

# **Language, Necessity and Convention**

Reconsidering the Linguistic Approach to Modality

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### Abstract

This thesis is an examination of the linguistic approach to modality (also known as ‘linguistic conventionalism’) – i.e. the view that necessity is to be explained in terms of the linguistic rules that we have adopted. Drawing on an investigation into the history of this approach, I argue against the currently prevalent attitude that it can be dismissed as misguided. The aim, however, is not to argue that the linguistic approach is correct, but, more modestly, to put it back on the table as an interesting and viable research program.

The thesis is divided into three parts. In part A, I articulate a conception of the commitments of the approach based on the ideas that influenced it, how it emerged and developed in the work of the logical positivists, and, in particular, the role it was meant to play in “making a consistent empiricism possible”.

Next, in part B, I defend the core ideas of the approach against various objections. Notably, I consider the objection that truth cannot be “created” by convention, the objection that necessities cannot be explained in terms of contingencies, and the objection that determining what the linguistic conventions are, unlike determining what the modal facts are, is a straightforwardly empirical matter.

In part C, finally, I turn to objections which purport to show that there are limits to what can be explained in terms of linguistic *convention*. Specifically, I consider whether we need to assume a non-conventional distinction between admissible and inadmissible linguistic rules, a non-conventional consequence relation, or a non-conventional starting-point in order to get the linguistic approach off the ground. An overarching question is whether we are forced to take some *logic* for granted in a way which would undermine the explanatory ambitions of the approach.

I argue that some of the prominent objections rely on misunderstandings, that some can be answered head-on, and that some point to genuine challenges and constraints which put pressure on the linguistic approach, but do not warrant a wholesale rejection of the view. Instead, they point to areas where further work is needed.



## Preface

**Declaration:** This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text.

It is not substantially the same as any that I have submitted, or, is being concurrently submitted for a degree or diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text. I further state that no substantial part of my dissertation has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text.

It does not exceed the prescribed word limit for the relevant Degree Committee.

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## Introduction

‘The philosophical problem of necessity’, Dummett tells us in a much-quoted passage, ‘is twofold: what is its source and how do we recognize it?’ (1959: 327) A once popular response to this problem – which, of course, was a problem long before Dummett’s formulation of it – goes roughly as follows:

The source of necessity lies in language and specifically in the conventionally adopted rules that govern the use of linguistic expressions. We are able to recognise necessity insofar as we know what these rules are and can reflect upon them.

This rough idea has been given various names, including ‘linguistic conventionalism’ and ‘the linguistic doctrine of necessary truth’. I shall call it ‘the linguistic approach to modality’, or just ‘the linguistic approach’.

Although this view is not without contemporary defenders,<sup>1</sup> it is now rather unfashionable. Two important developments are often said to be responsible for this: the attack on the analytic-synthetic distinction<sup>2</sup> and

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<sup>1</sup>Alan Sidelle (1989; 2009), Jody Azzouni (1990; 2014), Amie Thomasson (2007a; 2007b: ch. 3; 2009; 2013), and Jared Warren (2015a; 2015b; 2017) have all provided recent defences of the idea. In addition, Hans-Johann Glock has defended a broadly speaking Wittgensteinian approach (2003a; 2003b: ch. 3; 2008), and Richard Creath has defended Carnap’s views against some of Quine’s objections (1987; 1990a; 2003).

<sup>2</sup>The classic attack is of course due to Quine (1951), but White, Goodman and Tarski all voiced similar concerns at the time (cf. e.g. White 1950; Goodman, Quine, and White 1947; Tarski 1944; and Frost-Arnold 2013: 94–101).

Kripke's discovery of necessary *a posteriori* truths.<sup>3</sup> The full story, however, is bound to be more complicated, and has to do with related but broader changes in the philosophical climate, such as the demise of logical positivism, the weakening of Wittgenstein's influence, the backlash against "ordinary language philosophy", and the revitalisation of "robust metaphysics".

However, while the linguistic approach is currently unpopular, it is not neglected. Instead, it remains a quite popular *target*. It remains a target, it seems, not so much because there are occasional defenders, but because it is regarded as superficially – perhaps *very* superficially – appealing, and because it is perceived to have a certain grip on philosophers even if they do not explicitly endorse it – perhaps even explicitly reject it. Thus, for instance, Theodore Sider includes an attack on the linguistic approach in *Writing the Book of the World* (2011: §6.5), and prefaces this attack by noting that while few self-identify as 'conventionalists', there are a number of prevalent tendencies that would be explained by 'latent logical conventionalism' (Sider 2011: 97–8). The view is therefore deemed worthy of the occasional refutation.

In addition to the various objections that are levelled against it, there is, I think, a more general reason why many are now suspicious of the linguistic approach. If necessity has its source in language, it may be thought, then this makes the discovery of necessary truths, and perhaps even philosophy in general, a far less significant enterprise than it would be if necessity has its source in something like the deepest and most general features of reality. Accepting the linguistic approach, it may be felt, pushes us that much closer to Stephen Hawking's depressing view of the history of philosophy:

Philosophers reduced the scope of their enquiries so much that

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<sup>3</sup>Whether Kripke deserves sole credit here is contentious. According to Peter Hacker (2006: 131), William Kneale had already argued that there are *a posteriori* necessities in 1949 (cf. Kneale 1949: 78–89), and Quentin Smith (1995a; 1995b) argues that Ruth Barcan Marcus (1961) got there before Kripke. Scott Soames, however, is quite determined to defend Kripke's claim to priority (1995; 2006).

Wittgenstein, the most famous philosopher of this century, said, ‘The sole remaining task for philosophy is the analysis of language.’ What a comedown from the great tradition of philosophy from Aristotle to Kant! (1988: 193)

But thinking of modality as having its source in language does not force us to think of modal inquiry, or philosophy in general, as a trivial or insignificant matter. This only follows if we also hold that everything which is related to language is somehow superficial, or conceive of things in terms of a crude distinction between what is “really the case” and what is “merely a matter of how we speak”.

A quite opposite view would be that the world – the world that matters to us as thinkers, knowers and agents – is a world which is, to a great extent, shaped by how we represent it in language. Whether or not language is a strict prerequisite for thought, it can hardly be denied that it is a prerequisite for engaging with the world in the way that we do. In any reasonably sophisticated case of knowledge, we are dealing with linguistically structured knowledge, and the world we know is a linguistically structured world.

Against this background, becoming clearer about the conceptual structures we use to orient ourselves in the world should not be thought of as “mere knowledge of language” and irrelevant to our attempts to understand the world around us. Rather, the “analysis of language” could very well be said to be a *transcendental* investigation in something quite like Kant’s sense. Pursuing the linguistic approach would then hardly be much of a comedown from a tradition where Kant’s achievements are among the finest. So, while the linguistic approach to modality does indeed stand in opposition to the more robustly metaphysical tendencies in contemporary philosophy, the opposing picture need not be a deflationary, trivialising one – it could be rather more Kantian.

As may be suspected, my aim is to defend the linguistic approach. This aim is motivated, not by an unshakable confidence that this approach is

correct, but by its already mentioned superficial appeal, together with the suspicion that the reasons given for discarding it are far less decisive than they are typically taken to be. What I will try to establish is that the linguistic approach deserves to be taken seriously, and that if it should turn out to be unsatisfactory, this will be the result of a careful investigation of what it can and cannot explain, not because it is fundamentally incoherent, rests on a “category mistake”, or the like.

Consequently, my defence will be *minimal* in a certain sense. I shall have nothing to say, for instance, about ‘problem cases’ for the linguistic approach – cases, that is, which the linguistic approach may seem ill equipped to handle (the necessary *a posteriori* and colour exclusion cases may come to mind). Given the current state of the discussion, attempts to deal with such cases are bound to seem somewhat irrelevant. There are, it will be said, much more fundamental reasons for rejecting the approach.<sup>4</sup>

Indeed, my aim is not to *carry out* the linguistic approach, but, rather, to clear the way for such a project by articulating what the commitments of the approach are, and by responding to objections that are taken to disqualify it *prior to* detailed investigations of which necessary truths it is capable of accounting for. If I achieve this aim, we shall be in a better position to see what would be required in order to deal with various cases within the framework of the linguistic approach, but it will be an open (and interesting) question whether we can in fact do so.

The thesis falls into three main parts. Part A introduces the linguistic approach in more detail, with a focus on developing an account of its distinctive commitments. This is not a straightforward task. After all, a standard objection to the linguistic approach is that it amounts, upon scrutiny, to little more than a family of metaphors, pictures and slogans that resist proper explication. This, I will argue, is too pessimistic, but getting a grip on the commitments of the view is not a matter of hitting

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<sup>4</sup>For an illustration of this, see Stephen Yablo’s review (1992) of Alan Sidelle’s attempt (1989) to defend the linguistic approach from the objection that it cannot handle the necessary *a posteriori*.

upon a satisfactory one-line explication; we need to look at the historical context in which it was developed and the various concerns it was meant to address. This is unfortunate insofar as it means extra work, but fortunate insofar as the history of early analytic philosophy is interesting.

Part B focuses on various objections which purport to show that there is something thoroughly misguided with the general idea behind the linguistic approach. I argue that none of them warrant this conclusion: Some can be straightforwardly answered; others point to genuine constraints which an adherent of the linguistic approach must keep in mind, but do not suffice to undermine the view.

Then, in Part C, I turn my attention to a notion which (perhaps surprisingly) will have played a relatively minor role so far: namely *convention*. I consider a number of objections which question how much work this notion can do in this context, and which suggest that we are forced to take some logic for granted in a way which undermines the explanatory ambitions of the linguistic approach.

However, as the historical investigations in Part A will have made clear, the notion of convention, though certainly important, is not the be-all and end-all of the linguistic approach. Against this background, I argue that although there are limits to what can be termed ‘conventional’ here, none of these objections suffice to undermine the general idea behind the linguistic approach or show that there are in principle reasons why the explanatory project is doomed to failure. That being said, some of these objections raise thoroughly difficult issues, and point to genuine challenges which future work on the linguistic approach will need to take into account.





**Part A**

**Articulating the Linguistic Approach**



## The Commitments of the Linguistic Approach

The aim of this chapter is to articulate how we should think of the commitments of the linguistic approach. I begin (1.1) by providing some typical characterisations of the linguistic approach, taken from both historical and contemporary sources. These will serve both to identify the relevant tradition, and to highlight the variations that can be found within it. Next (1.2), I introduce the worry that, upon scrutiny, such formulations do not provide us with a satisfactory conception of just what the linguistic approach amounts to.

Attempts to spell out such formulations face two prominent pitfalls: We might end up making the linguistic approach trivial, in the sense that no particular account of the source of necessity is singled out, or we may end up making it obscure, in the sense that we lose our grip on how to assess whether it is correct or not. These pitfalls are illustrated in sections 1.3 and 1.4 respectively.

Finally, in sections 1.5 and 1.6, I argue that the way to avoid these problems is to understand the linguistic approach in terms of a second-order commitment to a *form of explanation* when it comes to modal matters.

## 1.1 Paradigmatic Characterisations

Let us begin by introducing some standard formulations of the linguistic approach from its heyday, stretching from the late 20's to the early 50's.<sup>1</sup> At the time, the view was orthodox enough that C. I. Lewis and C. H. Langford could remark passingly, in their textbook *Symbolic Logic*, that ‘the facts which the principles of logic state are simply facts of our own meanings in the use of language: they have nothing to do with any character of reality, unless of reality as exhibited in human language-habits.’ (1932: 212)

Historically, the linguistic approach is associated primarily with the logical positivism of the Vienna Circle, certain time-slices of Wittgenstein, and, to a slightly lesser extent, the “ordinary language philosophy” that used to be prevalent in Oxford.

Already in the *Aufbau*, Carnap wrote that ‘*Logic (including mathematics) consists solely of conventions concerning the use of symbols, and of tautologies on the basis of these conventions.*’ (1928a: 178) Those attending Schlick’s lectures in 1933–34 were taught that ‘logical laws are sign-rules, and thus rules that are ultimately laid down by ourselves, and could also have been different.’ (1987: 107) And Hans Hahn argued that ‘logic [...] does not deal with any objects at all; *it only deals with the way we talk about objects*’ (1933b: 29; emphasis in original). Thus, for instance, the laws of excluded middle and non-contradiction are said to ‘express agreement about the use of negation’<sup>2</sup> (1933b: 32–3). These ideas greatly impressed A. J. Ayer who confidently introduced them to Britain by announcing that all necessity is analyticity and that analytic statements ‘simply record our de-

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<sup>1</sup>While the approach was far from universally accepted, it was much discussed. Cf. the symposia by Broad, Porteous and Jackson (1936), Ayer, Whiteley and Black (1936), and Britton, Urmson and Kneale (1947), as well as papers by Quine (1936), Stebbing (1936), Hardie (1938), Ewing (1940), Lewy (1940), Malcolm (1940) and Kneale (1946).

<sup>2</sup>There is another (partial) translation of this text (by Arthur Pap) in Ayer (1959). Here it is said that it is a *convention* about the use of negation which is expressed. However, the original German has ‘Verabredung’, and reserving ‘convention’ for ‘Konvention’ seems like a good idea in this context. The German text can be found in Schulte and McGuinness (1992).

termination to use words in a certain fashion.’ (1936a: 114)

Wittgenstein, who discussed these ideas with members of the Vienna Circle, wrote that: ‘the connexion which is not supposed to be a causal, experiential one, but much stricter and harder, so rigid even, that the one thing somehow already *is* the other, is always a connexion in grammar.’ (1978: Part I, §128) ‘Grammar’, moreover, ‘is not accountable to any reality. It is grammatical rules that determine meaning (constitute it) and so they themselves are not answerable to any meaning and to that extent are arbitrary.’ (1974: Part I, §X.133) Thus: ‘the only correlate in language to an intrinsic necessity is an arbitrary rule.’ (1974: Part I, §X.133; cf. also 1953: §372) Similar ideas were picked up, to varying degrees, by some of Wittgenstein’s students and associates.<sup>3</sup>

Strawson, moreover, writes, in his *Introduction to Logical Theory*, that all concepts of ‘logical appraisal’ may be explained in terms of inconsistency, and that it is ‘our own activity of making language through using it, our own determination of the limits of the application of words, that makes inconsistency possible’. (1952: 9) ‘Behind inconsistencies between statements’, therefore ‘stand rules for the use of expressions.’ (1952: 10)

Certainly, we can find differences between these authors (and, indeed, between various formulations by the same author).<sup>4</sup> Still, a distinct tradition has, correctly to my mind, been identified here, centred around the common idea that the special status of necessary (or logical)<sup>5</sup> truths is due, in some sense, to the conventionally adopted rules that determine the correct use of the relevant expressions. This, moreover, is generally contrasted with the idea that logical necessities concern ‘reality’ or ‘objects’.

Turning now to more recent characterisations of the linguistic approach, here are three typical examples, taken from Gillian Russell, Theodore Sider

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<sup>3</sup>Including Wisdom (1938: 463), Lewy (1940), Malcolm (1940), Britton (1947), Lazearowitz (1972), and Ambrose (1974).

<sup>4</sup>For a systematic overview of some such differences, see Lambros (1975).

<sup>5</sup>Though the passages from Schlick, Hahn, Strawson, and Lewis and Langford mention only logic specifically, it is quite clear, at least in Schlick’s and Hahn’s case, that the explanation is supposed to account for necessary truths more generally.

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and Bob Hale, respectively:

My target in this paper is a view that has sometimes been called the ‘Linguistic Doctrine of Necessary Truth’ and sometimes ‘Conventionalism about necessity’. It is the view that necessity is grounded in the meanings of our expressions – meanings which are sometimes identified with the conventions governing those expressions – and that our knowledge of that necessity is based on our knowledge of those meanings or conventions. (G. Russell 2010: 267)

Behind the picture,<sup>6</sup> I suspect, there lies an identifiable – and mistaken – philosophical doctrine: the doctrine of logical conventionalism. [...] Logical conventionalism originated in “the linguistic theory of the a priori” [...] The core of the view is that an analytic truth, for instance the truth that all horses are horses, is true purely by virtue of linguistic conventions. By adopting certain rules governing the use of logical words like ‘all’, language users somehow make ‘all horses are horses’ true. (Sider 2011: 97–8)

Conventionalism – the thesis that necessary truths are simply truths guaranteed by conventions governing the use of words – is in some ways the clearest form of the doctrine of truth in virtue of meaning. In my view, the reasons for its failure are equally clear and decisive. (Hale 2013: 117)

There are interesting and important differences between these brief characterisations. Firstly, Sider talks about truth *by virtue of* linguistic conventions, Hale of truth *guaranteed by* such conventions, and Russell of necessity *being grounded* in these. While the ‘by virtue of’ and ‘grounded in’ locutions may be equivalent (perhaps only by being equally obscure), ‘guaranteed by’ is different. The existence of an aunt guarantees the existence of a niece or a nephew, but that is not to say that the existence of the niece or nephew

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<sup>6</sup>The ‘picture’ in question is the idea that logical expressions are formal and therefore not “worldly” in some sense. Sider opposes this because he wants to argue that logical expressions can carve the joints of worldly structures.

is grounded in the existence of the aunt. Moreover, there is surely a big difference between the idea that linguistic conventions can guarantee the truth of sentences, and the idea that they can *make sentences true*, which is how Sider puts it.

Secondly, Russell suggests that necessity can, on this view, be grounded in *meaning* without being grounded in convention, whereas Sider and Hale do not leave this possibility open. Related to this, Russell's way of putting it suggests that what she calls 'the linguistic doctrine of necessary truth' might be characterised as the view that necessity should be explained in terms of analyticity. Sider, on the other hand, takes 'the linguistic theory of the *a priori*' to be a particular view about what analyticity is.

Finally, in Russell's formulation it is *necessity* which is grounded in the meanings or conventions according to the conventionalist, whereas on Sider's it is *truth*.

These differences notwithstanding, as with the historical examples there is enough in common here that we can take these passages to discuss the same distinct tradition in the philosophy of modality and logic. It is this tradition that I will refer to using the label 'the linguistic approach'. To be sure, the central thesis remains rather vague, and perhaps it will turn out that inclusion in the tradition is best seen as a matter of family resemblance. But it is legitimate to talk of a distinct tradition here, both historically and as a matter of doctrine.

## 1.2 A Quinean Concern

It hardly needs pointing out that the characterisations provided above would have to be developed a lot further in order to provide us with anything like a satisfactory answer to Dummett's twofold problem. What we have here is only a starting-point.

Or so it seems, but it might be objected that it is generous to think that we have even this much. For one standard objection to the linguistic

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approach is, not so much that it is mistaken, but that it is simply unclear, upon scrutiny, what it amounts to. Characterisations like the above – and related slogans such as ‘truth by convention’ and ‘truth in virtue of meaning’ – it is said, crumble once we try to make them explicit, providing us little guidance, in the end, as to just what is being claimed and how we are to assess whether it is true or not. Here is Quine’s formulation of the worry:

Consider, however, the logical truth ‘Everything is self-identical’, or ‘ $(x)(x = x)$ ’. We *can* say that it depends for its truth on traits of the language (specifically on the usage of ‘=’), and not on traits of its subject-matter; but we can also say, alternatively, that it depends on an obvious trait, viz., self-identity, of its subject matter, viz., everything. The tendency of our present reflections is that there is no difference.

I have been using the vaguely psychological word “obvious” non-technically, assigning it no explanatory value. My suggestion is merely that the linguistic doctrine of elementary logical truth likewise leaves explanation unbegun. I do not suggest that the doctrine is false and some doctrine of ultimate and inexplicable insight into the obvious traits of reality true, but only that there is no real difference between these two pseudo-doctrines. (1960a: 355–6)

The context here is Carnap’s views on logical truth,<sup>7</sup> and it is noteworthy that Quine chooses the label ‘pseudo-doctrine’ to express his dissatisfaction. After all, Carnap himself frequently complained that philosophical disputes involved pseudo-doctrines aimed at addressing pseudo-questions (cf. in particular: 1928b; 1932; 1950), and I suspect that Quine chose this label precisely to alert Carnap to the fact that he was objecting on grounds that he thought Carnap, of all people, would have to acknowledge.

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<sup>7</sup>Though it is worth noting that the version which appears in Schilpp (1963) opens with a preface where Quine concedes that he is unable to state his objections in Carnap’s terms and that this ‘perhaps counts in favor of Carnap’s position.’



To get to the core of Quine's objection, then, it is useful to consider what motivated Carnap's own dissatisfaction with the "pseudo-doctrines" of traditional metaphysics:

Even in the pre-Vienna period, most of the controversies in traditional metaphysics appeared to me sterile and useless. When I compared this kind of argumentation with investigations and discussions in empirical science or in the logical analysis of language, I was often struck by the vagueness of the concepts used and by the inconclusive nature of the arguments. I was depressed by disputations in which the opponents talked at cross purposes; there seemed hardly any chance of mutual understanding, let alone of agreement, because there was not even a common criterion for deciding the controversy. (Carnap 1963a: 44–5)

One nice feature of this passage is that it brings out that Carnap's hostility towards traditional metaphysics was motivated, not by a dogmatic presumption in favour of verificationism, but by a belief in the importance of *communal standards* in intellectual enquiry. As the positivists saw things, wildly speculative and sometimes genuinely dangerous metaphysical systems were allowed to develop without any checks and balances – standards to which intellectual inquiry could be held accountable. The lack of such standards was perceived to be a serious obstacle to progress, both intellectually and politically.

This is precisely what Quine thinks is lacking in the present context. In calling the linguistic approach a pseudo-doctrine, he is expressing his concern that a debate between an adherent of this approach and someone who advocates, say, 'some doctrine of ultimate and inexplicable insight into the obvious traits of reality' is going to be precisely the kind of debate that Carnap himself would want to avoid, because it would be lacking 'a common criterion for deciding the controversy'.

The demand for some such criterion is clearly a sensible one. However, this is not to say that we have to accept anything like the stringent standards

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of Quine or the logical positivists. We need not demand, that is, that the linguistic approach must have *empirical* significance or be capable of something like formal proof. What we *do* need is an appropriately detailed account of which kinds of considerations would speak in favour of it, and what would speak against it. This, then, is our initial question:

What are the distinctive, practically workable, commitments of the linguistic approach?

In asking for *distinctive* commitments, we are asking for commitments that suffice to pin down an answer to the Dummett's question – that is, commitments which single out a particular account of the source of necessity and modal knowledge. It is not enough, however, to pin down such an answer. We also need to know what could actually be done to show that the answer is a good or a bad one. This is the point about asking for *practically workable* commitments. What does a proponent of the approach need to do? What is required from a refutation? We are not, of course, asking for a decision procedure, but an appropriately vague description of what a defence or refutation of the approach would look like.

The difficulty is that these two demands often pull in opposite directions. As we will see, attempts at explicating the kinds of characterisations we looked at in section 1.1 frequently leave us with no distinctive commitments at all, and natural strategies for rectifying this introduce notions which obscure how the relevant issues are to be settled, or, alternatively, end up rendering the linguistic approach obviously mistaken. This general pattern is quite recognisable in the literature. Thus, for instance, we find Arthur Pap complaining that 'to the extent to which a radical linguistic theory is true it is mostly trivial, and to the extent that it is not trivial, it is either false or meaningless' (1958: 163).

### 1.3 The Threat of Triviality

As we saw in section 1.1, the linguistic approach is sometimes characterised as the view that necessary truths are truths which are ‘guaranteed by the linguistic conventions’. It is natural, therefore, to suggest that an adherent of the linguistic approach is committed to something along the lines of:

(GUARANTEE). A sentence  $S$  is a necessary truth iff the truth of  $S$  is guaranteed by the conventions governing the use of words occurring in  $S$ .

We do, of course, have a good grasp on how to assess whether biconditionals are true or not: The question is whether the concept on the left-hand side is coextensional with that on the right-hand side. This, however, presupposes that we have an adequate grasp on when these concepts apply, and this is not clear when it comes to the right-hand side here. How do we settle, in any given case, whether the truth of a sentence  $S$  is guaranteed by the relevant linguistic conventions or not? Indeed, this is precisely the kind of question we find Quine pressing in the passage above.

A straightforward suggestion would be to spell out the notion of a guarantee modally, so as to get:

(MODAL). A sentence  $S$  is a necessary truth iff, necessarily, any sentence governed by the same linguistic conventions will be true.

Any immediate concern here is that this appeal to necessity comes with a circularity threat since necessity is precisely what we are trying to account for. This, however, is not the problem I want to focus on, since it is not too difficult to see that suggestions along these lines are heading for trouble regardless of this.

The problem, essentially, is that accepting (MODAL) is consistent with just about any view concerning the source of necessity, and so fails to provide us with any distinctive commitments. To see this, note that anyone

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who thinks that the linguistic conventions determine which proposition any given sentence expresses will be committed to the left-to-right direction of (MODAL). If a sentence  $S$  is necessary – if, that is, it expresses a necessary proposition – and the relevant conventions *suffice* to ensure that it expresses the proposition that it does, then it follows that any sentence governed by the same conventions will be true and indeed necessarily so. This, however, might simply be due to the fact that the conventions suffice to correlate the given sentence with a necessary proposition. Nothing follows about where that necessity ultimately came from.

As Theodore Sider puts it when rejecting a close cousin of (MODAL) – namely the idea that necessary truths “automatically” become true once endowed with meaning via the linguistic conventions:

The mere fact that it is necessarily true that it is raining if it is raining ensures that ‘If it is raining then it is raining’ is “automatically” – in the current sense of ‘automatically’ – true once it has been given its meaning. Conventionalism thus understood says little more than that logical and other analytic truths are necessary; nothing is left of the intuitive idea of their truth being grounded in conventions. (Sider 2011: 101)

Now, it is certainly far from clear that we should accept that the linguistic conventions always suffice to determine what propositions a sentence expresses (even a modest amount of semantic externalism should make us sceptical of this). The point, however, is not that it is mandatory to accept (MODAL), but that its status turns out to be largely independent of the questions we are interested in.<sup>8</sup> Even assuming it be true, we are not forced into any particular view about the source of necessity.

Although (MODAL) might not seem like a promising candidate to begin with, the problem just noted generalises to various other proposals we find

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<sup>8</sup>I might add that there are some complications with the right-to-left direction of (MODAL). A sentence like ‘this sentence is meaningful’ is arguably such that, necessarily, any sentence governed by the same linguistic conventions will be true. Still, this sentence isn’t necessary. Edward Craig seems to overlook such cases in the presentation of an argument which has informed the present discussion (cf. Craig 1975: 3–5).

in the literature. In the writings of the logical positivists, for instance, we frequently find passages such as:

To ascertain the truth of some sentences, e.g., “Some dogs are black”, it is further necessary to know certain facts of the world. In the case of other sentences, e.g., “all black dogs are black”, this is not necessary; to understand them is a sufficient basis for the determination of their truth. (Carnap 1963a: 915)

However, we can accept this passage in its entirety without committing ourselves to a particular view about what is responsible for the special status of the latter sentence. Simply put, we are in Euthyphro territory: Is the sentence necessary because understanding it suffices for knowing that it is true, or does understanding it suffice for knowing that it is true because it expresses a necessary proposition? The problem with (MODAL) and related formulation is that they do not pin down answers to such questions.

## 1.4 The Threat of Obscurity

As the Euthyphro analogy suggests, the problems in the previous section arose because we were not being explicit on what is meant to explain what. The linguistic approach is meant to be a view about why necessary truths are necessary, but (MODAL) and nearby suggestions only claim a *correlation* and are thus too coarse to capture this.

This suggests a straightforward fix. Why not just build the explanatory relation into our formulation of the commitments of the linguistic approach? As the passages quoted in section 1.2 indicate, there is no shortage of locutions in the philosopher’s vocabulary which could be used here. Popular choices include: logical necessity is *grounded* in linguistic matters; it is *due to* language; it is *reducible* to linguistic facts; linguistic conventions act as *truth-makers* for necessary truths, or perhaps *necessity-makers* (Sidelle 1989: 10); modal facts obtain *in virtue of* linguistic facts; and the list goes on.

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Picking a particularly popular candidate, an alternative way of spelling out (GUARANTEE) would be:

(GROUND). A sentence  $S$  is a necessary truth iff the truth of  $S$  is grounded in the conventions governing the use of words occurring in  $S$ .<sup>9</sup>

This does, I suppose, suffice to pin down a partial answer to Dummett's question, but the problem now is that it becomes less clear how to settle whether it is correct or not. This is not to say that the notion of grounding is inherently obscure and never useful. But even if we suppose that we understand the notion perfectly well, it remains the case that we do not have a good practical grasp on how to settle, for any particular grounding claim, whether it is true or not.

We are, effectively, just back in the predicament Quine complained about. How are we to settle whether the truth (or necessity) of 'everything is self-identical' is grounded in the linguistic conventions or, say, some obvious fact about reality? Whatever we think of the notion of grounding in general, it is difficult to see that it is suited to advance matters here. Briefly put: insofar as Quine had a point in his objection to Carnap, appealing to something like grounding is hardly a solution. It does not provide us with an improved grasp on, so to speak, the 'rules of the game'.

A further problem with this strategy is that grounding is a hyperintentional notion, which would mean that insofar as the grounding relation is itself understood to be objective and language-independent it would be off limits to the adherent of the linguistic approach. If grounding relations are objective, language-independent features of reality, then there is not much point in denying that modality is.

At this point we are confronted with something like a dilemma: We could either refrain from appealing to something like grounding, and stay silent

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<sup>9</sup>In fact, Russell (2010: 267), Sider (2011: 101) and Hale (2013: 117) all appeal to grounding to characterise the linguistic approach, although, as we have seen, Hale also employs other, non-equivalent notions.

on what explains what, thereby running the significant risk of ending up with no distinctive commitments at all. Or we could use some such notion to stipulate a particular explanatory relationship, but then risk losing our grasp on how to settle the question.

## 1.5 A Different Perspective on Explanation

The way forwards is to approach the notion of explanation from a different perspective. It is indeed correct that the linguistic approach is a view about what explains necessity, and this needs to figure in an account of its commitments. However, we need not think of this in terms of a commitment to a claim along the lines of (GROUND). Rather, the linguistic approach should be viewed as a commitment to actually carrying out an explanatory project of a particular kind.

The notion of grounding can be said to encapsulate a form of realism about explanation (Bliss and Trogon 2016). According to the grounding-enthusiast, to say that  $X$  explains  $Y$  is to say that the objective relation of explanation – grounding – holds between  $X$  and  $Y$ . The problem of arguing about this then presents itself in the form: How can we settle whether this relation is really there in a given case?

It is, however, possible to think of explanation along different lines. Explanation, it may be said, is something *we* do. To say that  $X$  explains  $Y$  is just to say that we can make  $Y$  intelligible in terms of  $X$ . If we can do this, we should be content – at least until and unless we discover new or remaining problems. As long as the explanation is a good explanation by all internal/operational criteria, there is no point in worrying about whether we are *in fact* tracking the (supposedly) real explanatory relations.

On such a conception, committing to ‘ $X$  explains  $Y$ ’ is not to be understood as a commitment to there being a relation of grounding holding between  $X$  and  $Y$ , but as a commitment to actually providing a detailed account which makes  $Y$  intelligible in terms of  $X$ . This is not, it is im-

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portant to note, to say that a good explanation is nothing more than an explanation which is psychologically convincing. As Michael Scriven puts it in the context of discussing historical explanations:

In so far as there are different respects in which one can be said to lack understanding of an act, a condition, a tendency a law, etc., so far there are different ways in which it can be explained. For historical explanations this *does not* have the consequence that explanations are judged by some purely subjective standard of empathetic acceptability, since to say that an historical phenomenon is understood is not to say that someone (or everyone) *thinks* he understands it. There are objective tests for understanding just as for knowing or inferring. (1959: 452)

Rejecting the idea that explanations are aimed at tracking objective explanatory relations is consistent with maintaining that explanations are offered, so to speak, within ‘the space of reasons’. Someone who wishes to reject a proposed explanation cannot merely insist that they are not satisfied with it. They should be able to say just why they think the explanation doesn’t work – by, e.g., pointing to a gap or denying some claim which the explanation relies on. Correspondingly, someone who offers an explanation cannot simply ignore such challenges by insisting that they do, after all, find the explanation enlightening.

Of course, there are no algorithms for determining just what challenges we are obliged to take seriously, but this is as it should be. The point is just that we do, after all, have a good grasp on how to evaluate explanations: We assess whether what is appealed to in the explanation (the ‘explanans’ in somewhat antiquated terminology) is true, we look for gaps, hidden assumptions, vicious circularities and so on.

Now, the grounding enthusiast may well try to appropriate this and say that these are simply the criteria we use to determine whether the grounding relation is present. This is not something I want to quarrel with here, but I do want to maintain that an appeal to grounding would remain,



as Wittgenstein might have put it, an ‘idle wheel’ in this context. For even if it said that our ability to make  $Y$  intelligible in terms of  $X$  somehow indicates that  $X$  metaphysically grounds  $Y$ , this still wouldn’t affect how we actually assess proposed explanations. For all *practical* purposes the question is just whether the explanations work by our operational criteria.

## 1.6 Characterising the Commitments

Let us now consider how this can inform our conception of the distinctive practical commitments of the linguistic approach. The suggestion is that this approach should be seen, not so much as a commitment to a particular claim, but as a commitment to offering explanations of how necessary truths get to be necessary which appeal only to “linguistic matters” – i.e. matters related to conventionally adopted rules governing the use of linguistic expressions. We can thus think of the commitments of the linguistic approach in terms of a *restriction* on what resources we can draw on when offering explanations of necessary truths. Just what these resources are (what gets to count as a linguistic matter in the above sense) is indeed a substantial question which we shall say more about in the next two chapters.

The point, for now, is that if we assume that we have a decent grasp on this (and we do, of course, have *some* grasp on this already), then this does provide us with a conception of the distinctive and practically workable conception of the commitments of the linguistic approach. An adherent of this approach needs to provide detailed explanations, purporting to show how the necessity of actual cases of necessary truth can be understood in terms of the rules that govern how we represent the world in language. The debate will then focus on:

- (A) Whether these are in fact good explanations. In particular:
  - (i) Whether the explanans is indeed true.
  - (ii) Whether there are any problematic gaps or hidden assumptions

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in the explanation.

(B) If so, whether the restriction to ‘linguistic matters’ is in fact respected.

(C) If so, whether there are recalcitrant cases which seem to resist explanation along similar lines.

This, then, is our reply to the Quinean concern: If the question is how we are to decide between the linguistic approach and ‘some doctrine of ultimate and inexplicable insight into the obvious traits of reality’, the answer must be that the devil is in the details. We need to see detailed explanations and assess them according to (appropriate versions of) the above criteria.<sup>10</sup>

I am, then, advocating a change in perspective, relative to some recent discussions, when it comes to how we should approach the commitments of the linguistic approach. The focus should be, not on whether some claim along the lines of ‘necessary truths are truths guaranteed by the linguistic conventions’ is true or not, but on whether an explanatory project of a particular kind can be carried out.

This is, I should add, also how adherents of the linguistic approach saw things – at least in their best moments. In Carnap’s *Introduction to Semantics*, for instance, L-truth (‘truth based on logical grounds’) is explicated as truth based on the semantic rules of the relevant language. However, the suggestion is not that these rules *somehow* manage to fix or guarantee the truth of certain sentences. If it were, then we would indeed be right to ask, with Quine, how to assess whether or not this is so. Instead, what Carnap says is that a sentence is L-true *if it can be shown* to be true using only the semantic rules of the language, without referring to “facts” (1942: 78). The question then becomes whether the rules are in fact up for the job Carnap has envisaged for them.<sup>11</sup>

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<sup>10</sup>It is worth noting that Quine himself would presumably be unmoved by this. But that is because he would reject the idea that there is such a thing as necessary truth which calls for explanation.

<sup>11</sup>Carnap’s account in his post-syntax period is not always given the attention it deserves in discussions of the linguistic approach, but Philip Kitcher’s discussion of what he calls ‘conceptualism’ is a welcome exception (1984: ch. 4).

Russell once remarked that the advantages of postulating what we want ‘are the same as the advantages of theft over honest toil’ (1919: 71). It is natural, perhaps, to suggest that this applies to the linguistic approach as well. This charge, however, is unfair if the commitments of the approach are understood along the above lines – *honest toil is then very much needed*.<sup>12</sup>

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<sup>12</sup>In fact, the demands are arguably greater on the adherent of the linguistic approach than on those advocating a robustly realist view of modality. The realist, after all, can accept that there are brute facts about modality which we may have no way of accounting for (cf. D. Lewis 1986: 103).



## The Roots of the Linguistic Approach

In this chapter and the next I will argue that the linguistic approach, as it emerged in the writings of the logical positivists, is best understood as a variation on a fundamentally Kantian theme. More specifically, my contention is that it should be understood as a linguistic turn (inspired by Wittgenstein's *Tractatus*), with a strong emphasis on convention (inspired by Poincaré's geometric conventionalism), on what we may call Kant's 'subjective-constitutive strategy'.

There are therefore three main historical influences which we need to consider in order to get a firm grasp on how the linguistic approach is meant to work: i) Kant's subjective-constitutive strategy, ii) Wittgenstein's linguistic turn, and iii) Poincaré's geometric conventionalism. In this chapter I introduce these ideas and the resources they provide us with when it comes to addressing the problem of necessity. An overarching theme will be that both the linguistic turn and Poincaré's conventionalism can themselves be understood as instances of the subjective-constitutive strategy, and that these developments improve on Kant's original version in various respects. In the next chapter, then, I shall look at how these ideas came together in the writings of the logical positivists.

Thus, the purpose of this chapter is to introduce three important influences on the linguistic approach, and to explain how these ideas are meant to work when it comes to addressing the problem of necessity with which we

started. This will then feed back into our account of the practically workable commitments of the linguistic approach, since we shall then be able to say more about what an explanation in accordance with this approach can and cannot appeal to, without undermining the ideas that motivated it in the first place.

### 2.1 Kant's Subjective-Constitutive Strategy

Some may find it odd to include Kant as a key influence on the logical positivists, especially when the issue is how to think about necessity and the *a priori*.<sup>1</sup> After all, the rejection of the synthetic *a priori* came to be one of the central tenets of logical positivism. This contrast is indisputable (although it is disputable whether the positivists were faithful to *Kant's* analytic-synthetic distinction when voicing their opposition). But although the positivists disagreed with Kant on this issue of how to categorise our knowledge, there is, I shall argue, a deep affinity between Kant's strategy for explaining the possibility of synthetic *a priori* knowledge, and the positivists' strategy for explaining the possibility of analytic/logical knowledge.

Such a connection would not be very surprising since no one in the German speaking world at the time could have escaped Kantian influence. Indeed, it has become increasingly clear that Kant was an important early influence on some of the most prominent members of both the Vienna Circle and the Berlin Society. Schlick, Carnap and Reichenbach, for instance, were all brought up on (neo-)Kantian ideas.<sup>2</sup> Moreover, as I shall go on to argue,

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<sup>1</sup>The positivists tended not to distinguish these. It is sometimes said that they failed to see that a distinction could be drawn, but this is potentially unfair since they typically maintained that neither notion was rigorous enough for their purposes, and preferred to provide some explication for the (single) phenomenon they thought needed explaining.

<sup>2</sup>For more on Schlick's Kantian roots, see Coffa (1991: ch. 9) and Friedman (1983). Carnap acknowledges Kant as an early influence in his autobiography (1963a: 4) – an influence which is evident in his dissertation *Der Raum* (published as Carnap 1922). For more on this, see Richardson (1998: ch. 6) and Carus (2007: ch. 4). Reichenbach, finally, refers to himself as 'originally Kantian' (1936: 4), and his first major work (1920), though partly a discussion of the departures from Kant that the theory of relativity mandates, is as much a discussion of which Kantian elements should be retained.

the positivists may also have inherited some Kantian ideas via Poincaré and (more contentiously) Wittgenstein.

I should say that my aim in what follows is not to provide the best possible Kant exegesis (clearly a task for an entirely different occasion), but to highlight certain features of the Kantian framework which are important to the understanding of the linguistic approach.

### **Kant's Problem**

The *Critique of Pure Reason* was written to address the question of how synthetic *a priori* judgements are possible, and specifically how such judgements get their necessity and strict universality (Kant 1787: B3–4). Kant too, then, is concerned with the problem of necessity; for it is the fact that we have knowledge of necessities which leads him to search for an alternative, non-empirical, source of knowledge.

By the time he started working on the *Critique*, Kant had become convinced that this problem was unsolvable within the framework of his inaugural dissertation. There he had argued that ‘things which are thought sensitively are representations of things *as they appear*, while things which are intellectual are representations of things *as they are*.’ (Kant 1770: 2:392) Moreover, sensibility (i.e. the faculty which provides the basis for ‘thinking things sensitively’) was said to be that ‘in virtue of which it is possible for the subject’s own representative state to be affected in a definite way by the presence of some object.’ (1770: 2:392) Since intellectual representation does not involve being affected in this way, such representations are *a priori*.

Two years later, in his famous letter to Marcus Herz, Kant explains why he is now dissatisfied with this framework:

I silently passed over the further question of how a representation that refers to an object without being in any way affected by it can be possible. [...] if such intellectual representations depend on our inner activity, whence comes the agreement that they are supposed

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to have with objects – objects that are nevertheless not possibly produced thereby? (1772: 10:130–1)

This question, Kant thinks, could not be answered while retaining his earlier assumptions. The following three theses cannot all be maintained:

(INDEPENDENCE.) The intellect provides us with representations of things as they are in themselves (independently of how they are presented to us).

(APRIORITY.) These representations are not due to us being affected by the objects.

(JUSTIFICATION.) We are justified in thinking that these representations agree with what these objects are actually like.

The problem, which I shall refer to as ‘the problem of inexplicable agreement’, is that (INDEPENDENCE) and (APRIORITY) together make it inexplicable why there should be agreement between our representations and how things actually are, thereby undermining (JUSTIFICATION). Indeed, there is, potentially, a more fundamental problem here: If we are not affected by the objects, how do we get to represent them at all (rightly or wrongly)?<sup>3</sup>

### The Copernican Revolution

Kant’s response, of course, was not to reject either (APRIORITY) or (JUSTIFICATION), but (INDEPENDENCE), maintaining instead that ‘we can cognize of things *a priori* only what we ourselves have put into them.’ (1787: Bxviii) This is the subjective element of what I am calling Kant’s ‘subjective-constitutive strategy’: *A priori* knowledge is essentially *self-knowledge*. More accurately, it is reflective knowledge of what we ourselves

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<sup>3</sup>These problems clearly have a lot in common with what is sometimes referred to as ‘Benacerraf’s problem’ (cf. Benacerraf 1973).



contribute in cognition and thought. When I know *a priori* that any object I encounter *must* have certain features, what I really know is something about the subjective principles that determine what can be experienced and thought by me. I know, that is, that the objects I encounter will be in accordance with the subjective principles which make experience and thought possible:

There is therefore only one way possible for my intuition to precede the actuality of the object and occur as an *a priori* cognition, *namely if it contains nothing else except the form of sensibility, which in me as subject precedes all actual impressions through which I am affected by objects*. For I can know *a priori* that the objects of the senses can be intuited only in accordance with this form of sensibility. (1783: 4:282)

We can have *a priori* knowledge of geometry, for instance, because such knowledge concerns, not a domain of independently existing geometrical objects, but rather the principles which structure our experiences of outer objects and make such experiences possible. I know that I won't encounter a counterexample to (for Kant) Euclidean geometry, because this geometry provides the structure which all my outer experiences must conform to.

### Obstacles to Genuine Necessity

Suppose that we grant both that there are such subjective principles which govern what can be experienced and thought, and that we can have some sort of transcendental insight into the workings of these principles. Would this ensure that what we know on this basis is *genuinely necessary*? Many have thought not. Here, for instance, is Bertrand Russell:

The thing to be accounted for is our certainty that the facts must always conform to logic and arithmetic. To say that logic and arithmetic is contributed by us does not account for this. Our nature is just as much a fact of the existing world as anything, and there

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can be no certainty that it will remain constant. It might happen, if Kant is right, that to-morrow our nature would so change as to make two and two become five. This possibility seems never to have occurred to him, yet it is one which utterly destroys the certainty and universality which he is anxious to vindicate for arithmetical propositions. (1912: 134–5)

Kant, Russell thinks, is at most able to explain why we do not *presently* experience counter-arithmetical states of affairs. But unless we can be confident that our cognitive faculties must remain fixed, we are not entitled to reject the hypothesis that we might nevertheless on day encounter such states of affairs in experience. Let us call this ‘the objection from alteration’.

Moreover, even if we suppose that the nature of our minds must remain fixed, Kant’s position, as Russell understands it, is hardly capable of accounting for the genuine necessity and strict universality of *a priori* propositions. Nothing has been said, after all, to rule out the possibility that there could *be* counter-arithmetical states of affairs, but that, because they are counter-arithmetical, they cannot be *experienced by us*. Russell, touches upon this concern too, when he writes:

Reflection, moreover, seems to make it clear that, if there is any truth in our arithmetical beliefs, they must apply to things equally whether we think of them or not. [...] Thus, Kant’s solution unduly limits the scope of *a priori* propositions’ (1912: 135–6).

Let us call this ‘the objection from restriction’.

A variation on this gives us a third objection: If the constitution of our minds does not just *filter* the input so that only what is in accordance with arithmetic gets through, but *actively structures* the input so as to be in accordance with arithmetic, then the worry arises that while we can only experience things to be in accordance with arithmetic, they might *really be* counter-arithmetical. This is reminiscent of one of Barry Stroud’s objections to the use of ‘transcendental arguments’ in the analytic tradition:

The skeptic can always very plausibly insist that it is enough to make language possible if we *believe* that *S* is true, or if it looks for all the world as if it is, but that *S* needn't actually be true. (1968: 255)

Let us call this 'the objection from distortion'.

At this point, Kant's strategy seems quite unsuited to account for the strict universality and genuine necessity of *a priori* propositions. Take a proposition of the form 'all *X* are *Y*'. The objection from restriction forces us to retreat to: 'all *X* capable of being perceived by us are *Y*'. The objection from distortion takes us to: 'all *X* capable of being perceived by us seem to be *Y* in experience'. And the objection from alteration, finally, leaves us with: 'all *X* capable of being perceived by us seem to be *Y* to us unless there is a change in our cognitive faculties'.

### The Subjective-Constitutive Strategy

These objections, however, rest on an oversimplified interpretation of Kant's ideas. While this interpretation correctly identifies the importance of subjective principles, it neglects the crucially *constitutive* role which these are meant to play.

Many interpreters of Kant have been puzzled by the fact that he not only argues that since space is a form of intuition, we cannot assume that things in themselves are spatial, but instead makes the stronger claim that things in themselves must be *non-spatial*, and that space is a *mere* form of intuition:

If we remove our own subject or even only the subjective constitution of the senses in general, then all constitution, all relations of objects in space and time, indeed space and time themselves would disappear, and as appearances they cannot exist in themselves, but only in us. (1787: A42/B59)

Shouldn't he rather have remained *agnostic* about the objective reality of space, and thus allowed for the so-called 'neglected alternative' (Hogan

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2010: 30) – namely that space is *both* a form of intuition *and* objectively real?

The answer is that this alternative is not neglected. Rather, it is precisely by denying that space is objectively real that Kant thinks he can establish the genuine necessity of for instance geometry:

Pure mathematics, and especially pure geometry, can have objective reality only under the single condition that it refers merely to objects of the senses, with regard to which objects, however, the principle remains fixed, that our sensory representation is by no means a representation of things in themselves, but only of the way in which they appear to us. From this it follows, not at all that the propositions of geometry are determinations of a mere figment of our poetic phantasy, and therefore could not with certainty be referred to actual objects, but rather, that they are valid necessarily for space and consequently for everything that may be found in space, because space is nothing other than the form of all outer appearances, under which alone objects of the senses can be given to us. (1783: 4:287)

It is *because* space is ‘nothing other than the form of all outer appearances’, that the propositions of geometry are ‘valid necessarily’ rather than ‘determinations of a mere figment of our poetic phantasy’. And the best way to appreciate Kant’s reasoning here is to consider precisely worries like those introduced in the previous section.

One such worry was that Kant’s strategy can only guarantee that, e.g., any triangle *encountered in experience* must conform to the theorems of (Euclidean) geometry, but that there might *really be* counterexamples to those theorems. If, however, space is a *mere* form of intuition, then this worry makes no sense. The domain of spatial objects cannot be greater than the domain of objects that I can encounter in outer intuition. Instead, the principles that govern the form of outer intuition are at the same time *constitutive* of the spatial objects, so that the notion of a spatial object is automatically a notion of an object capable of being experienced.

This is the point of Kant's insistence that 'the conditions of the **possibility of experience** in general are at the same time conditions of the **possibility of the objects of experience**, and on this account have objective validity in a synthetic judgment *a priori*.' (1787: A158/B197) Indeed, Kant is quite explicit that this is what is meant to block worries like those considered above:

If this subjective condition regarding form were not at the same time the universal a priori condition under which alone the object of this (outer) intuition is itself possible; if the object (the triangle) were something in itself without relation to your subject: then how could you say that what necessarily lies in your subjective conditions for constructing a triangle must also necessarily pertain to the triangle in itself? (1787: A48/B65)

It is, then, by making the principles *constitutive of the relevant objects* that Kant thinks he can solve, or rather dissolve, the problem of inexplicable agreement. The point of denying that, e.g., the triangle is something "in itself", is that this gets rid of the idea that there is a subject-independent standard to which our theorems of geometry must agree – thereby dissolving the worry that the verdicts we reach concerning triangles via *a priori* reasoning could still fail to agree with what triangles are actually like. Briefly put, the focus on *subjective* principles is meant to allow for the possibility of knowledge without input from external objects via the senses, and making these principles *constitutive* is meant to remove the notion of an external, subject-independent standard of correctness which judgements reached by reflecting on these principles could nevertheless fall short of.

We can summarise the main tenets of Kant's subjective-constitutive strategy as follows:

- (A) The starting-point is a problem of agreement between our judgements and reality in the absence of any external input.

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- (B) The answer takes place against the backdrop of a distinction between form and matter in experience and thought.
- (C) The formal principles are said to be contributed by the subject rather than the objects of knowledge. Therefore, in so far as the form in concerned, there is no need from input from the objects.
- (D) The subjective-formal principles are said to be *constitutive* of the relevant objects. That is, the objective (subject-independent) reality of these objects is denied, meaning that there is no subject-independent standard of correctness which our judgements could fall short of. This is meant to dissolve the problem of inexplicable agreement.

### **Difficulties with the Kantian Answer**

Before considering how these ideas were transformed by Poincaré and (arguably) Wittgenstein and later came together in the logical positivists' account of logical/necessary truth, I shall highlight four difficulties with the specifics of Kant's answer. This will allow us to see how the developments that lead to the linguistic approach might be said to improve on Kant.

Firstly, it is fair to say that Kant's claim that space and time are ideal has proved difficult to swallow. While Kant was eager to stress that his transcendental idealism is compatible with 'empirical realism' (1787: A370), others, it turned out, found this less comforting than he himself did. In addition to the sheer radical nature of the idea that space and time disappear once the subject is removed from the equation, there are other potential problems: Can we, for instance, make sense of sensation as a kind of input from objects without the notion of spatial distinctness? There is, of course, a vast amount that could be said here, but on the face of it at least, idealism about space and time is an obstacle to accepting the Kantian position.

A second but related problem concerns how Kant argues for this thesis. At least on the above interpretation the strategy is dialectically peculiar. Kant is relying on the claim that e.g. geometry could be genuinely necessary

*only if* space is ideal, and thus argues from the genuine necessity of geometry to the ideality of space. But we are given little reason to prefer *modus ponens* over *modus tollens* here, and as a consequence, it is not clear what reason we have to think that Kant is right about the ideality of space.<sup>4</sup>

Thirdly, while Kant's subjective-constitutive strategy might provide a response to the objections from restriction and distortion, it is not obvious that he has a satisfactory response to the objection from alteration. Russell asked, recall, for reassurance that we will never wake up to find that  $2+2$  has become 5. The subjective-constitutive strategy does provide a *potential* response: While we may not be able to rule out a change in our cognitive faculties, different subjective principles would be constitutive of different objects, and so could not give rise to counterexamples to what we now take to be necessary.

However, though such a response may be possible, it is far from clear that it is plausible in Kant's case. It is not clear, for instance, that a form of outer intuition governed by a different geometry would simply be constitutive of different objects. Even if Kant had been right about Euclidean geometry providing this form, he was never, it seems, in a position to rule out that this might change in such a way that, say, hyperbolic geometry takes over this role. And why shouldn't we say that, if so, then we would encounter counterexamples – in the form of for instance non-Euclidean triangles – to what was initially held to be necessary?

Fourthly, although Kant's appeal to *subjective* principles may succeed in explaining how we can do without external input in the case of knowledge of necessities, we are left with the question of how we get to have insight into the workings of these subjective principles. How, that is, are we supposed to know what the subjective-constitutive principles are? Why isn't this just an empirical question that should be settled by something like experimental psychology? Kant's strategy requires something like an epistemology of "transcendental psychology", but it is not clear what this would be.

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<sup>4</sup>Paul Guyer criticises Kant along these lines (cf. 1987: 362–9; 2008: 91–5 2014: 74).

## 2.2 Poincaré's Geometric Conventionalism

In the next chapter I will argue that the linguistic approach to modality should be viewed as a version of the subjective-constitutive strategy. Clearly, though, there are significant differences between Kant's position and that arrived at by the logical positivists, and one obvious such difference concerns the positivists' appeal to the notion of *convention*.

The positivist largely inherited the emphasis on convention from Poincaré's work on the nature of geometry. In this section I argue that although Poincaré of course departs from Kant in a number of ways, his conception of geometry does not depart from the subjective-constitutive strategy as such. In fact, the appeal to convention is particularly well-suited to the needs of this strategy – allowing us to address some of the problems just outlined regarding the specifics of Kant's solution.

### Poincaré and Kant

Although Poincaré's conception of geometry differs from Kant's in a number of ways, the overall context is 'conspicuously Kantian' (Ben-Menahem 2006: 42). The conclusion to chapter 4 of *Science and Hypothesis* is particularly helpful when it comes to identifying the Kantian as well as the anti-Kantian elements of his position:

Geometry [...] is not concerned with natural solids: its object is certain ideal solids, absolutely invariable, which are but a greatly simplified and very remote image of them. The concept of these ideal bodies is entirely mental, and experiment is but the opportunity which enables us to reach the idea. The object of geometry is the study of a particular "group";<sup>5</sup> but the general concept of a group pre-exists in our minds, at least potentially. It is imposed on us not

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<sup>5</sup>The context here is Klein's 'Erlangen program' (1872). The idea is to associate each geometry with a group of transformations on a geometrical space, so that the geometrical properties are those which are invariant under such transformations. For more on this, see e.g. Brannan, Esplen and Gray (2012: ch. 8).



as a form of our sensitiveness, but as a form of our understanding; only, from among all possible groups, we must choose one that will be the *standard*, so to speak, to which we shall refer natural phenomena. (1902: 70)

The talk about geometry as concerned with ideal bodies whose concept is entirely mental and, moreover, forms of sensitiveness and understanding clearly echoes Kant. Like Kant, Poincaré aims to explain the peculiar status of geometry by emphasising the *active* role of our cognitive faculties. Geometry is saved from being 'approximate and provisory' because it is not an (empirical) study of solid bodies, but rather the study of a group that pre-exists in *our minds* (at least potentially) and to which *we* refer natural phenomena. 'Space', he writes in his preface, 'is another framework which we impose on the world.' (1902: xxv)

There are, however, two clear departures from Kant here: Firstly, geometry is relocated from our sensitivity to our understanding. Secondly, and more significantly for our purposes, we are said to have a *choice* when it comes to which geometry we shall 'impose on the world'. Because of this, geometry cannot be synthetic *a priori*, and so Poincaré concludes that a new epistemological category is needed: '*The geometrical axioms are therefore neither synthetic a priori intuitions nor experimental facts. They are conventions.*' (Poincaré 1902: 50)

Poincaré's claim that we have a choice when it comes to geometry is, of course, based on the discovery of non-Euclidean geometries.<sup>6</sup> But the point is not simply that there are *consistent* alternatives to Euclid. This, after all, is not bad news for Kant; if denying a Euclidean postulate invariably led to contradiction, this would render geometry *analytic*, at least by one of Kant's criteria (1783: 4:267).

Poincaré's point, rather, is that if the geometrical axioms were synthetic *a priori*, they would be 'imposed on us with such a force that we could

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<sup>6</sup>It is worth noting, though, that Poincaré holds this choice to be quite restricted, since he assumes that 'the movement of an invariable figure is possible' (1902: 47), and so thinks (we would say mistakenly) that we must use a geometry of constant curvature.

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not conceive of the contrary proposition, nor could we build upon it a theoretical edifice.’ (1902: 48) One lesson of Poincaré’s famous ‘hyperbolic world’ thought experiment, however, is that, not only can we *consistently* deviate from Euclidean geometry:

there is nothing [...] to prevent us from imagining a series of representations, similar to our ordinary representations, but succeeding one another according to laws which differ from those which we are accustomed to. We may thus conceive that beings whose education has taken place in a medium in which those laws would be so different might have a very different geometry than us. (1902: 64–5)

Had things been different, it is quite conceivable, Poincaré thinks, that we would have used a different geometry. Consequently, it cannot be synthetic *a priori* in Kant’s sense. Still, Poincaré insists that we should not say that a different geometry would be *empirically confirmed* in such a scenario, and it is Poincaré’s line of reasoning here that allows us to see his conventionalism as an instance of the subjective-constitutive strategy.

### **Against the Empiricist Conception of Geometry**

Poincaré’s complaint that ‘if [geometry] were experimental, it would only be approximate and provisory’, can hardly be expected to convince someone who thinks that geometry is an empirical matter. As Yemina Ben-Menahem puts it: ‘It is almost as if he said “Were it empirical, it would be empirical.”’ (2006: 46)

He does, however, have other arguments. In particular, he is adamant that no experiment could ever force us to accept a particular geometry (1902: 75). It is a mistake, for instance, to think that we could settle this by experimenting on light rays:

If Lobatschewsky’s geometry is true, the parallax of a very distant star will be finite. If Riemann’s is true, it will be negative. [...] But what we call a straight line in astronomy is simply the path of a ray

of light. If, therefore, we were to discover negative parallaxes, or to prove that all parallaxes are higher than a certain limit, we should have a choice between two conclusions: We could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line. (Poincaré 1902: 72–3)<sup>7</sup>

Now, as many have pointed out,<sup>8</sup> this argument is puzzling since it appears to be relying on Duhemian considerations which are plainly too general to establish a specific thesis about geometry. Moreover, such considerations apply to subjects that are *paradigmatically empirical*, and so the fact that they can be applied here is not much of an argument against the empiricist conception of geometry.

It is clear, however, that Poincaré had something different in mind. Having argued, using essentially Duhemian considerations, that some principles of mechanics could be regarded as conventions, he considers precisely the charge that he has failed to secure a special status for geometry, but replies:

Such a conclusion would be illegitimate. The experiments which have led us to adopt as more convenient the fundamental conventions of geometry refer only to bodies which have nothing in common with those that are studied by geometry. [...] On the other hand, the fundamental conventions of mechanics and the experiments which prove to us that they are convenient, certainly refer to the same objects or to analogous objects. (1902: 136–7)

The real reason, then, for denying that geometry is empirical, is, not that we cannot experiment on geometrical objects *in isolation*, but that we cannot experiment on geometrical objects *at all*. 'Experiments only teach us the relations of bodies to one another. They do not and cannot give us the relations of bodies and space, nor the mutual relations of the different

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<sup>7</sup>According to legend, Gauss attempted to determine the geometry of space by experimenting on light rays transmitted between three mountain tops. It is, however, doubtful that this happened (cf. A.I. Miller 1972).

<sup>8</sup>E.g. Friedman (1995: 301–2), Ben-Menahem (2006: 57–8), and Stump (2015: 46).

parts of space.’ (Poincaré 1902: 79) We can, of course, go about measuring physical objects, but, since ‘physical entities do not come labeled with their geometrical identities’ (Ben-Menahem 2006: 55), we would then have to ask: ‘How shall we know that any concrete magnitude which I have measured with my material instrument really represents the abstract distance?’ (Poincaré 1902: 74)

### Conventionalism

Part of the motivation for Poincaré’s conventionalism, then, is his conviction that the subject-matter of geometry (the ideal solids and abstract distances) are forever beyond the reach of experiment. Since, then, this is settled neither by experience nor by our form of intuition, we have no choice but to settle it by convention – based, if possible, on considerations of convenience.

Now, this conclusion is consistent with a rather limited form of “pragmatic” conventionalism, according the choice must be settled by convention since we could never *know* what the true geometry is – even though there is a fact of the matter. Poincaré, however, explicitly rejects this idea, insisting that ‘one part of space is not by itself and in the absolute sense of the word equal to another part of space’ (Poincaré 1908: 418).<sup>9</sup> Instead, the very notion of equal distance is constituted by our choice of geometry. Consequently, Poincaré’s conventionalism takes on a more radical form:

What, then, are we to think of the question: Is Euclidean geometry true? It has no meaning. We might as well ask if the metric system is true, and if the old weights and measures are false; if Cartesian co-ordinates are true and polar co-ordinates false. One geometry cannot be more true than another; it can only be more convenient.  
(1902: 50)

Thus, when Poincaré says that geometry is a matter of convention, he is not just saying that since we can have no *evidence* in favour of a particular

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<sup>9</sup>This rejection seems to be based mainly on roughly speaking verificationist arguments (cf. in particular Poincaré 1908: 418).

geometry, we are equally justified in making any choice we see fit; he is saying that – as with our choice of units or coordinate system – there is no standard of correctness which our choice of geometry could fall short of. There is, therefore, no room for a problem of inexplicable agreement concerning how we know that the geometry we use is the objectively correct one.

This is why Poincaré's conventionalism should be seen as a version of the subjective-constitutive strategy. Both Kant and Poincaré maintain that the reason why geometry isn't 'approximate and provisory', is that it is a framework which the subject imposes on the world, and they both deny that it makes sense to worry that the geometry we impose on the world could fail to agree with the *real* geometry. The reason is, in both cases, that the very notion of a *real* (subject-independent) geometry is rejected.

We can summarise Poincaré's argument for geometric conventionalism as follows:

- (1) Space does not come with a built in metric: 'One part of space is not by itself and in the absolute sense of the word equal to another part of space' (Poincaré 1908: 418).
- (2) So, if there is such a thing as an objectively correct metric/geometry, it would have to be pinned down by the relations holding between objects existing in space: 'Space is in reality amorphous and the things which are therein alone give it a form.' (Poincaré 1908: 417)
- (3) But since these objects and the relations between them do not come with built in geometric identities, they do not bear on this issue: 'How shall we know that any concrete magnitude which I have measured with my material instrument really represents the abstract distance?' (Poincaré 1902: 74)
- (4) So, there is no such thing as the objectively correct metric.
- (5) So, the metrical properties of space are of subjective origin.
- (6) But, pace Kant, we are not forced by the structure of outer intuition to impose any *particular* metric on the world.

- (7) So, the metric, and therefore the choice of geometry, is a matter of convention.

### Convention and the Subjective-Constitutive Strategy

I maintain, then, that although Poincaré's geometric conventionalism is obviously different from Kant's conception of geometry, it remains an instance of the subjective constitutive-strategy. In fact, the notion of convention is remarkably well-suited to the needs of this strategy, since it nicely incorporates both the subjective and the constitutive aspects. Consider, for instance, how Richard Creath characterises this notion:

To say that postulates are laid down by convention commits one to the idea that there are alternative postulates that could have been chosen, but were not. It commits one likewise to the idea that no further epistemic justification for the choice of postulates is required. Conventions are not designed to reflect antecedent and independent facts; if they were thus designed one would have to show that they had done so. (1992: 147)

Thus, to say that something is a matter of convention is to say: i) that it is something which *we* have decided upon (perhaps implicitly), and ii) that in making this choice we are not subject to any *external standard of correctness*. It is precisely this combination of subjective origins and lack of external standards of correctness that is characteristic of the subjective-constitutive strategy.

Moreover, the appeal to convention allows us to avoid some of the shortcomings related to the specifics of Kant's proposal. In particular, we have a much better grip on what it takes for something to be a matter of convention, than we have on what it takes for something to be a transcendental form/category of intuition/reason. We noted above that Kant provides little argument in favour of the idea that space is a transcendental form of intuition. Moreover, it is far from clear how we might go about arguing for

this. It is comparatively clear, however, how to argue that something is a matter of convention: We do this by arguing that there are several equally valid alternatives. Indeed, C. I. Lewis went as far as saying that this is our *only* way of knowing that something is of subjective origin:

Such legislation can be recognized as our own act because the a priori principle which is definitive [...] *has alternatives*. [...] if what is a priori sprang from a transcendent mind, acting in unalterable ways, it never could be known to be our own creation or distinguished from those facts of life which are due to the nature of the independently real. (1929: 232)

Conventionalism, we may say, provides us with a particularly perspicuous form of the subjective-constitutive strategy.

Poincaré's account also improves on Kant's in a different way. Although he was no transcendental idealist, Poincaré can be said to advocate the "ideality" of space *insofar as it concerns geometry*. He agrees with Kant, for instance, that a triangle is not something "in itself", in the sense that it does not make sense to worry that our geometry misclassifies something as a triangle even though it *really* isn't. In Poincaré's case, however, this does not involve denying the objective reality of *space*, but only involves denying the objective reality of the *metric* of space. Poincaré's "idealism", therefore, is much less severe than Kant's.

## 2.3 Wittgenstein's Linguistic Turn

So far in this chapter I have introduced the subjective-constitutive strategy as a possible strategy for dealing with the problem of necessity, and I have argued that the appeal to convention that we find in Poincaré's work on geometry is well-suited to the needs of this strategy. The guiding thought, recall, is that these ideas make their way into the linguistic approach (as developed by members of the Vienna Circle), and that we can use this

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to provide a more definite account of the distinctive practically workable commitments of this approach.

In this section, I consider another key influence on the linguistic approach: the linguistic turn that we find in Wittgenstein's *Tractatus*. I will argue that, like Poincaré's conventionalism, this turn can be understood as a continuation of the subjective-constitutive strategy, and that it allows us to avoid some of the problems with the specifics of Kant's position.

This means that I will be outlining a broadly speaking Kantian interpretation of the Tractarian conception of logic. However, as with all things *Tractatus*, this is bound to be controversial, and I do not think that we have adequate grounds for advancing this as anything like 'the one true interpretation'. Rather, I shall be content to argue that there is a possible interpretation according to which the Tractarian account of logic is a linguistic version of the subjective-constitutive strategy. In the next chapter, moreover, I shall suggest that, independently of whether this was what Wittgenstein had in mind, it was something like this interpretation which informed the logical positivists and made it into the linguistic approach.

### **Logic as the Form of Linguistic Representation**

Kantian readings of the *Tractatus* have been around for some time. Erik Stenius, for instance, wrote in his 1960 book that: 'Wittgenstein was in essential respects a Kantian philosopher; his anti-Kantianism meant only that he – like other Kantians – transformed the system of Kant and thus created a Kantianism of a peculiar kind.' (1960: 214)<sup>10</sup> Although I'm not convinced that it is helpful to describe the *Tractatus* as a form of Kantianism (for this, the discontinuities are perhaps a few too many), I do agree that there is a level at which the *Tractatus* can be read as a transformation of Kantian ideas.

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<sup>10</sup>Others who find significant Kantian elements in the *Tractatus* include: Pears (1971: 45–7; 1987: ch. 1) Glock (1992; 1996; 1997) and Hacker (1989: 22–3; ch. 4).



The project which set Wittgenstein on the path which led to the *Tractatus* was to ‘both characterize the notion of logical truth and explain its nature.’ (Potter 2009: 49) It does perhaps not require too much of a squint to formulate Wittgenstein’s initial question as: ‘How are logically true propositions possible?’, and although there is very little epistemology in the *Tractatus*, it is clear that Wittgenstein was concerned with the question of how we can recognise logical truths as such.<sup>11</sup>

From early on, moreover, Wittgenstein thought that key to these questions was to provide ‘an account of the structure of propositions’ (Potter 2009: 49). This appeal to structure already suggests a Kantian analogy: Wittgenstein’s answer takes place against the background of a distinction between the formal/structural features present in all linguistic representation and what we talk about in such representation. And in parallel with Kant’s proposal, he holds that logic is special because it concerns these formal features alone (2.18–2.2; 6.12). ‘Logic’, that is, ‘comprises the most general preconditions for the possibility of representation.’ (Glock 2013: 573)

Thus, as Kant maintained that anything met with in outer intuition must conform to Euclidean geometry because this geometry provides the form of outer intuition and so makes experience possible, so Wittgenstein maintains that anything represented in language must be ‘logical’ because logic provides the form of linguistic representation and so makes such representation possible: ‘To present in language anything which “contradicts logic” is as impossible as in geometry to present by its co-ordinates a figure which contradicts the laws of space’ (1922: 3.032).<sup>12</sup>

Admittedly, this remark is somewhat puzzling since Wittgenstein would not want to deny that it is possible to formulate contradictions, and so it seems that we *can*, in a sense, present something which contradicts logic in language. However, the point, I take it, is that there are certain features

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<sup>11</sup>Both the truth-tables (4.31–4.46) and, in particular, the ab-notation (6.1203) can be seen as addressing this question.

<sup>12</sup>I am using the Ogden translation unless otherwise noted.

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which all linguistic representations must have, and that there can be no linguistic representation which contradicts *this*. These features then ensure that contradictions close off the entire logical space (4.463), which is why they are false no matter what is the case (4.464). Correspondingly, these formal features are also shown in the fact that certain truth-functional constructions yield tautologies – propositions which leave the entire logical space open (4.463) and are therefore true independently of what is the case (4.464).

According to Wittgenstein, then, the peculiar status of logical truths is due to them being guaranteed to be true by the formal features which all linguistic representations must have. The form of linguistic representation ensures that nothing illogical could be represented as true.

### Uncritical Realism?

Here, though, we also encounter a potential departure from Kant's strategy. It is crucial for Kant, after all, that the form of experience is of *subjective* origin, since 'we can cognize of things *a priori* only what we ourselves have put into them.' (1787: Bxviii) Wittgenstein, however, is explicit that the form of linguistic representation is also, in some sense, the form of *the world*: 'The fact that the propositions of logic are tautologies, *shows* the formal – logical – properties of language, of the world.' (6.12) 'The logical propositions describe the scaffolding of the world, or rather they present it.' (6.124)

Even those who are sympathetic to the idea that there are Kantian elements in the *Tractatus*, and its account of logical necessity in particular, have tended to think that the analogy breaks down at precisely this point:

Although for the *Tractatus* necessity is a matter of logical syntax, its ultimate source is not subjective, the structure of the mind or of language, but lies in a metaphysical essence of reality, albeit an ineffable one. (Glock 1997: 296)

That is to say: although logic arises from the formal features which all linguistic representation must exhibit, the source of these features lies, in turn, in the metaphysical structure of the world as laid out in the 1's and 2's. Pears is particularly explicit about this, and quite rightly labels this conception 'uncritical realism' (1987: 9), in direct opposition to Kant and the subjective-constitutive strategy:

The idea is that in all our operations with language we are really running on fixed rails laid down in reality before we even appeared on the scene. Attach a name to an object, and the intrinsic nature of the object will immediately take over complete control and determine the correct use of the name on later occasions. Set up a whole language in this way, and the structure of the fundamental grid will inexorably dictate the general structure of the logical system. (Pears 1987: 10)

## A Different Interpretation

However, this interpretation is not easy to square with what Wittgenstein says elsewhere. He insists, for instance, that 'theories which make a proposition of logic appear substantial are always false' (6.111), but if Glock and Pears are right, then there is a clear (though admittedly ineffable) sense in which propositions of logic are indeed substantial, since they are concerned (again, in some ineffable sense) with the metaphysical structure of the world. This has fuelled an alternative interpretative tradition, and going down this route, I shall argue, allows us to push the Kantian analogies further, and to see Wittgenstein's conception of logic as an instance of the subjective-constitutive strategy.

According to this interpretation, it is a mistake to think that the form of representation is dictated by a metaphysical structure outside of our language. Indeed, Hide Ishiguro (1969) and Brian McGuinness (1981), and more recently Marie McGinn (McGinn 2006), have all argued that Wittgenstein should not be read as doing metaphysics in the traditional sense in the opening 1's and 2's of the *Tractatus*:

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It was not Wittgenstein's intention to base a metaphysics upon logic or the nature of our language. He was not saying that there is something by which our grammar is determined, and therefore he did not try to infer features of the world from our language. (McGuinness 1981: 62)

Rather, they argue, the seemingly metaphysical picture (of objects, states of affairs etc.) must itself be understood from *within language*: 'Remarks that purport to be about the essential structure of the world are seen, on reflection, to be nothing more than a description of what is essential to a system for representing possible states of affairs in propositions' (McGinn 2006: 159). This 'has nothing to do with metaphysics and everything to do with the logical order of a system of representation, or with everything that is essential to the rules whereby language is projected onto reality.' (McGinn 2006: 159)

For example, 'an object in the Tractatus which is the reference of a name or simple sign can be viewed as simply the truth-value potential of a certain expression.' (McGuinness 1981: 65) That is to say, the criterion for being an object is a *language-internal* one. An object is the meaning of a particular kind of linguistic expression which functions within language in a particular way. There is no need to invest the objects with any further metaphysical significance, and so, pace Pears, they cannot be viewed as dictating the correct use of names from the outside.

In favour of this view, we may note that Wittgenstein says that 'if everything in the symbolism works as though a sign had meaning, then it has meaning.' (3.328)<sup>13</sup> Though this can hardly be said to be conclusive evidence, it is not straightforward to reconcile this remark with a robustly

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<sup>13</sup>This remark has an interesting history which, to some extent, supports the McGuinness-Ishiguro interpretation. In the original German (and the Pears-McGuinness translation) there is no reference to *the symbolism*. This was explicitly added by Wittgenstein, and he wrote to Ogden: 'Here I made the translation more explicit than the German text.' (Wittgenstein 1973: 25) As I understand him, what he wants to make explicit is precisely that if by all *language-internal* criteria a sign has meaning, then there is no further question as to whether it does have meaning.

realist construal of the “metaphysics” of the *Tractatus*. Such a construal would seem to bring with it (as it typically does) the possibility of a certain kind of scepticism: Why couldn't it happen that everything seemed to be in perfect order within our language, but that we were nevertheless failing to latch on the objects, or failing to use the signs in accordance with the actual possibilities for combining objects?

Clearly, much more would have to be said in order to satisfactorily decide between these interpretations. My aim, however, is the more modest one of arguing that if we adopt this more anti-metaphysical reading, then we can push the Kantian analogy further than Glock and Pears do.

### ‘Logic Is Transcendental’

The key point is that against the background of the McGuinness-Ishiguro interpretation, the idea that logical propositions mirror the form of the world does not constitute a fundamental break with the Kantian strategy. Kant, after all, would be happy to say that the form of outer intuition is mirrored in the form of the world as long as this is understood to be the *phenomenal* world. And if, now, ‘the form of the world’ in the *Tractatus* is, in accordance with the McGuinness-Ishiguro interpretation, made sense of via *language-internal* considerations – via, that is, considering how expressions behave within the symbolism – then we are here dealing with something akin to Kant's phenomenal world. That is to say: the world whose form is mirrored in logic is already a *linguistically representable world*. As Wittgenstein famously puts it: ‘the limits of the language (*the* language which I understand) mean the limits of *my* world.’ (5.62)

The idea that the Tractarian world should be understood along the lines of Kant's phenomenal world does, however, call for some serious qualification. It is quite clear that the Tractarian position differs significantly from Kant's since there is no room, in the *Tractatus*, for anything like Kant's *noumenal* world (this, I take it, is the point of remarks such as 5.64). Still, the point I am making stands: The form of the world, given the anti-

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metaphysical reading, is a notion which only makes sense insofar as the world is approach via language (compare: the geometry of the world is a notion which only makes sense insofar as the world is approached via outer intuition).

Once the idea of the form of the world is conceived along these lines, we may indeed say that ‘logic is transcendental’ (6.13) in something quite like Kant’s sense. Logic concerns the principles that govern how the subject represents the world in language and make such representation possible. These principles may be said to be subjective, not because we can view logic as something the subject imposes on the world (that would call for a conception of something like a noumenal world after all, and this we cannot have), but in the sense that they are intelligible only from the subject’s point of view – via considerations that are internal to the symbolism. This might be what Stenius has in mind when he writes:

For Wittgenstein, too, the form of experience is ‘subjective’ in the transcendental sense, the metaphysical subject being the ‘subject’ which uses and understands language, and which must be distinguished from the empirical self, which is part of the world describable in language. (1960: 220–1)

It is noteworthy, moreover, that ethics too is said to be transcendental (6.421), and that in the *Notebooks*, we find Wittgenstein elaborating on this as follows:<sup>14</sup> ‘Good and evil only enter through the *subject*. And the subject is not part of the world, but a boundary of the world.’ (1979a: 79; emphasis in original). Although, the *Notebooks* (being notebooks) must be used with some care, this does support the idea that logic too, in some sense, ‘only enters through the subject’.

I hope to have shown, then, that if we understand the “metaphysics” of the *Tractatus* along the lines of Ishiguro, McGuinness and McGinn, then

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<sup>14</sup>Admittedly, there are a number of textual complications here. Though the English translation of the *Notebooks* has Wittgenstein saying that ethics is transcendental, this appears to be a mistranslation since the German has ‘transcendent’ (1979a: 79). In the *Tractatus*, however, he does indeed use ‘transzendental’.

we can push the analogy between Kant's explanation of the synthetic *a priori* and Wittgenstein's explanation of logical truth further than is typically suggested. Indeed, we have gone some way towards understanding Wittgenstein's approach as an instance of the subjective-constitutive strategy: According to the *Tractatus* (given the present interpretation), logical necessity is a product of the form of linguistic representation. This form, moreover, is not something which is imposed on our language from without, but something which arises from language-internal considerations having to do with the representational activities of the subject.

In the next section I argue that Wittgenstein's 'fundamental thought' points to a further parallel between the *Tractarian* conception of logic and the subjective-constitutive strategy – a parallel which, as we shall see in the next chapter, becomes particularly important to the logical positivists and the linguistic approach.

## The Fundamental Thought

In the discussion of Kant, we considered the worry that even if we concede that anything encountered in intuition (as it is presently structured) must have certain features, there might still be counterexamples to what we take to be necessary. Kant's response here was his transcendental idealism: The subjective principles are constitutive of the objects in question (e.g. spatial objects), and so it does not make sense to worry, say, that although triangles presented to us must have angles adding up to 180 degrees, there might *really be* triangles which don't have this property.

Is there a parallel move in the *Tractatus*? There has been quite a lot of discussion as to whether the *Tractatus* is a work of transcendental idealism.<sup>15</sup> I shall, however, focus on a rather different issue than these discussions do, namely Wittgenstein's 'fundamental thought', namely that 'the

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<sup>15</sup>See Williams (1973); A. W. Moore (1985; 2003); Sullivan (1996; 2003); and Tang (2011).

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“logical constants” do not represent.’ (4.0312)<sup>16</sup>

There is an important (and under-appreciated) parallel between this idea and Kant’s idealism – in particular, Kant’s insistence that we must deny that, e.g., the triangle is something “in itself” in order to explain the status of geometry. We can see the parallel as follows: Logic, according to Wittgenstein, is *a priori*.<sup>17</sup> But this requires that there are no ‘objects of comparison’ in logic, since ‘a priori knowledge that a thought was true would be possible only if its truth were recognizable from the thought itself (without anything to compare it with).’ (3.05)<sup>18</sup> And, indeed, ‘it is the characteristic mark of logical propositions that one can perceive in the symbol alone that they are true’ (6.113; cf. also 6.126).

Putting these pieces together, the line of thought is as follows: Because the logical constants do not represent (there are no logical objects), there is no object of comparison in logic. This, in turn, allows logical propositions to be *a priori*, in the sense that we can determine their truth-values from the symbols alone, without determining whether the world is like this or that (cf. 6.1233). If, on the other hand, the logical constants *did* represent, then we should have to ask, paraphrasing Kant: How do you know that what belongs to your symbolic rules for constructing propositions must also pertain to the logical objects themselves?

Understood like this, the fundamental thought parallels Kant’s idealist move: The peculiar status of logic can only be explained once we get rid of the idea that logical truths are subject to an external standard of correctness – the idea, that is, that these truths are in the business of depicting some language-independent domain of logical objects. It is because there are no such objects, that the question of agreement (comparison) between the

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<sup>16</sup>Interestingly, the earliest philosophical remark we have from Wittgenstein records precisely this idea (2012: 22.6.1912; cf. also 1979a: 37).

<sup>17</sup>Cf. 5.133; 5.4731; 5.551–5.552; 6.3211

<sup>18</sup>I have used the Pears-McGuinness translation here since, frankly, the Ogden translation makes no sense: ‘Only if we could know a priori that a thought is true if its truth was to be recognized from the thought itself (without an object of comparison).’ The German reads: ‘Nur so könnten wir a priori wissen, dass ein Gedanke wahr ist, wenn aus dem Gedanken selbst (ohne Vergleichsobjekt) seine Wahrheit zu erkennen wäre.’



logical propositions and reality does not arise, and this is why these can be *a priori* without giving rise to a problem of inexplicable agreement.

We can see this in more detail by considering a variation on the worry we discussed in connection with Kant. For Wittgenstein, logic concerns the general structure of linguistic representation. This ensures that anything represented in language will “behave logically”. But, it may be asked, couldn't there still – in some ineffable sense – *be* counterexamples to logic, even if this means that they cannot be represented as such in language? Couldn't there really be contradictory states of affairs even if any attempt to represent these in language will inevitably represent them as false?

The point is that against the background of the fundamental thought this worry makes no sense. If logical constants – and conjunction and negation in particular – do not represent, then there cannot “really be” contradictions out there in the world. For how are we to make sense of these being *contradictions*? This would require, essentially, that one bit of reality should be the negation of another bit of reality, and this could only happen if negation had some objective correlate.<sup>19</sup> The fundamental thought, then, can do for Wittgenstein what Kant's idealism did for Kant.<sup>20</sup> The ideality of space allows Kant to deny that there could really be non-Euclidean triangles even if we cannot experience them as such; the fundamental thought allows Wittgenstein to deny that there could really be true contradictions even if we cannot represent them as such in language.

## Language and the Subjective-Constitutive Strategy

According to the interpretation just outlined, Wittgenstein's explanation of logical truth is an instance of the subjective-constitutive strategy, although

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<sup>19</sup>It is, I think, an interesting question whether the non-representational character of negation can be made consistent with everything Wittgenstein says in the *Tractatus*, but there is no denying that this is the official position (cf. e.g. 4.0621 and 5.44).

<sup>20</sup>Wittgenstein does have an additional trick up his sleeve: We cannot worry that there might be something that could not be represented in language since this would require us to think *non-sense*. This response, though, becomes less satisfying once we realise that the *Tractatus* itself consists of non-sense.

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transposed from intuition and thought to linguistic representation.

Logical truths are special because they are guaranteed to be true by the subjective (or at least language-internal) principles that anything represented in language must conform to. Moreover, these principles are also constitutive of the logical constants. Consequently, these belong *only* to the symbolism, and for this reason there is no ‘object of comparison’ in logic and so the question of agreement between the propositions of logic and reality cannot arise. Here Kant’s transcendental idealism is replaced by Wittgenstein’s fundamental thought: Where Kant denies the subject-independent existence of, e.g., geometrical objects, Wittgenstein denies the subject-independent (more precisely *language-independent*) existence of logical objects.

By relocating the strategy from intuition to linguistic representation, it should be noted, Wittgenstein avoids the radical nature of Kant’s transcendental idealism. The only things that Wittgenstein is forced to be “idealist” about are the logical constants, but as he himself put it: ‘That “or” and “not” etc. are not relations in the same sense as “right” and “left” etc., is obvious to the plain man.’ (1979a: 101)

As we saw above, a similar advantage can be claimed for Poincaré’s geometric conventionalism. However, we also saw that Poincaré’s argument relied on a specific conception of the nature of space. His particular instance of the subjective-constitutive strategy does not, therefore, generalise easily.<sup>21</sup> Wittgenstein’s account, on the other hand, is meant to apply, via the linguistic turn, to anything capable of being represented in language. It is therefore, on the face of it, quite general.

That being said, Wittgenstein is notoriously restrictive, in the *Tractatus*, when it comes to admissible explanations of modal phenomena. All necessity is meant to be logical necessity (6.37), which, in turn, is to be accounted for by truth-functional combinations that result in tautologies. All impossibility, correspondingly, is meant to be logical impossibility (6.375) – that

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<sup>21</sup>Of course, he did not intend it to generalise either.

is, truth-functional combinations that result in contradiction. Cases like colour exclusion are famously recalcitrant to this kind of treatment, leading Wittgenstein to conclude, later, that truth-functional considerations are not general enough to account for all necessity and impossibility.



## The Linguistic Approach and Logical Positivism

So far I have introduced Kant's subjective-constitutive strategy and argued that two key influences on the logical positivists – Poincaré's conventionalism and the linguistic turn inherent in Wittgenstein's account of logic – can be seen as instances of this strategy. I have also argued that the focus on convention, and the shift from intuition to language allows us to avoid some of the difficulties with the specifics of Kant's account. I now turn to how the linguistic approach emerges as a synthesis of these ideas in the work of the logical positivists. Here it is particularly helpful to look at the development of Schlick's views on the *a priori*, and this is the focus of section 3.1.

Next (3.2), I look at what the linguistic approach was meant to *do* for the logical positivists – in particular, how it was meant to 'make a consistent empiricism possible'. I argue that much of what the positivists themselves said about this is inadequate, but that a better answer can be extracted from the writings of Hans Hahn. On this conception, the defence of empiricism is closely tied precisely to the use of the subjective-constitutive strategy.

I then (3.3) consider various stages of Carnap's career, and argue that, although he spelt out the details differently at different times, he was committed to the linguistic approach throughout. This allows us to bring the linguistic approach into sharper focus by pinpointing what it is that remains constant throughout Carnap's career.

Finally (3.4), I summarise the main findings of this chapter and the previous one. In particular, I elaborate on what we can say about the commitments of the linguistic approach once we conceive of it as a linguistic version, with a strong emphasis on convention, of the subjective-constitutive strategy. In doing so I also contrast this approach with some superficially similar ideas that are not consistent with these commitments.

## 3.1 Schlick on the *A Priori*

### Schlick's Early Work

Schlick's earliest work on the nature of the *a priori* was decidedly Kantian (cf. Schlick 1910: 84), with no particular emphasis on either language or convention. When he turns his attention to Einstein's work on relativity, however, he comes to appreciate Poincaré's geometric conventionalism, and he explicitly notes that he sees the root of this view in Kantian ideas:

Henri Poincaré has shown with convincing clarity (although Gauss and Helmholtz still thought otherwise), that no experience can compel us to lay down a particular geometrical system, such as Euclid's, as a basis for depicting the physical regularities of the world. Entirely different systems can actually be chosen for this purpose, though in each case we also have at the same time to adopt other laws of nature. [...] The reason why this choice is possible lies in the fact (already emphasised by Kant) that it is never space itself, but always the mere spatial behaviour of *bodies*, that can become an object of experience, perception and measurement. (1915: 168–9)<sup>1</sup>

However, his work on Einstein's theories also convinced Schlick that Kant's epistemological system had to be rejected, and his *General Theory of*

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<sup>1</sup>Note that Schlick appears to appreciate that Poincaré's position relies on a specific view concerning space (cf. also Schlick 1925: 71). It is not entirely clear, therefore, that Michael Friedman is right to think (1995: 300–1) that Schlick read Poincaré as simply making a general Duhemian point about underdetermination (even though the above quote does continue in a way that might suggest this).

*Knowledge* can be read as an attempt to provide an epistemology that is fit for the new situation in the sciences.<sup>2</sup> At this stage, however, it is not Poincaré, but Hilbert and his method of implicit definition which is the strongest influence on Schlick, and the result is a thoroughly formalist conception of the *a priori*: ‘The construction of a strict deductive science has only the significance of a game with symbols.’ (1918: 35)

This, though, signals a departure from both Poincaré and the subjective-constitutive strategy we have been tracing so far. Kant’s project was to explain how we could justifiably come to think that the objects we encounter will conform to how we conceive of them *a priori*. Under the influence of Hilbert, Schlick now refuses to engage with this problem (cf. Goldfarb 1996: 215). The deductive sciences are *a priori* because they are radically *divorced* from the question of what we can encounter in experience:

In general, we concern ourselves with the abstract only to apply it to the intuitive. But – and it is to this point that our considerations return again and again – the moment we carry over a conceptual relation to intuitive examples, we are no longer assured of complete rigour. [...] in *implicit definition* we have found an instrument that enables us to determine concepts completely and thus to attain strict precision in thinking. To achieve this end, however, we have had to effect a radical separation between concept and intuition, thought and reality. (1918: 35–6)

In the second edition, Schlick appears to have become uneasy with this strict separation, and includes a new chapter, ‘Definitions, Conventions and Empirical Judgments’, where he introduces a distinct notion of convention which he explicitly attributes to Poincaré.<sup>3</sup> These conventions are quite different from the implicit definitions encountered earlier. They do not remain divorced from experience, but serve to coordinate concepts with reality (1925: 71) – e.g. they might be used to determine the physical significance of ‘equal periods of time’ (1925: 72).

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<sup>2</sup>Cf. e.g. Friedman (1991) and (2008: 95–6).

<sup>3</sup>For more on the differences between the two editions, see Uebel (2010: 288–9).

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However, these additions are not easily reconciled with the rest of the book. It would appear that the *a priori* no longer pertains exclusively to conceptual structures divorced from application to the intuitive, since we are now told that the newly introduced conventions (which are concerned precisely with such application) too can give rise to *a priori* knowledge. Indeed, ‘the axioms of the science of space’ are cited as an example here (1925: 74). On the other hand, we still find the claim, from the first edition, that *a priori* knowledge is only possible as long as we are dealing with conceptual structures cut off from application – including the claim that geometry is *a priori* only insofar as it is a ‘pure conceptual science’ which does *not* concern ‘the spatial relationships of reality’. (1925: 355)

Adding to the confusion, conventions are in one place casually *equated* with implicit definitions – as when Schlick writes that ‘geometry [...] does not proceed from synthetic *a priori* propositions. Instead, it proceeds from conventions (see Part I, §11), that is, from implicit definitions.’ (1925: 355)

In the end, it is difficult to avoid the impression that there is a serious tension in second edition of *General Theory of Knowledge*: The position inherited from the first edition is that knowledge can be *a priori* only insofar as it is a formal ‘game of symbols’ cut off from application to intuitive examples. But the *a priori* knowledge gained via the conventions introduced in the second edition, and, indeed some of the examples Schlick adds for this edition (1925: 75), simply do not fit this mould.

### **Wittgenstein’s Influence**

This tension is only resolved when Schlick adopts a new conception of formality under the influence of Wittgenstein’s *Tractatus*. In 1928, an infatuated Schlick proclaims that Wittgenstein has reconciled empiricism with the existence of logic and mathematics, since now the ‘nature of the logical is completely elucidated and established for all time to come.’ (1928: 136) The key to this, moreover, was that ‘an entirely clear and rigorous concept of “form” is provided, which banishes at a stroke those difficult problems



of logic which have lately given so much trouble to serious investigators.’  
(1928: 136)

The new conception of the formal is directly opposed to that we find in *General Theory of Knowledge*. Logic is formal, not because it is cut off from what we talk about in empirical claims, but because it concerns the framework for making such claims:

That the logical is in some sense *formal* is an old and oft-stated view; but the nature of pure forms had not really been clearly understood. The road to clarity on this subjects starts from the fact that every item of knowledge is an expression or presentation. It expresses the state-of-affairs known in it, and this can be done in any number of ways, in any language, and by means of any arbitrary systems of signs; all these possible modes of presentation, so long as they really express the same piece of knowledge, must for that very reason have something in common, and this common factor is their logical form.  
(Schlick 1930b: 156)

Note, however, that the new conception of form initially comes with a price: the rejection of conventionality. Poincaré has disappeared, and Schlick is now claiming that logic concerns precisely the *non-conventional* aspects of linguistic representation – i.e. what all possible modes of presentation *must* have in common.<sup>4</sup> Elsewhere, Schlick locates the formal in the rules that hold for the use of words (Schlick 1930a: 169),<sup>5</sup> but stops short of maintaining that these rules are adopted as a matter of convention. Indeed, in a conversation early in 1930 he pressed Wittgenstein precisely on the issue of the status of these rules: ‘But how do I know that precisely these rules are valid and no others? Can I not be wrong?’ (McGuinness 1979: 77)

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<sup>4</sup>Schlick is reported as saying, in a 1931 discussion: ‘There is an order which is independent of the phenomena and independent of our conventions. Syntax concerns that about language which is common to all languages.’ (Stadler 2001: 252)

<sup>5</sup>This paper, though published in 1932, was initially delivered as a lecture in 1930.

## The Return of Convention

During the 30's, however, Schlick gradually adopts the view that the *a priori* concerns the *conventionally* selected principles according to which we represent the world in language:

Our 20th century empiricism was founded and chiefly promoted by researchers who had made it their business to undertake a thorough philosophical examination of logic and mathematics. Their efforts have shown with ever increasing clarity that in logical and mathematical propositions it is broadly speaking a question of the rules whereby we form and transform linguistic symbols (propositions, numbers, propositional connectives) according to the conventions we have ourselves adopted for applying those symbols to the description of reality. They are therefore analytic (they say no more than is contained in the conventions defining them). (Schlick 1935b: 405–6)<sup>6</sup>

This renewed emphasis on convention might have been influenced by developments of Wittgenstein's thought at the time, but it was presumably also influenced by Carnap's syntax project. Moreover, Poincaré now makes a comeback:

As is well-known, Henri Poincaré has developed the procedure of convention with reference to the propositions of geometry [...] by essentially calling attention to the fact that those propositions in their application to the spatial properties of bodies are to be regarded as definitions. [...] In our manner of speaking we might express most briefly the insight into the conventional character of geometrical propositions by saying: geometry is the grammar of the language in which we describe the spatial relationships of physics. (Schlick 1935a: 437)<sup>7</sup>

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<sup>6</sup>Cf. also Schlick (1987: 107).

<sup>7</sup>In 1931, by contrast, when the role of convention was less clear for Schlick, he suggests that Poincaré's conventions are better understood as tautologies (1931: 194).

This conception of geometry should be compared to that of *General Theory of Knowledge*: While geometry is still said to be a conventional/definitional subject, this is not meant to suggest that it is an unapplied purely conceptual structure. Rather, geometrical propositions *in their application to the spatial properties of bodies* are regarded as definitions. This is indeed closer to Poincaré’s conception, but under the influence of the linguistic turn, Schlick now sees this as part of a more general strategy based on the general notion of a conventionally adopted linguistic rule.

Note that the new idea is *not* simply that necessary (or *a priori*) truths are “analytic”. That claim was present in *General Theory of Knowledge* as well. The real development concerns the specific understanding of analyticity, namely as arising from the conventionally adopted framework we employ in order to linguistically represent the world – as opposed to arising from a game with symbols cut off from application.

## **A Version of the Subjective-Constitutive Strategy**

What we have here is a version of the subjective-constitutive strategy which unifies some aspects of Poincaré’s conventionalism and the linguistic turn we find in the *Tractatus*. The special status of necessary truths is understood in terms of formal features of linguistic representation that *we are responsible for* and which are *not accountable to any external standard of correctness*, and so do not give rise to a problem of inexplicable agreement. This is particularly clear insofar as these features are said to be *conventions*.

There are, of course, several respects in which we have departed radically from Kant’s initial position. Most obviously: whereas Kant took the relevant subjective-constitutive principles to govern forms of intuition and categories of reason, the linguistic approach takes them to be conventionally adopted rules governing how we employ expressions in order to represent the world in language. Still, as I shall argue in more detail in the next section, the point of the subjective-constitutive strategy is largely retained.

Moreover, these developments arguably improve on Kant’s original pro-

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positional. We have already seen that appealing to convention provides us with a clearer grasp on how to argue that this strategy is appropriate, and the same can be said of the linguistic turn. Briefly put: we have a better grasp on the notion of a linguistic convention, than we have on the notion of a form of intuition.

We also avoid Kant's radical idealism. What we end up being "idealists" about here is essentially the principles of individuation reflected in the rules that govern our use of linguistic expressions. We must deny, that is, that these rules are dictated by something subject-independent like "the joints of reality" or antecedent concepts understood along Platonist lines, since this would raise the question of how we know that the rules we have adopted reflect how things really are. Instead, the rules we adopt must be regarded as *constitutive* of our concepts and the principles of individuation that they bring with them. Although there are those who would want to contest that our language is autonomous in this sense, it is surely less radical than denying the objective reality of space and time.

A third problem noted in connection with Kant stemmed from Russell's objection from alteration: Can we guard against the possibility that the subjective-constitutive principles might change so that we get counterexamples to what was previously regarded as necessary? This worry, however, has considerably less force in the context of the linguistic approach since the subjective-constitutive principles are now construed as *definitions*. A change here will therefore involve a change in meaning, and so, although changes to the relevant principles are certainly possible, there is little reason to think that this would result in there being *counterexamples* to what was previously held to be necessary, as opposed to effecting a change in subject.

Finally, we saw that Kant needs something like an epistemology of transcendental psychology to explain how we can have insight into the principles that govern intuition and thought. In the context of the linguistic approach, on the other hand, all we need is a comparatively unmythical capacity for reflecting upon the rules governing our use of linguistic expressions. How-

ever, although this is an advantage, we should not be carried away here, since one standard objection against the linguistic approach is that knowing what the linguistic rules are involves empirical knowledge of contingent matters, and cannot, therefore, provide the basis of modal knowledge, which is traditionally construed as *a priori* – or at least not straightforwardly empirical. We shall consider this objection in due course.

I should also draw attention to an advantage which this view has over the Tractarian position: The idea here is that our linguistic rules *quite generally* are exempt from an external standard of correctness. It is not a view about the logical constants specifically. Moreover, since such rules quite generally are supposed to explain necessity (and the *a priori*), there is no suggestion here (like there is in the *Tractatus*) that all necessity must be accounted for in terms of a highly restricted conception of logic.

## 3.2 Hans Hahn and Consistent Empiricism

Having seen how the linguistic approach emerges as a synthesis of a broadly speaking Kantian strategy, Poincaré's conventionalism and Wittgenstein's linguistic turn, I now turn to the question of what role this approach was meant to play in the broader context of logical positivism.

According to the positivists, the chief importance of the new conception of logical truth, together with the logicist project in the foundations of mathematics, was that it made a 'consistent empiricism' possible. The standard story told by the positivist in this context is, however, a rather shallow one, and does not stand up to scrutiny. However, a closer look at the writings of Hans Hahn reveals a more interesting picture, in which the defence of empiricism is intimately connected with precisely the use of the subject-constitutive strategy we have been tracing. In this section I first present and criticise the standard story, and then consider Hahn's views in more detail.

### The Standard Defence of Empiricism

According to the standard story,<sup>8</sup> the problem that faced empiricism was simple: Empiricism requires that all knowledge must be justified by observation, but observation does not provide adequate justification for necessary truths, in particular those of mathematics and logic. Recent developments were said to have solved this problem by: i) reducing mathematics to logic (barring some loose ends in *Principia Mathematica*), and ii) showing that logic itself is tautological and therefore, in some sense, “devoid of factual content”. This lack of factual content was supposed to exempt logic (and thus also mathematics) from the demands of empiricism:

Since empiricism had always asserted that all knowledge is based on experience, this assertion had to include knowledge in mathematics. [...] Our solution, based on Wittgenstein’s conception, consisted in asserting the thesis of empiricism only for factual truth. By contrast, the truths in logic and mathematics are not in need of confirmation by observations, because they do not state anything about the world of facts, they hold for any possible combination of facts. (Carnap 1963a: 64)

The last sentence here leaves it somewhat unclear how the notion of factual content ties in with the notion of holding for any possible combination of facts, but elsewhere Carnap is more explicit about this: ‘All valid statements of mathematics are analytic in the specific sense that they hold in all possible cases and therefore do not have any factual content.’ (1963a: 47; cf. also 1930: 143) This, however, amounts to simply equating ‘lack of factual content’ with being *necessary*, and if *this* is what is meant by ‘factual content’, then restricting the empiricist thesis to factual matters, is just to restrict it to *contingent* matters. This by itself could hardly be a vindication of empiricism in any significance sense. For even if a truth lacks factual content *in this sense*, this gives us no reason at all to think that it is somehow epistemically unproblematic.

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<sup>8</sup>Cf. e.g. Carnap (1930: 141–3; 1963a: 46–7), and Ayer (1936a: ch. 4).

Moreover, if this is what lack of content amounts to, then Carnap is much too quick in passages such as this:

Since all the sentences of logic are tautological and devoid of content, we cannot draw inferences from them about what is necessary or impossible in reality. Thus the attempt to base metaphysics on pure logic which is chiefly characteristic of such a system as Hegel's, is shown to be unwarranted. (1930: 143)

The examples used by the positivists are similarly unhelpful. Ayer, for instance, tells us that ‘“Either some ants are parasitic or none are’ provides no information whatsoever about the behaviour of ants’ (1936a: 103–4), and Carnap insists that ‘we learn nothing about reality from the tautology “Its is raining (here and now) or it is not raining.”’ (1930: 143). But even if we grant that there is a reasonable sense in which this provides no information about the behaviour of ants or the weather, we might still say that the realisation that this holds in every possible case teaches us something profound about the modal structure reality or logic. Gödel makes something like this objection when he writes that ‘one may very well say that the proposition mentioned above, although it says nothing about rain, does express a property of “not” and “or”’. (1995a: 362)

In the end, it is difficult to avoid the conclusion that the standard story of how new developments in logic and the foundations of mathematics made a consistent empiricism possible, relies on misleading blend of definitions and rhetorical devices. On a straightforward understanding of what it is to ‘lack factual content’, it could plausibly be said that if logic and mathematics lack factual content, then there is no real knowledge here, and consequently no threat to the empiricist thesis that knowledge must be backed up by observation. But if all it means is that logic and mathematics hold in all possible cases, then this warrants no such conclusion.

After all, the question all along was presumably: How do we know that something holds in every possible case assuming that this is not something we learn by observation? It is all well and good to back this up with truth-

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tables, but this only pushes the question back to the truth-tables themselves and how we know that they indeed take all the possibilities into account. To say that some truths lack factual content because they hold in all possible cases is clearly of no help here.

#### **Hans Hahn on Logic and Language**

We should not, however, conclude that the positivists' conception of logical truth had no bearing on the defence of empiricism they were interested in. A more interesting picture emerges if we look at the writings of Hans Hahn, who, according to Carnap, was the one who especially emphasised the connection between the new conception of logic and empiricism, both in publication and discussion (Carnap 1963a: 47).<sup>9</sup> I shall argue that Hahn's account of logical necessity and how it makes a consistent empiricism possible relies essentially on employing Kant's subjective-constitutive strategy.

There is no denying that the Vienna Circle was more concerned with stressing discontinuities with Kant than points of contact, and Hahn is no exception.<sup>10</sup> It is quite clear, in particular, that he rejected the synthetic *a priori*, and the notion of pure intuition. Still, we find no wholesale rejection of Kant, and at a more fundamental level there is agreement. In particular, Hahn is quite explicit that knowledge presupposes that the subject takes on an active role, and that this provides the key to the special status of logic. Thus he writes that 'in the acquisition of our knowledge we do not just receive the given but in addition process it', and maintains that 'this provides us with an opportunity to sketch a view of the *place of logic*' (Hahn 1930a: 22).

The view he goes on to sketch has a distinctly Kantian flavour, although it is transposed, under the influence of Wittgenstein's linguistic turn, from thought and intuition to *language*:

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<sup>9</sup>Hahn's views are rarely discussed today, but see Ablondi (2002) and Uebel (2005).

<sup>10</sup>Cf. e.g. Hahn (1930b: 10; 1933a: 74).



According to this view, logic is not something to be found in the given – or let us say: in the world. Logic is not, as used to be believed, a theory of the most general properties of objects, a theory of objects as such; rather, logic first arises when the given is processed, when the knowing subject confronts the given, tries to picture it to himself, and introduces a symbolism: logic is tied up with something’s being *said* about the world. (1930a: 22)<sup>11</sup>

Notice that Hahn is not just making the positive claim that how we talk about the world gives rise to logic, but also the negative claim that there is nothing analogous ‘in the world’ (or ‘the given’). There is, therefore, no sense in which the logic that arises from how we represent “the given” in language could fall short of an external standard of correctness. Indeed, Hahn explicitly draws on Wittgenstein’s fundamental thought here: ‘There [is] nothing in the world corresponding to the so-called logical constants (like “and”, “or”, etc.)’ (1930a: 24).

We have, then, the following two parallel strategies: Kant argued that the reason why geometry is necessary and knowable *a priori* is that it concerns the principles according to which the subject experiences objects, and he supplemented this with a denial that the objects of geometry correspond to anything outside of our form of outer intuition. Hahn argues that the reason why logic is necessary and knowable *a priori* is that it concerns the principles according to which the subject represents the world in language, and he supplements this with a denial that the logical constants correspond to anything outside of our language.

Further similarities emerge when we consider the problems Kant and Hahn took themselves to be addressing. Kant, recall, introduced his strategy in response to the problem of explaining the agreement that *a priori* representations are supposed to have with the objects even without any experiential input – the problem of inexplicable agreement. Hahn sees the linguistic approach as fulfilling an exactly analogous purpose:

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<sup>11</sup>Cf. also Hahn (1929: 40; 1931: 33; and 1933b: 29).

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Once we take this view of logic, a much-discussed problem dissolves of its own accord – the problem of the seemingly mysterious parallelism between the course of our thought and that of the world, the seemingly pre-established harmony between thought and world, which would enable us to discover something about the world by thought. (1930a: 24)<sup>12</sup>

Now, the idea that we could discover something about the world “by thought” is exactly what the logical positivists wanted to oppose by their empiricism. It is, therefore, by *dissolving* (as opposed to solving) this problem of “mysterious parallelism” that empiricism is supposed to be vindicated. This is why Hahn prefaces all of this by saying that ‘*only the elucidation of the place of logic and mathematics to be discussed below* (which is of very recent origin) *made a consistent empiricism possible.*’ (1930a: 21)<sup>13</sup>

As in Kant’s case, the problem is dissolved by employing the subjective-constitutive strategy – by conceiving of that which can be discovered ‘by thought’ (logic and mathematics) as concerning subjective principles which are not accountable to any further standard of correctness, so that no question of “parallelism” arises. Indeed, in a 1931 conversation, Hahn characteristically opposes Schlick’s claim that ‘the form of the facts is mirrored in the language’, by saying that ‘there is no connection here unless it is artificially construed. For the rules of syntax a logical justification cannot be given because it is only here that logic begins’ (Stadler 2001: 253). The rules of syntax, that is, are constitutive of the notion of a logical justification, and so there can be no question of getting these right.

Note that this defence of empiricism goes deeper than the standard story. The point is not just that logical truths are true in every possible case, but that *the reason why* this is so is that they arise from how we represent the world in language, independently of what it is actually like. Thus, while Hahn does adopt Wittgenstein’s ‘tautology’ terminology – with the implication that tautologies are in some sense devoid of content – what he

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<sup>12</sup>Cf. also Hahn (1933b: 28).

<sup>13</sup>Cf. also Hahn (1929: 41; 1931: 33; and 1933b: 38).

stresses is not that they are true no matter what, but that they arise merely from how we speak: ‘We must distinguish two kinds of propositions: those that say something factual and those that merely express a dependence in the assignment of designations to objects; let us call propositions of the latter kind *tautological*’ (1933b: 32).

This provides the more principled reason for the positivists’ insistence that logical truths, and necessary truths more generally, are without factual content. The thought is precisely that questions of logic can be settled, as Wittgenstein puts it, ‘without an object of comparison’. This is the sense in which the ‘universal validity’, ‘certainty’, and ‘irrefutability’ of the laws of logic ‘flows precisely from this from this circumstance, that they say nothing at all about objects’ (Hahn 1933b: 30). They say nothing about the objects because they are built into the framework we use to represent the world, and this framework is itself independent of what the objects are like.

The positivists’ use of the linguistic approach to defend their empiricism, then, relies crucially on the subjective constitutive strategy: The existence of necessary (and *a priori*) truths is meant to be consistent with empiricism because such truths concern merely the principles according to which the subject represents the world in language. These principles, moreover, are not subject to any external standard of correctness, but rather themselves constitutive of our concepts. There is therefore no problem of inexplicable agreement – no ‘mysterious parallelism between the course of our thought and that of the world’. Logical knowledge is meant to be a kind of self-knowledge, available by reflecting on the rules that govern our use of linguistic expressions.

### 3.3 Carnap and the Linguistic Approach

Carnap was the one, among the logical positivists, who treated questions about the foundations of mathematics and logic with the most rigour, and

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he was also the one who most explicitly endorsed a thoroughgoing conventionalism with respect to logic. This tends to be associated primarily with Carnap's syntax project which culminated in *Logical Syntax of Language*. However, Carnap was committed to something like the linguistic approach (and the subjective-constitutive strategy) long before the syntax project, and he retained this commitment after he abandoned it in favour of a semantic approach. Moreover, while the move to semantics did put a restriction on the appeal to convention, this should still not be thought of as a radical break when it comes to how Carnap thought about the nature of logical truth. In this section I trace these developments with a focus on the linguistic approach as something which remains constant throughout.

#### Carnap Before Syntax

As mentioned, Carnap holds, in the *Aufbau*, that logic consists solely of conventions concerning the use of symbols and tautologies based on these. We are also told that 'the symbols of logic (and mathematics) do not designate objects, but merely serve as symbolic fixations of these conventions.' (1928a: 178) Both the use of 'tautology' and the insistence that logical constants do not refer show the influence of Wittgenstein. The appeal to convention, on the other hand, is foreign to the *Tractatus*. We know, however, that Carnap was influenced by Poincaré's conventionalism from early on,<sup>14</sup> and it is not surprising, therefore, that he would put – perhaps without realising it – a conventionalist spin on the Tractarian position.

Carnap does not have much more to say about logic in the *Aufbau*. However, a paper from around the same time elaborates on these ideas:

In order to provide derivations of real concepts from other real concepts, we need, in addition to words for these concepts, intermediary signs which do not themselves refer to real concepts (using

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<sup>14</sup>We find references to Poincaré in his 1921 doctoral dissertation (published as Carnap 1922), and, as Carnap mentions in his autobiography (1963a: 15), he was strongly influenced by Poincaré's conventionalism in his (1923).

a word language, for example words like: ‘and’, ‘or’, ‘all’, ‘not’, ‘if...then...’, ‘equals’). They do help to say something about reality, but nothing in reality corresponds to them – they are only part of the statement. (1927: 358; my translation)<sup>15</sup>

Again we find Carnap making the *Tractarian* point that the logical constants belong solely to our way of representing the world and not to what is represented. This idea is later connected with the special status of logical/mathematical knowledge:

The formal concepts (logical and arithmetic concepts) serve only as a tool for representing knowledge of real concepts; so-called knowledge of formal concepts (e.g. mathematical knowledge) consists of tautologies. (Carnap 1927: 373; my translation)<sup>16</sup>

It is worth noting that although Carnap uses the notion of a tautology here, he nowhere provides a definition of this term along the lines of the *Tractatus*. There is no mention, for instance, of truth-tables. All we are given is an equation of the tautological and the *a priori*, and a contrast between tautologies and ‘propositions about reality’ (1927: 362). In addition, the reader could be expected to rely on the ordinary understanding of ‘tautology’ from grammar – that is, a sentence in which there is a linguistic redundancy of some kind.

In this context, then, calling formal knowledge ‘tautological’ is plausibly Carnap’s way of making precisely the point that it is an artefact of how the subjects represents genuine knowledge in language: Formal concepts are ‘only part of the statement’, and as a consequence, formal truths are

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<sup>15</sup>Wenn die Ableitung eines Realbegriffes aus anderen oder eine Aussage über Realbegriffe gegeben werden soll, so brauchen wir außer den Worten für diese Begriffe noch Zwischenzeichen, die selbst keine Realbegriffe bezeichnen (bei Verwendung von Wortsprache z. B. die Worte: ‘und’, ‘oder’, ‘alle’, ‘nicht’, ‘wenn...so...’, ‘derselbe’ und dergleichen). Sie verhelfen zwar dazu, etwas über die Wirklichkeit auszusagen; ihnen selbst entspricht jedoch nichts in der Wirklichkeit, sie formen nur die Aussage.

<sup>16</sup>Die Formalbegriffe (logische und arithmetische Begriffe) dienen nur als Hilfsmittel zur Darstellung der Erkenntnis von Realbegriffen; die sogenannten Erkenntnisse von Formalbegriffen (z. B. mathematische Erkenntnisse) sind Tautologien.

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tautologies, as opposed to ‘propositions about reality’. If this interpretation is correct, then Carnap was, by the late 1920’s, already committed to a linguistic approach to logical necessity, understood as a linguistic turn (inspired by Wittgenstein) on Kant’s subjective-constitutive strategy, with strongly conventionalist elements inherited from Poincaré.

#### The Syntax Project

It is a mistake, then, to restrict Carnap’s commitment to the linguistic approach or ‘conventionalism’ to his syntax project. Rather, the work culminating in *Logical Syntax of Language*<sup>17</sup> should be seen as a specific attempt to spell out and defend the view he already had in the late 20’s. Two developments probably convinced Carnap that this idea was in need of a more thorough treatment: i) the realisation that the Tractarian notion of a tautology is too weak to sustain a strong enough version of the logicist project; ii) Gödel’s demonstration that mathematical truth cannot be equated with provability.<sup>18</sup>

What is new in *LSL* is Carnap’s adoption of what he later (1963b: 928) calls ‘the formalist method’:

I intended to show that the concepts of the theory of formal deductive logic, e.g., provability, derivability from given premises, logical independence, etc., are purely syntactical concepts, and that therefore their definitions can be formulated in logical syntax, since these concepts depend merely on the forms of the sentences, not on their meanings. (1963a: 54)

Importantly, Carnap did not regard this as a fully-fledged commitment to formalism: While he accepted the *method*, he ‘did not accept the thesis of formalism and instead maintained that of logicism’ (Carnap 1963b:

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<sup>17</sup>The original was published in 1934, and the expanded English translation in 1937.

<sup>18</sup>For more on this, see e.g. Awodey and Carus (2007), Awodey (2007: §1), and Uebel (2007: §5.1.1).

928). In accordance with the method, he aims to conduct his investigations from a purely formal (syntactical) point of view, but he denies that logic/mathematics should be regarded as anything like a game with symbols cut off from application: ‘Since Schlick and I came to philosophy from physics, we looked at mathematics always from the point of view of its application in empirical science.’ (1963a: 48) *LSL* is meant to establish, not that logic/mathematics is cut off from application, but that notions like logical truth and provability can be fully explained without taking such matters into account.

Though the adoption of the method of formalism is a new development, the *reason* (or at least *one* reason) for adopting it is not. The point is precisely that if we can show that all formal/logical concepts can be adequately defined from a purely syntactic point of view – i.e. using definitions that refer ‘solely to the kinds and order of the symbols from which the expressions are constructed’ (Carnap 1937: 1) – then we shall have both defended and explicated the idea that formal concepts are special because they are, as he had put it earlier, ‘only part of the statement’.

It is not surprising, therefore, that we find Carnap making essentially the same point he had made in 1927 in a paper from the syntax period:

In adjoining the formal sciences to the factual sciences *no new area of subject matter* is introduced, despite the contrary opinion of some philosophers who believe that the “real” objects of the factual sciences must be contrasted with the “formal”, “geistig” or “ideal” objects of the formal sciences. *The formal sciences do not have any objects at all*; they are systems of auxiliary statements without objects and without content. (1935: 128)

The adoption of the formalist method, then, provides Carnap with a new way of spelling out an old idea.

Another development is that the conventionalist strand, although present before the syntax project, becomes much more pronounced. It is lifted to the foreground in the form of the Principle of Tolerance:

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*It is not our business to set up prohibitions, but to arrive at conventions. [...] In logic there are no morals. Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules in stead of philosophical arguments. (1937: 51–2)*

It is clear that Carnap did see the adoption of the formalist method as liberating debates in logic and the foundations of mathematics from the notion of correctness. Still, this is potentially puzzling since he held that logic was based on conventions as early as 1928. Moreover, he stresses in his Autobiography that the Principle of Tolerance – although first *formulated* in *LSL* – expresses an attitude that ‘remained the same throughout [his] life.’ (1963a: 18) What the formalist method brings to the table, then, is neither the idea that logic is in some sense tied to conventions, nor the idea that ‘everyone is free to use the language most suited to his purpose.’ (Carnap 1963a: 18)<sup>19</sup> The point, rather, is that by adopting the formalist method, the choice of logic – understood as a proof system – becomes *prior* to anything else. It is therefore not subject to any prior constraints, and the notion of correctness gets no grip at all:

Let any postulates and any rules of inference be chosen arbitrarily; then this choice, whatever it may be, will determine what meaning is to be assigned to the fundamental logical symbols. By this method, also, the conflict between the divergent points of view on the problem of the foundations of mathematics disappears. For language, in its mathematical form, can be constructed according to the preferences of any one of the points of view represented; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or the other of the choices leads, including the question of non-contradiction. (Carnap 1937: xv)

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<sup>19</sup>Nor, indeed, is the aim of reconciling formalism, intuitionism and logicism new (cf. Hahn, O. Neurath, and Carnap 1929: 311).



As I understand the development of Carnap's ideas, he was, even before the syntax project, sympathetic to the idea that there could be different but equally correct logics built into different but equally correct languages. But this is not yet the position of *LSL*, for it may still be debated, for a given logic (say intuitionistic logic), whether there is a coherent language for which this logic would be the correct one – whether we can, that is, find a suitable *interpreted* language which would give us, say, intuitionistic logic. *These* are the kinds of questions that are meant to be dissolved in *LSL* by the adoption of the formalist method.

### The Move to Semantics

So far I have argued that Carnap's commitment to the linguistic approach – understood as a linguistic version of the subjective-constitutive strategy with a focus on convention – antecedes the syntax project, and that the syntax project itself should be seen as a specific attempt to develop this view. I shall now argue that the linguistic approach survives when Carnap abandons the syntax project in favour of a focus on semantics.

In general, I agree with those (Creath 1990b; Ricketts 1996), that have argued that Carnap's move to semantics should not be thought of as a sharp break with his syntax project. It is also quite clear that this was Carnap's own view (Carnap 1942: 246). However, the more interesting questions are more local and concern how the move to semantics alters Carnap's conception of particular issues. Two such questions are particularly relevant for our purposes:

1. Does the move to semantics involve a break with the linguistic approach?
2. Does the move to semantics involve a significant restriction of the conventionalism inherent in the Principle of Tolerance?

## Semantics and Logical Truth

The explanation of logical truth is one of the most significant differences between *LSL* and the later works. The syntactical definitions of what Carnap calls ‘L-concepts’ are now abandoned, and semantic definitions are sought. Carnap no longer holds, that is, that it is appropriate to characterise these notions without taking question of meaning into account. Our first question is whether this development signals a rejection of the idea that logical truth is to be explained in terms of linguistic rules that are not accountable to any subject-independent standard.

There is one reason to think that it might. The idea would be that once this move is made, what is doing the explanatory work is not the linguistic rules we conventionally adopt, but relations between meanings which hold independently of such rules. On this picture, our linguistic rules would appear to be accountable to the meaning relations that in fact obtain, resulting in the abandonment of the subjective-constitutive strategy, and giving rise to a problem of inexplicable agreement: What reason do we have to think that the semantic relations are as we take them to be?

Some later passages from Carnap do indeed hint at a picture along such lines:

Once the meanings of the individual words in a sentence of this form are given (which may be regarded as a matter of convention), then it is no longer a matter of convention or of arbitrary choice whether or not to regard the sentence as true; the truth of such a sentence is determined by the logical relations holding between the given meanings. (1963c: 916)

It may even be suggested that Carnap here sounds rather like Gödel when he, in one of his more Platonist moments, maintains that ‘with mathematical reason we perceive the most general (namely the “formal”) concepts and their relations, which are separated from space-time reality’ (1995a: 354).

However, the similarities here are superficial and it would be a serious mistake to conflate Carnap’s and Gödel’s positions. They diverge on pre-

cisely the issue that is crucial for our purposes: whether the meanings are understood as a subject-independent standard of correctness that would introduce the kind of epistemological puzzles that the subjective-strategy is supposed to get rid of.

For Carnap, there is nothing in the appeal to meaning beyond an appeal to the semantic rules which we have adopted: ‘Within semantics, I stress the distinction between factual truth, dependent upon the contingency of facts, and logical truth, independent of facts and dependent merely on meaning *as determined by semantical rules*.’ (Carnap 1942: xi; my emphasis) These rules, moreover, are still adopted by convention. As he puts it in his earliest treatment of the subject after adopting the semantic point of view:

Even here, conventions are of fundamental importance; for the basis on which logic is constructed, namely, the interpretation of the logical signs (e.g. by a determination of truth conditions) can be freely chosen. (Carnap 1939: 28)

Gödel, on the other hand, was eager to stress that when we are dealing with relations between meanings/concepts, we are *not* dealing with something which has its basis in our linguistic rules: ‘I wish to repeat that “analytic” here does not mean “true owing to our definitions”, but rather “true owing to the nature of the concepts occurring therein” ’ (1995b: 321).

On inspection, then, the move to a semantic explanation of logical truth is less of a break with the earlier conception that it might seem. The idea is not that we need to appeal to “the meanings”, as opposed to the linguistic rules we have chosen to adopt. We are still to appeal to such rules, but we are dealing with rules of a different kind than those in *LSL*. Carnap’s semantic explanation of logical truth is therefore, in fundamental respects, quite continuous with the account in *LSL*. Both the syntactic and the semantic account are attempts to spell out an underlying idea that Carnap was committed to as early as 1927: namely that logical truth (which, for Carnap, includes everything traditionally referred to as ‘necessary’) arises from the

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rules that govern how we represent the world in language – rules, moreover, that are not accountable to any further, subject-independent standard.

#### Semantics and Convention

Our second question is whether the move to semantics involves a significant restriction on the Principle of Tolerance – that is, whether Carnap’s conventionalism is diluted. There are different views about this in the literature. Michael Potter, for instance, maintains that ‘the introduction of a theory of semantics is to be seen as a constraint on the Principle of Tolerance’ (2000: 272), whereas Thomas Ricketts holds that the ‘adoption of semantics does not seriously affect Carnap’s attitude of tolerance’ (Ricketts 1996: 246–7).

After the move to semantics, Carnap certainly does make it clear, as we have already seen, that there are limits to what is conventional. In particular, we cannot first settle on an interpretation of our language and then decide on a logic by convention (Carnap 1939: 27–8).<sup>20</sup> Nor, presumably, can we settle on a logic, and then freely decide on an interpretation. In this sense, the move to semantics does introduce new constraints; there are *some* morals in logic, namely soundness, and (perhaps) completeness.

However, some care is needed when comparing this to the syntax period. For there is a sense in which Carnap always accepted that there are such constraints linking the semantic and the syntactic. In *LSL*, after all, it is suggested that we should postulate a logic and let this choice *determine* the interpretation (Carnap 1937: xv). Moreover, Carnap’s objection to the strategy of first deciding on an interpretation and then determine the correct logic relative to this was not that the interpretation leaves the choice of logic completely undetermined; it was that matters of meaning are too vague for this to be a productive method. Indeed, during the syntax period, Carnap could still write:

It is often stated that the relation of entailment depends on the meanings of the propositions. In a certain sense we can agree with

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<sup>20</sup>Cf. also (1942: 247).

that; for when the meaning of two propositions is known, it is thereby determined whether one is the entailment of the other or not. (1934b: 10–1)

Even in the syntax period, then, Carnap recognised the (plain) fact that the choice of logic isn't independent of the interpretation of the logical expressions. It is just that because talk of meaning was viewed as insufficiently rigorous and therefore banned (somewhat ironically, in the spirit of tolerance) from discussions of logic, these constraints didn't appear on the radar. Against the background of the method of formalism, the question of soundness is not allowed to arise.

Once semantics is accepted as rigorous enough, however, so too are the questions of soundness and completeness. But this is, at bottom, just a broadening of what is available for scientific treatment. It does not represent a fundamental change in Carnap's tolerant attitude. In particular, it remains the case that we can speak whatever language we choose, and it remains the case that such different languages may give rise to different but equally "correct" logics. My contention, then, is that although the move to semantics does put some constraints on the radar because it allows for a rigorous treatment of the relationship between syntax (i.e. proof theory) and semantics, it does not alter the substance of Carnap's conventionalism.<sup>21</sup>

It is worth reiterating here a point made above: The transition from talking about syntactic rules to talking about semantic rules, does not involve rejecting the idea that logic is based on rules that are conventionally adopted (cf. Carnap 1939: 28). While the emphasis on convention is now less pronounced, and the the recognition of the limits of convention more explicit, the role of convention when it comes to the explanation of logical truth remains largely the same. As Richard Creath puts it 'the move to

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<sup>21</sup>An interesting question is whether the *general* semantic framework that Carnap constrains our choice of logic in some way. Even if this is so, however, it would not automatically be a break with the syntax period since an analogous question arises there with regard to the 'general syntax' laid out in Part IV of *LSL*.

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semantics does not change the content of Carnap's conventionalism, but it does change the form.' (1990b: 413)

#### **The Unity of Carnap's Views on Logical Necessity**

I maintain, then, that the linguistic approach was a constant theme in Carnap's thinking about the nature of logical necessity. He never gave up his early conviction that logic does not "correspond" to anything in reality, but arises solely from how we represent the world in language and the rules we have adopted for this purpose. Instead, he worked to spell out this idea in more detail, first, during the restrictive syntax period, by attempting to explicate the notions of logical truth and provability in terms of purely syntactic notions, and then, once he was satisfied that semantics had been put on a firm scientific basis, via a more liberal project that allowed appeals to semantic notions as well.

Still, he always endorsed a linguistic version of the subjective-constitutive strategy, with a strong emphasis on convention and tolerance. The idea was throughout that logical necessity should be explained in terms of the rules that govern linguistic representation, and that these rules are not responsible to any external reality or standard of correctness. Rather, the rules constitute the standard of correctness, since it is, as Hahn puts it, 'only here that logic begins' (Stadler 2001: 253).<sup>22</sup> For this reason there is meant to be no problem of inexplicable agreement, which, in turn, is meant to make empiricism consistent with the existence of necessary truths (with the Humean background assumption being that empirical evidence does not provide adequate justification for thinking that something is necessary).<sup>23</sup>

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<sup>22</sup>Analogously, we might say that, for Kant, it is only in the form of outer intuition that "geometry begins" since space is a *mere* form of intuition, and so there can be no question of justifying our form of intuition relative to the true geometry of space.

<sup>23</sup>This assumption is not, it is worth noting, automatically undermined by the "necessary *a posteriori*". For even if we learn that water is H<sub>2</sub>O via empirical investigation, this is not to say that such an investigation suffices to tell us that this is necessary (after all, thinking about what is the case on Twin Earth is hardly an empirical investigation).

### 3.4 More on the Commitments of the Linguistic Approach

In chapter 1 we said that the linguistic approach is best understood as a commitment to carrying out an explanatory project of a certain kind, where necessities are to be explained in terms of, broadly speaking, linguistic matters having to do with the rules that govern the use of linguistic expressions. We also noted that although this notion of a “linguistic matter” gives us some grasp on what adherents of the linguistic approach can appeal to in their explanations, it is in need of further explication.

The above investigation into the historical roots of the linguistic approach, and the role it was meant to play within the context of logical positivism, allows us to provide such a further explication. Our main guide here is the idea that the linguistic approach – given its roots, how it was developed, and what it was meant to accomplish – is a version of the subjective-constitutive strategy. An adherent of this approach cannot, therefore, appeal to anything which would compromise this.

Notably, this means that they are restricted to appealing to principles which can plausibly be regarded as: i) subjective, in the sense that they concern how the subject represents the world in language, as opposed to how the world is “in itself”; and ii) not accountable to an external standard of correctness – a standard of correctness, that is, which would introduce a problem of inexplicable agreement regarding how we get to be confident that the principles that govern how we speak are adequate relative to some language-independent domain.

Now, it may be suggested that explanations in accordance with the commitments of the linguistic approach must appeal solely to *conventional* features of linguistic representation, and so that whatever is appealed to in such explanations must admit of *alternatives*. And as we have seen, there are indeed great merits in appealing to convention in this context, since the presence of equally valid alternatives provide us with a reasonably clear

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indication that there is no external standard of correctness (e.g. the fact that a measurement in feet would be as correct as one in meters tells us that the units we use here are not in the business of correctly capturing something like the real units of nature).

However, this would be unduly restrictive. For given how we are understanding the linguistic approach, the appeal to convention, while highly significant, is, essentially, a means to an end. The *point* of highlighting that something is a matter of convention is to establish that there is no external standard of correctness, and therefore no room for a worry about whether we are “getting things right or not” in this regard. This means, however, that as long as an adherent of the linguistic approach can argue that they are not appealing to anything which would introduce such a worry, it need not undermine their aims if they appeal to something which cannot be straightforwardly be said to admit of alternatives, and so be a matter of convention. Even if there are aspects of how we represent the world in language which we cannot envisage alternatives to, this does not necessarily mean that these aspects are in the business of reflecting some language-independent reality; they might just be built into our very practice of linguistic representation as such.

Moreover, there is not much basis, in the writings of the logical positivists, for the notion that conventionality is the be all and end all of the linguistic approach. Even in *LSL*, where Carnap is at his most “conventionalist” and declares that ‘in logic there are no morals’ (1937: 52), it is not said that, when it comes to syntax, it is *all* a matter of convention. Carnap plainly *cannot* say this since the purpose of the entire Division B of Part IV of *LSL* is to ‘construct a *syntax for languages in general*, that is to say, a system of definitions of syntactical terms which are so comprehensive as to be applicable to any language whatsoever’ (1937: 167). In other words: not everything which is a matter of syntax admits of alternatives.

What we *do* frequently find is the idea that logical necessities are to be explained in terms of *conventionally adopted rules for using linguistic*



*expressions*. But to say that the rules are conventionally adopted is not to say that they are wholly conventional constructs with no non-conventional features. It may be, after all – indeed, it is quite plausible – that there are limits to what could count as a linguistic rule at all. Thus, a rule could be conventionally adopted (since we might blamelessly have adopted different rules) even if it has certain features which no linguistic rule could lack.

Moreover, appealing to such features in explanations of necessities could still be consistent with a linguistic version of the subjective-constitutive strategy *provided that* they are not understood as imposed on our language by an independent standard of correctness which would introduce a problem of inexplicable agreement regarding how we know that our language meets this standard.

The preceding chapters also allows us to say something about what would be a too *liberal* conception of the commitments of the linguistic approach. In particular, the aim is not to explain necessities in terms of some bland notion of *analyticity*. Indeed, we have already seen that at least two traditions which can be thought of in terms of analyticity are not consistent with the central tenets of the linguistic approach since they are in tension with the subjective-constitutive strategy.

First, formalism is not a version of the linguistic approach. The idea is not that necessary truths are special because they concern formal or pure conceptual structures that are cut off from application. As we saw, this conception of formality was abandoned by Schlick under the influence of Wittgenstein, and both Carnap and Hahn stress that the problem concerns how to account for the *applicability* of logic and mathematics to the empirical (Carnap 1963a: 48; Hahn 1931: 32). The linguistic approach, like the subjective-constitutive strategy in general, is meant to explain how we can know that the world we encounter must conform to certain principles, and it does this by locating these principles in the framework we use to represent the world in language.

Second, the linguistic approach should not be equated with what we may

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call ‘conceptual Platonism’ – the view that necessities are to be understood in terms of something like brute facts about meanings/concepts (cf. the Gödel quote above) which are independent of our linguistic practices. This would involve introducing a subject-independent standard which introduces the kind of agreement problem that both Kant and the logical positivists were determined to dissolve via the use of the subjective-constitutive strategy, and which, for the latter, was meant to make a consistent empiricism possible.

It is worth noting that the problem here isn’t talk about abstract meanings or concepts *as such*, but rather the idea that brute facts about some such domain can do any *explanatory* work here. The point is that if we maintain, say, that triangles necessarily have three sides because the concept *triangle* stands in a certain relation to the concept *three-sided*, but *deny* that this can be understood simply in terms of how we have decided to use the expressions ‘triangle’ and ‘three-sided’ (or ‘having three sides’), then this raises the question of how we know that this conceptual relationship obtains.

**Part B**

**Defending the Essentials**



## Transition to Part B

The general aim of the linguistic approach, then, is to explain necessary truths – that is: how they get to be necessary and how we get to appreciate this (cf. Dummett’s question) – in terms of subjective and normatively autonomous principles that govern how we represent the world in language. These principles, moreover, are reasonably described as *conventions* if there are alternative principles that could have been chosen instead.

Illustrating the model with a simple (but by no means unproblematic) example, the idea might be that the necessity of ‘all triangles have three vertices’ is to be explained (at least in part) by there being a linguistic rule (adopted, presumably, by convention) to the effect that ‘triangle’ correctly applies to an object *only if* it is a polygon with three vertices. This is meant to explain the *necessity* of the claim because the rule is meant to be what sets limits to our conception of what a triangle *could be*.

As to the epistemological side of the problem, these considerations are meant to be available to us by reflecting on how we use linguistic expressions, and there is supposed to be no room for the worry that the verdicts we reach in this manner are mistaken relative to what triangles are “really like”. This is because our rule for applying the word ‘triangle’ is meant to be constitutive of the very notion of a triangle. Hence, there is no room for the worry that that real triangles might fail to conform to our linguistic conception of them – and therefore no “mysterious parallelism”, no problem of inexplicable agreement.

It is worth noting, moreover, that this would allow us to know “in advance” that any triangle we encounter will conform to this pattern (cf. e.g. Schlick 1925: 75), and that it also explains why it appears misguided to carry out anything like an empirical investigation where we count the vertices of a representative sample of triangles. I mention this because it captures some of the traditional idea that such knowledge is *a priori*.<sup>24</sup>

In this part, I shall be concerned with the following question: Is the explanatory model that I have just illustrated even a coherent one? A prevalent attitude today is that it is not, and over the course of the next three chapters I shall consider the main arguments that underlie this attitude.

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<sup>24</sup>More generally, however, the notion of the *a priori* is notoriously difficult to pin down (for some of the complexities involved, see Kitcher 1980, Casullo 1988, Giaquinto 1996: §4 and Jenkins 2008: §1). There is, for instance, the question of whether introspectively reflecting on the linguistic rules should count as a kind of “experience”. As I see it, questions like these are not essential to our main concerns, which is part of the reason why I am chiefly approaching these issues via the comparatively clearer notion of necessity.

## Language, Truth and Necessity

The linguistic approach tends to be associated – or even downright identified – with the following line of thought:

(TRUTH-CONTRAST.) Necessary claims are not true in the way that contingent claims are true. In particular, they are not made true by “the facts” or “the world”. Instead, they are made true by meanings or linguistic conventions, or perhaps not really made true by anything at all, since they are *prescriptions* rather than assertions. This, in turn, explains why such claims are assertible independently of what the facts are, which is what their necessity consists in.

This association provides the basis, especially in much recent literature, for the rejection of the linguistic approach, since (TRUTH-CONTRAST) is said to fall victim to a number of conclusive objections.<sup>1</sup>

The main aim of this chapter is to show that this move is unwarranted, since, although (TRUTH-CONTRAST) should indeed be rejected, the linguistic approach was never committed to this idea in the first place. A secondary aim, however, is to show that some of the standard objections that tend to be levelled against (TRUTH-CONTRAST) are in fact much less convincing

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<sup>1</sup>Boghossian (1996), Hale (2013: §§5.2–5.3), Sider (2011: §6.5), and Williamson (2007: ch. 3) all target essentially (TRUTH-CONTRAST).

that they are often taken to be. This is important, for our purposes, since these objections, if sound, would also tell against the linguistic approach.

I begin by introducing various versions of (TRUTH-CONTRAST), before considering some standard objections that do not, I argue, hit the mark. I then provide what I take to be the *right* reason for rejecting this idea, explain why there was no reason for saddling the linguistic approach with (TRUTH-CONTRAST) in the first place, and argue that, moreover, there is little reason to think that the logical positivists endorsed this idea.

## 4.1 Versions of the Truth-Contrast Thesis

We can helpfully distinguish at least four versions of (TRUTH-CONTRAST):

- (i) *Truth in virtue of meaning.*<sup>2</sup> Here the thought is that, unlike contingent sentences, which, if true, owe their truth partly to their having the meaning they do and partly to the way the world is, a necessary truth ‘owes its truth value completely to its meaning, and not at all to “the facts”’ (Boghossian 1996: 363).

Such sentences would be, in Boghossian’s terminology, ‘metaphysically analytic’, where the analyticity is *metaphysical* (rather than *epistemological*) because it concerns, not how we might discover or justify the truth of the sentence, but what is responsible for making the sentence true. If we assume, now, that what a sentence means is determined by linguistic conventions, then we can presumably say that these are responsible, in the end, for making such sentences true.

- (ii) *Truth by convention.* The slogans ‘truth by convention’ and ‘truth in virtue of meaning’ are not always kept apart, but when they are, ‘truth by convention’ tends to suggest the more specific idea that necessary truths are true because they have been stipulated (perhaps

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<sup>2</sup>Obviously, this notion has been spelled out in a number of different ways. The following characterisation is not intended to do justice to all of these.



implicitly) to be so. That is, there is a convention in force to the effect that these sentences are to be treated as true come what may.

Understood like this, rejecting the notion of ‘truth by convention’, need not involve rejecting the notion of ‘truth in virtue of meaning’. It can be consistently maintained that ‘there is no truth by convention; there is only meaning by convention, and then truth in virtue of meaning’ (Coffa 1991: 321). Typically, the idea is that the sentences which are stipulated to be true contain some previously undefined term which thereby acquires meaning. In this case, necessary truths could be regarded as true by ‘implicit definition’ in Hilbert’s sense.

- (iii) *Resolute normativism*. A more radical idea is that necessary “truths” are not descriptive and therefore, in fact, not *true* at all, but rather *rules* which prescribe how linguistic expressions are to be used. Here is Ayer proposing this idea:

They make no statement whose truth can be accepted or denied.  
They merely lay down a rule which can be followed or disobeyed.  
Their necessity then, we must say, consists in the fact that it does not make sense to deny them. If we reject them we are merely adopting another usage from that which they prescribe.  
It is in this sense only that they cannot conceivably be false.  
(Ayer 1936b: 20)<sup>3</sup>

- (iv) *Modified normativism*. Resolute normativism immediately leads to difficulties of the Frege-Geach kind (Geach 1965), and, as Ayer later admitted (1946: 17), it is pretty undeniable that, by ordinary standards, necessary truths are indeed true. Resolute normativism is only, therefore, an option for someone not deterred by massive revisionism.

In response to this, it has been suggested that the spirit of normativism is quite consistent with attributing truth to necessary claims

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<sup>3</sup>This seems to signal a change from Ayer (1936a), published earlier that year.

#### 4. Language, Truth and Necessity

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in some minimal – typically deflationary – sense.<sup>4</sup> We may even allow that there are modal facts as long as this notion of a ‘fact’ is given a similarly deflationary reading (Thomasson 2009: 17). Still, the idea remains that, in the case of necessary claims, assertibility is a matter of a rule being in force, rather than the world being thus-and-so.

These four positions are all versions of (TRUTH-CONTRAST) insofar as they deny that necessary sentences are ‘made true by the facts’. This claim, of course, goes beyond the claim that necessary truths have a *special* relationship with the truth – they are, after all, *necessarily* true. Rather, the idea is that being a necessary truth is not just a matter of being a truth which is, in addition, necessary; being true (or, more generally, assertible) is *itself* supposed to be a very different matter here.

It is not always clear just how deep the contrast is supposed to be – for instance, whether we are supposed to regard ‘true’ as *ambiguous*. This would, perhaps, be a somewhat peculiar view,<sup>5</sup> so it is worth noting that (TRUTH-CONTRAST) does not entail such ambiguity. As Williamson puts it:

Analytic truths and synthetic truths are true in exactly the same central sense of ‘true.’ That is compatible with their being true in very different ways, just as being a mother and being a father are two very different ways of being a parent; ‘parent’ is not ambiguous between mothers and fathers. (2007: 58)<sup>6</sup>

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<sup>4</sup>Karl Britton is an early advocate of this idea (1947). More recent advocates include Hans-Johann Glock (1996; 2008) and Amie Thomasson (2007a; 2009; and 2013). For a number of criticisms, see Kalhat (2008a and 2008b).

<sup>5</sup>Although it is explicitly endorsed by Felix Kaufmann (1944: 66).

<sup>6</sup>However, Williamson immediately goes on to deny that there is such a distinction between two different ways of being true.

## 4.2 Standard Objections

### Sentences and Propositions

The contemporary literature contains plenty of arguments against (TRUTH-CONTRAST), many of which target the more specific idea that linguistic conventions alone can make sentences true – barring, perhaps, in cases where the sentence is a metalinguistic one.

One common such objection makes heavy weather of the distinction between sentences and propositions:

Given standard English usage, the sentence ‘bachelors are unmarried’ is true; if English had involved different conventions about what words mean, the sentence might have meant something different, and so it might have been false. However, the *proposition* expressed by the English sentence – that bachelors are unmarried – does not depend for its truth on how English works. (Sober 2000: 247)<sup>7</sup>

The point, of course, is not just that in addition to sentences, there are other truth-bearers – propositions – which do not owe their truth to linguistic conventions. This, after all, would leave room for the idea that (TRUTH-CONTRAST) is quite correct as a thesis about sentences. The further point is that if we concede that the truth of the proposition is explained by something non-conventional, then we are forced to concede this in the sentential case as well. This is indeed very plausible; it would be odd if the explanation of why a proposition is true were irrelevant to the explanation of why a sentence which expresses this proposition is true.

However, we have yet to be given any reason to accept that linguistic conventions cannot explain the truth of propositions, and this can hardly be taken for granted in the present context.

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<sup>7</sup>Variations on this point have been made by e.g. Ewing (1940: 219–20), C. I. Lewis (1946: 96–7), Pap (1958: 169–70), Lewy (1976: 58), Yablo (1992: 878), Boghossian (1996: 365), and Hale (2013: 120).

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In providing such a reason, moreover, there is a danger of relying on a question-begging conception of propositions.<sup>8</sup> Notably, we cannot just insist that propositions are abstract, language-independent entities, since ‘the advocate of the linguistic theory is likely to scoff at the suggestion that necessity is an intrinsic property of extralinguistic propositions’ (Pap 1958: 166). One such advocate, Jared Warren, describes the question-begging picture of propositions in a bit more detail:

This picture seems to assume that there are the propositions, somewhere *out there* all arrayed. They toil not, they spin not; they are timeless and forever. We corporeal beings work not with propositions but with sentences. Our conventions generate meaningful sentences simply by *attaching them* to particular propositions – like price tags at the grocery store. (2015b: 90)

If we assume some such picture, then it will indeed be natural to deny that our linguistic conventions could ever explain why a proposition is true. The propositions are all there – with their truth-values – quite independently of what the linguistic conventions are. However, no argument which presupposes this picture will be persuasive to an advocate of (TRUTH-CONTRAST).<sup>9</sup>

Merely drawing the sentence-proposition distinction does not, then, suffice to undermine (TRUTH-CONTRAST). If there is a cogent objection here, it must concern, not the distinction as such, but our reasons for thinking that linguistic conventions cannot be responsible for the truth-values of propositions.

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<sup>8</sup>In a footnote, Sober cites a passage by Boghossian which is meant to suggest that the argument works regardless of what propositions are like. This, however, is misleading since the relevant passage is aimed at establishing a different point, namely that recognising implicit definition as a route to *a priori* knowledge does not force us to accept that the resulting sentence is made true by the implicit definition (Boghossian 1996: 380).

<sup>9</sup>Thus, Giannoni defends his conventionalism by denying that complex propositions exist independently of being expressed by sentences (1971: 104). Whereas García-Carpintero and Pérez Otero, argue that the linguistic approach can meet the present objection by replacing what they call ‘P-propositions’ with ‘T-propositions’ (2009).

## English Chauvinism

In fact, Sober does provide such a reason:

There are bachelors in France who would very much resent the English chauvinism involved in saying that the way *English* works affects whether (French) bachelors are unmarried. ‘Quelle impertinence!’ one can hear them exclaim. The fact that bachelors are unmarried is no more dependent on English than it is on French. (2000: 247)<sup>10</sup>

Now, the rhetoric here is somewhat misleading. Of course, no bachelor, French or otherwise, should think that the linguistic conventions explain why *he* is unmarried. There might be all sorts of reason for *that*. What the linguistic conventions are supposed to explain is, rather, why he would cease being a bachelor if he did marry. Looking past this, we are left with something like the following argument (where *E* is some English sentence, and *F* a synonymous French one):

- (1) *E* and *F* express the same proposition.
- (2) The conventions of English are not the reason why the proposition expressed by *F* is true.
- (∴) The conventions of English are not the reason why the proposition expressed by *E* is true.

Though there are some complications that could be discussed in more detail, I am prepared to grant that this argument is valid. Premise (1), moreover, is conceded by assumption, so the question is whether we should accept (2).

Insofar as Sober provides an argument for (2), it hinges on the idea that failing to accept it would be an expression of English chauvinism. Certainly, it would be chauvinistic to say that French is governed by the conventions of English, but rejecting (2) does not require us to put matters in this way. It is enough to say that English and French might *share* some linguistic

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<sup>10</sup>Essentially the same point is made by Ewing (1940: 217–9; 1951: 41), Lazerowitz (1972: 235), Moore (1954: 311–2), and Lewy (1976: 2; 58).

conventions, or that the conventions of English and those of French might *share those features* that are (supposedly) responsible for the truth of the proposition in question. After all, if we are really dealing with synonymous sentences, we should expect a great deal of overlap between the relevant conventions of English and those of French (since that is the natural explanation of how the sentences got to be synonymous in the first place).

We would, of course, expect the *expressions* treated by the conventions of English to differ from those treated by the conventions of French. But unless what is supposed to account for the truth of the proposition concerns these particular expressions, there are not really any grounds for the accusation of English chauvinism here. And we should now note that advocates of the linguistic approach often insisted that the particular expressions are precisely *not* what is important. Here is Carnap – who appears to be Sober’s main target – in *LSL*:

From the syntactical point of view it is irrelevant whether one of two symbolical languages makes use, let us say, of the sign ‘&’ where the other uses ‘•’ (in word-languages: whether the one uses ‘and’ and the other ‘und’) so long as the rules of formation and transformation are analogous. (Carnap 1937: 6)

Indeed, Carnap stresses that whether a sentence is analytic, or an inference valid, is fundamentally a matter of the *formal structure* of the language, which is what notational variants have in common (1937: 5–6).

Against this background, it is not too difficult to imagine what Carnap would have said if someone objected, say, that the rules of English cannot explain the validity of conjunction elimination since this rule is valid in French as well.

Strawson makes a similar but more natural-language-oriented point. In the course of defending the view that logical properties are explicable in terms of linguistic rules, Strawson considers the objection (very much like Sober’s) that this cannot be right since the same logical statement can be

expressed in different languages governed by what seem to be different rules (1952: 10–1). He replies, however, that:

We might say that these were really different versions of the same rule; that in laying down inconsistency-rules in one language, we are implicitly laying down inconsistency-rules for the corresponding expressions in all languages; and that thus a linguistic expression *of the kind quoted* transcends the language of the words which it mentions. (1952: 11; emphasis in original)

It might seem like an extraordinary feat to implicitly lay down a rule for *all* languages, but the point is that this falls out of the criteria for translation:

When you draw the boundaries of the applicability of words in one language and then connect the words of that language with those of another by means of translation-rules, there is no need to draw the boundaries again for the second language. (Strawson 1952: 12)

Perhaps it will be said that it surely remains chauvinistic to say that the truth of a French sentence could be explained by English rules via rules of translation, but this is to get Strawson backwards. The point is, firstly, that the explanation offered in terms of the rules of English might be available in other languages since the rules of other languages might be ‘really different versions of the same rule’, and, secondly, that there is no mystery concerning how an explanation arrived at by reflecting on the rules of English could be universally applicable. If a statement is found to be inconsistent via the rules of English, any sentence (of any language) expressing that statement will be inconsistent. For it expresses the same statement because it is governed by relevantly similar rules.

I conclude, then, that we have not been given sufficient reason to accept (2) in Sober’s argument, and that it is therefore inconclusive against (TRUTH-CONTRAST).

## Pre-Linguistic Times

Before moving on to what I take to be the right reasons for abandoning (TRUTH-CONTRAST), I want to consider a passage from Paul Boghossian which has informed much of the recent discussion. In ‘Analyticity Reconsidered’, he objects to the notion of metaphysical analyticity as follows:

What is far more mysterious is the claim that the *truth of what the sentence expresses* depends on the fact that it is expressed by that sentence, so that we can say that what is expressed wouldn’t have been true at all had it not been for the fact that it is expressed by that sentence. Are we really to suppose that, prior to our stipulating a meaning for the sentence

Either snow is white or it isn’t

*it wasn’t the case that either snow was white or it wasn’t?* Isn’t it overwhelmingly obvious that this claim was true *before* such an act of meaning, and that it would have been true even if no one had thought about it, or chosen it to be expressed by one of our sentences? (1996: 365)<sup>11</sup>

There are, as I see it, a number of things going on in this passage, and though I do think it contains the seed of a telling objection against metaphysical analyticity, it also suggests some much less telling ones.

First of all, the talk about ‘the truth of what the sentence expresses’ and the assertion that ‘the claim’ was true before we decided to use a sentence to express it, both suggest that Boghossian is here making the point that the truth-values of propositions are independent of linguistic conventions. We have already seen that this objection is not at all decisive. Indeed, if by ‘claim’, Boghossian means ‘proposition’, then he appears to be assuming the question-begging picture of propositions sketched above. If, on the other hand, it means ‘sentence’, then, plainly, the claim was not true before we

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<sup>11</sup>Cf. also Stroud (1981: 242–3), Williamson (2007: 71–2) and Sider (2011: 102).



established a use for it., This matter, then, is not as overwhelmingly obvious as Boghossian suggests.

What is obvious – and I do think that this is the point that Boghossian wants to make – is simply that snow was white or not white before there were any languages around.<sup>12</sup> We need to tread carefully here, however, since we are in the vicinity of a famously dubious objection:

This short way with the issue reminds one of Lenin’s (disastrously incompetent) polemical book against Machian positivism. Lenin simply claimed that positivists, since they took human sensations as the class of truth-makers for all propositions (I am using the present-day terminology, not Lenin’s, of course), could not accept the statement that the solar system existed before there were human beings.<sup>13</sup> This argument simply assumes – what the positivists of course deny – that the positivists cannot *interpret* ‘the solar system existed before there were human beings’ in their rationally reconstructed “language of science”. (Putnam 2007: 158)

Putnam is here complaining about Michael Devitt’s approach to the realism/anti-realism debate, but, on the face of it at least, a similar complaint can be levelled against Boghossian’s ‘short way’ with metaphysical analyticity. He appears to suggest, after all, that adherents of (TRUTH-CONTRAST) cannot accept – what we all think is obvious – that snow was white or not white before there were any languages around. But nothing has been said to rule out the idea that the sentence ‘snow was white or not white before there were any languages around’ is made true by our *current* linguistic conventions, which is presumably what adherents of metaphysical analyticity would want to say anyway.

Certainly, this response is itself not unproblematic. In fact, we are touching upon a topic which will take centre stage in the next chapter. For

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<sup>12</sup>Note that if this is Boghossian’s point, then Eric Loomis’s response (2004) misses the mark.

<sup>13</sup>I suspect that this is somewhat apocryphal. We do not find this exact claim in Lenin’s book, although we do find similar ones (1909: 78–9).

the time being, however, my point is simply that if Boghossian is claiming that the adherents of (TRUTH-CONTRAST) are committed to denying obvious truths about the past or counterfactual scenarios, then his objection is much too quick.

### 4.3 In Favour of Ordinary Truth

We have seen, then, that neither a bare appeal to propositions, inter-linguistic considerations, or platitudes about pre-linguistic times suffice to undermine (TRUTH-CONTRAST). However, in this section I argue that it should nevertheless be rejected.

It is often objected against (TRUTH-CONTRAST) that the special kind of truth – i.e. ‘truth in virtue of meaning’ or ‘by convention’ – that necessary truths are said to possess is obscure: ‘This line is itself fraught with difficulty. For how can we make sense of the idea that something is made true by our meaning something by a sentence?’ (Boghossian 1996: 365) ‘Here I immediately come up against a stumbling block: what can be meant by saying that a statement *follows from the very meaning of its terms*? I should have thought that *one* statement can follow from *another*; but from the meaning – !’ (Waismann 1949: 27)

Such objections are certainly fair, but they are necessarily somewhat inconclusive. An advocate of (TRUTH-CONTRAST) will be quick to respond that even if they haven’t quite succeeded in spelling out this peculiar kind of truth so far, this is no reason to stop trying (although at some point a pessimistic induction presumably becomes warranted).

The more conclusive objection comes from the other direction: Instead of arguing that we lack an adequate account of the *special* kind of truth which necessary truths are said to possess, I shall argue that we lack an account of *ordinary* truth which justifies restricting this notion to contingent truths. I shall argue, that is, that insofar as we want to say that contingent truths are made true by “the facts” or “the world”, we have no reason to

refrain from doing so in the case of – at least some – necessary truths. This line, if successful, would undermine (TRUTH-CONTRAST) quite generally, including the normativist lines.

This is where I think the passage from Boghossian provides at least the starting point for a cogent objection. For rather than making the point that adherents of (TRUTH-CONTRAST) cannot say that snow was white or not white before languages were invented, this passage can be read as a reminder of the (trivial) point that the colour of snow is in no way influenced by linguistic matters. This can then form the basis of the following argument:

- (1) The colour of snow is a language-independent feature of the world.
- (2) ‘Snow is white or snow is not white’ is made true (at least in part) by the colour of snow.
- (∴) So, ‘Snow is white or snow is not white’ is made true (at least in part) by a language-independent feature of the world.

Now, as it stands, (2) is blatantly question-begging, since this is presumably something an adherent of (TRUTH-CONTRAST) would deny. The problem, though, is that it is quite difficult to find a principled reason for denying this (assuming that we are happy to talk about truthmaking at all).

Consider the disjunction ‘snow is white or snow is purple’. Presumably we may say that this sentence is true because one of its disjuncts is true, and if asked why this disjunct is true, we may say that this is because snow is indeed white. In the absence of a more specific account of what truthmaking is supposed to be, I take it that this is what is meant by saying that the above sentence is made true by the colour of snow (or the fact that snow is white).

However, we can of course say exactly the same thing about ‘snow is white or snow is not white’ (hereafter I call this sentence ‘*S*’, and the sentence ‘snow is white’ ‘*D*’), which suggests the following argument for (2):

- (1\*) *S* is true because it has a true disjunct *D*.
- (2\*) *D* is made true by the colour of snow.

(∴) So,  $S$  is made true by the colour of snow.

Though I don't expect advocates of (TRUTH-CONTRAST) to be immediately convinced by this brief argument, it adds further pressure since it pinpoints exactly what they are committed to denying. (2\*), after all, is uncontroversial in this context, so (1\*) must be the culprit.<sup>14</sup>

It is undeniable that  $D$  *suffices* for the truth of  $S$ , but that is not to say that this is what explains why  $S$  is true. Analogously, the fact that I'm vaccinated against a particular disease might suffice to ensure that I won't get ill, but it need not be the reason why I don't (I might never get in contact with the pathogen). A natural suggestion is that the vaccine is not the reason here because we don't get the appropriate counterfactual sensitivity: I would have been healthy even if I hadn't been vaccinated. And this line of thought would seem to give us reason to deny (1\*). For it is clear that  $S$  would have been true even if  $D$  had been false, suggesting that  $D$  being true is not the reason why  $S$  is true after all.

Something like this appears to be Amie Thomasson's line of thought. Thomasson advocates a version of what I am calling 'modified normativism' (variously under the labels 'modal normativism' and 'non-descriptivism about modality'), according to which both modal claims and analytic claims are prescriptive rather than descriptive. Although she is prepared to accept that analytic claims are true 'in a straightforwardly truth-conditional sense' (2007a: 147), she insists that this does not show that they require – or indeed have – truthmakers, which is, she maintains, the crucial issue in this context (2007a: 136–7).

Assessing this claim is somewhat difficult since Thomasson never provides any account of just what it takes for something to be a truthmaker for a sentence. It is, therefore, not entirely clear what the difference be-

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<sup>14</sup>I am assuming that the argument is accepted as valid. This may be contested because it assumes that truthmaking is transitive, and there are reasons for denying this (Tahko 2013). However, the reasons for denying transitivity would also be reasons to denying (1\*), and I don't think anyone who *accepts* (1\*) would want to deny that the reason why the disjunct is true is the reason why the disjunction is true. Thus, adherents of (TRUTH-CONTRAST) remain committed to denying (1\*).

tween having a truthmaker and ‘being true in a straightforwardly conditional sense’ is supposed to be.

My best attempt to make sense of this, however, is to utilise the above distinction between being *sufficient* for truth and *explaining* truth. Since, as we have seen, the colour of snow suffices to make *S* true, *S* is true in a straightforwardly conditional sense, but this is not yet to say that the colour of snow acts as a truthmaker here since this might be redundant in the proper explanation of why it is true (as the counterfactual insensitivity suggests). This is, I think, what Thomasson has in mind when she, after conceding that ‘all bachelors are male’ is straightforwardly true,<sup>15</sup> goes on to write:

But actual bachelors and their features are not truthmakers for the claim, for the analytic claim is guaranteed to be true regardless of any features of the world: it is vacuously true even if there are no bachelors whatsoever. (Thomasson 2007a: 148)

Now, I am not convinced that there is anything here which gives us reason to say that ‘all bachelors are male’ lacks a truthmaker whereas, say, ‘all humans are mortal’ has one. Certainly, universal claims are somewhat awkward for the truthmaker terminology *in general*, but Thomasson needs a contrast.

The point about the analytic claim being true even if there are no bachelors does, of course, *not* give us such a contrast. But the only suggestion, apart from this, is that there is no truthmaker since the sentence is ‘true regardless of any features of the world’. This, however, is presumably just to say that it is necessary, and it is clear that this cannot be the *criterion*, in the present context, for whether there is a truthmaker since that would make the question that Thomasson is interested in trivial.

A more promising route is to invoke the notion of counterfactual insensitivity to justify the inference from ‘this sentence would have been true

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<sup>15</sup>I suspect that this might well be *false*, but let us set this aside.

regardless of the features of the world' to 'the features of the world do not explain why it is true'.

Let us now probe this idea a bit further. Because of the awkwardness of universal claims, let us switch back to our sentence  $S$ .<sup>16</sup> The claim, then, is that the colour of snow is not what makes  $S$  true since it would have been true regardless of what the colour of snow is like. Just like the vaccination is not what makes me disease-free if I would have been disease-free regardless since I never encountered the pathogen.

However, we need to distinguish two cases: Do we get counterfactual insensitivity because there is an independent explanation even in the actual scenario (as in the vaccination case), or because a *new* explanation is available in the counterfactual scenario? The sentence 'Germany or Argentina won the 2014 World Cup' is true, and what makes it true is presumably that Germany won it. This is so *even though* the sentence would still have been true had they not won it (since they played Argentina in the final). Here counterfactual insensitivity gives us no reason to reject our original explanation, and it is quite clear why: In the counterfactual scenario a *different* explanation is available.

The question, then, is which category our instance of excluded middle falls into, and it is quite clear, I think, that there is no reason, on the face of it at least, not to put it in the second category. The reason why we have counterfactual insensitivity here is that a *different* explanation is available in the counterfactual scenario: If the colour of snow had been different, then this alternative colour would have made the second disjunct of  $S$  true.<sup>17</sup>

What this shows is that merely pointing to counterfactual insensitivity does not give us reason to abandon (1\*). If, therefore, the adherent of (TRUTH-CONTRAST) wants to reject it (as they have to), their reason must be the more specific idea that there is an independent reason why  $S$  is true

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<sup>16</sup>It should be noted, though, that Thomasson sets logical and mathematical truth aside (2007a: 135), so it's not clear what she would say about instances of excluded middle. Still, the main lessons below do carry over to the cases of analyticity which she does discuss.

<sup>17</sup>I am setting aside complications having to do with vagueness.

which has nothing to do with the colour of snow, indeed, nothing to do with either disjunct being true – e.g. that it has been stipulated to be true, or that there is a norm which says that it shouldn't be denied.

This is the point at which (TRUTH-CONTRAST) unravels. First, we are heading for a rather glaring violation of Occam's Razor: Not only have we introduced two distinct routes to truth for disjunctions – by having (at least) one true disjunct or by being made true by something like our linguistic conventions (which, it may be noted, certainly do *not* suffice to make either disjunct true),<sup>18</sup> it also turns out that the first route alone is perfectly adequate to account for the truths that need accounting for.<sup>19</sup>

Second, and more importantly, no *additional* route can solve the problem which (TRUTH-CONTRAST) was meant to solve. The strategy, recall, was to explain necessity in terms of the special kind of truth (or normative status) which is conferred on the sentence independently of what the facts are – and so explains its necessity. But now we are saying that the sentence is *also* made true by the facts (in an appropriately bland sense). And as soon as we make this concession, we realise that not only is it made true by the facts, it is *necessarily* made true by the facts, and this is something the additional kind of truth is quite unable to explain.

We can summarise the situation as follows: We ordinarily think that when a disjunction is true, then there are three (or two – depending on how we count) potential reasons for this:

- (i) ... because the first disjunct is true.
- (ii) ... because the second disjunct is true.
- (iii) ... because both disjuncts are true.

An adherent of (TRUTH-CONTRAST) cannot think that this list is exhaus-

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<sup>18</sup>Sider also presents an objection along these lines (2011: 104). Azzouni claims that the objection begs the question (2014: 47), but provides little detail.

<sup>19</sup>With the possible exception of instances of excluded middle with vague vocabulary and borderline cases, but if we are inclined to say that neither disjunct is true here, then we might be inclined to say that the disjunction isn't true either (unless we are attracted to supervaluationism).

tive since whichever of these scenarios obtain, it is going to be because the world is the way it is. They must therefore add something like:

(iv) ... because it is stipulated to be true (or accepted as a rule).

The problem, however, is that a sentence like  $S$  appears to be necessarily true even if we explicitly restrict ourselves to (i)–(iii) which removes our incentive for introducing (iv) in the first place. In other words, it is *necessarily* true that one of (i)–(iii) will hold in the case of  $S$  (given its meaning), and (iv) is powerless to explain this fact. Consequently, *adding* (iv) as an additional route to truth/assertibility has not provided us with the sought after explanation of necessity – and now Occam’s Razor really kicks in.

The upshot is that the motivation behind (TRUTH-CONTRAST) was misguided from the beginning. The idea was that some sentences are necessary because they are true/assertible for reasons that have nothing to do with what the facts are. We have seen, first, that we are forced to concede that such sentences are, in any case, *also* true/assertible for ordinary, factual reasons,<sup>20</sup> and, second, that they remain true even if we restrict ourselves to such reasons, making the hypothesis that there is some additional, special reason why they are true/assertible redundant.

### 4.4 From Truth to Necessity

(TRUTH-CONTRAST), then, should be rejected, and this might well be thought to be bad news for the linguistic approach, since the two are often assumed to be closely related.

However, there is no reason – at least on the face of it – why an adherent of the linguistic approach should be committed to this line of thought. The aim, recall, is to explain *necessity*, and the idea is to do so by appealing

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<sup>20</sup>More accurately, we have seen that this must be conceded in *some* cases. Nothing I have said shows that we must do so in the case of e.g. mathematical truths.



to the rules that govern linguistic expressions. This, however, is not to say that these rules must also provide the reason why these sentences are *true*.<sup>21</sup>

The idea that adherents of the linguistic approach are committed to (TRUTH-CONTRAST) is thus in need of argument. There aren't many such arguments in the literature, but Boghossian appears to present one. Having rejected the notion of metaphysical analyticity, he writes:

Why, if this idea is as problematic as I have claimed it to be, did it figure so prominently in positivist thinking about analyticity?

Much of the answer derives from the fact that the positivists didn't merely want to provide a theory of a priori knowledge; they also wanted to provide a reductive theory of necessity. The motivation was not purely epistemological, but metaphysical as well. Guided by the fear that objective, language-independent necessary connections would be both metaphysically and epistemologically odd, they attempted to show that all necessities could be understood to consist in linguistic necessities, in the shadows cast by conventional decisions concerning the meanings of words. Conventional linguistic meaning, by itself, was supposed to generate necessary truth; a fortiori, conventional linguistic meaning, by itself, was supposed to generate truth. Hence the play with the metaphysical concept of analyticity. (1996: 365)

As I read him, Boghossian is here saying that *if* your aim is to provide a reductive theory of necessity (as arguably was the aim of the logical positivists), then you have a reason to hold that some truths are metaphysically analytic. Furthermore, if your specific theory is that necessary truth is 'generated' by linguistic meaning (as is maintained by adherents of the linguistic

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<sup>21</sup>Sidelle too makes this point (1998). In fact, the term 'neo-conventionalism' has recently been used to denote positions that reject 'truth by convention' in favour of 'necessity by convention' (see e.g. Livingstone-Banks 2017). The next section will make it clear why I find this terminology misleading. I should also say that some versions of this "neo-conventionalism" (see Cameron 2009 and 2010, as well as Sider 2003: §4.2) do not really qualify as versions of the linguistic approach since there is no attempt to explain necessities in terms of linguistic rules in the relevant sense.

approach – at least if we squint a bit), then you *must* hold this, since, *a fortiori*, linguistic meaning would then have to generate truth.

This, however, is a bad argument. First, the *a fortiori* inference is dubious in general: A soda maker could be said to generate carbonated water, but it doesn't *a fortiori* generate water. Second, Boghossian's specific use of the inference appears to equivocate on 'generate'. There are two senses in which an adherent of the linguistic approach could be expected to accept that the linguistic conventions 'generate' the necessary truth of some claim *T*. It could mean a) that the conventions provide the explanation for why *T* is necessarily true, or b) that they suffice to ensure that *T* is necessarily true. If 'generate' is understood along the lines of (a), then the inference is invalid since there is no reason to think that the explanation of why *T* is necessarily true must '*a fortiori*' explain why *T* is true. But if we go with (b), then the conclusion poses no problem, since we can accept that the conventions suffice to *ensure* that *T* is true without having to say that they provide anything like the truthmaker for *T*. Boghossian's argument, therefore, is unconvincing.

Bob Hale also suggests that we can move from a thesis that adherents of the linguistic approach can be expected to accept to something like (TRUTH-CONTRAST). In his discussion of 'conventionalism', he defines this view, at the outset, as 'the thesis that necessary truths are simply truths guaranteed by conventions governing the use of words' (2013: 117). And we might well think that an adherent of the linguistic approach is committed to something along these lines. However, Hale then goes on to object, variously, that conventions cannot *create* truth (122), that necessities cannot be explained as truths *grounded* in meaning (117), and that conventions cannot *make* statements (118) or propositions (120; 122–3) true.

We are left wondering, though, how all of this is supposed to bear on the thesis that was originally formulated. This thesis certainly does not seem to *entail* any of the ideas that are later attacked. After all, the special theory of relativity guarantees, I suppose, that I'm not moving at a speed

exceeding  $c$ , but if I say that this is what *makes it true* that I'm not moving at such a speed, then I'm giving my legs too much credit. Nor would I say that my pace is *grounded* in Einstein's work or that his 1905 paper *created* the truth in question.

Neither Hale nor Boghossian, then, provide adequate reason for thinking that an adherent of the linguistic approach must be committed to (TRUTH-CONTRAST).

The idea that necessary truths are true in a quite ordinary sense does, however, lead to a natural worry against the background of the subjective-constitutive strategy. According to this strategy, after all, knowledge of necessities consists in reflective knowledge of principles – in this case linguistic rules – which are supposed to be exempt from any external standard of correctness. But, it may be asked, how could this be if necessity implies truth and a sentence is true just in case the world is such that the sentence's truth-conditions are satisfied? How could the rules be exempt from any external standard of correctness and yet be capable of guaranteeing truth – *ordinary* truth?

We shall return to this issue in Chapter 9. For now, I just want to briefly outline the general idea in order to slightly alleviate the worry: What the adherent of the linguistic approach insists is that when we establish that a sentence is necessarily true, we don't start from knowing something about what the world is or must be like and conclude that the truth-conditions of this sentence must be satisfied. Rather, we establish – purely on the basis of the linguistic rules which determine what the truth-conditions are – that these cannot but be fulfilled. Roughly speaking, the idea is that due to how we have set up the truth-conditions, we can know, in certain special cases, that the world will be such as to fulfil them quite independently of what it is actually like.<sup>22</sup>

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<sup>22</sup>For responses to Boghossian that draw on similar considerations, see G. Russell (2008) and Hofmann and Horvath (2008). These responses are, however, problematic for our purposes since they are content with taking the notion of *possibility* for granted.

## 4.5 In Defence of the Logical Positivists

The question remains, though, whether Boghossian is right that (TRUTH-CONTRAST) figured prominently in the work of the logical positivists, via a commitment to metaphysical analyticity. Unfortunately, he does not provide any textual evidence in this connection. Instead his discussion appears to be informed by Quine's paper 'Carnap on Logical Truth', which, it should be remembered, in one version (Schilpp 1963) opens with the disclaimer: 'My dissent from Carnap's philosophy of logical truth is hard to state and argue in Carnap's terms. This circumstance perhaps counts in favor of Carnap's position.'

Somewhat ironically, however, it would have been easy for Boghossian to find textual support for the idea that Carnap was committed to the notion of metaphysical analyticity. Consider, for instance, the opening passage of 'Meaning Postulates': 'Philosophers have often distinguished two kinds of truth: the truth of some statements is logical, necessary, based upon meaning, while that of other statements is empirical, contingent, dependent upon the facts of the world.' (1952: 65)

I am not denying, therefore, that the logical positivists did *on the face of it*, appeal to something like metaphysical analyticity. There are, however, two reasons why I think appearances are misleading here. First, while passages like this are relatively common when Carnap is engaging in "big-picture" talk, a quite different picture emerges when we look at his more detailed treatments of necessary truth.

At the time when he wrote 'Meaning Postulates', Carnap's preferred approach to semantics was via state-descriptions, and this approach is most fully presented in *Meaning and Necessity*. Here we read:

The connection between these concepts and that of truth is as follows: There is one and only one state-description which describes the actual state of the universe; it is that which contains all true atomic sentences and the negations of those which are false. Hence it contains only true sentences; therefore, we call it the true state-

description. A sentence of any form is true if and only if it holds in the true state-description. (1947: 10)

As to ‘the familiar but vague concept of logical or necessary or analytic truth’, Carnap first tells us that ‘this explicandum has sometimes been characterised as truth based on purely logical reasons, on meaning alone, independent of the contingency of facts.’ He then observes that the meaning of a sentence is determined by the semantical rules of the language, and so offers the following condition (described as a convention) which any definition of L-truth (as an explication of the familiar but vague notion) needs to satisfy:

A sentence  $\mathfrak{S}_i$  is **L-true** in a semantical system  $S$  if and only if  $\mathfrak{S}_i$  is true in  $S$  in such a way that its truth can be established on the basis of the semantical rules of the system  $S$  alone, without any reference to (extra-linguistic) facts. (1947: 10)

The explication he goes on to offer is simply that a sentence is L-true (in  $S$ ) if and only if it holds in every state-description (in  $S$ ), but what is more significant for our purposes is what Carnap says in order to show that this definition is indeed in accordance with his condition:

If  $\mathfrak{S}_i$  holds in every state-description, then the semantical rules of ranges suffice for establishing this result. Therefore, the semantical rules establish also the truth of  $\mathfrak{S}_i$  because, if  $\mathfrak{S}_i$  holds in every state-description, then it holds also in the true state-description and hence is itself true. (1947: 11)

This makes it quite clear that Carnap takes the appropriate understanding of ‘truth based on meanings’ to be truth (*ordinary* truth) that can be established via the semantic rules, and that L-truth, like all truth, is a matter of the sentence holding in the actual state-description. There is no basis, here, for a distinction between L-truths and other truths when it comes to what “makes them true”. Indeed, there is a very straightforward way of

introducing truthmakers into this framework should we want to. We could simply say that the truthmakers for a sentence  $R$  is the minimal subset  $M$  of the actual state-description such that  $R$  could be derived from  $M$  via the semantic rules of the relevant language.<sup>23</sup> Thus, returning to Boghossian's example, 'snow is white or not white' would receive exactly the same truthmaker as 'snow is white or purple'.

The second reason why we should be sceptical of the idea that the logical positivists were committed to Boghossian's notion of metaphysical analyticity is that it is far from clear that there is even room for such a notion in their overall outlook.

I am not here thinking of their notorious "rejection of metaphysics", but of their verificationist approach to meaning. It is not clear, after all, that a verificationist can even make the distinction which Boghossian makes between what would *justify* a belief in a sentence and what would *make the sentence true* – at least not if we are talking about *canonical* justifications. Against a verificationist background, talk about what the truth of a sentence is *based on* (cf. the opening of 'Meaning Postulates') just amounts to talk about what would constitute *evidence* for it. Thus, it makes perfect sense for Carnap to spell out the notion of 'truth based on meaning' (or 'logical reasons') as 'truth that *can be established* on the basis of semantical rules' in *Meaning and Necessity*.

Upon scrutiny, then, I think there is little or no evidence that the logical positivists ever bought into the idea of metaphysical analyticity, and I think few of them ever accepted (TRUTH-CONTRAST).<sup>24</sup>

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<sup>23</sup>a) We need a convention to settle cases where there is no *unique* such set. One option would be to take their union in such cases. b) There is also the issue of how to deal with universally quantified sentences (which is appropriate since this is indeed an issue for truthmaker theory).

<sup>24</sup>There are exceptions here. Most explicitly, we did see that Ayer subscribed to resolute normativism, at least briefly. Moreover, the axioms that partly constitute the languages in Carnap's *LSL* can *arguably* be described as 'true by convention'. However, since *LSL* explicitly shuns the notion of truth (1937: §60b), the suggestion that Carnap here subscribes to a version of (TRUTH-CONTRAST) is problematic. For a thorough discussion of whether Carnap was committed to 'truth by convention', see Ebbs (2011).

## The Contingency Problem

In the previous chapter we saw that one of the standard objections against the linguistic approach – that it is a mistake to think that the linguistic conventions explain why necessary sentences are true – is essentially correct, but irrelevant since it is enough that their *necessity* is so explained.

This, however, brings another standard objection into focus. For to say that necessity could be explained by something conventional is, so the charge goes, at best mystifying and potentially a straightforward contradiction:

If words are to be taken in their ordinary sense, it seems obvious that necessary truths cannot be true by convention. In common speech the word ‘necessary’ means much the same as ‘indispensible’, ‘inevitable’, ‘without any possible alternative’. [...] But in common usage the phrase ‘by convention’ always implies the possibility of an alternative. [...] It seems clear, therefore, that anyone who speaks of necessary truths as true by convention is guilty of self-contradiction, unless one or more of the terms he uses has a special sense in this context. Taken according to ordinary usage, his statement means that truths without alternatives nevertheless have alternatives. (Kneale 1947: 118–9)<sup>1</sup>

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<sup>1</sup>This objection can be found in a number of places; e.g. Black (1936: 28–9), Wild and Coblitz (1948: 665), Lazerowitz (1972: 234–5), and Horwich (1998).

I have already suggested that there might be room for a version of the linguistic approach which admits that there are limits to what can appropriately be described as *conventional* in this context. I should say immediately that I do not think that this provides us with a solution to the present objection. Fundamentally, the problem concerns the attempt to explain necessity in terms of something *contingent*, and while the appeal to conventions certainly makes this problem particularly sharp (since, as Kneale points out, conventions are contingent by definition), even non-conventional aspects of language-use would seem to be contingent insofar as the very existence of languages is a contingent matter.

Contemporary discussions tend to approach this ‘contingency problem’ via a more general argument which has come to be known as ‘Blackburn’s Dilemma’, and I shall do the same. I begin, therefore, by articulating Blackburn’s dilemma, its ‘contingency horn’ in particular, and how it applies to the linguistic approach.

### 5.1 Blackburn’s Dilemma

In ‘Morals and Modals’ (1986), Simon Blackburn presents a much discussed dilemma for what he calls the ‘truth-conditional approach’ to the problem of necessity. Suppose we want to explain why some necessary truth  $A$  is necessary, and we offer an explanation of the form ‘ $\Box A$  because  $F$ ’. Now, Blackburn writes:

either  $F$  will claim that something *is* so, or it will claim that something *must* be so. If the latter, there is no problem about the *form* of explanation, for one necessity can well explain another. But, as we have seen, there will be the same bad residual ‘must’ [...] Suppose instead that  $F$  just cites that something *is* so. If whatever it is does not *have to be* so, then there is strong pressure to feel that the original necessity has not been explained or identified, so much as undermined. (1986: 120–1)



In other words: if the explanans is necessarily true, we have only pushed the problem back, whereas if it is contingently true, it is unsuitable as an explanation of something genuinely necessary. In the former case we fail to *explain* necessity; in the latter we fail to explain *necessity*.<sup>2</sup>

As an example of an account that falls victim to the contingency horn, Blackburn considers precisely the idea that necessities are to be explained in terms of linguistic conventions:

Suppose a theorist claims that twice two must be four because of a linguistic convention, [and] suppose it is denied that there is any residual necessity, that we *must* make just those conventions. [Then] there is a principled difficulty about seeing how the kind of fact cited could institute or be responsible for the necessity. This is because the explanation, if good, would undermine the original modal status: if that's all there is to it, then twice two does not have to be four (1986: 121).

While Blackburn's reasoning is intuitively plausible, one crucial notion – that of necessity being “undermined” – is not spelled out in any detail. However, more recent discussions of the dilemma – going back to Hale (2002b)<sup>3</sup> – explicate the reasoning along the following lines:

1.  $\neg F \Box \rightarrow \neg \Box A$  (Premise: consequence of ‘ $\Box A$  because  $F$ ’)
2.  $\Diamond \neg F$  (Premise: contingent explanans)
3.  $\Diamond \neg \Box A$  (From 1 and 2 using counterfactual logic)
4.  $\neg \Box A$  (From 3 in the modal logic S4)

We thus reach the conclusion that  $A$  isn't necessary after all. I will refer to this as ‘Hale's formalisation of the contingency horn’.

There are two standard objections to this argument (Hale 2002b: 302–3), neither of which is wholly satisfactory. First, it is pointed out that (1) is not a straightforward consequence of ‘ $\Box A$  because  $F$ ’ since there are

<sup>2</sup>For a somewhat different interpretation of the dilemma, see Shalkowski (2008).

<sup>3</sup>Cf. e.g. Lange (2008), Hanks (2008), Cameron (2010), and Morato (2014).

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counterexamples such as causal pre-emption cases (cf. D. Lewis 2000: 184). I shall set this response aside without much comment, but I do think it is poorly motivated unless we can offer some positive reason for thinking that such considerations are relevant here – which seems, on the face of it, doubtful.<sup>4</sup>

Next, it is said that assuming the S4 axiom ( $\Box\varphi \rightarrow \Box\Box\varphi$ ) – needed in order to go from (3) to (4) – ‘pretty directly begs the question’ in this context (Hale 2002b: 302). There are some reasons to be sceptical of this however. First, a self-ascribed ‘conventionalist’ such as Alan Sidelle explicitly considers the rejection of S4 to be misguided (2009: 226), and it is difficult to find textual support for the idea that historically prominent advocates of the linguistic approach found this principle problematic.

Second, as Sidelle points out (2009: 227), the supposed rationale for rejecting the S4 axiom is somewhat unstable. The idea, presumably, is that whereas it is necessary that, e.g., triangles have three sides, this need not be necessary relative to a world where the linguistic conventions are different (say, where ‘triangle’ is governed by the rules which in fact govern ‘rectangle’), and so it is not necessarily necessary. The problem, however, is that if this gives us reason to say that it’s not necessary that triangles have three-sides relative to this world, why does it not also give us reason to say that this is *false* relative to this world? Why is it only modal claims that are evaluated relative to *their* linguistic conventions?

Thirdly, I do not think that the S4 axiom is needed to deliver the conclusion that Blackburn was aiming for – namely that a contingent explanans *undermines* the necessity we set out to explain. Even without the S4 axiom, the formalised contingency horn delivers the conclusion:  $\Diamond\Diamond\neg A$ . The negation of  $A$ , that is, is found to be possibly possible, and that is already to take away some of the force which we ordinarily ascribe to (logical/metaphysical) necessity. For we think that what is necessary holds *no matter what*, and not just in “accessible worlds” (whatever that means in this context). Put

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<sup>4</sup>For a more thorough discussion of this, see Hanks (2008: 306).

differently: logical necessity *as ordinarily understood* is S5 necessity. If we cannot have this, then necessity as ordinarily understood is indeed, in some sense, undermined.

That, of course, is not to say that someone cannot bite the bullet here and accept the necessity-undermining contingency horn while rejecting S4. Still, I shall assume that this option is not available to anyone who wants to explain *genuine* necessity. And I shall say that the necessity of a putatively necessary truth  $p$  has been *undermined* if and only if there is any sense at all in which  $p$  could have been false or possibly false. Given this explication, the status of the S4 axiom is irrelevant to the discussion.

## 5.2 The Wright-Sidelle Strategy and Its Limits

Another possible response to the contingency horn is to maintain that (1) should be rejected, but not because of anything like causal pre-emption. In the previous chapter we noted that Boghossian's complaint that the adherent of metaphysical analyticity must deny, e.g., that snow was white or not white before there were languages around is too quick since they might insist that this claim is true because of our current linguistic conventions.

Both Crispin Wright (1985: 189–92) and Alan Sidelle (2009: 229–32) have offered an analogous response to the contingency problem: Even counterfactual scenarios (other possible worlds), they say, are described using our *actual* linguistic conventions.<sup>5</sup> Thus, a counterfactual like ' $p \Box \rightarrow \neg \Box(\text{Triangles have three sides})$ ', will have a false consequent no matter what we substitute for ' $p$ '. It does not matter what the linguistic conventions are like at other worlds; triangles with more or less than three sides are ruled out – across logical space – by our actual linguistic conventions.

This is both true and important, but not quite satisfactory. For if the linguistic approach does not entail the kind of counterfactual *covariance*

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<sup>5</sup>Wright regards this as a higher-order convention (1985: 190).

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we expect from explanations, then it becomes quite unclear what kind of explanation is really on offer. Indeed, Sidelle is well aware of this worry and puts it well:

If our conventions really do explain the modal facts, then changes in our conventions would have to effect what is modally the case, and so, contrapositively, [...] if they don't have such an effect, then they cannot really provide our explanations here. (2009: 232–3)

This problem is particularly pressing given that I have proposed to understand the linguistic approach in terms of a commitment to carrying out a particular explanatory project. The reason is that if it turns out that we must appeal to some *non-standard* explanatory relation here – which cannot be assessed by our ordinary criteria – then it is far from clear that this provides us with a practically workable grasp on this commitment after all.<sup>6</sup>

We can put the situation we are in as follows: The Wright-Sidelle strategy shows us that an adherent of the linguistic approach can allow for a perfectly good sense in which the modal facts remain fixed in counterfactual scenarios – even counterfactual scenarios where the linguistic conventions are different. That, however, still leaves us with the following question: Is there, if the adherent of the linguistic approach is correct, still *some* sense in which the modal facts would have been different had our linguistic conventions been different? If the answer to this question is ‘no’, then we start to lose our grip on what kind of explanation the linguistic approach is meant to offer. But if the answer is ‘yes’, then this threatens to undermine necessity after all.

My strategy will be to put these worries to rest by articulating a sense in which we *do* get covariance, but without undermining necessity. I shall begin by showing that even if we accept that there is a commitment to

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<sup>6</sup>For this reason I don't think Sidelle's own attempt to address this worry (2009: 233–6) can help us here.

covariance, we have reason to think that this cannot be adequately captured in the manner of (1) in Hale's formalisation of the contingency horn.

### 5.3 Doubts about Hale's Formalisation

For our present purposes, we may say that according to the linguistic approach, *all* modal facts<sup>7</sup> are due to the linguistic conventions. Taking covariance for granted, this presumably means that had there been no linguistic conventions, there would have been no modal facts, and in particular, no such fact as, e.g., that necessarily all triangles have three sides. Now, let ' $L$ ' be the claim that there are linguistic conventions and ' $p$ ' the claim that all triangles have three sides.

Hale's formalisation assumes that the commitment to covariance can be put in the form ' $\neg L \Box \rightarrow \neg \Box p$ '. This, however, is equivalent to ' $\neg L \Box \rightarrow \Diamond \neg p$ ', which reads: 'If there had been no linguistic conventions, it would have been possible for triangles to not have three sides'. Clearly something has gone wrong. The residual "possibility fact" is obviously contrary to the idea that *all* modal facts are due to linguistic conventions.

We can, of course, do the same trick the other way around. Let ' $q$ ' be the claim that some human is ten feet tall. According to the linguistic approach, the fact that this is possible is due to certain linguistic conventions. If, however, we take this to yield a covariance commitment like ' $\neg L \Box \rightarrow \neg \Diamond q$ ', then we get ' $\neg L \Box \rightarrow \Box \neg q$ ', i.e. 'If there had been no linguistic conventions, it would have been necessary that no human is ten feet tall'. This residual "necessity fact" is, again, obviously contrary to the central tenet of the linguistic approach.

These formalisations, then, certainly yield undesired consequences, but they are so *immediately* contrary to the ideas they were meant to capture that we should suspect the problem to lie with the formalisations rather than with what was to be formalised. That is to say: even if we are prepared

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<sup>7</sup>That is: all facts about the widest kind of necessity and possibility.

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to accept that the linguistic approach does yield a commitment to some kind of covariance, it is far from clear that Hale's formalisation hits the mark. (1) does not seem to capture a kind of covariance that could plausibly be attributed to the adherent of the linguistic approach.

It is worth noting that the problems are not plausibly attributed to the use of the would-counterfactual. Rather, what is problematic is the assumption that the counterfactual scenario can be described by negating an *ordinary modal claim* expressed using a standard modal operator. It is not difficult to see why this is a problematic assumption in this context: The modal operators are *irreducibly modal*, in the sense that putting a negation sign in front of one just yields another modal claim. Consequently, using these threatens to beg the question: We are supposed to consider the idea that there might have been no such thing as modality, but the notation we use to express this commits us to modal facts no matter what.

Now, it should be pointed out here that one person's question-begging might be another's *reductio*. If what "begs the question" is the interdefinability of the modal operators, then perhaps so much the worse for the linguistic approach. We *do*, after all, think that these are interdefinable. However, nothing said so far suggests that someone advocating the linguistic approach must reject the modal operators as standardly used. What we have seen is that *if* we express the covariance commitments of this approach straightforwardly using these operators, *then* their interdefinability begs the question. But the correct response to this, I will argue, is to deny the antecedent: It is a mistake to think that we can understand the commitment to covariance in this way.

In the next section, therefore, I shall aim to do two things: i) articulate the sense in which we do get covariance according to the linguistic approach; ii) show why it would be a mistake to formalise this in the way that Hale does. My main line of thought can be summarised as follows: We need to make a distinction between two kinds of covariance, which may be labelled 'internal' and 'external' covariance. (1) in Hale's formalisation

expresses internal covariance, but the adherent of the linguistic approach is only committed to the external variant. In section 5.5, I shall then go on to argue that external covariance does not undermine necessity.

## 5.4 Making Sense of Covariance

I shall proceed by introducing the distinction between internal and external covariance (as well as some related terminology) via an example taken from aesthetics, before applying this framework to the modal case.

### An Analogy from Aesthetics

Consider someone, Emmy, who holds that there are aesthetic facts, but thinks that these are explained, in the end, by facts about human preferences. In keeping with standard assumptions about explanation and covariance, she therefore accepts:

(A) Had there been no humans, there would have been no aesthetic facts.

Now suppose that she comes across a lake filled with rubbish in an otherwise beautiful landscape and proclaims:

(B) Had there been no humans (and hence no rubbish), this lake would have been beautiful.

Is Emmy guilty of contradiction? I would say no, or at least not necessarily.

The reason is that these counterfactuals are ambiguous. In each case we could either evaluate the entire counterfactual in accordance with the aesthetic facts that the actual human preferences (supposedly) give rise to, or we could first assume that the antecedent is true, ask how that would affect the human preferences and what aesthetic facts they would give rise to, and see whether this would make the consequent true. Thus, focusing on (B), we could either imagine how the lake would look in a human-free world,

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and ask whether it would be beautiful according to the actual, preference-dependent, aesthetic facts, or we could consider what the human preferences would be like if there were no humans and ask whether they would sustain the fact that the lake is beautiful (presumably not).

I shall call counterfactuals read along the former lines *internal counterfactuals*, and counterfactuals read along the latter lines *external counterfactuals* (the rationale behind the terminology will become clear shortly). A reasonable interpretation of Emmy would be that (A) should be understood as an external counterfactual, and (B) as an internal one. It is (A), moreover, which expresses her covariance commitment, and so we may say that her meta-aesthetic views commit her to *external covariance*.

Before applying these ideas to the modal case, we should note a crucial point about negation. Because Emmy accepts the covariance commitments that come with her meta-aesthetic beliefs, she is happy to assert:

(C) Had there been no humans, the lake would not have been beautiful.

In accordance with what we have just said, this counterfactual should be read externally. However, this isn't quite enough to ensure that it gets interpreted correctly. The reason is that the denial in the consequent is also ambiguous. It is natural, after all, to read 'the lake is not beautiful' as expressing an aesthetic judgement. This, however, would be to misrepresent Emmy. She is not saying that if there had been no humans, the correct aesthetic verdict on the lake would be 'not beautiful', she is saying that the lake wouldn't be beautiful since there would have been no correct aesthetic verdict at all. In keeping with the above terminology, we may say that Emmy intends the negation to be read *externally*.

Borrowing a standard trick from the analysis of fictional discourse, we can make all the distinctions we need here using a single operator and scope distinctions. By introducing an operator 'Æ' (read along the lines of 'according to the aesthetic facts') we can distinguish the following three readings of (C):



- (C<sub>1</sub>)  $\mathcal{A}(\text{No humans } \Box \rightarrow \neg \text{beautiful lake})$   
 (C<sub>2</sub>)  $\text{No humans } \Box \rightarrow \mathcal{A}(\neg \text{beautiful lake})$   
 (C<sub>3</sub>)  $\text{No humans } \Box \rightarrow \neg \mathcal{A}(\text{beautiful lake})$

Here, (C<sub>1</sub>) is an internal counterfactual, whereas (C<sub>2</sub>) and (C<sub>3</sub>) are both external ones. The difference between the latter two, moreover, is that (C<sub>2</sub>) contains an internal negation, whereas (C<sub>3</sub>) contains an external one.<sup>8</sup> The appropriate interpretation of (C) is (C<sub>3</sub>), which can be read along the lines of: Had there been no humans, then it would not have been the case that, according to the aesthetic facts, the lake is beautiful. Note, in particular, the difference between the consequents in (C<sub>2</sub>) and (C<sub>3</sub>): The former says that it is an aesthetic fact that the lake is not beautiful; the latter says that it is not an aesthetic fact that the lake is beautiful.

## The Modal Case

I maintain that, in the context of the linguistic approach, analogous ideas apply to the modal case.<sup>9</sup> Consider a counterfactual like:

- (D) Had there been no linguistic conventions, it wouldn't have been necessary that all triangles have three sides.

Read internally, we are to evaluate the entire counterfactual in accordance with the modal facts which our actual linguistic conventions give rise to; read externally we are to first consider what the modal facts would have been like had the antecedent been true, and then ask whether this would render the consequent true. Moreover, we can distinguish between an internal and an external reading of the negation in the consequent: It could be read as attributing the modal status *not necessary* to the proposition in question, or it could be read as *withholding* the modal status *necessary*.

<sup>8</sup>Hence the terminology: the counterfactual in (C<sub>1</sub>) is internal since it is within the scope of the operator – similarly with the negation in (C<sub>2</sub>).

<sup>9</sup>Although there is one glaring disanalogy: Counterfactuals are themselves modal. I shall return to this issue in section 5.6 below.

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Again, we can make all the needed distinctions by introducing an operator, ‘ $\mathcal{M}$ ’, read along the lines of ‘according to the modal facts’:

(D<sub>1</sub>)  $\mathcal{M}(\text{No conventions } \Box \rightarrow \neg \Box \text{triangles have three sides})$

(D<sub>2</sub>)  $\text{No conventions } \Box \rightarrow \mathcal{M}(\neg \Box \text{triangles have three sides})$

(D<sub>3</sub>)  $\text{No conventions } \Box \rightarrow \neg \mathcal{M}(\Box \text{triangles have three sides})$

(D<sub>1</sub>) says that the entire counterfactual holds relative to the modal facts which our actual linguistic conventions give rise to. (D<sub>2</sub>) says that the nearest world with no linguistic conventions would be such as to give rise to the modal fact that triangles do not necessarily have three sides, and (D<sub>3</sub>) says that this world would be such as to *not* give rise to the modal fact that triangles necessarily have three sides.

Now, it should be reasonably clear, once these distinctions are drawn, that the adherent of the linguistic approach is committed, not to internal, but to external covariance, and specifically to covariance claims which share the form of (D<sub>3</sub>).

This issue can helpfully be approached via the framework of possible worlds (reading ‘ $\mathcal{M}$ ’ as ‘according to the framework of possible worlds’). Note, to begin with, that an adherent of the linguistic approach cannot be expected to take the notion of a possible world as an explanatory primitive. That would make the linguistic approach redundant. Instead, they must be understood as attempting to explain what a possible world *is* – attempting to explain, that is, what makes the possible worlds *possible*.<sup>10</sup> The answer they give is, roughly, that a possible world is a scenario that can be described without infringing on our linguistic conventions.

This allows us to see both why (D<sub>1</sub>) does *not* capture an appropriate covariance commitment, and why an adherent of the linguistic approach is perfectly within their rights to say that (D<sub>1</sub>) is false. Starting with the latter point, (D<sub>1</sub>) tells us to evaluate the entire counterfactual in accordance

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<sup>10</sup>This, I take it, is Crispin Wright’s point when he says that ‘what the conventionalist *ought* to be advancing is a thesis about the meanings of the modal operators in the first instance’ (1985: 173).

with the framework of possible worlds that our actual linguistic conventions give rise to, and it says that if we do, then we will see that if we go to the nearest world in which there are no linguistic conventions, we shall find that there is a world in which it is false that triangles have three sides.

However, if the criterion for being a possible world is that it can be described without infringing on our actual linguistic conventions, then there won't be any worlds containing such triangles *anywhere* in logical space (assuming that our linguistic conventions indeed rule out such things.) This is really just an application of the Wright-Sidelle strategy, albeit in a more radical form: The point isn't just that we *describe* other possible worlds using our actual linguistic conventions, but that our actual conventions determine what possible worlds there are in the first place.

As to the first point, since the linguistic approach is an attempt to explain why the framework of possible worlds looks the way it does, the appropriate covariance commitments are not captured by claims about how the worlds are related *within* the framework which our actual conventions have given us. This is also what an adherent of the linguistic approach should say about Hale's formalisation: (1) is just a standard modal-counterfactual claim – a mere description of how things are within the domain of possible worlds generated by our actual linguistic conventions. Consequently, (1), like (D<sub>1</sub>), fails to capture an appropriate covariance commitment.

(D<sub>3</sub>), on the other hand, is exactly the kind of claim we are looking for. We are trying to explain why it is that  $\mathcal{M}(\Box\text{triangles have three sides})$ , and this means that, given an explanans  $E$ , the covariance commitment will be of the form ' $\neg E \Box \rightarrow \neg\mathcal{M}(\Box\text{triangles have three sides})$ ' – which says that had  $E$  not been the case, then it would not have been the case that according to the framework of possible worlds, triangles necessarily have three sides, or, alternatively: Had  $E$  not been the case, it would not have been a modal fact that, necessarily, triangles have three sides.

## 5.5 External Covariance and Genuine Necessity

### Avoiding the Contingency Horn

So far, I have argued that although the linguistic approach does yield covariance commitments, these should not be expressed, as in Hale’s formalisation, using internal counterfactuals, but using external ones. Crucially, this allows us to make sense of the linguistic approach as an attempt at *explanation* in a quite ordinary sense, with the usual commitments which come with that. It remains, however, to show that this provides us with a response to Blackburn’s dilemma. We do this by showing that a commitment to external covariance does not yield the conclusion that necessity is undermined.

When (1) in Hale’s formalisation is replaced by the appropriate external counterfactual, the reasoning of the contingency horn goes as follows:

1.  $\neg F \Box \rightarrow \neg \mathcal{M}(\Box A)$  (Premise: consequence of ‘ $\mathcal{M}(\Box A)$  because  $F$ ’)
2.  $\Diamond \neg F$  (Premise: contingent explanans)
3.  $\Diamond \neg \mathcal{M}(\Box A)$  (From 1 and 2 using counterfactual logic)

Now, we said above that the necessity of a putatively necessary truth is undermined if and only if there is some sense in which its negation is or could have been possible. Crucially, (3) commits us to no such thing. (3) concedes that it could have failed to be the case that: According to the framework of possible worlds, necessarily  $A$ . It is conceded, that is, that the modal fact  $\Box A$  might not have obtained. However, we cannot infer from this that the *contrary* modal fact  $\neg \Box A / \Diamond \neg A$  could have obtained. There is no inference, that is, from ‘ $\Diamond \neg \mathcal{M}(\Box A)$ ’ to something like ‘ $\Diamond \mathcal{M}(\neg \Box A)$ ’.<sup>11</sup>

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<sup>11</sup>This marks a difference between the present proposal and that of Iris Einheuser (2006: 476–8). Einheuser distinguishes two notions of possibility:  $\Diamond_c \varphi$  (which is true at  $w$  iff it is true at some world which differs from  $w$  only in virtue of its ‘carving’), and  $\Diamond_s \varphi$  (which is true at  $w$  iff it is true at some world which differs from  $w$  only in virtue

We have already seen one reason why this inference must be rejected: ‘ $\diamond\neg\mathcal{M}(\Box A)$ ’ might be true because the counterfactual scenario sustains *no* domain of possible worlds, in which case there would be no potential for contrary modal facts either. If the linguistic approach is correct, this is presumably the case in possible worlds where there are no linguistic conventions. Furthermore, ‘ $\diamond\neg\mathcal{M}(\Box A)$ ’ might be true because, even though the counterfactual scenario does sustain an alternative domain of possible worlds, these worlds are silent on whether ‘*A*’ is true or not, and so deliver no verdict regarding the modal status of ‘*A*’. This is plausibly the case if the relevant linguistic conventions don’t suffice to express the proposition expressed by ‘*A*’.

The reasoning of the contingency horn, then, does not yield the conclusion that necessity has been undermined when applied to an external counterfactual. We reach the conclusion that the modal fact in question might have been lacking, but since we cannot “push the negation through”, we cannot infer that there might have been a contrary modal fact.

It might be objected that this is based on too narrow an understanding of what it is for necessity to be undermined. For why shouldn’t we say that the necessity of a putatively necessary truth is already undermined by the concession that there are possible circumstances in which there would have been no such modal fact? Of course, this is not really about that particular word ‘undermined’; rather, it has to do with why we find the conclusion of Blackburn’s contingency horn problematic. The reason, I would say, is that we want ascriptions of necessity to retain their *force*. We want to hold on to the idea that if something is genuinely necessary, then there is *absolutely no sense* in which it could have been false, or even possibly false, and that this holds *unconditionally*. No matter how strange we imagine things to be, it remains the case that what is necessary is still true.

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of its ‘substratum’), and argues that the ‘conventionalist’ is only committed to claims like: ‘ $\diamond_c\neg\Box_s(\text{Bachelors are unmarried})$ ’. However, if the modal operators remain interdefinable (and we are not told otherwise), this gives us: ‘ $\diamond_c\Box_s\neg(\text{Bachelors are unmarried})$ ’. Although I am sympathetic to much of what Einheuser says, it is not clear that this avoids the charge that the necessity in question has, in some sense, been undermined.

My reason, then, for holding that necessity is not undermined by the above concession is simply that this idea can be retained. While it is conceded that there is *a* sense in which it is correct that had the linguistic conventions been different or lacking, it wouldn't have been necessary, say, that all bachelors are unmarried, this *in no way* suggests that there could have been married bachelors or that such people might have been possible. What is conceded is that the framework which delivers this modal verdict, might not have been able to deliver one, or, indeed, not existed at all.

### Ruling Out Contrary Modal Facts

The modified contingency horn, then, does not *entail* that contrary modal facts are possible, and so does not entail that necessity is undermined. This, however, is not all that reassuring. After all, this does not guarantee that no alternative set of linguistic conventions *would* give rise to contrary modal facts. Why shouldn't it be the case, if the linguistic approach is correct, that had or linguistic conventions been different enough, they would have given rise to, say, the modal fact that four-sided triangles are possible?

The question here is whether, for some putatively necessary truth '*A*', we might find ourselves committed to a counterfactual of the form ' $F \Box \rightarrow \mathcal{M}(\Diamond \neg A)$ ', where '*F*' is possible. This would deliver the conclusion ' $\Diamond \mathcal{M}(\Diamond \neg A)$ ', which would count as undermining the necessity of '*A*'.

However, we already have the resources needed to address this worry. Strawson, recall, argued that if we can explain why a sentence is necessary in terms of the conventions of English, then there is no mystery regarding how we know that any sentence (in any language) expressing the same proposition is also necessary. The reason, essentially, is that the linguistic considerations we appeal to in order to explain the necessity of the English sentence *also* provide the criteria for whether a sentence in a different language expresses the same proposition. So, if we find some sentence which is not susceptible to a similar explanation, that shows that we are not dealing with a sentence which expresses the same proposition.

The same reasoning can be applied to other *possible* languages, and this largely removes the present worry. Even though it is certainly possible that some sentence which we take to be necessary could have been governed by linguistic conventions which would render it both contingent and false, we would also have to be able to say that it still expresses the same proposition. It is quite unclear, however, why we would ever say this if the relevant linguistic conventions are different, and even differ so as to alter the modal status and truth-value of the sentence. As Richard Creath put it when discussing Carnap:

Nor is one free to affirm in one system what one denies in another. After all, the meanings are not fixed until after the linguistic conventions are established. If different rules are laid down, then there is not common claim with a single meaning that is variously asserted and denied. (1990a: 10–1)<sup>12</sup>

Thus, not only can we block the *inference* (via the contingency horn) to the possibility of contrary modal facts, there is also a strategy available for ruling out such things.

## 5.6 A Glaring Disanalogy

We should now consider the glaring disanalogy between the modal case and the aesthetic case which I used to motivate the internal-external distinction, namely that counterfactuals are themselves modal, and so are themselves typically understood in terms of a framework of possible worlds.<sup>13</sup> Consequently, it may seem that while there can be genuinely external counterfactuals with regard to aesthetics, the notion of a *modally* external counterfactual is problematic. For if we try to “step outside” the modal framework,

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<sup>12</sup>Sidelle also makes this point (2009: 230).

<sup>13</sup>There are some dissenters from this view. Anscombe, for instance, suggests that ‘subjunctive counterfactuals’ are truth-functional in her (1975), although she does have some doubts about this proposal (1981b: 196–7). Note that although Goodman’s somewhat influential proposal (1947) does not invoke possible worlds, it is still modal since it invokes the notion of entailment.

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then we shall be left without a good way of making sense of the counterfactuals.

Put differently, the point is that a modally external counterfactual, when properly spelt out, is not of the form ' $p \Box \rightarrow \mathcal{M}(\Box q)$ ', but, rather:

$$(E) \mathcal{M}(p \Box \rightarrow \mathcal{M}(\Box q))$$

However, while this *does* constitute a disanalogy with the aesthetic case, it is not clear that it is a problem. Certainly there is no *general* problem with operators being nested in this way, and, more importantly for our purposes, the nesting in (E) does not create any problems when it comes to evaluating this sentence. Indeed, looking back on the discussion in 5.4, we may say that we were working with tacit  $\mathcal{M}$ -operators up front all along.

With a bit of artificial rigour, we can be slightly more specific about how to evaluate (E). The linguistic approach provides us with a theory  $T$ , about how the linguistic conventions give rise to modal facts, which we can view as a function from sets of linguistic conventions to domains of possible worlds. (E) is then evaluated by first going to the nearest world in which  $p$  is true, asking what conventions are present there and what domain of possible worlds these would give us in accordance with  $T$ . We then ask whether this domain is such that necessarily  $q$ . The process here does indeed involve considering two domains of possible worlds, as is reflected in the double use of the  $\mathcal{M}$ -operator, but this causes no obvious *problems*.

It might be said that there are bound to be complications since, given that counterfactuals are modal, the linguistic conventions would have to settle, not just what is possible and necessary, but also which counterfactuals are true. This, however, is a mistaken demand. The linguistic conventions should not be expected to explain every fact which contains a modal component. For instance, they should not be expected to settle whether something is *physically* possible or not. Similarly, they should not be expected to settle the “modal distances” that are crucial to the evaluation of counterfactual claims. Consequently, an adherent of the linguistic approach can (and, I



think, should) reject the idea that the linguistic conventions must explain the truth-values of counterfactual claims in general.

Still, if our linguistic conventions determine facts about necessity and possibility, then this will presumably *affect* claims which have modal aspects – such as claims about physical possibility and counterfactuals. This, I think, must be conceded, albeit with a qualification: There is no reason to suspect that this will affect anything other than *external* claims about counterfactuals and physical possibility. And since such claims are really only of concern to philosophers, there is not much potential for any serious revisionism here.

Let us consider this in a bit more detail. If, as I have suggested, the adherent of the linguistic approach should grant that, in the external sense, there would have been no modal facts if there were no languages, then this would presumably include facts about physical modality and counterfactuals. The reason is that these notions, at least as they are typically understood, involve a background appeal to the broader kind of modality that the linguistic conventions are said to explain.

A standard suggestion, for instance, is that an event is physically possible iff its occurring is consistent with the laws of physics. But the notion of *consistency* is one that the linguistic approach is obligated to explain, and this might well lead us to conclude that there is a sense in which there would not be facts about consistency in the absence of language, and *that* might commit us to saying that there are no facts about physical possibility in the absence of language either.

Now, it *certainly* sounds odd to say, e.g., that if there had been no linguistic conventions, then it wouldn't have been physically possible for birds to fly. However, this at least becomes *less* odd if we remind ourselves of the proper *external* interpretation of this claim. We are not, of course, saying that it would be physically impossible for birds to fly in this case; we are saying that the framework we use to make sense of physical possibility and impossibility alike would be lacking. And this, I think, is a bullet the

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adherent of the linguistic approach might be able to bite.

Reverting to the disanalogy that prompted this discussion, I don't claim to have shown that this disanalogy definitely causes no problems, but I do hope to have shifted the burden of proof: If there is a problem here, it needs to be argued for. More generally, the interaction between logical modality and other modal phenomena is, I think, an area in which further research is both needed and likely to shed light on the tenability or otherwise of the linguistic approach.

## The Character of Linguistic Rules

In the previous two chapters we have responded to two influential objections against the linguistic approach. In doing so, we have also clarified two aspects of the explanations that adherents of the linguistic approach are committed to providing. With regard to what is to be explained (the “explanandum” in somewhat antiquated terminology), we have seen that this is the *necessity* of necessary truths, not their truth; and with regard to the explanatory relation itself, we have seen that this can be understood in the standard way, with the commitments to covariance which that entails.

In this chapter, I shall consider two objections that target that which is offered as an explanation (the “explanans”) – namely the conventionally adopted linguistic rules.<sup>1</sup> These objections are, first, that our linguistic practices are simply too messy to provide us with anything like the definite rules we would need for the purposes of such explanations, and, second, that what these rules are is plainly an empirical matter and that this clashes with the central tenet of the subjective-constitutive strategy. I shall begin by expanding on why an adherent of the linguistic approach is committed to approaching matters via actual linguistic practices in the first place.

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<sup>1</sup>Leaving aside, for now, the idea that an appeal to more general features of linguistic representation might also be needed.

## 6.1 Rejecting the Myth of a Museum

In his Dewey Lectures, Quine provided a now famous characterisation of a certain simplistic view of meaning: ‘Uncritical semantics is the myth of a museum in which the exhibits are meanings and the words are labels. To switch languages is to change the labels.’ (1968: 186) On this kind of picture, we invest a word with meaning by simply *correlating* it with a meaning, and the correct rules for using the word are then dictated by the properties of that meaning. This same idea was criticised by Wittgenstein under the heading of ‘meaning bodies’:

We are tempted to think we can deduce the rules for the use of a word from its meaning, which we supposedly grasp as a whole when we pronounce the word. This is the error I would eradicate. [...] To say that the use of a word, e.g., ‘cube’, follows from its meaning is to treat the word as if it were the visible face of a hidden body, its meaning, whose rules of combination with other hidden bodies are given by the laws of geometry. (1979b: 50–1)

It is sometimes suggested that Wittgenstein’s target in passages like this is the linguistic approach as understood by members of the Vienna Circle (Baker and Hacker 2009: 367). But although it is possible to find occasional passages in the positivists’ writings which suggest some such picture, the idea that they subscribed to the ‘museum myth’ of ‘meaning bodies’ does not stand up to scrutiny (Glock 2003a: 156). To take but one example, Carnap opposes precisely this idea in the preface to *LSL* (1937: xv).

Moreover, this picture is antithetical to the linguistic approach as it was presented in Part A. I there argued that the central goal is to dissolve ‘the problem of the seemingly mysterious parallelism between the course of our thought and that of the world, the pre-established harmony between thought and world, which would enable us to discover something about the world by thought’ (Hahn 1930a: 24), and that the strategy is to do so, in Kantian fashion, by abolishing the idea of an external standard of cor-

rectness when it comes to necessary (logical) truths. The logical positivists therefore maintained that ‘there [is] nothing in the world corresponding to the so-called logical constants (like “and”, “or”, etc.)’ (Hahn 1930a: 25), and more generally that ‘logic [...] does not deal with any objects at all; it only deals with the way we talk about objects’ (Hahn 1933a: 29).

The Museum Myth – being essentially a version of the “conceptual Platonism” we discussed in section 3.4 – is in clear tension with this project. For here the meanings would indeed constitute an external standard of correctness and we should have to ask how we get to be justified in thinking that the rules which we *believe* follow from these meanings actually do so. More generally, when it is said that necessity is to be explained in terms of the linguistic rules, we cannot go on to say that the “proper” rules are dictated by something *behind* our linguistic practices – some subject-independent standard which these practices may fall short of. That would land us right back with the problems the linguistic approach was meant to dissolve.

The worry, however, is that it is not clear, on reflection, that abandoning the museum myth in favour of focusing on our actual linguistic practices squares any better with the aspirations of the linguistic approach. First, it may be said that the linguistic approach presupposes the *neatness* of the Museum Myth since our linguistic practices do not provide us with appropriately determinate facts about what the linguistic rules are, and so leaves us without a proper basis for the explanations that we want to provide. Second, it might be said that it is an empirical question what the linguistic rules are, and so that we have hardly succeeded in avoiding external standards of correctness here. In section 6.2 I consider the former of these worries (via an objection due to Marcus Giaquinto);<sup>2</sup> in sections 6.3–6.5 I consider the latter.

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<sup>2</sup>Substantial parts of this discussion have appeared as Nyseth (2017).

## 6.2 The Threat of Indeterminacy

In ‘The Linguistic View of *a Priori* Knowledge’ (2008), Giaquinto argues that *a priori* knowledge cannot be explained in terms of knowledge of synonymy because, although meanings are not as indeterminate as Quine thought (see Quine 1960b: ch. 2), ‘there is enough indeterminacy of lexical meaning for ordinary statements of synonymy to fall short of fact-hood’ (2008: 91).

I shall not contest Giaquinto’s thesis of ‘moderate indeterminacy of meaning’, but I shall contest the conclusion he draws regarding the ‘linguistic view of a priori knowledge’. As I see it, the moderate indeterminacy thesis does not pose a problem for this view. Indeed, once the dialectical situation is made explicit, we can see that, if anything, Giaquinto’s considerations speak in favour of it.

The details of Giaquinto’s argument are in two respects slightly tangential to our concerns. Firstly, this argument targets the appeal to synonymy specifically, whereas we are interested in linguistic rules more generally. Secondly, the position he is attacking is understood as a view about the *a priori*, whereas we are understanding the linguistic approach primarily in terms of the problem of necessity.<sup>3</sup> However, the general lesson I’m going to draw is independent of these two points.

### Synonymy and *a Priori* Knowledge

According to the linguistic view, one route to *a priori* knowledge is to deduce it from synonymies and logical truths that are independently known *a priori*. Here is one example Giaquinto considers (I will refer to this general scheme as ‘the synonymy model’):<sup>4</sup>

- (1) All cows are cows (logical knowledge)

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<sup>3</sup>For this reason, I shall refer to the view Giaquinto is attacking as ‘the linguistic view’ rather than ‘the linguistic approach’.

<sup>4</sup>Giaquinto’s model is a simplification of one due to Boghossian (cf. his 2003b). For an objection to Boghossian’s line of thought, see Nyseth (Forthcoming).

- (2) ‘Cow’ means the same as ‘female bovine’ (semantic knowledge)  
(∴) So, all cows are female bovines (new *a priori* knowledge)

As Giaquinto is quick to point out (2008: 90), there are a number of potential issues with this idea: First, he says, we may doubt whether the logical truth is available *a priori*.<sup>5</sup> Second, we may doubt whether the relevant semantic knowledge is available *a priori*. And third, we might worry that claims of the form ‘X means the same as Y’ are typically not strictly true because there is no fact of the matter.<sup>6</sup>

Giaquinto discusses the first of these worries in the second part of his paper, and is willing to disregard the second for the sake of argument. His objection in the first part of the paper concerns the third worry.<sup>7</sup>

### Giaquinto’s Objection

In order for synonymy-based explanations of *a priori* knowledge to work, there must be semantic facts underlying claims such as (2). Giaquinto claims, however, that if we reflect upon certain questions about how to individuate meanings, we shall have to conclude that this requirement is typically not met, and that it is simply indeterminate whether two words are synonymous.

To substantiate this, Giaquinto considers the question: ‘Does “cow” (in English) mean the same as “vache” (in French)?’ Well, we first observe that ‘cow’ is used in certain ways that differ from how ‘vache’ is used. For instance, ‘cow’ is applied to female elephants and whales, whereas ‘vache’ is not (2008: 92). This by itself does not establish that the synonymy doesn’t hold, for we might well say that ‘cow’ is used in several (somewhat similar)

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<sup>5</sup>Indeed, if the linguistic view is to be an explanation of the *a priori* in general, it must be shown, not just that this is *a priori*, but that the *a priori* status of logic can be explained in a way that is consistent with the central tenets of this view.

<sup>6</sup>A fourth worry concerns the inference itself: Is it valid? Can this be known *a priori*? And if so, can this knowledge be explained by the linguistic view?

<sup>7</sup>We shall consider essentially the second worry in sections 6.3–6.5 below, and we shall return to considerations related to the first in chapter 9.

senses in English, but that one of these is synonymous with the French word ‘vache’. What this response makes clear, however, is that:

The question whether the noun ‘cow’ has a meaning with respect to which it is synonymous with the noun ‘vache’ depends on a further question: Does the noun ‘cow’ have *one* meaning covering several kinds (female bovines, female elephants, female whales etc.), or *two or more* related meanings with disjoint extensions, one of which covers just female bovines? (2008: 92)

Giaquinto considers several ways of tackling this question, but argues that neither lexicography (2008: 92–3) nor cognitive semantics (2008: 93–6) provides any reason to think that there is a determinate answer here.

Though the problem is initially presented in terms of synonymy across languages, this is not essential. In particular, the same worries arise if we replace the French ‘vache’ with the English ‘female bovine’, and if so, premise (2) above is in trouble. Of course, if this phenomenon were peculiar to the word ‘cow’ and perhaps a few others, then it wouldn’t be all that significant, but Giaquinto argues that the issue will affect most other – but perhaps not all – cases of (purported) synonymy, including that favourite example: ‘bachelor’/‘unmarried man’ (2008: 97–8).

### **Is Moderate Indeterminacy a Problem?**

I shall not contest Giaquinto’s reasons for thinking that moderate indeterminacy of meaning is a widespread phenomenon. My point concerns, rather, what we should conclude if Giaquinto’s claims about this are correct. He thinks it shows that there is something wrong with the linguistic view of *a priori* knowledge:

The general point is this. For the linguistic story to work at all, words must have determinate meanings and speakers must know just what those meanings are. In many examples this double requirement is not satisfied. (2008: 96)



But at this point we should ask: ‘Examples of what, exactly?’ Surely, if this is to be an effective objection against the linguistic view – which is, after all, a claim about how to explain the *a priori* – we must be dealing with examples of *a priori* knowledge. The problem is that insofar as Giaquinto is right about the cases he considers, these aren’t examples of *a priori* knowledge after all.

Take the claim that all cows are female bovines. That might indeed seem like a candidate for *a priori* knowledge. But if Giaquinto is right, and it is indeterminate whether ‘cow’, can be correctly applied to female elephants, then it is equally indeterminate whether all cows *are* female bovines. So, since we can only know what is true, this cannot be an instance of *a priori* knowledge after all. Similarly for the other cases Giaquinto considers: If the meanings are indeterminate in the way he suggests, the appropriate lesson appears to be, not that these are counterexamples to the linguistic view, but that these aren’t candidates for *a priori* knowledge in the first place.<sup>8</sup>

Indeed, we can tweak the example so as to make it clear that it is specifically *a priori* knowledge, and not knowledge in general, that is undermined by moderate indeterminacy. Suppose, for example, that all species except humans and bovines become extinct, then it would be true and knowable that all cows are female bovines – even if the meaning of ‘cow’ is indeterminate in the way Giaquinto suggests. However, this would still not be knowable *a priori* since, presumably, we would have to ascertain empirically that there are no female whales, elephants etc. What the lack of a semantic fact precludes is not knowledge, but *a priori* knowledge specifically.

We thus reach a conclusion quite opposed to that drawn by Giaquinto, namely that we can know *a priori* that all cows are female bovines if and only if there is a relevant semantic fact of the matter. Although this offers only (very limited) inductive evidence for a correlation between semantic facts and the possibility of *a priori* knowledge, and although a mere *correlation* does not suffice to establish the *explanatory* relation claimed by

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<sup>8</sup>It should be noted that Giaquinto is not arguing against the very possibility of *a priori* knowledge (2008: 111).

advocates of the linguistic view, I maintain that, nevertheless, evidence in favour of a correlation here is surely a point in favour of the linguistic view, rather than a point against it.

It is noteworthy, therefore, that other things Giaquinto says further suggest a correlation here. Since he is advocating a *moderate* form of indeterminacy, he is conceding that there are some semantic facts. For instance, he tells us that it is a semantic fact that ‘cow’ cannot be correctly applied to female spiders. Now, if this is correct (and making the concessions that Giaquinto makes about other problematic aspects concerning the linguistic view), then it would seem that we can know *a priori* that no cow is a female spider.

Moreover, Giaquinto does in fact concede that there are *some* synonymy facts - notably cases where a term is introduced as abbreviating another expression and perhaps a few others (2008: 100–1). In these cases, then, the synonymy model is presumably not threatened by moderate indeterminacy of meaning.

The lesson Giaquinto draws from this is that ‘*a priori* knowledge via knowledge of synonymies is at best very restricted’ (2008: 101). However, this assessment is misleading. It makes it sound as if the synonymy model is at fault for being less explanatory than it ought to be. But, as we have seen, establishing this would require a case of *a priori* knowledge which the synonymy model ought to account for but doesn’t, and no such case is provided. Thus, while Giaquinto might indeed have established that ‘*a priori* knowledge via knowledge of synonymies is at best very restricted’, he has not given us reason to think that it isn’t exactly as restricted as it ought to be.

### Conclusion Regarding Indeterminacy

This discussion was motivated by the objection that an adherent of the linguistic approach cannot think of the linguistic rules that are supposed to explain necessity in terms of our actual practices, since these practices

are too messy to provide us with rules that are determinate enough for the needs of the linguistic approach. This is, I suspect, one of the reasons why it is sometimes taken for granted that an adherent of the linguistic approach must buy into some version of the Museum Myth.

Generalising from the above, we can now say that an adherent of the linguistic approach can very well accept that our practices are messy and indeterminate, as long as they are not messy and indeterminate *relative* to the necessary truths. That is to say: there is no problem here unless we find some indisputably necessary truth where the relevant linguistic rules are too messy to sustain any plausible explanation.

It is noteworthy, in this connection, that reasonably clear cases of necessity tend to be found precisely in areas where linguistic *rigour* is emphasised – areas, that is, where we are required to be as explicit as possible about our definitions. This, moreover, would hardly be news to someone like Carnap, who emphasised that the notion of analyticity is typically not straightforwardly applicable to “pre-scientific” parts of natural languages (1966: 260; cf. also 1955: 40–1 and Richardson 1997: 152).<sup>9</sup> In cases where we *do* have linguistic rigour, on the other hand, even Quine grants that *maybe* the linguistic approach does have something going for it (1951: 26; 1960a: 358; 361), although he still has some misgivings (1960a: 361–2).

The frequently messy and indeterminate character of our linguistic practices, then, does not automatically threaten the linguistic approach. Still, it might be asked how this approach fares if indeterminacy is a *completely general* phenomenon. Even this, however, would not be a straightforward refutation. Someone advocating general indeterminacy could, in principle, maintain that a necessary truth *would* be a truth guaranteed by the linguistic rules, although, as a matter of fact, there are no such truths.<sup>10</sup> Of course, there would be little reason to care about the linguistic approach

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<sup>9</sup>Carnap in fact articulates a potential counterexample to ‘no bachelor has a wife’ (1966: 261) which is quite reminiscent of counterexamples articulated by post-Quineans in the business of attacking the notion of analyticity (cf. e.g. Harman 1967: 139; 1994: 45; 1996: 398; and Giaquinto 2008: 97–8).

<sup>10</sup>A case can be made, I think, that this is Quine’s attitude.

if there are, in fact, no necessary truths. However, for the purposes of this thesis, I am assuming the existence of the phenomenon and asking whether the linguistic approach might provide an adequate explanation of it.

Note, finally, that these considerations are relevant in somewhat different contexts as well. First, Putnam has argued (1962a; 1962b; 1975; 1976) that although Quine was wrong to deny the existence of analytic truths, he was quite right to deny the significance of this phenomenon. Strict analyticity, as he sees it, is only possible when we have ‘one criterion words’, and these are rare and insignificant (1962b: 659). Even if this is so, however, the appropriate lesson need not be that the linguistic approach is misguided; it might just be that necessities are equally rare.<sup>11</sup>

Second, it might be suggested that the linguistic approach presupposes an implausibly *internalist* account of meaning. Putnam (1975), Kripke (1972) and Burge (1979; 1986) have all famously argued that matters of meaning frequently depend on features of the external environment (e.g. facts about chemistry, histories of usage and expert judgements), and if so, then there might well be facts about our linguistic rules which are not available to us in anything like the way adherents of the linguistic approach suggest.

Here too, however, the important question is not whether externalist considerations are important, but whether they are important in contexts where modal knowledge is possible. Although there are influential arguments – based on alleged cases of *a posteriori* necessity – which suggest that this is indeed the case, it is not universally accepted. This is not an issue that will be discussed here, but – as already mentioned – one strategy would be to argue that although we need empirical investigation to determine that a claim like ‘water is H<sub>2</sub>O’ is *true*, the principles that allow us to infer from this that it is *necessary* can be explained in accordance with the linguistic approach – drawing on aspects of our linguistic practices that do not rely on externalist considerations.<sup>12</sup>

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<sup>11</sup>Indeed, this appears to be how Putnam sees it (1962b: 660).

<sup>12</sup>In addition to Sidelle’s proposal (1989), two-dimensional semantics provides us with

Thus, while both lexical indeterminacy and semantic externalism certainly put pressure on the linguistic approach, the very existence of these phenomena does not suffice to undermine it.

### 6.3 The Retreat to Idiolects

I now turn to the second problem with conceiving of linguistic rules in terms of our actual linguistic practices – namely that it is plainly an empirical question what these practices are:

If an analytic proposition “calls attention to linguistic usages,” it states an alleged matter of fact about the majority of writers and speakers of the language which the speaker is using. It can be tested by reading the works of a representative selection of persons who use language, and seeing whether they do use words and phrases in the manner alleged. The analytic proposition is therefore quite plainly synthetic and empirical. (Broad 1936: 107)<sup>13</sup>

It was this objection which led Ayer to abandon his position in *Language, Truth and Logic* in favour of the normativism in (Ayer 1936b). After he became dissatisfied with this position too, he tried to defuse the worry by pointing out that thinking of necessary truths as truths *guaranteed by linguistic rules* need not amount to thinking of these as being *about* linguistic rules (1946: 16–7). This is presumably quite correct, but it is difficult to see that it gets to the heart of the matter. For if knowing what the linguistic rules are requires an empirical investigation of the linguistic community, and modal knowledge is based on such linguistic knowledge, then it remains the case, it seems, that modal knowledge too requires some such investigation.

More importantly, this would seem to turn empirical facts concerning the public language into the kind of external standard of correctness which

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some resources here (cf. e.g. Chalmers 2006) – as does some of Albert Casullo’s work on the relationship between necessity and apriority (1977; 2010).

<sup>13</sup>Cf. also Ewing (1940: 213–4), Pap (1958: 165–6) and Burgess (1997: 30–1).

## 6. The Character of Linguistic Rules

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it is essential for the positivists to avoid. As Baker puts it: ‘If the concept of analytic truth is to remove an epistemological problem about the possibility of *a priori* knowledge,<sup>14</sup> it seems that the conventions of symbolism must be transparent or self-evident.’ (1988: 235) Plainly though, it is neither transparent nor self-evident how linguistic expressions are used in public languages.

A common suggestion for securing such transparency or self-evidence is to maintain that we should not be focusing on public languages but on *idiolects*.<sup>15</sup> This appeal to idiolects is, however, problematic for several reasons. To begin with, many are suspicious of the very notion of an idiolect – especially insofar as an idiolect is understood to be anything like a language in its own right:

But an idiolect is not a language; there is no describing any individual’s employment of his words without account being taken of his willingness to subordinate his use to that generally agreed as correct. That is, one cannot so much as explain what an idiolect is without invoking the notion of a language considered as a social phenomenon. (Dummett 1974: 528)<sup>16</sup>

Furthermore, even if we disregard this worry, we might be troubled by the suggestion that modal inquiry is really carried out with reference to such languages. If the linguistic approach is forced to admit that we are really interested in our own idiolects, then this gives the approach a ‘solipsistic drift’ (Baker 1988: 235) which is at odds with the appearance that modal inquiry is no less *communal* than other kinds of inquiry.

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<sup>14</sup>Again, although we are primarily interested in knowledge of necessities (and not *a priori* knowledge as such), we are assuming (with Hume) that such knowledge cannot be based on empirical evidence alone.

<sup>15</sup>In particular, this is sometimes charitably suggested by critics of the linguistic approach, at least for the sake of argument – see, e.g. Sober (2000: 52) and Giaquinto (2008: 90)

<sup>16</sup>For more scepticism about idiolects see e.g. Dummett (1991: 105–6), Wiggins (1997) and Marconi (2015: 49) – although Marconi stops short of endorsing this scepticism. For a good discussion of some of these issues, see Heck (2006).

Finally, it is, in fact, far from clear that idiolects are as epistemologically unproblematic as they are sometimes made out to be. Broad, for instance, did not think that retreating to idiolects suffices to avoid the charge that analytic propositions would be empirical according to the linguistic approach:

If an analytic proposition states that the person who records it intends to use certain words in certain ways, it evidently makes a statement about the present experiences of the speaker and about his future behaviour. The former can be tested only by the speaker himself introspectively. The latter can be tested by seeing how the speaker does use these words in the later parts of his discourse. The analytic proposition is therefore quite plainly synthetic and empirical. (1936: 107)

It is certainly true that if a claim about my idiolect is a claim about how I will go on to use linguistic expressions in the future, then we are dealing with an empirical prediction. And even if this is a prediction that I am in a particularly good position to make, the answer is neither “transparent” nor “self-evident”. In response to this, it is tempting to maintain that what we are really interested in is the language that a given person speaks *at a given time* – their “instantaneous idiolect” so to speak.

Even this, however, does not guarantee transparency and self-evidence. Suppose that someone, Mary, is asked what rule governs her own use of ‘bachelor’. Quite plausibly she might reply that she takes ‘bachelor’ to be correctly applicable to a person  $x$  if and only if  $x$  is both male and unmarried. The interlocutor then points out that, according to this rule, ‘bachelor’ is correctly applicable to male infants and toddlers. Now, Mary *might* reply that this is indeed the case (perhaps with the disclaimer that she considers this use to be pragmatically misleading). However, it is also perfectly conceivable that she would just admit to having made a mistake and say that, contrary to her initial judgement, the rule she goes by is *not* that ‘bachelor’ can be applied to all and only unmarried males, but that it can be applied to all and only unmarried *adult* males.

This should emphatically *not* be described as a case where Mary is correct about her idiolect when she gives her initial response, but *changes* her language when it is pointed out that ‘bachelor’, so understood, would be applicable to infants and toddlers. This would be to say, I take it, that had Mary been asked at the slightly earlier time whether toddlers can be bachelors she would have answered affirmatively, but we might perfectly well assume that this is false. Moreover, going down this route would create a number of problems. After all, why did Mary say that she had made a mistake? And what motivated the hypothesised sudden change in language?

Instead, I maintain, this should be regarded as a perfectly mundane case of Mary being mistaken about her own (instantaneous) idiolect. Moreover, we do not have to resort to imagined cases in order to see that this is possible. Before reading Putnam (cf. 1962b: 660; and 1975: 161) I would probably have said, if asked, that my use of the word ‘cat’ was governed, in part, by the rule ‘apply “cat” to  $x$  only if  $x$  is an animal’ and my use of the word ‘pencil’, in part by: ‘Apply “pencil” to  $x$  only if  $x$  is an artefact’. But when I *did* read Putnam, I discovered that I was wrong about this. I did not discover that my use of those words diverged from the public language, nor did I decide to change my usage, I discovered that I was mistaken about how I, *at that time*, was inclined to use those expressions.<sup>17</sup>

The proposed retreat to idiolects, then, is not without its problems. First, the notion of an idiolect as a distinct language is controversial; second, focusing on idiolects threatens to make the linguistic approach unduly solipsistic; and third, it is not clear that idiolects deliver the transparency and self-evidence which they were meant to secure. In the remainder of this chapter I shall argue that, in fact, we can do without both the appeal to idiolects, and the aim of self-evidence. In the next section I outline why we shouldn’t aim for self-evidence in this context, and, crucially, why this is consistent with the subjective-constitutive strategy. Then, in section 6.5, I argue that we can address the concern that public languages introduce

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<sup>17</sup>For more on whether one can be mistaken about ones own idiolect, see George (1990), Higginbotham (1991) and Barber (2001).



external standards of correctness without bringing in idiolects – at least in any controversial sense.

## 6.4 External and Internal Standards of Correctness

Baker claims that ‘if the concept of analytic truth is to remove an epistemological problem about the possibility of *a priori* knowledge, it seems that the conventions of symbolism must be transparent or self-evident.’ (1988: 235) And we have seen that it is doubtful that retreating to (instantaneous) idiolects succeeds in making these conventions ‘transparent or self-evident’ – at least if that is supposed to preclude the possibility of making mistakes about what the conventions are.

On reflection, however, we might wonder whether we should even *want* to secure this kind of transparency in the first place. It is, after all, a plain fact that we frequently *do* make mistakes in areas, like logic, which the linguistic approach is meant to account for. If we hold that such knowledge is based on a knowledge of linguistic conventions that we could not possibly be mistaken about, then we now need to explain how it is that mistakes nevertheless creep in. I am not saying that this is a problem that could not possibly be addressed, but the fact that this emerges as a problem should make us ask ourselves whether we should really want to rule out the possibility of being mistaken about the linguistic rules.

Baker’s suggestion is that such transparency is needed in order to ‘remove an epistemological problem about the possibility of *a priori* knowledge’. But it isn’t obvious why this should be so. The problem in question is, I take it, what we have been referring to as ‘the problem of inexplicable agreement’. The linguistic approach attempts to address this problem by getting rid of the idea of an external standard of correctness, so that it becomes clear why there is no need for ordinary empirical input in order to establish agreement between our modal beliefs and the external world.

This, however, does not immediately imply that there can be no room for mistakes on this conception. To see this, we may return to the case of Mary above. I maintain that we can make sense of her mistake without bringing in an external standard of correctness which would, as Hahn puts it, make the possibility of knowledge here a matter of ‘mysterious parallelism’.

The key point is just that Mary’s mistake was revealed to her *by further reflection*: She corrected *herself*. Unlike in a case where, say, Mary makes a mistaken prediction about how she or others will use ‘bachelor’ in the future, this was not a case of Mary’s belief failing to match an external standard of correctness. This is indicated by the fact that Mary did not require any additional *information* in order to determine that she had made a mistake. She was only asked a question which resulted in her reflecting on how she would use the expression in a hypothetical case which she hadn’t yet considered. The correction came, we may say, from the same source as the initial judgement – namely from Mary’s reflecting on her use of linguistic expressions.

Nor is there any great mystery as to how this could happen. Mary’s initial verdict was a universal statement: ‘Bachelor’ can be applied to all and only unmarried males. However, the full range of relevant cases need not have occurred to her. She might just have tried to imagine some “arbitrary” unmarried male and concluded that she would apply ‘bachelor’ here, without realising that she was in fact assuming this to be an adult. When she is later asked to consider the case of a male child, this assumption comes to light and she revises her judgement.

What this shows is that we can make sense of the possibility of making a mistake without appealing to the kind of external standard of correctness which would reinstate the inexplicable agreement problem. An adherent of the linguistic approach need not, therefore, maintain that our linguistic conventions must be self-evident in a way that would rule out this possibility.

Relatedly, an adherent of the linguistic approach is not, as it is some-

times said, committed to the idea that knowledge of necessities must be *unrevisable* knowledge. Putnam, for instance, writes that: ‘If S is true simply by virtue of what S means, and meanings are invariant under scientific and common-sensical belief fixation, then the status of S must likewise be so invariant. S must be an *unrevisable* truth’ (1986: 414).<sup>18</sup>

But this argument is problematic: If ‘the status’ in question refers to the truth-value of the sentence, then it must indeed be invariant (this is the case quite generally as long as the meaning is held fixed and the sentence doesn’t contain indexicals). However, in order for this to relate to revisability, it must refer, rather, to whether the sentence is *believed* to be true, and if so the argument fails. The reason is that such a belief may be revised as a result of revising a belief about what the meaning of the sentence is. Someone can coherently maintain, that is, that the meaning of a sentence suffices to guarantee that it is true, while conceding that they might be mistaken about this, since it might turn out that they had an inadequate conception of what the meaning of the sentence was. This, for instance, was what happened in the case of Mary and the sentence ‘all unmarried males are bachelors’.

There is, then, no need for the kind of unrevisability, self-evidence or infallibility that idiolects are sometimes said to secure. This is to some extent *good* news for those advocating idiolects since, as we saw, it is doubtful that they actually manage to secure this. However, it also removes much of what motivated the appeal to idiolects in the first place. And since we have seen that this appeal is problematic for other reasons, it would be good if it could be avoided. This is the conclusion I shall argue for in the remainder of this chapter.

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<sup>18</sup>The final section of Quine (1951) is sometimes read as containing a similar argument: No statement is immune to revision, so no statement is analytic. However, Quine has also been read as suggesting that since no statement is immune to revision there is no *need* for the notion of analyticity.

## 6.5 Avoiding Idiolects

I shall proceed as follows: First, I shall recast some of these issues in terms of a distinction between what we may call sentential and propositional doubts. Next, I shall argue that we can explain – without bringing in idiolects in any controversial sense – how someone could obtain knowledge at the propositional level in the way suggested by the linguistic approach even if they are relying on a mistaken conception of the linguistic rules in the course of doing so.

### Sentential and Propositional Doubts

It is a commonplace observation that it is possible to be right about something while being mistaken about how to express it in a given (or indeed any) language. Schlick at one point suggests that this provides us with something like a solution to our present worries, since it allows us to deny that doubts concerning whether we have an accurate conception of the public language translate into doubts concerning what we take to be necessary:

The set of symbols  $3 + 1 = 4$  designates an analytic proposition whose validity cannot be doubted once it is understood, since we then know that by definition the symbols  $3 + 1$  and  $4$  have the same meaning. But now I can in principle doubt whether it really was the symbol ‘4’ that was inserted as an abbreviation for ‘ $3 + 1$ ’, and whether it did not perhaps look like ‘5’ or even ‘!!’. Here I could indeed be the victim of a deception of memory. But [...] such a doubt does *not* give rise here to that uncertainty which is characteristic of a hypothesis, for it is not a doubt about the truth of a given proposition, but rather about whether the way I have chosen to present the proposition obeys the symbolic rules that are *otherwise customary*. (1935b: 411–2)

However, while it is certainly true that we can doubt whether a given sentence is true without doubting whether the proposition we think it expresses

is true, this does not absolve the linguistic approach. For, we may ask, what remains of this approach if it does not matter, in the end, whether we get the linguistic rules right or not? Isn't this just to concede that the appeal to such rules is really redundant here?

We can put the problem as follows: The claim is that we obtain knowledge of necessities by reflecting on the rules that govern our linguistic expressions. Now, either we need to get these rules *right* or we don't. If we do, then we are confronted with an external standard of correctness which undermines the subjective-constitutive strategy; but if we don't, then it becomes quite unclear what the role of the linguistic rules is meant to be.

Indeed, it may be said that this is where we should appeal to idiolects. We could then say that although we *do* need to get the linguistic rules right, these need not be the rules of a public language like English. Instead, they could be the rules of our respective idiolects – languages which are allegedly accessible (in some sense) to each of us on reflection. This, as I read her, is Gillian Russell's line of thought:

For suppose I have come to believe, by empirical means, that, in a certain dialect of English, *bachelor* refers to all and only unmarried men and so become justified, by the now familiar reasoning, in my belief in the proposition that all bachelors are unmarried. I might be wrong in my empirically justified belief about the English dialect. Perhaps my data was misleading in some way. Yet this need not undermine my justification for believing that all bachelors are men, because I could justify my belief that all bachelors are men using any language in which *bachelor* has the reference determiner which I thought it had in the English dialect – including my own idiolect (G. Russell 2008: 211).

Thus, our idiolects provide each of us with a language which allows us to reason *correctly* for conclusions even if it turns out that we are incorrect relative to the public language.

My contention is that while there isn't necessarily anything wrong with putting matters like this, the appeal to idiolects is not really doing any work here, and that we do not need to find a new language relative to which we can think of ourselves as reasoning correctly when our reasoning turns out to be incorrect relative to the public language. Instead, we can remain content with acknowledging that we are reasoning incorrectly, but in ways which are consistent with the idea that we thereby obtain knowledge at the propositional level. Roughly the idea is that even if we are aware that we might be reasoning incorrectly relative to the public language, this need not undermine our confidence in the conclusion we arrive at, as long as it is clear that the mistake will be irrelevant once we turn from sentences to propositions.

### Reasoning with Mistaken Linguistic Assumptions

Let us consider a case in which someone arrives at the conclusion that something is a necessary truth via a mistaken conception of what the rules of the public language are. Suppose that Lucy confuses 'equilateral' and 'quadrilateral', and therefore takes 'every rectangle is equilateral' to express a necessary truth. She might have arrived at this conclusion as follows:

The rules for applying 'rectangle' dictate that 'rectangle' is applicable to an object only if this is a polygon with four edges, and the rules for applying 'equilateral' tell us that if something is a polygon with four edges, then this is sufficient for 'equilateral' to be correctly applicable. Thus, our linguistic rules establish that whenever 'rectangle' is applicable, 'equilateral' too is applicable, and this suffices to *guarantee* the truth of 'all rectangles are equilateral'.<sup>19</sup>

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<sup>19</sup>Clearly this cannot be the whole story: We would need, for instance, to bring in the rules governing the universal quantifier, and a complete explanation would have to account for Lucy's knowledge of logic as well. However, this suffices for our present purposes.

Note, to begin with, that although Lucy assents to ‘all rectangles are equilateral’, she should not be portrayed as believing the proposition which *we* would refer to as ‘the proposition that all rectangles are equilateral’. This would not only be uncharitable, it would be to ignore the fact that she has a misguided conception of how to use ‘equilateral’. Instead, we should say that one aspect of this mistake is that she also believes, mistakenly, that the sentence ‘all rectangles are equilateral’ says that all rectangles are quadrilateral.<sup>20</sup>

Thus, although Lucy will presumably abandon her belief that ‘all rectangles are equilateral’ expresses a necessary truth once her mistake is pointed out to her, she may legitimately insist that she was right about what she meant – namely that, necessarily, all rectangles are quadrilateral. That is to say: her mistaken belief about English did not lead her to a mistaken belief about geometry. And this tells us that although the public language provides an external standard of correctness when it comes to the metalinguistic belief that a particular English sentence expresses a necessary truth, it need not act as such a standard with regard to Lucy’s belief that the geometrical proposition in question is necessary – *even if* she arrived at this belief via reflecting on linguistic rules which she erroneously believed to govern English.

Crucially, this is not to say that the appeal to linguistic rules was in fact redundant in the story concerning how Lucy obtained the propositional knowledge that, necessarily, all rectangles are quadrilateral. After all, her route to this conclusion would be closed off without any such appeal. The question is what role her misguided conception of the rules of English can be allowed to play with regard to the propositional knowledge she reaches and why her mistake turns out to be irrelevant in this context.

It is important here to keep in mind what conception of propositions an

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<sup>20</sup>She will, of course, assent to the *sentence* ‘“all rectangles are equilateral” means that all rectangles are equilateral’, but as Dummett points out: ‘It is not sufficient, for someone to know what the sentence “The Earth moves” means, for him to know the M-sentence relating to it [i.e. ““The Earth moves” means that the Earth moves’] to be true; he must know the proposition expressed by that M-sentence.’ (1975: 110)

adherent of the linguistic approach can be expected to accept. As I have already remarked, an adherent of this approach will presumably not be happy to think of propositions as extralinguistic entities which are, in some sense, *explanatorily prior* to the notion of a meaningful sentence. We should not be starting with the notion of a proposition and trying to make sense of the notion of a meaningful sentence by trying to explain how sentences get to be correlated with propositions.

Instead, the adherent of the linguistic approach will say that we should start with the notion of a meaningful sentence (a sentence which has been given a use by our linguistic conventions) and think of a proposition as a theoretical notion that is arrived at by abstracting from certain features of sentences. In particular, we should abstract from anything which concerns which *expressions* are used in the sentence and fixate solely on how the rules dictate that the sentence is to be used (independently of “what it looks or sounds like”).

We may then say that a sentence *R* expresses the same proposition as a sentence *S* iff the expressions in *R* are governed by rules that are analogous to the rules that govern the corresponding expressions in *S* – where to say that two rules are *analogous* is to say that they are identical except for the fact that they treat of different expressions.<sup>21</sup>

Against this background, coming to know that a given *sentence* is necessary amounts to realising that the linguistic conventions that govern the use of that sentence suffice to guarantee its truth. And realising that the *proposition* is necessary is just a matter of realising that *any* sentence governed by analogous rules will similarly be guaranteed to be true. Roughly speaking, then, the idea is that coming to know that some particular proposition is necessary is to come to know that *if* a sentence is governed by such-and-such rules (regardless of which expressions are used), then it will be necessary.<sup>22</sup>

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<sup>21</sup>This demand could be relaxed somewhat to yield a less fine-grained conception of propositions.

<sup>22</sup>As we have already seen, adherents of the linguistic approach frequently stress that



We can now return to Lucy and the proposition that all rectangles are quadrilateral. If we simplify somewhat,<sup>23</sup> we can say that knowing that the proposition that all rectangles are quadrilateral is necessary amounts to knowing that any sentence of the form ‘All  $\zeta$  are  $\xi$ ’ is necessary provided that ‘ $\zeta$ ’ is governed by the rule that it is applicable to an object iff it is a polygon with exactly four edges and four right angles and ‘ $\xi$ ’ is governed by the rule that it is applicable to an object iff it is a polygon with four edges. That is to say: it is like coming to know that the sentence is necessary except that we disregard which expressions are used for the predicates.

Reaching this propositional knowledge, then, is a matter of coming to know that any sentence governed by such rules will be necessary. Crucially, though, obtaining this knowledge is consistent with mistakenly believing that the English sentence ‘All rectangles are equilateral’ is governed by such rules. Thus, I maintain that the fact that Lucy has a misguided conception of the rules of English does not preclude her from relying on this conception in order to obtain modal knowledge at the propositional level. What she learns by reflecting on the linguistic rules she *mistakenly* believes to govern English is that any sentence governed by analogous rules (rules that are identical once we abstract from the particular expressions mentioned) is guaranteed to be true, and this realisation is not threatened by the fact that she mistakenly believes ‘All rectangles are equilateral’ to be one such sentence.

Once matters are put like this, it becomes clear, moreover, that nothing much is added by insisting that there *is* such a sentence in Lucy’s idiolect. Lucy’s propositional knowledge does not presuppose that we can find some sentence which is in fact governed by the relevant rules, and so looking for some such sentence – and claiming to find one in the idiolect – is besides the point.

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it is rarely of much importance, for the purposes of the explanations they are interested in, which linguistic expressions are used for which purposes (cf. in particular Carnap 1937: 6; and Sidelle 2009: 235).

<sup>23</sup>The simplification being that we restrict ourselves to the predicates involved.

The mistake which Lucy makes is, of course, a particularly straightforward one – being a simple mix-up of two English words. It would certainly be rash to generalise from this to conclude that other kinds of mistakes could be treated in similar ways. However, my more limited aim has been to show that thinking of the linguistic approach in terms of the public language does not *automatically* introduce the kind of external standard that we have been trying to avoid. It remains open to the adherent of the linguistic approach to argue that mistakes relative to public language do not undermine the possibility of knowledge at the propositional level. There is, therefore, no *in principle* objection here, but rather a general worry that should be kept in mind by someone who attempts to carry out the linguistic approach in detail.

## Part C

### Exploring the Limits of Convention



## Transition to Part C

In part B, my focus was mainly on objections which purport to show that the very idea of explaining necessary truths in the manner suggested by the linguistic approach is fundamentally misguided. I argued that none of these objections manage to establish this strong conclusion, although it should be conceded that some of them do put extra pressure on the linguistic approach in various ways.

Notably, we have seen: i) that although critics are correct that linguistic conventions by themselves do not “make necessary truths true”, this can pretty much be discarded as irrelevant; ii) that explaining the necessity of a necessary truth in terms of something contingent does not automatically undermine the necessity in question; and iii) that the adherent of the linguistic approach need not make a problematic appeal to idiolects.

I shall now move on to objections which, instead of targeting the very idea of explaining necessary truths in terms of the rules that govern linguistic representation, purport to establish that such explanations will inevitably fall short of the aim of the linguistic approach – namely that of addressing the philosophical problem of necessity.

More specifically, these objections all suggest, in one way or another, that there are limits to what can be explained in terms of linguistic *convention*, and that this undermines the explanatory ambitions of the linguistic approach. The general worry is – as Putnam puts it – that ‘the “exciting” thesis that logic is true by convention reduces to the unexciting claim that

*logic is true by conventions plus logic*' (1979: 424) – or, as we might rather put it: The exciting project of explaining necessities in terms of linguistic conventions reduces to the unexciting project of explaining necessities in terms of linguistic conventions plus logic.

I shall consider three worries along such lines: i) that we need to make a non-conventional distinction between admissible and inadmissible linguistic rules, and that this distinction will be based, in some sense, upon the notion of a logically correct rule; ii) that we need to appeal to a non-conventional notion of logical consequence; and iii) that we need to take a non-conventional starting-point for granted, and that this starting-point will consist of basic logic.

## Inadmissible Conventions?

In this chapter I consider the worry that there might be non-conventional constraints on what linguistic rules we can legitimately adopt. This threatens to undermine the linguistic approach in a couple of ways: First, it would then be natural to worry that the source of these constraints could frustrate its explanatory ambitions. If, for instance, it is suggested that we should only adopt rules that are, in some sense, logically coherent or innocent, then it would seem that, far from it being the case that the linguistic rules explain the logical necessities, logic constitutes an antecedent constraint which explains which rules may be legitimately adopted.

Second, there is an epistemological concern: If there are such constraints, then the obvious question is how we could know that they are being respected – how we could know, that is, that the rules we employ are indeed legitimate. The question then becomes whether we can answer this question without surrendering the subjective-constitutive strategy and reintroducing the problem of inexplicable agreement.

The chapter is structured as follows: I begin (7.1) by motivating the idea that there are objective constraints on what linguistic rules we may adopt via a discussion of so-called ‘disharmonious’ rules. Next (7.2), I consider a line of response advocated by Peacocke and (at one point) Boghossian, but argue that this line is unavailable to an adherent of the linguistic approach given how the commitments of this approach were articulated in Part A. I

then (7.3) argue that the better response is to maintain that disharmonious rules are not objectively bad – or more accurately: They are objectively bad only in a pragmatic sense which is compatible with the linguistic approach. Finally, I articulate (7.4) and respond to (7.5) a remaining worry: Namely that the linguistic approach is powerless to account for the fact that it is possible, given a set of introduction rules to, in a sense, *derive* the appropriate elimination rules.

## 7.1 The Problem of Disharmonious Rules

The most prevalent reason for supposing that there are objective, normative constraints on what linguistic rules we can adopt comes from the idea that such rules can take the form of introduction and elimination rules (cf. Gentzen 1935), and that we must require that introduction rules and elimination rules are appropriately related – that they are, as it is said, *in harmony*. Intuitively, the thought is that the elimination rules should allow us to “get out” no more or less than we “put in” via the introduction rules.<sup>1</sup>

Unfortunately, harmony is by no means guaranteed. Most infamously, there is Prior’s (1960) *tonk* connective, which is governed by the following pair of introduction and elimination rules:

$$\text{Tonk-I: } \frac{\varphi}{\varphi \text{ tonk } \psi} \qquad \text{Tonk-E: } \frac{\varphi \text{ tonk } \psi}{\psi}$$

The problem, of course, is that such disharmonious rules license inferences which it seems plain that shouldn’t be licensed. Indeed, with the help of these rules ‘any statement whatever may be inferred, in an analytically valid way, from any other’ (Prior 1960: 38–9).

However, disharmonious rules may be problematic without having this disastrous effect. Here is an example due to Michael Dummett:

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<sup>1</sup>There have been several suggestions for pinning down this notion more precisely. Steinberger (2011) provides a good introduction to the difficulties present here.



A simple case would be that of a pejorative term, e.g. ‘Boche’. The condition for applying the term to someone is that he is of German nationality; the consequences of its application are that he is barbarous and more prone to cruelty than other Europeans. We should envisage the connections in both directions as sufficiently tight as to be involved in the very meaning of the word: neither could be severed without altering its meaning. (1981: 454)

Although the rules for ‘boche’ do not automatically make all sentences of the language assertible, it remains plausible that these rules are objectively faulty (and not just for ethical reasons), since they, again, appear to license inferences that aren’t truth-preserving.

The immediate problem for the linguistic approach, then, is that we cannot, it seems, say merely that an inference is logically valid if it is licensed by our linguistic rules; at the very least we must add the requirement that the rules aren’t defective due to disharmony. But that raises the question of how to explain this constraint, and we might well worry that the linguistic approach lacks the resources to do so.

## 7.2 The Peacocke-Boghossian Response

An adherent of the linguistic approach has two broad options here: They could argue that, contrary to appearances, disharmonious rules are not objectively bad in the sense of allowing invalid inferences, or argue that such rules can be banned for reasons that do not undermine the linguistic approach. In this section I shall consider a suggestion for banning such rules which, however, fails to satisfy this condition. To be clear: I shall not be criticising the proposal as such, only arguing that it is incompatible with the linguistic approach understood as a version of the subjective-constitutive strategy.

It is frequently claimed that the tonk rules fail to pick out a genuine meaning since there simply is no such connective. In fact, Prior himself

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offers this diagnosis:

There are in fact *no* contonktion-forming signs [...]; and the information that ‘tonk’, or anything else, is such a sign, is simply false. ‘Contonktion-forming sign’, like ‘present King of France’, is a perfectly clear description which applies to nothing whatever. (1964: 192)<sup>2</sup>

Now, if this suggestion is provided without any further story about when and why proposed definitions *do* succeed in picking out genuine connectives (and meanings more generally), then we are confronted with the kind of conceptual Platonism that we have already established is incompatible with the linguistic approach: If it is just a brute fact about some language-antecedent conceptual domain that no meanings obey disharmonious rules, then we would have to ask, first, how we know that this is so, and, second, how we know that other (harmonious) rules always succeed in picking out meanings. Briefly put: we would be confronted with the kind of agreement problem which the linguistic approach is designed to dissolve.

Whether the suggestion that ‘tonk’ fails to pick out a genuine meaning is of any use in this context will therefore depend on what the criteria for picking out a genuine meaning are. A prominent contemporary suggestion – advocated by Peacocke and, at one point, Boghossian – goes as follows:

On the approach I myself favour, the semantic value of a logical constant is fixed as that function which makes truth-preserving those transitions involving the constant which one who understands the constant must find primitively (underderivatively) compelling. This approach still leaves room for justification, for when a proposed logical constant does have a sense, there will be a semantic value for which its distinctive methods of inference are correct. This kind of justification is not vacuous, since there are some “rules” for which no

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<sup>2</sup>Sider too advocates this diagnosis (2011: 103).

truth-preserving semantic value can be given, and those for “*tonk*” are amongst them. (Peacocke 1992: 803)<sup>3</sup>

For our purposes, however, this suggestion won’t do. The constraint that the rules must be truth-preserving introduces precisely the kind of external standard of correctness which we are crucially trying to avoid. Indeed, the problem can be sharpened. For note that it is not just truth-preservation *as a matter of fact* that is needed, but *guaranteed* truth-preservation. After all, Peacocke would want to rule out a connective such as:

$$\text{Klonk-I: } \frac{\varphi}{\varphi \text{ klonk } \psi} \qquad \text{Klonk-E: } \frac{\varphi \text{ klonk } \psi}{\text{Snow is white}}$$

Since ‘snow is white’ is true, these rules are *actually* truth-preserving,<sup>4</sup> and so, if these are to be ruled out by invoking truth-preservation, it is clear that *guaranteed* truth-preservation must set the bar. However, guaranteed truth-preservation is just logical validity, and since this is something the linguistic approach is meant to explain, the account would end up being viciously circular.

The problem here is brought out quite clearly in the following passage by Boghossian:

But we can readily see that there can be no consistent assignment of truth value to sentences of the form ‘A tonk B’ given the introduction and elimination rules for ‘tonk’. Given those rules, both ‘A → A tonk B’ and ‘A tonk B → B’ have to come out tautologous, for any A or B. It is impossible to satisfy that demand. (2001: 32–3)

Thus, essentially, the linguistic rules cannot explain why tautologies are tautologies; rather, the tautologies are invoked to explain why linguistic rules are genuine linguistic rules.

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<sup>3</sup>Cf. also Boghossian (2001: 33–4), although Boghossian offers a different account in (2003a) and (2003b), and acknowledges the change of heart in (2008: 5).

<sup>4</sup>At least if we are willing to say that ‘ $\varphi$  klonk  $\psi$ ’ is true if ‘ $\varphi$ ’ is.

### 7.3 Disharmony as a Pragmatic Problem

A more promising route, in the present context, is to argue that while disharmonious rules are problematic, they are problematic for solely *pragmatic* reasons. This would require us to both: i) explain why these rules are *not* problematic in the non-pragmatic sense that they license invalid inferences, and ii) explain what the pragmatic problem consists in. I shall attempt these tasks in order.

#### Tolerating Tonk

With regard to (i), it will perhaps come as no surprise that we can use a strategy which has been invoked several times already: Here too it is highly relevant to point out that a change in our linguistic conventions must be expected to affect the meaning of sentences in our language. It is, after all, the primary purpose of the linguistic conventions to fix what sentences mean.

This means that we cannot assume that sentences retain their customary meanings if ‘tonk’ is introduced. As Dummett rightly points out:

When an expression, including a logical constant, is introduced into a language, the rules for its use should determine its meaning, but its introduction rules should not be allowed to affect the meanings of sentences already in the language. If, by its means, it becomes possible for the first time to derive certain such sentences from other such sentences, then either their meanings *have* changed, or those meanings were not, after all, fully determined by the use made of them. In either case, it will not be true that such a derivation demonstrates that the conclusion holds good according to previously acknowledged criteria. The introduction of a new constant has created new criteria for the truth of statements not containing it. (1991: 220)

If, now, the meaning of a sentence such as ‘snow is green’ changes as a result of adopting the tonk rules, we are not in a position to say that because these

rules allow us to infer ‘snow is green’ from, say, ‘all humans are mortal’, they fail to preserve truth. The reason is that we have been given no reason to suppose that ‘snow is green’ is false in a language governed by such rules.

More recently, Jared Warren has offered a more detailed version of this response. He insists that in order to show that ‘tonk’ constitutes anything more than a pragmatic problem, we must rely on what he calls ‘the translation mistake’ – that is: ‘the mistake of being misled by superficial features into thinking that a homophonic translation is appropriate when it is not’ (2015c: 8). Warren initially suggests that in the case of a community operating with the tonk rules, homophonic translation is strongly discouraged due to charity considerations (2015c: 8). However, he later points to what I agree is the more principled reason (which is also the reason given by Dummett above): The inadequacy of homophonic translation follows naturally from the very account of meaning for which tonk is supposed to be a problem (2015c: 14).

The idea, after all, is that meaning is determined via rules which establish when sentences can be correctly asserted.<sup>5</sup> Thus, if the rules are changed so as to make it correct to utter a sentence in a circumstance where it previously wouldn’t have been correct, then the meaning of this sentence has changed. But this is precisely what adding ‘tonk’ does. Indeed, a neat way of seeing this is to notice that the rule for tonk elimination is also an introduction rule for sentences quite generally.

Moreover, by drawing on related ideas we can say more about exactly how the content of a sentence changes when ‘tonk’ is introduced: It is natural to say, in this context, that what a sentence *says* is given by the range of circumstances in which it would be correct to assert it (in accordance with the rules which have been adopted). As Dummett puts it: ‘we know what has been asserted when we know in what case the assertion is correct’

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<sup>5</sup>Note that both introduction rules and elimination rules can be viewed in this light – the elimination rules acting as licenses to assert other sentences. Note, also, that these rules are presumably not all intra-linguistic.

## 7. Inadmissible Conventions?

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(1991: 166).<sup>6</sup> Thus, for instance, I know that ‘snow is green’ says that snow is green because I know that an assertion of this sentence would be correct only if this is the case (this is how the sentence is capable of conveying this information).

If we introduce ‘tonk’, however, we are allowing that ‘snow is green’ (and, indeed, any sentence of the language) could *also* be correctly asserted in any circumstance which is such that at least one sentence of the language gets classified as assertible. The upshot, then, is that once the rules for tonk are adopted, all sentences of the language get the same meaning – they all end up saying, basically: *At least one sentence of this language is assertible*. Alternatively, each sentence can be read as a disjunction of everything that was expressible prior to introducing ‘tonk’.<sup>7</sup>

Against this background we can in fact go beyond the claim that homophonic translation is inappropriate when dealing with a language containing ‘tonk’, and make the case that the tonk rules *are* truth-preserving after all. For the natural way to understand *truth*, on this conception – at least as an initial approximation – is to say that a sentence is true if and only if it would be correct to assert it – if, that is, the circumstances are such that the rules we have adopted license uttering it.<sup>8</sup> Crucially, this means that if a language contains ‘tonk’ and at least one sentence of the language is true (correctly assertible), then *all* sentences of the language are true.

Williamson makes the following comment about ‘tonk’ and truth-preservation:

If assent to instances of those rules is necessary for understanding them, because necessary for understanding ‘tonk,’ it hardly follows

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<sup>6</sup>We can leave open, for our purposes, whether it must always be possible, in principle, to *recognise* whether an assertion is correct.

<sup>7</sup>Thus, for instance, if the original language only contained ‘snow is white’ and ‘snow is green’ (governed by the customary rules), these sentences would both end up saying that snow is white or green once ‘tonk’ is introduced, since they would both end up being assertible just in case snow is either white or green.

<sup>8</sup>Note that we may allow that an assertion could be correct in this sense even if it is *unjustified* because the speaker does not have adequate grounds for believing that it can be correctly uttered.

that the rules are truth-preserving (in the context of someone who understands ‘tonk’); they are so only if either every sentence or no sentence of the language is true (including atomic sentences, in which ‘tonk’ does not occur). (2007: 79–80)

The response just offered admits that this is quite right, but adds that, due to how the linguistic rules affect the semantics of the language, introducing *tonk* has the effect of *ensuring* that either every sentence or no sentence of the language is true.

Similar considerations apply to ‘boche’. Here the elimination rule simultaneously acts as an introduction rule for ‘*x* is barbarous’. Consequently, a sentence like ‘Smith is barbarous’ does not retain its meaning when ‘boche’, as Dummett understands it, is introduced.<sup>9</sup> We are not forced, therefore, to concede that the rules for ‘boche’ are objectively bad because they license inferences that are not truth-preserving. Indeed, since someone’s, say Smith’s, being German is now sufficient to make an utterance of ‘Smith is barbarous’ correct, the adherent of the linguistic approach should maintain that this sentence now says, essentially, that Smith is either barbarous or German – thus ensuring that the boche rules *are* truth-preserving.

It may be objected, perhaps, that this overlooks that our linguistic practices are interconnected with practices more broadly. It may be said, for instance, that if it is correct to call someone ‘barbarous’, then this also licenses *treating* them in certain ways, such as criticising or even punishing them. And yet we should certainly object to the idea that we could make it correct to criticise or punish every German person simply by adopting certain linguistic conventions. Thus, it might be said that the boche rules are objectively bad after all, since they allow us to *invalidly* infer that someone ought to be criticised or punished even if they don’t.

However, an adherent of the linguistic approach is well within their rights to insist that we would be wrong to justify punishing or criticising

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<sup>9</sup>I should say that I’m not convinced that ‘boche’, understood as strictly as Dummett requires, is part of English.

someone via a bare appeal to the boche rules. Even if it was *initially* OK to infer, from the fact that ‘barbarous’ correctly applies to someone, that they deserve some form of punishment, it should simply be insisted that once ‘boche’ is introduced, ‘*x* is barbarous’ no longer has a meaning which would make it OK to criticise or punish someone on the basis that ‘barbarous’ correctly applies to them. To think that it does would simply be to make an *ethical* mistake.

### Pragmatic Problems

I conclude, then, that the case of disharmonious rules does not force the adherent of the linguistic approach to admit that some linguistic rules are inadmissible due to licensing invalid inferences. It remains to show that we have a plausible explanation of why such rules are nevertheless held to be defective in some way. In the case of ‘tonk’ this is quite clear: It is obviously a pragmatic disaster that all sentences of the language end up being, essentially, devoid of content. We end up with a language in which all sentences are assertible if at least one is, and so the expressive power of the language will be next to nothing. This, however, does not generalise to ‘boche’ which does not have such disastrous global effects. Is there anything we can point to which suggests why these rules are nevertheless pragmatically problematic?

I think there is. Consider again Dummett’s claim that ‘when an expression, including a logical constant, is introduced into a language, the rules for its use should determine its meaning, but its introduction rules should not be allowed to affect the meanings of sentences already in the language’. Why, now, is this to be avoided? Well, plainly, it is likely to cause considerable difficulties if we introduce new vocabulary in such a way that we are forced us to revise our understanding of expressions whose meanings are already, so to speak, “internalised”.

I am, for instance, accustomed to hearing ‘Smith is barbarous’ as a claim about Smith’s character – as an assertion which is correct if and only



if Smith is barbarous. If, now, ‘boche’ were to be introduced, then, we might say, I would not be hearing this claim correctly. For now we are supposing that Smith does not have to be barbarous in order for it to be correct to assert this sentence. It might be correct to assert it simply because Smith is German. Consequently, I would then have to *train* myself to hear ‘Smith is barbarous’ as saying that Smith is *either barbarous or German* (these now being the scenarios in which the sentence can be correctly uttered). It is quite clear, as I see it, that we have good pragmatic reasons to try to avoid situations like this (which is not to say, of course, that these reasons must always be decisive).

## 7.4 Excessively Weak Elimination Rules

So far, I have argued that a plausible case can be made that: i) disharmonious linguistic conventions are not objectively *mistaken* in the sense of allowing invalid inferences, and ii) the perceived defectiveness of such rules can be explained in terms of pragmatic considerations.

However, this does not quite suffice to dispel the worry that there is a significant problem for the linguistic approach in the vicinity. Up until now, we have been concerned with cases where an elimination rule allows us to make an inference that is not (on the face of it) justified by the corresponding introduction rule – cases, that is, where the elimination rule is too *strong* relative to the introduction rule. Another possibility, though, is that the elimination rule might be too *weak* compared to the introduction rule, and so *fail* to license inferences which appear to be perfectly justified by the relevant introduction rule(s) (Steinberger 2011: 621).<sup>10</sup>

Perhaps it may be suggested that such cases do not pose a threat to the linguistic approach. After all, the problem with ‘tonk’ and the like was that the rules seemed to license objectively mistaken inferences, thereby raising the question of whether logic places an antecedent constraint on

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<sup>10</sup>In Dummett’s terminology a system in which the elimination rules are neither too weak nor too strong is said to be *stable* (1991: 287).

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which rules may be legitimately adopted. If the elimination rules are too *weak*, however, there is no danger of this happening, and this may lead someone to say that such rules aren't really that problematic. There is nothing objectively wrong, it may be said, with speaking a language which does not take full advantage of the deductive resources.

However, the problem which excessively weak elimination rules poses for the linguistic approach cannot be so easily dismissed: Even if there is nothing objectively wrong about employing such rules (which isn't clear), we are still left with a pressing question: When we say that an elimination rule fails to take full advantage of the deductive resources in the corresponding introduction rule, what explains why this is so? According to the linguistic approach, after all, logical necessities and validities are to be explained in terms of the linguistic rules, but here it seems that *by hypothesis* we are not appealing to the linguistic rules since these are what is being criticised.

Suppose, for instance, that we are confronted with a language just like English except that 'and' (which I shall hereafter call 'and\*') is governed by the following pair of rules:

$$\text{And*}-\text{I: } \frac{\varphi \quad \psi}{\varphi \text{ and* } \psi} \qquad \text{And*}-\text{E: } \frac{\varphi \text{ and* } \psi}{\varphi}$$

In other words, 'and\*' is governed by the same rules as our conjunction, except that the elimination rule is asymmetric – allowing us only to infer the first conjunct. The problem, now, is that it is extremely difficult to resist the conclusion that the members of this linguistic community *would be completely justified*, were they to also infer the truth of the second conjunct\* from a given conjunction\*. This inference, we want to say, would be logically valid *for them*. But, of course, *their* rules don't license this, so what explains this validity?

Put differently, the point is that Gentzen seems to be quite right when he writes: 'The introductions represent, as it were, the "definitions" of the symbols concerned, and the eliminations are no more, in the final analysis, than the consequences of these definitions.' (1935: 295) How can an

adherent of the linguistic approach account for the appearance of *consequence* – the seeming correspondence between introduction rules and the *right* elimination rules – here?

## 7.5 Explaining the Correspondence

One strategy open to an adherent of the linguistic approach is to argue that since the rules for *and*<sup>\*</sup> were introduced *in a language*, the relationship between the introduction rule and the (proper) elimination rules could be explained by appealing to the linguistic conventions of the language in which they were introduced. However, I shall be considering a response along different lines: In what follows I shall argue that the “correspondence” between introduction and elimination rules – when the character of these rules are properly understood – effectively takes care of itself.

### The Need for Bounds

The first thing to note is that it is in fact false that we can determine the appropriate elimination rules once we are given the introduction rules. We also need the information that these are *all* the introduction rules.

This point is quite uncontroversial, but it may be helpful to consider an example: Suppose we are told that some connective ‘#’ is governed by the following introduction rule:

$$\#-I: \frac{\varphi \quad \psi}{\varphi \# \psi}$$

If we are asked what the corresponding elimination rule is, it would indeed be natural to suggest, essentially, a notational variant of conjunction elimination. And we might argue for this as follows:

#-I tells us that in order to reach the conclusion that ‘ $\varphi \# \psi$ ’ is assertible we should have to go via a step at which both ‘ $\varphi$ ’ and ‘ $\psi$ ’ are established to be assertible. So, if we are confronted

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with an assertion of ' $\varphi \# \psi$ ', and we suppose that it has been reached in a legitimate way, then we should be in a position to assert both ' $\varphi$ ' and ' $\psi$ '.<sup>11</sup>

This reasoning, however, patently relies on the assumption that what we have been given is the *only* route to asserting ' $\varphi \# \psi$ '. For suppose, now, that we are told that there is also a further introduction rule:

$$\#-I_2: \frac{\text{Snow is white}}{\varphi \# \psi}$$

Clearly, our suggested elimination rule would now be quite unwarranted, and the general lesson is neither controversial nor difficult to spot: In order to form an adequate conception of the proper elimination rule(s), we need a conception of *everything* that is regarded as a sufficient ground for asserting a sentence of the relevant kind, and that is to say that we need to know all the introduction rules and *that* these are all the introduction rules – or as I shall say: We need a *bounded* conception of these rules.

### Inferentialism and Natural Language

The next thing I want to draw attention to is that there is in fact a slight awkwardness in transferring talk about introduction and elimination rules from the context of a formal proof-system to the context of natural language like English.

In the context of a formal system, after all, it is quite sensible to lay down, say, that the introduction rule for conjunction is:

$$\frac{\varphi \quad \psi}{\varphi \wedge \psi}$$

and maintain that this should be understood, quite rigidly, as saying that a conjunction can be asserted (or written down) if and only if both conjuncts

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<sup>11</sup>More accurately, the argument establishes that this rule could be justified on the basis of  $\#-I$ ; it does not quite suffice to establish that this is the strongest rule that could be so justified.

are already asserted (written down at the earlier stage). This allows for a straightforward decision procedure when it comes to assessing whether a conjunctive sentence has been asserted in accordance with the rules or not: We simply check whether we find both conjuncts at the previous stage.

However, this is plainly *not* the introduction rule that governs conjunction in natural languages, as is clear from the fact that we cannot check whether a conjunctive claim has been correctly asserted by checking whether the conjuncts have already been asserted. After all, I might well be asserting a conjunction in accordance with the rules of English even if neither I nor anyone else have ever asserted the conjuncts. Nor should we say, I think, that *strictly speaking* the rule is that both conjuncts must already be asserted, but that we are tacitly allowing a short-cut whereby we can assert a conjunction when we realise that we *could* have taken the trouble to assert the conjuncts first. What reason is there for saying that this is a short-cut with regard to the rule we use as opposed to simply being the rule we use?

I am not, of course, suggesting that those who advocate inferentialism as an account of natural language semantics disagree with this. My point is just that, *unlike* in the case of a formal proof-system, the introduction rule for conjunction in natural language cannot be: a conjunction is assertible if both conjuncts are asserted; it must be something like: a conjunction is assertible if both conjuncts are *assertible* – that is to say: ‘ $\varphi$  and  $\psi$ ’ is assertible if ‘ $\varphi$ ’ is assertible and ‘ $\psi$ ’ is assertible.

An analogous observation pertains to elimination rules. The fact that someone (either me or someone else) has uttered the conjunction ‘snow is white and grass is purple’ does not make it correct for me to utter ‘grass is purple’. The utterance of this sentence *might be mistaken* even if the conjunction has been asserted because it does not follow that the conjunction was in fact (correctly) *assertible*.<sup>12</sup> In other words: it is not a rule of English that ‘ $\psi$ ’ can be asserted if ‘ $\varphi$  and  $\psi$ ’ has been asserted. What is

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<sup>12</sup>Someone who infers a (false) conjunct from a (mistaken) conjunction may of course be blameless, in a certain sense, but we still don’t treat this as a correct utterance.

arguably a rule is that ‘ $\psi$ ’ can be asserted if ‘ $\varphi$  and  $\psi$ ’ is *assertible*.

This is not to deny that the inference from ‘ $\varphi$  and  $\psi$ ’ to ‘ $\psi$ ’ is quite correct. The point I am making concerns, rather, how this inferential matter can be said to inform our conception of how to use the sentences involved, and what I am stressing is that, in a natural language context, correctly asserting a sentence cannot be equated with correctly inferring it.

Based on this, I maintain that if we are to transfer talk about introduction and elimination rules from the context of a proof-system to the context of natural language semantics, then these should be understood as regulating *assertibilities* rather than assertions: Such rules govern relations of assertibility between sentences quite independently of whether they are actually asserted. On the introduction side of things, they tell us how the assertibility of a sentence can be established on the basis of the assertibility of other sentences; on the elimination side, they tell us how the assertibility of the sentence can be unpacked into the assertibility of other sentences.

This, we may note, somewhat blurs the distinction between an inferentialist and a truth-conditional treatment of the logical constants. Indeed, these essentially coincide if, in accordance with what as said in the previous section, we understand truth to be a matter of making assertions in accordance with the rules. For to say, e.g., that a conjunction is assertible if both conjuncts are assertible, is then to say that a conjunction is true if both conjuncts are true. However, this is, I maintain, entirely appropriate. It would be a weakness of the inferentialist account if it didn’t allow us to recover something in the vicinity of the truth-conditional clauses.

### **Excessively Weak Elimination Rules Again**

We can now return to our initial problem. The question was how to account for the fact, made vivid by excessively weak elimination rules in particular, that the elimination rules can generally be seen as consequences of the introduction rules (and, of course, vice versa). That is, the introduction rules appear to suffice to guarantee that certain *other* inferences are truth-

preserving, and the linguistic approach seems ill-equipped to explain this.

The solution is to point out that this problem vanishes once we both: i) recognise that this is only true if our conception of the introduction rules is *bounded*, and ii) understand the rules as regulating *assertibilities* as just argued. The crucial point is that if the rules are understood in terms of assertibility, then *there is no difference* between setting a bound on the introduction rules and establishing elimination rules. Take the introduction rule for conjunction:

(A) ‘ $\varphi$  and  $\psi$ ’ is assertible if ‘ $\varphi$ ’ and ‘ $\psi$ ’ are assertible.

Turning this into a *bounded* introduction rule is, in effect, to replace it with:

(B) ‘ $\varphi$  and  $\psi$ ’ is assertible if and only if ‘ $\varphi$ ’ and ‘ $\psi$ ’ are assertible.

But going from (A) to (B) is plainly equivalent to adopting, in addition to (A), the elimination rule:

(C) If ‘ $\varphi$  and  $\psi$ ’ is assertible, then both ‘ $\varphi$ ’ and ‘ $\psi$ ’ are assertible.

Now, it may be asked if this isn’t just a matter of *deriving* the elimination rule from the bounded introduction rule (in accordance with the logic which governs the conditionals here), and whether it isn’t precisely this entailment which an adherent of the linguistic approach would struggle to explain.

This, however, would be to misunderstand the point I’m making. First, as mentioned, it is open to an adherent of the linguistic approach to point out that when the rules are explicitly formulated like this, we can presumably rely on the linguistic conventions of the metalanguage to explain how (C) follows from (B) should we want to.

The point I am making here, however, is that if we turn our focus away from matters having to do with the *formulation* of the rules, and just consider what it would be like for rules like these to actually be implemented in linguistic practices, then the whole question of justifying elimination rules on the basis of introduction rules disappears. The reason

## 7. Inadmissible Conventions?

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is that once the rules are understood to be governing assertibility relations, we cannot even make the required distinction between adopting a bounded introduction rule and adopting the “right” elimination rule.

To see this, let us return to the  $\text{and}^*$  rules presented above. My claim is that if we imagine a community who goes by  $\text{And}^*\text{-I}$ , and who: i) treats this rule as governing relations of *assertibility*, and ii) does not treat sentences of the form ‘ $\varphi$  and\*  $\psi$ ’ as assertible in any other circumstances, then we are automatically imagining a linguistic community which treats ‘ $\varphi$ ’ and ‘ $\psi$ ’ as assertible whenever ‘ $\varphi$  and\*  $\psi$ ’ is assertible – a linguistic community, that is, which goes by the “correct” elimination rule. It makes no sense to suppose that they go by the weaker rule  $\text{And}^*\text{-E}$ , since the bounded introduction rule already ensures that situations in which they treat ‘ $\varphi$  and\*  $\psi$ ’ as a correct assertion are situations in which they treat both ‘ $\varphi$ ’ and ‘ $\psi$ ’ as correct assertions. There is, therefore, no question of them having to derive the right elimination rules; they are already built in.

Fundamentally, the point is that once we drop the requirement that the assertions actually have to be *made*, we lose the “directionality” which allows us to even make a proper distinction between introduction and elimination here. Indeed, it becomes misleading to talk about two different kinds of rules at all, as opposed to a single underlying correlation between the assertibility of certain sentences and certain conditions being fulfilled. The introduction aspect tells us that if the conditions are satisfied, then the sentence can be correctly asserted, whereas the elimination aspect tells us that if the sentence is correctly asserted, then the conditions are satisfied.



## Conventions and Consequences

In this chapter, I consider various versions of the worry that the linguistic approach must assume – and therefore leave unexplained – a non-conventional consequence relation in order to account for how we get from a (presumably finite) set of initial linguistic conventions to all the necessary truths that need explaining.

I begin (8.1), by discussing the famous Quine-Carroll regress argument. This argument is frequently thought to show that unless some notion of logical consequence is taken for granted, any linguistic convention will be “logically inert” in a way which makes it impossible to explain how finitely many conventions could give rise to infinitely many logical necessities. I argue that – as Quine himself acknowledged – the argument does not suffice to establish this. In the course of doing so, I shall attempt to clear up some of the confusion that surrounds this argument in the literature, where there is surprisingly little agreement as to just what its scope is.

In section 8.2, I consider a related argument, prominently articulated by Dummett, which suggests that an adherent of the linguistic approach faces an unpalatable choice between appealing to an unexplained notion of consequence and surrendering the notion of a rationally compelling proof. I argue that this dilemma – at least as initially formulated – can be avoided via an unconsidered alternative.

As Dummett himself stresses, however, the true force of this dilemma

emerges only against the background of Wittgenstein’s “rule-following considerations”. Although this Wittgensteinian theme raises issues that go well beyond anything that can be satisfactorily dealt with here, I shall argue (8.3) that a decent case can be made that an adherent of the linguistic approach can and should accept the lessons of Wittgenstein’s arguments.

### 8.1 The Quine-Carroll Regress

In this section I shall proceed as follows: After articulating the regress argument, I consider the standard response (suggested by Quine himself), namely to allow linguistic conventions to be implicitly adopted. What is puzzling about this response, however, is that some commentators have found it quite unclear how this move could be of any help – indeed, they have found it quite clear that it is of *no* help. Leaving this puzzle unresolved for the time being, I move on to another suggested response: allowing linguistic conventions to take the form of inference rules. Here too, however, we find that whereas some tout this as a solution, others appear to find it obvious that this move is futile. I then argue that the proper response to the regress argument involves combining elements from both these ideas, and suggest that the lack of agreement in the literature is due to the fact that this has not been sufficiently appreciated.

#### The Regress Argument

In section II of ‘Truth by Convention’ (Quine 1936),<sup>1</sup> Quine considers whether the method of “postulation”, whereby we assign truth-values to sentences by stipulation, could be said to make propositional logic true by

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<sup>1</sup>This paper is frequently grouped together with Quine (1951) and (1960a) as a paper in which Quine straightforwardly attacks Carnap’s views (cf. e.g. Becker 2012: ch. 1). However, Richard Creath has argued (to my mind persuasively) that this is mistaken, and that the earlier paper is ‘a request for further clarification’, which is ‘done in the spirit of a friendly amendment to Carnap’s work.’ (1987: 478) Quine himself said of ‘Truth by Convention’ that it ‘already bore the seeds of [his] apostasy’ (1985: 121–2; cf. also 1986: 16).

convention. He observes (1936: 107) that since there are infinitely many theorems of propositional logic, we cannot achieve this by postulating each theorem individually. Instead we must ‘avail ourselves of conditions finite in length which determine infinite classes of expressions.’ (1936: 107)

Initially, Quine remarks, optimistically, that ‘such conditions are ready at hand’ (1936: 107), and proposes the following three conventions:

- (I) Let all results of putting a statement for ‘ $p$ ’, a statement for ‘ $q$ ’, and a statement for ‘ $r$ ’ in ‘If if  $p$  then  $q$  then if if  $q$  then  $r$  then if  $p$  then  $r$ ’ be true. (1936: 108)<sup>2</sup>
- (II) Let any expression be true which yields a truth when put for ‘ $q$ ’ in the result of putting a truth for ‘ $p$ ’ in ‘If  $p$  then  $q$ ’. (1936: 108)
- (III) Let all results of putting a statement for ‘ $p$ ’ and a statement for ‘ $q$ ’ in ‘If  $p$  then if  $\neg p$  then  $q$ ’ or ‘If if  $\neg p$  then  $p$ , then  $p$ ’, be true. (1936: 110; ‘ $\sim$ ’ replaced with ‘ $\neg$ ’)

In more standard (and less horrible) terminology: (I) introduces the axiom schema ‘ $(p \rightarrow q) \rightarrow ((q \rightarrow r) \rightarrow (p \rightarrow r))$ ’, (II) introduces modus ponens, and (III) introduces the two axiom schemas ‘ $p \rightarrow (\neg p \rightarrow q)$ ’ and ‘ $(\neg p \rightarrow p) \rightarrow p$ ’.

In the third section of the paper, however, Quine adopts a more critical approach. This is where he introduces the regress argument:

In the adoption of the very conventions (I)–(III), etc., whereby logic itself is set up, however, a difficulty remains to be faced. Each of these conventions is general, announcing the truth of every one of an infinity of statements conforming to a certain description; derivation of the truth of any specific statement from the general convention thus requires a logical inference, and this involves us in an infinite regress. (1936: 120)

Before explaining in detail just how the regress is generated, Quine informs us that ‘for present purposes’, it will be simpler to rewrite (II) as:

<sup>2</sup>The formulation has been altered to mention the relevant sentence schema explicitly.

## 8. Conventions and Consequences

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(II') No matter what  $x$  may be, no matter what  $y$  may be, no matter what  $z$  may be, if  $x$  and  $z$  are true [statements] and  $z$  is the result of putting  $x$  for ' $p$ ' and ' $y$ ' for ' $q$ ' in 'If  $p$  then  $q$ ' then  $y$  is to be true. (1936: 103; square brackets in original)

This, though, is somewhat cumbersome, so I suggest that we simplify it as follows:

(II\*) For all sentences  $x$ ,  $y$  and  $z$ , if  $x$  and  $y$  are true, and  $y$  is a conditional sentence with  $x$  as its antecedent and  $z$  as its consequent, then  $z$  is also true.

Suppose, now, that we have already established the following:

- (i)  $A$  is true.
- (ii)  $B$  is true.
- (iii)  $B$  is a conditional sentence which has  $A$  as its antecedent, and  $C$  as its consequent.

And the aim is to use (II\*) to get from (i)–(iii) to:

- (iv)  $C$  is true.

Let us concede, for the sake of argument, that we can use universal instantiation on (II\*) to get:

- (1) If  $A$  and  $B$  are true, and  $B$  is a conditional sentence with  $A$  as its antecedent and  $C$  as its consequent, then  $C$  is also true.

And let us also concede that we can use conjunction introduction to render (i)–(iii) as:

- (2)  $A$  and  $B$  are true, and  $B$  is a conditional sentence with  $A$  as its antecedent and  $C$  as its consequent.

Now, clearly, we can infer (iv) from (1) and (2) using modus ponens. However, the whole point of the task we are now engaged in is to show how (II\*) is actually responsible for licensing this move. Thus, we need to show how (II\*) allows us to get from (1) and (2) to (iv). But this task, as Quine points out, is ‘exactly analogous to our original task’ (1936: 104). That is to say, we are now charged with the task of showing how (II\*) allows us to get from:

- (i') (1) is true.
- (ii') (2) is true.
- (iii') (2) is a conditional sentence which has (1) as its antecedent, and (iv) as its consequent.

to:

- (iv') (iv) is true.

We are thus faced with an instance of the same problem we started out with, and a little reflection reveals that there is indeed an infinite regress here: If we did try to make it explicit how (II\*) allows us to get from (i')–(iii') to (iv'), we would merely discover that yet another instance of essentially the same task needs to be carried out. It appears, then, that adopting convention (II\*) ‘gets us nowhere, very slowly’ (Hale 2013: 119).

As Quine suggests in a footnote (1936: 121), the regress here is essentially that pointed out by Lewis Carroll in ‘What the Tortoise Said to Achilles’ (1895). The point, however, is that it takes on a particular significance in the context of the linguistic approach: The question isn’t just whether we can avoid the regress, but whether we can do so without surrendering this approach in the process.

## Implicit Conventions

Quine himself suggested one strategy for doing so:

It may be held that we can adopt conventions through behaviour, without first announcing them in words; and that we can return and formulate our conventions verbally afterward, if we choose, when a full language is at our disposal. [...] So conceived, the conventions no longer involve us in a vicious regress. Inference from general conventions is no longer demanded initially, but remains to the subsequent sophisticated stage where we frame general statements of the conventions and show how various specific conventional truths, used all along, fit into the general conventions as thus formulated. (1936: 123)<sup>3</sup>

Although Quine worried that ‘in dropping the attributes of deliberateness and explicitness from the notion of linguistic convention we risk depriving the latter of any explanatory force’ (1936: 106), more recent writers have tended to think that Quine was overly pessimistic here. A prevalent attitude in the contemporary literature, therefore, is that as long as we accept (as we should presumably want to accept anyway) that some linguistic conventions are initially adopted implicitly through behaviour, then the regress argument can pretty much be set to one side.<sup>4</sup>

What is puzzling, however, is that a competing strand in the literature holds that appealing to implicit conventions achieves nothing at all in this context. Hale, for instance, writes that ‘Quine ought not have so readily conceded that his regress can be avoided by retreat into inexplicitness’ (2013: 126), and elaborates on this as follows:

Resort to some inexplicit analogue of explicit stipulation – some sort of communal agreement to treat the relevant sentences as immune to revision – would still run foul of the problem of finitude. For we

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<sup>3</sup>Quine reiterates that the regress argument only targets *explicit* conventions in (1960a: 357) and again in (1969).

<sup>4</sup>Richard Creath (1987; 2003), Paul Syverson (2003), Jody Azzouni (2014) and Jared Warren (2017) all take this line, typically citing the work of David Lewis (1969) as contributing to rehabilitate the notion of an implicit convention. Quine too concedes that Lewis’s work addresses his earlier concerns about this notion (1969).

would still have, at best, only finitely many direct guarantees of conventional truth, and any attempt to provide, for the infinite remainder by appeal to logical consequence would serve only to highlight further as yet unconventionalized necessities (Dummett's point), or would degenerate into an infinite regress (Quine's). (2013: 127)

The puzzle is not just that Hale *disagrees* with the claim that the regress can be avoided by appealing to implicit conventions, but that, on his reading, it is quite mysterious why anyone would even think so in the first place. The explicit/implicit issue appears quite orthogonal from the central problem of getting infinitely many necessities from finitely many conventions.

Some of Quine's remarks related to this adds to the puzzlement. Having presented the regress, he writes:

Alternatively the difficulty which appears thus as a self-presupposition of doctrine can be framed as turning upon a self-presupposition of primitives. It is supposed that the *if*-idiom, the *not*-idiom, the *every*-idiom, and so on, mean nothing to us initially, and that we adopt conventions (I)–(VII) by way of circumscribing their meaning; and the difficulty is that communication of (I)–(VII) themselves depend upon the free use of those very idiom which we are attempting to circumscribe, and can succeed only if we are already conversant with the idioms. (1936: 104)

This certainly does suggest a reason why relying solely on *explicit* conventions will be doomed to failure: In order to understand the explicit conventions, we need to already understand the relevant language – including any logical expressions that figure in those conventions. Since, now, we cannot avoid employing such expressions here, this route is closed off.

This is certainly very plausible, but it is puzzling why Quine would invoke anything as complicated as the regress argument to make *this* point. It is quite obvious, after all, that we cannot, in general, rely *solely* on explicit conventions: We cannot teach someone their first language by *telling them* what the linguistic conventions are. As Richard Creath puts it: 'Quine's

claim would be little more than an instance of the more general truth that without a language you cannot say much of anything.’ (2003: 248)

The suggested appeal to implicit conventions thus leaves us with a number of puzzles: Just what is the relevant difference between implicit and explicit conventions supposed to be? Why is there so little agreement as to whether this response is successful? And how does the regress argument go beyond the fairly mundane point that the use of the logical constants cannot be taught explicitly to someone who knows no such expressions? We get a better understanding of these issues, I believe, by looking at a different response to the regress argument.

### Conditional Legislations and General Dispositions

As mentioned, Quine’s regress is a close cousin of that of Lewis Carroll. So, given that a standard diagnosis of what goes wrong in Carroll’s dialogue is that the Tortoise is failing (perhaps deliberately) to draw the distinction between premises and rules of inference (cf. e.g. Clark 2007: 97–9), it is natural to ask whether Quine’s regress can be avoided if we appeal, not just to postulation of truth, but also to linguistic conventions that license rules of inference. This, effectively, is Theodore Sider’s line of thought when he suggests that someone who wants to avoid the regress might appeal to ‘conditional legislations’ – where such a legislation ‘results in its being the case that if certain sentences are true by convention, then so is a certain further sentence.’ (2011: 100)

Again, however, the literature on the subject is puzzling. First of all, it appears that Quine *does* consider precisely this idea. His convention (II), after all, was:

- (II) Let any expression be true which yields a truth when put for ‘*q*’ in the result of putting a truth for ‘*p*’ in ‘If *p* then *q*’.

This is a pain to read, but what it says is that whenever we have two sentences such that one is a conditional and one is the antecedent of that



conditional, and both these sentences are true, then we are to assign True to the consequent of this conditional as well. Isn't this precisely the kind of convention that Sider is proposing? And if so, how could it possibly be a way of avoiding the regress?

Indeed, others have read Quine's regress as targeting precisely the suggestion that Sider offers as a response. Hale, for instance, sees Quine's argument as torpedoing the idea that we could get infinitely many logical truths from finitely many conventions by 'conventionalizing the means of deduction' (2013: 119) – for instance by including the rule of modus ponens as a convention. Again we see that there is surprisingly little agreement as to just what the regress argument shows and how it is meant to work.

Here, however, Quine must take some of the blame. For although his convention (II) *is* in the form of a 'conditional legislation' (i.e. inference rule), there is a rather suspicious transition in the move from (II) to (II'), and, in particular, to his instruction, when presenting the regress, that we are to 'take (II') *as a premiss*' (1936: 120; my emphasis).<sup>5</sup> This is markedly different from what seemed to be the initial instruction in (II). We are not told, as before, to distribute truth-values in accordance with (II), but to assign the truth-value True to (II') itself. We may say that whereas (II) was introduced as a *postulation* – an instruction as to how to distribute truth-values – (II') is to be treated as a *postulate* – a sentence which is stipulated to be true.

In the end, then, convention (II) is simply not treated as a conditional legislation, and so the appearance that Quine considers such legislations is largely illusory. Still, this is not quite to show that the regress can be avoided. For it may be said that the proper lesson of Quine's argument is that we cannot *really* adopt such (general) conditional legislations unless some background logic is already in place. Thus, Hale writes:

Our convention corresponding to *modus ponens* is, and has to be, general. To apply it, we must make some inferences. In this case,

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<sup>5</sup>Carlo Giannoni also criticises Quine on these grounds (1971: 60).

besides observing that a truth results from substituting  $q$  for  $B$  and some truth  $p$  for  $A$  in  $A \rightarrow B$ , we must infer from our general convention that if a truth results from putting  $q$  for  $B$  and  $p$  for  $A$  in  $A \rightarrow B$ , then  $q$  has to be true, and thence infer (by *modus ponens* *once again*) that  $q$  is to be true. Since these inferences must themselves be accorded the same or similar treatment, we can never get to first base. (2013: 120)

In other words: to really adopt a functioning modus ponens rule, we would have to adopt a *general* convention that is applicable in infinitely many cases, but in order to do *this* we already need some logic.<sup>6</sup>

However, this passage trades on an ambiguity with regard to *generality*. It is true that there is a good sense in which the modus ponens convention must be *general*, and it is also true that if adopting modus ponens involves coming to accept a general – in the sense of ‘universally quantified’ – claim of the form ‘For all sentences, if so-and-so, then infer so-and-so’, then inferences are needed both in order to apply it to a specific case and in order to move from the antecedent to the consequent. It is *not* true, however, that adopting a general convention must involve accepting a universally quantified claim (even implicitly), as opposed to acquiring a general disposition to treat certain sentences as assertible in certain contexts – at least there is nothing in the regress argument which rules this out.

Nor do I think we should *want* to rule it out, since we should then have to deny, e.g., that a child could implicitly adopt a general convention to call red objects ‘red’, unless she has already mastered modus ponens and the rule for eliminating the universal quantifier (we may in fact suppose that the child has not learned to use either quantifiers or conditionals). Even if *we*, if asked to make explicit the convention that the child has mastered, might say that she has internalised the instruction: ‘For all objects  $x$ , if  $x$  is

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<sup>6</sup>Hale’s way of putting it suggests that even if we did adopt a general modus ponens convention, there would still be the issue of how to apply it. I prefer to say that this casts doubt on whether such a convention could be adopted at all since it is unclear what it would be to adopt such a rule without proceeding to make correct applications of it. I shall return to this issue when discussing the rule-following considerations below.

red, then ‘red’ can be predicated of  $x$ ’, this is not to say that she, in order to apply this rule, must first infer, by universal elimination, an instance of this sentence, and then use modus ponens to arrive at the consequent.

More generally, the point is that the regress does not really get a grip if we allow that adopting a linguistic convention could amount to implicitly acquiring a general disposition, since we have been given no reason to suppose that exercising a general disposition on a particular occasion must be a matter of *logical inference*: ‘The sense in which the truth of individual logical truths “follows from” conventions is not the same as that in which the consequent of a logically true conditional follows from the antecedent.’ (Tennant 1987: 88)<sup>7</sup>

## Conclusions Regarding the Quine-Carroll Regress

Quine was therefore entirely right to make the concession that he did: There is nothing in the regress argument which rules out the idea that finitely many conventions could give rise to infinitely many logical necessities, provided that adopting a linguistic convention could amount to something like implicitly acquiring a general disposition with regard to language-use.

Moreover, we can now say more about why there is so little agreement as to whether making the conventions implicit is of any help in this context. For although those, like Hale, who deny this are right that we do not avoid the regress *merely* by making the conventions implicit, they are wrong more generally insofar as they fail to see that by allowing implicit conventions we open the door to the idea that adopting a linguistic convention could amount to acquiring a general disposition through behaviour.

Thus Hale is correct that if our focus is on ‘conventional assignments of truth to complete sentences’, it does not much matter whether this is understood to be carried out explicitly or implicitly (2013: 126–7): Since we can only do this for finitely many sentences, it won’t get us to infinitely many logical truths, and, as the regress make vivid, it doesn’t help if these

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<sup>7</sup>Cf. also A. Miller (2014).

sentences themselves are universal claims. The importance of allowing linguistic conventions to be implicitly adopted emerges only when we conceive of these in terms of general dispositions. For now it makes a big difference whether we are meant to conceive of adopting a convention in terms of agreeing to a sentence which captures the relevant disposition, or whether we are just meant to *acquire* the disposition through training.

So, taking Quine's convention (II) as an example, the idea isn't that we might implicitly *accept this sentence*, but that we might implicitly align our linguistic behaviour with (II): We might simply start treating any sentence which constitutes the consequent of a true conditional with a true antecedent as itself being true, and so acquire a general disposition which would result in us making inferences in accordance with modus ponens.

Now, I should say that this is primarily intended as an *in principle counterexample* to the regress argument. I do not think that this is the account that an adherent of the linguistic approach should give about either modus ponens in particular or propositional logic more generally. First, I doubt that our dispositions are as straightforward as this (sometimes we are disposed to reason according to modus tollens rather than modus ponens). Second, and more importantly, I do not think that an adherent of the linguistic approach should be focusing on *postulation of truth* (either directly or conditionally); the linguistic conventions that are implicitly adopted are more plausibly construed as establishing relations of assertibility between sentences as outlined in the previous chapter.

Still, it is crucial to show that the regress *can be avoided*, since it would otherwise be very natural to suppose that any version of the linguistic approach would have to fall victim to it. Moreover, the key to avoiding the regress – namely the appeal to general dispositions – figures prominently in what I take to be the most promising version of the linguistic approach as well.

## 8.2 Explaining Consequences

The regress argument provides one possible route to the conclusion that no finite set of linguistic conventions could account for infinitely many logical truths, and I have argued that this route is unsuccessful. This, though, is not to say that there are no problems in the vicinity: One especially natural worry is that even if we assume that an initial set of conventions *can* give rise to further necessities/validities, this would still be a *problem* for the adherent of the linguistic approach, since they would be powerless to explain the status of this very entailment. Roughly put: if it said that adopting such-and-such conventions commits us to so-and-so, then the natural follow-up is (of course): ‘And what explains *that?*’ with the insinuation that the linguistic approach here reaches a dead end.

In this section I approach this issue via a dilemma articulated by Dummett (1959). Although Dummett’s discussion eventually draws on Wittgenstein’s ‘rule-following considerations’, the dilemma is initially presented in a more naïve form. Since this naïve form of the dilemma embodies an objection which many have found to be convincing in its own right, I shall postpone bringing in the rule-following considerations to section 8.3, and first discuss the dilemma as initially presented.

### Dummett’s Dilemma

Dummett’s dilemma centres around the question: *Do our linguistic conventions have consequences?* If the adherent of the linguistic approach denies this, she is, in Dummett’s terminology, a ‘full-blooded conventionalist’; if she affirms it, she is a ‘modified conventionalist’. The problem is that neither position is satisfactory.

The following example will help to illustrate the difference between modified and full-blooded conventionalism as well as the difficulties faced by each:<sup>8</sup> Let us suppose that an adherent of the linguistic approach holds

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<sup>8</sup>This example is not found in Dummett (1959), but is used by Jared Warren (2017)

that both *modus ponens*:

$$\text{MP: } \frac{\varphi \quad \varphi \rightarrow \psi}{\psi}$$

and *conditional proof*:

$$\text{CP: } \frac{\begin{array}{c} \overline{\varphi} \text{ (i)} \\ \vdots \\ \psi \end{array}}{\varphi \rightarrow \psi} \text{ (i)}$$

are valid rules of inference because these rules are directly adopted as linguistic conventions, thereby (partly) circumscribing the meaning of the material conditional.

Now consider another rule of inference featuring only the conditional, namely *hypothetical syllogism*:

$$\text{HS: } \frac{\varphi \rightarrow \psi \quad \psi \rightarrow \chi}{\varphi \rightarrow \chi}$$

The question which divides the modified and the full-blooded conventionalist is: Did we already commit to (HS) when we adopted (MP) and (CP)? The modified conventionalist says yes. The validity of (HS) is a consequence of (MP) and (CP), so having accepted these two, we have no choice but to admit that (HS) is a valid rule of inference. The full-blooded conventionalist on the other hand, will say that accepting (HS) is a new convention and not a consequence of (MP) and (CP). Nothing about the acceptance of (MP) and (CP) *forces* us to accept (HS); this, rather, involves a new stipulation which further circumscribes the meaning of the material conditional, thus modifying the concept (cf. Dummett 1959: 333).

It is not difficult to see why someone who approaches this issue with a standard conception of proof in mind would be troubled by full-blooded conventionalism. If the full-blooded conventionalist is right, we are never forced to accept the conclusion of a proof; there is always an element of 

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in a discussion of the regress argument.

choice. Dummett thought (controversially) that Wittgenstein was prepared to accept this conclusion, but Dummett was not prepared to do so himself:

It seems extraordinarily difficult to take this idea seriously when we think of some particular actual proof. It may of course be said that this is because we have already accepted the proof and thereby subjected our concepts to the modification which acceptance of the proof involved; but the difficulty of believing Wittgenstein's account of the matter while reading the proof of some theorem with which one was not previously familiar is just as great. (1959: 333)<sup>9</sup>

Now, it is important to be clear about what is and is not being claimed here. The full-blooded conventionalism can presumably accept that, *phenomenologically speaking*, we often find proofs to be *compelling*.<sup>10</sup> They will deny, however, that proofs are *rationally* compelling. Thus, even if it may well be the case that I cannot resist accepting a step in a proof, the full-blooded conventionalist will say that I would not have made a *mistake* if I had refused to accept it. To use a helpful analogy suggested by Crispin Wright (1990: 95): We can acknowledge that we are often phenomenologically compelled to find a situation funny while maintaining that we would not be “getting the world wrong” if we didn't.

The crucial claim, then, is that, according to the full-blooded conventionalist, we are never *rationally* compelled to accept a given step in a proof. But as Dummett points out in a later paper on the subject (Dummett 1993): A conception of proof which denies that proofs are rationally compelling is hardly a conception of *proof* at all. Thus, full-blooded conventionalism ‘can make our linguistic practices the whole source of necessity and of truth only by discrediting those practices, and, indeed, the concepts of necessity and truth themselves: that is *its* incoherence.’ (1993: 457)<sup>11</sup>

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<sup>9</sup>Indeed, in a later paper Dummett uses considerations like this to ‘conclude that the celebrated “rule-following considerations” embody a huge mistake’ (1993: 460).

<sup>10</sup>At least Wittgenstein does accept this in places (1978: Part III, §55) – although there are also places where he appears to deny it (1978: Part IV, §30).

<sup>11</sup>Note, however, that in this paper, Dummett uses the label ‘full-blown internalism’

## 8. Conventions and Consequences

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The problem for modified conventionalism, on the other hand, is that it appeals to resources which it cannot account for, namely a convention-independent notion of consequence:

This account is entirely superficial and throws away all the advantages of conventionalism, since it leaves unexplained the status of the assertion that certain conventions have certain consequences. It appears that if we adopt the conventions registered by the axioms, together with those registered by the principles of inference, then we *must* adhere to the way of talking embodied in the theorem; and *this* necessity must be one imposed upon us, one that we meet with. It cannot itself express the adoption of a convention; the account leaves no room for any further such convention. (Dummett 1959: 328–9; cf. also 1993: 460)<sup>12</sup>

Note that the problem with modified conventionalism, as Dummett here presents it here, is apparently disconnected from Wittgenstein's rule-following considerations. Indeed, Dummett has yet to bring these into the discussion, and when he does bring them in, their role is, initially, to explain why anyone (Wittgenstein, he thinks) would ever embrace full-blooded conventionalism.

The dilemma, then, as it is initially formulated, is this: The adherent of the linguistic approach owes us an explanation of why it is that (HS) follows from (MP) and (CP). However, if they say, with the full-blooded conventionalist, that this is due to some *further convention*, then they undermine the very notion of proof which they are trying to account for; and if they say, with the modified conventionalist, that this is due to something *else*, then they have abandoned the idea that necessities are to be explained in terms of linguistic conventions.

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instead of 'full-blooded conventionalism' – presumably because he is writing for a volume on Putnam.

<sup>12</sup>This objection has been made by many, including Ewing (1940: 237; 1951: 34–4), Putnam (1979: 424), Stroud (1981: 234), Wright (1980: 348), Baker (1988: 235), and Juhl and Loomis (2010: 187).



## A Neglected Alternative

However, this dilemma is too quick in that it overlooks an alternative. To see this, let us consider how an exchange between Carnap (in his syntax period)<sup>13</sup> and Dummett on this topic might go: Continuing our above example, Carnap proposes a formal language which contains both (MP) and (CP), but not (HS) among the explicitly stated “rules of transformation”. Dummett asks whether Carnap nevertheless agrees that (HS) is a valid rule of the language, and Carnap replies that, yes, this is indeed so because its validity follows from the transformation rules – in particular, it follows from the stipulated validity of (MP) and (CP).

Dummett now objects that, in that case, Carnap has not succeeded in explaining logical truth/validity in terms of the conventionally adopted rules; instead, he is presupposing an *unexplained* notion of consequence, which leaves him unable to account for the convention-transcendent logical truth that *if* we accept (MP) and (CP), then it *follows* that we must accept (HS) as well.

Carnap is slightly puzzled by this accusation, and replies that he can very well explain this in terms of the conventionally adopted transformation rules of his language. He offers the following proof:

$$\text{PROOF HS: } \frac{\frac{\overline{\varphi} \quad (1) \quad \varphi \rightarrow \psi}{\psi} \quad \psi \rightarrow \chi}{\frac{\chi}{\varphi \rightarrow \chi} \quad (1)}$$

This proof shows, Carnap says, why it is that if you accept (MP) and (CP), then (HS) will hold as a derived rule. And crucially, he continues, this explanation appeals *only* to the conventionally adopted rules (MP) and (CP) themselves. There is no appeal either to a *further* convention or to facts about consequence which elude explanation. What exactly is it, Carnap then asks Dummett, that this leaves unexplained, thereby rendering the account ‘entirely superficial’?

<sup>13</sup>Although let us suppose that he was acquainted with Gentzen’s work.

Now, perhaps Dummett will point to some tacit assumption in this proof. It may be said, for instance, that in order to see this as a proof of (HS), we must make certain high-level assumptions about how proofs behave – notably that if we chain valid steps together, then the result is a valid proof.<sup>14</sup> However, even if Carnap were to accept this, this would only force him to retreat to the position that (HS) is a consequence of (MP), (CP) *and* some rule which licenses chaining together valid steps; it would not show that there is something fundamentally incoherent about the idea that (HS) might be explained as a consequence of some initial set of conventions.<sup>15</sup>

The point I am making, then, is not so much that this particular proof definitely settles the matter, but that the dilemma is too quick since it overlooks the possibility of giving an explanation of (HS) in terms of the very conventions that are said to have (HS) as a consequence. Thus, more generally, it is far from clear that we are forced to choose between appealing to either convention-transcendent principles or *further* conventions when attempting to explain how one necessity could be a consequence of an initial set of conventions: It might be that we can offer an explanation which appeals only to these initial conventions themselves.<sup>16</sup>

Of course, the objection is certainly fair insofar as it points out that the modified conventionalist cannot *merely* say, if asked why (HS) is valid, that this is a consequence of the conventions we have adopted. They cannot simply invoke the notion of consequence and then leave this entailment unexplained. However, there is a big difference between just saying that (HS) follows from our conventions, and giving a detailed explanation, in terms of these conventions, which shows *why* we are committed to (HS) if

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<sup>14</sup>The idea, essentially, would be that we need some rule corresponding to the ‘cut rule’ of the sequent calculus.

<sup>15</sup>As a matter of fact, however, I doubt (for reasons given in the previous chapter) that it makes sense to have a debate about something analogous to the cut rule if we understand the rules to be governing relations of assertibility in a natural language.

<sup>16</sup>This is, I believe, essentially the response favoured by Jonathan Bennett in a discussion of Dummett’s argument (1961: 19–20). Neil Tennant offers similar considerations against Quine’s regress argument in his (1987).

we commit to (MP) and (CP), and nothing has been said which rules out this latter option.

### Matters of Convention

It may be said that even if this is so, the modified conventionalist is still forced to concede that it is not a *matter of convention* that if we adopt (MP) and (CP), then (HS) too must be valid – for we cannot, according to the modified conventionalist, choose to adopt (MP) and (CP) and yet reject (HS).

This, however, is not really news. For one lesson from chapter 5 is that an adherent of the linguistic approach should refrain – *quite generally* – from saying that necessary truths hold as a “matter of convention” in this sense. Take, for instance, the claim that triangles are necessarily three-sided. Even the modified conventionalist should say that this is no matter of convention, and insist that *we could not choose to have it otherwise*. The idea, rather, is that we can offer an explanation of why this is necessary which appeals only to the linguistic rules we have conventionally adopted. And while saying that the rules have been *conventionally* adopted is to say that we could have adopted different rules, it is *not* to say that we could have decided to make it false (or contingently true) that triangles have three sides. The reason, which is presumably familiar by now, is that the conventions are meant to be constitutive of our concepts – which means that a change in the conventions would result in a change of subject.

This, I maintain, is also the line that the modified conventionalist should take with regard to the claim that (HS) is a consequence of (MP) and (CP). They should, that is, concede that it is no “matter of convention” that this is so (in the sense that we could not choose to have it otherwise), and instead maintain that the real question is whether we can provide an adequate explanation – in terms of the linguistic rules that we have conventionally adopted – of why this entailment holds. What I have been arguing in this section is that Dummett’s dilemma, in its initial “naïve” formulation, does

not foreclose the possibility of such an explanation.

### 8.3 The Rule-Following Considerations

However, it is clear that Dummett sees the real source of the problem with modified conventionalism to lie in Wittgenstein's rule-following considerations: 'I believe that whether one accepts Wittgenstein's account or rejects it, one could not after reflecting on it remain content with the standard view which I have called modified conventionalism.' (1959: 341)

In this section I ask, therefore, whether the rule-following considerations undermine the response to the dilemma that was just provided. Although this is a massive topic that cannot be fully dealt with in this context, I shall argue that a reasonable case can be made that these considerations do not undermine the linguistic approach – at least provided that we can learn to live with a thoroughly language-internal notion of normativity in this context.

#### Dummett's Dilemma Again

Let us return to (PROOF HS). As Dummett reads Wittgenstein, the problem concerns how the acceptance of the general rules (MP) and (CP) suffice to license *the particular use* made of those rules in the proof:

In order to follow the proof, we have to recognize various transitions as applications of the general rules of inference. Now even if these rules had been explicitly formulated at the start, and we had given our assent to them, our doing so would not in itself constitute recognition of each transition as a correct application of the rules. (Dummett 1959: 330)

For the lesson of Wittgenstein's remarks is precisely that:

there is nothing in our formulation of the axioms and of the rules of inference, and nothing in our minds when we accepted these before

the proof was given, which of itself shows whether we shall accept the proof or not (Dummett 1959: 330).

The full-blooded conventionalist is prepared to accept this conclusion and maintains that it is, in effect, a matter of convention whether we accept a given step in a proof as a genuine application of a general rule. But what can the modified conventionalist say here? It is because Dummett thinks that no good conventionalist answer is forthcoming that he sees Wittgenstein's remarks as undermining this proposal.

Note, in particular, that if the modified conventionalist maintains that it is simply a brute conceptual fact about the rules we have conventionally adopted what the correct applications are, then we should indeed agree with Dummett that this 'throws away all the advantages of conventionalism' (1959: 328). The reason is that this would essentially be a version of the conceptual Platonism we have already rejected (back in chapter 3) as inconsistent with the subjective-constitutive strategy. For if it is simply a brute fact about some particular rule how it is to be *correctly* applied on some particular occasion, then we should again have to ask how we get to be justified in believing that what we *think* is the correct way of applying it actually *is*. We are thus once again faced with a problem of inexplicable agreement.

The dilemma has thus re-emerged in the following form: What explains why a given application of a general convention is a *correct* application? If we hold this too to be a matter of convention, then we are giving up on the notion of proof as rationally compelling. But if we hold that this is explained by some non-conventional fact  $F$ , then we are left with the question 'and how do we get to know that  $F$  is the case?' – the kind of question which the linguistic approach was meant to get rid of.

The topic of rule-following is, of course, a vast one which has inspired a huge literature, and it would be foolish to think that it could be adequately treated in the present context. I shall be content with arguing: i) that the coherence of the dilemma just stated can be challenged, and ii) that it *may*

be possible for an adherent of the linguistic approach to hang on to an adequate notion of proof even in the face of the rule-following considerations. I concede, however, that this is a complicated issue deserving of a fuller treatment than I can offer here.

### Language-Internal Conceptions of Normativity

It is a staple of the linguistic approach that the notion of logical correctness only makes sense relative to a given language (linguistic/logical framework). Here is Alan Richardson's characterisation of Carnap's view, according to which not only logical correctness, but "theoretical" normativity more generally is a language-relative matter:

The very notion of a theoretical reason, therefore, makes sense only internal to a logical framework. Thus, there is no realm of theoretical reasons that can be appealed to in advance of the adoption of a logical system. (Richardson 2007: 300)<sup>17</sup>

There are, however, two things that can be meant by saying that the notion of logical correctness only makes sense relative to a given language. First, it may be a mere insistence that normative-logical claims must be put in the form of conditionals where the antecedent specifies the relevant linguistic context. This would involve insisting, for instance, that we shouldn't say that modus ponens is a valid rule of inference *as such*, but, rather, that *if* such and such linguistic conventions have been adopted, *then* modus ponens is a valid rule of inference.

"Relativisms" like this, however, do not really avoid the notion of an absolute standard of correctness. Here is Putnam making the point:

When one first encounters relativism, the idea *seems* simple enough. The idea, in a natural first formulation is that every person (or, in a modern 'sociological' formulation, every culture, or sometimes every

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<sup>17</sup>Cf. also Friedman (1995: 313) and Creath (2007: §I).

‘discourse’) has his (its) own views, standards, presuppositions, and that truth (and also justification) are relative to *these*. One takes it for granted, of course, that whether  $X$  is true (or justified) relative to these is *itself* something ‘absolute’. (1981: 121)

The point, of course, is that *merely* conditionalising doesn’t get rid of an absolute (here: language-transcendent) standard of correctness since the conditionals themselves would constitute precisely such a standard.

I shall call this the ‘superficially language-relative conception of normativity’, in contrast with a *thoroughly* language-relative one. This latter idea is considerably harder to articulate, but can be approached via the Wittgensteinian theme that justification must come to an end *within the context of a shared language*. On this conception, saying that the notion of logical correctness only makes sense relative to a language is thus to say something about when a *justificatory* project is possible at all. The following remark by Wittgenstein is suggestive:<sup>18</sup>

‘I write the number “16” here *because* it says “ $x^2$ ” there, and “64” here because it says  $x^3$  there.’ That is what every justification looks like. In a certain sense it takes us no further. But indeed it can’t take us *further* i.e. into the realm of metalogic. (The difficulty here is: in not trying to justify what admits of no justification.) (1974: Part I, §61)

Now, we can, presumably, go *a bit* further with the justification than Wittgenstein allows: We can break down the calculations (spell out the exponentiations in terms of multiplications, carry out the multiplications as explicitly as possible etc.). However, the more important point is that if, now, someone contests these calculation (in a way which does not amount to pointing out something we recognise as a mistake on our part), then we can do no more than say that this is how we’ve defined exponentiation, multiplication and so on. And if *this* is contested (in the manner of the

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<sup>18</sup>The context is a table which is to be filled in with various powers of various numbers.

rule-following sceptic), then the attempt at justification must give way to *instruction*.<sup>19</sup>

On a thoroughly language-internal conception, then, the idea is not that claims about logical correctness must be conditionalised, but that there are no (theoretical) normative constraints – including conditional ones – outside the context of a shared language. If the assumption of a shared language breaks down – if, say, it transpires that someone is using ‘+’ in a *quus*-like fashion – then there can be no question of trying to justify one way of using the expression over the other.

Arguably, we find this attitude in a reply Carnap made to a criticism by E. W. Beth (1963). This criticism was based, in part, on the idea (closely related to the rule-following considerations) that the syntactical rules which Carnap articulates in *LSL* don’t suffice to settle the correct use of the relevant expressions since these rules do not by themselves rule out deviant interpretations. Here is the relevant passage of Carnap’s reply:

Since the metalanguage *ML* serves as a means of communication between author and reader or among participants in a discussion, I always presupposed, both in syntax and semantics, that a fixed interpretation of *ML*, which is shared by all participants, is given. This interpretation is usually not formulated explicitly; but since *ML* uses English words, it is assumed that these words are understood in their ordinary senses. The necessity of this presupposition of a common interpreted metalanguage seems to me obvious. (1963b: 929)

Carnap is here partly making a fairly trivial point about communication: namely, that successful communication presupposes that the parties (to a reasonable extent) share a common language. However, we can also connect this with Carnap’s language-internal conception of normativity. The point would then be that it is only within the context of the metalanguage that

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<sup>19</sup>Although a certain kind of justification is still possible: If both parties agree to regard the linguistic community as an authority, then a justification in terms of the verdicts of others could be provided.



we can think of the syntactical rules which Carnap articulates in *LSL* as yielding any normative constraints. Even conditional claims like ‘if you adopt the rules of Language I, you must accept so-and-so’ do not register normative constraints which can be utilised to justify a particular way of “going on” *unless we can take the assumption of a shared metalanguage for granted*.

This is not to say that such conditional claims are *wrong* unless they are qualified. We are not mistaken when we say that if someone (*anyone*) uses ‘+’ to mean *plus*, they ought to say that  $68 + 57 = 125$ . The point, rather, is that the constraints which such claims purport to register are entirely devoid of normative/critical potential outside the context of a shared language.

To see why this is, we may bring in the rule-following considerations. For the lesson of these Wittgensteinian remarks (as I read them anyway) is that there is no appropriate way of discharging the antecedent in a claim like ‘if someone uses ‘+’ to mean *plus*, they ought to say that  $68 + 57 = 125$ ’. We have no *neutral* criterion for saying that someone (including ourselves) is using an expression with a particular meaning or, equivalently, have adopted a particular linguistic convention. We cannot, for instance, point to anything concerning how the expression was used in the past.

By saying that we have no *neutral* criterion I mean to draw attention to the fact that our criterion here is simply whether someone goes on to use expressions in what we take to be the appropriate ways – or, if not, are at least responding appropriately to correction. This is, I believe, the point Wittgenstein makes in the following famous passage:

For what we thereby show is that there is a way of grasping a rule which is *not* an interpretation, but which, from case to case of application, is exhibited in what we call “following the rule” and “going against in”. (Wittgenstein 1953: §201)

It is clear, however, that if our criterion for whether someone has adopted (or ‘grasped’) the *plus* rule is that they go on to apply ‘+’ in the expected ways, then we could not use this criterion to discharge the antecedent in

the above conditional and then use this to criticise someone who *deviates* from us. For by our own lights we shall then *not* be dealing with someone who has adopted the rule in question.

Against this background, then, we may regard the rule-following considerations as pushing us from a superficially language-relative conception of normativity to a thoroughly language-relative one. The conditionals arrived at on the superficial conception turn out to be “normatively inert” outside the context of a shared language because – as the rule-following considerations teach us – we cannot discharge the antecedent in a way which would allow us to appeal to these conditionals in cases where the assumption of a shared language breaks down. we cannot, therefore, use them to *justify* one way of ‘going on’ over another.

### Rejecting the Dilemma

Suppose, then, that the adherent of the linguistic approach accepts the lessons of the rule-following considerations and consciously accepts a thoroughly language-internal conception of normativity. Where does this leave them with the dilemma we started out with? We may start by noting that the distinction between modified and full-blooded begins to seem somewhat problematic. For it really isn’t obvious whether we should say that this person advocates the modified or the full-blooded variant.

On the one hand, we cannot expect this person to say, with the full-blooded conventionalist, that it is possible to adopt (MP) and (CP), and then decide, by a further convention, whether the applications that are made of these rules in (PROOF HS) are correct applications. That, after all, would require some *independent* grasp on what adopting (MP) and (CP) amounts to. But we said that one lesson of the rule-following considerations is that there is nothing which establishes that a particular linguistic rule/convention has been adopted except simply going on to apply the relevant expressions in the expected ways. Thus, if we look at (PROOF HS) and come to the verdict that the conditional is indeed being used in accor-

dance with (MP) and (CP) here, then there is no room for the idea that we could have adopted these conventions without allowing these applications.

And yet it would clearly be misleading to suggest that we have anything like a straightforward vindication of modified conventionalism. There the idea was that we could *impose* normative constraints on ourselves by doing something called ‘adopting linguistic conventions’, which would then commit us to go to apply linguistic expressions in certain ways. This idea, however, is incompatible with a thoroughly language-internal conception of normativity, since it would mean acknowledging language-transcendent (albeit conditional) normative constraints of the form ‘if you have done so-and-so, then this expression should be used thus-and-so’.

Neither modified nor full-blooded conventionalism, then, adequately captures the present position, and the reason is that they both rely on an assumption which someone who has accepted the lessons of the rule-following considerations and adopted a thoroughly language-internal conception of normativity rejects. Both positions assume, that is, that we can *first* form a conception of what it is to adopt (MP) and (CP), and *then* ask what (if anything) has thereby been settled with regard to how to apply these rules.

This, however, is the picture which is exposed as mistaken by the rule-following considerations. We cannot point to anything which would establish that someone has adopted a particular rule (say the addition rule rather than the quaddition rule). All we can do is consider whether they are, in actual cases, using the expression in what we would regard as the appropriate way.

The response to the dilemma, then, is to reject the legitimacy of the perspective from which it is posed: We cannot make a distinction between the “act” of adopting a linguistic convention and the *practice* of applying it in the ways we take to be appropriate. We cannot therefore ask whether the (alleged) gap between a general convention and its applications is “bridged” by a further convention (full-blooded conventionalism) of something else (modified conventionalism).

## Proof and Normativity

Rejecting the dilemma, however, is perhaps not enough. For we should still ask whether the present position manages to avoid the undesirable consequences of both modified and full-blooded conventionalism. These were: i) that modified conventionalism brings in an external standard of correctness, and ii) that full-blooded conventionalism undermines our conception of proof as rationally compelling.

Now, there is, I think, little grounds for the charge that a thoroughly language-internal conception of normativity shares the vices of modified conventionalism. Conversely, however, the danger of undermining the notion of proof as normatively compelling seems highly present. Indeed a natural suggestion is that the position we have ended up with is essentially one that allows us to *talk* as a modified conventionalist (by denying that we could adopt a linguistic rule and *then* settle how to apply it by a further convention) but which has as all the normativity-undermining consequences of the full-blooded variant.

There is certainly some justification to this criticism and we should be clear about what is conceded: Accepting a thoroughly language-internal conception of normativity means acknowledging that accepting a step in a proof is not a matter of being rationally compelled to do so by what has gone before (the training one has received, how expressions have been used in the past etc.). This after all would be to admit of language-transcendent (albeit conditional) normative constraints along the lines of ‘if you have received such-and-such training, then you must acknowledge this step as valid’. *Within* the context of a shared language, we may take it for granted that accepting a given step is the correct thing to do, but if someone contests this (in the manner of the rule-following sceptic), then there is no justification which we could offer.

There is, therefore, indeed a sense in which we are free, at any given stage, to reject or accept a step in a proof: There is no language-transcendent standard which determines which option is the correct one

– even relative to how the relevant expressions have been used in the past etc.

This admittedly sounds bad, given that we want to hang on to a notion of proof which is rationally constrained in certain ways. Certainly, we must avoid a kind of relativism about proofs, according to which there is no fact of the matter as to whether something can be proved or not, and an invalid proof of something could be made valid simply by accepting it. I shall close this chapter by outlining a potential strategy for avoiding this conclusion which is open to someone who advocates both the linguistic approach and a thoroughly language-internal conception of normativity.

Consider the following example: Suppose someone claims to have disproved Goldbach’s conjecture, but that it turns out that their proof relies on using ‘+’ in accordance with Kripke’s *quus* rule (Kripke 1982: 9).<sup>20</sup> The natural response is of course to reject the proof on the grounds that this is not the correct way of adding. But let us suppose that the deviant mathematician then invokes a dose of rule-following scepticism: There is nothing we can point to, they say, which establishes that their proof is not in accordance with how ‘+’ has been used so far.

We reply that, be that as it may, it does not change the fact that we have, here and now, a conception of how ‘+’ is to be used and that the proof in question does not conform with this conception. The deviant mathematician objects that this is just a matter of us being *phenomenologically* compelled to treat ‘+’ in a certain way. It doesn’t establish that there is anything *wrong* with their proof.

Here is how I think someone who advocates a thoroughly language-internal conception of normativity should reply: There is indeed nothing wrong with the proof as such, and there is no objective standard which could be used to show that the proof is mistaken relative to how ‘+’ has been used in the past or the like. *However*, the crucial point is that our present conception of how ‘+’ is to be used (the source of the “phenomeno-

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<sup>20</sup>The conjecture is then easily disproved by showing that even numbers greater than 112 cannot be written as sums of two numbers at all, let alone primes.

logical compulsion”) *also* informs our conception of what Goldbach’s conjecture states (since it involves the notion of addition). We are therefore perfectly within our rights to say that although we cannot point to anything which establishes that the proof is mistaken (except our own verdicts to the contrary), it does not prove Goldbach’s conjecture as we (here and now – informed by our present conception of how to add) understand it. *It is simply a proof of something else.*

Although we may of course recognise the deviant mathematician’s right to *call* what they have disproved “Goldbach’s conjecture”, this is not particularly troublesome. For we retain the right to look at Goldbach’s conjecture – as we understand it – and deny that *it* could be disproved in the manner suggested.

I propose, then, that an advocate of the thoroughly language-internal conception should aim to constrain the notion of a proof, not by appealing to some objective fact which is meant to determine in advance whether a given step in a proof should or should not be accepted, but by appealing to the relationship between one’s conception of how the relevant expressions are to be used and one’s conception of what is to be (or has been) proved. We may then say that the notion of proof is constrained by the fact that if one decides to accept some particular step rather than reject it or vice versa, then this will change what has or has not been proved in the process. As Wittgenstein puts it: ‘In order to see *what* has been proved, look at the proof.’ (1978: Appendix III, §17)

The crucial point is that this is compatible with conceding that we cannot *justify* our conception of how the expressions are to be used (that we cannot justify, for instance, our insistence that ‘+’ is not governed by the *quus* rule). We don’t need such a justification as long as we can say that it is this conception which constrains what counts as a proof of Goldbach’s conjecture by our standards.

There is of course a lot more to say about this issue, and the strategy I have offered for rationally constraining the notion of a proof can certainly be

challenged. For my present purposes, however, I am content to have argued that such a strategy *is* available, and I concede that whether the linguistic approach allows for a satisfactory notion of proof depends on whether this (or some other) strategy can be worked out and defended in more detail.

Let me close the discussion of these complex and difficult issues by bringing it explicitly back to the linguistic approach and the question of whether our linguistic conventions can have consequences. The view we have ended up with answers this affirmatively. Adopting (MP) and (CP), for instance, gets you (HS) for free. If it is asked what explains this, the proper answer (given in section 8.2 above) is to give a proof of (HS) which relies only on (MP) and (CP) – a proof, that is, which appeals neither to a further convention, nor “something else”. If, now, it is asked why adopting the *general* conventions (MP) and (CP) should force us to recognise this *particular* proof, we can do no more than say that accepting proofs like this is the criterion for having adopted those conventions. And if someone disagrees, then we should simply admit that no further *justification* can be offered; all we can do is try to teach them our ways (if they are interested).





## First Principles

In this chapter I consider the worry that to get the linguistic approach off the ground in the first place, we need to assume a non-conventional starting-point, and, in particular, that we need to take *some logic* for granted in order to explain any necessities or validities in the manner suggested.

I begin (9.1) by considering how this affects some typical passages from the logical positivists. Then, in section 9.2, I defuse the worry that this problem is obviously inescapable because any attempt at explanation must rely on some reasoning and hence some logic. I do so by drawing on Dummett's distinction between gross and pragmatic circularity.

A standard suggestion for getting the linguistic approach off the ground is to appeal to implicit definition. In section 9.3 I briefly explain why I am not convinced that this is the quick fix it is sometimes made out to be. Instead, I shall focus on the idea that our linguistic conventions can interact in ways such that certain sentences come out as true no matter what is the case.

In section 9.4 I articulate and discuss a dilemma for this view: If 'no matter what is the case' is a quantification over *possibilities*, then we have smuggled in what we ought to be explaining, but if *impossible* cases are allowed, then we will not be able to explain any necessities at all. I consider, but reject, the suggestion that the notion of a possible case could itself be "conventionalised", before I suggest my preferred way forwards: The

adherent of the linguistic approach should aim to explain how a sentence gets to hold in every possible case *by* explaining why no scenario in which the sentence doesn't hold gets to count as a possible case.

In the remainder of the chapter I offer two explanations aimed to illustrate this idea and act as “proofs of concept”: an explanation of the validity of conjunction elimination (9.5), and an explanation of (a version of) the law of non-contradiction (9.6).

## 9.1 The Problem

Consider the following passage from Ayer's *Language, Truth and Logic*:

We see, then, that there is nothing mysterious about the apodeictic certainty of logic and mathematics. Our knowledge that no observation can ever confute the proposition ‘ $7 + 5 = 12$ ’ depends simply on the fact that the symbolic expression ‘ $7 + 5$ ’ is synonymous with ‘12,’ just as our knowledge that every oculist is an eye-doctor depends on the fact that the symbol ‘eye-doctor’ is synonymous with ‘oculist.’ And the same explanation holds good for every other *a priori* truth. (1936a: 114–5)<sup>1</sup>

There are – quite clearly – plenty of problems with this passage. However, the objection most relevant to our present purposes tends to be associated with Quine, who pointed out that *definition* – by which he understood the introduction of one expression as an abbreviation for another – cannot explain necessary truth “from scratch”:

Considered in isolation from all doctrine, including logic, a definition is incapable of grounding the most trivial statement; even ‘ $\tan \pi = \frac{\sin \pi}{\cos \pi}$ ’ is a definitional transformation of an antecedent self-identity, rather than a spontaneous consequence of the definition.’ (1936: 91–2; cf. also 1960a: 361 and Lycan 1994: 270.)

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<sup>1</sup>Cf. also Hahn (1933b: 29).

Applied to Ayer's example, the point is that the synonymy in question does not suffice to explain the necessity of 'every oculist is an eye-doctor' since the necessity of 'every eye-doctor is an eye-doctor' must be assumed and remains unexplained. The general lesson is that synonymy-based explanations are incapable of getting the linguistic approach off the ground.<sup>2</sup>

This by itself, though, is not much on a problem. It is quite clear, after all, that not all linguistic conventions take this form, and so it remains open to an adherent of the linguistic approach to maintain that we must appeal to rules of a different kind to get things going. However, alternative suggestions seem to give rise to analogous problems. Consider Hans Hahn's way of accounting for (an instance of) the laws of non-contradiction (LNC) and excluded middle (LEM):

Take for instance objects which can be assigned a colour. We learn – by training, as I should like to put it – to assign the designation 'red' to some of these objects, and we make an agreement to assign the designation 'not red' to any others. On the basis of this agreement we can now state the following proposition with absolute certainty: None of these objects is assigned both the designation 'red' and the designation 'not red', which is usually expressed briefly as follows: No object is both red and not red. And since we have made the agreement to assign the designation 'red' to some of these objects and the designation 'not red' to *any* others, we can also state the following proposition with absolute certainty: Every one of these objects is assigned either the designation 'red' or the designation 'not red', which is usually expressed briefly as follows: Every object is either red or not red. (1933b: 29–30)<sup>3</sup>

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<sup>2</sup>Interestingly, Ayer appears to implicitly concede that an antecedent principle is needed when he mentions Poincaré's objection that 'if everything is to proceed from the principle of identity, everything must be reducible to it' (1936a: 115; cf. Poincaré 1902: 1–2). However, he never discusses the source of this principle.

<sup>3</sup>A similar but more rigorous attempt at explaining the law of excluded middle can be found in Carnap (1947: 9–11). Cf. also Carnap (1942: 79) for a slightly different approach. The shortcomings are largely the same however.

Before considering the problems with this passage, it is worth pointing out that Hahn's idea is that the logical necessities arise from how the *semantic* application-conditions of 'red' and 'not red' are *related* through the rules which we lay down. This is noteworthy because this idea is rarely considered in much detail by critics of the linguistic approach who tend to focus on truth-stipulation or conventions establishing purely syntactic relationships.

Now, Hahn's claim that we learn to apply 'red' to certain objects is potentially misleading, since it suggests that the use of 'red' is taught by *enumerating* the objects to which it correctly applies. That, of course, is incorrect: We don't learn which objects can be designated 'red'; we learn what an object *must be like* in order for 'red' to be applicable. We learn, that is, to associate a certain – presumably vague – criterion with 'red'.

Once we acknowledge this, however, Hahn's explanations amount to little more than applying the relevant logical principles in the metalanguage. Thus, the explanation of why LEM holds clearly relies on the assumption that the criterion determinately either applies or fails to apply in any given case. Correspondingly, in the case of non-contradiction, it is simply assumed that we could never find that the criterion both is and is not satisfied.<sup>4</sup>

Again, then, we see that the proposed explanations do not get going unless some basic principles of logic are taken for granted. Indeed, in this case the problem is deepened by the fact that the principles taken for granted are the very principles that are supposed to be explained.

### 9.2 Rule-Circular Explanations

Now, it might well be thought that this predicament is unavoidable and obviously so. After all, the task of explaining something without employing *any* kind of reasoning is clearly insurmountable. And where there is reasoning there is presumably – hopefully – logic. Thus it seems plain that there can be no explanation at all which does not take some logic for granted.

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<sup>4</sup>Indeed, even if we did proceed via enumeration, we should ask: Why are we confident that every relevant object was accounted for, and that no object was treated twice?

As Henry Sheffer put it, we are in a ‘logocentric predicament’: ‘*In order to give an account of logic, we must presuppose and employ logic.*’ (1926: 228)

This is perfectly true. So, if there is to be any hope for the linguistic approach – or, indeed, any other attempt to account for the basis of logical truth and validity – it must be possible to appeal to logic – in the course of explaining logic – without thereby rendering the explanation viciously circular.

Fortunately, an influential (though by no means uncontroversial) suggestion by Dummett opens up precisely this possibility.<sup>5</sup> We must distinguish, Dummett thinks, between *gross* circularity, which ‘consists of including the conclusion to be reached among the initial premises of the argument’ (1991: 202), and *pragmatic* circularity, where at least one of the inferential steps in the argument relies on the logical law whose validity we are attempting to justify (1991: 202). While gross circularity is always bad, pragmatic circularity *need* not be so. If the argument is supposed to be *suasive* – that is, capable of convincing someone who doubts the law in question – then pragmatic circularity must indeed be avoided. However, if the argument is merely intended to *explain* the validity of law – in a context where no one is doubting its correctness – then ‘pragmatic circularity need do it no harm.’ (1991: 202)<sup>6</sup>

Why is this so? The reason given by Dummett is that gross circularity is always bad because ‘if one sets oneself to derive a conclusion from a set of premisses that contains that conclusion, one cannot fail; and succeeding at a task at which one cannot fail neither proves anything nor explains anything.’ (1991: 202) ‘The mere occurrence of a pragmatic circularity’, on the other hand, ‘does not guarantee success’ (1991: 202).

I agree with Dummett that pragmatic circularity might be harmless in

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<sup>5</sup>Dummett’s suggestion has inspired a lot of literature on the subject – and continues to do so. Relatively early discussions include Friedman (1979: 372–3) and Haack (1982), but more recent discussions tend to focus on Boghossian’s use of these ideas (notably in his 2000 and 2001). Such recent discussions include Wright (2001; 2004), Hale (2002a), Ebert (2005), Tennant (2005), Giaquinto (2008), and Dogramaci (2010).

<sup>6</sup>Cf. also Dummett (1973: 9–10).

a purely explanatory context. However, some of Dummett's remarks are potentially misleading. It is natural to read the distinction between gross and pragmatic circularity as a distinction between appealing to the logical law we are trying to explain *as an axiom*, and appealing to it *as a rule of inference*. This should give us some pause since this latter distinction is quite feeble, in the sense that it is typically straightforward to swap axioms for inference rules without diminishing the strength of our proof system. We might suspect, therefore, that if gross circularity is going to "guarantee success", then exchanging this circularity for the pragmatic variant won't affect this.

However, the crucial distinction is not between appealing to axioms and appealing to rules of inference, but between appealing to what we are trying to explain in the "explanans" and appealing to it in the course of transitioning from the explanans to the explanandum. This is important because, as we shall see, a rule of inference might play either of these roles, and if we appeal to a rule of inference in the explanans while attempting to explain the validity of that very rule, we will indeed have a case of gross circularity.

Consider the following "explanation" of the validity of modus ponens:

Take two arbitrary sentences ' $p$ ' and ' $q$ ' and assume that ' $p$ ' and ' $p \rightarrow q$ ' are both true. Using the rule of modus ponens, we may then conclude that ' $q$ ' is true. We have thus shown, for arbitrary sentences ' $p$ ' and ' $q$ ', that if ' $p$ ' and ' $p \rightarrow q$ ' are both true, then so is ' $q$ '. This explains the validity of modus ponens.

This explanation should be regarded as *grossly* circular. But this is not entirely clear from Dummett's discussion since the use of modus ponens here could reasonably be described as confined to taking a step in accordance with the rule. However, I would suggest that the true nature of the appeal to modus ponens here becomes clear if we ask specifically for the explanans.

We ask, that is: what does this explanation say about *why* modus ponens is valid?

It then emerges that what we have here is an explanation where the *explanans itself* is a proof.<sup>7</sup> Taking a birds-eye view, the form of the explanation is, essentially: modus ponens is valid because, without making any assumptions about the relevant sentences, it is provable that if ‘ $p \rightarrow q$ ’ and ‘ $p$ ’ are both true, then ‘ $q$ ’ is true as well. Since, now, the claim is that modus ponens is valid because it is provable in a certain way, the resources needed for the purposes of the proof must be regarded as part of the explanans. And since the proof in question relies on using modus ponens, this renders, I maintain, the explanation grossly circular; it illegitimately offers the validity of modus ponens as part of the reason why modus ponens is valid.

We may contrast this with a different attempt to explain the validity of modus ponens:<sup>8</sup>

- (1) If modus ponens is in harmony with the introduction rule for ‘ $\rightarrow$ ’, then modus ponens is valid.
  - (2) Modus ponens is in harmony with the introduction rule for ‘ $\rightarrow$ ’.
- ( $\therefore$ ) Modus ponens is valid.

Here too we are using modus ponens, but whatever we may think of this explanation in general, it is not, I maintain, grossly circular, and what sets it apart from the explanation considered above is that we are not here appealing to a proof using modus ponens *in the explanans*. Indeed, the explanans here – that is (1) and (2) – does not consist of a proof at all. The claim is not that modus ponens is valid because it is provable; it is that modus ponens is valid because it is in harmony with the relevant introduction rule. It is true that explanation *as a whole* takes the form of a proof which uses modus ponens – herein lies the pragmatic circularity –

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<sup>7</sup>Another example of the same: citing Euclid’s proof to explain why there is no highest prime number.

<sup>8</sup>I am not offering this as my preferred explanation, but as an illustration.

but the point I am making is that this is crucially different from presenting a proof which uses modus ponens as the reason why modus ponens is valid.

I maintain, therefore, that pragmatically circular explanations of logical principles are admissible, and that the way to tell whether an explanation is grossly or merely pragmatically circular is to get clear about what is playing the role of explanans. It would be too quick, therefore, to dismiss any attempt – the linguistic approach included – to explain logical necessity and validity in general on the grounds that the explanations must use logic.

Clearly, though, this realisation falls massively short of showing that the linguistic approach manages to avoid our present worries. The explanations offered by Ayer and Hahn were all *grossly* circular, and we may reasonably worry that it is this lesson which generalises.

### 9.3 Implicit Definition

One widespread strategy, among those advocating the linguistic approach – or at least *elements* of the linguistic approach – for avoiding this conclusion is to maintain, essentially, that we should not be trying to show that valid inferences and basic logical truths are explained by more fundamental linguistic rules; instead, we should let the linguistic rules themselves take the form of what we are trying to explain.

We may distinguish two versions of this idea: Firstly, there is the suggestion, typically traced to Hilbert, that we can endow expressions with meaning by stipulating that certain sentences containing those expressions are to be true.<sup>9</sup> Secondly, there is the suggestion, typically traced to Gentzen, that we can do so by stipulating that certain inferences – introduction and elimination rules – are to hold.<sup>10</sup>

As to the latter suggestion – which is, I think, the more plausible one as far as logic is concerned – I maintain, in accordance with what was said in chapter 7, that when properly spelt out, it is not clear that it is

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<sup>9</sup>We encountered this idea above in the context of Quine's regress argument.

<sup>10</sup>This idea figured prominently in chapter 7 and parts of chapter 8.



essentially different from the suggestion that we endow logical constants with meaning by laying down conditions for correctly asserting sentences where the relevant constant is the main logical connective. This, however, is the strategy I shall be pursuing below, so I will set this to one side for the moment.

The idea that we can endow logical constants with meaning by directly stipulating that certain sentences containing those constants are to be assertible no matter what faces a difficulty that raised its head in chapter 4. Suppose that it is stipulated that ‘snow is white or snow is not white’ is to be assertible. Clearly, this cannot be the *sole* convention governing the relevant vocabulary; there must be other conventions governing both when these particular disjuncts are assertible and when disjunctions in general are assertible.

If so, however, then it would presumably be an option to retain these latter conventions, but drop the convention that ‘snow is white or snow is not white’ is to be assertible no matter what. And the problem, now, is that once we contemplate this option, it seems quite clear that the sentence remains assertible no matter what even if the convention that stipulates that this is to be so is dropped, which suggests that the hypothesis that there is a convention to this effect was explanatorily idle to begin with.

It is worth noting that this problem also affects the suggestion that implicit definition allows us to account for cases of (apparent) necessity that are recalcitrant to linguistic explanation. It is sometimes suggested that the necessity of sentences like ‘nothing can be green and yellow all over’ is due to this being a (partial) implicit definition of the relevant colour predicates (cf. e.g. Glock (2003a: 156)). But since there are plainly also other conventions governing the use of ‘green’ and ‘yellow’, this would suggest that if we were *just* operating with these latter conventions, then we might give up the above claim, and this does not appear to be the case.

## 9.4 Assertibility in All Cases

Since I am not convinced that appealing to implicit definition is going to advance matters in this context, I shall revert to focusing on linguistic rules cast in a more perspicuous form – specifically in terms of assertibility- or application-conditions, pretty much following in footsteps of Hahn and Carnap in his semantic phase. The overarching idea here is that these rules explain necessity by explaining how certain sentences get classified as assertible *no matter what is the case*, and that they explain validity by explaining why certain sentences are guaranteed to be assertible, given that other sentences are assertible.

### A Dilemma about Cases

However, talk about ‘assertibility no matter what is the case’ suggests a more principled reason (compared to our misgivings over the specifics of Ayer’s and Hahn’s explanations) why we cannot ‘get the linguistic approach off the ground’ without making a grossly circular appeal to logic. The worry I have in mind is inherent in the following passage by Timothy Williamson:

‘All furze is furze,’ unlike many logical truths, is obvious. That does not justify the idea that it imposes *no* constraint on the world, rather than one which, by logic, we easily know to be met [...] What case does the constraint exclude? That not all furze is furze, of course. To complain that ‘Not all furze is furze’ does not express a genuine case is to argue in a circle. For it is to assume that a genuine constraint must exclude some logically consistent case. (Williamson 2007: 65)

We can put the objection in the form of a dilemma: either the notion of a case is understood in some completely unrestricted way (so as to include what we would call *impossible* cases), in which case there are no prospects for explaining logical necessity via this route, or we say that we are interested only in the *logically possible* cases, in which case we are plainly presupposing exactly what we were supposed to be explaining.

Thus, it may be said, it is indeed inevitable that we must appeal to some logic in order to get the explanations of the linguistic approach going in the first place: We need logic to set boundaries to the notion of a ‘case’ *before* the linguistic conventions can be used to explain how some sentences get to be assertible in all cases.

### Conventionalising Cases

One suggestion might be to maintain that the the notion of a case could itself be settled by something like convention. Arguably, Carnap’s notion of a state-description could be understood along such lines. As mentioned in chapter 4, one of Carnap’s aim in *Meaning and Necessity* was to articulate a notion of L-truth which explicated the notion of necessity, and which did justice to the idea that a sentence in a language  $S$  is necessary iff ‘its truth can be established on the basis of the semantical rules of the system  $S$  alone, without any reference to (extra-linguistic) facts.’ (1947: 10) His strategy was to define L-truth as truth in all ‘state-descriptions’ and argue that the semantical rules suffice in order to establish that certain sentences have this property.<sup>11</sup> A state-description, relative to a language  $S_1$  is defined as ‘a class of sentences in  $S_1$  which contains for every atomic sentence either this sentence or its negation, but not both, and no other sentences’ (1947: 9).

Although Carnap does justify his notion of a state-description in terms of it ‘obviously [giving] a complete description of a possible state of the universe of individuals with respect to all properties and relations expressed by predicates of the system’ (1947: 9), it is difficult to imagine that he would not have tolerated alternative notions – say intuitionistic state-descriptions based on the idea of a Kripke model – giving rise to different sets of L-truths. Given an interpretation along these lines, we could say that Carnap’s notion

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<sup>11</sup>More accurately, he is content with providing an example illustrating the idea (cf. 1947: 11).

– as an explication of the notion of a ‘possible case’ – is laid down by something like convention.<sup>12</sup>

Unfortunately, however, this just moves the bulge in the carpet with regard to the dilemma. For now it becomes unclear why we should think that the actual world can be modelled as a possible case *in this sense*. And it is crucial that we are confident that this holds, since if it doesn’t, we shall not be able to infer that a sentence is true from the fact that it is L-true. In more usual terminology: we lose the inference from necessity to truth (and from truth to possibility).<sup>13</sup>

Conventionalising the notion of a possible case, then, is not a promising strategy. It is worth noting, moreover, that Carnap’s position here is somewhat unsatisfactory even if we reject this conventionalist interpretation and fall back on his claim that, essentially, it is obvious that there will be a state-description corresponding to the actual state of the universe. For even if this *is* obvious, it is quite misleading to suggest that he has given us a definition of L-truth which conforms to the constraint that such truth can be established on the basis of the semantic rules *alone*. After all, this allegedly obvious fact is hardly itself a semantic rule, and this assumption is doing a lot of work here.

### Language Internal Constraints

How, then, should we respond to the above dilemma? Here is the strategy I shall be pursuing: It is quite correct that the aim should be to explain why certain sentences are assertible in every *possible* case. However, the explanation need not work by assuming the notion of a possible case as *antecedently given*. Rather, we should try to explain how a sentence gets to hold in every possible case *by* explaining why certain ways of describing are ruled out by how we have set up the language. The thought would be that

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<sup>12</sup>As Anthony Quinton remarked in a review of the book: ‘Carnap sees himself as proposing conventions rather than as asserting truths.’ (1959: 201)

<sup>13</sup>A similar problem might affect some of the “neo-conventionalist” treatments of modality mentioned in footnote 21 of chapter 4.

such *language-internal* considerations relating to our linguistic conventions suffice to explain why certain sentences are guaranteed to be assertible (thus circumscribing the notion of a possible case), and how the assertibility of one (or more) sentence(s) might guarantee that of another.

What the dilemma teaches us is that any attempt to start with some notion of a ‘case’ and then explain, using the linguistic conventions, why certain sentences hold in all cases is, in the present context, a non-starter. What we should be doing is starting with our linguistic practices and try to explain how, for some sentences, these make no room for the possibility that it might be correct to assert them. Thus, certain “cases” are ruled out, not because they are antecedently classified as impossible, but because we find that our linguistic conventions don’t allow for the world to be described in certain ways.

The question, of course, is whether the adherent of the linguistic approach can point to any mechanisms which could have this effect. Or, put differently: whether we can explain how the assertibility of a sentence could be *guaranteed* by our linguistic conventions without relying on an antecedently constrained notion of a possible case. I think we can, and in the remainder of this chapter I shall aim to illustrate this idea and argue for its coherence via two “case studies”: an explanation of the validity of conjunction elimination and an explanation of the necessity of (one version of) the law of non-contradiction.

## 9.5 Case Study I: Conjunction Elimination

An initial attempt at a linguistic explanation of conjunction elimination might go as follows: We start by articulating a linguistic convention claimed to govern the assertibility of conjunctive statements:

(CONJUNCTION). ‘ $\varphi$  and  $\psi$ ’ can be correctly asserted *iff* ‘ $\varphi$ ’ can be correctly asserted and ‘ $\psi$ ’ can be correctly asserted.

We then say that, against the background of this linguistic convention, we know that any situation in which ‘ $\varphi$  and  $\psi$ ’ can be asserted, will be a situation in which ‘ $\varphi$ ’ can be asserted. Assertibility, therefore, is guaranteed to be preserved in moving from one to the other.

In response to this we might imagine someone objecting along the lines of Williamson:

Sure, this explanation works if we *assume* that all cases conform to this pattern. If, however, we also allow cases in which it is true *that  $\varphi$  and  $\psi$* , but not true *that  $\varphi$* , then the explanation collapses. Consequently, the validity of the inference is not explained by (CONJUNCTION) alone; it relies, also, on an antecedent (and so far unexplained) restriction of the notion of a possible case.

To this the adherent of the linguistic approach can respond that (CONJUNCTION) *also* explains why such “cases” are spurious and why we are completely justified in setting them aside. Firstly, they insist on a “language first” approach where our grasp on the notion of a ‘case in which it is true *that  $\varphi$  and  $\psi$* ’ is given by our grasp on what it takes for a case to be such that asserting ‘ $\varphi$  and  $\psi$ ’ would be correct, and similarly for ‘ $\varphi$ ’.

Next, they say that the reason why no case is such that ‘ $\varphi$  and  $\psi$ ’, but not ‘ $\varphi$ ’ is assertible, is that the criterion *which we ourselves laid down* for the assertibility of the former involves the assertibility of the latter. So, quite without making any prior assumptions about what the world must be like, we know – on the basis of the linguistic rule we laid down – that if it is such that ‘ $\varphi$  and  $\psi$ ’ is assertible, then it is also such that ‘ $\varphi$ ’ is assertible.

Thus, we set aside the “counter-case” not because it represents an impossibility in some robustly metaphysical and language-antecedent sense, but because our linguistic practices – and (CONJUNCTION) in particular – do not allow for the world to be described like this. And the reason for *that* is that our criterion for assessing the assertibility of ‘ $\varphi$  and  $\psi$ ’ is such that if this sentence is found to be assertible, there is no *further* question, no *fur-*

*ther* investigation that could be carried out, as to whether ‘ $\varphi$ ’ is assertible. This has already been established.

(CONJUNCTION) alone, then, suffices to tell us that *no matter what* a case is like, it is going to be such that if it licenses an utterance of ‘ $\varphi$  and  $\psi$ ’, then it also licenses an utterance of ‘ $\varphi$ ’. The reason is that our only criterion for assessing the assertibility of the former involves determining that the latter is assertible.

These are thorny issues, so it might worth recapping some of the essential points of the dialectic. The claim is that if I suppose that the world is such that ‘ $\varphi$  and  $\psi$ ’ is assertible, then I am also in a position to know – solely via my knowledge of how the assertibility-conditions of conjunctive statements relate to the assertibility-conditions of the conjuncts – that ‘ $\varphi$ ’ is assertible. I am not antecedently ruling out certain cases as impossible – just relying on the fact that the assertibility of one sentence also establishes the assertibility of the other because of how we have set up the language.

Now, it might be asked: why not just say that if I know *that*  $\varphi$  and  $\psi$ , then I also know *that*  $\varphi$ , since I know that the latter fact is, so to speak, contained in the former? Isn’t this to make the same point, but without going via the linguistic rules? To this the adherent of the linguistic approach should say that while all of this may well be true, the real question is *how* you know this. This is what is explained, as they see it, via knowledge of assertibility-conditions which we ourselves have laid down.

More importantly, there is a *point* to understanding what we know along these lines, which is to avoid the problem of inexplicable agreement that we found articulated in the writings of both Kant, Wittgenstein and the logical positivists. For if we say that we know that one fact is included in another, then we should ask what reason we have to think that this is indeed so, and, furthermore, that it *must* be so. Why should the facts be so related just because you *think* they are? It is *this* question that the linguistic approach is meant to dissolve by recasting the knowledge in question as self-reflective knowledge concerning the assertibility-conditions of sentences.

## 9.6 Case Study II: The Law of Non-Contradiction

The foregoing is, as I see it, an acceptable explanation of the validity of conjunction elimination which conforms to the constraints of the linguistic approach. It might quite reasonably be said, however, that this is a particularly straightforward case. I shall now turn to a much more complicated issue: how we might go about explaining why instances of the law of non-contradiction are necessarily true.

There is no chance of doing justice to all the complexities here, and it should be noted that my goal is not to offer anything like a justification of classical logic against the objections of someone like the dialetheist (recall Dummett's distinction between suasive and explanatory contexts). Rather, my goal is the more limited one of showing that it is in principle possible to get the linguistic approach of the ground in a way that isn't grossly circular, and simultaneously illustrate how explanations of necessity in accordance with the linguistic approach might go.

Note, in particular, the following limitation: I shall be content with explaining the validity of *one* principle which may, I claim, legitimately be consider a version of the law of non-contradiction. I am not claiming that there are no other versions of this law which may lack a proper justification. The underlying reason is that I shall base the explanation on a specific understanding of negation without claiming that no connective could differ from this and still deserve to be called 'negation'. Nor shall I claim that the 'not' of English is the negation I shall be discussing. This is acceptable, I maintain, because my overall aim is not to justify the law of non-contradiction specifically, but to provide a proof of concept with regard to the linguistic approach.

I shall proceed by first outlining the explanation while glossing over a number of problematic aspects and then proceed to discuss these problematic aspects.



## Outline of the Explanation

The law of non-contradiction (LNC) comes in many forms.<sup>14</sup> One important distinction, for our purposes, goes between what we may call ‘prohibitive’ and ‘permissive’ laws. A prohibitive law states that, regardless of what is the case, certain utterances will be classified as mistaken/unassertible, and a permissive law states that, regardless of what is the case, certain utterances will be classified as correct/assertible.

The permissive law we will be aiming to explain – in accordance with the overall commitments of the linguistic approach – is:

(LNC<sup>+</sup>). Sentences of the form ‘ $\neg(\varphi \wedge \neg\varphi)$ ’ are classified as assertible regardless of what is the case.

However, the route I shall propose for explaining (LNC<sup>+</sup>) goes via two prohibitive laws, which may be called, respectively, the ‘distributive’ and the ‘collective’ law of non-contradiction:<sup>15</sup>

(LNC<sup>D</sup>). Regardless of what is the case, we could never find that both ‘ $\varphi$ ’ and ‘ $\neg\varphi$ ’ are assertible.

(LNC<sup>C</sup>). Regardless of what is the case, we could never find that a sentence of the form ‘ $\varphi \wedge \neg\varphi$ ’ is assertible (i.e. such a sentence fails to be assertible no matter what is the case).

The first step is to explain (LNC<sup>D</sup>) by appealing to the following linguistic convention governing negation:

(NEGATION). ‘ $\neg\varphi$ ’ is assertible if and only if ‘ $\varphi$ ’ fails to be assertible.

The core idea is that if this is the *sole* criterion for determining whether a sentence of the form ‘ $\neg\varphi$ ’ is assertible, then we can rule out counterexamples

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<sup>14</sup>For a comprehensive survey, see Grim (2004).

<sup>15</sup>For more on this distinction, and some of the complexities involved, see Varzi (2004).

## 9. First Principles

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to (LNC<sup>D</sup>) via reasoning similar to that employed to rule out counterexamples to conjunction elimination. There we said that if a conjunction has been found to be assertible, then there is no *further* question as to whether the conjuncts are assertible. In the case of (LNC<sup>D</sup>) the reasoning I have in mind would go roughly as follows:

In order to have a counterexample to (LNC<sup>D</sup>), it needs to be the case that ‘ $\varphi$ ’ is assertible. But if ‘ $\varphi$ ’ is assertible and (NEGATION) provides the *only* route to finding that negative sentences are assertible, then there is no *further* question as to whether ‘ $\neg\varphi$ ’ is assertible. For if the question of assertibility has been answered affirmatively, then the question of “failure of assertibility” has *thereby* been answered negatively. That is: if a sentence is found to be assertible, there is no further question as to whether it might also *fail* to be assertible. Thus, the assertibility of ‘ $\neg\varphi$ ’ is ruled out by the very same procedure that establishes the assertibility of ‘ $\varphi$ ’, and so we could never find that both these sentences are assertible.<sup>16</sup>

To be clear: there are a number of potential problems here which we shall consider shortly. For now, I want to outline the rest of the explanation. The step from (LNC<sup>D</sup>) to (LNC<sup>C</sup>) is explained via an appeal to (CONJUNCTION).<sup>17</sup> Provided that this is the *only* route to assertibility for conjunctions, it is clear that if there can be no counterexamples to (LNC<sup>D</sup>), then there can be no counterexamples to (LNC<sup>C</sup>) either. This is because the only way for a sentence of the form ‘ $\varphi \wedge \neg\varphi$ ’ to be assertible would be

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<sup>16</sup>It is worth noting that the proof-theoretic approach to explaining logical validity notoriously struggles to explain principles like (LNC<sup>D</sup>). Thus, Dummett concludes that ‘the principle of consistency is not a logical principle: logic does not require it, and no logical laws could be framed that would entail it.’ (1991: 295) And Neil Tennant suggests that at this point ‘one needs a metaphysical basis for logic’, which he believes ‘is to be found in our sense of contrariety, a sense that follows inexorably from our deploying perceptual concepts and objectual categories, and from our understanding of the fundamental features of bodies and events occupying space and time.’ (1999: 217)

<sup>17</sup>Modified so as to govern ‘ $\wedge$ ’ rather than ‘and’.

for both conjuncts to be assertible, and this is what has just been ruled out. Such sentences, therefore, will fail to be assertible, no matter what.<sup>18</sup>

The step to (LNC<sup>+</sup>) is now straightforward. Given that such sentences fail to be assertible no matter what, and (NEGATION) tells us that if a sentence fails to be assertible, then this suffices for its negation to be assertible, we can see that sentences of the form ' $\neg(\varphi \wedge \neg\varphi)$ ' will be assertible no matter what.

This, then, is the outline of the explanation. I shall now turn to the worries that were glossed over above, and in particular the perfectly reasonable worry that this explanation is grossly circular.

### The Threat of Gross Circularity

With regard to any purported explanation or justification of LNC, the natural suspicion is going to be that it all boils down to applying the law in the meta-language. It might well be thought that this is what is happening above, and that nothing said so far constitutes much of an improvement on for instance the explanation provided by Hahn.

Here is an immediate worry along such lines: We said that ' $\neg\varphi$ ' is assertible iff ' $\varphi$ ' fails to be assertible, and argued from here that if we find that ' $\varphi$ ' is assertible, then that is the end of the matter as far as ' $\neg\varphi$ ' is concerned, which thus fails to be assertible. But here it may be asked: isn't this just to assume, at the outset, that ' $\varphi$ ' could not both be and fail to be assertible? And isn't just a disguised appeal to something like LNC?

There are, in fact, two potential issues here which should be kept apart: First of all, it is natural to worry that we might have a sentence whose assertibility is governed by criteria which could conflict so that the sentence fails to be assertible by some standard(s) and yet gets classified as assertible by other(s). We might thus end up with grounds for classifying a sentence

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<sup>18</sup>Note that 'failure of assertibility', as it is understood here, is not a constructive notion. Notably, we cannot infer from the fact that a conjunction fails to be assertible that one of the conjuncts must fail to be assertible.

as assertible and yet also grounds for saying that it fails to be assertible, and this threatens to yield a counterexample to (LNC<sup>D</sup>). I shall treat this issue below under the heading of ‘Conflicting Conventions’.

In this kind of counterexample our reason for saying that the sentence fails to be assertible is not merely that we “lack” any grounds for deeming it assertible. The thought, rather, is that we have *both* grounds for classifying it as assertible *and* grounds for saying that it fails to be assertible. However, we could also imagine someone objecting that it is grossly circular to assume that we could not both *lack* any adequate grounds for asserting a given sentence and yet also *have* grounds for asserting it. Before I turn to the issue of conflicting conventions, I shall argue that this idea can be rejected as incoherent.

I shall argue, that is, that we can defend the following principle without making the proposed explanation of (LNC<sup>D</sup>) grossly circular:

It is incoherent to suppose that we could both *lack* any grounds for asserting a given sentence and yet *also* find it to be assertible.

As circumstantial evidence that an appeal to this principle is not just a covert appeal to some version of LNC, we may note that the most prominent critic of LNC explicitly endorses it:

With dialetheism assumed, let us return to the issue [...] of the conditions under which a negated sentence is true. There, I argued that the mere failure of the truth of  $\alpha$  is sufficient ground for the truth of  $\neg\alpha$ . Now consider the situation in which both  $\alpha$  and  $\neg\alpha$  are true. Here the Fact in virtue of which  $\neg\alpha$  is true cannot be the mere “negative” one that that  $\alpha$  is not true. There must be a “positive” Fact. (Priest 2006: 67)

It should also be noted that – at least as far as I know – there are no candidates in the literature which suggest that we could ever find that it would be correct to assert a particular sentence, and yet *also* find that there is a *complete lack* of reasons for deeming it assertible. All the putative

counterexamples to LNC are cases where we have, so to speak, *too many reasons* – i.e. we have reason to assert a sentence and also reason to deny it or utter its negation.

The fact that no alleged counterexample to LNC tells against the above principle, and the fact that Graham Priest explicitly endorses it, strongly suggests that appealing to it is not a grossly circular appeal to LNC. Still, we need to say more about where this principle comes from in order to assess whether appealing to it is compatible with the constraints of the linguistic approach. How, then, do we rule out the idea that a sentence might be assertible and, *in addition*, be such that adequate grounds for asserting it are completely lacking?

Relating this back to the above dilemma, we may imagine some objecting that this relies on an unexplained restriction with regard to the notion of a case: We are simply assuming that no case is such that it both: i) delivers the verdict that a particular sentence is assertible, and ii) delivers the verdict that there are no adequate grounds for asserting this sentence.

Again, however, the response is that this does not represent an antecedent, “metaphysical” restriction on the notion of a case, but is itself explained by our linguistic practices. For – quite in accordance with the subjective-constitutive strategy – it is *we* who settle what it is for a case to be such that a particular sentence is assertible. And we can legitimately insist that if we find that the sentence *is* assertible, then there is no further question as to whether we might also *lack* any reason for deeming it assertible. The very practice of evaluating the assertibility of sentences ensures that any such question has thereby been answered negatively.

This is not – an adherent of the linguistic approach should insist – based on appreciating some robustly metaphysical fact to the effect that the presence of a ground for asserting a sentence is incompatible with the absence of such grounds. Rather, it is a matter of reflecting on how we evaluate assertibility and what we count as having or lacking grounds for deeming sentences assertible.

## Conflicting Conventions

I now turn to the question of whether our linguistic conventions could deliver conflicting verdicts with regard to the assertibility of a given sentence, and thus give us both reason to say that it is assertible and reason to say that it fails to be assertible.

Here is a variation on an example suggested by Priest (2006: 68): Consider the expression ‘right-wing’ as used to describe political parties. There is no single test for whether a party is right wing; rather, we have a set of conditions associated with this predicate and no precise rules for settling how many conditions must be satisfied, or which conditions take priority.

It seems quite conceivable, therefore, that we could be confronted with a party, *P*, which has a set of characteristics such that we should ordinarily say that the ‘*P* is right-wing’ is assertible, but which also has a set of characteristics such that we should typically say that it would be a mistake to call it right-wing – that is: We should say that ‘*P* is right-wing’ fails to be assertible.

To make things more definite, let us imagine a linguistic community which takes advocating *laissez-faire* capitalism as a necessary and sufficient condition for applying ‘right-wing’ to a political party – sufficient because they take this to automatically license the assertion of ‘*x* is right-wing’; necessary because they criticise those who assert ‘*x* is right-wing’ when *x* does not advocate *laissez-faire* capitalism.

Let us suppose, however, that they also take advocating a form of social conservatism to be a necessary and sufficient condition in exactly the same way. Of course, it is plain to us that these conventions *can* come into conflict, but we may imagine that this linguistic community has never paid much attention to this. All the political parties they have had to deal with have had either both these traits or none of them, and the community is not, we may suppose, really in the habit of reflecting on their use of language.

Suppose, now, that they are confronted with party, *Q*, that advocates *laissez-faire* capitalism, but is in no sense socially conservative. In this case,

we might well say that, by their standards, the sentence ‘ $Q$  is right-wing’ both is and fails to be assertible.

The question I am interested in is: do examples like this threaten the above explanation of (LNC<sup>D</sup>)? First impressions might suggest that they do. For if we get the verdict that the sentence both is and fails to be assertible, then (NEGATION) would seem to straightforwardly deliver that both the sentence and its negation are assertible.<sup>19</sup>

Now, I do not want to deny that we *could* (and even sometimes *do*) react to such cases in this way. It is quite intelligible that the linguistic community above might decide to accept the sentence ‘ $Q$  is right-wing and  $Q$  is not right-wing’. However, I do want to deny that this is forced even against the background of (NEGATION). That is, I claim that it is possible to understand this convention in such a way that the explanation offered above is immune to cases, such as the above, where multiple criteria come into conflict.

We must be cautious here since we are essentially operating with two distinct notions of assertibility at this point. There is, on the one hand, the idea that a sentence can be assertible by *some* standard, and, on the other hand, the question of assertibility *tout court* – the question, that is, of whether a sentence is in fact treated as assertible *in practice* when everything has been accounted for. The former does not entail the latter since if we find that our conventions deliver conflicting verdicts with regard to a sentence, there is still the question of how we handle this: Do we treat this as a license to utter the sentence? Do we treat such utterances as exempt from criticism?

When, for instance, the above linguistic community finds that ‘ $Q$  is right-wing’ is assertible by one of their standards, but that there is a conflict, there is a question of whether they in fact end up *treating* this sentence as assertible in practice.

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<sup>19</sup>We also seem to get a straightforward counterexample to (LNC<sup>C</sup>). Whether we would need to give up (LNC<sup>+</sup>) is a more complicated matter.

If, now, we understand the occurrence of ‘fails to be assertible’ in (NEGATION) as ‘fails to be assertible by some (i.e. at least one) standard’, then cases like the above will deliver counterexamples. However, on a different reading, (NEGATION) tells us that ‘ $\neg\varphi$ ’ is assertible iff ‘ $\varphi$ ’ fails to be assertible *tout court*, and on this reading, I maintain, the explanation given above is not troubled by conflicting conventions.

The point is that (NEGATION) then tells us to use the very same standard when evaluating the assertibility of a sentence, as when evaluating its assertibility in the context of evaluating its negation. Thus, if we suppose that our conventions yield conflicting verdicts with regard to a sentence ‘ $\varphi$ ’, then the question is: what do we do now? How do we in fact *treat* this sentence? It is clear that if we are to get a counterexample to (LNC<sup>D</sup>), we must treat it as, after all, assertible. But now (NEGATION) tells us that it is *this* verdict which is relevant when it comes to settling whether ‘ $\neg\varphi$ ’ is assertible, and thus delivers the verdict that asserting ‘ $\neg\varphi$ ’ would be a mistake. Since, now, (NEGATION) by hypothesis provides the *only* route to assertibility for negative sentences, we see that counterexamples to (LNC<sup>D</sup>) remain ruled out.

This is important because it allows us to deny that we are appealing to an implausibly “neat” – and grossly circular – conception of the possible outcomes when we attempt to assess the assertibility of a sentence. After all, it would clearly be inadmissible to assume, in this context, that we will always receive, straightforwardly, either a positive or a negative verdict and never both (this was essentially our objection to Hahn’s brief explanation).

However, we are now allowing that reaching a verdict here might be a thoroughly messy process. It might be that we reach no verdict at all, or that different criteria pull us in opposite direction. Still, the thought is that if we are to get a counterexample to (LNC<sup>D</sup>) from this, the mess needs to be tidied up in a certain way. Specifically: we need to treat cases of conflict as cases where the sentence in question is, after all, assertible in practice. This is because we need clear-cut assertibility in order to have a clear-



cut counterexample to (LNC<sup>D</sup>) (which is a matter of joint assertibility). And now we can say – based on the present understanding (NEGATION) – that we *still* do not get a counterexample because by deeming the conflicted sentence assertible *tout court*, we have thereby removed any reason we might have had for deeming its *negation* assertible.

### Conclusion Regarding the Law of Non-Contradiction

I maintain that this is an explanation (or at least a decent sketch of an explanation) of why instances of ‘ $\neg(\varphi \wedge \neg\varphi)$ ’ are necessary (guaranteed to be true no matter what is the case) provided that the linguistic conventions stated above are in force. And I maintain that this explanation conforms to the restrictions of the linguistic approach. It is based solely on what can be gathered from reflecting on the relevant linguistic practices and it does not introduce an external standard of correctness which would result in a problem of inexplicable agreement. These practices alone, that is, suffice to guarantee the assertibility of the sentences in question.

Still, it should be noted that the explanation does appeal to principles which it would hardly be appropriate to describe as linguistic *conventions*. Notably, the principle that we could not both have an adequate reason for asserting a sentence and yet lack any such reason fits awkwardly with this terminology, since it is far from clear that we could have chosen to adopt a linguistic practice which rejects it.

However, to say that this principle is an inevitable part of our linguistic practices is not to say that it is forced upon us by an external standard of correctness. It might just be integral to the very practice of evaluating whether sentences are assertible or not. There is then no mystery – no problem of inexplicable agreement – concerning how we can reach a justified belief in this principle simply by reflecting on this practice: For although it is inevitable, it is not in the business of accurately reflecting some language-independent domain.



## Conclusion

Nothing I have said licenses anything like the conclusion that the linguistic approach solves the problem of necessity which Dummett nicely formulated. However, I hope to have shown that it deserves to be counted among genuine *candidates* for a solution – and, indeed, a moderately promising one.

My main aim has been to argue that we do not have sufficient grounds for the currently orthodox view that this approach may be dismissed because it is thoroughly misguided and doomed to failure. And my contention is that none of the objections considered above are as devastating as they are often made out to be: Some miss the mark entirely, some can be answered head-on, and some point to genuine challenges and constraints which put pressure on the linguistic approach, but do not warrant rejecting it.

The challenge which emerges from the rule-following considerations – concerning how to account for a rationally constrained notion of proof – is, as I see it, a particularly difficult one. Although I have argued that there is a strategy available here, I fully concede that further work needs to be done in order to determine whether it can, in the end, do the job.

Apart from this concern (and various others mentioned along the way), the major question is, of course, whether the linguistic approach can account for all the necessities that need accounting for. My examples throughout have been either logical necessities or traditional analyticities, and it is certainly not unreasonable to worry that even if the linguistic approach can be employed with some success in these areas, there will be other necessities

## Conclusion

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which it cannot adequately account for. This, however, is not a reason to dismiss the approach, but a reason to engage in detailed investigations of what it can and cannot explain.

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