with the resources of first-order logic alone.

This is crucial, and also revealing. For, can a case be made that (1)'s structure as exhibited by (2) is indeed *logical* structure? For Quine, for example, the "non-firstorderizability" of (1) shows that, despite all appearance[13], (1) is a mathematical statement, as we need to quantify over sets in order analyse its structure properly:

$$\text{(9)} \qquad \exists s(\exists x\, x \in s \wedge \forall x \forall y[(x \in s \wedge Axy) \supset (x \neq y \wedge y \in s)])$$

In addition to the logical structure displayed by the first-order variables and quantifiers and the truth-functional connectives, the sentence exhibits also, in Quine's sense, some set-theoretical content, indicated by the use of '$\in$'. (Consequently, Quine only mentions (1), the Geach-Kaplan sentence, in the chapter entitled *Glimpses Beyond* of his *Methods of Logic* (Quine, 1982).)

Boolos' aim here is to show that second-order resources, plurally interpreted in the way introduced in section 4.2 below, are not only a natural extension of the first-order resources, but were indeed part of the package all along. Before this reasoning is presented, it is worth remarking that Boolos follows this strategy already in his earlier *On Second-Order Logic.* Boolos not only points out the continuity of first- and second-order logic as far as the definitions of validity and (semantic) consequence

[13](Resnik, 1988), p. 84, points out that the Geach-Kaplan sentence really shows how natural language sometimes masks ontological commitment, and that "Boolos has turned the Geach-Kaplan [sentence] on it head." I will come back to this problem in section 4.4 of this chapter, and also, in more detail, in chapter 5.

go,[14] but also argues that some sets of sentences are "apparently inconsistent" that cannot be shown to be so in first-order logic. In second-order logic, however, the inconsistency of the following finite sets can be exhibited: {'Smith is an ancestor[15] of Jones', 'Smith is not a parent of Jones', 'Smith is not a grandparent of Jones', 'Smith is not a grand-grandparent of Jones', ...}; {'It is not the case that there are infinitely many stars', 'There are at least two stars', 'There are at least three stars', ...}; or {'x is a natural number', '$x \neq 0$', '$x \neq 1$', '$x \neq 2$', ...}.

The reason why these sets cannot be shown to be inconsistent is easily seen in the compactness theorem for first-order logic: if an infinite set of first-order sentences is inconsistent, then a finite subset of it is inconsistent. All finite subsets of the sets given as examples above, however, are consistent.

Boolos then compares the situation to that of, e.g., the set {'It is not the case that there are at least three stars', 'It is not the case that there are no stars', 'It is not the case that there is exactly one star', 'It is not the case that there are exactly two stars'}. This set can be shown to be inconsistent in pure first-order logic. The argument that Boolos might allude to is that the cases are sufficiently similar to suggest that the logic that shows the infinite sets to be inconsistent is logic in the same right as the logic that show the finite sets to be inconsistent.[16] This of course does not follow on its own: one could also use set theory or arithmetic to show that these sets are inconsistent, and presumably that does not show that set theory or arithmetic are proper logic. What Boolos does instead is to heap up evidence that first- and second-order logic share their most important features in common, while

[14](Boolos, 1975), p. 48.

[15]Frege's second-order definition of the so-called *ancestral* is require for this. It will be explained a little further below.

[16](Boolos, 1975), p. 49.

arguing that their differences – like the completeness of first- and the incompleteness of second-order logic[17] – are less important.

To come back to the sets above, an opponent of Boolos' could further argue that the inconsistency of the infinite sets is not logical, but rather mathematical or conceptual. How convincing one finds it that the infinite sets above are, in spite of Boolos' insistence, logically consistent, will presumable depend on what one thinks about the status of second-order logic. The sets can still be "inconsistent" in another sense, if they are not logically inconsistent, like the set {'a is red', 'It is not the case that a is coloured'}, for example (this is then, of course, not the technical, logical notion of inconsistency anymore).

As mentioned above, the strategy of pointing out similarities and continuities between first- and second-order logic is followed by Boolos in his case for the plural interpretation, too.[18] On the first page of his (Boolos, 1984b), he identifies as "the devices of standard logic" the usual propositional connectives, quantification, cross-reference, and identity. The devices of cross-reference in English are words like 'it',

[17](Boolos, 1975), pp. 49–53; I will come back to this in my chapter 6 where I discuss the incompleteness of second-order logic in detail.

[18]In (Boolos, 1994) a very similar general strategy is used again. Boolos discusses here Putnam's remarks on second-order logic in his *Philosophy of Logic* (Putnam, 1971), pp. 30-32, and his *Peirce the Logician*, in (Putnam, 1990), esp. p. 259. Boolos seems to agree with Putnam that statements of the validity of logical truths and logical inferences have to be logical truths themselves, but argues against him that we thereby have to accept more second-order logic than Putnam wants to admit. Putnam claims that if we accept the validity of a syllogism like BARBARA, we implicitly accept the second-order statement '$\forall F \forall G \forall H([\forall x(Fx \supset Gx) \wedge \forall x(Gx \supset Hx)] \supset \forall x(Fx \supset Hx))$'. For Putnam, the properly logical fragment of second-order logic stops at Π_1 sentences, however (i.e. second-order sentences with only one uninterrupted sequence of prenex universal second-order quantifiers in front of an otherwise second-order quantifier free formula). The "existentially committing" Σ_1 sentences (the analogue for second-order existential quantifiers) are out of the scope of logic proper for him. Boolos observes that statements of validity of some Π_1 sentences, in turn, are themselves Π_2 (one uninterrupted sequence of second-order universal quantifiers, followed by a such a sequence of existentials, followed again by a such sequences of universals). Thus, if statements of the validity of logical truths have to be logical truths themselves, there is no stopping at Π_1, as Putnam wishes; we soon have to go beyond the Σ_1 sentences that Putnam wants to rule out. For details see (Boolos, 1994), pp. 238–242.

‘that’, and ‘who’, which correspond to variables in formal languages.

What the example sentences (1) and (6) share in common is that they make use of plural devices of cross-reference, like ‘them’ instead of ‘it’. That this is the case is, according to Boolos, hidden in (1), and only shows up when one tries to formalise the sentence. It becomes perfectly clear, however, in the step from (4) to (6); in (6) merely the singular word forms that occur in (4) are replaced by their plural versions. If (2) and (7) indeed display the genuinely *logical* form of (1) and (6), respectively, this should show up somehow in the logical inferences that these sentences are involved in. It should be the case, for example, that some of the logical consequences that can drawn from (1) cannot be captured and shown to follow using a formalisation that is not broken down far enough in logical structure, like the caricaturesque (8) as a formalisation for (1). Boolos presents a further example to make this point:

(a) If there are some persons of whom each parent of any one of them is also one, then if each parent of Yolanda is one of them, Xavier is one of them; and someone is a parent of Yolanda.

(b) Every parent of someone red is blue.

(c) Every parent of someone blue is red.

(d) Yolanda is blue.

(e) Therefore, Xavier is either red or blue.

After a minute of thought[19] the argument strikes one as logically valid. After a further minute the reader familiar with Gottlob Frege’s work recognises that premise

[19](Boolos, 1984b), p. 60, guides his readers through the argument.

(a) defines 'ancestor'. One of Frege's the major achievements of part 3 of his *Begriffsschrift*, (Frege, 1879). is the definition of the *ancestral* of a relation. The definition of the ancestral is usually taken to be in terms of classes. As Boolos puts it for 'parent' and its ancestral 'ancestor':

> Frege's definition of 'x is an ancestor of y' is: x is in every class that contains y's parents and also contains the parents of every member.[20]

There is no need, however, to mention classes in the definition of the ancestral of a relation. The definition of the (weak) ancestral R_* of a relation R is achieved in second-order logic like this:

(Def-R_*) $$R_*(x,y) =_{\mathrm{df}} \forall F([Fy \wedge \forall w \forall z((Fz \wedge Rwz) \supset Fw)] \supset Fx)$$

Boolos aims at making plausible that the second-order quantifier in the Fregean definition of the ancestral corresponds to the use of plural pronouns and quantifier phrases in English, and hence no commitment to classes arises at all, despite Quine's attempts to argue so.[21] Using Frege's definition of 'ancestor' (as the ancestral of 'parent') we can identify premise (a) in the argument above with the statement 'Xavier is an ancestor of Yolanda'.[22] Then, the argument can be formalised like this:

(a′) $P_*(x,y)$

(b′) $\forall w \forall z((Pzw \wedge Rw) \supset Bz)$

[20](Boolos, 1985a), p. 73.
[21]See (Quine, 1986a), pp. 64–66, and my chapter 3.
[22](Boolos, 1994), p. 241.

(c′) $\forall w \forall z((Pzw \wedge Bw) \supset Rz)$

(d′) By

(e′) $\therefore Rx \vee Bx$

where '$P_*(x, y)$' in premise (a′) abbreviates its second-order definition. The unpacked sentence is a direct formalisation of (a) in second-order logic. (As Boolos remarks, the 'there are' in the antecedent of (a) expresses universal quantification, "as does the 'there is' in 'If there is a logician present, he should leave.'"[23]) The inference is inherently second-order. It cannot be recast in pure first-order logic; but perhaps even more then (1) and (6), (a) does not give the appearance that we are secretly quantifying over sets.

4.2 The Plural Interpretation

Let us come back to second-order set theory. The view that second-order quantifiers range over sets, viz. subsets of the (first-order) domain, leads into problems when the domain contains all sets and hence does not form a set itself. Second-order quantification would thus not be available in the case of set theory.

> The principle drawback [...] is that there are certain assertions about sets that we wish to make, which certainly cannot be made by means of a first-order formula [...] but which, it appears, could be expressed by means of a second-order formula if only it were permissible so to express them. To declare it illegitimate to use second-order formulas in discourse

[23](Boolos, 1984b), p. 60.

about all sets deprives second-order logic of its utility in an area in which it might have been expected to be of considerable value.[24]

Boolos has statements in mind like the separation axiom. In first-order set theory, separation can only be used as an axiom schema. Boolos considers this as unsatisfactory. The reason, according to Boolos, why we accept the first-order schema of separation, is because we accept (implicitly?) its second-order universal closure '$\forall X \forall z \exists y \forall x (x \in y \equiv [x \in z \wedge Xx])$'.[25] The flight to proper classes is unacceptable to Boolos, as mentioned in the beginning of this chapter. Thus, he proposes a plural interpretation of the second-order quantifiers that makes use of the devices of cross-reference available in English – e.g., plural pronouns – and avoids the use of classes.[26]

In order to do so unambiguously, English has to be augmented with additional devices for cross-reference in the form of indices for expressions like 'it', 'that', and 'them'. Boolos uses letters as subscripts, as in 'it_x', 'that_y', 'them_X', 'that_X', 'them_Y', etc. He submits that this "extension" of the fragment of English that is required for the plural interpretation is innocuous:

> I want to emphasize that the addition to English of operators '$\text{it}_{()}$', '$\text{that}_{()}$', '$\text{them}_{()}$', etc. or variables 'x', 'X', 'y', etc. is not contemplated here. The 'x' of 'it_x' is not a variable but an index, analogous to 'latter'

[24](Boolos, 1984b), p. 60.

[25]This thought can also be found in (Kreisel, 1967), p. 147.

[26]The interaction between plural quantification and set theory is investigated in detail by (Uzquiano, 2003), (Burgess, 2004), and (Lewis, 1991); in the case of Lewis, also mereology is used. (For a discussion of Lewis' construction see my section 9.3 in the forthcoming (Cohnitz and Rossberg, 2006).) A discussion of these is omitted here as the *philosophical* question at hand is whether a plural interpretation of the second-order quantifiers can serve to legitimise second-order logic and rid it of (additional) ontological commitment to sets. The *technical* question of what can be achieved using plural quantification in set theory is outside the scope of this thesis.

in 'the latter', or 'seventeen' in 'party of the seventeenth part'; 'X' and 'x' in 'them$_X$' and 'it$_x$' no more have *ranges* or *domains* than does '17' in 'x_{17}'. We could just as well have translated the language of second-order set theory into English augmented with pronouns such as 'it$_{17}$', 'them$_{1879}$', etc. or an elaboration of the 'former' / 'latter' usage. Note also that such augmentation will be needed for the translation into English of the language of *first*-order set theory as well.[27]

The "translation manual" for the translation from second-order set theory into augmented English, then, looks as follows. Where 'V' and 'v' are dummies for second- and first-order variables, respectively, that for determinacy lend their typographical shape to the indices of augmented English, translate:[28]

- $\ulcorner Vv \urcorner$ as $\ulcorner$it$_v$ is one of them$_V$$\urcorner$;
- $\ulcorner v \in v' \urcorner$ as $\ulcorner$it$_v$ is a member of it$_{v'}$$\urcorner$;
- $\ulcorner v = v' \urcorner$ as $\ulcorner$it$_v$ is identical with it$_{v'}$$\urcorner$;
- '$\neg$' as 'it is not the case that';
- '$\wedge$' as 'and' (use similarly obvious translations for the other truth-functional connectives, or reduce those in the common fashion to '$\neg$' and '$\wedge$');
- where φ^* is the translation of φ, translate $\ulcorner \exists v \varphi v \urcorner$ as $\ulcorner$there is a set that$_v$ is such that $\varphi^* \urcorner$

[27](Boolos, 1984b), p. 68; all emphases are Boolos'.

[28]See (Boolos, 1984b), pp. 67–68, or (Boolos, 1985b), pp. 165–166.

If such a translation is wanted for a second-order language that does not include set theory, the latter clause can be straightforwardly mended: the translation, then, reads more generally: $\ulcorner$there is something that$_v$ is such that $\varphi^*\urcorner$.

The translation of sentences of the form $\ulcorner\exists V\varphi(V)\urcorner$ is not quite as straightforward. The problem that arises is that in the standard semantics one can assign the empty extension (or the empty set) to predicates, hence second-order variables need to have it in their range (speaking from the standpoint of standard semantics, again). The sentence '$\exists X\neg\exists xXx$', for instance, is valid in second-order logic and hence second-order set theory. This has to be respected by Boolos "translation manual", too. 'There are some sets such that no set is one of them', however, turns the valid '$\exists X\neg\exists xXx$' into a falsehood. Thus, Boolos inserts an "escape clause" into the translation that accounts for the empty extension of standard semantics. (Note also, that Boolos' plurals have to be understood as "logicians' plural", in the sense that 'there are some Fs' is taken to be true if there is only *one* F, despite the inclination to think that there should be at least two.) The final clause of the translation reads thus:

- Let φ^* be the translation of φ, and let φ^{**} be the translation of the result of substituting an occurrence of $\ulcorner\neg v = v\urcorner$ for each occurrence of $\ulcorner Vv\urcorner$ in φ. Then translate $\ulcorner\exists V\varphi\urcorner$ as $\ulcorner$either there are some sets that$_V$ are such that φ^* or $\varphi^{**}\urcorner$.

In the context of non-set-theoretical discourse, this clause can be mended again analogous to the way described above for the first-order existential quantifier.

Given this disjunctive translation of the second-order quantifier, the second-order validity '$\exists X\neg\exists xXx$' comes out as the true sentence 'either there are some sets that$_X$

are such that it is not the case that there is a set that$_x$ is such that it$_x$ is one of them$_X$ or it is not the case that there is a set that$_x$ is such that it is not that case that it$_x$ is identical to it$_x$'. Put in a more colloquial way this says, 'either there are some sets such that no set is one of them or no set is not self-identical'. The second disjunct is true, and hence the disjunction is.

Let us take a second-order set theoretical statement, '$\exists X \forall x(Xx \equiv \neg x \in x)$', as a further example to see how Boolos' translation works. The translation reads: 'either there are some sets that$_X$ are such that every set is such that it$_x$ is one of them$_X$ if, and only if, it is not the case that it$_x$ is a member of it$_x$ or every set is such that it is not the case that it$_x$ is identical to it$_x$ if, and only if, it is not the case that it$_x$ is a member of it$_x$'. If we paraphrase this sentence into the slightly more elegant English sentence, 'either there are some sets such that every set is one of them if, and only if, it is not a member of itself, or every set is a member of itself', it is easy to see that the first disjunct is true (it plurally refers to all sets), and hence the disjunction is true, too.

It should be noted that I sneakingly inserted a translation for the universal quantifier for the examples just mentioned. Boolos' translation manual has no clause for universal quantifiers, though, not for first-, and especially not for second-order ones. (Boolos, incidentally, smuggles a translation for the first-order universal quantifier in, too.) When he first proposed the translation, Boolos believed that there was no non-artificial way to translate the second-order universal quantifiers, and so resorted to the instruction to exchange all occurrences of $\ulcorner \forall V \urcorner$ for $\ulcorner \neg \exists V \neg \urcorner$.[29] David Lewis later suggested that 'No matter what some things may be they$_X$ are such that' is

[29](Boolos, 1984b), p. 68. Michael Resnik complains about this lack of translation instruction for second-order universal quantifiers in (Resnik, 1988), pp. 86–87.

a perfectly acceptable rendering of '$\forall X$'.[30] For simplicity's sake we can stick to the universal-quantifier-avoiding version, though.

Boolos claim is that this translation shows that sentences like the Geach-Kaplan sentence (1), 'Some critics admire only one another', do not bring about any ontological commitment to sets when formalised in second-order logic, as in (2), contrary to how Quine would have it:

> [D]oes asserting [(1)] commit one to the existence of a non-empty class of critics? The difficulty with the question is that the ground rules for answering it appear to have been laid down, by Professor Quine.[31]
>
> [W]hat makes first-order logic the touchstone by which the ontological or existential commitment of [such] statements are to be assessed? The statements do not appear to commit us to classes; why believe that it is their translation into the notation of first-order logic augmented with variables ranging over classes that determines what they are actually committed to?[32]

Boolos takes the plural interpretation of the second-order variables to show that no additional ontological commitment is brought about by second-order logic. Ontological commitment occurs at first order; second-order variables do not range over a new kind of objects – e.g., classes – but rather refer to the same old objects in a new way, viz. plurally.[33]

[30] See e.g. (Lewis, 1991), p. 74; compare also (Boolos, 1994), pp. 242-243.

[31] (Boolos, 1985a), p. 76; compare chapters 3 and 5 of this thesis for Quine's criterion of ontological commitment.

[32] (Boolos, 1985a), p. 78. A point similar to this is made and discussed in detail in the following chapter, chapter 5, of this thesis.

[33] (Boolos, 1984b), p. 72.

The plural expressions that are used to interpret the second-order quantifiers are just the plurals of ordinary English which every competent speaker of English understands. Thus, Boolos claims, he provides a

> coherent and intelligible way of interpreting such second-order formulas [...] even when the first-order variables in these formulas are construed as ranging over all sets or set-like objects there are. The interpretation is given by translating them into the language we speak [...]. It cannot seriously be maintained that we do not *understand* these statements [...].[34]

Section 4.4 below will discuss how this last one of Boolos' statements concerning the plural interpretation of the second-order quantifiers has been challenged. Before I turn to that, however, a fairly important omission in the "translation manual" has to be noted.

4.3 Polyadic Predicates

In his *1879?* (Boolos, 1994) Boolos credits Hartry Field with being the first to notice that the plural translation of the second-order quantifiers only works for *monadic* second-order logic, i.e. where the quantified second-order variable takes the place of a one-place predicate.[35] As early as his *Nominalist Platonism* (Boolos, 1985a), however, he suggested that in "many of the most important applications of second-order logic, a pairing function will be available and monadic variables can then be made to do the work of all second-order variables."[36] So, if one-place predicate

[34](Boolos, 1984b), p. 69.
[35](Boolos, 1994), p. 243.
[36](Boolos, 1985a), pp. 75–76, fn. 3.

variables refer plurally to objects, two-place predicate variables refer plurally to ordered *pairs* of such objects for Boolos, three-place predicate variables to ordered *triples*, and so on. The "many of the most important applications" that Boolos has in mind are mathematical theories such as set theory or arithmetic. The mathematical resources available there are sufficient to define a pairing function, and the technical difficulty is elegantly circumvented, as long as enough mathematics is available.[37]

John Burgess, Allen Hazen and David Lewis showed in an "Appendix on Pairing" to Lewis' *Parts of Classes*[38] how such a pairing function can be constructed without using mathematics – or at least without overtly using mathematics. In *Parts of Classes* (Lewis, 1991), Lewis reconstructs Zermelo-Fraenkel set theory in a system he calls *Megethology*. Megethology is basically a mereology that is strengthened by monadic second-order quantification – plurally construed, following Boolos – and some choice principles. Since Lewis wants to reconstruct set theory (and with it the whole of mathematics in what he claims to be a nominalistically acceptable way, or at least as close as one can sensibly get to it), he cannot use the set-theoretical definition of an ordered pair (or any other mathematical definition) to simulate polyadic second-order quantification. Burgess, Hazen and Lewis use clever coding tricks that only require what is anyway available in Megethology to construct a pairing function.[39] They have to assume, however, that there are infinitely many objects for this coding to work. In the context of Lewis construction this is not problematic – in a sense – as Lewis has to assume the universe to be inaccessibly

[37]A pairing function in set theory is given by the well-known '$f(x, y) = \{\{x\}, \{x, y\}\}$'; in arithmetic, a pairing function can be defined as '$g(x, y) = 2^x \cdot 3^y$', for example.

[38](Burgess et al., 1991).

[39]The details are omitted here. We will see a little further below why this way out is unattractive anyway.

large anyway for other reasons.[40]

That Burgess, Hazen and Lewis have to assume the infinity of the domain is hardly surprising: any domain that is closed under pairing is infinite.[41] To rely on this assumption, however, sits uneasily with Boolos' claim that second-order quantification does not involve any ontological commitment that is not already present in the first-order case. Resnik notes in this context that while defining a pairing function might come free in cases where enough mathematics is available anyway, in theories where devices for forming pairs (or, more generally, tuples of arbitrarily large degree) are not available, the ontological commitment is pumped up in a significant way.[42] A general, ontologically neutral, solution for this problem is necessary for Boolos' plural interpretation to be successful.

Agustín Rayo and Stephen Yablo attempt such a solution.[43] They utilise an idea taken from Arthur Prior.[44] Prior contends that

> we form colloquial quantifiers [...] 'however', 'somehow', 'wherever' [...] [and] no grammarian would count 'somehow' as anything but an adverb,

[40]Lewis defines sets (except for the empty set) as mereological sums (or fusions) of singletons. He hence only has to show that everything has a singleton, and mereological fusion will take care of all other sets. Lewis takes it to be sufficient for his project to show that some singleton function exists, and it can be shown that such a function is guaranteed to exist if the universe is of inaccessible cardinality. For a discussion of Lewis' project and especially a criticism of the Burgess-Hazen-Lewis pairing construction see my forthcoming (Cohnitz and Rossberg, 2006), section 9.3. – (Hazen, 1997b), shows how polyadic second-order quantification can be simulated in monadic *third*-order logic. This will not help here, however, even if we ignore for the moment that Boolos' plural translation gives us no handle whatsoever on quantification that is higher than *second*-order (see (Boolos, 1994), p. 244). A further problem is, as also shown by Hazen in his paper, that the Axiom of Choice is needed for this simulation. Since the Axiom of Choice, presumably, is existentially committing, the result is of little use for an ontologically neutral construal of polyadic second-order quantification, despite its being of interest for other contexts. See also the discussion of this in (Hazen, 1997a).

[41](Shapiro, 1991), p. 63.

[42](Resnik, 1988), p. 86.

[43](Rayo and Yablo, 2001).

[44](Prior, 1971), chapter 2, esp. p. 35–38.

> functioning in 'I hurt him somehow' exactly as the adverbial phrase 'by treading on his toe' does in 'I hurt him by treading on his toe' [...]. [This] is simply to extend the use of the "thing" quantifiers in a perfectly well-understood way, as in 'He is something that I am not – kind' [...] 'something' here is quite clearly adjectival rather than nominal in force.[45]

Rayo and Yablo claim that "non-nominal quantifiers – quantifiers like 'somehow' and adjectival 'something' – carry no commitments."[46] (It was, incidentally, already noted by Peter Simons that Prior's arguments might be of interest in the debate about ontologically noncommittal interpretations of higher-order quantification.[47]) Rayo and Yablo present four arguments (of allegedly increasing "weight") for this claim:[48]

- *Argument from Instances*: Use of a quantifier commits one at most to entities of the kind referred to by the phrases its bound variables stand in for. The phrases a non-nominal variable stands in for – phrases like 'by treading on him', and 'kind' – do not refer at all. So non-nominal quantifiers carry no commitment.

- *Argument from Entailment*: Suppose that 'I hurt him somehow' were committed to entities beyond those presupposed by 'I hurt him by treading on him', that is, me and him, and (maybe) my foot. Then 'I hurt him somehow' would not be trivially entailed by 'I hurt him by treading on him' – because it is no trivial matter whether these additional entities exist. 'I hurt him somehow' *is*

[45](Prior, 1971), p. 37.
[46](Rayo and Yablo, 2001), p. 81.
[47](Simons, 1993), p. 261, and (Simons, 1997), p. 264.
[48]Quoted verbatim from (Rayo and Yablo, 2001), pp. 81–82.

however trivially entailed by 'I hurt him by treading on him'. So there is no additional commitment. [...]

- *Argument from Consistency*: Suppose that 'something' in 'a is something that b is too' carried commitment to BLAHs [sic] – properties, say, or sets. Then to say 'a is something that b is too, but there are no properties or sets to witness the fact' would be self-undermining. And in general it isn't. Sometimes indeed the claim is importantly true: 'a is something that b is too, viz. not a member of itself, but as we know from Russell's paradox there is no witnessing set'. [...]

- *Argument from Cardinality*: By Cantor's theorem, every domain contains objects $x, y, z, ...$ such that no domain element contains all and only those objects. Another way to put it is that the following is mathematically impossible:

  (i) take any objects you like, there's an object containing them and nothing else.

  But it is not at all impossible – it is on one reading quite *true*

  (ii) take any objects you like, they are something that the rest of the objects are not.

  The die-hard objectualist might try to construe the 'something' in (ii) in terms of container-objects somehow eluding the grasp of the initial 'any objects you like'. But this escape hatch can be closed by stipulating that the initial quantifier is absolutely universal. Not only does this stipulation fail to make (ii) look any less consistent, (ii) continues to look *true*. It could not be true on the stipulated reading if 'something' had ontological import.

Rayo and Yablo continue to give examples how, using this idea, inferences that look like they should be formalised using polyadic second-order quantifiers, can be captured using these adverbial expressions, much in the fashion of Boolos' plural interpretation. For instance, from the sentence

(10) Connecticut is related to Delaware in that the former is larger than the latter; Texas is so (thus, likewise) related to Nebraska.

it follows

(11) Connecticut is related to Delaware somehow such that Texas is so related to Nebraska.

(10) and (11) are straightforwardly formalised, using 'L' to stand for '... is larger than ...' and the name letters in the obvious way, as

(10′) $Lcd \wedge Ltn$

(11′) $\exists R(Rcd \wedge Rtn)$

It would be tempting now to suggest a translation or $\ulcorner \exists R...Rxy... \urcorner$ as $\ulcorner$something$_x$ is related to something$_y$ somehow such that ... x is so related to y ...$\urcorner$. This however runs into the problem that the second-order logical truth

(12) $\exists R \forall x \forall y \neg Rxy$

comes out as something like 'something is related to something somehow such that no objects are so related', which is false.[49] Rayo and Yablo therefore insert an escape

[49](Rayo and Yablo, 2001), p. 83.

clause that is taking care of the "empty relation" in a way analogous to Boolos' for the plural interpretation on the monadic second-order quantifiers. $\ulcorner \exists R ... Rxy ... \urcorner$ will be interpreted as $\ulcorner$something is-or-isn't related to something somehow such that ... x is so related to y ...$\urcorner$.

(12) thus is interpreted as saying that something "is-or-isn't related to something somehow such that no objects are so related".[50] Rayo and Yablo take this to be the solution. Presumably they are able to understand this sentence, that some people who are not used to such formulation might struggle to make sense of. The authors admit the difficulties in parsing this translation and suggest an "abbreviation" of the garbled translation to 'somehow things relate such that...'.[51] Taking for granted that this expresses the same thought for the sake of the exposition, a Rayo-Yablo translation function 'Tr' for dyadic second-order logic is defined recursively like this (the symbolisation conventions are slightly adjusted in order to fit those used for Boolos' translation manual; '$\frown$' denotes concatenation):

- $\mathrm{Tr}(\ulcorner \neg\varphi \urcorner)$ = 'it is not the case that'$\frown\mathrm{Tr}(\ulcorner \varphi \urcorner)$
- $\mathrm{Tr}(\ulcorner \varphi \wedge \psi \urcorner)$ = $\mathrm{Tr}(\ulcorner \varphi \urcorner)\frown$'and'$\frown\mathrm{Tr}(\ulcorner \psi \urcorner)$
- $\mathrm{Tr}(\ulcorner \exists v\varphi \urcorner)$ = $\ulcorner$something$_v$ is such that$\urcorner\frown\mathrm{Tr}(\ulcorner \varphi \urcorner)$
- $\mathrm{Tr}(\ulcorner \exists R\varphi \urcorner)$ = $\ulcorner$somehow$_R$ things relate such that$\urcorner\frown\mathrm{Tr}(\ulcorner \varphi \urcorner)$
- $\mathrm{Tr}(\ulcorner Rvv' \urcorner)$ = $\ulcorner$it$_v$ is so$_R$ related to it$_{v'}\urcorner$

Rayo and Yablo express faith in the unproblematic extendibility of this translation to the case of predicate variables with degree greater than 2, i.e. for n-adic predicate

[50](Rayo and Yablo, 2001), p. 84.
[51](Rayo and Yablo, 2001), p. 84.

variables for $n > 2$. They also discuss the interaction of their translation with Boolos' plural interpretation of the monadic second-order quantifiers, and some strategies for incorporating a translation of monadic predicate variables into their translation. The details need not concern us here. The basic idea behind their approach should be clear.

In the next section, where further criticisms of Boolos' plural interpretation are discussed, we will also see that the problems the Boolos strategy runs into arise if anything to an even graver extent for the Rayo-Yablo translation for dyadic (or polyadic) second-order logic.

4.4 Still Wild

It first has to be noted that Boolos' argument for not only the legitimacy, but also the (near) indispensability of second-order quantification, plurally construed, for the domain of set theory, rests on his rejection of proper classes and his insistence on absolutely unrestricted (first-order) quantification. In both cases there is room to demur.

Boolos insists that set theory is supposed to be a theory of *all* collections, and that there are therefore no collections or set-like entities that are not sets.[52] If we want to speak about *all* of them, as we often do, the device to do so must be plural, as there is no set of all sets.

> Other possible responses, of course, are to admit a hierarchy of classes, superclasses, superduperclasses, etc., to claim that, actually, we can't talk about everything at once, to invoke some doctrine of typical or

[52] See (Boolos, 1974), p. 36; (Boolos, 1994), pp. 239–240.

> systematic ambiguity, or to mutter something about a ladder.[53]

Michael Resnik, for example, points out that all the sentences of second-order set theory that Boolos argues turn out contradictory if the second-order quantifiers are interpreted as ranging over sets although second-order logic declares them to be valid, can just as well be formulated using proper classes (or "ultimate classes", as Resnik calls them following Quine[54]). So interpreted, the sentences come out correct. Concerning this argument, we have therefore to attest a "clash of intuitions", a stand-off of opinions. If proper classes etc. are rejected, plurals might be the only way out. One man's *modus ponens* is the other man's *modus tollens*, however. If someone just will not admit second-order quantifiers that do not range over higher-order entities like classes, and finds plural quantification unacceptable, then she can just as well draw the conclusion that, no: set theory is not a theory about *all* collections, and there are collections that are not sets, or that we just *cannot* speak absolutely unrestrictedly *for this reason*.

Let us now consider Boolos' arguments that sentences like the Geach-Kaplan sentence (1) (far above) cannot be formalised in pure first-order logic, but can be formalised in second-order logic. Since (1) is obviously not committed to sets, however, it cannot be right that second-order quantifiers have to be interpreted as ranging over sets. Resnik remarks that in opposition to Boolos' claims, (1) is usually taken to be an example for the fact that natural language sometimes masks ontological commitment, and that thus "Boolos has turned the Geach-Kaplan [sentence] on it head."[55]

While (1) does not mention classes on the surface, Resnik nevertheless holds that

[53] (Boolos, 1994), p. 241.
[54] (Resnik, 1988), pp. 78–80; for the term 'ultimate class' see (Quine, 1969b), p. 3.
[55] (Resnik, 1988), p. 84.

he is "inclined to understand it as saying:

(1′) There is a non-empty collection of critics each member of which admires no one but another member."[56]

A Boolosian interpretation of (1), or its formalisation (2) (see far above), on the other hand, looks like this:

(1″) "There are some critics such that any *one of them* admires another critic only if the latter is *one of them* distinct from the former."[57]

Far from conceding, in light of this translation into English plural talk, Resnik rejoins:

> [T]his sentence seems to me to refer to collections quite explicitly. How else are we to understand the phrase 'one of them' other than referring to some collection and as saying that the referent of 'one' belongs to it?
>
> Of course, if we render [(2)] and [(1″)] as [(1)], we can avoid the problematic phrase 'one of them', and see that [(1″)] is just an awkward way of putting [(1)]. But I have two worries with this way out. First, we have no assurance that we can always smooth out Boolos's translations in this way. Second, I started out by worrying about the ontic commitment of [(1)]. The bucket was passed to [(2)] and thence to [(1″)]. If [(1″)] is just an awkward way of saying [(1)], we have come full circle.[58]

[56](Resnik, 1988), p. 77.
[57](Resnik, 1988), p. 77; Resnik's emphases.
[58](Resnik, 1988), p. 77–78.

Again, we are faced with a stand-off between Boolos and Resnik – and a stand-off at best, for Boolos' critics can strengthen their position. To see how, let us first consider the support that Stewart Shapiro offers for Resnik's claims:

> whatever understanding we do have of plural quantifiers is mediated by our understanding of sets and classes.[59]

This is at least the case, as Shapiro qualifies later, if the plural quantifications are meant to be rigorous enough, and "sufficiently determinate", for their use in mathematics:

> Consider a statement of second-order real analysis of the form
>
> $\forall X \exists Y \Phi(X, Y)$
>
> The opening second-order quantifier can be given both a plural and an ordinary, standard reading. It had better be the case that if we read the quantifiers as plurals, we will get exactly the same truth-value, in general, as we would if we understand the quantifiers as ranging over sets of real numbers. If effect, there needs to be a "plurality" (so to speak) for each set of real numbers. Does the English plural construction have that determinate meaning?[60]

The same sentiment is expressed by Allen Hazen[61] and Ignazio Jané.[62] It is tempting, however, to think that Resnik, Shapiro, Hazen and Jané here put the cart before horse. The understanding of 'set' that is relevant here is presumably the one that is conveyed by the iterative conception of set or the axioms of ZF. In either case,

[59] (Shapiro, 1993), p. 472.
[60] (Shapiro, 2005a), p. 764.
[61] (Hazen, 1993), pp. 138-139.
[62] (Jané, 2005), pp. 805–807.

it is a highly technical understanding. Is the understanding of plural constructions in English not prior to that? Presumably, a child asking her parents for some of the sweets in the bowl does not need a grasp of the iterative hierarchy or ZF to competently use the required plural phrase to express her desire.[63] As Boolos put it: "The interpretation is given by translating them into the language we speak [...]. It cannot seriously be maintained that we do not *understand* these statements".[64]

The concern expressed by Boolos' critics mentioned above, however, is with the determinacy of plural constructions. The formal system of second-order logic with its standard formal semantics (or a Henkin semantics, for that matter) guarantees a rigour that ordinary language lacks – this is the whole point about formal languages if we follow Frege. The determinacy of the second-order quantifiers is meant to be guaranteed by the formal semantics, too.[65] Relying on an ordinary language notion in the interpretation (or understanding) of this formal system appears to be a step in the wrong direction.

If anything, the Rayo-Yablo translation fares even worse on this count. The sentences that come out of the translation function are difficult enough to understand as they are. How can we decide whether constructions involving 'somehow things relate such that' is not committed to any additional entities? Rayo and Yablo make a decent case that the 'somehow' is referentially innocuous, but what about 'things' – do we need another plural construction to cash that out? – or what about the mention that things *relate*? Can this really be understood independently of a

[63]Pieces of rhetoric found in Boolos like "It is haywire to think that when you have some Cheerios, you are eating a *set* – what you're doing is: eating THE CHEERIOS", (Boolos, 1984b), p. 72, suggest that something along these lines might not be too far off his argument advanced in favour of plurals.

[64](Boolos, 1984b), p. 69.

[65]For a critical discussion of the claim that formal semantics guarantee determinacy see my chapter 7.

developed theory of relations (which pumps up our ontological commitments)? If we can have some grasp just from the use of 'relate' in ordinary English, will it be precise and determinate enough for the purposes of higher mathematical theories? It does not even seem to be clear beyond doubt that the order of arguments is preserved in the natural language rendering. It seems clear, in any case, that if the case against Boolos' plural interpretation can be made, Rayo and Yablo's translation falls with it.

Rayo's precisification of a logic of plurals in (Rayo, 2002) and his associated interpretation of the second-order quantifiers might come as a rescue, at least for the monadic case. Rayo formulates a *plural first-order language* (PFO) that in addition to the usual first-order terminology has plural variables, written as 'xx', 'xx_1', 'yy', etc., and plural quantifiers '$\exists xx$', '$\exists xx_1$', etc. (in order not to confuse these plural variables with the pair of singular variables that take the argument places in a dyadic predicate, I will in this section always use brackets and commas for the case of (monadic as well as polyadic) predication as in '$F(x)$' or '$R(x, y)$'; 'xx_i' is just *one* variable that refers plurally to objects in the first-order domain – it cannot be split up). The language also contains a binary constant '$\prec$' which takes an ordinary singular variable as first, and a plural variable as second argument. The intended interpretation of $\ulcorner x_i \prec xx_j \urcorner$ is $\ulcorner \text{it}_i$ is one of $\text{them}_j \urcorner$ (note, to avoid confusion, that the conventions for indexing here vary from Boolos', albeit in an unsubstantial way).[66] The Geach-Kaplan sentence (1) – see section 4.1 of this chapter – is thus formalised in PFO as

(13) $$\exists xx \forall y \forall z[(y \prec xx \wedge A(y, z)) \supset (y \neq z \wedge z \prec xx)]$$

[66](Rayo, 2002), p. 442.

Note that Rayo's plurals, just like Boolos' plurals, admit of just one object counting as some *things*.

Next, Rayo enriches PFO by plural predicates like $\ulcorner\textsc{Scattered}(xx_i)\urcorner$ (for $\ulcorner$they$_i$ are scattered$\urcorner$; a *one*-place prediacte that takes a plural variable), or $\ulcorner\textsc{Surrounded}(x_i, xx_j)\urcorner$ (for $\ulcorner$it$_i$ is surrounded by them$_j\urcorner$; a two-place predicate that take a singular variable as first, and a plural variable as second argument). Such plural predicates appear desirable in light of examples like

(14) The boys carried a piano.

which is intended to say that the boys did the carrying *together*, and not that each of the boys carried a piano on his own. Other examples are easy to find: think of seashells that are scattered on the beach, musicians performing a symphony, Russell and Whitehead writing the *Principia Mathematica*, the Peano Axioms implying *Goldbach's Conjecture*, Bob Dylan being surrounded by fans, etc. Call the thus enriched language 'PFO$^+$'.

The one-predicates of PFO$^+$ are divided into plural and singular predicates, and something analogous is true of the polyadic predicates. (For simplicity's sake I will stick to one-place predicates in the following. The generalisation to many-place predicates is straightforward.) This means that PFO$^+$ has two predicates for carrying a piano, for example, 'CarryAPiano' and '*CarriesAPiano*',[67] if you will, for singular and plural predicates, respectively. Intuitively, so Rayo allows,[68] the sentences (14) and

[67] I use *italics* for singular predicates (following the standard of ordinary predicate logic) and small capitals for plural predicates; (Rayo, 2002) uses small capitals for singular predicates and **boldface** for plural predicates.

[68] (Rayo, 2002), p. 451.

(15) Samson carried a piano.

should, nevertheless, employ the same predicate 'carried a piano'. The formalisations of (14) and (15) show that PFO^+ does not respect this intuition:

(14′) $\exists xx(\forall y[y \prec xx \equiv Boy(y)] \wedge \textsc{CarriedAPiano}(xx))$

(15′) $CarriedAPiano(s)$

To rectify this, Rayo defines plural counterparts of singular predicates. For a singular predicate '$F(x)$' define its *plural counterpart*, '$\mathrm{F}^\dagger(xx)$', to be:[69]

$$\text{(Def-F}^\dagger\text{)} \qquad \forall x \forall yy[\forall z(z \prec yy \equiv z = x) \supset (\mathrm{F}^\dagger(yy) \equiv F(x))]$$

Rayo then introduces a further binary constant symbol '$\precsim$' that takes two plural arguments, such that $\ulcorner xx_i \precsim xx_j \urcorner$ has the intended interpretation $\ulcorner$they$_i$ are some of them$_j\urcorner$, and defines the plural predicate '$\mathbf{1}(xx)$' as

$$\text{(Def-}\mathbf{1}\text{)} \qquad \mathbf{1}(xx) =_{\mathrm{df}} \forall yy(yy \precsim xx \supset xx \precsim yy)$$

i.e. '$\mathbf{1}$' is true of some objects if, and only if, there is exactly one of them. This provides enough resources to formulate PFO^+ using only plural predicates (and no singular ones) and '$\precsim$' (instead of '$\prec$') without loss of expressive power. Indeed, Rayo shows the two formulations to be equivalent.[70]

[69] (Rayo, 2002), p. 452. Rayo uses '$\mathbf{F}^*$' for plural counterparts, rather than '$\mathrm{F}^\dagger$'; in order to rule out any possible confusion with the ancestral, as in 'R_*', I avoid his notation here.

[70] (Rayo, 2002), pp. 452–453; the details are omitted here.

Rayo uses PFO$^+$ to give a formal theory of truth for second-order logic.[71] The Boolos-inspired translation of monadic second-order logic into plural logic, however, makes use only of PFO. It uses the "definitional equivalencies"

- $X_i(x_j) \equiv_{\text{df}} x_j \prec xx_i$
- $\exists X_i(\varphi) \equiv_{\text{df}} \exists xx_i(\varphi) \vee \varphi^*$
  where φ^* is the result of substituting $\ulcorner x_j \neq x_j \urcorner$ everywhere in φ for $\ulcorner X_i(x_j) \urcorner$[72]

It is clear how this interpretation of monadic second-order logic in PFO is the exact analogue of Boolos' natural language plural interpretation. Rayo spells out the latter formally, and thus – to come back to the original question – might be taken to provide a rigorisation of Boolos' interpretation that was found lacking by Resnik, Shapiro, Hazen, and Jané, as noted above.

What has happened, however, and how does it help? The original question was how to understand formal second-order languages (without interpreting them as some form of class theory). Boolos provided an informal translation using plural constructions of English. This was found wanting in rigour and structure, and subject to the vagaries of natural language. Now Rayo has introduced a *new* formal language to make the translation more rigorous. So, the obvious next question is: how do we understand *that* formal language. It is not even as well-studied as second-order logic in its proof-theoretic properties. It seems that we have traded in a fairly well understood logic for a brand-new one that is not yet sufficiently explored and understood.

[71] (Rayo, 2002), appendix 4; the details are omitted here.
[72] (Rayo, 2002), p. 455.

Two ways seem to be open. Both, however, appear undesirable. Firstly, one could take the intended interpretation of PFO (and PFO^{+}) to be a natural language semantics. On this horn of the dilemma, however, no progress is made compared to Boolos' original interpretation. We have merely added a further, unnecessary level of languages. The second option is to give a formal semantics or interpretation to PFO. There will certainly be a set-theoretic semantics for PFO, but nothing is gained if it is done like this. Rayo toys with the idea of a atomistic mereological semantics for PFO^{+}.[73] "Pluralities" might become reified in the guise of mereological sums on this approach. But since by Cantor's Theorem there are more "pluralities" then objects for any given domain, the claim that no additional ontology is implicit in second-order quantification, again, could not be upheld. Analogously, for n atoms there are $2^n - 1$ entities formed by mereological summation – basically the powerset of the domain, but barring the empty set, as classical mereologies dispense with a "null sum". Going down this route, it appears more and more likely that our understanding of plurals is indeed mediated by our understanding of classes as Boolos' critics submit.[74]

Also, we are thrown back into the circle sketched above: how is the mereological interpretation (or any other formal semantics, for that matter) going to be given? If one would proceed informally, the imprecision worry would rise its head again. Using an interpretation given by means of a formal system on the other hand – indeed, Rayo suggests '$\lhd$' for a notion of belonging to a plurality, mereologically construed,

[73](Rayo, 2002), p. 453–455.

[74]A similar strategy, using Leśniewski's *Ontology* – his term logic that he builds his Mereology on – was already gestured at by Peter Simons (see (Simons, 1997), pp. 268–270; compare also (Simons, 1985)). Simons is interested in a nominalistically acceptable construal of higher-order quantification. As mereology is traditionally seen as a nominalistically acceptable system *par excellens*, the worries raised above presumably do not apply to him. Space constraints make it impossible investigate Simons' approach in due detail in this thesis.

and suggests interpreting '$\precsim$' as '$\lhd$'! – raises the question for an interpretation of *that* symbolism.

Whatever the prospects for a formal semantics that satisfies all demands might be, though, again it seems that the step via PFO is an unnecessary detour: whatever interpretation can be given to PFO (which then interprets monadic second-order logic) can presumably be given to monadic second-order logic *directly.* This is, of course, not to say that PFO and PFO$^+$ are not interesting and worth exploring – it merely says that they offer no prospect as a tool to dispel doubts about monadic second-order logic.

Øystein Linnebo oberves that the translation from monadic second-order logic into PFO in fact shows just how strong the assumptions on the allegedly innocuous plural constructions have to be. In order for the interpretation to work, an analogue of the second-order comprehension schema (or the corresponding rules) is needed. Linnebo calls this the "plural comprehension schema", (Comp-P):[75]

(Comp-P) $$\exists x\varphi \supset \exists xx \forall x(x \prec xx \equiv \varphi)$$

where φ is in the language PFO and does not contain 'xx' free. It is possible (and maybe necessary) to distinguish between *predicative* and *impredicative* plural comprehension in the usual fashion (see chapter 2, section 3.1, above). The former is sufficient if a predicative second-order theory is meant to be interpreted, while the latter is needed if the second-order theory is impredicative. Since the latter case is presumably the one that is wanted,

> what we need to justify is that there are pluralities corresponding to *all*

[75](Linnebo, 2003), p. 74.

> expressions of the form 'the φs', even where 'φ' contains bound plural variables. But in order to do this, we must understand what these bound plural variables range over. This means that we must understand the notion of a *determinate range of arbitrary sub-pluralities* of the logical domain.[76]

The problem becomes particularly pressing if we want to use the plural quantifiers in the domain of set theory. The notion of an arbitrary sub-plurality becomes highly complicated. One may doubt whether our natural language plural expressions give us a range for the plural quantifiers that is sufficiently determined in this area – which, recall, according to Boolos is "an area in which [second-order logic] might have been expected to be of considerable value."[77] How many "pluralities" of members of an infinite set there are seems to be in a straightforward way related the continuum problem (see chapter 7 below), and it is rather unlikely that the understanding we have of plurals from our ordinary language use will decide that.

4.5 Summary

In this chapter, I presented Boolos' response to the Quinean charge that second-order logic is committed to sets. Boolos proposes a plural interpretation of the monadic second-order quantifiers and argues that second-order quantification thus construed is not ontologically committing over and above the commitments of the first-order fragment of a theory. The plurally interpreted second-order variables, Boolos contends, do not refer to new objects, but rather refer in a new way to the

[76](Linnebo, 2003), p. 85.
[77](Boolos, 1984b), p. 65.

old objects: they refer plurally.

The principal problem we met was that Boolos translation only captures monadic second-order logic. While Boolos is content that in all the interesting cases there will be enough mathematics available to define a pairing function, this provides no general solution. There is no guarantee that there will always be a pairing function available – indeed, in finite domains there can be none. It was suggested that the Rayo-Yablo translation of polyadic second-order logic was less comprehensible than Boolos' plural translation, and that the problems that the latter faces, are even more pressing for the former.

Boolos' critics have claimed that his promotion of plural quantification over a construction that uses classes comes to a stand-off and a clash of intuitions concerning the respective merits and preferences involved on each side. It was further argued by Resnik, Shapiro and others that our understanding of the plural constructions Boolos uses is either too imprecise to be useful, or else is mediated by our (highly theoretical) understanding of set theory. Attempting to rigorise the plural interpretation as a remedy did not lead to a cure, but merely prolonged the suffering.

It seems quite interesting in this context that Boolos suggests, concerning the problem of polyadic quantification, that the mere fact that English lacks the resources to translate many-place predicate variables along the line of his plural interpretation need not be an objection. He states that

> the provision of a translation scheme into an antecedently understood language need not be the only way to confer sense upon statements in some notation; we didn't learn out mother tongue that way, for sure. And after all, we understand the basic formal machinery of second-order

> logic rather well; the syntax (including the devices for quantification and predication, as well as elementary proof theory) of polyadic second-order notation can be understood by one who understands that of polyadic first-order logic and monadic second-order logic.[78]

It will become clear in chapter 8 just how sympathetic I am to the view that we can understand a great deal about logic by grasping the formal language and rules of inference. But then, accordingly, in the case of Boolos' plural interpretation one might start to wonder what all the laborious effort was for.

Overall, Boolos' project does not seem come to a satisfactory end. It is wanting in the various respects mentioned above, with no solution appearing to be forthcoming. In the next chapter, chapter 5 of this thesis, I tackle the Quinean claims about the set-theoretical commitments of second-order logic from a different angle. I argue that a revision of Quine's criterion of ontological commitment is needed to make it work for even minimally interesting cases, and that the most straightforward revision will render second-order logic ontologically innocuous, provided it can be argued that second-order logic is legitimate, independently of a set-theoretical construal of it. Such an approach will be developed in chapter 8.

[78](Boolos, 1994), p. 243.

Chapter 5

Ontological Commitment

5.1 Quine's Criterion

Statements for Quine's criterion for ontological commitment can be found in many of his papers. Here are some examples:

> [E]ntities of a given sort are assumed by a theory if and only if some of them must be counted among the values of the variables in order that the statements affirmed in the theory be true.[1]

> To be assumed as an entity is, purely and simply, to be reckoned as the value of a variable. [...] The variables of quantification, 'something', 'nothing', 'everything', range over our whole ontology, whatever it may be; and we are convicted of a particular ontological presupposition if, and only if, the alleged presuppositum has to be reckoned among the entities over which our variables range in order to render one of our affirmations

[1] (Quine, 1953), p. 103.

true.[2]

> Another way of saying what objects a theory requires is to say that they are the objects that some of the predicates of the theory have to be true of, in order for the theory to be true. But this is the same as saying that they are the objects that have to be values of the variables in order for the theory to be true. [...] Our question was: what objects does a theory require? Our answer is: those objects that have to be values of variables for the theory to be true.[3]

And of course the most famous one:

> The universe of entities is the range of values of variables. To be is to be the value of a variable.[4]

Quine considers formalised theories, and asks for the ontological commitment of these theories, i.e. what things have to exist in order for the theory to be true. Ultimately, and quite intuitively, the ontological commitment of a theory can be read off from the existentially quantified sentences of the theory. If, for example, the theory includes the statement '$\exists x(x \text{ is red})$' the theory is committed to red things. There have to be red things in order for this sentence, and hence the theory, to be true. Or, put in Quinese, it has to be assumed that there are red things in the range of our variables in order for the affirmed statement '$\exists x(x \text{ is red})$' in the theory to be true.

It might seem now, that Quine misses something. What if the theory says that Socrates is a philosopher? Is it not intuitively the case that this theory is committed

[2](Quine, 1948), p. 13.
[3](Quine, 1968), pp. 95–96.
[4](Quine, 1939), p. 708.

to philosophers in general, and Socrates is particular? 'Socrates is a philosopher', however, surely is formalised as something like 's is a philosopher', or shorter 'Ps' which contains neither a variable, nor an existential quantifier.

To see that there is, for Quine, no problem in this, first we have to observe, that we are primarily concerned with the ontological commitment of theories. Theories are standardly taken to be closed under logical consequence, i.e. a theory is a bunch of statements that are taken to be true, the *axioms*, plus every statement that follows logically from them. Now, to see that a theory that has 'Ps' as an axiom is indeed, according to Quine's criterion ontologically committed to philosophers and Socrates, observe that the statements '$\exists x Px$' and '$\exists x(x = s)$' both follow from 'Ps' and hence belong to the theory. The range of the existentially quantified variables includes – for the sentences to be true – philosophers and Socrates, respectively, and so the theory is committed to these entities.

Quine has something more up his sleeve, however. Even from the sentence 'Socrates is a philosopher' itself we can read off the ontological commitments, if we just look close enough. For Quine, proper names like 'Socrates' are just definite descriptions in disguise. Socrates is just the unique object that "socratises", and so 'Socrates is a philosopher' is really properly formalised as '$\exists x(Sx \wedge \forall y(Sy \supset y = x) \wedge Px)$'. So, here we have our existential statement, and philosophers and socratisers are amongst the values of the variables, or at least "have to be reckoned among the entities over which our variables range in order to render our affirmation true".

En passant, Quine brings proper names properly into line with ontological commitment being to a *kind* of thing – it is only that proper names commit to very unique kinds of things, viz. kinds that are uniquely instantiated – while his original motive for this move was to solve the problem of empty names. Famously, Russell solved the problem of the present king of France with his theory of descriptions.[5] 'The present king of France is bald' seems to be false, since presently there is no king of France. But then, 'The present king of France is not bald' should also be false, for the same reason. How can that be, if the two sentences are negations of each other? Russell's solution, standard today, is that they are *not* negations of each other, it is just the surface grammar of the sentences that makes us believe that. In fact, the sentences say '$\exists x(Kx \wedge \forall y(Ky \supset x = y) \wedge Bx)$' and '$\exists x(Kx \wedge \forall y(Ky \supset x = y) \wedge \neg Bx)$', where '$K$' stands for 'is presently king of France', and 'B' for 'is bald'. So they are not negations of each other at all, both can be false, and are.

Quine takes this a step further and applies Russell's insight to empty names, like 'Pegasus' as well. Were 'Pegasus is a winged horse' formalised as 'Wp', then on the standard treatment of names as always referring, it would follow that '$\exists x(x = p)$' is true, no matter whether we claim that 'Wp' is true or not. The existential statement would follow from the logical truth '$\forall x(x = x)$' if we can instantiate it to Pegasus, and hence has to be taken as true as well. On Quine's treatment, 'Pegasus is a winged horse' comes out as '$\exists x(x \text{ pegasises} \wedge \forall y(y \text{ pegasises} \supset y = x) \wedge x \text{ is a winged horse})$' which, alas, is false due to the lack of winged horses and unique pegasisers. Since no proper names are involved, however, the existence of Pegasus does not have to be asserted by saying so.

As said above, Quine's criterion seems very intuitive indeed. After all, is it not straightforward, if anything ever was, that a theory that says that there are red things is committed to the existence of red things? And is that not exactly what Quine's criterion says? Is surely seems so. It is, however, *not exactly* what Quine's

[5] (Russell, 1905).

criterion says, but it *is* exactly where Quine's criterion gains its credence from.

Consider the following theory. Take as sole axiom the sentence formalised as 'p', and close the theory under logical consequence. Here we have a theory formalised in propositional logic. Variables and quantifiers do not even belong to the language. We are forced to say that either Quine's criterion does not apply, or that this theory does not have any ontological commitment; neither seems satisfactory. Say, 'p' stood for 'Unicorns exist', surely we would want to say that the theory is committed to the existence of unicorns. To fix this, one might point out that formalising 'Unicorns exist' as 'p' is not sufficient, since it does not exhibit the full logical form of the sentence. We should, therefore, formalise 'Unicorns exist' as '$\exists x\, Ux$' which makes the ontological commitment that arises from this sentence explicit. This move shows a parallel to Quine's treatments of names: Names, too, are not broken down far enough in the logical analysis, according to Quine, if just symbolised by a constant letter. Clearly to display the ontological commitments of sentences in which names figures means replacing them by definite descriptions.

What this shows us, is that Quine's criterion for ontological commitment does not stand on its own, at least not if we think that it is purely descriptive. The normative aspect of it – or the normative aspect by which it has to be supplemented in order for it to be generally applicable – is that the theories have to be formalised in a certain way. It looks as though this way can be characterised in the following way: The logical structure of the sentences of the theory has to be analysed as deeply as possible to exhibit the ontological commitments. Restricting ourselves to propositional logic in the formalisation of theories will not do generally, and equally might the use of names instead of definite description mask our ontological commitments.

5.2 Universals

Quine has provoked no little amount of criticism concerning the demand to formulate theories in a certain way.[6] In order to bring the sentences of a theory into the Quinean standard form, the ordinary language sentences often have to be paraphrased or regimented. We have already seen how sentences which on the face of it do not seem to contain variables or quantifiers are regimented and formalised into sentences that contain existential quantifiers. On the flip side, not every English sentence that starts with 'There is' will end up being an existentially quantified statement in the formalised theory. It is difficult, if not impossible, to come up with standards for such a paraphrase or regimentation. This is particularly pressing concerning the problem of universals that Quine dealt with extensively. A famous passage from his *On What There Is* reads:

> Now let us turn to the ontological problem of universals: the question whether there are such entities as attributes, relations, classes, numbers, functions. McX, characteristically enough, thinks there are. Speaking of attributes, he says: "There are red houses, red roses, red sunsets; this much is prephilosophical common sense in which we must all agree. These houses, roses, and sunsets, then, have something in common; and this which they have in common is all I mean by the attribute of redness."

Quine, however, denies this:

> The words 'houses', 'roses', and 'sunsets' are true of sundry individual entities which are houses, roses, and sunsets, and the word 'red' or 'red

[6] See for example (Alston, 1958).

> objects' is true of each of sundry individual entities which are red houses, red roses, and red sunsets; but there is not, in addition, any entity whatever, individual or otherwise, which is named by the word 'redness', nor, for that matter, by the word 'househood', 'rosehood', 'sunsethood'.[7]

Quine does not want to allow abstract objects into his ontology – except for classes which he "grudgingly admitted"[8] – so he paraphrases the apparent commitment to "things" like redness away. A problem arises, however, since paraphrase seems to by symmetrical: If a sentence like e.g. 'Some man has a high age' can be paraphrased into 'Some man is old', why is it that this shows that neither of the sentences, deep down, carry any ontological commitment to abstract objects *ages*. Could it not be the case, that the former displays the ontological commitments, rather than the latter? It might just be that '$\exists x \exists y(x$ is a man $\wedge\ y$ is an age $\wedge\ x$ has $y)$' brings out the hidden ontological commitment of 'Some man is old', just like the Quinean treatment of proper names did, or the proper formalisation of 'Unicorns exists' as '$\exists x\, Ux$' rather than 'p'.

5.3 Second-Order Quantification

Rather than going deeper into the problem of paraphrase and regimentation, let me raise some more issues. If the task should be to exhibit the logical form of the sentences of the theory in as detailed a manner as possible, Quine needs to tell a story about sentences that are not expressible in pure first-order logic alone if one wants to exhibit their logical structure in as detailed a manner as possible. One

[7](Quine, 1948), pp. 9–10.
[8](Quine, 1975), p. 100.

of the most famous example is the so-called Geach-Kaplan sentence: 'Some critics admire only one another'. What the sentence is meant to express is that a group of critics, not necessarily all critics there are, are such that each of them admires everyone else in this group, but nobody else.[9] George Boolos picks up on this.[10] He gives a formalisation in second-order logic:

$$\exists X[\exists x Xx \wedge \forall y \forall z((Xy \wedge Ayz) \supset (y \neq z \wedge Xz))] \tag{1}$$

Quine himself prefers the first-order formalisation using set theory:

$$\exists x[\exists y(y \in x) \wedge \forall y \forall z((y \in x \wedge Ayz) \supset (y \neq z \wedge z \in x))] \tag{2}$$

Note that Boolos assumes a domain of critics. In the context of the discussion of ontological commitment it might be preferable to mention critics explicitly as in:

$$\exists X[\forall x(Xx \supset Cx) \wedge \exists x Xx \wedge \forall y \forall z((Xy \wedge Ayz) \supset (y \neq z \wedge Xz))] \tag{1$'$}$$

In any case: why does Quine bring sets into play? (2) gives rise to an ontological commitment to sets of which there was no talk in the Geach-Kaplan sentence. Why is the paraphrase a good one, if 'Some critics admire only one another' only seems to speak about flesh and blood critics, and not about sets? Quine's reason is that he believes second-order logic to be set theory anyway, except that it hides its

[9] Quine credits Peter Geach with the discovery of the sentence, and David Kaplan with the proof that it cannot be expressed satisfactorily in first-order logic. See (Quine, 1974), p. 111, and (Quine, 1982), p. 293.

[10] (Boolos, 1984b). Boolos also gives Kaplan's proof of the "non-firstorderisability" of the Geach-Kaplan sentence.

ontological commitments.[11] Since a formalised theory is meant to make its ontological commitments explicit such hidden ontology cannot be accepted. This, however, raises the question: Why *should* second-order logic be set theory in disguise?

It seems that Quine has merely taken his criterion for ontological commitment at face value. If the ontological commitment is exhibited by the values that the variables must take to make the sentences true, a theory that has higher-order variables, must be committed to whatever higher-order entities these range over. As we have seen above, Quine does not want to admit properties or attributes into his ontology. Yet, the expressive limitations of pure first-order logic forces him to admit *something*. The only acceptable "higher-order objects" for Quine are also the only abstract objects acceptable to him: classes, or sets, which, while being indispensable as Quine contends[12], at least have the advantage of being extensional.[13] For Quine, (2) merely makes explicit the commitments to sets that also arises from (1), albeit in a hidden way.

Quine has been criticised by Crispin Wright[14], and more recently again by Agustín Rayo and Stephen Yablo[15], on the ground that this view together with Quine's views about predication does not appear to create a stable position. Quine insists that predicates do not refer, and that their use in first-order theories does not commit us to universals or abstract entities. If Quine, however, believes that quantifying into predicate position exhibits a commitment to sets (at best), should this ontological commitment not already arise for the unquantified use of predicate letters? For the case of names, at least, Quine argued that this is the case, and even

[11](Quine, 1986a), pp. 64–66.
[12]See (Quine, 1950).
[13](Quine, 1975), p. 100
[14](Wright, 1983), pp. 132–133
[15](Rayo and Yablo, 2001), pp. 79–80

gave, as we have seen, a paraphrase of names that makes this explicit.

Quine would probably formalise 'Tomatoes and sunsets are red' as

$$\forall x(Tx \supset Rx) \wedge \forall x(Sx \supset Rx) \tag{3}$$

If this statement is not committing to anything beyond tomatoes, sunsets and things that are red, why should the paraphrase of it, 'Tomatoes and sunsets share something in common, namely that they are red', do so? This paraphrase is straightforwardly formalised as:

$$\exists X(\forall x(Tx \supset Xx) \wedge \forall x(Sx \supset Xx) \wedge \forall x(Xx \equiv Rx)) \tag{4}$$

If (4) is committed to something else but tomatoes, sunset, and red things, it seems that (3) is, too. If (3) isn't, then neither is (4).

Let us for the while just note that we are dealing with difficult issues here, and then move on to what hopefully brings the solution. It appears that Quine's insistence on first-order formalisation is problematic. We have seen that his criterion of ontological commitment has a normative side (or has to be supplemented by more Quinean philosophy to make it work). When formalising a theory one *ought* to do it in the right way, and this right way is doing it in standard first-order logic. The reason for this is, that in doing so we make the ontological commitments of a theory explicit. Quine takes this to be a trivial point which is only concerned with explicitness about the assumed ontology.[16] To claim, however, that other formalisations, like second-order ones, are committed to sets, since first-order set

[16](Quine, 1981), p. 175.

theory is the straightforward way – if not the *only* way – to re-capture in a first-order theory what is expressed in the second-order theory seems to beg the question. What Quine basically says is that, when put into the standard first-order paraphrase, the range of the variables show us the ontological commitments of a theory. Someone who does not believe in the first place that first-order formalisation is the only legitimate way to go will not be convinced of this special status of first-order (plus set theory) versions of a theory.[17] The circle basically looks like this:

Quine: [start] Second-order logic is not suitable to exhibit the ontological commitments of a theory.

Opponent: Why?

Quine: Because second-order theories hide their ontological commitment to sets.

Opponent: Why do you think they are committed to set?

Quine: Because you need set-theory to paraphrase it into first-order, and set theory is committed to sets.

Opponent: Why do I have to paraphrase it into a first-order theory?

Quine: Because you need a canonical formulation in first-order logic, in order to see the ontological commitments of a theory.

Opponent: Why?

Quine: Because my criterion for ontological commitment requires a formalisation as a first-order theory in a canonical form.

Opponent: Why do you require that?

[17] As we have seen in the last chapter, (Boolos, 1985a), pp. 76–77, puts forward a similar criticism.

Quine: Go back to [start].

It might of course be possible to break this circle, but as I said above, this is just meant to flag the problem: It seems primarily to be the Quinean preference for first-order logic that motivates his criterion for ontological commitment, which, in turn, seems to be his main reason for his preference.

5.4 Polyadic Predicates Again

Now, however, let's take Quine head on, and grant him a first-order regimentation. What ontological commitments does a sentence like 'Everybody loves somebody' carry? It is tempting to say that it is committed to lovers, but is it admissible to ignore the fact that the sentence really contains a relation, and not the predicate 'is a lover'? It also leaves out the beloved.[18]

One might also consider a theory whose sole axiom says: 'There is something that is taller than something', or formalised: '$\exists x \exists y\, Txy$'. What is this theory committed to? What *kinds* of things is it committed to? The criterion is convincing enough when it comes to one-place predicates. 'Something is red' carries ontological commitment to red things, but it does not seem at all straightforward what happens when we are dealing with relations. Maybe we have to say that we are committed to that kind of things that are taller than some other thing, and things that have something taller than them?

To generalise the point, let us consider the following theories. $\mathbf{T_1}$, to start with, has the following three axioms:

[18](Resnik, 1988) makes this latter point, and also comes up with further complications. See also (Simons, 1997) on this issue.

A1.1 $\forall x Rxx$

A1.2 $\forall x \forall y (Rxy \supset Ryx)$

A1.3 $\forall x \forall y \forall z ((Rxy \wedge Ryz) \supset Rxz)$

What are the ontological commitments of $\mathbf{T_1}$? We would commonly say that $\mathbf{T_1}$ characterises 'R' as an equivalence relation. To say that the theory is committed to things that stand in an equivalence relation to each other, however, quantifies over relations: *There is* an equivalence relation in which these things stand to each other. This should not be acceptable for Quine. It is hard to see how the quantification over relations in this statement about the ontological commitment of $\mathbf{T_1}$ could be paraphrased away, short of repeating the axioms. If just repeating the axioms is fine, though, why not do so generally, and hence also in the case of higher-order sentences? It would then be difficult to see why we had to reformulate second-order theories in the language of set theory to expose the ontological commitment.

In order to emphasises the point, consider $\mathbf{T_2}$:

A2.1 $\forall x \forall y \forall z ((Rxy \wedge Ryz) \supset Rxz)$

A2.2 $\forall x \exists y Rxy$

A2.3 $\forall x \forall y \forall z ((Rxy \wedge Rxz) \supset Ryz)$

It is very difficult to formulate the ontological commitment of this theory and avoid both of the following two "traps": Giving an explication of the ontological commitment in terms of the *relation* that is characterised ("a relation that is transitive, serial, and euclidian") – Quine would not want to say that this first-order theory is committed to the existence of relations[19] – and not merely saying something like:

[19]See e.g. (Quine, 1951a), p. 204.

"The theory is committed to things such that φ", where φ is (more or less) a repetition of the axioms.

Might it be that the ontological commitment of polyadic theories is to kinds of *tuples* of things, rather than just kind of things as in the monadic case? A dyadic relation might just be construed as a predicate applying to an ordered pair, a triadic one as a predicate applying to triples, and so on. For Quine, this must be unacceptable as well. His paradigm for philosophical explication, we read in *Word and Object*, is the Wiener-Kuratowski definition of an ordered pair.[20] '$\langle x, y \rangle$' is defined as '$\{ \{x\}, \{x, y\} \}$', a set of sets. It should not turn out to be the case, though, that not just higher-order logic, but polyadic first-order logic already commits a theory to sets. Quine's conception of relation-symbols is that of many-place predicates. They are as neutral as the common, one-place predicates. These do not refer to properties or attributes, and those do not refer to relations. Both properties and relations are abstract universals that Quine rejects. This means, however, that it cannot be in a Quinean spirit to adjust the criterion such that the commitment is to, e.g., kinds of pairs. Two-place predicates are merely true or false of things they apply to, just like one-place predicates. No *further* ontological commitment arises than to the kind of things that are in the range of the quantifiers, those that have to exist in order to make such a sentence true. This, however, is precisely the problem: What kinds of things, exactly, have to exist, in order to make a sentence containing a two-place predicate true, turns out be difficult to figure out.

Note, that the difficulty does not arise, as it might seem at first glance, from the fact that no intuitive interpretation for 'R' is given in the two sample theories above. First of all, such an interpretation is not needed for monadic theories. A

[20](Quine, 1960), §53.

theory containing '$\exists x F x$' as a theorem is committed to Fs – whatever they might be. It would be strange indeed if such an intuitive interpretation is required for the polyadic, but not the monadic case. Also, consider $\mathbf{T_3}$, with 'M' standing for the dyadic relation 'is married to':

A3.1 $\forall x \forall y (Mxy \supset Myx)$

A3.2 $\forall x \neg Mxx$

A3.3 $\exists x \exists y \exists z (Mxy \wedge Mxz \wedge y \neq z)$

A3.4 $\forall x \forall y \forall z ((Mxy \wedge Mxz \wedge y \neq z) \supset \neg \exists v (Mzv \wedge v \neq x))$

To say that the ontological commitment is to married people does not do justice to the case. People are not always married in the way described by the axioms. It seems, however, that something ontologically interesting is said, and that is that this *kind* of people are married in a certain way, the way that is specified by [A3.1]–[A3.4]. This might pre-systematically be better expressed by saying that marriage is organised in a certain way, the $\mathbf{T_3}$-way: symmetric, irreflexive, polygamic, harematic.[21] It is hard to see a way to couch any of this in terms of Quine's criterion of ontological commitment that would be acceptable to Quine, and still allow for a discrimination against higher-order languages.

$\mathbf{T_3}$, of course, is false of now-a-days western countries (at least if we take marriage to be the legal institution), but might well be true of societies in other times and places. It does not matter, though, if it is not: The ontological commitment of it must not depend on the way the world *is*. Rather, it tells us what the world *has to be like*, or more specifically, what kinds of things have to exist in order for it to be

[21] Not necessarily sexist, though. $\mathbf{T_3}$ leaves open that members of any gender can be married to more than one person.

true. This last specification, however, is the one that creates the problems. Quine's criterion for ontological commitment works best when we are dealing with one-place predicates only. This is hardly surprising, since the criterion is meant to tell us what kinds of things there have to be in order for the theory to be true. One-place predicates are the lexical items we use when we talk about kinds of things.

Could this not be just shrugged off? Why not bite the bullet and say that $\mathbf{T_3}$ has no ontological commitment? The axioms of the theory still say something that we can talk about. Such a strategy would render the best part of the project of a criterion of ontological commitment pointless. In order for the statements of $\mathbf{T_3}$ to be true certain things have to exist; this much is clear. If a criterion of ontological commitment cannot deliver the answer what those things are, with what right is it called a criterion of ontological commitment? It becomes clearer how pressing the problem is when we consider $\mathbf{T_4}$ as an example:

Suc1	$\exists! x \forall y \neg Sxy$	(zero)
	$0 =_{df} \iota x(\forall y \neg Sxy)$	(definition)
Suc2	$\forall x \exists! y Syx$	(functionality)
Suc3	$\forall xyz((Szx \wedge Szy) \supset x = y)$	(uniqueness)
Sum1	$\forall xy \exists! z Axyz$	(functionality)
Sum2	$\forall x Ax0x$	(zero)
Sum3	$\forall xyzv[(Szy \wedge Axzv) \supset \forall w(Axyw \supset Svw)]$	(successor)
Prod1	$\forall xy \exists! z Pxyz$	(functionality)
Prod2	$\forall x Px00$	(zero)
Prod3	$\forall xyzv[(Szy \wedge Pxyv) \supset \exists! w \exists! u(Pxyw \wedge Awxu)]$	(successor)
Ind	$[\Phi 0 \wedge \forall x(\Phi x \supset \exists y(Sxy \wedge \Phi y))] \supset \forall x \Phi x$	(induction)

$\mathbf{T_4}$ can obviously be recognised as a formulation of Peano Arithmetic without function symbols or one-place predicates. Note that the definition of zero is for the convenience of easier legibility only, and can of course be eliminated. In order to do without it, replace Sum2, Prod2, and Ind by the following:

Sum2′	$\forall xyz(\neg Syz \supset Axyx)$
Prod2′	$\forall xyz(\neg Syz \supset Pxyy)$
Ind′	$[(\forall xy(\neg Sxy \supset \Phi(x)) \wedge \forall x(\Phi(x) \supset \exists y(Sxy \wedge \Phi(y)))] \supset \forall x\Phi(x)$

If one wanted to take the bullet-biting way out, and accept that theories that do not figure one-place predicated do not have ontological commitment, $\mathbf{T_4}$ would also have to be considered void of ontological commitment. This should be a highly unwelcome result. It would mean that with $\mathbf{T_4}$ we could have arithmetic as an ontological free lunch.

5.5 A New Criterion

The basic idea behind the "new criterion" is to accept the intuitively correct part of Quine's criterion: a theory that includes '$\exists xFx$' is committed to Fs; but to accept *only* this part and mend the shortcomings. The following proposed solution to the problem of relations will also without further adjustments be applicable to higher-order languages. No re-phrasing, translating, or re-formalising in a different language, like set-theoretical language in Quine's case, is necessary. The criterion will deliver the intuitively correct result for higher-order theories, and do so in a purely formal way. Contra Quine, no commitment to sets will result, unless a set-theoretical language is indeed used.

Quine's criterion works so well in the case of one-place predicates, because it is about commitment to *kinds* of entities, and one-place predicates are the lexical items that we use to indicate the kinds that the discourse is about. I avoid writing 'predicates refer to kinds' partly because I, like Quine, do not believe that predicates refer – nor that "kinds" exist in any metaphysically robust sense, for that matter. More importantly, though, and independently of any "taste for desert landscapes"[22], it should be possible to find a neutral way of stating the "special" status of one-place predicates without settling this metaphysical question first.

In the case of a two-place predicate, like '... loves ...', it seems intuitive that the commitment should be to those kind of things that occupy either of the argument places. This much was mentioned above already. We met the problem that, in the case of '... loves ...', the kinds 'lover' and 'lovee' can be extracted fairly easy, since there are English predicates for just those kinds that are required in this case. This is not always the case, however, as was shown at the example of the theories $\mathbf{T_1}$–$\mathbf{T_3}$. The idea to handle ontological commitment of polyadic theories in roughly this way, is nevertheless the right one. There are well-known formal methods to form one-place predicates on basis of many-place predicates, and so it is not necessary to rely on natural language paraphrase of anything like it. One way is to use explicit definition:

(5) $$\text{Lover}(x) =_{df} \exists y\, \text{Loves}(x, y)$$

(6) $$\text{Lovee}(x) =_{df} \exists y\, \text{Loves}(y, x)$$

[22](Quine, 1948), p. 4.

Alternatively, one can stay in the object language and use the λ-operation:

(7) $$\forall x(\text{Lover}(x) \equiv \lambda z[\exists y\,\text{Loves}(z, y)]x)$$

(8) $$\forall x(\text{Lover}(x) \equiv \lambda z[\exists y\,\text{Loves}(y, z)]x)$$

I consider these ways as interchangeable. Using the λ-operator is merely an alternative way of stating what otherwise could be said through metalinguistical explicit definitions. The notational advantage is that the defined predicates can be mentioned a little less cumbersomely using the λ-operator.

The new criterion operates by forming one-place predicates out of the primitive many-place predicates of the theory in this way, that occur *in the right way* in the axioms of the theory. The theory will have commitment to those kinds of entities that the new one-place predicates distilled from the old primitive predicates apply to.

We cannot simply take all primitive predicates, however, and form the kind terms from them with this method. A theory containing '$\exists x \neg Fx$' as its only axiom surely is not committed to Fs (or, more pedantically, the $\lambda x[Fx]$'s). The kinds that the theory is committed to rather have to be extracted by deleting the initial quantifier in such axioms, one at a time, enclosing the resulting expression with square parenthesis, and prefixing a λ-operator that is binding the variables in the places of those that the deleted quantifier was binding. The resulting expression then designates the kinds of things that the theory commits us to. '$\exists x \neg Fx$' will give us '$\lambda x[\neg Fx]$' as a commitment, or, equivalently, the non-Fs, which is the right result.

A theory with the axiom '$\exists x \forall y$ Loves(x, y)' will have amongst its ontological commitments the $\lambda x[\forall y \, \text{Loves}(x, y)]$, i.e. the kind of things that love everything.

It is not sufficient, however, to just extract kinds from the axioms of a theory. Any given theory commits us to those kinds of things that have to be assumed to exist in order for the axioms, but also everything that follows from them, to be true. A theory with the two axioms '$\forall x(Fx \supset Gx)$' and '$\exists x Fx$', does not only commit us to Fs, but also to G's: '$\exists x Gx$' is amongst the theorems of the theory. More strikingly, a theory that contains the two axioms '$\forall x$ Round(x)' and '$\forall x$ Square(x)' is not only committed to round things and squares, but also to round squares (assuming we are not formulating a theory in free logic). Given Quine's elaboration about the impossibility of a "round square cupola on Berkeley College"[23] one can only assume that this would have been also amongst Quine's desiderata for a precisely formulated criterion of ontological commitment.

The new criterion, thus, suggests itself as follows:

(NC) A theory $\mathbf{T}$ is ontologically committed to entities of kind $\mathcal{K}$ if, and only if, $\mathcal{K}$ is $\ulcorner \lambda\xi[S^*] \urcorner$, where S^* is an open sentence that is the result of deleting an initial first-order existential quantifier Q in a sentence S that holds according to $\mathbf{T}$, and replacing in it the variable formerly bound by Q with a variable ξ that does not occur in S.

The 'holds' can be understood as 'is a theorem' or as 'is true', depending on whether the relevant (or preferred) conception of theories is proof-theoretic or semantic, respectively. The replacement of the variable is a precaution against conflicts concern-

[23] Quine also discusses examples like this one in (Quine, 1948), pp. 4–9.

ing the binding of variables.

A welcome feature of (NC) is that we need not quantify over relations to state the ontological commitments of a theory (e.g., $\mathbf{T_1}$ committing us to the existence of an equivalence relation). All our ontological commitment is to things in the (first-order) domain, as it should be, also following Quine's demands. Also, the sample sentences already used above:

$$(3) \qquad \forall x(Tx \supset Rx) \wedge \forall x(Sx \supset Rx)$$

$$(4) \qquad \exists X(\forall x(Tx \supset Xx) \wedge \forall x(Sx \supset Xx) \wedge \forall x(Xx \equiv Rx))$$

((3) standing for 'Tomatoes and sunsets are red' and (4) standing for 'Tomatoes and sunsets share something in common, namely that they are red') bring with them provably coextensional commitment (according to the new criterion), as it should be. Also in the case $\mathbf{T_4}$ we get the correct result: a commitment to successors, successees, a uniquely instantiated successee-less kind of thing, etc.

Moreover, the Geach-Kaplan sentence:

$$(1') \qquad \exists X[\forall x(Xx \supset Cx) \wedge \exists x Xx \wedge \forall y \forall z((Xy \wedge Ayz) \supset (y \neq z \wedge Xz))]$$

brings a commitment to critics, to admirers, to admirees, etc. – but not to sets.

It is worth noticing that the formulation of (NC) is based on considerations concerning the modification of the basic idea behind Quine's criterion for ontological commitment to make it applicable to theories containing many-place predicates. The "spirit" of the Quinean criterion is preserved, and the found solution can with some right be called "Quinean", too. The range of first-order variables still determines the ontological commitment, and now, with (NC), there is a formal way of stating what kinds of entities a theory commits us to – in the monadic, as well as the polyadic case. Quine's criterion was not modified in order to render second-order quantification ontologically innocuous. Rather, the modifications made to accommodate many-place predicates led to a formal criterion that happens to be applicable to higher-order languages, too.

Quine insists that his criterion of ontological commitment is not about what there is, but rather about what a certain discourse has to *assume* there to be.[24] In his reformulations of statements apparently referring to universals, he shows, or at least claims to show, that one does not have to assume that universals exist. One also does not have to assume that Pegasus exists in order to deny its existence. With (NC) it makes sense to say that a discourse representing the Geach-Kaplan sentence as (2) (Quine's set-theoretical rendering) is committed to sets, while discourses that do not mention sets also do not have to assume sets. The new criterion points out *generally*, and not just for monadic first-order theories, *what kinds* of entities a theory is committed to, but it stays neutral on the question what the ontological status of kinds *as such* is. Whether they are sets, attributes, or not to be taken with ontological seriousness: the outcome of this question should be the same for first- and second-order theories.

(NC), of course, is only applicable to languages that are legitimate in some sense. If modal languages, for example, were illegitimate in some way, it would not make much sense to claim on basis of (NC) that the sentence '$\exists x \Box Fx$' carries commitment to things that are necessarily F: $\lambda v[\Box Fv]$. The same holds for higher-order

[24](Quine, 1948), pp. 15–16.

languages as well. If it can be established on some grounds that higher-order quantification is illegitimate in some way, (NC) does not apply to such languages. (NC) then cannot be used with these languages, simply because the languages cannot be used at all. The purpose of the chapter was not to establish that higher-order languages are legitimate – rather, the whole rest of this thesis argues for this. What was shown here was merely that, assuming that second-order logic cannot be disqualified on other grounds, Quine's claim that the use of second-order quantifiers commits one to sets cannot be upheld. Our ontology is what is in our first-order domain; first-order variable range over the things that exist (also this is strikingly Quinean). Quantifying into predicate position does not bring about additional ontological commitment.

Chapter 6

Semantic Incompleteness*

6.1 Introduction

As mentioned in chapters 1 and 3, the incompleteness of second-order logic with respect to standard semantics (see chapter 2 for the formal details) is often cited as a reason why second-order logic is not *proper* logic. The worries here seem to be epistemological in nature.[1] One important worry is that the second-order consequence relation as characterised by the standard model theory is intractable in some sense: there can be no deductive system that is both sound and complete with respect to it, as follows from Gödel's incompleteness theorem (see chapter 2). I make no attempt to dispel worries concerning this intractability of the standard model-theoretic consequence relation in this chapter. These problems are analysed in detail in chapter 7. In chapter 8, however, I present a conception of logic that proceeds via deductive systems, and argue that second-order logic, so construed, should indeed be counted as logic in a proper sense ('proper logic' is also explicated there).

*This chapter is in parts based on my (Rossberg, 2004).

[1](Cutler, 1997), for example, is explicit about this.

It will also be discussed there how the deductive system on its own (i.e. not with respect to a given model-theoretic semantics) is effected by Gödel's incompleteness theorem.

In section 6.4 where I discuss a few arguments from the ongoing discussion about the status of second-order logic, I touch upon issues related to this intractability, while the focus, however, remains on the question whether the incompleteness result can be turned into an argument against the logicality of second-order logic.

A second worry that is attached to incompleteness presumably draws on the fairly negative connotation of the term. 'Incompleteness' suggests that something is *missing*. This, in turn, suggests that we have not succeeded in characterising all of the logical consequences. What sense of "all" is that here, though? Is propositional logic not a proper logic because it does not (and cannot) capture theorems of predicate logic? The obvious answer to this question will be given below. So, if this is not what is meant, what other sense of capturing *all* logical consequences is relevant here? Also, should it worry us so much that not all logical consequences are captured? Is that not even too much to ask for? Is it not more important, anyway, that nothing which is not a logical consequence is falsely declared as such? And what exactly, then, *is* the deficiency that is witnessed by the incompleteness of second-order logic?

This chapter provides answers to these questions (although in some cases the answers are preliminary and a more comprehensive answer is not available before "the deductivist account of logic" is properly introduced in chapter 8). In this chapter I argue that the mere lack of a completeness theorem, despite being an interesting result, cannot be held against the status of second-order logic as a proper logic. Neither does the impossibility of a sound and complete deductive system for the standard model show this. It turns out that, crucially, the question of completeness comes into play only after it has been settled whether a given system is a proper logic, or a given consequence relation coincides with logical consequence.

6.2 Logical Consequence

The unqualified claim that second-order logic is incomplete cannot stand in any case; it needs to be made more precise. The deductive system is not semantically incomplete in and of itself; rather, it is incomplete with respect to some specified formal semantics. The deductive system of second-order logic, for example, is incomplete with respect to the *standard* model-theoretic semantics. As laid out in chapter 2, a model in the standard model theory consists of a domain of objects and an interpretation function. This function assigns objects in the domain to names in the languages, subsets of the domain to one-place predicates, subsets of the Cartesian product of the domain with itself to two-place predicates (sometimes also called binary relation symbols), and so on. The first-order quantifiers range over the domain, while the second-order quantifiers range over the subsets of the domain in case the quantifier binds a one-place predicate variable, over the subsets of the Cartesian product of the domain with itself in case the quantifier binds a two-place predicate variable, and so on.

It is well known that standard semantics is not the only semantics available. *Henkin semantics*, for example, specifies a second domain or the upper case constants and variables. The second-order quantifiers binding predicate variables, e.g., can be thought of as ranging over a subset of the full powerset of the first-order domain. What is relevant to the present discussion is that the deductive system of second-order logic is sound and complete with respect to a Henkin semantics.[2] (For details see chapter 2.) To pick up on a thought of Shapiro's, one might think that standard (as opposed to Henkin) semantics does not provide *enough* models to invalidate all sentences of the language of second-order logic that are not theorems. Shapiro dismisses this view.[3] He rather thinks that Henkin models are insufficient, in a sense, since the second-order variables are not guaranteed to range over the full powerset of the domain in a Henkin semantics. Shapiro argues in various places[4] that second-order logic with standard model-theoretic semantics is the right logic, at least for mathematical practice. (The discussion of this view is the topic of my chapter 7.)

I will not take sides here in the debate regarding whether standard or Henkin semantics is the "right" semantics. Note, however, that with respect to a Henkin semantics we have a completeness theorem for second-order logic. It would be bizarre, though, to claim that the incompleteness complaint is thereby refuted: it cannot be enough to just provide *some* semantics for which a completeness proof is possible. Note that Henkin and standard semantics are not the only available options. As we have already seen in chapter 4, there is, for example, the plural interpretation of second-order logic that was suggested by George Boolos.[5] Other options include (presumably) game-theoretical semantics, and (definitely) category-theoretical or topological semantics.[6] Peter Simons suggested a Leśniewskian semantics.[7] Surely, those do not exhaust the alternatives. It seems not unlikely that the important

[2](Henkin, 1950).

[3](Shapiro, 1998), p. 141; but see also his fn. 10 on the same page.

[4]See e.g. (Shapiro, 1985), (Shapiro, 1991), (Shapiro, 1997), (Shapiro, 1998), (Shapiro, 1999).

[5]See (Boolos, 1984b), (Boolos, 1985a), (Boolos, 1994).

[6](Awodey and Butz, 2000); see also (Awodey and Reck, 2002b), §3.

[7]See (Simons, 1985) and (Simons, 1993), and also (Simons, 1997).

philosophical issues concerning completeness and logical consequence will be obscured rather than elucidated by bringing up too many different approaches to formal semantics and a discussing which one "gets it right".

I will therefore in this chapter talk about formal or model-theoretic semantics *in general*, in the same way that I do not single out one specific deductive system. I will consider model-theoretic approaches to characterise logical consequence in general, irrespective of which model theory (if any) might be the right one to codify logical consequence, on the one hand. On the other hand, I will consider deductive systems in the abstract, again irrespective of which specific deductive system (if any) might properly capture logical consequence. Further, the discussion will turn out to be easier if a term is available that applies to both deductive systems and model-theoretic semantics. I will use 'formal system' to cover both.

So, we are looking for formal systems which capture, codify, axiomatise, characterise and/or formalise in some way the notion of *logical consequence*. One might hope that the meta-theoretic results of soundness and completeness deliver something informative concerning the question whether we succeeded or failed in our endeavour. One might, for example, be tempted to read the soundness result as: "We will not deduce a sentence from a class of premises that is not a logical consequence of them" (we will come back to this), and the completeness result accordingly as: "We can deduce every sentence from a class of premises that is a logical consequence of them". This should give us pause, however.

Logical truths are (by definition) logical consequences of the empty class of premises and hence, by monotonicity, logical consequences of every class of premises. Certainly a logical truth of first-order logic cannot be deduced in the deductive system of propositional logic. Propositional logic is complete, and is a proper logic if anything is. Yet a logical truth like '$(\forall x(Fx \supset Gx) \wedge Fa) \supset Ga$' escapes its consequence relation. The solution to this "puzzle" is of course simple. Propositional logic lacks the expressive power that first-order sentences require. It can therefore not be accused of not capturing logical truths of predicate logic. Something similar seems to hold for the relation between first- and second-order logic. Also note that, as stated above, completeness is a meta-theoretic relation that holds (or fails to hold) between a deductive system and a formal semantics. The two consequence relations we are dealing with are thus a deductive consequence relation and the model-theoretic consequence relation of a formal semantics. Completeness shows that every model-theoretic consequence of a set of sentences is also a deductive consequence of those sentences. But how does *logical* consequence get into the picture?

We are dealing with two formal systems here, the deductive system and the formal semantics, each of them equipped with their own consequence relation. If there is a soundness and a completeness proof we know that this duplication does not matter: their respective consequence relations are co-extensive. If we now know for some independent reason that we can infer nothing but logical consequences of given premises according to one system, then this will be the case for the other system, too. If completeness fails, however, this guarantee is not available.

If in such a case one decides that it is the deductive system that faithfully captures the pre-theoretic notion of logical consequence, one will presumably hold that the model theory is defective in the sense that it produces a surfeit of consequences of a set of sentences which are not actually logical consequences of it. One might then say that the model theory does not provide an appropriate model of the logical consequence relation that is specified by the deductive system, and consequently reject the semantics. John Etchemendy seems to hold such a view with respect to second-order logic.[8] (I discuss Etchmendy's views on logical consequence in chapter 9.) The failure of completeness does not disqualify the deductive system from capturing logical consequence and therefore from being a proper logic (other features might still have this effect, of course). If, on the other hand, one has convinced oneself that model theory is the system that properly codifies logical consequence, one will presumably think that the right thing to do, when one wants to do logic, is just that, viz. model theory. It is at least *prima facie* hard to see why the lack of a complete deductive system should cast doubt on the model-theoretic system as a logic if one has independent reasons to believe the model theory to properly capture logical consequence.

Part of the importance of soundness and completeness results is thus that they inform us about important features of the two systems. For example, they show that results from the one system can be carried over to the other.[9] If I prove a theorem of first-order logic then I know, by soundness, that this sentence is valid, i.e. it is true in all models. If I provide a model that makes a sentence false, again by soundness I know that I will not be able to prove it. Completeness allows us to make these transitions in the reverse direction. A model-theoretic argument can establish that a sentence is a consequence of some other sentences. If completeness holds one knows that there is also a derivation in the deductive system that establishes this result. The reason to prefer a proof to be carried out in one or the other system might have to do with the time it takes to carry out the proof, lemmata that are available that will aid one in the proof, or other matters of convenience. Essentially,

[8](Etchemendy, 1990), esp. pp. 158–159.
[9]See e.g. (Cutler, 1997), p. 80.

however, we have two formal systems that in a sense do the same job, and deliver the same results, which cannot be a disadvantage.

Now consider the case of a second-order inference and imagine a logician who is convinced that the standard model theory properly captures the logical consequence relation. A derivation in the deductive system is, by soundness, as good as a model-theoretic argument as a means of showing that the conclusion of the derivation is a logical consequence of its premises. The mere fact that there are semantic consequences for which there is no derivation in the deductive system does not throw any doubt on it tracking the logical consequence relation (again, other considerations might well do so).

I said above that one might be tempted to think that a soundness proof shows us that one can only derive logical consequences in a deductive system. It seems quite obvious now that this can only be the case if one already has established independently that the model-theoretic consequence relation embraces only logical consequences.

Soundness proofs can, however, establish something like relative consistency. If we know, for whatever reason, that the model theory is consistent, then a soundness proof assures us that the deductive (or axiomatic) system that is sound with respect to this (consistent) model-theoretic semantics, is consistent, too. This latter point makes it clear that we need a proviso concerning all the mentioned benefits of the meta-theoretic proofs of soundness and completeness: the benefits are only available to the extent that the meta-theory can be trusted. For the extreme case, assume the meta-theory used to carry out the soundness and completeness proofs is inconsistent. This means that *everything* can be proven in it.[10] On this assumption,

[10]Assuming the logic of the meta-theory is classical, and not, e.g. a relevant logic in which *ex*

the soundness and completeness proofs carried out in this inconsistent meta-theory do not show anything, of course. There are also less dramatic ways in which the meta-theory might not be beyond doubt. It might, for example, make some substantial assumptions that should not be taken for granted in a given context. For the purpose here, I will assume that everything is in good order with the meta-thoery.

6.3 Refining the Picture

The rather loosely presented considerations above require refinements or explanation in three respects.

(I) I have been speaking of the logical consequence relation that is captured by a formal system. It seems better to speak of a *part* of logical consequence here, or of a sub-relation of the (pre-systematic) logical consequence relation, or something alike. Propositional logic, for example, is a proper subsystem of first-order logic, yet even if we had reason to believe that first-order logic captured logical consequence exhaustively and in its entirety we would not want to deny the status of a proper logic to propositional logic just because it fails to capture all of logical consequence.

Further, it is difficult to imagine how we should ever find out that we have indeed exhausted logical consequence. It seems that there is always the epistemic possibility that some modes of inference that are properly regarded as genuinely logical have been overlooked so far. Rayme Engel, for example, specialises in the construction of formal systems that capture hitherto unrecognised (allegedly) logical inferences.[11] Whether the systems presented by Engel are indeed genuinely logical

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falso quodlibet does not hold, i.e. in which a contradiction does not entail every sentence.

[11](Engel, 1976) presents a logic that contains as only primitive quantifier the binary 'more ... than' (as in 'more Fs than Gs are φ') on basis of which he can define other quantifiers, including

systems would have to be investigated in detail, but is of no concern here. Also without a contemporary witness, however, the point should be clear enough.

If, as I argue in chapter 8, logical consequence is indeed best captured with a deductive system, there might be reasons to believe (deriving from Gödel's incompleteness theorems) that *in principle* no one system can capture the whole logical consequence relation. Shapiro and others are, amongst other reasons, led to reject the attempt to characterise logical consequence by these Gödel-type reasons. These matters are discussed in more detail in chapter 7 and 8. It seems convincing, however, that even if Gödel-type reasons *can* indeed be avoided, Engel-type examples would still put us into a position where we could not be sure whether we have captured every logical consequence there is. What we can maybe hope for, though, is a characterisation of a well-defined fragment of the (pre-systematic) logical consequence relation. Propositional logic,[12] for example, seems to be a very good candidate for capturing properly the logical consequence relation of sentential connectives. I argue in chapter 8 that second-order logic is an adequate characterisation of generality in the common name-predicate analysis of sentences (see there).

(II) This also leads to the second point. I have been mentioning a pre-systematic (or, how some prefer, pre-theoretic) notion of logical consequence, and that the goal of construction of formal logics is to capture it. A couple of things need to be remarked concerning this. Firstly, if it was possible to give a real definition, and – best – a purely formal one, then it seems that despite my suggestion in the preceding two paragraphs, this would mean that a once-and-for-all codification

the usual first-order existential and universal quantifiers (but see on such quantifier also the first section of (Boolos, 1984b)). Other examples include (Engel, 1989) and (Engel and Yoes, Jr., 1996).

[12]There is of course a debate about *which* proposition logic is the right one: classical propositional logic, or one of the competitors? I cannot discuss this question here.

of logical consequence is achievable. Not only can this be turned on its head – the remarks above suggest that such a once-and-for-all, all-comprising definition is impossible – but I will give additional grounds in chapter 8 why such a definition is not to be hoped for.

Furthermore, I want to suggest neither that there has to be a determinate pre-systematic notion that we are out to formalise, nor that there is or can be none. I take it that, for the discussion here, my arguments are sufficiently general to ensure that it should play no role whether there is an objective logical consequence relation, or whether it is purely conventional, whether "logical consequence" is a rather vague ordinary language concept and thus subject to an explication in Carnap's sense, whether logical consequence is eventually characterised when the pre-systematic notion is brought into a reflective equilibrium with some useful formal notion, as Nelson Goodman[13] and, following him, Michael Resnik[14] would have it, or whatever. (Again, for more on these matters see my chapter 8.)

(III) The third point, finally, requires the most space for its refinement, and will take up rest of this section. In section 6.2 above I assumed in my discussion of what the presence or lack of a soundness or completeness can show, a picture that is too simple. The combinations of which consequence relation could match which one, and in which way, provide many more possible outcomes. In the following I attempt to give a more detailed picture of the landscape in the hope that in doing so it will also become even clearer what role the meta-theoretical results play.

Let us start with the example where an attempt to provide a formal system as an alternative to one that is in use already leads to the discovery of a surprising

[13](Goodman, 1983), pp. 63–64.
[14](Resnik, 1985).

mismatch between the original system and the new one. Let us say we have two deductive systems, X and Y. X is traditionally thought to capture a certain class of logical inferences. The new system Y is suggested because it contains some features which are thought to be advantageous in application; it might be that it allows for shorter proofs, for example. When the logician constructs Y she wants to come up with a system that is equivalent to X in the sense that any sentence is amongst the theorems of X if, and only if, it is amongst those of Y.[15] Now let us imagine it turns out that the two formal systems do not match in that sense. I will consider three ways in which this could be the case:[16] (1) Y is a proper extension of X, i.e. all theorems of X are theorems of Y, but not the other way around, (2) X is a proper extension of Y, or (3) the systems contradict each other, i.e. the union of the class of theorems of X and the class of theorems of Y is inconsistent.[17] The reaction to this as described above is to decide on one of the systems, the one that properly captures the pre-theoretic notion (if either of the two does). Let us say that in cases (1) and (2) one would convince oneself that the respective stronger systems do not generate theorems that lie outside the pre-theoretic notion. Now, in the first case it seems that the natural response would be henceforth to stick to the new system Y. It formalises a part of the pre-theoretic consequence relation that was hitherto unaccounted for. Since X is a subsystem of Y we know that all the sentences we have proved in the past in X also will be theorems of Y. In the second case one

[15] I restrict myself here and in the following to the theorems of the systems for the sake of the simplicity of the exposition. The picture extends straightforwardly to the consequence relations.

[16] One can in fact distinguish at least six relations in which two such systems can stand to each other: The three I discuss in the following, the case where the systems are disjoint, and two cases of overlap: in one the union of the systems is consistent, in the other one it is not. In any case, the resulting situations would be similar enough in the relevant respects to the ones I describe.

[17] We might differentiate between considering the union and the closure of the union. This would give us even more cases than mentioned in the previous footnote.

would probably try to fix Y, if possible, such that it captures the missing part of the consequence relation as well.

If we, on the other hand, come to believe that the "extra theorems" of the respective stronger deductive systems are illegitimate in the sense that they lie outside of what is warranted by the pre-theoretic notion, in case (1) we might try to weaken Y until it matches X and so does not generate these "extra theorems" anymore. (2) will now be the more interesting case, however. We are in the position of having discovered that the old system X allowed us to prove sentences which on reflection upon the pre-theoretic notion should not be provable at all. A likely response is to stick to the new system, Y, in the future and weaken the old one, X, to see which axioms or rules are responsible for the "false" theorems. Depending on how well entrenched X was, some revision of previous results derived in X might be necessary.

Case (3) is equally interesting. As before, one first has to find out in this case which of the two systems really does capture the pre-theoretic notion. And it might turn out that it is neither (as it might turn out in *all* cases). It seems that a case of type (3) is most likely to occur when the logician develops the new system because she is unsatisfied with the old one. She is then most likely to design a formal system directly according to reflection on the pre-theoretic notions and to be only little, if at all, guided by the old system. In any of these cases, though, the mere mismatch of the systems is not going to provide one with an answer to the question: Which of the systems is to be modified or rejected? But it is a valuable indicator that one has to look out for arguments and that some adjustments are needed in at least one of the systems.

It is worth emphasising that fit between two systems does not guarantee that the intended notion is properly captured. The original system might fail to provide theorems it should have, or have too many. If the new system is then designed to match the old one, this failure will carry over. The mere proof that two systems agree on their theorems cannot show that the pre-theoretic notion is captured unless one antecedently has independent reasons to believe that one does. Equally, in the case of a mismatch there is certainly no guarantee that one of the systems is the correct one. Both might be wrong.

I doubt that anyone would have deep objections to my case descriptions. If I am right about the relation between formal semantics and deductive systems with respect to pre-theoretic notions then the same picture must hold true in this case too. Let X and Y above be a deductive system and a formal semantics, respectively. (1) then corresponds to a situation in which we have soundness and incompleteness, as is the case for second-order logic with standard semantics, (2) would be completeness, but failure of soundness, (3) the failure of both. For what follows I will draw a distinction between a *deductivist* and a *semanticist* account of logic. A proponent of the deductivist account holds that the best way to characterise the pre-theoretic notion of logical consequence is by means of a deductive system, while the semanticist prefers model theory.

Let us consider (1), and the case of second-order logic with standard semantics on a semanticist account. In light of Gödel's incompleteness theorem there is no hope of coming up with some additional axioms or inference rules that will provide us with a deductive system that will allow us to deduce everything that is given by the standard model-theoretic consequence relation. Where there is a proof in the deductive system, however, we can rely on it, since the soundness theorem holds. For the rest we have to do model theory which, after all, on the semanticist assumption is the right place to look for logical consequence anyway. On the deductivist conception, however, the system that properly captures logical consequence is a deductive one, but for some purposes we might require a complete model theory. Rather than rejecting model-theory as a whole, we would employ a Henkin semantics.[18] (2) corresponds to second-order logic with entirely unrestricted Henkin models: completeness holds, but soundness fails.[19] If we decide to favour the deductive system we will restrict the class of Henkin models in a way that puts us into the position to prove soundness. Should we decide that the Henkin semantics captures what we are after we have to weaken our deductive system (however hard it might be to imagine cogent reasons for adopting this last option). Concerning (3) we again find ourselves in the position where both systems are most clearly up for discussion; although, it has to be stressed again, this is the case for all three scenarios.

Failures of match between formal systems which are meant to capture the same pre-theoretic notion show us that some reconsideration needs to be undertaken. The failure of soundness or completeness is a special and important case of this, and there are some well-known examples of such investigations which have given rise to fruitful developments in formal logic. Tarski's original model-theoretic account, for example, arose from his dissatisfaction with the previous characterisations of the concept of logical consequence.[20] This first account operated with a domain that does not change in cardinality.[21] Hence for an infinite domain, for any $n \in \mathbb{N}$ any sentence of the language that expresses ⌜there exist at least n objects⌝ comes out as a logical truth – a feature usually deemed undesirable. Perhaps even worse, for

[18](Shapiro, 1999), p. 51, suggests that Henkin semantics is the right tool to study the deductive system of second-order logic, for example.

[19]See (Shapiro, 1991), p. 88.

[20]See (Tarski, 1936b).

[21]See (McGee, 1992).

any finite domain it depends on its respective size which sentences of this form are logical truths. Up to the n that is the cardinality of the domain the sentences come out as logical truths, for greater n they come out as logical falsehoods. (A more detailed discussion of this can be found in chapter 9 of this thesis.) This is repaired in the model theory that we use today, and model theory has grown to be a very powerful and fruitful area of mathematical logic.

Should meta-theoretical studies show a mismatch between two systems – and one of those possible mismatches is what we commonly call semantic incompleteness – independent arguments have to be provided concerning which of the two systems (if any) is the one that properly captures the pre-theoretic notion. When the pre-theoretical notion that is to be formalised is that of logical consequence, incompleteness alone cannot serve as an argument to disqualify a system as a proper logic, since it does not provide us with a criterion as to whether we should side with the deductivist or the semanticist, i.e. whether to reject the deductive system or the model theory. Much less is it acceptable to dismiss both of a pair of formal systems, say the deductive system of second-order logic together with its standard model theory, on the grounds that they do not match.

6.4 Some Arguments for Completeness

Having the above framework in place makes it easier to assess arguments to the effect that completeness is required for a proper logic. It is only rarely the case, however, that the attempt is made. Usually it is assumed without argument that a proper logic has to be complete.

This tendency is witnessed, for example, by the otherwise conscientiously argued paper by Leslie Tharp (Tharp, 1975). To be fair, Tharp does not attempt to establish that completeness is necessary for the status *proper logic*, but rather assumes it for the purpose of his discussion. His paper is concerned with extensions of first-order logic that have the completeness property, especially the extension of first-order logic with the quantifier 'there are uncountably many'. While adding the quantifier 'there are countably many' to standard first-order logic leads to a system that is not complete with respect to the standard semantics, adding 'there are uncountably many' results in a system for which completeness can be proved. Tharp provides subtle arguments why, despite completeness, this extension of first-order logic should still be ruled out as proper logic. His discussion of these cardinality quantifiers is of no concern for the discussion of completeness here. Note, however, that both quantifiers are definable in second-order logic (see chapter 7); so if the case that is made for second-order logic in this thesis is successful, these quantifiers become available with it.

What is of interest, however, is that Tharp *presupposes* for his discussion that a proper logic should be complete. (Tharp, 1975) bears the title *Which Logic is the Right Logic?*, and Tharp is aware of the fact that making this presupposition in this context might seem a bit strong. He comments that

> one should not claim to see a priori that a logic must be complete in order to be a theory of deduction. The point is rather that when it is discovered that the best known candidate satisfies such a condition, that tends to establish a sense of logic and a standard to be applied to competitors.[22]

[22](Tharp, 1975), p. 6.

As already mentioned, it would not be charitable to interpret Tharp as intending to put this forward as an argument for completeness. As such it would, if valid, also license the easy rejection of second-order quantifiers, since the "best known candidate" (which presumable means the generally accepted one), i.e. first-order logic, satisfies the condition of not having them. Tharp, incidentally, acknowledges second-order logic as the most promising contender for the title, but remarks that "in fact it is not accepted".[23] Again, had Tharp intended the above quote as an argument, maybe one would have to resent what a close call it was: had second-order logic become (historically) the accepted logic, presumably, a proper logic would then have had to be *incomplete*, since the then generally accepted one (which would have been second-order logic) is. Surely, no such argument can be in good standing.

One way in which the existence of a completeness proof can indeed be interesting is pointed out by Georg Kreisel.[24] We are in the business of capturing the (pre-theoretic) notion of logical consequence. Now we convince ourselves that (i) any derivation in our deductive system is licensed by our pre-theoretical notion. Further, we convince ourselves that (ii) any pre-theoretically valid inference is also valid in the model-theory. A completeness proof then ties all three notions together and shows that they are all equivalent. This is easiest to see if we restrict the argument for the sake of simplicity to logical truths; it generalises straightforwardly to logical consequence. If a sentence is a theorem of the deductive system, then, by (i), it is also a logical truth. By (ii), any logical truth is a validity of the model theory. By completeness, then, any logical truth is a theorem, and so all three notions are co-extensive. (This is also assuming transitivity, which should be safe, however.)

[23](Tharp, 1975), pp. 6–7.
[24](Kreisel, 1967), pp. 152–157.

This does not show, however, that the considerations presented above in favour for rejecting completeness as a criterion for being a proper logic, are flawed in any way. As (i) and (ii) state, it first needs to be shown that the pre-theoretic notion is indeed sandwiched in this way between the deductive system and model theory. It does not follow from the impossibility of applying Kreisel's strategy that either the deductive system or the model theory fails to capture logical consequence. In the case of second-order logic, (i) and (ii), presumably still hold. Assuming standard semantics, completeness fails, and so the three notions cannot be tied together. This might show that the standard semantics declares sentences to be valid that are not logical truths[25], but it might just as well be the case that the deductive system misses some out. The third option is, of course, that *both* are the case.

One more thing needs to be remarked on Kreisel's sandwich technique. If indeed it is true, that step (i) – every theorem is a logical truth – holds for second-order logic too, how can it be that the technique works, and the completeness proof for first-order logic ties all three notions together?[26] The *second*-order theorems cannot be captured by this. This point is analogous to the one made in section 6.2 of this chapter. The Kreisel strategy can only work for first-order logic, if we restrict ourselves to a proper sub-relation of logical consequence, i.e. on whatever the deductive system and model theory of first-order logic applies to. We therefore have to concentrate on a specific deductive system and a model theory proposed for it, and then see if the relevant fragment of the pre-theoretic notion of logical consequence is couched between it. The success of this for *some* deductive systems and their respective model-theories, however cannot be turned into an argument against

[25]Indeed, this is what I will argue in chapter 7.

[26]This point does not rely on second-order logic being part of the example. The same can be observed for propositional logic in comparison to first-order predicate logic.

systems or semantics where this technique fails.

Sure enough, if we employ Kreisel's technique, and (i) and (ii) hold, but completeness fails, it cannot be the case that *both* systems capture the fragment of logical consequence in its entirety. It could, however, still be the case that the deductive system captures only, and even all, of the appropriate sub-relation of logical consequence. The model theory, then, would declare too many sentences to be validities. On the other hand, it could be the model theory that captures all and only the logical truths of the appropriate fragment of the pre-theoretic notion. Steps (i) and (ii), then, would provide us with a kind of soundness proof for the deductive system. Than means that any theorem we prove in the deductive system would still be a a logical truth, while on the other hand not all logical truths can be proven as theorems.[27]

Let me repeat the point made in the sections above: without anything like Kreisel's steps (i) and (ii) in the background, soundness and completeness proofs show no such thing as that a characterised consequence relation is logical. The only thing they show is that the consequence relation of the given deductive system is co-extensive with the one of the chosen model theory. The pre-theoretic notion of *logical* consequence might not come into play at all. John Etchemendy puts this point something like this:[28] image a co-extensiveness proof for two deductive systems, S and T. What they show is that

$\Gamma \vdash_S \varphi$

[27]There is as a third option the possibility of error: There could be an overlooked ambiguity involved in that (i) established that every theorem is a logical truth of one fragment of logical consequence, while (ii) established that every logical truth within *another* fragment of logical consequence is also a model-theoretic validity. This possibility is of no systematic interest in the discussion here.

[28](Etchemendy, 1990), pp. 3–4, see also pp. 144–148.

holds if, and only if,

$$\Gamma \vdash_T \varphi$$

Would this provide an argument that both '$\vdash_S$' and '$\vdash_T$' characterise logical consequence? Surely not; both deductive systems might even be inconsistent, and the equivalence would still hold. This insight, however, does not depend on whether we write '$\vdash$' or '$\vDash$'. In the end, both merely designate the consequence relations in formal systems – that of a deductive system and that of a formal semantics, respectively. A guarantee that one or the other characterises the properly *logical* consequence relation is not to be gained from this, but rather has to be provided additionally.

A different strategy to deny second-order logic the status of a proper logic due to its incompleteness is put forward by Stephen Wagner in his *The Rationalist Conception of Logic* (Wagner, 1987). Wagner provides two arguments as to why a proper logic has to be complete, both of which draw on epistemic considerations. Wagner employs a notion of *ideal justification* which will also play a crucial role in my argumentation of chapter 8. Starting from a set of premises, a proper logic should provide a way to arrive at a consequence which is ideally justified by these premises. Wager spells this out like this:

> Any deductive consequence C of a set Σ of statements can [...] be mechanically calculated: a finite series of steps leads from premises in Σ to C, with each step governed by a rule the applicability of which can be recursively determined. [...] But if it is mechanically determinable whether a given argument is in fact a proof of logic, there must also be a recursive characterization of the entire set of logical rules. This suffices

> for the recursive enumerability of the consequence relation, that is, for completeness. Thus it is natural to identify logic with FOL [first-order logic], since completeness is typically lost when we go beyond it.[29]

The notion of completeness that Wagner operates with here is not that of semantic completeness that was discussed so far, i.e. that every deductive consequence is also a semantic one. The feature of a consequence relation that Wagners draws on is called *effectiveness* by Shapiro:[30]

Effectiveness: A consequence relation is *effective* if, and only if, the class of consequences it determines is recursively enumerable.

A logic is complete in this sense if, and only if, its consequence relation is effective. The first argument that Wagner puts forward can hence be reconstructed like this:

(i) In a proper logic, any sentence which is a deductive consequence of a set of premises can be mechanically calculated.

(ii) If a consequence can be mechanically calculated, the consequence relation is recursively enumerable.

(iii) If a logic has a recursively enumerable consequence relation, then it is complete in the sense that it has an effective consequence relation.

(iv) Therefore, completeness is a necessary condition for a proper logic.

(i) through (iii) are not very informative, and hence it is surprising that the strong conclusion (iv) should follow from it. (i) says that a deductive consequence has to be

[29](Wagner, 1987), p. 8.
[30](Shapiro, 1991), p. 5.

mechanically calculable, but this is trivial: what is a deductive consequence of some premises is determined by a purely syntactical derivation in the deductive system of a logic. Natural deduction systems might appear to require a certain amount of "creativity" to find the way from the premises to the conclusion, but it is usually agreed that tree (or tableaux) systems are sufficiently mechanical to qualify. A tree system for second-order logic that is equivalent to its standard deductive system was introduced by Richard Jeffrey.[31]

(iii) is just the definition of the notion of completeness as given above. This leaves us with (ii), which is, assuming the correctness of the Church-Turing thesis, more or less a definition of 'mechanically calculable'.[32] As Wagner puts it, "calculability is properly understood as nothing less than recursiveness".[33] The problem of decidability might lurk here which I will put to the side for a while, as this is important for Wagner's second argument and will be discussed in this context.

We have to ask, however, what the consequence relation is, that is talked about in each of the statements. (i) talks explicitly about deductive consequence. If completeness (as defined above from effectiveness) is to hold as a criterion to rule out second-order logic, however, the consequence relation relevant there has to be semantic. Otherwise, the condition Wagner cites merely demands that the the class of deductive consequences is recursively enumerable. This demand is met, however, not only by first-, but also by second-order logic.[34]

The semantic completeness of first-order logic shows that in its case the semantic consequence relation is also recursively enumerable. It follows from the semantic

[31](Jeffrey, 1967), chapter 9; compare also (Bell et al., 2001), chapter 3.3.
[32]Compare (Shapiro, 1991), p. 50.
[33](Wagner, 1987), p. 8.
[34]See (Shapiro, 1991), p. 161.

completeness of second-order logic with respect to a Henkin semantics show, that also this semantic consequence relation is recursively enumerable. As second-order logic is incomplete with respect to the standard semantics, we cannot have a guarantee in this way that the standard semantic consequence relation is recursively enumerable – and in fact we cannot have *any* such guarantee, since it is *not* recursively enumerable.[35]

Wagner, hence, is faced with a dilemma: either his first argument here is equivocating on 'consequence', or he presupposes that deductive and semantic consequences coincide. The latter, however, means presupposing semantic completeness. At best, the argument, hence, does not go through in light of the considerations concerning semantic completeness presented above: if semantic completeness is lacking, one has to decide whether one wants to claim that the deductive system or the model-theoretic semantics correctly characterises logical consequence. Given the conception of logic that Wagner advances it seems that he should side with the deductive system – compare (i). Then it is not clear, however, why he should demand that the deductive consequence relation must be complete with respect to the model-theoretic semantic. That in the case of second-order logic the standard semantic consequence relation is not recursively enumerable should not matter: it does not represent logical consequence anyway. In the worst case, the argument might be construed as begging the question.

That Wagner's *Rationalist Conception of Logic* is best thought of as deductive in nature is also witnessed by his description of how one has to proceed according to it. Proofs have to be broken down into "immediate steps" that are "self-evident".

[35]Compare (Shapiro, 1991), p. 162; see also (Boolos and Jeffrey, 1985), pp. 203–204.

Wagner bases his account on "Fregean rationalism".[36] It is indeed very much in the spirit of at least some of Frege's remarks about logic. Wagner does not quote Frege,[37] but Frege's remarks are quoted and discussed in my chapter 8. (The quotations are of no immediate concern here, however.)

It is slightly surprising that Wagner writes: "I think that for Frege, logic must turn out to be FOL [first-order logic]."[38] It was Frege, after all, who not only was the first to present a rigorous formulation of a conprehensive formal system of predicate logic in his *Begriffsschrift*,[39] – inclusive of second-order quantification – but also put it to such an elaborate use in his *Grundgesetze der Arithmetik*[40] [Basic Laws of Arithmetic]. Wagner, however, claims on basis of his second argument that the Fregean position, properly understood, would limit this conception of logic to first-order. Completeness, again, plays an important role in this.

Wagner characterises the advantage of the position he derives from Frege like this:

> [I]mmediate steps [...] have the epistemic property of being self evidently, transparently correct, as directly obvious as anything can be. A logic with a consequence relation based on immediate steps then offers an epistemic gain. Once we have a proof of Q from P in such a logic, we know that the argument for Q need not and cannot be made more persuasive or clear.[41]

Wagner identifies these "immediate steps" with single applications of basic rules

[36] (Wagner, 1987), p. 4.
[37] See, however, his characterisation of the Fregean position (Wagner, 1987), pp. 3–9.
[38] (Wagner, 1987), p. 7.
[39] (Frege, 1879).
[40] (Frege, 1893) and (Frege, 1903).
[41] (Wagner, 1987), p. 13.

of inference, like *modus ponens*, conjunction elimination, or the introduction of the existential quantifier, for example. He explains that on the Rationalist Conception of Logic it would be desirable to be able to check every sequence of sentences mechanically to determine whether it is a proof of the last sentence of the sequence or not. *Decidability* would thus be called for:

Decidability: The consequence relation of a system S is *decidable* if, and only if, there is an effective method for telling of each sentence in the language of S whether or not it is a theorem of S.

Full first-order logic, alas, is not deciable.[42] Wagner contends, however, that in fact something weaker already suffices:

> Church's theorem rules out a decision procedure for logical implication in FOL.[43] But we can have something weaker that is still of interest and has traditionally been taken for granted: an effective way to tell whether Q follows from P *by an immediate step.* Views connecting logic to inference and argument give the consequence relation more structure than we have so far allowed. They recognize a decidable collection of immediate inferences (e.g., universal instantiation, modus ponens, conjunction introduction) such that whenever $R(Q, P)$ [i.e. P follows from Q according to the consequence relation R] there is a chain $Q, Q_1, Q_2, ..., Q_n, P$ in which each link follows immediately from its predecessor. This condition, together with the decidability of immediate consequence, restricts us to complete logics [...].[44]

[42] This is entailed by Church's negative answer to the *Entscheidungsproblem*; see (Church, 1936).
[43] See footnote 42 above.
[44] (Wagner, 1987), p. 13.

We can note that a similar situation as with the first argument arises again. If the consequence relation is meant to ultimately proceed by "immediate steps", and these steps are single applications of basic rules of inference, then the consequence relation must be characterised deductively. If the remark about the decidability of immediate consequence, however, is mean to be the reason why we are restricted to complete logics, it must be understood as referring to semantic consequence. This argument, too, is thus either equivocating on 'consequence' or presupposing semantic completeness, in the same way the first one above does.

The situation can be described more intuitively like this: the method Wagner describes for characterising logical consequence as based on basic rules of inference (incidentally, this is roughly the method I suggest in chapter 8) demands that it must be possible to break down every proof into single applications of the basic rules of inference. Only in this way can we be sure that each consequence we derive is ideally justified by way of self-evident steps on the basis of the premises that it is derived from. This means that we must be able to decide effectively whether something is an application of a basic inference step. Since there are only finitely many basic rules of inference, we can do this (vagueness and the like are presumably idealised away). It makes no difference for this, however, whether we are confronted with a deductive system of first-, or of second-order logic. Second-order deductions also can be broken down into steps of single applications of basic inference rules, and second-order logic also has only finitely many basic rules of inference. To demand additionally, that in this way we must be able to get a step-by-step proof for every *semantic*, and not just every deductive consequence of a set of premises, means additionally demanding semantic completeness. This, however, is precisely what is under discussion, and what Wagner argues follows from his considerations (maybe with a detour via effectiveness, but in this case the arguments I gave above apply again). It does not follow from them, however; or rather the demand for completeness follows only if completeness is antecedently demanded.

A few more words about decidability should be useful. As mentioned above, full first-order logic is not decidable. *Monadic* first-order logic, however, is. Boolos asked already in his *On Second-Order Logic* why the emphasis is always made on completeness, when decidability seems to be as desirable a feature.[45] Presumably, restricting ourselves to monadic first-order logic would leave us with too weak a logic to be useful for any interesting purpose. It might also seem that the system we are left with seems to be *incomplete*, in want for a better word, meaning that some integral seeming parts, i.e. many-place predicates, have been left out. Monadic first-order logic might not give the impression of an organic whole. The same can easily be claimed on basis of the lack of second-order quantifiers, however. Allowing to have devices that generalise over the positions of some expressions of the language (names), but not others (predicates), also might appear unsatisfactory.

Additionally, and with respect to decidability, it should be noted that monadic *second*-order logic is decidable, too. Already Leopold Löwenheim's celebrated original decidability proof for monadic predicate logic included monadic second-order quantification.[46] More recently, Michael Rabin has presented a proof for decidability of monadic second-order theory with two successor functions.[47]

That decidability is not available if we go beyond monadic logics is insurmountable. It is tempting, however, to side with Wagner in the following:

> Perhaps we can grant that we should at least have the positive half of a

[45](Boolos, 1975), pp. 50–51.
[46](Löwenheim, 1915), p. 462, Satz 5; compare also the respective remark in (Gandy, 1988), p. 61.
[47](Rabin, 1969).

> decision procedure for the class of proofs: a way to establish mechanically that something is a proof, if it is.[48]

This means that it should be possible to verify that something is a proof, if it is one. If we settle for a characterisation of logical consequence with a deductive system, this is available by the "method of single steps" described above, both for polyadic first- and for polyadic second-order logic.

Darcy Cutler, in his review of (Shapiro, 1991), makes it clear that all these demands on the meta-theoretical properties of the consequence relation are epistemic in nature.[49] He follows Shapiro in distinguishing between a "foundational" and a "semantic" approach to logic.[50] The foundational approach seems to coincide with what I called 'the deductivist approach' above. Shapiro coins the term because he diagnoses foundationalism in the philosophy of mathematics as a reason for the preference for the deductive system. Foundationalism is roughly the view that mathematics can be derived from first principles or a secure "bedrock" of non-mathematical truths.[51] Logicism, for example, was a foundationalist project. The basic principles from which, e.g., Frege and Russell wanted to derive mathematical knowledge were principles of logic, or at least what they took to be principles of logic. Frege's system turned out to be inconsistent due to his Basic Law V, and Russell's Theory of Types is today widely regarded as containing too much mathematics still to count as logic.

There are certainly other reasons to prefer the deductive approach to the model-theoretic (or semantic) one than foundationalism. Moreover, giving one of the two

[48](Wagner, 1987), p. 12.
[49](Cutler, 1997), pp. 79–81, 84–85.
[50](Shapiro, 1991), p. 35–40; (Cutler, 1997), p. 79.
[51](Shapiro, 1991), p. 26.

competitors a name that alludes to a position in the philosophy of mathematics whose historically most prominent form – logicism – is commonly regarded as having failed today appears slightly tendentious – it hardly comes as a surprise that Shapiro himself argues that "the semantic [conception of logic] is more plausible."[52] Another reason to prefer the deductivist approach is epistemic. The epistemic advantage of the deductive account is what Cutler argues for, and also the background for the position I develop in chapter 8. Since this has little to do with foundationalism as a motivation to prefer a deductive over a model-theoretic system, I will stick to the label 'deductivist', rather than adopt Shapiro's label 'foundational'.

Cutler submits that "logical consequence is an epistemic relation" for the deductivist:

> 'c is a logical consequence of P' means that given knowledge of the certain truth of P, the truth of c can also be known with certainty. On [this] approach, knowledge of logical consequence arises ultimately from deduction. We show that c is a logical consequence of P by deducing c from P.[53]

Cutler further remarks that for deductivists "who accept semantics, completeness is a desirable property of a logical system."[54] Deductivists believe that logical consequence is best characterised using a deductive system. If a semantics is provided that is both sound and complete with respect to the deductive system that one believes to correctly codify (part of) logical consequence, then one can rest assure – doubts about the meta-theoretical proofs aside – that "semantic consequence does

[52](Shapiro, 1991), p. 35.
[53](Cutler, 1997), p. 79.
[54](Cutler, 1997), p. 80.

not outstrip deductive consequence".[55] Thus even from a deductivist perspective, model-theoretic arguments can be trusted in this case. Completeness guarantees that the conclusion can also be deduced from the premises in the relevant deductive system. This was stressed in the previous section already.

Cutler then, however, concludes that such a deductivist must reject second-order logic as it is incomplete.[56] As stressed above, this does not follow: what the deductivist has to reject is (merely) the standard semantics for second-order logic, or rather what must be rejected is that its consequence relation is logical. There is no need, though, to abandon on these grounds the deductive system of second-order logic. Rejecting the claim that a certain model-theoretic semantics produces only logical consequences should not in general be a big problem for a deductivist, however.

There might be interesting psychological mechanisms at work that make someone who is overtly a deductivist (like Wagner, if my interpretation of his arguments is correct) subconsciously believe that the semantic characterisation of logical consequence is nonetheless the standard which the deductive system must live up to. A similar situation might arise for a semanticist who does not want to let go of the epistemic advantages of the deductive system but refrains from taking a deductivist position. I will not speculate about the psychological reasons for believing either. I have argued that both positions are untenable.

[55](Cutler, 1997), p. 80.
[56](Cutler, 1997), p. 81.

6.5 Conclusion

I argued above that a completeness proof can provide valuable insights into the properties of formal systems. This is because soundness and completeness really constitute equivalence proofs. The failure of equivalence of two systems which are meant to formalise the same pre-theoretic notion – in the case at hand the notion of logical consequence – suggests that these systems have to be investigated again to see what is wrong with at least one of them. One will always have to provide independent arguments, however, why a formal system under discussion does not properly capture (part of) logical consequence. The mere perfect match of a model theory to a sound deductive system, i.e. completeness, *cannot* provide such an argument unless one has already established on independent grounds that one of the systems does in fact properly capture this part of logical consequence.

Suppose, for example, that the standard model theory properly captures logical consequence. It should therefore be the case that the best possible outcome for a deductive system would be that it is sound and complete with respect to standard model theory. If soundness fails, the deductive system must be dismissed, since, say, it allows to derive theorems that are not validities, i.e. – in this case – logical truths.[57] If completeness fails, however, this surely does not show that the model theory did not properly capture logical consequence. The deductive system on this view is a potentially useful *addition* to the model theory. The latter cannot to be judged with reference to the former, as it is *supposed* that the model theoretic way

[57]In principle soundness could fail without this being the case. All theorems could be logical truths, but some deductive consequences would not be semantic consequences. In this case, the deduction theorem has to fail, which states that 'q' is derivable from 'p' in the deductive system S if, and only if, '$p \supset q$' is a theorem of S. This can be generalised to sets of premises: just take 'p' to be the conjunction of all premises in the set.

is the correct one.

The same holds *mutatis mutandis* if the deductive system is given precedence. All that should be required to get a semantics relative to which any given deductive system is complete is a sufficiently cunning model-theorist. Whether this semantics answers to the pre-theoretic notion that was meant to be captured is an entirely different issue and must be argued for independently. Of course, should it turn out that it does not capture that notion, and the deductive system is sound and complete with respect to this semantics, so much the worse for the deductive system. But note that in such a case a *completeness* theorem, rather than an incompleteness result, seals the fate of this system. If the logic starts out as a model-theoretic one, it is much less clear whether we can find a complete deductive system for it or a complete axiomatisation of it. Because of Gödel's incompleteness results, a complete deductive system cannot generally be expected to be available. But the mere lack of such a system cannot count as showing that the model-theoretic logic is not a proper logic, especially not if we independently decide that the model theory properly captures the part of the logical consequence relation we set out to characterise.

The considerations of this chapter certainly do not yet establish that second-order logic is proper logic. What I have argued for is merely that the incompleteness of deductive systems of second-order logic with respect to standard semantics alone does not show that either of the systems is *not* a proper logic. Chapters 7 and 8 will investigate how second-order logic fares on the semantic and on the deductivist account.

Chapter 7

The Semantic Conception

7.1 Introduction

Taking up the thread from last chapter, this chapter too will be concerned with how to characterise logical consequence properly. Stewart Shapiro's suggestion is that employing the model theory of the standard semantics of second-order logic provides the correct way to do so. His argument is from the role he assigns to second-order logic in codifying mathematical practice. All his arguments that will be discussed below and that suggest that first-order logic is insufficient for this purpose, straightforwardly apply to second-order logic interpreted with a Henkin semantics, too.[1] Usually, I will therefore not mention this separately in the following. Shapiro's claim that first-order logic is not suitable for mathematical practice[2] is investigated in detail in sections 7.2 and 7.3 below. Section 7.4 will deal with various objections to the acclaimed advantages of second-order logic (with standard semantics).

The main proponents of the usefulness of second-order logic on this concep-

[1](Shapiro, 2005a), p. 771.
[2](Shapiro, 1985); see also (Shapiro, 1991), esp. chapter 5.

tions are, next to Shapiro, Georg Kreisel[3] and John Corcoran.[4] The argument from mathematical practice runs like this: important branches of classical mathematics obviously have intended interpretations (*pace* Hilbert); these branches include theories like arithmetic, real and complex analysis, and prehaps set theory. Their first-order axiomatisations are found unsatisfactory by Shapiro, since they allow for non-standard interpretations.[5] First-order analysis, which is intended to be about the real numbers, for instance, has a model in which the domain is countable, although Cantor's famous diagonalisation shows that the set of reals is uncountable.

Shapiro thus submits that second-order axiomatisations are to be preferred. The second-order versions of arithmetic and analysis are *categorical*, as introduced in chapter 2 above, and explained in detail in section 7.2.1 of this chapter below. Categorical axiomatisations have a *unique* model, up to isomorphism – all models are isomorphic to each other, and thus to the intended one. All "non-standard" models of the wrong cardinality are ruled out. Shapiro concludes that if logic is supposed to codify correct inference, then for the realm of mathematics it has to be second-order logic, as first-order logic cannot codify the correct inferences in a second-order language.[6]

We will see below that some substantial mathematical content comes with the model-theoretic consequence relation. In particular, a fair bit of set theory is, in a sense that is spelled out in detail below, included in the model-theoretic consequence relation. This might seem not too surprising since the standard model theory is formulated in set theory. This is also the case, however, for Henkin semantics

[3]See (Kreisel, 1967).
[4]See e.g. (Corcoran, 1973), (Corcoran, 1980) and (Corcoran, 1987).
[5](Shapiro, 1985), pp. 714–715.
[6](Shapiro, 1985), p. 716.

and also for first-order logic. The standard model theory of second-order logic is more intimately entwined with set theory, though, as will be discussed in detail in section 7.4 below. So, it seems, that for the *semantic conception* of logic at least, Quine's claims that if one uses second-order logic, a fair bit of set theory slips in unheralded[7] is true after all.

Shapiro is the first to accept that. He claims that given the need for second-order resources to do justice to mathematical practice, this has to be accepted. So, while for Quine set theory is the last resort to make sense of higher-order quantification, as explained in chapter 3 of this thesis, Shapiro embraces the set theoretical presuppositions of the model-theoretic consequence relation from the start. He also draws a further conclusion:

> Quinean holism is extended to logic itself. There is no sharp border between mathematics and logic, especially the logic of *mathematics*. One cannot expect to do logic without incorporating some mathematics and accepting at least some of its ontology.[8]

Shapiro quotes Jon Barwise for support:

> As logicians, we do our subject a disservice by convincing others that logic is first-order and then convincing them that almost none of the concepts of modern mathematics can really be captured in first-order logic.[9]

This, however, does not show that second-order logic on the semantic conception is

[7](Quine, 1986a), p. 68.

[8](Shapiro, 1991), p. vi; Shapiro's emphasis. See also (Shapiro, 1985), p. 716; (Shapiro, 1991), p. 48; (Shapiro, 1999), p. 51; (Shapiro, 2005a), p. 772.

[9](Barwise, 1985), p. 5.

indeed logic[10] – proper or not. It also does not show that the use of it is legitimate at all (prescinding from the issue of logicality altogether). One might want, or even *need*, second-order logic for certain purposes one has in mind, but that does not show that one can also *have* it. Let us, however, review in detail the advantages that Shapiro claims for the standard model theory of second-order logic before discussing the problems that come along with it.

7.2 Mathematical Theories and their Intended Interpretations

The goal of this section is to explain in detail why Shapiro contends that first-order axiomatisations are unsatisfactory for classical mathematical theories with intended interpretations.[11] The first of the two subsections will examine the advantages of categorical axiomatisations and explain why these cannot be had in first-order languages. The second subsection is about the mathematical practice of embedding a weaker theory into a stronger one in order to facilitate proofs about the weaker theory. The claim here is that no first-order axiomatisation can make sense of this practice due to the lack of categoricity. To explain why this should be so is the aim of the two subsections below.

Shapiro's conclusion is that the correct logic for mathematics has to be (at least)[12] second-order:

[10]This is also remarked by (Jané, 2005), p. 789.

[11](Shapiro, 1985), pp. 714–715.

[12]The presence of the 'at least' here and in the quotation below pays tribute to the fact that it is not established by any of Shapiro's arguments that second-order logic will suffice. It might be that it will turn out that a higher-order logic is needed that goes beyond second-order. Compare concerning this the *partial* reduction result for higher-order logic to second-order by (Hintikka,

One of the purposes of logic is to codify correct inference. Thus, if my major conclusions are correct, the underlying logic of many branches of mathematics is (at least) second-order: one cannot codify the correct inferences of a second-order language with a first-order logic. It follows that the inconvenient technical properties and presuppositions of second-order logic must be accepted. The correct conclusion, I believe, is that there is no sharp boundary between logic and mathematics. The study of correct inference, like almost any other science, involves some mathematics and some mathematical presupposition.[13]

7.2.1 Categoricity

The categoricity result was already stated in chapter 2 above. To recapitulate, (Corcoran, 1980) defines the notion like this:

Categoricity: A set of sentences S is *categorical* if, and only if, every two models (or interpretations) which satisfy S are isomorphic.[14]

A mathematical theory is standardly defined as a set of sentences that contains the axioms in question and is closed under logical consequence. *A fortiori* an axiomatised mathematical theory T is *categorical* if, and only if, every two models which satify T are isomorphic.

1955); see also (Shapiro, 1991), section 6.2. Note that the proof as given depends on the first-order domain being set-sized, and thus does not hold for second-order set theory which would presumably be the most interesting case. Note also, that the result only applies to the class of second-order validities, and *not* to the consequence relation in general. This could be taken to limit its interest significantly.

[13] (Shapiro, 1985), p. 716.

[14] (Corcoran, 1980), pp. 190-191; see also (Shapiro, 1991), pp. 12, 82.

Shapiro glosses this as that a categorical axiom system has “essentially only one model” which is its intended interpretation.[15] All other models of the theory are isomorphic to this standard interpretation and thus display the same structural features. Shapiro contends that it is essential to the practice of mathematics that mathematicians pick out the structures they want to talk about; presumably they are able to do so. It seems right to say that a mathematician asserting that there are infinitely many prime numbers makes this assertion about the natural numbers which are the domain of the intended model of the Peano-Dedekind axioms for arithmetic.

Shapiro argues that classical mathematical theories, like arithmetic and analysis, have intended interpretations:[16] for arithmetic it is the natural numbers, for real analysis the real numbers, for complex analysis the complex plane, and (perhaps)[17] for set theory the iterative hierarchy. Categorical characterisations of these classical mathematical theories,[18] however, must be second-order. “Many writers,” as Corcoran writes, ”have noted that the many known categorical characterizations of the familiar classical systems all involve languages of second order, at least.”[19] These authors prominently include Georg Kreisel[20] and Richard Montague.[21]

In first-order languages the theorems of Löwenheim-Skolem and Löwenheim-Skolem-Tarski hold (see chapter 2), and they obviously directly contradict cat-

[15](Shapiro, 1991), p. 82.

[16](Shapiro, 1985), p. 715; (Shapiro, 1991), p. 117.

[17]The qualification is due to the existence of “alternative” set theories, like Peter Aczel’s Antifoundational Set Theory. This set theory asserts that some sets contain themselves, contradicting Zermelo-Fraenkel set theory (ZF) and not being in accordance with the iterative hierarchy (although the axioms can be modeled on it, see (Aczel, 1988)). Quine’s system of *New Foundations*, (Quine, 1937), that was already mentioned in chapter 3, is another example, and there are others.

[18]Quasi-categorical, in the case of ZF – see chapter 2 above.

[19](Corcoran, 1980), p. 192.

[20](Kreisel, 1967).

[21](Montague, 1965).

egoricity. (I will for the sake of simplicity henceforth refer to the Löwenheim-Skolem and Löwenheim-Skolem-Tarski theorems together as 'Löwenheim-Skolem theorems'.) The Löwenheim-Skolem Theorem states, that if a set of first-order sentences has an uncountable model, then is also has a countable model. The intended model of the first-order theory of real analysis is uncountable – there are uncountably many reals – but by Löwenheim-Skolem it also has a countable model. The categoricity of real analysis claims that all its models are isomorphic; thus a categorical axiomatisation cannot be first-order. Analogously, the intended model of Peano Arithmetic is countable; by Löwenheim-Skolem-Tarski (sometimes call "Upwards Löwenheim-Skolem Theorem") it has uncountable models, too, and hence a categorical characterisation of arithmetic cannot be first-order. These considerations also show that second-order logic with Henkin semantics has the same shortcoming as first-order logic in this respect: the Löwenheim-Skolem theorems hold here, too, so no categoricity results for infinite structures can be available. Kreisel observes that all finite structures are categorically characterisable in first-order, but no infinite stuctures are. He thus concludes that 'being first-order categorically characterisable' is co-extensive with 'being finite'.[22]

It can further be observed that, in general, the logic that is used to give categorical axiomatisations with an infinite intended domain *cannot* be semantically complete, at least not if the deductive system is sound. To see this it suffices to show that categoricity contradicts compactness, since, as mentioned in chapter 2, compactness is a simple corollary of soundness and completeness (see there). It is easy to see that that categoricity contradicts compactness, however.

Theorem: No logic in which a categorical axiomatisation of arithmetic can be given

[22](Kreisel, 1967), p. 148.

has the compactness property.

Proof: Assume compactness for *reductio*. Consider the set of sentence Γ that is the union of the set of the second-order axioms of arithmetic with the (infinite) set Δ that contains every sentence of the form $\ulcorner n \neq s^i 0 \urcorner$, where '$s$' stands for the successor function, 'n' is an individual constant, and $\ulcorner s^i \urcorner$ denotes a sequence of 's' of length i, for $i \in \omega$ (including 0). (That means that the sentences in Δ say that 'n' neither denotes 0 nor any of its successors, i.e. that it does not denote a natural number.) Every finite subset of Γ is obviously satisfiable. Hence, by compactness, Γ is satisfiable. It is not satisfied by the intended model, however. By hypothesis the axioms are categorical, however, and thus all its models are isomorphic to the intended one. Contradiction. $\square$

This result generalises in the expected way.[23] No sound and complete logic thus can give categorical characterisations of infinite structures.[24]

It is in this sense that Shapiro claims that "completeness is most emphatically not desired".[25] It is crucial for Shapiro's project that one can provide categorical axiomatisations for the classical mathematical theories. This cannot be done with any complete (and sound) logic. The "inconvenient technical properties" of second-order logic with standard semantics, like incompleteness and non-compactness, therefore, "must be accepted."[26]

Most prominently, Thoralf Skolem challenged the claim that infinite mathematical structures can be characterised up to ismorphism. From his (Skolem, 1922) on, Skolem argues on basis of the theorems that bear his and Löwenheim's names that

[23](Shapiro, 1991), pp. 112-113.
[24](Shapiro, 1985), p. 718; (Shapiro, 1991), p. 111–113; see also (Read, 1997).
[25](Shapiro, 1999), p. 56; see also (Shapiro, 1985), p. 715.
[26](Shapiro, 1985), p. 716.

there is an unavoidable relativism concerning the interpretation of mathematical theories, which is often referred to as '*Skolem's Paradox*'.[27] Skolem discusses the foundation of mathematical theories on first-order set theory and observed that, like any first-order theory, first-order set theory has a countable model. Shapiro explains:

> For example, one cannot claim that a given domain D is uncountable *simpliciter*, but only uncountable relative to a given model (containing D) of set theory. For any such D, one cannot rule out the possibility that D may be countable relative to a richer model, one that contains a function from the natural numbers onto D. Skolem held that this relativity also applies to the Dedekind notions of 'finite' and 'simply infinite system'.[28] Given the modern trend of regarding virtually all mathematical notions as set-theoretic, Skolem's conclusion would entail the relativity of just about everything.[29]

Shapiro's reply is essentially that it should be assumed that mathematicians know what they talk about.[30] Mathematicians should be taken to successfully refer to the (preformal) mathematical structure that they seem and believe themselves to be talking about. First-order languages, thus, do no justice to mathematical practice. The categorical second-order axiomatisations on the other hand, characterise the subject matter of mathematics up to isomorphism, and thus describe the mathematical structures in a way that is necessary, but also sufficient for mathematical

[27]See (Shapiro, 1991), chapter 7, and (Shapiro, 2001) for a detailed discussion of Skolem's Paradox.
[28]For the notion of *Dedekind infinity* see section 7.3.1 below.
[29](Shapiro, 1991), p. 183.
[30](Shapiro, 1985), p. 716; compare also (Shapiro, 1991), p. 119.

practice.[31] Shapiro concludes the description of this project in his first article on the topic like this:

> One of the purposes of logic is correct inference. Thus, if my major conclusions are correct, the underlying logic of many branches of mathematics is (at least) second-order: one cannot codify the correct inferences of a second-order language with a first-order logic.[32]

It is not required of mathematicians that they explicitly use second-order systems for their proofs. Shapiro insists that mathematical practice is largely informal. It is the logician of philosopher of mathematics who uses the second-order systems in order to make sense of the mathematicians' practice.

7.2.2 Embedding

The more general idea that mathematicians refer to the intended structures of classical mathematical theories like arithmetic, real and complex analysis, and perhaps set theory, gains significance for mathematical practice if one considers what Shapiro calls 'embedding'.[33]

It is common mathematical practice not to restrict oneself to the resources available in the mathematical theory one of whose theorems is meant to be proven. This is especially true if theorems about the natural numbers are to be found. Peano Arithmetic is a fairly weak system, as mathematical theories go, and proofs sometimes can be found more elegantly and more quickly if stronger theories, like set theory for example, are used.

[31] (Shapiro, 1999), p. 56.
[32] (Shapiro, 1985), p. 716.
[33] (Shapiro, 1985), pp. 728–729; (Shapiro, 1991), pp. 123–124; see also (Kreisel, 1967), p. 148.

Also, as Kreisel illustrates,

> very often the mathematical properties of a domain D become only graspable when one embeds D in a larger domain D'. Examples: (1) D integers, D' complex plane; use of analytic number theory. (2) D integers, D' p-adic numbers; use of p-adic analysis. (3) D surface of a sphere, D' 3-dimensional space; use of 3-dimensional geometry.[34]

Such proofs "from the outside"[35] are common in mathematics. We typically allow that a proof carried out in, say, real analysis, that is purportedly about the natural numbers, is true of them. This requires, however, that the mathematician can pick out a substructure in the structure of the reals, that corresponds to the natural numbers.

> The practice of embedding structures indicates a further area in which categoricity is important. The point is that in order to embed a structure D into a structure E, one must have a means of recognizing that a substructure of E is isomorphic to D. Otherwise, one cannot be certain that D really is a substructure of E. The formal analogue of this requirement is a categorical characterization of D.[36]

Without this guarantee, the claim goes, we could hardly make sense of mathematical practice. If the mathematician would not be certain to have proven her theorem about a structure that is isomorphic to the natural numbers, how could she be sure that what she proved is true of them? Only a categorical axiomatisation can make sense of this practice. For all we would know in the absence of categoricity, the

[34](Kreisel, 1967), p. 166.
[35](Shapiro, 1999), p. 47.
[36](Shapiro, 1985), p. 728.

mathematician could have proven something about a *non-standard model* of arithmetic. Again, no proof explicitly formalised in a second-order theory is demanded of the mathematician; it is the philosopher of mathematics who observes, according to Shapiro, that one cannot do justice to the mathematician's practice if a first-order formalisation is presupposed.

The second-order axiomatisation of arithmetic is categorical, and so all its models are isomorphic, and hence in all relevant respects similar to the natural numbers. Whatever can be shown to be true in any of these models, hence, must be true of the natural numbers. Thus, categorical axiomatisations do justice to such proofs "from the outside".

Gödel's incompleteness theorem shows that for any consistent axiom system of a mathematical theory at least as strong as arithmetic there are true sentences that are not provable in that system. If we embed a mathematical structure in a richer one and prove propositions about it in the stronger theory that is about this richer structure, because of the incompleteness result we cannot be sure that the truth proven about the embedded structure is also provable from the axioms characterising it.

There is in general no telling to what extent the truths of a theory outstrip the theorems, in the sense that, as Shapiro stresses,[37] it is usually not clear in advance how much mathematical resources are needed for a proof of a statement. This question is usually non-trivial even *after* the proof is carried out. A given proof shows what resources were in fact used, but it is not in general clear whether another proof could not establish the same result more economically.

To mention a now famous example, Andrew Wiles' celebrated proof of Fermat's

[37](Shapiro, 1985), p. 728; (Shapiro, 1991), p. 124; (Shapiro, 2005a), p. 777.

Last Theorem does not proceed by deriving the statement from the Peano-Dedekind axioms of arithmetic. Rather, Wiles proved another conjecture, concerning complex analysis, which entails Fermat's Last Theorem. So, Wiles' proof requires at least complex analysis, while Fermat's Last Theorem – that there are no natural numbers x, y, z (other than 0) such that $x^n + y^n = z^n$ for any natural number n greater than 2 – is about the natural numbers. It is still an open question whether the theorem can be derived from the Peano-Dedekind axioms. Nevertheless, Wiles' proof is taken to show something about the natural numbers, and presumably rightly so.

7.3 Expressive Power

As mentioned above, many important mathematical concepts are not expressible in pure first-order logic. Second-order logic, interpreted with standard semantics, however, has the required expressive resources, and so statements about infinite cardinalities, well-orderings, choice functions and other mathematical concepts can be expressed in purely second-order logical terminology. In this section I will concentrate on the expressibility of infinite cardinalities, as the philosophical discussion usually concerns examples that involve these. Both the claimed advantages of second-order logic with standard semantics, and the reasons for the dissatisfaction of its opponents with it, become particularly apparent here.

To be sure, the opponent of second-order axiomatisations can point out that while these concepts might not be expressible in pure first-order logic, they are so in first-order set theory, for example in Zermelo-Fraenkel set theory (ZF), or ZFC where the Axiom of Choice is required. Shapiro holds against this, however, that in this case Skolem strikes back: first-order ZF itself, like any first-order theory, is subject to the Löwenheim-Skolem theorems. That means that despite the fact that the vast iterative hierarchy is the intended model of ZF, ZF has a countable model. A first-order ZF sentence that asserts truly of a set R that it is uncountable, say, a set isomorphic to the reals, in an important sense only does so on the standard interpretation. A countable model of ZF will still make that sentence true, but it, of course, cannot assign an uncountable set as an interpretation to R. We end up with a (countable) non-standard model of the real numbers, and a sentence that is intended to express the uncountability of the reals that is made true by a countable model.[38]

The connections to the importance of categoricity, as described above, should be apparent. Models of the "wrong" cardinality are ruled out by categoricity. So, cardinality statements can be made determinately, if indeed the categoricity results can keep this promise. Before going into the criticisms, however, it seems advisable to have a look at the second-order formalisations that are the subject of the debate.

7.3.1 Cardinalities

To express in second-order logic that a mathematical concept, e.g. *natural number*, has an infinite extension the notion of Dedekind infinity can be used.[39] In the more common, set-theoretical formulation, it can be defined that a set α is Dedekind infinite if, and only if, there is an injection (one-to-one function) from α to a proper subset of it. To express in second-order logic with standard semantics a sentence 'INF(F)', stating that the extension of a predicate 'F' is infinite, all we have to do

[38](Shapiro, 1985), pp. 734–739; (Shapiro, 1991), pp. 110–116.

[39]To show that a Dedekind infinity is indeed infinite the Axiom of Choice is needed. As this is usually assumed in the model theory this does not pose a technical problem here; see (Shapiro, 1991), p. 130, n. 7. A philosophical discussion of the Axiom of Choice cannot be delivered here.

is find an open sentence that expresses the condition for Dedekind infinity. It can be done like this:[40]

$$\text{INF}(X):$$
$$\exists f[\forall x \forall y(fx = fy \supset x = y) \wedge \forall x(Xx \supset Xfx) \wedge \exists x(Xx \wedge \forall y(Xy \supset fy \neq x))]$$

'INF(X)' is satisfied by a model if, and only if, the class assigned to 'X' by the standard model theory is infinite. Now it is easy, of course, to characterise finitude using this:

$$\text{FIN}(X): \quad \neg\text{INF}(X)$$

Note that neither of the formulae contains any non-logical vocabulary. The influence of the assumption of the standard semantics is also particularly apparent here. 'FIN(X)' states that there is *no* such function. The second-order quantifiers have to be assumed to have *all* functions in their range. The standard semantics is considered guaranteeing this.

Using an individual constant for zero, '0', and a constant function letter for successor, 's', one can construct first-order formulae that are satisfiable only on infinite domains. The following is such a formula:

$$\forall x \forall y(sx \neq 0 \wedge (sx = sy \supset x = y))$$

So we can, in a sense, also express that a predicate φ has an infinite extension:

$$\forall x \forall y(sx \neq 0 \wedge (sx = sy \supset x = y)) \wedge \varphi(0) \wedge \forall x(\varphi(x) \supset \varphi(sx))$$

[40]The exposition below largely follows (Shapiro, 1991), pp. 100-105; see also (Garland, 1974).

These can thus be taken to express the infinity of the domain or of the extension of a predicate in a first-order language (with non-logical constants). This characterisation has the flaw, however, that the negations of the above formulae do *not* express finitude, as will be shown below.

It is well know that first-order logic with identity can express any specific finite cardinality. 'There are at most three things that are F', for instance, can be expressed as:

$$\exists x \exists y \exists z \forall w (Fw \equiv (w = x \vee w = y \vee w = z))$$

It cannot, however, be expressed in any first-order language that a predicate has a finite extension without specifying a finite upper bound on its size. This is a corollary of the compactness theorem:

Theorem: Let S be a set of sentences formalised in a first-order language. Let $\ulcorner\varphi(x)\urcorner$ be an arbitrary open sentence of that language, containing 'x' free. If, for each natural number n, there is a model of S in which the extension of φ has at least n members, then there is a model of S in which the extension of φ is infinite.

Proof: Let 'c_0', 'c_1', 'c_2', ... be individual constants that do not occur in φ or in any of the sentences that are member of S. Let S' be the set such that

$$S' =_{\text{df}} S \cup \{c_i \neq c_j \mid i < j\} \cup \{\varphi(c_i) \mid i \in \omega\}$$

If a subset T of S' has n members then T is satisfiable in any model of S in which the extension of φ has at least $2n$ elements – simply assign different elements of the extension of φ to the new constants that occur in T. T was chosen arbitrarily, so every subset of S' is satisfiable, and, by compactness, S' is satisfiable. But any model of S' is a model of S in which the extension of φ is infinite.[41] $\square$

As already mentioned, the second-order expressions of finitude and infinity are by far not the only mathematical concepts that can be defined in pure second-order logic. George Boolos, for example, has shown that even comparatively simple notions like 'is equinumerous with' or 'has an extension that is at least as large as the extension of' cannot be expressed in a first-order language, but require second-order resources.[42]

It is possible to express the equinumerousity of two predicates in second-order logic, because a bijection (a function that is one-to-one and onto) between the extensions of two predicates can be expressed:

EQUIN(X, Y):

$$\exists f(\forall x \forall y((Xx \wedge Xy \wedge fx = fy) \supset x = y) \wedge \forall x(Xx \supset Yfx) \wedge \forall x(Yx \supset \exists y(Xy \wedge x = fy)))$$

With the help of this we can also easily express *being countable* (that is, being either countably infinite, i.e. equinumerous with the natural numbers, or finite) in second-order logic. If 'X' has a countable extension, then every predicate 'Y', such that everything that is 'Y' is also 'X', has an extension that is either finite, or equinumerous with 'X'. Thus:

[41] Compare (Shapiro, 1991), pp. 101–102.
[42] (Boolos, 1981).

$$\text{COUNT}(X): \quad \forall Y(\forall x(Yx \supset Xx) \supset (\text{FIN}(Y) \vee \text{EQUIN}(X,Y))$$

To see that this expresses countability, suppose that 'X' was uncountable, say, 'is a real number'. There would be a predicate 'Y' that applied to countably many real numbers, say, 'is a real number expressible as a whole number'. Clearly, everything in the extension of 'Y' is also in the extension of 'X', but 'Y' is neither finite nor equinumerous with 'X'.

Is countably infinite, or *is of cardinality* $\aleph_0$, i.e. the cardinality of the natural numbers, is thus easily expressed as:

$$\text{ALEPH-0}(X): \quad \text{COUNT}(X) \wedge \text{INF}(X)$$

Having thus characterised the smallest infinite cardinality, it is straightforward to express the next smallest, $\aleph_1$, and the next smallest after that, $\aleph_2$, and so on, using the same idea:

$$\begin{aligned} \text{ALEPH-1}(X): \quad & \text{INF}(X) \wedge \neg\text{ALEPH-0}(X) \wedge \forall Y(\forall x[Yx \supset Xx) \\ & \supset (\text{FIN}(Y) \vee \text{ALEPH-0}(Y) \vee \text{EQUIN}(X,Y)] \end{aligned}$$

$$\begin{aligned} \text{ALEPH-2}(X): \quad & \text{INF}(X) \wedge \neg\text{ALEPH-0}(X) \wedge \neg\text{ALEPH-1}(X) \\ & \wedge \forall Y(\forall x[Yx \supset Xx) \supset (\text{FIN}(Y) \vee \text{ALEPH-0}(Y) \\ & \vee \text{ALEPH-1}(X) \vee \text{EQUIN}(X,Y)] \end{aligned}$$

This process can obviously be continued, to characterise any cardinality $\aleph_n$, for all finite n. Indeed, it does not stop there: *being of cardinality* $\aleph_\omega$ (and beyond) can be expressed, too,[43] but I will not go into the details here. Instead of chasing higher up in the hierarchy of cardinalities, let us turn to the most prominent example in

[43]Compare (Shapiro, 1991), pp. 104-105.

the more recent discussion of second-order logic: the continuum problem.

7.3.2 The Continuum Hypothesis

One of the most notorious problems in set theory is that of the continuum hypothesis. It is known that the continuum, i.e. the cardinality of the real numbers, is the cardinality of the powerset of the natural numbers, i.e. of a countable infinite set. Recall that the powerset of a set α is the set that contains all and only the subsets of α. Generally, if a set has the cardinality κ, its powerset has the cardinality 2^κ. We say that countable infinite sets are of cardinality $\aleph_0$, and so the continuum has the cardinality $2^{\aleph_0}$. The question thus arises: is the continuum the *smallest* infinite cardinality that is larger than $\aleph_0$, or are there uncountable sets that are smaller than the continuum? This question is known as the *continuum problem.* Georg Cantor conjectured that the size of the continuum was indeed the next cardinality after $\aleph_0$: this is his *continuum hypothesis.*

Formally, it can be expressed as:

$$\aleph_1 = 2^{\aleph_0}$$

Cantor's formulation of the continuum hypothesis is that every subset of the set of the real numbers is either countable or can be bijected with the reals. Kurt Gödel showed in 1938 that the continuum hypothesis is consistent with first-order ZFC.[44] Paul Cohen managed to prove in 1963 that its negation is consistent with first-order ZFC too.[45] It was thus established that the continuum hypothesis is independent of

[44](Gödel, 1938)

[45](Cohen, 1963); (Cohen, 1964).

first-order ZFC. Indeed, Chuaqui has shown that it is also independent of second-order ZFC.[46]

The continuum hypothesis can be generalised to the thesis that for every cardinality the smallest larger one is reached by applying the powerset operation to it. Formally:

$$\aleph_{\alpha+1} \; = \; 2^{\aleph_\alpha}, \text{ for any ordinal } \alpha$$

This is the *generalised continuum hypothesis*. Note that this conjecture is not restricted to finite ordinals. It is meant to apply *all* the way up. The generalised continuum hypothesis is independent of ZFC, too, as is also shown by Gödel and Cohen.

We have seen above that the different cardinalities $\aleph_n$, for $n \in \omega$ can be expressed in pure second-order logic; and it was said that indeed all cardinalities $\aleph_\alpha$, for any ordinal α, can so be expressed. We can, indeed, also express *being of the size of the continuum*.[47] In order to do so, notice that for a continuum-sized set X there is a countably infinite set S and a binary relation R with X as its domain and S as its range, such that (i) for each Y that is a subset of S there is an x in X such that Y is $\{y \mid Rxy\}$, and (ii) for every x and y in X, if $\{z \mid Rxz\} = \{z \mid Ryz\}$ then $x = y$. R is hence a bijection between X and the powerset of S. The following second-order formula expresses *being continuum-sized* in accordance with these considerations:

CONTINUUM(X):

$\exists S \exists R[\text{ALEPH-0}(S) \wedge \forall x \forall y(Rxy \supset (Xx \wedge Sy))$

$\qquad \wedge \forall Y(\forall y(Yy \supset Sy) \supset \exists x \forall y(Rxy \equiv Yy))$

[46] (Chuaqui, 1972); see also (Weston, 1977).

[47] For the following formalisation see (Shapiro, 1991), pp. 105–106.

$$\wedge \forall x \forall y([Xx \wedge Xy \wedge \forall z(Rxz \equiv Ryz)] \supset x = y)]$$

This definition in effect says that being continuum-sized is being bijectable with the powerset of a countably infinite set. So now we can express the continuum hypothesis in pure second-order logic. Call this second-order sentence 'C'.

$$(C) \qquad \forall X[\text{ALEPH-1}(X) \equiv \text{CONTINUUM}(X)]$$

C not only expresses the continuum hypothesis, but is also linked with it in a much stronger way. In the standard model theory, the range of the second-order variables is the powerset of the domain. If the domain is countably infinite, the range of the second-order variables is continuum-sized. What the cardinality of this is, depends, thus, on what way the continuum hypothesis is decided. Moreover, whenever the domain is infinite (countably infinite or larger), it will depend on the continuum hypothesis whether any given predicate whose extension is continuum-sized has the cardinality $\aleph_1$ or not. Should the continuum hypothesis turn out to be true, however, any extension of a predicate that is continuum-sized is of size $\aleph_1$ *in every model* – there just cannot be any model in which a predicate is assigned a continuum-sized set that is not $\aleph_1$, as no such sets exist if the continuum hypothesis is true. Hence, C will be true in all models, i.e. a validity of the standard semantics of second-order logic. To put it in a different way: the standard semantics of second-order logic declares C to be a *logical truth* if, and only if, the continuum hypothesis is true. This seems hard to swallow to many.

Before we turn to the criticism connected to the case of the continuum problem, however, it is worth adding some more details surrounding the issue. One point is that it is *not* the case that '$\neg C$' is a true in all models if the continuum hypothesis is false. '$\neg C$' is trivially true in every countable model, i.e. it is true irrespective of the truth or falsity of the continuum hypothesis. There is another sentence, call it 'D', however, that is a validity of the standard semantics of second-order logic if, and only if, the continuum hypothesis is false:

$$(D) \qquad \forall X[\text{ALEPH-1}(X) \supset \neg\text{CONTINUUM}(X)]$$

Neither C, nor D, nor their negations, are theorems of the deductive system of second-order logic.[48] Thus, we have here an example for the incompleteness of second-order logic with respect to the standard semantics: one of C and D is a logical truth according to the standard semantics that cannot be proven in any standard deductive system of second-order logic.

It does not stop there. There is a similar pair of sentences, one of which is declared a logical truth if, and only if, the generalised continuum hypothesis is true. Another second-order sentence – and this also is a sentence that contains only logical vocabulary – asserts the existence of inaccessible cardinals, and is declared a logical truth by the standard semantics, if, and only if, there are inaccessible cardinals.

The examples mentioned so far all use the expressive resources of second-order logic to express these statements directly. There is a trick, however, whereby in general *all* truths of set theory[49] can be turned into second-order sentences that in a way correspond to them. The recipe is the following: take the second-order axioms of ZF (or ZFC, if you will) in their canonical formulation with '$\in$' as only non-logical constant; take their conjunction – let us call it Z. Let P be the true ZF sentence in question; form the conditional '$Z \supset P$'. Replace every occurrence of '$\in$' by a

[48](Shapiro, 1991), p. 105.

[49]At least those not depending on inaccessible cardinals; see footnote 50 below.

two-place predicate variable in this conditional and bind that variable with a prenex universal quantifer. The resulting generalised sentence will in all cases be true in all models, as follows from the quasi-categoricity[50] of second-order ZF.

This method works for second-order arithmetic and second-order real analysis just as well as for set theory. All of the truths will, when conditionalised to the conjunction of their axioms and generalised, turn into validities of the standard semantics of second-order logic. To borrow Ignacio Jané's metaphorical description of the situation, second-order logic, in a sense, "has all the solutions to the problems of ordinary mathematics, but it keeps them to itself".[51] None of the mathematical truths that are unprovable in their respective theories will be provable in their conditionalised and universally generalised form in second-order logic, on pain of contradiction (provided only that the deductive system of second-order logic is consistent). If there were a sentence amongst them that is a theorem of second-order logic, one could within the second-order theory instantiate this theorem, and discharge the antecedent with the axioms. This would prove the respective statement in the second-order theory, contradicting the assumption that this is not possible. For the case of arithmetic the mentioned technique will play an interesting role later on, in chapter 8. Let us now turn to a critical discussion of the mentioned results.

7.4 Substantial Content

The interesting question for the status of second-order logic as proper logic must be its delineation with respect to mathematics. Shapiro, as mentioned in the beginning

[50]Quasi-categoricity means intuitively that all models are isomorphic up to the lowest inaccessible rank, see chapter 2, section 5.3, above.
[51](Jané, 1993), p. 80.

of this chapter, rejects this question. He holds in an extreme Quinean[52] fashion that there is no sharp boundary between logic and mathematics, and that in the case of the logic of mathematics, it is particularly unsurprising that we find it difficult to tell which side of the divide we are on.

Not everyone agrees with Shapiro on this, however. As is discussed below, some of Shapiro's critics find it especially in the realm of mathematics *crucial* to draw this distinction. In the next chapter, chapter 8, I will make a case that there is an interesting and important line to be drawn, and that this line can be drawn according to fairly well-respected criteria. Much of the criticism that is discussed in the following will provide material for the next chapter in which the model-theoretic approach to logical consequence is rejected in favour of an account that characterises logical consequence with the aid of inference rules – what I call the *Deductivist Conception*.

Much of the criticism, however, has force even if one is not interested in some notion of *proper* logic. One line of criticism challenges the alleged advantages of second- over first-order logic, especially the categoricity results. A good bit of the criticism turns on the substantial mathematical content that second-order logic carries. The basic idea would be that, contrary to Shapiro's insistence, the mathematical presuppositions of the standard model theory are problematic and that this mathematical content gets in the way of the envisioned benefits in the use of second-order logic. That this should be so might be more obvious if one is interested in the notion of a proper logic. Shapiro evades this criticism by rejecting a sharp distinction between logic and mathematics. Some of Shapiro's critics, however, argue that in axioma-

[52]Quine himself sometimes, especially in his discussion of second-order logic, seems to consider the border between proper logic and mathematics sacrosanct, while the entire rest of the "web of believe" is seemless according to him.

tising mathematical theories second-order logic fares no better than first-order set theory – or even worse. An argument to that effect, as discussed a little farther below, does not depend on a distinction between logic proper and mathematics.

Let us start out with an objection to second-order logic that does depend on a notion of proper logic. In an extreme form, aversions against mathematical content in a logic might even take its ability to express certain mathematical concepts as disqualifying a system from the status of proper logic. Jan Woleński puts this idea forward in various of his papers in which he discusses first-order logic. Surprisingly, he claims that "any theory, which distinguishes cardinalities is not a logic."[53] This statement, if true, would rule out first-order logic with identity which is exactly the logic that Woleński favours as "the one true" logic.[54] Any finite cardinality can of course be expressed in first-order logic, as already mentioned. There are at least three cats, for example, can be expressed as

$$\exists x \exists y \exists z (Cx \wedge Cy \wedge Cz \wedge x \neq y \wedge y \neq z \wedge x \neq z)$$

with 'C' standing for 'is a cat'. There are exactly two cats can be formalised in

[53](Woleński, 2004), p. 388.

[54]See, for example, his (Woleński, 1999) and (Woleński, 2004). Woleński's main argument for first-order logic seems to be intended to go along the lines of the arguments from completeness that were discussed, and found flawed, in chapter 6. Woleński presents a meta-theoretical axiomatisation of what he takes to be logical consequence. Interestingly, it only accounts for truth-functions and first-order quantifiers. He then shows that the standard semantics of first-order logic is adequate for his axioms, and observes that the deductive system of first-order logic is complete with respect its standard semantics and, by transitivity, to his axiomatisation. He does not present an argument that his axioms *exhaust* logical consequence (the presystematic notion) – and *vis-à-vis* the question whether they at least *track* logical consequence he essentially restricts his remarks to the claim that his axioms are obvious. One wonders what he takes his equivalence proofs to show about logical consequence, and how it follow from them that the "only true logic" is standard classical first-order logic.

first-order logic as:

$$\exists x \exists y (Cx \wedge Cy \wedge x \neq y \wedge \forall y [Cz \equiv (x = z \vee y = z)])$$

To interpret Woleński charitably, his concern might be about *infinite* cardinalities – that a logic should not be able to distinguish between them. One still wonders why, however. The reason cannot be that infinite cardinalities belong in the domain of mathematics and not of logic, for the same could be said to be true of finite cardinalities. While each finite cardinality can be expressed in first-order logic, *being finite* is not expressible with the resources of first-order logic. As shown in section 7.3.1 above this follows from the compactness theorem.

Moreover, claims about the cardinality of the domain can be expressed in first-order logic for the finite case. That the domain has a cardinality no larger than 3, for example, is expressed as:

$$\exists x \exists y \exists z \forall w (x = w \vee y = w \vee z = w)$$

This sentence is, of course, not provable,[55] but neither are the cardinality statements that can be made in second-order logic. As Fraser MacBride puts it,

> it is one thing to be able to express a mathematical claim employing these notions in a second-order language. It is quite another thing to be logically obliged to endorse such claims. Until it is shown that second-order expressible mathematical claims fall amongst the class of second-order logical truths, it remains to be established that the employment of

[55]One cardinality statement, however, is provable in standard first-order logic, viz. that the domain in non-empty: '$\exists x(x = x)$'.

second-order inference patterns draws upon distinctively mathematical expertise.[56]

The last sentence, of course, alludes to the case of the continuum problem that will occupy us again a little further down.

One might also put forward an argument against Woleński's claim along the lines of some of Boolos' arguments. As already mentioned in chapter 4, Boolos advances – especially in (Boolos, 1975) – some arguments to the effect that second-order logic is a natural extension of first-order logic, in the sense that some of the jobs we want logic to do, can only in some special cases be fulfilled by first-order logic. In order to be able to do these things[57] generally, we need second-order logic. Providing the expressive resources for making cardinality statements might be one such feature: first-order logic with identity can only express finite cardinalities, while second-order logic (with standard semantics) can express infinite cardinalities (and 'being finite'), too.

As stressed above, Shapiro argues in a more general way that first-order logic just is too weak to express many of the concepts that are important for mathematics; cardinality concepts are a prominent case. A logic that cannot distinguish infinite cardinalities, is of little use in an area where one wants to reason about these: mathematics. This point was already elaborated above.

Even Shapiro admits, however, that the case of the second-order sentence C (see section 7.3.2) that is a validity of the standard semantics if, and only if, the continuum hypothesis is true might be seen as problematic. While the question to him is of little importance as he rejects the sharp distinction between logic and

[56](MacBride, 2003), p. 139.

[57]One of Boolos' examples was showing that a set of sentences is inconsistent; see (Boolos, 1975), p. 49, and also my chapter 4, section 1.

mathematics, he agrees in his most recent paper on higher-order logic that "[i]t is perhaps counterintuitive to hold that such principles are "logical"."[58] It seems to many, that the continuum hypothesis is a mathematical statement if anything is.[59] Additionally, we have seen in section 7.3.2 how, in a sense, all set theoretical truths (barring inaccessibles) have their corresponding sentences amongst the second-order validities.

While Shapiro takes this to provide evidence for his claim that there is no sharp boundary between logic and mathematics,[60] Ignacio Jané suggested – first in (Jané, 1993) – that, however this may be, there is still trouble on the horizon if we proceed as Shapiro suggests. To repeat the quotation already given above, Jané is on the one hand concerned that

> [i]n a certain, perhaps metaphorical sense, [second-order logic] has all the solutions to the problems of ordinary mathematics, but it keeps them to

[58](Shapiro, 2005a), p. 772.

[59]A logician who is well acquainted with second-order logic once mentioned to me that it is not so far-fetched to consider C a *logical* truth if one looks at the actual, written out, second-order formula and clears ones mind of the set theoretical terminology that it is usually employed to talk about it. I have to confess that my intuitions about logical truths run out long before complicated cases like this. For the sake of the reader who wants to try her or his luck, here is C written out in fully explicit second-order form:

$\forall X[(\exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Xx \supset Xfx) \wedge \exists x(Xx \wedge \forall y(Xy \supset fy \neq x))] \wedge \neg(\forall Y(\forall x(Yx \supset Xx) \supset (\neg\exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Yx \supset Yfx) \wedge \exists x(Yx \wedge \forall y(Yy \supset fy \neq x))] \vee \exists f[\forall x\forall y((Xx \wedge Xy \wedge fx = fy) \supset x = y) \wedge \forall x(Xx \supset Yfx) \wedge \forall x(Yx \supset \exists y(Xy \wedge x = fy))]) \wedge \exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Xx \supset Xfx) \wedge \exists x(Xx \wedge \forall y(Xy \supset fy \neq x))]) \wedge \forall Y(\forall x[Yx \supset Xx) \supset (\neg\exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Yx \supset Yfx) \wedge \exists x(Yx \wedge \forall y(Yy \supset fy \neq x))] \vee (\forall Y(\forall x(Yx \supset Yx) \supset (\neg\exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Yx \supset Yfx) \wedge \exists x(Yx \wedge \forall y(Yy \supset fy \neq x))] \vee \exists f[\forall x\forall y((Yx \wedge Yy \wedge fx = fy) \supset x = y) \wedge \forall x(Yx \supset Yfx) \wedge \forall x(Yx \supset \exists y(Yy \wedge x = fy))]) \wedge \exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Yx \supset Yfx) \wedge \exists x(Yx \wedge \forall y(Yy \supset fy \neq x))]) \vee \exists f[\forall x\forall y((Xx \wedge Xy \wedge fx = fy) \supset x = y) \wedge \forall x(Xx \supset Yfx) \wedge \forall x(Yx \supset \exists y(Xy \wedge x = fy))]]) \equiv \exists S\exists R[(\forall Y(\forall x(Yx \supset Sx) \supset (\neg\exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x(Yx \supset Yfx) \wedge \exists x(Yx \wedge \forall y(Yy \supset fy \neq x))] \vee \exists f[\forall x\forall y((Sx \wedge Sy \wedge fx = fy) \supset x = y) \wedge \forall x(Sx \supset Yfx) \wedge \forall x(Yx \supset \exists y(Sy \wedge x = fy))]) \wedge \exists f[\forall x\forall y(fx = fy \supset x = y) \wedge \forall x((x \supset (fx) \wedge \exists x((x \wedge \forall y((y \supset fy \neq x))]S)) \wedge \forall x\forall y(Rxy \supset (Xx \wedge Sy)) \wedge \forall Y(\forall y(Yy \supset Sy) \supset \exists x\forall y(Rxy \equiv Yy)) \wedge \forall x\forall y([Xx \wedge Xy \wedge \forall z(Rxz \equiv Ryz)] \supset x = y)]]$

[60](Shapiro, 1985), p. 716; (Shapiro, 1991), p. 48.

> itself [...].[61]

His complaint does not stop with the observation that we are put into an awkward epistemic situation, however. He also emphasises that in the case of axiomatised theories it is important that any assumption made about the objects that the theory is intended to be about has to be explicit in the axioms. The logic that the axiomatisation is carried out in must not "add any mathematical presuppositions through the back door".[62] In order to be absolutely explicit about the subject matter that an axiomatic theory is about, nothing substantial should be hidden in the consequence relation.

> The point is not that these presuppositions may be false, but that they are substantial. (Should we need them, we could postulate them besides the proper axioms of the theory.)[63]

Both Jané and Shapiro agree that in order to find out something about a given subject matter, the axioms are investigated. While Shapiro contends that these axioms must be categorical, however, Jané is concerned that, if the standard model theory of second-order logic is used, the possibility for such investigations is blocked for the realm of set theory.

The case of the continuum hypothesis serves as an example. If we add the continuum hypothesis (or its negation) as an assumption to the first-order axioms of ZF, we can investigate how the set theoretical universe behaves by studying the axioms. In the case of second-order ZF, however, and if the semantic account is assumed, the addition of the continuum hypothesis will be superfluous if it is true, as

[61] (Jané, 1993), p. 80

[62] (Jané, 1993), p. 67; compare also (Jané, 2005), p. 787.

[63] (Jané, 1993), p. 67.

then the corresponding sentence of second-order logic is a validity. If the continuum hypothesis is false, however, we end up in inconsistency. We will not be able to tell either way, however. No sound deductive system of second-order logic can be complete with respect to the standard semantics, and the continuum hypothesis (or its negation) is one of the truths that the consequence relation keeps to itself.[64]

The truth of the continuum hypothesis depends on how many subsets of an infinite set there are, or, in other words, on the behaviour of the powerset operation. Jané argues that second-order logic

> makes use of the power set operation, and an essential use at that, since on it rests the determinacy of the consequence relation. [...] [A]ny divergence in accounting for the content of the powerset is bound to disturb [...] the consequence relation [of the standard semantics]; in other words, different accounts of their content will give rise to different consequence relations.[65]

It is in this way that the consequence relation of the standard semantics of second-order logic carries set-theoretical content. The determinacy of the powerset operation has to be assumed in order for the result mentioned in section 2 above – categoricity and expressive strength – to deliver the benefits that Shapiro claims. The continuum hypothesis, for example, depends precisely on the behaviour of the powerset operation. It is exactly the question what subsets of a given set (or domain) there are. The determinate outcome of this in the consequence relation in the standard semantics is a result of the *assumption* that the powerset is determinate. The subsets of the domain are the range of the second-order variables, and impor-

[64](Jané, 2005), pp. 791–792.
[65](Jané, 2005), p. 800.

tantly, it has to be *all* subsets. What *all the subsets* of an infinite domain amount to, however, *is* the continuum problem.

We are thus thrown back to Skolem's Paradox. Just like the case described above for "categorical" characterisations given in first-order set theory, we can still hold in a sense that an infinite structure characterised in a second-order axiomatisation has essentially only one model in that all of its models are isomorphic. This, however, is relative to how the second-order quantifiers are determined, i.e. what all the subsets are that the second-order variables range over. All models are isomorphic to each other on each of the ways to determine the subsets, but they all vary together with the different determinations.[66]

Arguments very similar to Jané's criticism have been put forward by other authors, too; (Weston, 1976), for example, makes essentially the same point. Jody Azzouni has a related complaint. He argues that second-order axiomatisations rule out non-standard models merely by fiat. It has to be assumed that one can refer to the standard model in meta-theory of second-order logic, in order to be able to explain the successful communication between mathematicians, and how they manage to refer to the intended structure. Azzouni concludes that

> second-order logic is *treacherous*: its sirenlike notation can lull philosophers into an inadequate appreciation of how it gains its expressive power.[67]

Jouko Väänänen makes a similar point, and in addition remarks that there is no telling by just looking at the second-order language whether a standard, or a Henkin

[66](Jané, 2005), pp. 803–804.
[67](Azzouni, 1994), p. 18.

semantics is assumed.[68]

At least Väänänen's observation misses the point. That the syntax of a second-order language, not even together with the deductive system, does not determine the semantics is hardly surprising – recall the range of options for different semantics for the same deductive system mentioned in section 2 of chapter 6 of this thesis. The all-important feature of Shapiro's conception is that logic is conceived of model-theoretically. We decide in advance that the tool we want to use in our endeavours is the standard semantics. The mathematical notions that we are interested in are then captured using this tool.

Moreover, Väänänen's second complaint, that model theory could not be formulated before the development of set theory,[69] does not hit Shapiro. Firstly, the Henkin semantics that Väänänen favours is not available without set theory either, and the same goes, incidentally, for the model theory of first-order logic. Secondly, Shapiro has no qualms admitting the mathematical character of the standard semantics of second-order logic. Shapiro's claim is not that it is mathematically innocent; quite to the contrary. His claim is that the standard semantics is the only way to make sense of mathematical practice, as this practice presupposes that mathematicians refer to intended interpretations and are able to pick out infinite structures up to isomorphism. Categoricity guarantees, or, rather, is meant to guarantee, determinacy.

This is the crucial point, however. Consider, for example, Shapiro's rebuttal of Azzouni's claims:

> I do not think that I (or Boolos, Corcoran, Hellman, etc.) are lulled in

[68](Väänänen, 2001), pp. 505, 506.
[69](Väänänen, 2001), p. 505.

> this way. [(Shapiro, 1991)] should not be read as providing a solution to the problem of reference. [...]
>
> The substantial claim of [(Shapiro, 1991)] [...] is that a logic with substantial presuppositions has an important role in foundational studies, but this role is not that of justifying mathematical practice or of explaining how successful reference and communication works. [...]
>
> Azzouni agrees that I [...] "want to show that mathematical practice is best understood in second-order terms...". I do not see how I was concerned with the "problem of referential access" at all.[70]

This much can probably be granted. The driving idea, however, that I take to be behind Azzouni's attack and that is spelled out properly by Jané and Weston – the question of the determinacy of the interpretation of second-order quantifiers in the standard semantics – is still to be answered. Otherwise the claim that second-order axiomatisations do justice to the mathematical practice as described by Shapiro (which is the main motivation for his approach) cannot be upheld.

Hartry Field argues that the indeterminacy worries that arise concerning the powerset operation arise in the the same way concerning the second-order quantifiers:

> The basic point is one that was well made many years ago in (Weston, 1976): any argument that claims about the size of the continuum don't have determinate truth value will carry over to an argument that 'all subsets' is indeterminate, and hence that it is indeterminate what counts as a 'full' interpretation. [...] So even though *relative to any specific conception of 'fullness'* the full interpretation of second-order set theory

[70](Shapiro, 1999), p. 58; compare also (Shapiro, 2005a), p. 775.

> is quasi-unique, that doesn't do anything to alleviate the basic indeterminacy.[71]

Field contends that categoricity proofs no more shows that the structures in question are characterised determinately, than the proof in classical first-order logic that 'Fx' is equivalent to '$\neg\neg Fx$' shows that the predicate 'F' has a determinate extension.[72] At best, it shows that the two share their indeterminacy. The extension of '$\neg\neg F$' co-varies with that of 'F'; they are equivalent, but also equally (if at all) indeterminate.

The most we can hope for is that categoricity shows that any two models of a theory are "co-indeterminate" in the sense described above. This seems to be determinacy enough for Shapiro. He submits that scepticism or relativism about second-order quantification amounts to scepticism or relativism about the determinacy of the (pre-formal) mathematical notions.[73] While Field holds that second-order quantification is indeterminate because the set-theoretical notions are, Shapiro assumes that the second-order quantifiers are determinate, because he accepts that the classical mathematical discourse is in good standing.

It also seems that it does not disturb him that he has to presuppose the determinacy of the second-order quantifiers in order to uphold the claim that these categorical axiomatisations of mathematical theories make sense of the mathematical practice (as described by him). He cites Alonzo Church as an ally:

> It is true that the non-effective notion of [second-order] consequence, as we have introduced it [...] presupposes a certain absolute notion of *all* propositional functions of individuals. But this is presupposed also in

[71](Field, 2001), p. 352; Field's emphasis.
[72](Field, 2001), p. 357.
[73](Shapiro, 1991), p. 207.

> classical mathematics, especially classical analysis.[74]

Shapiro stresses that the important word is "presupposes". He holds that

> [t]he thesis that we understand second-order languages with standard semantics is of-a-piece with the claim that we understand ordinary mathematical discourse. It is no less – and no more – problematic.[75]

Shapiro contends that asking for the determinacy of second-order quantification beyond what is given in the model theory leads into the following infinite regress. If the challenge, that the informal presupposition of the determinacy of the second-order quantifiers on the standard model-theoretic interpretation, has to be rigorously formalised, is allowed, the formal characterisation of the second-order quantifiers has to be given in a meta-theory. If the meta-theory is first-order set theory, Skolem's Paradox applies straight away. If the meta-theory is given in second-order set theory, the challenger can ask for the meaning of the second-order quantifiers in the meta-theory. If the informal presupposition of determinacy is not enough here either, a formal characterisation of the meta-theoretical quantifiers had to be given in a meta-meta-theory. And so on.[76]

Since such a regress must be stopped, Shapiro rejects the question. The informal characterisation of the second-order quantifiers as determinate is on a par with the assumption that the informal mathematical discourse is determinate. All three, Kreisel,[77] Corcoran,[78] and Shapiro,[79] state that the standard of mathematical discourse is some kind of "informal rigour" (this term is coined by (Kreisel, 1967)).

[74](Church, 1956), footnote on p. 326; Church's emphasis.
[75](Shapiro, 2005a), p. 775.
[76](Shapiro, 1991), section 8.1.
[77](Kreisel, 1967).
[78](Corcoran, 1973).
[79](Shapiro, 1985), (Shapiro, 1991), esp. chapter 5.

Shapiro is explicit that the formalisation in a second-order language is not meant to serve as a justification or explanation of this.[80]

If the above mentioned regress is to stop at an informal level, however, one wonders what was achieved. The informal mathematical practice is presupposed to be in good standing and in particular determinate, and the second-order axiomatisations of mathematical theories is not meant to *add* anything to this. To the contrary, it is presupposed that the second-order quantifiers are determinate, because otherwise they would not do justice to the mathematical discourse. Shapiro's argument in the beginning, however, was that second-order axiomatisations do justice to the mathematical practice because they deliver categorical axiomatisations. It seems we have come full circle. If the belief that categoricity shows determinacy ultimately *presupposes* the determinacy of both informal mathematical practice and the informal characterisation of the second-order quantifiers, why not leave matters at the level of informal mathematical practice in the first place?

7.5 Conclusion

The expressive power and possibility of categorical axiomatisations of infinte structures might be a motivation to use second-order logic with standard semantics, but, as Jané observes, it does not show that it is logic.[81] Indeed, as was shown above, heavy mathematical content is carried by the consequence relation of the standard semantics. This leads Shapiro to reject the sharp distinction between logic and mathematics; he submits that the "study of correct inference, like almost any other

[80](Shapiro, 1999), p. 58.
[81](Jané, 2005), p. 789.

science, involves some mathematics and some mathematical presupposition."[82]

The arguments presented towards the end of the last section suggest that the claimed importance of standard semantics of second-order logic for doing justice to mathematical practice can only made if the categoricity proofs indeed prove the determinacy – as opposed merely to co-indeterminacy – that is really presupposed for (informal) mathematical discourse. As turns out, this claim to prove determinacy hangs on the determinacy of the second-order quantifiers on the standard model-theoretic interpretation. So it appears that this determinacy also has to be merely presupposed.

I therefore submit that the reasons that Shapiro offers to adopt the model-theoretic interpretation of second-order logic evaporate, and with them his reason to accept mathematical content in the logical consequence relation and his reason to reject the sharp distinction between logic and mathematics.

In the next chapter, chapter 8, I argue for a notion of logic, characterised deductively, that licences second-order quantification as properly logical. With it will go a rejection of the standard model theory for second-order logic for almost all purposes that concern logic proper.

[82](Shapiro, 1985), p. 716.

Chapter 8

The Deductivist Conception

Semantical passages will be distinguished from others by being printed in smaller type, the small type serving as a warning that the material is not part of the formal logistic development and must not be used as such.
(Church, 1956, p. 68)

8.1 Introduction

The aim of this chapter is to introduce the Deductivist Conception of Logic, and argue that according to it, second-order logic should indeed be counted as logic in a proper sense. The aim of the Deductivist approach is to characterise the pre-systematic notion of logical consequence by the means of deductive rules of inference. There are a number of objections in the literature against syntactic characterisations of logical consequence, some of which, as I show in section 8.3, do not apply to the Deductivist approach. Two major objections, however, apply in a particularly pressing way, and these will be discussed at the end of the chapter. These objections will not be resolved, but a range of options for their resolution is outlined and discussed.

8.2 Themes from Frege

Gottlob Frege is in many respects the father of modern logic. To borrow Michael Dummett's words:

> The publication of the *Begriffsschrift* marked, as Quine says,[1] the beginning of modern logic. [...] It is the first formulation of the functional calculus of second order. [...] In it the modern notation of quantifiers and variables appears for the first time, as also for the first time a modern treatment of sentential operators (quite different from Boole's). Negation, implication, the universal quantifier and the sign of identity are taken as primitive: in virtue of the device of quantification, Frege is able for the first time in the history of logic to give an adequate account of the logic of statements involving multiple generality, and to introduce variables for relations and functions. [...][2]

And the catalogue of achievements goes on. The *Begriffsschrift* (Frege, 1879) also contains a definition of the *ancestral*,[3] as already mentioned in my chapter 4 above, an extremely powerful notion with the help of which Frege was able to define the predecessor relation (his version of the successor function that is common today) for

[1]Dummett alludes to (Quine, 1960), p. 163.
[2](Dummett, 1981), pp. xxxv-xxxvi.
[3](Frege, 1879), §26.

arithmetic in his *Grundgesetze der Arithmetik* [*Basic Laws of Arithmetic*] (Frege, 1893) – a crucial step in his foundation of number theory.

Reason enough to conjecture what Frege might have thought proper logic was, and thereby to find an inspiration to develop a Deductivist account, that has already been mentioned in the preceding chapters. It would have been tempting to call the Deducticist approach *Fregean*, except that Frege held many views about logic, not all of which are compatible with the account I develop below. In a posthumously published fragment that was probably meant to be part of an introductory logic textbook[4], he spends over 20 pages developing his ontology of thoughts and arguing that it is part of logic.[5] As already apparent from chapter 5 above, the Deductivist account is designed to be neutral on ontological questions. A second reason not to call the account *Fregean* is that nothing really hangs on the question whether I get Frege's intentions right (although I think I do, at least partially). The Deductivist account is not intended as a faithful version of Frege's view of logic. Frege's insight will merely be used as a starting point.

The *Begriffsschrift* is not only the name of Frege's book, but also the name of the logic that Frege develops in this book. In the preface, he writes that the purpose of this logic is[6]

> to test in the most reliable way the proper connection of a chain of inference and to reveal every presupposition that wants to slip in unnoticed, so that the latter can be examined for its origin.[7]

[4](Kambartel, 1982), p. XX.

[5](Frege, 1897).

[6]In this section I use my own translations of the quotations from Frege I give. The bibliography at the end of this thesis contains references to the published standard translations of all of Frege's writing that I use.

[7]"[...] die Bündigkeit einer Schlusskette auf die sicherste Weise zu prüfen und jede Vorraus-

In a later publication, *On the Scientific Justification of the Begriffsschrift*, Frege affirms that the reliablity he demands, cannot achieved when arguments are presented in ordinary language:

> The forms in which inference is expressed are so varied, so loose and flexible, that presuppositions can easily sneak through unnoticed which then are overlooked when the necessary conditions for the conclusion are listed.[8]

While ordinary language, Frege states, is a good starting point to develop an understanding of logical consequence,[9] it is insufficient for serious justificatory enterprises.

> [Ordinary] language in this respect can be compared to the hand, which despite its adaptability to the most diverse tasks, is still inadequate. We build for ourselves artificial hands, tools for particular purposes, which work with more precision than the hand can provide. [...] Ordinary language too is inadequate in this way. We require a totality of symbols from which every ambiguity is banned, which has a strict logical form from which the content cannot slip away.[10]

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setzung, die sich unbemerkt einschleichen will, anzuzeigen, damit letztere auf ihren Ursprung untersucht werden könne." (Frege, 1879), p. IV; see footnote 6.

[8] "Die Formen, in denen das Folgern ausgedrückt wird, sind so vielfältige, so lose und dehnbare, daßsich leicht Voraussetzungen unbemerkt durchschleichen, die dann bei der Aufzählung der nothwendingen Bedingungen für die Giltigkeit [sic] des Schlußsatzes übergangen werden." (Frege, 1882b), p. 50; see footnote 6 above.

[9] Frege is particulatly clear about this in (Frege, 1891).

[10] "Die Sprache kann in dieser Hinsicht mit der Hand verglichen werde, die uns trotz ihrer Fähigkeit, sich den verschiedensten Aufgaben anzupassen, nicht genügt. Wir schaffen uns künstliche Hände, Werkzeuge für besondere Zwecke, die so genau arbeiten, wie die Hand es nicht vermöchte. [...] So genügt auch die Wortsprache nicht. Wir bedürfen eines Ganzen von Zeichen, aus dem jede Vieldeutigkeit verbannt ist, dessen strenger logischer Form der Inhalt nicht entschlüpfen kann." (Frege, 1882b), p. 52; see footnote 6 above.

In the same spirit, but with a different metaphor, Frege had already written in the *Begriffsschrift* that

> I believe I can make the relationship of my *Begriffsschrift* to the language of everyday life clearest if I compare it to that of the microscope to the eye. [...] [A]s soon as scientific purposes place great demands on the acuteness of differentiation, the eye turns out to be inadequate.[11]

So, Frege demands a rigorous and precise formal system, that can be used as a tool for justifying judgements on basis of other judgements. In his *Grundlagen der Arithmetik* [*Foundations of Arithmetik*] he writing becomes more explicit with respect to why he is interested in this:

> Now these distinctions between a priori and a posteriori, synthetic and analytic, concern, as I see it, not the content of the judgement, but the *justification* for making the judgement. [...] When a sentence is called a posteriori or analytic in my sense one judges [...] about the ultimate ground upon which rests the justification for holding it to be true. If the truth concerned is a mathematical one [the question for this justification] is moved [...] to the sphere of mathematics. The problem now becomes that of finding the proof of the sentence, and of following it up right back to the primitive truths. If in carrying out this process, one comes only to general logical laws and definitions, then the truth is an analytic one [...]. [If] the proof can be derived exclusively from general laws, which

[11] "Das Verhältnis meiner Begriffsschrift zu der Sprache des Lebens glaube ich am deutlichsten machen zu können, wenn ich es mit dem des Mikroskops zum Auge vergleiche. [...] Sobald [...] wissenschaftliche Zwecke grosse Anforderungen an die Schärfe der Unterscheidung stellen, zeigt sich das Auge als ungenügend." (Frege, 1879), p. V; see footnote 6 above.

> themselves neither need nor admit proof, then the truth is a priori.[12]

What Frege is aiming at is the logicist foundation of mathematics that he attempted to carry out formally in his *Grundgesetze der Arithmetik* [*Basic Laws of Arithmetic*]. In the preface to the first volume he writes:

> The ideal of a rigorous scientific method for mathematics, that I have strived to realise here [...] I want to characterise as follows. One cannot insist on proving everything, as this is impossible; but, it can be demanded, that all propositions that are used without being proved, are explicitly declared as such, so that one can see conspicuously, what the whole edifice rests upon. [...]
>
> In virtue of the gaplessness of the chains of inferences it is achieved that each axiom, each presupposition, hypotheses, or however else one might want to call that which a proof rests upon, is brought to light; and thus one gains a foundation for the assessment of the epistemological nature of the proven law. Though it has already been proclaimed many times that arithmetic is just enhanced logic; yet this remains disputable as long as transitions occur in the proofs, that do not conform to recognised logical laws [...]. Not until, these transitions are analysed into simple logical steps can one be convinced that nothing but logic forms the basis. [...]

[12] "Jene Unterscheidungen von a priori und a posteriori, synthetisch und analytisch, betreffen nun nach meiner Auffassung nicht den Inhalt der Urtheils, sondern die Berechtigung zur Urteilsfällung. [...] Wenn man einen Satz in meinem Sinne aposteriorisch oder analytisch nennt, so urteilt man [...] darüber worauf im tiefsten Grunde die Berechtigung des Fürwahrhaltens beruht. Dadurch wird die Frage [nach der Berechtigung] dem Gebiete [...] der Mathematik zugewiesen, wenn es sich um eine mathematische Wahrheit handelt. Es kommt nun darauf an, einen Beweis zu finden und ihn bis auf die Urwahrheiten zurückzuverfolgen. Stösst man auf diesem Wege nur auf allgemeine logische Gesetze und auf Definitionen, so hat man einen analytische Wahrheit [...]. Ist es [...] möglich, den Beweis ganz aus allgemeinen Sätzen zu führen, die selber des Beweises weder fähig noch bedürftig sind, so ist die Wahrheit a priori." (Frege, 1884), §3, pp. 3–4; my emphasis; see footnote 6 above.

> If one considers everything to be in good order, one thus knows exactly the foundations on which every single theorem rests.[13].

To borrow Stephen Wagner's phrase, that was "too good to be true".[14] Frege's project came tumbling down when Bertrand Russell discovered his famous paradox. (Russell's paradox is briefly discussed in chapter 3 above.) Frege tried an *ad hoc* fix for the paradox which does not work, and abandoned his project shortly after for good. The planned third volume of the *Grundgesetze* never appeared.

Frege continued to insist, however, on the importance of the *epistemic* role of logic, and on gapless, absolutely explicit and rigorous justification that he also had already put emphasis on in his *Grundlagen*.[15] In one of his papers, that belong to the so-called "Frege-Hilbert-Debate", he demands that everything that is needed to justify a certain thought, must be stated explicitly in the premises of an argument that leads to that thought and that no other inferences then logical ones are to be employed in the demonstation of that justification. He sums this with the quip:

> About what is foreign to logic, logic knows only what occurs in the

[13] "Das Ideal einer streng wissenschaftlichen Methode in der Mathematik, das ich hier zu verwirklichen gestrebt habe, [...] möchte ich so schildern. Dass Alles bewiesen werde, kann zwar nicht verlangt werden, weil es unmöglich ist; aber man kann fordern, dass alle Sätze, die man braucht, ohne sie zu beweisen, ausdrücklich als solche ausgesprochen werden, damit man deutlich erkenne, worauf der ganze Bau beruht. [...] Durch die Lückenlosigkeit der Schlussketten wird erreicht, dass jedes Axiom, jede Voraussetzung, Hypothese, oder wie man es sonst nennen will, auf denen ein Beweis ruht, ans Licht gezogen wird; und so gewinnt man einen Grundlage für die Beurtheilung der erkenntnistheoretischen Natur des bewiesenen Gesetzes. Es ist zwar schon vielfach ausgesprochen worden, dass die Arithmetik nur weiter entwickelte Logik sei; aber das bleibt solange bestreitbar, als in den Beweisen Uebergänge vorkommen, die nicht nach anerkannten logischen Gesetzen geschehn [sic] [...]. Erst wenn diese Uebergänge in einfache logische Schritte zerlegt sind, kann man sich überzeugen, dass nichts als Logik zu Grund liegt. [...] Wenn man Alles in Ordnung findet, so kennt man die Grundlagen genau, auf denen jeder Lehrsatz beruht. " (Frege, 1893), pp. VI–VII; see footnote 6 above.

[14] (Wagner, 1987), p. 3.

[15] (Frege, 1884), §90.

premises; about what is proper to it, it knows all.[16]

Frege's demands are that a proper logic should be a *rigorous tool for justification*. The *Begriffsschrift*, his logic, introduced to this end, is a formalised language that contains what we now call *logical constants* and symbols for individual constants, predicates, and variables. One assumption that I will make is that the predicates have a precise extension. Vagueness and ambiguity is to be banned, in order to achieve the rigour that Frege demands.

Frege lets us know that an argument that is given to support a certain conclusion must be such that everything that is required to justify this conclusion is contained in the premises. The inferences that are drawn to get from the premises to the conclusion must be none other than logical; otherwise it cannot be guaranteed that really everything that the justification rests on in indeed contained in the premises. Steven Wagner introduces for this the term "ideal justification" in his (Wagner, 1987) (see also chapter 6 above). He explains that an argument ideally justifies a conclusion on basis of the premisses if, and only if, we can reach the conclusion by a chain of self-evident inferences from the premises. This chain of self-evident steps corresponds to the "gaplessness" that Frege requires.

What are we to make of the notion of self-evidence here? It is clearly a pre-systematic notion. Wagner tries to use it to spell out the pre-systematic notion of *logical consequence* that was already appealed to in chapters 6 and 7. There are some preformal notions in the vicinity of that of a self-evident step from one sentence to another: "intuitively correct inference" is often used, for example. 'Intuition', however, is used to mean many different things, and the danger of confusing it with

[16] "[V]on dem ihr Fremden weiß die Logik nur, was in den Prämissen vorkommt, von ihrem Eigenen weiß sie alles." (Frege, 1906), p. 428; see footnote 6 above.

the notion that Kant (or Brouwer or Gödel) used to appeal to some special mental, perception-like faculty is high. The Deductivist account will not depend on any such notion, and thus I will largely avoid the use of the word 'intuitive'.

One constraint on properly logical consequence must be that it is distinct from mathematical consequence, in particular as exemplified by set theory. It must be possible to use ideal justifications in the area of mathematics. As Frege demands, everything that an (now using the new terminology) ideally justified consequence needs for its justification has to be contained in the premises, and in the case of mathematics, in the axioms. Should the pre-systematic consequence relation already involve mathematics, this constraint is violated.

We have seen in chapter 7 that the standard model theory makes rich mathematical assumptions. For our purpose here it is therefore of no use. This does not only go for the standard model theory, but presumably for most model theories. What they have in common is that they are mathematical theories, typically formulated in terms of set theory. Trying to characterise a notion of ideal justification with a mathematical tool is risky, however. It is difficult to guarantee that the mathematical content is not slipping into the argument, and providing something extra that is not contained in the premises.

The example earlier discussed in detail was the continuum hypothesis. A certain sentence C that contains only logical vocabulary will come out true in all models – on the standard model-theoretic interpretation – if, and only if, the continuum hypothesis is true. It seems clear that no properly logical consequence relation should declare sentences to be logical truths that correspond in such a way to mathematical statements. This holds, on particular, for a sentence corresponding to the continuum hypothesis. The continuum hypothesis is independent of Zermelo-Fraenkel set theory (ZF), which, in turn, is one of the strongest mathematical theories we presently have.

It is a pure artefact[17] of the model theory that C is declared a logical truth. It only turns out to be that way, because the standard semantics insists on the determinacy of the powerset of an infinite domain. Because of this there are just not enough models provided by the model theory to give a counter-model for C. Shapiro raises and rejects this point,[18] but I submit that it is essentially correct: C should not come out as a logical truth, and it does not on a Henkin semantics which provides more models, as it does not presuppose that the second-order quantifiers range over the full classical powerset. C is a validity for the standard semantics if the continuum hypothesis is true, because, and only because, of the set-theoretical structure that the models will have *if* the continuum hypothesis is true. This is mathematics creeping into the consequence relation if anything is, and this needs to be banned if we want to characterise a notion of genuine logical consequence.

As already mentioned, however, Henkin semantics is also set-theoretical, and indeed formal semantic theories in general are usually mathematical theories. There does not seem to be in general any way of being sure that these mathematical theories keep their mathematical content to themselves. If mathematics is used, there will always be some mathematical assumptions in the background. In order to make sure that these do not distort the ideal justification we are aiming to characterise, model-theoretic techniques are best dispensed with altogether.

It might be tempting to think that a completeness proof guarantees that the mathematical content in the model-theoretic consequence relation remains harmless,

[17]This notion is used by (Shapiro, 1998) for features of a model that are not intended.

[18](Shapiro, 1998), p. 141; but see also fn. 10 on the same page

but this depends on whether the meta-theory in which the completeness proof is carried out can be "trusted". An mentioned in chapter 6 above, in the extreme case, when the meta-theory is inconsistent, this is blatantly clear.

The suggestion is therefore to characterise logical consequence using a deductive system. This also does justice to Wagner's observation that we should have a gapless chain of self-evident steps from the premises to the conclusion. Wagner suggests that these can be identified with single applications of basic inference rules, like conjunction eleminition, *modus ponens*, or universal instantiation. If all of these basic steps are indeed self-evident, then every deductive consequence is ideally justified on basis of its premises, since a deductively valid argument can always be broken down into single applications of basic inference rules. This should satisfy Frege's demands.

This procedure should also be sufficient to guarantee what Patricia Blanchette demands for a proper logic, likewise with recourse to Frege, and what she calls "epistemic inertia".[19] Our epistemic situation must not get worse from premises to conclusion. If the premises are analytic or *a priori*, then so is the conclusion. This "quality-control check"[20] that a proper logic is meant to provide, should indeed be guaranteed if *nothing* is added by way of a hidden assumption in a logical inference step.

It may be observed, however, that there are many inferences that appear self-evident, that we would not want to call logical. The inference from 'a is red' to 'a is coloured', for example, appears to be beyond any doubt. We would not want to call this inference logical, however. It is rather *analytical* entailment, i.e. it depends only

[19](Blanchette, 2001), p. 124.
[20](Blanchette, 2001), p. 117.

on the meaning of the predicates in the sentences. Genuinely logical inferences are traditionally considered to be also analytic, however. The special kind of analytic inferences that are also logical inference have sometimes been characterised as those that are *formal* in a special way, or *purely syntactical*. In the following section I discuss, assess, and reject this kind of characterisation.

8.3 Purely Syntactic Approaches

It has to be asked first what 'purely syntactic' or 'purely formal' is supposed to mean. Nelson Goodman points out that if we are presented with a purely formal system and told nothing about what any of the symbols mean, we would not even be able to identify which of the symbols belong to which category or what sort of system it is.[21] We need to be given at least *some* content, some interpretation or guidelines to be able see what is characterised. Without any instructions whatsoever, the following diagram could mean anything whatsoever:

$$\frac{p \wedge q}{q}$$

To push it to the extreme, it could be just one primitive sign, maybe a proper name. We need to be given some restriction on how to interpret the symbols in front of us.

Once we have established that we are dealing with inference rules of a given system, John Etchemendy observes that a purely syntactic approach is tied to a specific language; it does not seem to be general enough to just characterise the inferences in one language. Moreover, will any inference do? Certainly not. Etchemendy observes

[21](Goodman, 1956), pp. 202–203.

that claiming that we have to restrict ourselves to sound deductive systems brings us back to where we started. In order to characterise what the sound systems are, we have to appeal to a notion of logical consequence so that we can say that the sound systems are those that allow us to derive only logical consequences. Which the logical consequences are, was our original question, however.[22]

The next idea for a syntactic characterisation is that the introduction- and elimination-rules for the logical constants determine what inferences can be drawn. What are the logical constants, however? Again, not any selection will do. Arthur Prior's famous example of the "logical constant" 'tonk' clearly points out the problem.[23] 'tonk' is characterised by the following inference rules:

$$\frac{p}{p \ \mathsf{tonk}\ q} \qquad\qquad \frac{p \ \mathsf{tonk}\ q}{q}$$

If this rule is added to any standard propositional logic, everything will be derivable from an arbitrary premise.[24] A restriction on the admissibility for introduction- and elimination-rules might be that they have to have certain features. Conservativeness and harmony are prominent candidates. The introduction- and elimination-rules for a new constant are conservative over a system if, and only if, after adding these rules to the system, no sentences that do not contain the new constant are provable which were not provable before.[25] It can depend on what logical constants are already in the system, however, whether the new constant introduced is conservative over the system or not. Introducing classical negation into a logic that has no logical

[22] (Etchemendy, 1990), pp. 2–3.

[23] (Prior, 1961).

[24] For a discussion concerning in how far the rules for 'tonk' give the connective a meaning (or rather fail to do so) see (Ebert, 2005), chapter 3.3.

[25] (Hacking, 1979), for example, insists on conservativeness.

constants so far will be conservative. If the logic already has a conditional, however, Peirce's Law, '$((p \supset q) \supset p) \supset p$', which is not provable from the rules for the conditional alone (at least not in the most common system, see below), becomes a theorem. So, classical negation is not conservative over this system.

Harmony, as championed by Dummett[26], is less sharply defined, but the basic idea is that the elimination rule should be in keeping with the introduction rule. The elimination should be exactly as strong as is justified by the introduction rule. The rules are meant to be symmetric in this way. It seems that this rules out '`tonk`'. It also seems to give intuitionists an argument to rule against classical negation. Not so. Stephen Read shows that in an intuitionistic sequent calculus, in which the introduction- and elimination rules for negation are in harmony, as demanded, a modification of the structural rules turns the system into one that is equivalent to classical proppsitional logic. The modification is that multiple consequences are to be allowed. Read also shows that Peirce's Law is provable without negation in such a system.[27]

Generally, as Graham Priest points out,

> introduction and elimination rules are superimposed on structural inferential rules [...]. Such structural rules are not inevitable, and the question therefore arises, how *these* rules are to be justified.[28]

There is range of examples of systems with structural rules that are very different from the "ordinary one". So-called sub-structural logics, for example, have been studied extensively recently.[29] Sub-structural logics weaken the structural rules for

[26] See in particular chapters 9 and 11 of (Dummett, 1991b).
[27] (Read, 2000), §3.2.
[28] (Priest, 1999), §3.
[29] See, for example, (Restall, 2000); for a concise introduction see (Restall, 2002).

classical logic in various ways, and can thus create a background against which the connectives of various relevant logics behave "nicely", like the classical connectives against the background of the classical structural rules. The intuitionistic relevant logic introduced by Neil Tennant[30] has harmonious introduction and elimination rules for the logical constants, but does not have transitivity as a structural rule. This means that it is not in general the case that if '$p \vdash q$' and '$q \vdash r$', then '$p \vdash r$'. Even 'tonk' received a kind of renaissance recently. Roy Cook presents a logic that like Tennant's does not have transitivity as structural rule in which 'tonk' does not produce triviality (triviality in the sense that every sentence is deducible from arbitrary premises).[31]

These last examples probably seem strange to most, and appear to have "intuitively" little to do with logical consequence, while the more common systems seem to capture the pre-systematic notion of logical consequence much better. This, however, is the whole point of the exercise. Nothing that makes the more common system "good" and the less common systems "strange" depends on purely formal features of the systems. It is rather our examining of the systems and comparing them to what we think about logical consequence that makes us balk at some of the systems.

No "purely formal" criterion, for example, says that the structural rule of contraction should be included in a system. Contraction is the rule that if '$p, p \vdash q$' then '$p \vdash q$'. We understand that this says if 'q' is derivable from 'p' and 'p' (yes: 'p' twice), then 'q' is also derivable from 'p' alone. Rejecting this *obvious* triviality seems absurd. It is just one of the structural rules, however, that is up for grabs.

[30]See (Tennant, 1986a) and (Tennant, 1987).
[31](Cook, 2005).

Rejecting contracting as a structural rule provides a fruitful background for relevant and paraconsistent logic, however, as *ex falso quodlibet* is not in general valid anymore.[32]

8.4 The Deductivist Approach

The moral to be drawn from the considerations of the last section is not that logical consequence cannot be characterised by inference rules. Rather these examples suggest that the attempt to find formal criteria for a purely syntactical characterisation of proper logic that do not involve checking the rules against a pre-systematic notion does not seem possible.

My suggestion is therefore to characterise the logical consequence relation by a system whose basic rules of inference fit our pre-systematic notion of logical consequence. Such a characterisation will be fallible, we might make mistakes, but those can be corrected by changing the rules. Before I spell out the proposal in more detail, it should be remarked that so far I have been describing the situation from a very realist point of view, as if there was an independently existing consequence relation. The Deductivist account is independent of such an assumption. Whether there is a logical consequence relation that is entirely independent of human beings, or whether it is some relation that is in some way dependent on us or our language use, plays no role in determining the shape of the Deductivist proposal.

It might be instructive to think of the axiomatisation of some scientific concepts as a parallel. Whether the concepts are understood in a realist or, say, conventionalist way does not matter in the process of providing the axioms – the way the

[32] See e.g. (Schroeder-Heister, 2003).

axiomatic system is arrived at is the same in either case. Experiences are tested against the axiomatic system, and in the case of a mismatch adjustments are made: either the axioms are changed, or the experiences are re-investigated and maybe re-interpreted. The usual criteria for the evaluation of scientific theories will play a role: simplicity, fruitfulness, exactness, etc. I come back to this strategy a little farther below.

Characterising logical consequence, I suggest, should proceed in a similar way. Axiomatisation is not the ideal way, however, to come up with a logical theory. If axioms for logical consequence are provided, a complete *theory* is gained from those, by closing under *logical consequence* – this is, however, what we are after. At least, some rules of inference are needed to operate the axioms. Typically these are a rule of substitution and *modus ponens*. A more direct approach seems to be to dispense with the use of axioms, and rely only on inference rules.[33] Basic rules of inference have been said above to fit the demand for ideal justification. While this might be possible with suitable axioms, introduction- and elimination-rules are more directly linked to our justificatory practice, and thus seem better suited to codify logical consequence. We will also see below how the meaning of the logical constants can be linked straightforwardly to their operational rules.

Coming back to the question of realism or conventionalism concerning logical consequence: in either case, the data we take in to create the system of logic that is meant to capture the logical consequence relation concerns our inferential practice. Of course the logical system is not to describe how we actually reason in all respects. We need to idealise mistakes away and find a way to make the system safe in the

[33]Axioms can be viewed as inference rules of the form: "at any line of the proof Φ can be inserted" (where Φ is the axiom in question). This does not change the general point, however.

sense that if the rules are applied correctly, then whatever is deduced is ideally justified, in the sense described in section 8.2 above, viz. that the conclusion is deduced from the premises by a gapless series of self-evident steps. The self-evident steps were identified as the single applications of introduction- or elimination-rules for one of the logical constants.

We have seen above that the logical constants can be characterised by their introduction- and elimination rules, but there is no general formal criterion forthcoming to determine with ones the correct logical constants are. Here we have to check against the pre-theoretic notion of logical consequence. As Goodman describes it

> The point is that the rules and particular inferences [the pre-systematic practice] alike are justified by being brought into agreement with each other. *A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend.* The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either.[34]

What Goodman introduces here (in the end to find his solution to the problem of induction) is what John Rawls later called "reflective equilibrium". We do not need to follow Goodman in his constructionalist interpretation of that technique, however. Goodman sees the consequence relation as changing with the social practice. It might, however, also be the case that we are approximating our practice by trial and error to the true, independently existing consequence relation. The methodology must be the same. We adopt inference rules in attempts to codify logical

[34](Goodman, 1983), p. 64; Goodman's emphasis.

consequence. If a rule, when applied correctly, reliably only allows us to infer true sentences from true sentences, and moreover, if by our best considered judgement, the consequences are justified solely on basis of the premises and nothing else is needed, then we will stick to the rule as codifying the logical consequence relation. If a rule fails to do so, at least if it does in a significant way, and if that failure cannot be explained, by our best judgement, by a mistake the reasoner made in applying it, then we will change the rule, as we convinced ourselves that it does not characterise an inference steps that accords to the logical consequence relation.

To this end we design logical systems with formal languages. In these systems the logical constants have a precise behaviour. They are characterised by their inference rules, and these rules determine which sentences can be inferred from which sentences. The job of the logical constants is to indicate the inferential role that the sentences stand in with respect to each other. Depending on what the logical constants in sentences are, we can apply the basic rules of inference to it. If we assume the sentences 'p' and 'q' for example, we know that we can infer '$p \wedge q$' from them, because this is how '$\wedge$' is characterised. We can say that the meaning of the logical constants (in a system) is thus given by its introduction- and elimination-rules (in that system).

We should strive to design a system that contains only such rules as allow us to draw ideally justied conclusions from premises. Applying such a system is, of course, difficult. We have to match ordinary language sentences to the formal language, and this is another source of possible mistakes. Usually we know more-or-less exactly, however, how to represent an ordinary language sentence in a formal language, when we are sufficiently familiar with the system. The logical constants are certainly inspired in some way by the natural language locutions, but as Frege warned us, ordinary language lacks the precision that is required for ideal justification.

The logical constant '$\wedge$', for example, is thus rigorised from the ordinary language word 'and' (or its equivalents in other languages). It is rigorised in the sense that it obeys completely its introduction- and elimination rules. We, therefore, have to be careful not to translate any sentence that contains an occurrence of 'and' as a conjunction. The sentence 'You come one step closer and I call the police', for example, should, on the most straightforward understanding, not be formalised as a conjunction. We know that, because we would not want to apply conjunction elimination to it. Note, that 'and' is one of the easier cases, if not the easiest case. A formalisation of an English sentence needs care, and often we have to check whether what we mean by that sentence is indeed done justice to by using a logical constant that was inspired by an ordinary language word, that might figure in the sentence in question.

Once this "translation" is done, the inference rules can be applied in an almost mechanical fashion. If the system is indeed codifying logical consequence, all sentences that are inferred from given premises are ideally justified on their basis. As soon as the sentences are formalised so that the inference rules are applicable to them, we do not need to know anymore, what any of non-logical vocabulary means to draw the inferences, we only have to apply the rules. The demand for rigour, once the formalisation is done, is met. Note, that the problems and vagaries that arise before we managed to get the sentence into the formal language, are by no means specific in any way to the Deductivist conception of logic. Any logic that uses a formal language will have this translation problem.

All this is pretty much commonplace. We do not have to start from scratch and try to come up with a system of logic. Logic has been studied for a long time, and many well-established systems of logic exist. My discussion is concerned with the specific question whether second-order inference is indeed logical, i.e. whether inferences drawn in accordance with the rule for the second-order quantifiers are part of the logical consequence relation. To get clear on this question it seems advisable stay away from the discussion whether classical logic or one of its competitors are to be preferred. I will, for simplicity's sake, assume classical logic. The arguments I present should be general enough, however, to carry over to intuitionistic or relevant logic, *mutatis mutandis*. If the following argumentation establishes that second-order quantification is properly logical, then should the correct logic turn out to be intuitionistic, my argument should carry over with minimal adjustments.

Let us then assume that the inferences we draw in accordance with classical first-order logic count as cases of logical consequence. The typical objection that are put forward against the second-order quantifiers were investigated in some of the previous chapters. Quine's claim that second-order logic is really set theory in disguise depended on his criterion for ontological commitment which was found deficient above. According to the new criterion no such commitment arises with second-order quantification, or, at least, no *additional* commitment arises. If the quantifiers now turn out to be logical, they do not range over sets unless predicates refer to sets already. The alleged mathematical content of second-order logic was attributed to the standard model theory. Within this framework of a Deductivist position, this problem should not arise at all. If the quantifiers turn out to be logical on the account here, they will figure in ideally justifying inferences and thus be incapable of bringing any content into an inference that would prevent a conclusion being ideally justified on the basis of its premises.

As mentioned above, the role of the logical constants is to indicate what inferential relations the sentences of the language stand in to each other. These inferential relations are determined by the introduction- and elimination rules for the logical constants. Let us thus have a look at the introduction- and elimination-rules for the first- and second-order universal quantifiers, as introduced in chapter 2:

$$\frac{\Phi(t)}{\forall x \Phi(x)} \forall^1\text{-I} \qquad\qquad \frac{\Phi(T)}{\forall X^n \Phi(X^n)} \forall^2\text{-I}$$

$$\frac{\forall x \Phi(x)}{\Phi(t)} \forall^1\text{-E} \qquad\qquad \frac{\forall X^n \Phi(X^n)}{\Phi(\Xi)} \forall^2\text{-E}$$

Φ is an open sentence matching the number of argument places of the expression it applies to, in the case of the second-order rules possibly just one term; 't' is a term of the language.

Restrictions:

$\forall^1$-I 't' does not occur free in any of the assumptions that $\Phi(t)$ depends on.

$\forall^1$-E 't' is free for 'x' in Φ.

$\forall^2$-I 'T' is a n-place predicate letter and does not free in any of the assumptions that $\Phi(T)$ depends on.

$\forall^2$-E Ξ is an open sentence with n argument places; no variable in Ξ is bound in $\Phi(\Xi)$ that are not already bound in Ξ.

Note that neither the schematic letter are part of the language, nor the 'n'. They are merely placeholders for object-linguistic expressions of the appropriate syntactic category.

The rules for the second-order quantifiers are analogous to the ones for the first-order quantifiers, except for differences required by the different syntactic role that predicate have from names. First-order logic has logical constants for generalisation over the name position – the first-order quantifier – but the predicate letters do not play any role in inference. Given the similarity of the rules, and the curious lack of inferential function of the predicate letters, should second-order quantification not be allowed as logical? First-order quantifiers are well entrenched in our inferential practices and generalisation over name position is considered properly logical. What would keep us from applying a very similar device for generality to predicate position, too?

The obvious worry is that the first-order quantifiers range over the objects of the domain. We do not want to commit us to predicates having a referent, however, so we should not quantify into predicate position. The reply here is, that on the Deductivist approach the quantifiers (both first- *and* second-order) should not be thought of as objectual. Rather, the quantifier rules are quasi-substitutional. 'Quasi-substitutional' here mean that the schematic introduction- and elimination-rules specify what inferential role the sentences containing the respective quantifiers stand in with respect to other sentences. A range of entities is neither mentioned, nor specified – and to that extend the account of the quantifiers is a form of substitutional quantification.

Ordinary substitutional quantifiers, however, require a specified substitution class of expression that can be substituted. The Deductivist approach dispenses with this, too, in favour of a *fully schematic* approach, in a sense analogous to what Shaughan Lavine describes for axiom schemata.[35] The fully schematic rules for the

[35](Lavine, 1994), pp. 230–240.

quantifiers (and also the sentential connectives, incidentally) allow any expression whatsoever, as long as it is of the right syntactic type, into the scope of the first- and second-order quantifier rules. The idea of full schemata allows for *arbitrary extensions* of the language. Acceptance of such a schema entails accepting it for every (possible) extension of the language, not just for the expressions contained in it. This is not only unproblematic for logical rules, but, as Harris argues, characteristic of them.[36] The fully schematic logical rules are applicable to *any* language (that has the corresponding syntactic categories) and allow for arbitrary extensions of the language. Thus Etchemendy's complaint mentioned in section 8.3 above, concerning how purely syntactic approaches are deficient because they are tied to a language, does not apply to the Deductivist account. So, it seems, the last remaining objection against syntactic approaches is shown not to apply to the Deductivist position either. The generality of the quasi-substitutional account is secured, because it is fully schematic.

This almost characterises the Deductivist approach to logic. One remark about the second-order quantifiers has to be still made, however (before we conclude that they belong into the realm of logic). The $\forall^2$-E rule allow to instantiate with open sentences and not just predicate letters; this has to be explained. Consider the case of explicit definition. Say, a predicate 'R' is defined to hold of any object if, and only if, both 'K' and 'B' hold of it. We introduced, by way of definition a new predicate into our language. The quantifier rules are fully schematic, so they apply to the new predicate. Assume we instantiate a universally quantified sentence with it. Directly after this step, we infer from 'Rv', '$Kv \wedge Bv$'. As this is how 'R' is explicitly defined, this is admissible. It seems now, that the quantifier rule applied

[36] (Harris, 1982).

to a logically complex predicate, or an *open sentence*. Every open sentence can by explicit definition be turned into a single predicate and can be eliminated in favour of the more complex expression again.

In general it seems that open sentences have the same syntactic type as predicates. Consider the open sentence: '$\forall x((Rx \wedge Cx) \equiv H\ldots)$'. As in the case of any predicate, inserting a name, say 'e', into the space results in a sentence. Moreover, filling the free space with a variable and binding it with a prenex quantifier also results in a sentence. A fully general account, that declares the second-order quantifier to be a means of generality concerning the entire syntactic type should thus not only allow predicate letters as instances of the $\forall^2$-E rule, but *all* predicate expressions, inclusive of open sentences.[37]

Thus, I propose, the second-order quantifiers are part of logic proper. I take the objections to them that were raised so far to be answered:

> they do not bear extra ontological commitment (indeed, they need not be considered to have a "range" at all);
>
> the consequence relation is deductive, and hence very straightforward to handle and not intractable at all;
>
> the rules for the quantifiers allow for gapless ideal justification as required by Frege in order to secure the epistemic impact of logic;
>
> the alleged mathematical content of the quantifiers is an artefact of the model theory: on the Deductivist conception the quantifiers merely indicate inferential role.

[37]Worries about impredicativity will be addressed in section 8.5.2.

Generality – what is commonly called ‘quantification’ – is a properly logical device. The restriction of the application of generality to name position appears arbitrary, since on the Deductivist conception the objections to it can be refuted. Thus, the second-order quantifiers and their operational rules “complete”, in a way, first-order logic which has a device for generality but only applies it to one of its two (or three if function letters are counted extra) syntactical positions.

How close, now, is this conception – one that involves characterising a pre-systematic notion of logical consequence with inference rules that have no further justification other than to be in accordance with what we are pre-systematically willing to infer (in some idealised way) – to Frege? As mentioned above, it is not central to the Deductivist account to capture Frege’s intentions. My main motivation in the earlier quotation was to pay proper respect to the thinker who influenced me to advance along the Deductivist line. For what it is worth, however, here is what Warren Goldfarb reckons Frege’s position to be:

> Now, I believe Frege would reject the idea that inference rules rest on or presuppose the principles expressing their soundness. Rather, our appreciation of the validity of the rules is not the recognition of the truth of any judgment at all; it is manifest in our use of the rule, in our making one assertion on the basis of another in accordance with the inference rule. There is nothing more to be made explicit, although of course individual instances of the inference rule can always be conditionalized and asserted as logical truths.[38]

[38](Goldfarb, 2001), p. 38; a similar interpretation of Frege’s conception of logic is proposed by Thomas Rickett, (Rickett, 1985), p. 7.

8.5 Two Problems for the Deductivist Approach

In this section I present two difficulties that the Deductivist approach faces: a kind of inherent incompleteness of second-order logic, and concerns about the impredicativity of the introduction- and elimination rules. The first worry seems to be specific to the Deductivist approach, and not a general problem that any account faces (although the Deductivist approach might not be the *only* one to which this worry applies). The impredicativity worry seems to be pressing for the Deductivist account, too, although all syntactic approaches will have to find an answer to it.

In neither case do I offer a definitive answer to the problems. Concerning both objections I outline a range of options how one could understand and accordingly reply to the challenges from a Deductivist point of view. I indicate in each case where my sympathies lie, and why – despite the lack of a conclusive argument.

8.5.1 Inherent Incompleteness

In chapter 6 I rejected semantic incompleteness as a viable criterion to judge whether a deductive system can count as a proper logic. One cannot escape Gödel's incompleteness results, however, and for the Deductivist conception of logic no less than for others a possible problem lurks here.

Consider the following case, using the conditionalisation techique described in chapter 7 above. Let '$PA2$' be the conjunction of the second-order axioms of Peano Arithmetic. By Gödel's incompleteness proof, there is a sentence of second-order Peano Arithmetic that is true according to the axioms, but not provable from it. Call this sentence 'G'. The conditional '$PA2 \supset G$' should thus also be true. It cannot be provable, however, on pain of contradiction (if second-order Peano Arithmetic is consistent). If it was provable, then the antecedent could be discharged and we would have a proof for 'G', contradicting Gödel's proof.

Consider now the universal generalisation of the conditional. The universal generalisation is achieved by replacing all arithmetical constants in the conditional by appropriate variables, and binding them by prenex universal quantifiers: '0' is replaced by a first-order variable and the symbol for the successor function is replaced by a one-place function variable (and analogously for '+', '·', etc., should such constants be present in the axioms – as mentioned in chapter 2, section 5.3, these functions are definable in second-order Peano Arithmetic). The resulting sentence contains only second-order logical vocabulary. This sentence cannot be a theorem of second-order logic. If it was, *a fortiori* it would be a theorem of second-order Peano Arithmetic. It could then be instantiated with the standard arithmetical constants, and would result in '$PA2 \supset G$' which, as noted, cannot be provable.

The generalised conditional should be considered true, however. 'G' is true of the natural numbers, so '$PA2 \supset G$' is true, as mentioned above. The universal generalisation of that conditional asserts that whichever of its instances makes the antecedent of the conditional true, also makes the consequent true. The antecedent is the conjunction of the second-order Peano-Dedekind axioms, however, and those describe the natural numbers. If the generalised conditional is instantiated in any way such that they are true, 'G', thus, will also be true. So it seems that we have a sentence of pure second-order logic (it contains no non-logical vocabulary) that is true but not provable. Does that show that second-order logic is inherently incomplete? Perhaps not quite yet.

In order to show that '$PA2 \supset G$', and with it its generalisation, are true it seems that we have to appeal to extra-logical resources. A case could perhaps be made that a theory of truth or a semantical theory will be needed, and that such a theory will have to incorporate at least some mathematics. From the Deductivist point of view, a truth predicate will have to be introduced into the language, together with suitable rules or axioms that describe its behaviour. In a theory that contains second-order Peano Arithmetic and a truth-predicate for it, the Gödel sentence 'G' of second-order Peano Arithmetic (n.b. the theory *without* the truth predicate) is provable, and hence also the conditional. As 'G' can be proven in this theory, but cannot be proven in second-order Peano Arithmetic, the former is strictly stronger than the latter. Indeed, we know by Tarski's Theorem that no (classical) theory can consistently contain its own truth-predicate. This shows that extra resources are needed. If these extra resources are mathematical, and not logical, one could perhaps argue that the generalised conditional is not a logical truth, despite its being true and containing only logical vocabulary. It would be, in a sense, on a par with the second-order sentence expressing the continuum hypothesis that was discussed in chapter 7.

This strategy does not seem to be open, however, for Daniel Leivant shows that a truth predicate for second-order arithmetic is definable with the help of the resources of third-order logic.[39] Third-order logic is the next step up in an infinite hierarchy of nth-order logics, that is in many ways similar to the hierarchy of types of simple type theory. Second-level predicates are introduced that apply to the "ordinary" predicates of first- and second-order logic which are now called first-level predicates. Third-order variable stand in the place of second-level predicates in the same way that second-order variables stand in the place of first-level, i.e. ordinary predicates. Third-order quantifiers bind these.

[39]In (Leivant, 1994), §3.7.

With the help of this third-order defined truth-predicate for second-order Peano Arithmetic, we can derive 'G' in third-order logic from '$PA2$', since these are the axioms of second-order Peano Arithmetic. As mentioned above, in the presence of a truth predicate this is possible. We can then conditionalise 'G' to '$PA2$'. Since our conditional '$PA2 \supset G$' is now a *theorem* of third-order logic, we can infer by first- and second-order ∀-introduction the universally generalised sentences. We have thus proven that the second-order sentence is a theorem of third-order logic, but it is not a theorem of second-order logic. Call this the *non-conservativeness* of third- over second-order logic. Since the sentence contains only second-order terminology this non-conservativeness demonstrates – with purely (third-order) logical means – an inherent incompleteness of second-order logic.

According to (Leivant, 1994) the result for the truth predicate for arithmetic generalises for higher orders. Call a system that combines nth-order logic (for any $n \in \omega$) with the second-order axioms of Peano Arithmetic nth-order arithmetic. For any such system of nth-order arithmetic, a truth predicate is definable in $(n+1)$th-order arithmetic. An argument that is a generalised version of the one just sketched establishes, using the result stated by Leivant, that in general $(n+1)$th-order logic is non-conservative over nth-order logic. Symbolised, the situation looks as given below, with 'G_n' now standing for the Gödel sentence of nth-order arithmetic:

$$\nvdash_{\text{2OL}} \quad PA2 \supset G_2$$

$$\vdash_{\text{3OL}} \quad PA2 \supset G_2$$

$$\nvdash_{\text{3OL}} \quad PA2 \supset G_3$$

$$\vdash_{\text{4OL}} \quad PA2 \supset G_3$$

$$\nvdash_{4\mathrm{OL}} \quad PA2 \supset G_4$$

$$\vdots$$

The '2OL', '3OL', and '4OL' indices specify that the stated provability or non-provability result holds for the deductive system of second-, third-, and fourth-order logic, respectively. Where the respective conditional is provable, it universal closure is provable, too, by first- and second-order ∀-introduction.

Note, by the way, that a non-conservativeness result for second- over first-order logic cannot be shown with this template. For first-order arithmetic is not finitely axiomatised – the induction axiom is a *schema*, and cannot figure as such in a conjunction in the way that '$PA2$' is the conjunction of the second-order axioms, where the second-order induction axiom is a sentence of the language.

The problem for Deductivism that arises from this inherent incompleteness is not that some logical truth cannot be proven. As indicated above, Deductivism does not claim that there is *one* system in which the entire pre-systematic consequence relation can be captured. It rather sets constraints on which systems are acceptable as proper logics. The non-conservativeness of $(n+1)$th- over nth-order logics thus at most[40] shows that for any proper logic there will always be a stronger one, i.e. one that proves more logical truths than the one before.

The non-conservativeness problem concerns rather the role of the quantifiers in the Deductivist approach. The meaning of a quantifier was said to be characterised by its inferential role. The virtue of this approach is that no model theory or any other external resources are needed. Whether a sentence is declared a logical conse-

[40]Whether it shows even this depends on the outcome of some of the considerations that are discussed below.

quence of some premises depends *only* on whether it can be proven in the deductive system. What can be proven, in turn, depends only on what logical constants are contained in the sentences, since those determine the inferential relations that sentences stand in with respect to each other. This, however, now seems to be a claim that cannot be upheld – given the non-conservativeness result. For the universal generalisation of '$PA2 \supset G$' contains only second-order logical constants, but it cannot be proven in second-order logic. Second-order logical truth seems not to be, contrary to Deductivism, determined by the inference rules of second-order logic.

As far as I can see there are four options how to react to this inherent incompleteness of second-order logic from a Deductivist perspective.

(I) *Denying the Data*

That indeed a problem is created by the non-conservativeness of third- over second-order logic depends on third-order logic being proper logic in the Deductivist sense. If it is not – if, for example, third-order logic is a mathematical theory as opposed to second-order logic which is a proper logic – then the proof relies on non-logical resources and the sentence in question is not a logical truth after all. It would be "merely" a mathematical truth expressible with purely second-order resources. Option (I) would thus say that second-order logic is not inherently incomplete at all: all its logical truths are provable, or it least the contrary has not been shown by the argument above.

In a similar fashion the second-order sentence expressing the infinity of the universe (see chapter 7, section 3.1) is a mathematical truth ('universe' here does not refer to the physical universe, but is inclusive of abstract, mathematical objects). If arithmetic is correct, there are infinitely many natural numbers, and this suffices for the universe being infinite; we do not even have to venture into set theory for that. The second-order sentence expressing infinity, however, is not a logical truth – not even on the standard semantical conception.

What would the reasons be to reject third-order logic as logic, however? The introduction- and elimination rules for the third-order quantifiers (they are given under option (III) below – see there) are surely sufficiently similar to the rules for the second-order quantifiers. They are, at any rate, if the rules for the second-order quantifiers are sufficiently similar to the rules for the first-order quantifiers which was one of the reasons to accept the second-order quantifiers in the first place. Also that there are standardly no second-level predicates in the formal language is not a good argument. There are standardly (if the standard is first-order logic) no second-order quantifiers in the formal language.

A tempting line of argument might be that by admitting first-level predicates as argument places of second-level predicates, we implicitly require that first-level predicates refer to *objects* – as (so the argument says) predication can only be of objects. From the Deductivist standpoint, however, logic should be neutral concerning ontology. A system that commits us to some second-order ontology, then, would have to be disqualified.

As Michael Dummett points out, however, first-order quantifiers themselves can be thought of as second-level predicates:

> Of, for instance, an atomic sentence, formed by putting a proper name in the argument-place of a simple (first-level) predicate, Frege says that it is used to make a statement about an object (the referent of a name), saying of it that it falls under a certain concept (the referent of a predicate).

> If, however, we consider a sentence formed by attaching a [first-order] quantifier to a first-level predicate, Frege considers the quantifier as a predicate of second level, having a second-level concept as its referent: and he accordingly says that such a sentence is used to make a statement about a first-level concept (the referent of a first-level predicate), saying of it that if falls under the second-level concept for which the quantifier stands.[41]

It is a mistake, therefore, to think that we commit ourselves to extra ontology by taking the step to third-order logic. If indeed the consideration that second-level predicates are so committing were correct, then *first-order* quantifiers might already be suspect in this way. If Dummett's observation is right, however, then it is a mistake to think that predicates always require expression that refer to objects to fill their argument places. In any case, it does not appear to be the case that we are *forced* to take a view of predication that is committing in such a manner; but then third-order logic cannot be disqualified in this way.

We can restate the point using the formal language of third-order logic. The "official" Deductivist version of third-order logic uses introduction- and elimintation-rules, of course. For the purpose here, however, it is easiest to state the point using the third-order comprehension schema of an axiomatised version. This comprehension schema, simplified for the monadic case, with '$\mathcal{X}$' being a third-order variable looks like this:

$$\exists \mathcal{X} \forall F [\mathcal{X}(F) \equiv \Phi(F)]$$

The usual provisos apply. As in the case of the second-order comprehension schema,

[41](Dummett, 1981), p. 174.

any open sentence can take the place of Φ – including $\ulcorner \exists x(\ldots x) \urcorner$. We thus get as an instance of the third-order comprehension schema:

$$\exists \mathcal{X} \forall F[\mathcal{X}(F) \equiv \exists x(Fx)]$$

If one wanted to pursue option (I) in order to react the problem of inherent incompleteness it would have to be shown that the Frege-Dummett view is in some relevant way mistaken. As, presumably, the construal of first-order quantifiers as second-level predicates is not dependent on a Fregean ontology of concepts, no additional commitment appears to occur by going third-order.

Overall, this option does not appear to be too attractive.

(II) *Re-Interpreting the Result*

Another option would be to observe that the language in which the proof is carried out has changed – not only so far as third-order vocabulary is now present, but concerning the second-order quantifiers themselves. As stated above, the second-order inference rules are construed as fully schematic, in Lavine's sense. That means that we allow the rules to operate on *any* extension of the language. So now third-order formulae are allowed as instances of the second-order rules. It will, again, be easier to see what is happening if we consider a axiomatic system. Consider the second-order comprehension schema for the monadic quantifier:

$$\exists X \forall y[Xy \equiv \Phi(y)]$$

'Φ' can now contain third-order vocabulary. We get instances like this one, for example:

$$\exists X \forall y [Xy \equiv \forall \mathcal{X} (\neg Fy \vee \mathcal{X}(F))]$$

Since there is, in particular, a truth predicate for the second-order fragment of the third-order language, the expressive resources have changed. As far as the second-order fragment is concerned, these expressive resources might as well be non-logical; their logical structure cannot be analysed by the resources of second-order logic. It is only in the third-order language that the logicality of the new expressions is revealed.

It seems, however, in order to make this option work, more needs to be said about the connection between third- and second-order logic. Merely pointing out that there are more expressions in the language now which the second-order fragment of third-order logic cannot discern, might not appear to be enough.

(III) *Modifying the Approach*

Maybe the approach to specify the introduction- and elimination-rules for the quantifiers as specific to the different levels was mistaken. The fact that the quantifiers rules look so similar can perhaps be treated as evidence that there is really just *one* pair of rules for the universal quantifier (and one pair for the existential quantifier) that schematically applies across the board, so to speak, at every level.[42] Thus we could replace these specific rules for (monadic) universal quantifier elimination

$$\frac{\forall x \Phi(x)}{\Phi(\xi)} \forall^1\text{-E} \qquad \frac{\forall X \Phi(X)}{\Phi(\Xi)} \forall^2\text{-E} \qquad \frac{\forall \mathcal{X} \Phi(\mathcal{X})}{\Phi(\mathfrak{X})} \forall^3\text{-E} \qquad \ldots$$

(where '$\mathfrak{X}$' in the $\forall^3$-elimination rule reserves a place for an arbitrary second level

[42]Crispin Wright suggested this option in an *Arché* research seminar in September 2004.

open sentence) with one general rule:

$$\frac{\forall X^n \Phi(X^n)}{\Phi(\Xi^n)} \forall^{(n+1)}\text{-E}$$

and analogously for the introduction rule for the universal quantifier (and the rules for the existential quantifier). The 'n' does not belong to the language (just as the Greek letters do not), but merely indicates the level of the variable. Next to the usual restriction that apply (as introduced in section 8.4 above), we will have to specify that only open sentences of the right level n are used in place of 'Φ', that the argument places match, and also introduce a special requirement for the case where we are dealing with first-order variable – names will be the very special case of zero-level open sentences.

We would thus characterise the quantifiers of *all* levels at once, so one could argue that the inherent incompleteness does not really arise. The quantifiers do not get their meanings separately at each level. Rather, universal quantification is a rule that operates at all levels.

This proposal is hard to assess. A technical worry would be whether the levels of the quantifiers go into the transfinite, or whether the hierarchy of nth-order logics is restricted to finite n. There is, after all, transfinite type theory,[43] and there are strong structural similarities between the theory of types and higher-order logic. Depending on how the result of Leivant and the non-conservativeness argument generalise into the transfinite, the inherent incompleteness objection might come back: ω-order logic (if indeed there is such a system) might turn out to be non-conservative over the system specified with the general rules for quantifiers at all

[43]See (Andrews, 1965).

levels. Or is the general rule not restricted to the finite levels?

A bit closer to earth is the worry that, given the general rule, the meaning of the second-order quantifier has, in some sense to do with, for example, 1879th-order quantification. On the other hand, one might just take this to be just the lesson of the non-conservative result: quantification at any level cannot be seen in isolation. The mere fact that it is an unusual way to think about quantification might be outweighed by the apparent similarity of the rules at the different levels. They clearly follow a common pattern in *some* sense. What needs to be argued if this option is chosen is that (i) the sense in which these rules follow the same pattern is indeed relevant for this discussion, and that (ii) this in fact dissolves the objection. Should it turn out that the only way to make sense of these general rules that apply to all levels is to instantiate them (in some sense) to each level, then the question arises in what sense it is really the general rule that gives meaning to the quantifiers at each level, rather than the respective instances of it.

(IV) *Stop Worrying and Learn to Love the Result*

Shapiro writes in his discussion of logical consequence that

> there is no reason to expect that the pretheoretical, modal notion of logical consequence is effective, and that there is a complete deductive system for it.[44]

His aim is, of course, to dispel worries about the intractability of the standard model-theoretic consequence relation of second-order logic – especially with respect to the impossibility of a complete deductive system for this consequence relation. As I argued in chapter 6 above, I agree with him that semantic incompleteness does

[44](Shapiro, 2005a), p. 772.

not provide an argument that a given system is not a proper logic. I also argued in section 8.4 above that the lack of an all comprising system that characterises the pre-systematic consequence relation is not a refutation of the Deductivist project. The important feature is that *only* genuinely logical consequences are declared as such by a system, not that *all* are.

Above, I mainly took that to apply to different kinds of inference pattern. First-order logic is proper logic although it does not present the whole story of logical consequence, and not even, in some sense the whole of the language that it is about: it contains predicates, but no inference rules for generality that applies to them. Adding such rules together with the range of vocabulary required to use them results in second-order logic.

Maybe the best way to answer to the inherent incompleteness challenge is to take a more radical stance along these lines. Maybe we can just accept the fact that there are there are logical truths (and logical consequences) of second-order logic that cannot be captured with the inference rules of second-order logic. To resolve the worry will require, however, that the Deductivist account of the logical constants is understood in a different way. The account of the logical constants was that these indicate what inferential relations the sentences of the language stand in to other sentences. So far, this was naturally understood as saying that these inferential relations are determined by the logical vocabulary *contained in* the sentence. I submit that there is room to reject this condition.

The view behind the requirement that only the logical vocabulary that figures in the sentences that the inference is concerned with contributes towards determining whether or not an inference is valid, is the *molecular* view of meaning, most prominently defended by Dummett.[45] Someone who favours classical logic, however, probably has to be reject this view in any case. The example is once again *Peirce's Law*: '$((p \supset q) \supset p) \supset p$'. Peirce's Law is a classical tautology, but not provable intuitionistically. What is required in a natural deduction system is a proof by *reductio* that involves at one step a double-negation elimation which us not in general available intuitionistically. Peirce's Law does not contain a negation sign at all, however. If the meaning of the conditional is given by its introduction- and elimination-rules and the validity of the sentence is supposed to flow, as it were, from nothing but the vocabulary contained in it, then the presence or absence of rules for negation in the logic should play no role whatsoever: a proof by reductio should not be needed for a sentence that expresses a logical truth if that sentence does not contain a negation – let alone a proof that involves double negation elimination. So, whoever wants to hold on to classical propositional logic has to reject the molecular view in favour of a more holistic approach.

It can be observed that the molecular view need not be taken on board: the Deductivist approach does not require it. I have run the two views together so far, and maybe that appears to be the natural thing to do, but they can be divided. Deductivism can be spelled out in a way that is compatible with a holistic approach if the requirement that the inference rules determine the entailment relations that sentences stand in with respect to each other is relaxed to encompass the rules for *all* the logical vocabulary that figures in the deductive system. *All* the sentential operators of (classical) propositional logic combine to determine that Peirce's Law can be proven, not just the rules for the conditional. Something similar could be defended for second-order logic: the inference rules for the logical constants of

[45] See (Dummett, 1991b), esp. chapters 9–11.

second-order logic do not determine all the logical truths and consequences of second-order logic; the inference rules of other logical constants – in the example above, those for the third-order quantifiers – are needed in order to establish that some second-order sentences express logical truths, or that some second-order sentences are logical consequences of other second-order sentences. The description of the situation seems to be correct. Is it acceptable as a Deductivist approach, however?

I suggest that it is. Logical systems codify parts of the logical consequence relation. Stronger logical systems codify a larger part of it than weaker systems. This is no problem from the Deductivist point of view, as stressed before in this chapter. That a system codifies all logical consequences of the language that it is formulated in need not be part of a properly Deductivist view. To repeat a point that was made several times already: the crucial feature is that *only* logical consequence can be derived from given sentences, not that *all* are captured. Covering more logical consequences with a system is better than covering less, of course. If need be, however, the Deductivist can always resort to a stronger system. What matters is that the inferences are logical. This feature is preserved.[46]

8.5.2 Impredicativity

The inference rules for the second-order quantifiers, as introduced above are *impredicative* in the sense that they allow us to infer sentences from a quantified sentence that contain subformulae in the relevant place that themselves contain second-order

[46]It should be noted that the standard semantic approach is not affected by this problem, albeit only in the remarkable way that it does not have an effective system at all. All logics up the hierarchy are sound with respect to the standard semantics. Thus, our example sentence is a second-order validity of the standard semantics already. This is easy to see: the Gödel sentence of second-order arithmetic has to be true in all standard models that make the axioms true. Nothing changes by going up higher in the hierarchy.

quantifiers. An example for this is the inference:

$$\frac{\forall X(Xa \vee Fa)}{(\exists Y \exists x(Yx \supset x = a)) \vee Fa}\ \forall^2\text{-E}$$

The open sentence inserted for 'X' is $\ulcorner \exists Y \exists x(Yx \supset x = ...)\urcorner$. It contains the second-order quantifier '$\exists Y$'. For fully impredicative inference rules there is no limit to the complexity of the open sentences. Second-order quantifier rules that allow only the substitution of open sentences that do not contain second-order quantifiers themselves are called *predicative*.

There seem to be two worries concerning this impredicativity of the second-order quantifiers, both to be found in the writings of Dummett. The first is that the impredicative rules are not comprehensible, presumably because they do not establish a determinate meaning. In *Frege: Philosophy of Language* Dummett writes:

> To grasp *what* concept a given predicate stands for is to be aware of the conditions under which the predicate is true of an arbitrary object. These we evidently cannot know, in the case of a predicate which involves quantification over concepts, until we know what is the totality of concepts which constitutes the domain of such quantification: and so the vicious circle is as overt as it could be.[47]

Dummett considers second-order quantification unproblematic as long as it is predicative (read 'second-order' for Dummett's 'second-level' here):

> Expressions for second-level generality are deeply embedded in natural language, but are there always interpretable as ranging over a predicative

[47](Dummett, 1981), p. 540.

> totality; assertions of the existence of a property is taken as needing justification by production of an instance. Second-level quantification is immune to question so long as it remains predicative [...].[48]

However later, in his *Frege: Philosophy of Mathematics*, Dummett's concerns are not only whether it is possible to *understand* impredicative second-order quantifiers. He considers impredicative second-order quantification to be inconsistent, and blames it "with a little help of Basic Law V"[49] for the inconsistency for Frege's system of the *Grundgesetze* in which Russell's Paradox can be derived (see chapter 3 above for a brief treatment of Russell's Paradox). George Boolos[50] and Crispin Wright[51] argue against this latter view of Dummett's. Boolos remarks that, against Dummett's contentions, a different source for the inconsistency of Frege's system should be identified: "the culprit is the obvious one, Basic Law V."[52]

This is not the place to rehearse this debate.[53] I take it that there are generally no real doubts about the consistency of even impredicative second-order logic. From the Deductivist point of a view the soundness proof with respect to the standard semantics can, of course, not be appealed to, while typically it is taken to show that second-order logic is consistent if the fragment of first-order ZFC that is needed for the semantics is consistent. There are other semantics with respect to which the deductive system of second-order logic is sound, like Henkin or topological semantics.[54] All of these are mathematical interpretations, however. Maybe more acceptable to

[48](Dummett, 1981), p. 541.
[49](Dummett, 1991a), p. 219.
[50](Boolos, 1993).
[51](Wright, 1998).
[52](Boolos, 1993), p. 221.
[53]An illuminating formal discussion of Dummett's view on impredicativity in a more general setting can be found (Weir, 1998).
[54]See (Awodey and Butz, 2000).

the Deductivist for this purposes a cut-elimination theorem for second-order logic, like the theorem proven by Dag Prawitz.[55]

It is anyway clear that the consistency of a system can only ever be proved in another system that is stronger. This is indicated by Gödel's incompleteness proof. According to Gödel's incompleteness theorems, no consistent system that includes arithmetic can prove its own consistency. This does not mean that consistency proofs are uninformative in general. Doubts about higher-order quantification, as already mentioned in the paragraph above, might be soothed by an interpretation in first-order set theory. Another example which is commonly cited as particularly illuminating in this context is Gentzen's consistency proof for arithmetic.[56] A consistency proof that is given in a familiar, trusted system for a new and unfamiliar one, certainly can count as an interesting and reassuring result.

Generally, however, the Deductivist account does not seem to have room, or need, for consistency proofs, in a way. The point here is that the Deductivist conception of logic sets in at the very lowest level. There can be no more secure a

[55](Prawitz, 1964); see also (Prawitz, 1968). Cut-elemination typically entails consistency, and is accepted by constructivists as showing that a proof-theoretic system is in good standing. Such a proof should thus also be "safer" than, say, a model-theoretic interpretation. Cut-elimination is roughly the idea that proofs can be "normalised" in the sense that applications of an introduction-rule for a logical constant that are followed by the corresponding elimination-rule can be eliminated from the proof. A proof can be shown to "make no detours" in this way. The cut-elimination theorem for a system says that every proof that is carried out in it can be normalised in this way. (Also (Hacking, 1979) insists on the importance of cut-elimination proofs.) Cut-elimination proofs for impredicative second-order logic are usually believed to be unavailable by constructively acceptable means; indeed, Prawitz' proof is not constructive insofar as it makes use of impredicativity in the meta-theory (classical negation is not used, however). (Zucker, 1980), p. 637, points out that it is a common mistake to assume that cut-elimination cannot be proven constructively. This error, Zucker suggest, is a result of confusing pure higher-order logic with higher-order number theory; a constructive cut-elimination proof for the latter is indeed not known. He refers to a comparatively "trivial" constructive proof for impredicative second-order logic that is given by Feferman in his review article (Feferman, 1977) of (Takeuti, 1975); Takeuti makes a potentially misleading statement to this effect. I am indebted to William Stirton for pointing me to Zucker's review article.

[56](Gentzen, 1936a).

system than proper logic. The demand for a formal proof of the consistency of a system of logic cannot be met: Any system that could deliver such a proof would have to use a formal logic, too. Logical consequence is characterised in deductive systems. I argued above that these systems are in one way or other constructed to codify logical inference, and that beyond our considered judgement, guided by the criteria for genuinely logical inferences as described above, no guarantee of safety is available. The basic inferences are, to our best judgement, properly logical. Recall in this context also Warren Goldfarb's interpretation of Frege's conception of logic:

> I believe Frege would reject the idea that inference rules rest on [...] principles expressing their soundness. Rather, our appreciation of the validity of the rules is [...] manifest in our use of [them], [...] in our making one assertion on the basis of another in accordance with the inference rule. There is nothing more to be made explicit [...].[57]

Even the father of model theory, Alfred Tarski, expresses the idea that logic is the basic discipline:

> [L]ogic itself does not presuppose any preceding discipline [...].[58]

Presumably it will suffice to dispel worries about the consistency of the impredicative second-order quantifiers to give a satisfactory answer to the first of Dummett's worries, the one concerning whether they have a determined meaning. If it can be argued that the impredicative second-order quantifiers have a determinate (non-trivial) meaning and that they are properly logical, this entails that second-order logic is consistent. As it seems indeed important for the Deductivist (as for any)

[57](Goldfarb, 2001), p. 38.
[58](Tarski, 1946), p. 119.

account that the quantifiers can be understood, Dummett's first worry has to be addressed in any case.

A philosophically satisfactory account of impredicativity would require an entire research project on its own – at least. I cannot do justice to the question here. A few more or less general remarks will have to suffice for the present purpose.

Retracting

One possibility would be, of course, to take Dummett's complaints (once they are sufficiently clarified) to be evidence that our judgements about the introduction- and elimination rules of the second-order quantifiers being impredicative were wrong, and that indeed only predicative rules can be allowed. This would leave us with predicative second-order logic, which is rather weak compared to fully impredicative second-order logic.[59] This alone would be no objection to such a step. Should it turn out that only the predicative rules for the second-order quantifiers reliably produce properly logical consequences, this would not be cancelled out in some way by considerations about the usefulness of the impredicative rules.

A further restriction that would presumably avoid the charge would be to restrict the rules to ramified instances. Bertrand Russell introduced the Ramified Theory of Types after his discovery of the contradiction in Frege's system of the *Grundgesetze*. Russell, like Dummett, considered the impredicativity involved to be the source of the paradox, and strived to ban all impredicativity from his system.[60] Not only did he introduce the type structure, but he also introduced what he called 'ramification'. In each type, the quantifiers of the first level ('level' here is not to be confused with

[59]Such a system is proposed, for example, in (Cocchiarella, 1974); see also (Church, 1956), §58.
[60](Russell, 1908), (Whitehead and Russell, 1913); see also (Goldfarb, 1989).

the level of predicates as it was used before – those would be types for Russell) have to be predicative. These level-one sentences then can be subject to level-two quantification, and so on. This ramification hampers the applicability of the system significantly, so that Russell notoriously had to introduce an Axiom of Reducibility that in effect cancels out the restriction of the ramification.

If we think as the variables of Type Theory as ranging over classes, the ramifications would demand that for the definition of a class at a type, only classes that were already defined at a lower ramification level of that type can be used. On such an interpretation the Axiom of Reducibility asserts that, in fact, all classes that are defined along the ramification levels already exist at level 0. Adopting a ramified second-order logic,[61] again, would give us a rather weak system. As for predicative second-order logic, that is not in itself a reason to reject the proposal, however.[62]

Generality

One presupposition of quantification was that it is a device of generality for the predicate position. I argued that open sentences fulfill the same syntactic role as predicates that enter the formal language as predicate constants. It does not seem to matter whether predicates have logical structure themselves or not. Restricting this generality seems to contradict one of the basic building blocks of the Deductivist approach: the understanding of the inference rules as fully schematic.

To achieve a sufficiently general notion and avoid the problems of the standard substitutional account, it had to be insisted that the rules hold for *any* extension

[61] See (Church, 1956), §58, for a formal system of ramified second-order logic; such a system is also sketched in (Shapiro, 1991), pp. 64, 69–70.

[62] As Prawitz, for example, remarks, "[f]rom a strictly intuitionistic view-point, probably only ramified predicate logic is meaningful." (Prawitz, 1965), p. 66, fn. 1.

of the language. If this does not hold, then the inferences according to the rules in question do not count as cases of logical consequence. I cited the case of an explicit definition. A new predicate defined on the basis of some predicates that are already in the language must not be barred from getting into the inference rules.

This seems to be the key thought that needs to be developed to find an answer to the impredicativity challenge. If Dummett is right, then the very introduction of the impredicative second-order quantifiers extends the language with expression for which the rules do not hold anymore. Can it be argued against Dummett that our acceptance of the rules covers the case, so to speak, when we realise that there are open sentences that contain the second-order quantifiers to which the second-order quantifiers apply?

Consider the case of the rules for conjunction. We will find that we apply conjunction introduction to sentences that themselves are conjunctions of further sentences. This is certainly innocuous. So, what is the relevant difference? Dummett mentions the range of the quantifiers. On a Dummettian understanding of quantification, it makes only determinate sense to quantify, if the range of the quantifiers is fixed. For the realist, impredicativity thus poses no problem. The functions or concepts that the quantifiers range over in the realist picture exist independently of human activity. For an anti-realist like Dummett, functions have to be constructed in some way. Putting these two Dummettian views together, we do not have a fixed range for the quantifiers if that range has to contain expressions that are only defined with the use of the quantifiers that are supposed to range over them.

The Deductivist approach was designed to be neutral on metaphysical questions. It construes quantification as quasi-substitutional, thus the quantifiers do not, strictly speaking, *have* a range. The new criterion of ontological commitment that the approach uses, for example, describes the kinds of entities that a theory is committed two, but stays neutral on the question whether *kinds* as such are entities (universals, sets, ...), or whether maybe the talk of kinds is just a sloppy way of saying that the theory proves that this-and-that predicate (the "kind") is true of something. Interpreted in a Quinean fashion, this would not commit us to anything beyond the objects. This kind of neutrality should be preserved in the response to Dummett, too, but of course without begging the question against him, by simply stating that the quantifiers do not have a range.

Two Possible Ways Out

Maybe a heuristic will help. Our understanding of impredicative second-order quantifiers might come in stages. We recognise the rule as codifying logical consequence as long as we only have predicative instances. Once these are thus secured it would also be "safe" to have open sentences containing predicative quantifiers as instances of second-order quantifiers.[63] It is clear where this is going: it reconstructs (on a heuristic level) Russell's ramifications. Ridding ourselves of the heuristic eventually, would be parallel to Russell's adopting of the Axiom of Reducibility. All the impredicativity banning measures are abandoned and we convince ourselves that, *really*, we have been quantifying unrestrictedly across all ramification levels all along. Also the second-order quantifiers we started out with, and that were thought of as predicative, are fully impredicative quantifiers. Trying to argue that way would invite criticisms similar to those that were raised by Frank Ramsey against Russell for adopting the Axiom of Reducibility.[64] It might seem like a cheat. This impression

[63]This was suggested by Crispin Wright in discussion during an *Arché* research seminar in April 2004.

[64]Compare (Ramsey, 1925).

would have to be explained away.

A more direct strategy might be better. It could be argued that the schematic rules are in fact accepted. As they are *fully* schematic, the extensions of the language that the impredicative quantifiers themselves bring are also accepted as admissible instances. This is what we committed ourselves to when we accepted the quantifiers in the first place. This approach is probably best dubbed "digging in the heels". Without further arguments it is little more than mere insistence that the idea of a fully schematic rule brings with it that instances can be impredicative.

It can be noted, however, that something analogous happens if we instantiate first-order quantifiers with definite descriptions, or form conjunctions from conjunctions. A line of argument might start from the observation that the intuitionist explanation – provided by Heyting – of the conditional involves the notion of a proof: '$p \supset q$' is explained as saying that a proof of 'p' can be extended to a proof of 'q'. The proof for 'p', however, might in turn itself involve conditionals. We encounter also here a certain impredicativity.[65] A normalisation (or cut-elimination) proof is sometimes taken to guarantee that this his harmless. Such a proof exists also for impredicative second-order logic, however, as mentioned above.

Maybe, however, the case should be made that the onus is on Dummett to convince the Deductivist that quantification only makes sense if there is a definite range for the quantifier. Ranges of quantifiers, after all, are not part of the core Deductivist agenda. The challenge to the challenger is to provide an example of a sentence that is declared a logical truth by the deductive system of impredicative second-order logic (i.e., that is a theorem of it) that does not accord to the pre-systematic notion of logical consequence.

[65] Compare (Dummett, 1977), §7.2.

Chapter 9

Appendix: Etchemendy on Logical Consequence

9.1 Introduction

In his *The Concept of Logical Consequence* (Etchemendy, 1990) John Etchemendy attacks the Tarskian model-theoretic approach to logical consequence on various levels.[1] His complaint is that the standard model-theoretic definition of logical consequence is neither useful as a conceptual analysis, nor is it even extensionally adequate. With respect to second-order logic he specifically argues that the standard model-theoretic semantics "overgenerates" in the sense that it provides too many allegedly logical consequences and validities.[2] I will present his criticisms in

[1] Indeed, his attack had started already in a couple of earlier papers, compare (Etchemendy, 1988a) and (Etchemendy, 1988b).

[2] (Etchemendy, 1990), pp. 123–124.

sections 9.3 and 9.4 below. These broadly fit with the observations that were made in chapter 6 of this thesis.

It is often believed that Etchemendy rejects model-theoretic techniques altogether. The reason for this belief presumably is, as Etchemendy states in his as yet unpublished (but freely available on his internet website) (Etchemendy, 1999)[3], that the positive part of his programme is largely missing from his book. Indeed, what Etchemendy claims here, is that his criticism of the Tarskian approach

> is not aimed at model-theoretic techniques, properly understood, nor at the view that logical consequence is a fundamentally semantic, not syntactic notion.[4]

Even without recourse to Etchemendy's unpublished paper it is obvious from his other publications that he favours a semantic approach,[5] and it is also not too difficult to get that idea from (Etchemendy, 1990). It is clear from Etchemendy's distinction between representational and interpretation semantics, and his descriptions and evaluations of the two conceptions, that he takes the representational account to be the correct one. This will be discussed in section 9.5.

I argued in chapter 8 that the Deductivist approach is better suited to characterise logical consequence than the model theoretic account. Etchemendy, while rejecting the Tarskian approach to logical consequence, is still opposed to a "purely syntactic approach".[6] His rejection of such an approach is even more acerb than his rejection of Tarski – and this too could be taken as a hint that his own sympathies lie

[3] I am indebted to Professor Etchemendy for granting me permission to quote his unpublished paper.

[4] (Etchemendy, 1999), p. 2.

[5] See for example (Barwise and Etchemendy, 1995).

[6] (Etchemendy, 1990), p. 2.

with a semantic approach. His rejection of syntactic approaches is also rather swift and sketchy, as he only uses this rejection as a platform for his thorough discussion of the Tarskian approach. As indicated in chapter 8, I agree with his criticism of such positions. Moreover, the Deductivist approach as characterised above does not qualify as *purely* syntactical, and thus, as argued in chapter 8, the criticism does not apply. In section 9.6 I compare Etchemendy's representational semantics with the Deductivist account on the one hand, and the standard semantical approach, as described in chapter 7 above, on the other.

Note that Etchemendy prefers to talk about logical truth instead of logical consequence throughout the book. He takes the concept of logical consequence to be the important one, however:

> Logic is not the study of a body of trivial truths; it is the study of the relation that makes deductive reasoning possible.[7]

At least we agree about that much. The issue will come up again in section 9.6.

9.2 Etchemendy's Strategy

Etchemendy offers arguments that are supposed to illuminate why two allegedly compelling reasons to adopt Tarski's account do not work, and do not motivate the Tarskian account at all. Etchemendy then proceeds in the second half of his book by arguing that the account should be rejected altogether. He claims that the two reasons that make the Tarskian analysis appear attractive are:[8]

[7] (Etchemendy, 1990), p. 11.
[8] See (Etchemendy, 1990), p. 95.

- Appearance of having the virtues of representational semantics
- "Tarski's Fallacy"

"Tarski's Fallacy" has been widely discussed.[9] The idea here is the following: The appeal of Tarski's account is that it gives the prospect of capturing the pre-systematic intuition that the truth of the premises of an argument, in some sense, guarantees the truth of the conclusion. It is not the case that the conclusion just happens to be true; the premises, if they are true, in some sense force the conclusion to also be true.[10] According to Etchemendy, Tarski promises this with the following sentence:

> [I]t can be proved, on the basis of this [Tarski's] definition, that every consequence of true sentences must be true.[11]

Etchemendy argues that this sentences is ambiguous between a wide-scope and a narrow-scope reading of the necessity-operator "must":

(W) *Necessarily*, if C is a Tarski-consequence of Γ, then: If every sentence in Γ is true, then C is true.

(S) If C is a Tarski-consequence of Γ, then: *Necessarily*, if every sentence in Γ is true, then C is true.

Etchemendy argues that the stronger claim (S) is the appealing one, but the only one that can really be argued for is the weaker (W). (W) has the logical form

[9] See, for example, (Ray, 1996), (Shapiro, 1998), and (Sher, 1996). Etchemendy's point of view is defended, especially against (Ray, 1996), by (Hanson, 1999).

[10] See (Etchemendy, 1990), ch. 6, esp. pp. 82–83.

[11] (Tarski, 1983), p. 417; see (Etchemendy, 1990), p. 91. The new translation (Tarski, 2003), p. 186–187, does not disagree in the relevant aspect.

$\ulcorner\Box(p \supset (q \supset r))\urcorner$, while (S) is of the form $\ulcorner(p \supset \Box(q \supset r))\urcorner$. Obviously, (W) does not imply (S) – and it is a fallacy to conflate these two. The promise is therefore empty and cannot serve as a good reason to accept Tarski's account.

Much of the discussion was generated by Etchemendy's remarks in the concluding section of his chapter on "Tarski's Fallacy".[12] Here, Etchemendy gives the impression that Tarski must have either committed an elementary and rather blunt modal fallacy, or (maybe even consciously) deceived his readers. I take the discussion, whether "Tarski's Fallacy" is indeed a fallacy that does not allow for a more charitable reading, and if so, whether it was deception or a mistake, to be rather exegetical and tangential to my project.

For what it is worth, let us take a look at the German passage:

> So kann man insbesondere auf Grund dieser Definition beweisen, daß jede Folgerung aus lauter wahren Aussagen wahr sein muß.[13]

There is a more charitable interpretation of this passage. The 'muß' ['must'] might well have been added merely for emphasis, and not intended by Tarski to indicate a claim of a necessary connection. It is probably ever more common in German than it is an English to add 'muß' as an auxiliary verb to a sentence merely to emphasis the strength of ones own belief in the statement. This would entail that Tarski was writing sloppily, which does not seem too likely – but maybe more likely than him committing a modal fallacy or (perhaps active) deception.

[12](Etchemendy, 1990), pp. 90–94. Interesting here, as both Stephen Read (Read, 1994) and Peter Simons (Simons, 1987) point out, is that Etchemendy calling this fallacy "Bolzano's Fallacy" in his earlier publication (Etchemendy, 1983), p. 330, did not create an uproar like the one that followed Etchemendy's announcing it as "Tarski's Fallacy".

[13](Tarski, 1936b), p. 9.

The historical and exegetical questions concerning Tarski are of no real interest here, however, and I will not go deeper into the details. Thorough historical and exegetical analyses can be found in (Gómez-Torrente, 1996) and the extensive translators' introduction by Madga Stroińska and David Hitchcock to (Tarski, 2003). For the purpose of this discussion I agree with Etchemendy that

> the important issue is not what Tarski was thinking when he wrote the paper, but whether the account he proposed is correct.[14]

The reasons that Etchemendy gives for rejecting Tarski's account are its alleged *conceptual* and *extensional inadequacy*. I will discuss these two inadequacy allegations in the following two sections.

9.3 Conceptual Inadequacy

Tarski's account of logical consequence is in all respects relevant for the discussion here sufficiently similar to the standard model-theoretic semantics introduced in chapter 2. A sentence is a logical truth, according to this account, if it is satisfied by all models. What Etchemendy calls the *conceptual inadequacy* of Tarski's *reductive* account has to do with the reduction of the modal concept of *logical truth* – logical truths are *necessary* – to "ordinary" truth, statements about how the world actually *is*.[15] To see what Etchemendy is intending, consider the following sentences:

(1) Lincoln was president.

(2) $\forall x \forall P[x\ P]$

[14](Etchemendy, 1999), pp. 1–2.

[15]See (Etchemendy, 1990), pp. 95–100.

To figure out whether (1) is a logical truth, we can just consider its universal closure (2). In (2) all expressions of (1) that are not in a class of fixed terms $\mathfrak{F}$ have been replaced by variables of the right type and these are bound by prenex universal quantifiers. The fixed terms $\mathfrak{F}$ have to be determined in some way in advance, and Etchemendy takes issue with this, too, but for now we can just take for granted that they are the usual logical constants: truth-functional connectives, universal and existential quantifiers, and the identity sign.

Now Etchemendy states that according to Tarski's account sentences like (1) are logical truths if their universal closure is true – this is what according to Etchemendy satisfaction by all models (in modern model-theoretic semantics; for Tarski it was satisfaction by all sequences) comes down to.[16] To elucidate what he diagnoses as the mistake in the analysis, Etchemendy presents three principles:

(*i*) If a universally quantified sentence is *true*, then all of its instances are *true* as well.

(*ii*) If a universally quantified sentence is *logically true*, then all of its instances are *logically true* as well.

(*iii*) If a universally quantified sentence is *true*, then all of its instances are *logically true*.

(*i*) and (*ii*) are uncontroversial, they are simply statements of the rule of *universal instantiation* and of the *closure principle*, respectively. (*iii*) is the *reduction principle* that Etchemendy attributes to Tarski. In this principle he sees both the attraction and the failure of Tarski's account:

[16](Etchemendy, 1990), p. 97.

> This is an important selling point for Tarski's account. Our ordinary concepts of logical truth and logical consequence involve various notions that are notoriously difficult to pin down, notions like necessity, *a priori*, analyticity, and so forth. But if [Tarski's] account is correct, what it achieves is a truly remarkable reduction of obscure notions to mathematically tractable ones. If it is right, the analysis shows that we can in fact sidestep all of these difficult concepts, that we can give a mathematically precise definition of the *logical* truths of a language if we can just define the notion of *truth* for a slightly expanded language – or, what comes to the same thing, if we can define the notion of truth relative to an arbitrary interpretation [...].[17]

The mention of a slightly expanded language, of course, refers to the fact that the quantifiers and variables we need to form the universal closures, like (2), might not have been in the original language.

The remarkable advantage of the reductive account is unfortunately unavailable, since (*iii*) "is simply false":[18] the mere truth of a universally quantified sentence cannot guarantee the logical truth of its instances. The universal sentence might just be accidentally, or historically, true, or a truth of physics or mathematics. In those cases its instances would not be *logically* true, but, presumably, historically, or physically, or mathematically. Etchemendy does not go into more detail concerning the general acceptability of the principle, since it seems so obviously false. He considers two ways, however, in which the analysis might be rescued by modifying principle (*iii*) to make more moderate claims. He argues that these cannot be

[17] (Etchemendy, 1990), p. 99.
[18] (Etchemendy, 1990), p. 100.

defended either.

The first modification is to construe the account as giving an analysis of an irreducibly relational notion, viz. logical truth with respect to an arbitrarily specified class $\mathfrak{F}$ of fixed terms. Principle (*iii*) modified in such a way to accommodate this reads:

(iii') If a universally quantified sentence is *true*, then all of its instances are *logically true with respect to those expressions not bound by the initial universal quantifiers.*[19]

It seems quite unintuitive to speak of *logical* truth in such a case. Does it even make sense to speak of logical truth with respect to some expressions? Etchemendy suggests that the most natural way to think of the notion analysed in (iii') as that of *analytic truth*:[20] sentences that come out as "logically true" according to it, seem to be true solely in virtue of the meanings of the terms that do not get bound by initial quantifiers in the closure of the sentence, i.e. those terms that are in $\mathfrak{F}$. Think of $\mathfrak{F}$ as containing the truth-functional connectives and 'is a man' as well as 'is a bachelor'. The sentence

(3) John is a man or John is not a bachelor.

comes out as "logically" (= analytically) true according to (iii') because its closure

(4) $\forall x$[x is a man or x is not a bachelor]

is true. If this was a successful analysis, and we were, additionally, in position to specify a class $\mathfrak{F}$ of genuinely logical expressions, one might argue that this taken

[19](Etchemendy, 1990), p. 101.
[20](Etchemendy, 1990), p. 103.

together would indeed give a successful analysis of the concept of logical truth. Fixing $\mathfrak{F}$ to contain all and only logical expressions would yield that the sentences declared logically true by (*iii*′) are true solely in virtue of meaning of the logical vocabulary they contain. This is, of course, a traditionally accepted definition of 'logical truth'.

Etchemendy argues, however, that (*iii*′) does not give a satisfactory account of analytical truth. Analytical truth, as stated above, requires that the sentences are true *solely* in virtue of meaning of the terms involved, but (*iii*′) requires the world, as it *actually* is, to cooperate. To see this, consider the sentence

(5) Leslie was a man or Leslie was not a US president.

If we keep the predicates and the truth-functional connectives fixed, the closure of the sentence comes out true, despite it not being true in virtue of the meanings of the fixed terms. It is an historical accident that the USA (as of now) has had no female president; it is not true solely in virtue of the meaning of 'was a US president'. The world might have been different in such a way that a woman was elected into the office, and then (5) would not come out analytically true according to (*iii*′). Such a dependence on how the world happens to be, however, must not influence whether a sentence is analytically true or not.

Etchemendy concludes that (*iii*′) is conceptually inadequate, in that "it does not capture, or even come close to capturing, the ordinary concept of logical truth."[21] Whether the analysis delivers the right result depends on how the world actually is, and therefore can deliver neither the analyticity that is usually demanded for logical truth, nor, for that matter, its necessity or aprioricity.

[21](Etchemendy, 1990), p. 108.

The second modification of (*iii*) that Etchemendy considers, therefore, builds on the idea that certainly seemed to be the obvious fix all along: the restriction of $\mathfrak{F}$ to distinctively logical terms. Taking it for granted – for the moment – that we can determine what "distinctively logical" terms are, the second modification of (*iii*) is:

(iii'') If a universally quantified sentence is true, *and the constant expressions appearing in its matrix*[22] *are of a distinctively logical sort*, then all of its instances are logically true.[23]

But despite its *prima facie* intuitively correct appearance, Etchemendy argues, "the account remains dependent on completely nonlogical facts [...] no matter how narrowly we construe the notion of a "logical" expression".[24] The most obvious example, according to Etchemendy, has to do with the size of the universe. Consider the following sequence of sentences:

σ_2: $\exists x \exists y(x \neq y)$

σ_3: $\exists x \exists y \exists z(x \neq y \wedge y \neq z \wedge x \neq z)$

$\vdots$

Each sentence σ_n expresses that at least n things exist. None of these should come out as logical truths, but, according to (iii'') some of them will, since, assuming that the existential quantifier, the truth-functional connectives and the identity sign are distinctively logical terms, some of their (trivial) closures:

[22]The *matrix* of a universal closure $\ulcorner \forall x_1 \ldots \forall x_n[S'] \urcorner$ is the sentential function $\ulcorner S' \urcorner$; see (Etchemendy, 1990), p. 168, n. 8.1.
[23](Etchemendy, 1990), p. 110.
[24](Etchemendy, 1990), p. 110.

$[\sigma_2]$: $[\exists x \exists y (x \neq y)]$

$[\sigma_3]$: $[\exists x \exists y \exists z (x \neq y \wedge y \neq z \wedge x \neq z)]$

$$\vdots$$

clearly, are in fact true. (Since the original sentences do not contain any non-logical vocabulary, no terms need to be replaced by variables and bound by prenex universal quantifiers in order to construct their "universal" closures.) Should the universe be infinite, all of them come out true. As is well known, this defect is easily fixed in modern model theory by varying the size of the domain[25], which corresponds to varying the interpretation of '$\exists$' in Etchemendy's presentation of Tarski's account. But, straightforwardly, such variations can only *restrict* the range of the quantifier. If the universe were finite,[26] still some of the *negations* of the sentences σ_2, σ_3, ... would come out as logically true, again depending on the actual size of the universe. To overcome this problem, too, standard model theory assumes the axiom of infinity in the semantics.

And thus, Etchemendy concludes, it becomes clear how we simply used a series of "tricks" to achieve the extensional adequacy of the account.[27] Nothing *inside* the account guarantees that the "right" sentences, i.e. those which *are* logical truths, are declared as such. On the standard semantics, the guarantee comes from the set-theoretical axiom of infinity in the typically used Zermelo-Fraenkel set theory – should we use instead a set theory that does not have that axiom, this guarantee

[25]For this this and other "fixes" of Tarski's account see (McGee, 1992). (Priest, 1995) and (Shapiro, 1998) suggest ways to amend the Tarskian account that, they argue, are not as unnatural and arbitrary as Etchemendy claims any mending must be. See also (Shapiro, 2005b), (Priest, 1999), and chapter 11 of (Priest, 2006).

[26]The point here is of course not, that finitism is true, but rather that it is not *logically* false. The size of the universe is not a matter of pure logic, but a substantive fact.

[27]See (Etchemendy, 1990), pp. 114–116.

would vanish. It is therefore a mathematical principle, or a fact about the how the world actually is (it would have to be infinite), that makes the account extensionally correct; it is not delivered by the analysis.

Again, Etchemendy concludes, we can see that Tarski's account fails to be conceptually adequate. Either the world as it actually is has to cooperate to make the account deliver the right results, or else it depends on mathematical principles. Neither speaks for the conceptual adequacy of the reductive analysis; logical truth is not supposed to depend on mathematical truth or the size of the universe – or more generally, the way the world is.

To insist that the mere possibility of an infinite universe is sufficient to deliver the right result,[28] does not come as a rescue, as Etchemendy points out.

> Indeed, this is simply another way of saying that the [universal closures], though perhaps *actually* true, *could* have been false. [...] In other words, we are simply observing that [they], even if true, [are] not *necessarily* true.[29]

This, however, is the whole point. To argue in that way is to say that neither the reduction principle (*iii*), nor any of its modifications, give the right analysis. To insist on the necessity of the universal closure, means to fall back on principle (*ii*), as given above.[30] This principle certainly holds, but does not offer a reductive analysis: The status of a sentence as a logical truth, cannot be reduced in an interesting way to its universal closure being a logical truth. If we had figured out how to determine the latter, this would presumably also yield the result for the former.

[28](Priest, 1995), (Schurz, 1994), and (Sher, 1996), e.g., argue in that way.
[29](Etchemendy, 1990), p. 120.
[30]See also (Etchemendy, 1990), p. 143.

9.4 Extensional Inadequacy

It might still seem that with some help from the outside, the standard model theory can deliver the right result extensionally.[31] Etchemendy, however, poses problems for this, too. On the one hand, he argues that extensional adequacy can only be achieved for rather weak languages. For stronger languages, e.g. second-order languages, this does not appear to be possible, since the validities outrun what can reasonably be called logical truths. Etchemendy calls this the problem of *overgeneration* which I will deal with first. The other problem is dubbed *undergeneration*, and is mainly concerned with the lack of any guarantee that we have actually captured all of the logical truths of a language.

As the prime case of overgeneration, Etchemendy cites the notorious case of second-order logic and the continuum hypothesis.[32] (The continuum hypothesis is discussed in detail in my chapter 6 above.) The language of pure second-order logic with the standard semantics has enough expressive resources to express statements about transfinite cardinalities. Amongst others the continuum hypothesis can be expressed. The continuum hypothesis states that the cardinality of the continuum, which is the cardinality of the set of the real numbers, is the least cardinality greater than that of the set of the natural numbers.

Both the continuum hypothesis and its negation are consistent with the axioms of Zermelo-Fraenkel set theory (ZF). That means that the continuum hypothesis is independent of ZF, an open problem of set theory. Hence, it seems, that the continuum hypothesis is a substantial mathematical truth (or falsehood as it might

[31] (Shapiro, 1998) argues in response to Etchemendy that this can be done, and shows in detail how he takes it to be achievable.

[32] Etchemendy refers to this in several passages, e.g. pp. 127, 132, 154, 158; the main exposition is on pp. 123-124.

be) if there are any, and not a logical one.[33] Yet, the pure second-order sentence that corresponds to its truth is a validity of the standard model theory if, and only if, the continuum hypothesis is true; it would be true in all models. It being true all models, however, means that it comes out as a *logical* truth, while it clearly should not.

Etchemendy contends that the frequent claim that this shows that second-order logic just is set theory in disguise, is a misdiagnosis of the situation. It merely shows that the standard model theory *overgenerates*. It declares some sentences to be logical truths that are not and this is due to the "faulty account of the logical properties". The continuum hypothesis is just another example of a sentence that is declared logically true by the reductive account because its (trivial)[34] closure happens to be true – true in the set-theoretic universe whose principles are used to formulate the model theory. A similar result, spelled out in more detail, was reached in chapter 6 above.

The second problem of the extensional adequacy that Etchemendy outlines focuses on the lack of any guarantee that we have indeed captured all of the logical truths of a given language.

> If our aim is to characterize the set of logical truths (or the logical consequence relation) for an antecedently given first-order language, then there is no general way, short of fixing all of the expressions in the language, to guarantee that the model theory captures them all. Indeed, once we focus on any interesting first-order language, such as the language of elementary arithmetic, it seems clear that the standard model

[33] See also (Etchemendy, 1990), p. 82.

[34] There is no non-logical terminology involved in its formulation, and hence no terms have to be replaced by variables and bound by prenex quantifiers.

> theory does undergenerate. It is only our uncritical adoption of the model-theoretic analysis that has obscured this simple point.[35]

Etchemendy explains the undergeneration of the model theory for elementary arithmetic:

> When we apply the standard, interpretational semantics to this language, our specification of the logical properties falls short of their genuine extension – as Tarski himself, indirectly, pointed out. For example, instances of the ω-rule [...] are wrongly judged invalid. Of course, it is easy to see *why* these instances are not declared valid – their validity depends on the meanings of various expressions not held fixed – but this does not give us a solution to our problem, namely, specifying the genuine extent of the consequence relation for the given language. If this is our goal, Tarski's account does not, in general, allow us to steer a course between the complementary hazards of over- and undergeneration.[36]

The point that Etchemendy is concerned about is not only that there is no *general* way to make sure that the account does not undergenerate. The deeper concern is that there is nothing *internal* to the account from which we can decide whether our result is the right one. Since there is no general way to prevent undergeneration, we have, in order to get the "help" from, e.g. the set theoretic principles in the model theory, to know in advance what the logical truths are, so that we can tinker with the model theory in the right way, in order to get the extension right.[37] For this, we then need an *external justification* that we got the extension right, for there is

[35](Etchemendy, 1990), p. 151.
[36](Etchemendy, 1990), pp. 132–133.
[37]See e.g. (Etchemendy, 1990), p. 93.

nothing within the account that allows one to decide whether the goal is met.[38]

The conclusion that Etchemendy draws is that while we already had to give up the hope of the conceptual adequacy of the Tarskian account (see the previous section), it is only in some weak languages that we can gain even extensional adequacy. Not even here, however, do we have a general method to achieve that, let alone to do so with the resources provided by the account itself – external resources are needed to achieve this minimal success.[39]

9.5 Representational Semantics

As already mentioned, Etchemendy claims that one of the features that makes Tarski's reductive analysis and other interpretational accounts like the standard model theory seem so attractive, is that it appears to have the virtues of a *representational semantics*.[40] In his later paper Etchemendy emphasises:

> It is important to understand that my rejection of the reductive analysis of logical consequence is not an attack on model theory or model-theoretic semantics *per se*, but rather on a particular view of these techniques.[41]

The reader, unfortunately, does not get much of an idea of the details of such semantics from (Etchemendy, 1990). Etchemendy is more explicit his the later paper, however.

[38] See e.g. (Etchemendy, 1990), pp. 131, 133, 141–142.
[39] (Etchemendy, 1990), pp. 157–158.
[40] See (Etchemendy, 1990), p. 95.
[41] (Etchemendy, 1999), p. 24.

> The guiding idea of the representational view of model theory is simple, and in fact widely held, though not widely articulated. [...] The set-theoretic structures that we construct in giving a model-theoretic semantics are meant to be mathematical models of logically possible ways the world, or relevant portions of the world, might be or might have been. They are mathematical models in a sense quite similar to the mathematical models used study, say, the possible effects of carbon dioxide in the atmosphere, only they are used to study semantic phenomena, not atmospheric [...]. I call this view of model theory "representational" because the set-theoretic structures are seen as full-fledged representations: models of the world.[42]

From the technical point of view, a representational semantics does not seem to differ much from the standard model theory.[43] The difference seems to be largely in the use of the standard model theory. The idea can be spelled out roughly as follows.[44] If we want to figure out the truth-value of a sentence, say, 'Snow is white' we have to pay attention to two things, the world and the language. That 'Snow is white' is true is due to the world being such that it is true, and due to the sentence meaning what it does. Had the world been different, say, in that snow had been blue, the sentence would have been false. In the world as it actually is, had 'Snow is white' meant that grass is purple, the sentence also would have been false.

[42](Etchemendy, 1999), pp. 25-26.

[43]Although it has to be qualified that this is only part of the picture: Etchemendy also wants to include, e.g., reasoning with diagrams into his account; see (Etchemendy, 1999), pp. 22–23, 35–36; see also (Barwise and Etchemendy, 1995). I will in the following for simplicity's sake restrict my considerations to the linguistic fragment of Etchemendy's account.

[44]The exposition, unless stated otherwise, follows (Etchemendy, 1990), pp. 18–23 and 55–64.

Etchemendy considers ordinary truth tables, like the one following, as a simple example of his point:

p	q	$p \wedge q$
TRUE	TRUE	TRUE
TRUE	FALSE	FALSE
FALSE	TRUE	FALSE
FALSE	FALSE	FALSE

If we move from row to row in a truth-table, and so go through the different possible distributions of truth values of the atomic sentences, and the resulting truth values of the complex sentences they figure in, we have exactly those two ways of understanding what we are doing. We can either take ourselves to consider the sentences as having different meanings in different rows, such that they come out true or false according to how the world is; or we can interpret what we are doing as considering different ways the world might have been, holding the meaning of the sentences fixed. The former account is the interpretational one; the atomic sentences are re-interpreted from line to line. The Tarskian account and standard model theory are interpretational in this way. The sentences are re-interpreted in the models. The latter account on the other hand is representational; single rows in the truth-tables represent the world in different ways. The correct representation of the actual world is the one that declares those sentences true which say what actually is the case, and those false that do not.

It might seem that the difference between representational and interpretational semantics comes down merely to what we take ourselves to be doing when we are doing semantics, and that the result effectively comes out the same in both cases.

Etchemendy explains that it would be a mistake to think that the difference between the approaches is merely one of perspective, and that the re-interpretations of the language match the possible configurations of the world. Were it the case, the difference would indeed come down a mere question of perspective concerning what we think we are doing, when we are moving from one row in a truth table to another one. Etchemendy provides two examples where the approaches come apart.

First, he considers a Tarskian semantics where 'or' is not counted amongst the constants, and hence gets re-interpreted in the models as different truth functions. This is possible on the interpretational account, Etchemendy states, since certainly 'or' could have meant something different,

> [b]ut there is no plausible way of understanding, representationally, models in which 'or' is assigned, say, the truth function ordinarily expressed by 'and'. This is not to say that such models depict extremely bizarre "possible worlds", worlds we have difficulty conceiving. There is just no representational counterpart to such a Tarskian semantics.[45]

Etchemendy's second example exposes some of the philosophical background by which the representational account is driven. Consider the sentence '$2 + 2 = 4$'. Typically, on the standard account '2', '4', and '+' are not held fixed and so the sentence does not come out as a logical truth. (In arithmetic '2', '4', and '+' are constants, of course, not in pure logic, however.) The same reasoning as in the example before applies here again: Certainly, '2', '4', and '+' could have meant something different from what they usually mean. They could refer to the empty set, the singleton of the empty set and union, respectively, for example. Under such

[45] (Etchemendy, 1990), p. 62.

an interpretation '$2 + 2 = 4$' comes out false.

> There is no way to construe the model described as somehow representing a "possible world" in which two plus two does not equal four. That way madness lies: '$2 + 2 = 4$' might well have said something else [...]. What it says – that is, what it actually says – is, however, necessarily true.[46]

It becomes apparent that Etchemendy draws the line like this: Each expression of a language could have meant absolutely anything else (presumably, as long as it falls into the same semantic category). That means, interpretational semantics in principle can have a wide range of models that allow one to re-interpret every expression of the language. A model cannot be a representation of the world if it declares something false that just cannot be false, i.e. something that is necessarily true. The standard model theory, of course, distinguishes between logical constants and terms which are re-interpreted. We have already seen in the previous sections, however, that Etchemendy considers it arbitrary what is included in the class $\mathfrak{F}$ of "logical" constants.

The representational account obviously presupposes an account of necessity in order for it to work. And, indeed, Etchemendy does not see representational semantics as in any way illuminating logical or necessary truth.

> When we are doing representational semantics, we appeal to modal notions from the very outset, in assessing the adequacy of our class of models and our definition of truth in a model.[47]

Indeed, the representational semantics Etchemendy envisions does not seem to be able to distinguish between the two. It is important that in a representational

[46](Etchemendy, 1990), p. 62.
[47](Etchemendy, 1990), p. 99.

semantics the models

> represent all and only genuinly possible configurations of the world. [...] Certainly, all necessary truths of a language – of whatever ilk – should come out true in every model of a representational semantics. If they do not, this just shows that our semantics for the language is somehow defective [...]. But this is only a test of adequacy of the semantics, not a sign that we also have an analysis of necessary truth.[48]

This is, however, not considered to be a failure of representational semantics, but rather an intended feature. Etchemendy views representational semantics as a tool to study analytical and other necessary entailments and truth of a language – "of whatever ilk". The specifically *logical* truths and entailments play no special role, and

> it would clearly be wrong to view representational semantics as giving us an adequate analysis of the notion of logical truth. For one thing, if there are necessary truths that are not logically true, say, mathematical claims, then these will also come out true in all models of a representational semantics. More importantly, even if we are prepared to identify necessary truth with logical truth – an identification most people would balk at – it is still clear that representational semantics affords no net increase in the precision or mathematical tractability of this notion. Any obscurity attaching to the bare concept of necessary truth will reemerge when we try to decide whether our semantics really satisfies the representational guidelines – in particular, when we ask whether our models

[48] (Etchemendy, 1990), pp. 25–26.

represent all and only genuinly possible configurations of the world.

> The value of representational semantics does not lie in an analysis of the notions of logical truth and logical consequence, or in the analysis of necessity or analytic truth. Rather, what this approach gives us is a perspicuous framework for characterizing the semantic rules that govern our use of the language under investigation.[49]

Etchemendy reckons not only that the Tarskian reductive account failed, but also that reductive accounts generally have no chance of succeeding, if they purport to reduce the modal concept of logical truth in some way to "ordinary" truth. His own proposal to proceed by using representational semantics for logical studies – rather broadly construed[50] – is not a reductive account. Rather, the presupposed pre-systematic notions are to be represented in the models of the semantics (hence, representational semantics) and can then be studied with the usual[51] model-theoretic techniques. Etchmendy has also given up on a distinction between genuinely logical consequence and other forms of necessary entailment, including analyticity, mathematical entailment, geometric inference,[52] and others. So, while he considers it "perfectly obvious why an adequate representational semantics can yield necessary truths, and hence logical or analytical truths, insofar as these are species of those"[53], a distinction between the different species does not appear to be on the agenda.

The similarities between Shapiro's approach, as outlined in chapter 7 above, and Etchmendy's are apparent. Shapiro also presupposes a pre-systematic notion of

[49](Etchemendy, 1990), p. 25.
[50]See also my discussion of this broad construal in section 9.6 below.
[51]See footnote 43 above.
[52]See (Etchemendy, 1999), pp. 16–17, 35.
[53](Etchemendy, 1990), p. 95.

consequence and argues (from mathematical practice) that this notion is captured by the standard semantics of second-order logic. He rejects the (sharp) distinction between logic and mathematics. It appears as if Etchemendy just takes matters one step further. Section 9.6 below investigates in how far this is indeed the case.

9.5.1 Historical Intermission

A historical remark might be of interest, although it is almost irrelevant for the systematic discussion. Throughout his *The Concept of Logical Consequence* (Tarski, 1936b), Tarski remarks that the notion of logical consequence, as he found it characterised at the time, is not in good standing. It seems plausible to suggest, therefore, that Tarski's aim was rather akin to an *explication* in Carnap's sense than to a reduction. Concepts that are found too imprecise for scientific purposes can, according to Carnap, be explicated, i.e. replaced by new, precise concepts, that can fulfill the job. The heart of an explication is mere stipulation, but good explications have to accord (at least) to four conditions Carnap sets up: (i) *similarity*, the explicatum applies to most cases to which the explicandum applied so far; (ii) *exactness*, the rules of use exactly determine the explicatum and introduce it into a well-connected system of scientific concepts, (iii) *fruitfulness*, that scientific progress is better facilitated by adopting this explication, rather than sticking to the imprecise concept or adopting another explication, and (iv) *simplicity* (I will refrain from attempting to elucidate what this amounts to).

It is clear that Tarski considered the concept of logical consequence in need for revision, so the preliminaries for an explication are satisfied. Tarski argued for the fruitfulness of his notion, and, incidentally, it appears that he was right about that.

Tarski also points out a few times, that it should be clear how well his definition fits the notion that most people operate with.[54] This is Carnap's criterion of similarity. Is it reasonable to suppose, however, that Tarski had an explication in mind? After all, the *locus classicus* for the introduction of the notion of an explication is Carnap's *Logical Foundations of Probablity*[55] which was not published before 1950.

Tarski met Carnap in Vienna in 1930,[56] and there was a strong interaction between the two. It does not appear too unlikely that discussion between them and in meetings of the *Vienna Circle* already surrounded the idea of an explication, even if not under that name. In several passages of Carnap's *The Logical Syntax of Language* which appears only a year after Tarski's *The Concept of Logical Consequence* the basic idea seems to be present.[57] This might count as evidence for the conjecture that something like the notion of an explication was probably already around at a time before Tarski published his paper.[58] Ironically, Etchemendy's project is parallel to Tarski's, if Tarski's project was even close to being an explication. His complaint against the Tarskian account is that the notion that it characterises is not in good shape, and that the interpretational approach to model theory should therefore be abandoned in favour of a representation conception. Etchemendy acknowledges the importance of Tarski for the development of the model theory which is necessary also for his own account. He claims, presumably correctly, that "there is little question that model theory in its present form owes more to Tarski's work

[54] See e.g. (Tarski, 1936b), p. 9; (Tarski, 1983), p. 417; (Tarski, 2003), p. 186.

[55] (Carnap, 1950).

[56] See (Coffa, 1991), p. 280.

[57] (Carnap, 1937), see e.g. pp. 41–45, 100.

[58] For what it is worth: Carl Gustav Hempel describes Tarski's definition of truth as an explication, see (Hempel, 1970), p. 663, while Karl Popper thought that it was not, see (Popper, 1982), p. 273ff, however relevant that might be in this context. Alberto Coffa calls Tarski's definition of logical consequence an explication, see (Coffa, 1991), p. 286–287.

than to the work of any other single individual."[59] He seems to fail to appreciate, however, how similar (at least structurally) Tarski's project was to his own.

9.6 Comparisons

First, it is worth noting that it is far from clear that a wider account of valid inference must bring with it a rejection of the distinction between difference species of valid inference. Graham Priest, for example suggests an approach to characterise (at least) two kinds of "validity" in a system that is based on model-theoretical consequence: "deductive validity" and "inductive validity". Deductive validity should here not be understood as alluding to a deductive system; it is merely the way to contrast some notion of logical consequence (which in Priest is also wider than what I introduced in chapter 8 above) with inference by induction.[60]

Priest's approach would probably count as representational: the way he describes how the models of the systems must correspond to possible situations sounds pretty much like Etchemendy's characterisation of representational semantics, with 'situations' instead of 'ways the world may be'. Etchemendy's case for representational semantics thus should be independent of his rejection of the concept of (properly) logical consequence.

One could even argue *ad hominem* against Etchemendy that his rejection of a delineation of logical truth as a proper subconcept of necessary truth puts him into an unstable position. Etchemendy uses a distinction between logical and other nec-

[59](Etchemendy, 1999), p. 24.

[60](Priest, 1999); see also chapter 11 of (Priest, 2006). Priest "doctors" (his expression) for this the Tarskian approach to account for the consequence relation for deductive validity in his system (as opposed to the inductive consequence relation). Priest first suggested mending the Tarskian account like this in (Priest, 1995). See also footnote 25 above.

essary truths throughout his book.[61] In fact, Etchemendy's arguments concerning the conceptual and extensional inadequacy of Tarski's reduction seems to depend in many places on the possibility of a distinction between, especially, logical truths on the one, and mathematical truths or analytic truths on the other hand.

Etchemendy seems to be specially opposed to the idea that the continuum hypothesis might be a logical truth: "clearly, neither it nor its negation is a logical truth."[62] Presumably the continuum hypothesis is, if true, a *necessary* truth, however, as it is a mathematical statement. As such, it would have to be captured in one way or other by the representational semantics.[63] Etchemendy insists that second-order logic is legitimate, and that second-order languages certainly have consequence relations, just like first-order languages.[64] The question is now, does Etchemendy envision a representational semantics that uses the standard model theory for the second-order quantifiers, or rather a Henkin interpretation? In the first case, as was shown in chapter 7 above, there is a second-order sentence corresponding to the truth of the continuum hypothesis in the sense that will come out as a validity if, and only if, the continuum hypothesis is true.

It seems that if Etchemendy settles on the standard semantics, his approach is, in a sense, an extension of Shapiro's. Shapiro rejects any sharp distinction between logic and mathematics. Etchemendy argues for an approach to consequence that includes logical, mathematical, and all other necessary truths. Both want to capture pre-systematic notions in this area with the aid of formal semantics. Let me also bring the Deductivist conception into the picture. This account too aims to chara-

[61] See (Etchemendy, 1990), e.g. pp. 82, 100, 103, 104, 122–123, 132, 139, 147, 148, 154, and many other places.
[62] (Etchemendy, 1990), p. 123.
[63] Compare (Etchemendy, 1990), p. 25, 62.
[64] (Etchemendy, 1990), p. 124, 158.

terise a pre-systematic a notion. It restricts itself to genuinely logical consequences, however, and uses inference rules, rather than model-theoretic constructions. It appears that the relation between the three can be represented as in the diagram below, where D stands for a system of the Deductivist approach, S for one on the standard semantic account, as exemplified by Shapiro, and R for an Etchemendian representational semantics.

$$D \longrightarrow S \longrightarrow R$$

The arrow indicates that the consequence relation characterised by the system on the left-hand-side is included in the system on the right-hand-side. Thus, for the example of second-order logic, all theorems of the deductive system are validities of the standard semantics, and they are also valid on the representational account. A representational semantics might contain validities like ‘All bachelors are unmarried’, or ‘All red things are coloured’, on the other hand, which are not declared valid by the standard semantics. For the case of second-order logic, though, it does not seem to make a difference at all whether we adopt S or R. This means, in turn, that for the case of second-order set theory, Etchemendy runs into the same problems as Shapiro, that were outlined in chapter 7 above: the truth or falsity of the continuum hypothesis is included in the semantics, in some sense, but our studies of it are blocked in the way Jané pointed out. Thus, the representational account too has all the answers to the open problems of set theory, but keeps them to itself.[65]

If Etchemendy envisions the use of a Henkin style semantics, the picture looks more like this:

[65]Paraphrasing Jané’s remarks about the standard semantics, (Jané, 1993), p. 80. See my chapter 7.

$$\begin{tikzcd}[row sep=small] & S \\ D \arrow[ur] \arrow[dr] & \\ & R \end{tikzcd}$$

The problems of the standard semantics are thus avoided, but the question is now to what extent the models of the representational semantics are indeed a faithful representation of the subject matter, if we turn to second-order set theory. The Löwenheim-Skolem Theorems hold for Henkin semantics, and so there are models of varying cardinality. In particular, there is a countable model of set theory. One can argue, with Shapiro, that this just cannot represent the universe of sets faithfully. If all necessary truths of a given topic are to be captured by the representational semantics, then a Henkin semantics does not appear to be suited to do the job for any area in mathematics that is about infinite domains, and so especially not for set theory.

So, I conjecture, Etchemendy will have to adopt the standard semantical treatment of the second-order quantifiers for mathematical theories. A categorical axiomatisation of infinite structures seems crucial to a faithful representation of it – this is the case that Shapiro makes. Then, however, Etchemendy's approach is (at least formally) indistinguishable from Shapiro's for the case of second-order set theory, and thus has the same problems.

The conclusion to be drawn is, I believe, that it is important to make a strict distinction between logic and mathematics; mathematics here is represented by set theory. Using model-theoretic techniques is an enterprise in applied set theory, modeling is a mathematical practice. This much is agreed by most proponents of model-theoretic techniques, including Etchemendy and Shapiro.

I intend this conclusion to be a statement about *proper* logic, and not about other projects that are carried out using formal semantics. As outlined in chapter 8 above, this is not a mere quarrel about names, but has epistemic significance. The fruitfulness of model-theoretic studies is not to be underestimated, and the techniques involved are powerful. They are also mathematical. In most areas, this will not matter; if the use of mathematics is not problematic for most purposes, then neither is the use of a mathematical model theory. This includes most purposes that Etchemendy has in mind. There is a need for a notion of proper logic, however, especially for foundational purposes. Where epistemological status matters, proper logic has a clear advantage, as argued in chapter 8. In particular, foundational studies in (the philosophy of) mathematics should not presuppose set theory. Otherwise the approach will not be able to account for all areas of mathematics: set theory itself cannot get a satisfactory treatment.

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