

# More reflections on consequence\*

Julien Murzi<sup>†</sup> & Massimiliano Carrara<sup>‡</sup>

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## Abstract

This special issue collects together nine new essays on *logical consequence*: the relation obtaining between the premises and the conclusion of a logically valid argument. The present paper is a partial, and opinionated, introduction to the contemporary debate on the topic. We focus on two influential accounts of consequence, the model-theoretic and the proof-theoretic, and on the seeming platitude that valid arguments necessarily preserve truth. We briefly discuss the main objections these accounts face, as well as Hartry Field's contention that such objections show consequence to be a primitive, indefinable notion, and that we must reject the claim that valid arguments necessarily preserve truth. We suggest that the accounts in question have the resources to meet the objections standardly thought to herald their demise and make two main claims: (i) that consequence, as opposed to *logical* consequence, is the epistemologically significant relation philosophers should be mainly interested in; and (ii) that consequence is a paradoxical notion if truth is.

**Keywords:** logical consequence; logical inferentialism; truth-preservation; validity paradoxes

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<sup>†</sup>University of Kent and Munich Center for Mathematical Philosophy, Ludwig-Maximilians Universität, München, [j.murzi@kent.ac.uk](mailto:j.murzi@kent.ac.uk).

<sup>‡</sup>University of Padua and Cogito, Philosophy Research Centre, [massimiliano.carrara@unipd.it](mailto:massimiliano.carrara@unipd.it).

# 1 Introduction

We all seem to have an intuitive grasp of the notion of *logical validity*: we reject arguments as invalid, on the grounds that a purported conclusion does not *logically follow from* the premises.<sup>1</sup> Similarly, we feel compelled to accept the conclusion of an argument on the grounds that we accept its premises and we regard its conclusion as a *logical consequence* of them. But what *is* logical validity? And, if logically valid arguments are valid in virtue of the meaning of the logical expressions, how to account for the meaning of the logical vocabulary? Moreover, is logical consequence a species of a more general notion, viz. (non-purely logical) validity?

Orthodoxy has it that logical validity plays a threefold epistemic role. First, logically valid rules are the most general rules of *thought*: logic, or consequence, records rules for correct thinking.<sup>2</sup> For instance, one such rule has it that, if  $\Gamma$  is a logically inconsistent set of sentences, then one *ought not* to believe each of the  $\gamma \in \Gamma$ . Similarly, another rule states that, if  $\alpha$  logically follows from  $\Gamma$ , one *ought not* to believe each  $\gamma \in \Gamma$  and disbelieve  $\alpha$ .<sup>3</sup> Second, the premises of a logically valid argument are standardly thought to *justify* the argument's conclusion (Etchemendy, 1990; Priest, 1995; Etchemendy, 2008). Thus,

(1) If the doorbell rings at 10:00am, then the postman is at the door

and

(2) The doorbell rings at 10:00am

jointly *justify*

(3) The postman is at the door,

*qua* premises of the logically valid argument (1)-(3). Deduction is then seen as a way to extend one's stock of known or justified beliefs (Boghossian, 2003; Williamson, 2000; Rumfitt, 2008; Prawitz, 2012). If we know (1) and (2), and we know (1)-(3) to be valid, inferring (3) from (1) and (2) is sufficient for thereby coming to know (3).<sup>4</sup> Finally, facts about logic are typically considered to be

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<sup>1</sup>Here and throughout we take 'validity', 'following from' and 'consequence' to express the same relation.

<sup>2</sup>Thus, Frege writes that, in the sense of law that "prescribes what ought to be ... logical laws may be called laws of thought" Frege (1998, I, xv). See also Frege (1956, p. 325). For a defence of the normative role of logic, see e.g. MacFarlane (2004), Christensen (2004), Field (2009b) and Wedgwood (2013). For an influential criticism, see Harman (1986, 2009).

<sup>3</sup>We'll say a bit more about logical consequence and normativity in §4 below.

<sup>4</sup>Similarly for justified beliefs.

knowable *a priori*, if knowable at all: we can find out that (3) follows from (1) and (2), so to speak, in our armchair (Tennant, 1997; Boghossian, 2000; Etchemendy, 1990, 2008; Hanson, 1997).

Any of these roles would suffice to make of validity a central philosophical concept. If validity has normative import for thought, then it will arguably have a central role in any account of *rationality*. If it allows us to extend our stock of known, or justifiably believed, propositions, it will play a key role in any account of knowledge and justification. And, if large parts of arithmetic can be derived from (higher-order) logic plus definitions, as neo-logicians contend (Wright, 1983; Hale and Wright, 2001), it might be argued that the epistemology of logic is at least related in important ways to the epistemology of arithmetic itself.<sup>5</sup> How, then, to account for validity?

We focus on two different pre-theoretic *notions* of validity: a semantic notion, call it  $\text{validity}_{se}$ , according to which valid arguments preserve truth, and a syntactic notion, call it  $\text{validity}_{sy}$  according to which an argument  $\langle \Gamma \therefore \alpha \rangle$  is valid just if, as Stewart Shapiro (2005a, p. 660) puts it, “there is a deduction of  $[\alpha]$  from  $\Gamma$  by a chain of legitimate, gap-free (self-evident) rules of inference.” Accordingly, we mainly focus on two prominent, and familiar, accounts: the model-theoretic and the proof-theoretic, aimed at modelling  $\text{validity}_{se}$  and

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<sup>5</sup>To be sure, each of the foregoing roles has been disputed. Thus, Harman (1986) has influentially questioned whether logic can be especially normative for thought; the principle that knowledge is closed under known logical consequence, and is, for that reason, normative for thought, has been attacked on a number of counts (Dretske, 1970; Nozick, 1981); and it can be doubted that the *a priori* knowability of logical facts is especially relevant, if one doubts the relevance of the *a priori*/*a posteriori* distinction itself (see e.g. Williamson, 2008, pp. 165-9). However, even if correct, these views would not dethrone validity from its central theoretical place. For instance, Harman himself (see e.g. Harman (1986, p. 17) and Harman (2009, p. 334)) concedes that facts about *validity*, as opposed to facts about *logical* validity, are normatively relevant for thought (more on validity in §4 below). And even if knowledge is not in general closed under known logical consequence, the fact remains that our stock of known and justifiably believed proposition can be, and typically is, extended via deduction. See also Gila Sher’s reply (Sher, 2001) to Hanson (1997), where she maintains a neutral position w.r.t. the *a priori* condition.

validity<sub>sy</sub> respectively.<sup>6</sup>

Consider validity<sub>se</sub> first, viz. the thought that valid arguments necessarily preserve truth, for some *alethic* interpretation of ‘necessary’ (i.e. such that necessary sentences are true). If we took necessary truth-preservation to be also sufficient for validity, we would have an informal account of validity—call it the *modal account*:

(Modal)  $\langle \Gamma \therefore \alpha \rangle$  is valid iff, necessarily, if all  $\gamma \in \Gamma$  are true, so is  $\alpha$ .

It is generally thought, however, that necessary preservation of truth is not sufficient for logical consequence: logically valid arguments should be valid in virtue of their *form*, and Modal clearly doesn’t account for this. For one thing, on commonly held assumptions it validates formally invalid arguments such as  $\langle x \text{ is H}_2\text{O} \therefore x \text{ is water} \rangle$ . For another, it fails to account for the *a priori* of logical validity: one can hardly know *a priori* that water H<sub>2</sub>O entails that  $x$  is water (see e.g. Hanson, 1997). To be sure, the notion of possibility in question might be taken to be logical. Then, the modal account would not validate formally invalid arguments. But—the point is familiar enough—this would introduce a threat of circularity: the notion of *logical consequence* would be defined via an even more mysterious notion of *logical necessity*. *Obscura per obscuriora*.<sup>7</sup>

What is needed, then, is a way to tie truth-preservation to formality: valid arguments preserve truth *in virtue* of their form, and of the meaning of the logical vocabulary.<sup>8</sup> Logical orthodoxy has it that this is captured by the so-called *model-theoretic* account of consequence, which can be traced back to the

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<sup>6</sup>We will occasionally comment (especially in §5 below) on a third account of validity, *primitivism*, which we briefly introduce at the end of this section. A fourth account is also worth mentioning: *deflationism about consequence* (Shapiro, 2011). According to this, the point of the validity predicate is to enable generalisations such as

Every argument of the form (1)-(3) is valid.

The validity predicate is governed, and perhaps implicitly defined, by (some version of) what Beall and Murzi (2013) call the *V-Scheme*

$\langle \alpha \therefore \beta \rangle$  is valid if and only if  $\alpha$  entails  $\beta$ ,

much in the same way as *truth* is governed by (some version of) the T-Scheme. For a recent criticism of deflationist accounts of consequence, see Griffith (2013). For reasons of space, we won’t discuss deflationist accounts, except to notice, in §4 below, that they give rise to validity paradoxes.

<sup>7</sup>See e.g. MacFarlane (2000b, pp. 8-9) and Field (2013). Sher makes a move in this direction when speaking of models representing “formally possible structures” (see e.g. Sher, 1996). The move is criticised in Sagi (2013).

<sup>8</sup>For an excellent discussion of three different notions of formality in logic, see MacFarlane (2000a).

work of Bernhard Bolzano and, especially, Alfred Tarski.<sup>9</sup> According to this, an argument is valid if and only if it preserves truth in all models. More precisely:

(MT)  $\langle \Gamma \therefore \alpha \rangle$  is valid (written:  $\Gamma \models \alpha$ ) iff, for every  $\mathcal{M}$ , if  $\gamma$  is true in  $\mathcal{M}$ , for all  $\gamma \in \Gamma$ , then  $\alpha$  is true in  $\mathcal{M}$ ,

where a model  $\mathcal{M}$  is an assignment of extensions (of the appropriate type) to non-logical expressions of the language (of the appropriate type), and truth-in- $\mathcal{M}$  is defined recursively *à la* Tarski. Crucially, the models quantified in in MT are *admissible* models: models that respect the meaning of the logical vocabulary, i.e. expressions such as ‘if’, ‘not’, ‘every’ etc. If truth-in-a-model is a model of truth, then  $\mathcal{M}$  clearly is a way to make validity<sub>se</sub>, and Modal, more precise.

However, formality is also arguably captured by the following textbook definition of the *proof-theoretic* account of consequence, aimed at capturing the aforementioned validity<sub>sy</sub>:

(PT)  $\langle \Gamma \therefore \alpha \rangle$  is valid (written:  $\Gamma \vdash \alpha$ ) if and only if there exists a derivation of  $\alpha$  from  $\Gamma$ .<sup>10</sup>

To make the definition more plausible, we may require that the derivation in question be *gap-free*, in the sense that each of its steps is intuitively valid and may not be broken into smaller steps. For instance, assuming (as it seems plausible) that *modus ponens* is gap-free in the required sense, arguments of the form  $\langle \alpha \rightarrow \beta, \alpha \therefore \beta \rangle$  are valid according to PT.<sup>11,12</sup>

For first-order classical logic, and many other logics, it can be shown that MT and PT are extensionally equivalent. More precisely, a Soundness Theorem shows that if  $\Gamma \vdash \alpha$ , then  $\Gamma \models \alpha$ , and a Completeness Theorem shows that, if  $\Gamma \models \alpha$ , then  $\Gamma \vdash \alpha$ . However, completeness and related metalogical properties such as compactness and the Löwenheim-Skolem property, are not always enjoyed by proof-systems, as in the case of second-order logic.<sup>13</sup> Hence, MT and PT are not in general guaranteed to be extensionally equivalent.<sup>14</sup> Yet it

<sup>9</sup>See e.g. Bolzano (1837) and especially Tarski (1936). For a recent collection of essays on Tarski’s philosophical work, see Patterson (2009).

<sup>10</sup>See also Shapiro (1998) and Shapiro (2005a).

<sup>11</sup>Incidentally, we notice that PT, so interpreted, is fully in keeping with the etymology of ‘consequence’ (from the Latin *consequi*, ‘following closely’).

<sup>12</sup>It might be objected that PT is not purely syntactic, given that it implicitly, or even explicitly, relies on a notion of soundness. We’ll return to this point in §2 below.

<sup>13</sup>The Compactness Theorem states that a set of sentences in first-order logic has a model iff all its finite subsets have a model. The Löwenheim-Skolem states that, if  $\Gamma$  has a countably infinite model, then it has models of every infinite cardinality.

<sup>14</sup>Nor is either account guaranteed to be equivalent to M, as we have just seen.

might be thought that the foregoing accounts are not necessarily in conflict. A certain kind of *pluralism* about consequence would see each of M, MT and PT as tracking different and equally legitimate notions of consequence: respectively, a metaphysical, a semantic, and a syntactic one (see e.g. Shapiro, 2005b).<sup>15,16</sup> We're not unsympathetic to this view, although, for reasons of space, we won't argue for it here. We'll focus instead on some of the main challenges to the foregoing accounts—challenges that are sometimes thought to collectively motivate a form of scepticism about validity, to the effect that the notion is *undefinable*.

We mention three objections, to be introduced in more detailed in due course. The first has it that Modal seemingly entails Curry-driven triviality: in a nutshell, if (we can assert that) valid arguments preserve truth, then (we can also assert that) you will win tomorrow's lottery (Field, 2008; Beall, 2009; Murzi and Shapiro, 2012).<sup>17</sup> The second maintains that MT is epistemically inert, and would seem to allow facts about validity to be influenced by contingent features of the world (Etchemendy, 1990; Field, 1991; McGee, 1992b,a; Etchemendy, 2008; Field, 2013). The third assumes that proof-theoretic accounts identify consequence with derivability in a given system, and urges that this is problematic: the same logic can be axiomatised in many ways, and it would seem arbitrary to identify logical consequence with *one* such axiomatisation (Etchemendy, 1990; Field, 1991). Similar considerations have led some—chiefly, Hartry Field—to deny that valid arguments necessarily preserve truth and conclude that consequence isn't definable, and must rather be seen as a “primitive notion that governs our inferential or epistemic practices” (Field, 2009b, p. 267).<sup>18</sup>

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<sup>15</sup>Shapiro (2005b, p. 667) writes that  $\models$  and  $\vdash$  “each correspond to a different intuitive notion of logical consequence: the blended notion [MIX, to be introduced in §2 below] and [(PT)] respectively. Both of the latter are legitimate notions, and they are conceptually independent of each other”.

<sup>16</sup>The notion of logical pluralism we mention in the main text is a pluralism about our *conceptions* of logical consequence. It is not necessarily a pluralism about the *extension* of the consequence relation, and is hence different from what is standardly known as *logical pluralism*: the view that there are least two correct logics. For one thing, two different conception of consequence might be extensionally equivalent—this is an immediate consequence of the Soundness and Completeness Theorems. For another, the consequence relation of rival logics might be characterised, at a general enough level of abstraction, in the same way, e.g. as preservation of truth in all models. What varies is one's conception of what a model is (models could be intuitionistic, classical etc.). For a classical presentation of logical pluralism, see Beall and Restall (2006). For a recent collection of essays on logical pluralism, see Cohnitz et al. (2013).

<sup>17</sup>More precisely, the claim that, for all  $x$  and for all  $y$ ,  $x$  entails  $y$  only if, if  $x$  is true, then  $y$  is true, entails  $A$ , where  $A$  is an arbitrary sentence. See §4 below.

<sup>18</sup>Field's argument is already present, *in nuce*, in Field (1991). It is then developed in Field (2008) and Field (2009b). Its fuller and most explicit presentation can be found in Field (2013).

In this paper, we review these (and other) objections and, with a view towards resisting Field’s scepticism, we point to possible ways the objections may be blocked. Moreover, we argue that, if validity is to play its standardly assumed epistemic role, then *consequence*, rather than *logical consequence*, is the key relation we should be interested in. Our claim has far reaching consequences: while the notion of logical consequence is consistent, (non-purely logical) validity is inconsistent, and, indeed, trivial. It gives rise to paradoxes of its own: paradoxes of (non-purely logical) validity.

Our plan is as follows. We discuss model-theoretic (§2) and proof-theoretic accounts (§3) first. We then turn to the modal account of validity, the claim that valid arguments necessarily preserve truth, and to some related paradoxes: the paradoxes of naïve validity, as we shall call them (§4).<sup>19</sup> We finally end with some concluding remarks (§5). Along the way, we briefly introduce the contributions to this special issue.

## 2 Model-theoretic consequence

According to the model-theoretic account of consequence, an argument  $\langle \Gamma \therefore \alpha \rangle$  is valid if and only if every model that makes every sentence in  $\Gamma$  true also makes  $\alpha$  true. In short: an argument is valid iff it has no *counterexamples*, i.e. iff there is no model that makes the premises true and the conclusion false.

A model is easily described: it consists of a nonempty set of objects  $D$  (the *domain*) and an *interpretation* (multi-)function assigning objects and relations over  $D$  to, respectively, the singular terms and the non-logical predicates of the language. Truth-in-a-model is then recursively defined *à la* Tarski. It is less clear, however, how models should be interpreted. In his classical criticism of Tarski’s account of consequence, John Etchemendy (1990) famously distinguishes two ways of understanding the notion: models can be seen either as reinterpretations of the non-logical vocabulary, or as descriptions of ways the world could be. Etchemendy labels them, respectively, *interpretational semantics* (henceforth, RS) and *representational semantics* (henceforth, IS).<sup>20</sup> Both RS and IS can be seen as ways to make the validity<sub>se</sub> formally tractable. Intuitively, IS is meant to

<sup>19</sup>The terminology is introduced in Murzi and Shapiro (2012).

<sup>20</sup>Although RS and IS express different ways of understanding models, in what follows we will often take them to be shorthand for, respectively, interpretational and representational validity (i.e. model-theoretic validity where models are understood according to RS/IS).

capture the idea that logically valid arguments preserve truth in virtue of the meaning of the logical vocabulary. As for RS, it aims at capturing the thought that valid argument preserve truth in all possible circumstances. Both IS and RS are problematic, however.

According to IS, each argument is associated with a class of reinterpretations of its non-logical vocabulary. The argument is valid iff, for any such reinterpretation *I*, if the argument's premises are true on *I*, so is the conclusion. That is, whether an argument is valid or not depends on the *actual* truth of a certain universal generalisation. This already suggests a possible challenge to the account: since what actually is the case is contingent, is there not a risk that the account's verdicts depend on contingent features of the world? Etchemendy (1990) offers a battery of arguments aiming at showing, among other things, that this is precisely what happens. Here we can only offer a brief and incomplete summary of Etchemendy's influential discussion.

To begin with, Etchemendy argues that IS is epistemologically, and hence *conceptually*, inadequate: it doesn't help us extend our stock of known or justifiably believed propositions via deduction (Prawitz, 1985; Etchemendy, 1990; Prawitz, 2005). As Graham Priest puts it:

If the validity of an inference is to be identified with the truth of a universal generalization then we cannot know that an inference is valid unless we know this generalization to be true. But we cannot know that this generalization is true unless we know that its instances are true; and we cannot know this unless we know that every instance of an argument form is materially truth preserving. Hence, we could never use the fact that an argument form is valid to demonstrate that an argument is materially truth preserving. Thus the prime function of having a valid argument would be undercut. (Priest, 1995, p. 287)

Here we simply notice that the objection doesn't obviously affect the pluralist view we alluded to in the preceding section.<sup>21</sup> If there really are different, independent conceptions of consequence, then it would be a mistake to expect any one of them to satisfy all of the standardly assumed features of logical consequence we listed at the outset.<sup>22</sup> More precisely, we should not expect either RS

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<sup>21</sup>It might also be argued that the argument contains a non-sequitur: in general, we do not come to know the truth of a universally quantified sentence by first coming to know the truth of each of its instances.

<sup>22</sup>We should stress that the kind of pluralism we're waiving towards here is of the conceptual



or IS, and validity<sub>se</sub> more generally, to account for the a priori knowability of facts about logical consequence.

Etchemendy's main criticism, however, is that IS is *extensionally inadequate*. More specifically, he claims that it *overgenerates*: in certain conceivable scenarios, it declares to be logically valid arguments that are (intuitively) *not* logically valid.<sup>23</sup> Etchemendy focuses on *cardinality sentences*: purely logical sentences about the number of objects in the universe (Etchemendy, 1990, p. 111 and ff). Consider, for instance, a sentence saying that there are fewer than  $n + 1$  objects in the universe. If the universe is actually finite, and if it actually contains exactly  $n$  objects, such a sentence will be true in all models, and hence logically true according to IS. That is, the example effectively shows that there is a sentence of first-order logic that is logically true just if the universe is finite (and logically contingent otherwise). But this, Etchemendy says, is counterintuitive: facts about the number of objects there happen to be in the universe are not *logical* facts. Etchemendy further argues that assuming that the universe is necessarily infinite (as suggested by McGee, 1992b) will not do. The assumption, effectively equivalent to assuming the Axiom of Infinity in set-theory, is not logical, and the account would still be "influenced by extra logical facts" (Etchemendy, 1990, p. 116). Once more, the account appears to be conceptually inadequate.

Second-order logic with standard semantics provides another, much-discussed example of overgeneration (Etchemendy, 1990, 2008). If we are prepared to accept a standard semantics for such a logic, it is a well-known fact that there are sentences of second-order logic equivalent to, respectively, the Continuum Hypothesis (CH) and its negation.<sup>24</sup> Since standard axiomatisations of second-order logic with standard semantics are categorical, i.e. all of their models are isomorphic, it follows that either CH or its negation is true in all second-order models, and is therefore a logical truth of second-order logic. This again seems hard to swallow. We know that CH is independent of ZFC, i.e. it can only be decided in theories that are stronger than ZFC. But ZFC is strong enough to represent all of the known mathematics, and hence seems far too strong a

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kind, and is hence different from the pluralism of the extensional kind discussed in e.g. Beall and Restall (2006).

<sup>23</sup>Etchemendy also argues that IS *undergenerates*, i.e. it fails to recognise the validity of some intuitively logically valid arguments. We focus on overgeneration for reasons of space, but briefly mention possible examples of undergeneration in §4 below.

<sup>24</sup>In the standard semantics for second-order logic, second-order variables range over all subsets, i.e. the power set, of the (first-order) domain. For details, see Shapiro (1991).

theory to count as logical.

It might be thought that problems of extensional adequacy for IS can be overcome by an adequate choice of the set of logical constants. Indeed, Etchemendy himself argues that IS's prospects are crucially tied to the availability of a distinction between the logical and the non-logical vocabulary: a notoriously hard question, with immediate consequences for the issue of extensional adequacy.

On the one hand, if not enough logical expressions are recognised as such, the account will undergenerate. Trivially, for instance, if  $\wedge$  is not recognised as logical, then the theorem  $(\alpha \wedge \beta) \rightarrow \alpha$  won't be classified as logically valid either. On the other, if too many expressions are recognised as logical, it will overgenerate instead. A bit less trivially, suppose we took 'President of the US' and 'man', alongside 'if', to be logical. Then,

(4) If Leslie was a president of the US, then Leslie was a man

comes out as logically true, contrary to intuition (Etchemendy, 2008).

To be sure, there exist well-known accounts of logicality. The standard account has it that logical notions are *permutation invariant*: they are not altered by arbitrary permutations of the domain of discourse.<sup>25</sup> A less standard (but still influential) account ties logicality to certain proof-theoretic properties, such as *proof-theoretic harmony*, about which more in §3 below. But Etchemendy advances general reasons for thinking that no satisfactory account of the logical/non-logical divide can be forthcoming. He writes:

[A]ny property that distinguishes, say, the truth functional connectives from names and predicates would still distinguish these expressions if the universe were finite. But in that eventuality [the] account would be extensionally *incorrect*. (Etchemendy, 1990, p. 128)

To unpack a little: any account of the logical/non-logical divide is, if true, necessarily true. And yet, there are counterfactual situations in which any such account would get things wrong. For instance, if the universe were finite and contained exactly  $n$  objects, we've already seen that IS would declare 'There are more than  $n$  objects' to be logically false, contrary to intuition. Etchemendy

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<sup>25</sup>For discussion of the permutation invariance account, see e.g. Tarski (1986), Sher (1991), MacFarlane (2000a, Ch. 6), Bonnay (2008), Feferman (2010). See also Gil Sagi's and Jack Woods's contributions to the present volume.

concludes that any account of the logical/non-logical can at best accidentally get things right: it cannot in general guarantee extensional correctness.<sup>26</sup>

This argument is too quick, however. For one thing, proof-theoretic accounts of consequence are not obviously undermined by cardinality considerations (though they of course face other problems, as we'll see in the next section). This suggests that Etchemendy's argument, even if sound, only works on the assumption that consequence may not be defined in proof-theoretic terms. For another, so-called 'mixed' accounts of consequence are seemingly immune to Etchemendy's objection from finitism, and hence appear to invalidate Etchemendy's argument against the possibility of drawing an adequate logical/non-logical divide. For instance, both Hanson (1997) and Shapiro (1998) advocate versions of the following *mixed* account:

(MIX)  $\langle \Gamma \therefore \alpha \rangle$  is valid iff it necessarily preserves truth for all uniform reinterpretations of its non-logical vocabulary.

According to MIX, sentences such as 'There are fewer than  $n$  objects' would not be logically true, even if the universe were actually finite, since it *could be* infinite. If that's correct, Etchemendy has not quite shown that no correct account of the logical/non-logical divide can be forthcoming.<sup>27</sup>

In view of the foregoing objections, Etchemendy (2008) suggests that RS affords a more appropriate interpretation of the claim that an argument is valid iff it preserves truth in all models. The thought, then, is that models describe *possibilities*, as opposed to providing interpretations to the non-logical vocabulary. Truth-preservation in all models then becomes a way to formally cash out *necessity*, viz. the thought that "if an argument is logically valid, then the truth of its conclusion follows necessarily from the truth of the premises" (Etchemendy, 2008, p. 274).

The model-theoretic account, so interpreted, no longer depends on the availability of a satisfactory account of the logical/non-logical divide: the account keeps the interpretation of both the logical and the non-logical vocabulary fixed, and varies the circumstances with respect to which the truth of sentences, so interpreted, is to be evaluated.<sup>28</sup> However, even assuming that RS has the resources to meet the remaining objections we have listed so far, the account

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<sup>26</sup>For an excellent critical introduction to the problem of logical constants, see Gomez-Torrente (2002).

<sup>27</sup>For a criticism of MIX, see MacFarlane (2000b).

<sup>28</sup>Etchemendy (2008, p. 288 and ff.) argues that we may still vary the interpretation of some

faces problems of its own. The main difficulty is what McGee (1992b) calls the *reliability problem*. Models have *sets* as their domain, and are therefore ill-suited to represent ways the *universe* could be, since, the thought goes, the universe actually contains all sets, and there is no set of all sets. It seemingly follows that truth in all models does not imply truth, since any model-theoretic validity could be actually false. As Field puts it: “there is no obvious bar to a sentence being valid (or logically true) and yet not being true!” (Field, 2008, p. 45). In short: RS is, just like IS, in danger of being extensionally inadequate. For instance, it declares sentences such as

(5) There exist proper classes

logically false, which seems problematic on at least two counts. First, it might be argued that (5) is true, and we certainly do not want our account of consequence to be inconsistent with certain set-theoretic facts. Second, one might insist that, even assuming that (5) were false, it would not be *logically* false.<sup>29</sup> We consider three possible replies.

To begin with, it might be responded that the inability of models to represent real possibilities is an ‘artefact’ of the model-theory. Etchemendy pushes this line:

I will set aside the important question of how we know our models actually depict every relevant possibility. Merely intending our semantics in this way is not sufficient, since limitations of our modeling techniques may rule out the depiction of certain possibilities, despite the best of intentions. This is arguably the case in the standard semantics for first-order logic, for example, where no models have proper classes for domains. Similarly, if we built our domains out of hereditarily finite sets we would have no model depicting an infinite universe. These are not problems with representational semantics per se, but with our choice of modelling techniques. (Etchemendy, 2008, p. 26, fn. 19)

Field may insist that the argument fails to convince: one might object, as he does, that standard modelling techniques should *not* be chosen, since they do

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terms, if we wish to study the logic of some other terms. But this no longer presupposes the availability of an absolute distinction between what’s logical and what isn’t.

<sup>29</sup>For further discussion, see Field (1991, p. 7) and McGee (1992b).

not allow us to adequately model actual truth. But Etchemendy could retort that his arguments are aimed at (or should be seen as) challenging the conceptual equivalence between logical truth and *truth-in-all-structures*, a notion that is in turn modelled by the notion of truth-in-a-model. For this reason, one should not overstate a model's inability to represent a (non-necessarily set-theoretic) structure, and hence the universe.<sup>30</sup>

A second possible reaction would be to let *proper classes*, which contain all sets, be the domains of our models. But Field persuasively argues that this won't do either (Field, 1991, p. 4). As he observes, the problem surfaces again at the next level, since there is no class of all classes. If models cannot represent possibilities containing all sets, classes cannot represent possibilities containing all sets *and all classes*.

The issue here is intimately related to the problem of *absolute generality*.<sup>31</sup> More precisely: the reliability problem, in its original formulation, only arises if we accept that we can quantify over absolutely everything. The assumption is natural enough: after all, how can 'all' fail to mean *all*? Yet, the assumption can be argued to lie at the heart of the set-theoretic, and perhaps even semantic, paradoxes.<sup>32</sup> Consider for instance the Russell set  $r$  of all and only those sets in  $D$  that aren't members of themselves:  $\forall x(x \in r \leftrightarrow x \notin x)$ , where ' $x$ ' ranges over sets in  $D$ . If  $D$  contains *everything*, including  $r$ , we can then derive  $r \in r \leftrightarrow r \notin r$ , a contradiction. However, if  $r$  lies *outside*  $D$ , i.e. if there is at least one object that is not included in our would-be all inclusive domain, this last step is invalid, and no contradiction arises. To be sure, rejecting absolute generality is a controversial, and problematic move.<sup>33</sup> Here we simply observe that, if

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<sup>30</sup>For more discussion on this point, see MacFarlane (2000b) and §5 below.

<sup>31</sup>For an excellent introduction to absolute generality, see e.g. Rayo and Uzquiano (2006).

<sup>32</sup>See e.g. Simmons (2000), Glanzberg (2004), Shapiro and Wright (2006) and references therein.

<sup>33</sup>To begin with, it is even unclear whether absolute generality can be coherently rejected. Saying that no sentence will ever quantify over everything won't do, since either 'everything' means *absolutely everything*, an incoherent notion if absolute generality is rejected, or 'everything' is itself restricted, in which case the doctrine doesn't state what is meant to state. See Williamson (2003, §§ V-VI) and Button (2010) for a response to the objection. Moreover, the assumption that sets are *indefinitely extensible* is also problematic. Here is Field:

One natural way to defend the indefinite extensibility of ontology is to argue that mathematical entities are fictions, and that it's always possible to extend any fiction. But (i) finding a way to fruitfully extend a mathematical fiction is not a routine matter; and (ii) when working within a given fiction of any kind that we know how to clearly articulate, it makes sense to talk unrestrictedly of all mathematical objects. (Field, 2008, p. 35)

such a move is made, our current model-theoretic techniques need not be inadequate: a generality relativist may maintain that our conception of the universe's domain can always be represented by a *set*, even if such a conception can always be expanded so as to include a *larger* set. In Michael Dummett's term, our conception of set would then be *indefinitely extensible*.<sup>34</sup>

A third response to the reliability problem is to point out that, for first-order languages, we have a *guarantee* of extensional adequacy. For one thing, following Kreisel (1967), it can be argued that the Completeness Theorem guarantees that the two notions of intuitive and model-theoretic validity extensionally coincide. This is Kreisel's so-called *squeezing argument*.<sup>35</sup> If our proof-system is intuitively sound, we know that, if  $\alpha$  is derivable from  $\Gamma$ , then the argument from  $\Gamma$  to  $\alpha$  is intuitively valid, in the sense of preserving truth in all structures. But if  $\langle \Gamma \therefore \alpha \rangle$  preserves truth in all structures, it preserves truth in all *model-theoretic* structures, and is therefore model-theoretically valid. By the Completeness Theorem, we can conclude that  $\alpha$  is derivable from  $\Gamma$ . In short:  $\langle \Gamma \therefore \alpha \rangle$  is model-theoretically valid iff it is intuitively valid. For another, the Löwenheim-Skolem theorem assures us that, if an argument formulated in a first-order language has a counterexample (i.e. if there is a way to make its premises true and its conclusion false), then there is a model in which it can be represented (i.e. if there is a model that makes its premises true and its conclusion false). For first-order languages, truth-in-all-models indeed implies truth.

This response is correct, as far as it goes. But it also has limitations. As we mentioned in §1, neither the Completeness Theorem nor the Löwenheim-Skolem theorems hold for higher-order logics. Thus, Field alleges that "*it is only by virtue of an "accident of first order logic" that the Tarski account of consequence gives the intuitively desirable results*" (Field, 1991, p. 4; Field's italics). Similarly,

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In short: Field takes indefinite extensibility to be an ontological doctrine, to the effect that the mathematical universe can always be expanded, and assumes that a natural way to make sense of "the indefinite extensibility of ontology" is to adopt a *fictionalist* account of mathematics, where the fiction is constantly expanded. Generality relativists need not share either assumption, however. For one thing, the extensibility argument (Russell's Paradox above) indicates *exactly* how domains can be extended, without needing to resort to a fictionalist account of mathematics. For another, they may object that indefinite extensibility need not be an *ontological* doctrine. The extensibility argument can simply (and more plausibly) be taken to show that we can't *think* of absolutely everything, and it would be a mistake to infer ontological conclusions from this epistemological claim.

<sup>34</sup>See Dummett (1993).

<sup>35</sup>For discussion of the argument, see e.g. Field (1991), Field (2008), Etchemendy (1990), Hanson (1997), Smith (2011).

Etchemendy writes that

in the absence of a completeness theorem, our only legitimate conclusion is that *either* the deductive system is incomplete, *or* the Tarskian definition has overgenerated, *or possibly both*. (Etchemendy, 2008, p. 285)

He further argues that it will not do to insist that second-order logic isn't logic, since "second-order languages, like all languages, have a logical consequence relation" and "the idea that studying the logic of these languages is somehow not the business of logic is hardly a supportable conclusion" (p. 277). However, while Etchemendy is correct to point out that the claim "that *second-order logic is not logic ...* has to count as one of the more surprising and implausible conclusions of recent philosophy" (p. 286), the model-theorist need not be committed to such a claim in order to defend model-theoretic accounts of consequence from extensionality concerns. Examples of overgeneration such as CH need not show that higher-order logics are themselves lacking: the culprit may well be their standard model-theoretic interpretation.<sup>36</sup> For instance, it is well-known that second-order logic can be interpreted as a multi-sorted first-order logic for which the standard metalogical results of first-order logic hold.<sup>37</sup>

Responses to Etchemendy's accusations of conceptual and extensional inadequacy are legion. Beyond the articles we've already cited (McGee, 1992b; Priest, 1995; Hanson, 1997; Shapiro, 1998), we limit ourselves to citing the work of Mario Gomez-Torrente, Greg Ray and Gila Sher. Both Gomez-Torrente and Greg Ray criticise the historical component of Etchemendy's critique and defend Tarski's Thesis, that logical truth is truth in all models, from Etchemendy's objections (Ray, 1996; Gomez-Torrente, 1996, 1998, 2008). Sher argues that logical consequence ought not to satisfy an a priority constraint. Moreover, in her view, logical consequence can be defined in model-theoretic terms, on an understanding of 'model' that is neither interpretational nor representational, so that, she claims, Etchemendy's criticism doesn't apply (Sher, 1991, 1996, 2001, 2008).

At least four contributions to the present volume deal with issues related to model-theoretic accounts of consequence. In "Formality in Logic: From Logical Terms to Semantic Constraints", Gil Sagi defends a model-theoretic

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<sup>36</sup>See Etchemendy (1990, pp. 123-124).

<sup>37</sup>For details, see Shapiro (1991).

account of consequence according to which logic is formal but logical terms—irrespective of whether we think there is precisely one such class, or we adopt a relativistic approach, and assume there can be more than one—are not central for defining consequence. In “Logical Indefinites”, Jack Woods generalises the Tarskian permutation-invariance account of logicity to logical indefinites, such as indefinite descriptions, Hilbert’s  $\epsilon$ , and abstraction operators. In “Validity and actuality”, Vittorio Morato compares two different model-theoretic definitions of validity for modal languages. Finally, in “A note on logical truth” Corine Besson argues that instances of logical truths need not be themselves logically true. Against this backdrop, her paper offers a way to deal with the existential commitments of classical logic that does not resort to free logics.

### 3 Proof-theoretic consequence

Let us now turn to the proof-theoretic account, as described by PT. According to this, consequence is identified with *deducibility*: an argument  $\langle \Gamma \therefore \alpha \rangle$  is valid iff there exists a derivation of  $\alpha$  from  $\Gamma$  each of whose steps is gap-free and intuitively compelling. The account is formal, insofar as logical consequence is identified with derivability in a system of rules of a certain form. It also doesn’t obviously overgenerate, provided we choose logical rules that are gap-free and intuitively compelling. It may address the problem of extending knowledge, or justified belief, by deduction, on the assumption that simple inference rules are entitling—either because they are *reliable* (Rumfitt, 2008) or because they are constitutive of our understanding of the logical expressions (Peacocke, 1987; Dummett, 1991; Peacocke, 1992; Boghossian, 2003).

The latter view is in effect a form of *logical inferentialism*, a doctrine which is often associated with proof-theoretic accounts of consequence. According to this, the meaning of a logical expression  $\$$  is fully determined by the basic rules for its correct use—in a natural deduction system,  $\$$ ’s introduction and elimination rules (I- and E-rules, for short), and to understand  $\$$  is to master, in some way to be specified, such basic rules.<sup>38</sup> With these assumptions on board, the logical inferentialist is in a position to argue that, since to (be disposed to) infer according to  $\$$ ’s I- and E-rules is at least a necessary condition for understanding  $\$$ , we’re thereby *entitled* to infer according to such rules, so that when we infer

<sup>38</sup>See e.g. Gentzen (1934), Popper (1947), Kneale (1956), Dummett (1991), Tennant (1997).



according to them, if the premises are known, or justifiably believed, then so is the conclusion.<sup>39</sup> Finally, the a priority box can also be argued to be ticked, on the assumption that knowledge by deduction is a byproduct of our linguistic competence (more specifically: of our understanding of the logical vocabulary), which is standardly thought to be *a priori*.<sup>40</sup>

Proof-theoretic accounts of consequence are sometimes too quickly dismissed. Thus, Field writes that “proof-theoretic definitions proceed in terms of some definite proof procedure”, and laments that “it seems pretty arbitrary which proof procedure one picks” and “it isn’t very satisfying to rest one’s definitions of fundamental metalogical concepts on such highly arbitrary choices” (Field, 1991, p. 2). Etchemendy similarly observes that “the intuitive notion of consequence cannot be captured by any *single* deductive system” (Etchemendy, 1990, p. 2), since the notion of consequence is neither tied to any particular language, nor to any particular deductive system.

This objection may be correct as far as it goes. But it is somewhat off target. Defendants of proof-theoretic accounts of consequence typically refrain from equating logical consequence with derivability in a single deductive system: this is the main lesson they draw from (Rosser’s strengthening of) Gödel’s First Incompleteness Theorem, that any deductive system containing enough arithmetic is incompletable, if consistent (see e.g. Prawitz, 1985, p. 166).<sup>41</sup> Perhaps for this reason, Etchemendy considers the idea that consequence be identified with “derivability in some deductive system or other”, but argues that this won’t work either, since “any sentence is derivable from any other in *some* such system” (Etchemendy, 1990, p. 2). Consider, for instance, Arthur Prior’s infamous binary connective *tonk* (see Prior, 1960):

$$\text{tonk-I} \frac{\alpha}{\alpha \text{ tonk } \beta} \quad \text{tonk-E} \frac{\alpha \text{ tonk } \beta}{\beta} .$$

<sup>39</sup>The account is defended in Boghossian (2003, 2012). See Prawitz (2012) for a more recent proposal along similar lines.

<sup>40</sup>Inferentialists *identify* knowledge of  $\$$ -I and  $\$$ -E with being disposed to use  $\$$  according to its I- and E-rules. For a recent criticism of the dispositionalist account of understanding, see Besson (2012). For a response, see Murzi and Steinberger (2013). An (influential) criticism by Timothy Williamson will be discussed in the main text below.

<sup>41</sup>Although the Gödel sentence of any theory  $T$  to which the First Incompleteness Theorem applies is not itself provable in  $T$ , one can nevertheless informally prove it *outside* of  $T$ . One can then formalize this informal proof in an extended theory  $T'$ , which will in turn have its own Gödel sentence. And so on. As Michael Dummett puts it: “the class of [the] principles [of proof] cannot be specified once and for all, but must be acknowledged to be an indefinitely extensible class” (Dummett, 1963, p. 199). See also Myhill (1960).

If transitivity holds, and if we can prove at least one formula, it is easy to see that these rules allow us to prove *any* formula in the language, thereby yielding triviality and, provided the language includes negation, inconsistency. Logical consequence had better not be identified in a system including *these* rules! Etchemendy concludes that

at best we might mean by “consequence” derivability in some *sound* deductive system. But the notion of soundness brings us straight back to the intuitive notion of consequence. (Etchemendy, 1990, pp. 2-3)

In short: proof-theoretic definitions of consequence are either arbitrary or hopelessly circular. We disagree.

Etchemendy is right in assuming that, if consequence is defined as derivability in some system or other, one will need to provide criteria for selecting admissible systems. But his argument is still too quick: soundness is not the only available criterion for selecting admissible rules. More specifically, it is not the criterion inferentialists typically resort to when confronted with the issue of selecting *logical* rules. Since Gerhard Gentzen and Nuel Belnap’s seminal work (Gentzen, 1934; Belnap, 1962), inferentialists impose *syntactic* constraints on admissible logical rules: both *local* ones, such as *proof-theoretic harmony*, concerning the form of admissible introduction and elimination rules, and *global* ones, such as *conservativeness*, concerning the properties of the formal systems to which they belong. For reasons of space, we exclusively focus on local criteria.

Consider the standard introduction and elimination rules (thereafter, I- and E-rules respectively) for  $\wedge$ :

$$\wedge\text{-I} \frac{\alpha \quad B}{\alpha \wedge \beta} \quad \wedge\text{-E} \frac{\alpha \wedge \beta}{\alpha} \frac{\alpha \wedge \beta}{\beta} .$$

To be sure, these rules are intuitively sound. But there is more: unlike  $\text{tonk-I}$  and  $\text{tonk-E}$ , they are perfectly balanced in the following sense: what is required to introduce statements of the form  $\alpha \wedge \beta$ , viz.  $\alpha$  and  $\beta$ , perfectly matches what we may infer from such statements. In Michael Dummett’s term, the I- and E-rules for  $\wedge$  are in *harmony* (Dummett, 1973a, 1991). Intuitively, a pair of I- and E-rules is harmonious if the E-rules are neither too strong (they don’t prove too much), nor too weak (they don’t prove too little). For instance,  $\text{tonk}$ ’s E-rule is clearly too strong: it allows to infer from  $\text{tonk}$ -sentences way more than was required

to introduce them in the first place.<sup>42</sup>

This intuitive idea can be spelled out in a number of ways. Dummett (1991, p. 250) and Prawitz (1974, p. 76) define harmony as the possibility of eliminating *maximum formulae* or *local peaks*, i.e. formulae that occur both as the conclusion of an I-rule and as the major premise of the corresponding E-rule (see also Prawitz, 1965, p. 34). The following reduction procedure for  $\rightarrow$ , for instance, shows that any proof of  $B$  via  $\rightarrow$ -I and  $\rightarrow$ -E can be converted into a proof from the same or fewer assumptions that avoids the unnecessary detour through the introduction and elimination of  $A \rightarrow B$ :

$$\begin{array}{c} \Gamma_0, [A]^i \\ \Pi_0 \\ \frac{B}{A \rightarrow B} \\ \frac{A \rightarrow B}{B} \end{array} \quad \begin{array}{c} \Gamma_1 \\ \Pi_1 \\ A \end{array} \quad \rightsquigarrow_r \quad \begin{array}{c} \Gamma_1 \\ \Pi_1 \\ \Gamma_0, A \\ \underbrace{\hspace{2cm}} \\ \Pi_0 \\ B \end{array}$$

where  $\rightsquigarrow_r$  reads ‘reduces to’. Dummett (1991, p. 250) calls this *intrinsic* harmony. He correctly points out, though, that it only prevents elimination rules from being stronger than the corresponding introductions, as in the case of Prior’s tonk. It does not rule out the possibility that they be, so to speak, too weak (see Dummett, 1991, 287).<sup>43</sup> A way to ensure that E-rules be strong enough is to require that they allow us to *reintroduce* complex sentences, as shown by the following *expansion*:

$$\begin{array}{c} \Pi \\ A \wedge B \end{array} \quad \rightsquigarrow_e \quad \frac{\frac{\Pi}{A \wedge B} \wedge\text{-E}}{A} \quad \frac{\frac{\Pi}{A \wedge B} \wedge\text{-E}}{B} \wedge\text{-I} \\ A \wedge B$$

where  $\rightsquigarrow_e$  reads ‘can be expanded into’. This shows that any derivation  $\Pi$  of  $A \wedge B$  can be expanded into a longer derivation which makes full use of both  $\wedge$ -I and  $\wedge$ -E.

Accordingly, a pair of I- and E-rules for a constant  $\$$  can be taken to be *harmonious* iff there exists both reduction and expansion procedures for  $\$$ -I and

<sup>42</sup>An harmonious rule of tonk-E given tonk-I would rather allow us to eliminate  $A$  from  $A$  tonk  $B$ , not  $B$ .

<sup>43</sup>For instance, a connective  $\odot$  satisfying the standard I-rules for  $\wedge$  but only one of its E-rules would be intrinsically harmonious, and yet intuitively disharmonious: its E-rule would not allow us to infer from  $\alpha \odot \beta$  all that was required to introduce  $\alpha \odot \beta$  in the first place.

\$-E.<sup>44,45</sup> It might then be argued that logical I- and E-rules must at least be harmonious, and that logical consequence coincides with derivability in any system satisfying harmony, and perhaps other syntactic constraints.<sup>46,47</sup> *Pace* Etchemendy, an account along these lines would not obviously presuppose a prior grasp of the notion of logical consequence.

In any event, logical inferentialists need not identify logical consequence with derivability in a given system.<sup>48</sup> They may provide a proof-theoretic account

<sup>44</sup>See e.g. Davies and Pfenning (2001) and Francez and Dyckhoff (2009). Read (2010) dismisses accounts of harmony which require a reducibility requirement, on the grounds that they deem as harmonious connectives such as  $\odot$ . However, while Read is right in thinking that reducibility alone isn't sufficient for harmony, it doesn't follow from this observation that it is not necessary.

<sup>45</sup>Thus, the above reduction and expansion procedures for  $\rightarrow$  show that the standard I- and E-rules for  $\rightarrow$ , respectively, Conditional Proof and *modus ponens*, are harmonious.

<sup>46</sup>One common motivating thought behind the requirement of harmony is that *logic is innocent*: it shouldn't allow us to prove atomic sentences that we couldn't otherwise prove (Steinberger, 2009). A different motivating thought is that I-rules determine, in principle, necessary and sufficient conditions for introducing complex sentences. The necessity part of this claim is in effect Dummett's Fundamental Assumption, that "[i]f a statement whose principal operator is one of the logical constants in question can be established at all, it can be established by an argument ending with one of the stipulated I-rules" (Dummett, 1991, p. 252). The Assumption lies at the heart of the proof-theoretic accounts of validity to be introduced in the main text below. To see that it justifies a requirement of harmony, we may reason thus. Let  $\mathcal{CG}[\alpha]$  be the canonical grounds for a complex statement  $\alpha$ , as specified by its I-rules. Then, by the Fundamental Assumption,  $\beta$  follows from  $\mathcal{CG}[\alpha]$  if and only if  $\beta$  follows from  $\alpha$  itself. For suppose  $\beta$  follows from  $\alpha$ . Since  $\alpha$  also follows from  $\mathcal{CG}[\alpha]$ ,  $\beta$  itself follows from  $\mathcal{CG}[\alpha]$ . Now suppose  $\beta$  follows from  $\mathcal{CG}[\alpha]$ . Assume  $\alpha$ . By the Fundamental Assumption,  $\mathcal{CG}[\alpha]$  itself follows. Hence, on our assumption that  $\beta$  follows from  $\mathcal{CG}[\alpha]$ , we may conclude  $\beta$ , as required. In short: it is a consequence of the Fundamental Assumption that complex statements and their grounds, as specified by their I-rules, must have the same set of consequences. I- and E-rules must be in *harmony* between each other: one may infer from a complex statement nothing more, and nothing less, than that which follows from its I-rules. For a discussion and criticism of the Fundamental Assumption, see Dummett (1991, Ch. 12), Read (2000), Murzi (2010), and Francez and Murzi (2014).

<sup>47</sup>It should be mentioned that harmony may not be a sufficient condition for logicity. Read (2000) discusses an example of an intuitively harmonious and yet inconsistent connective,  $\bullet$ , with the following I- and E-rules:

$$\begin{array}{c} [\bullet]^i \\ \vdots \\ \bullet\text{-I, } i \frac{\perp}{\bullet} \end{array} \quad \bullet\text{-E} \frac{\bullet \quad \bullet}{\perp} .$$

The example is controversial, however, since  $\bullet$  is not harmonious if a reducibility requirement is built into the definition of harmony.

<sup>48</sup>Indeed, they may even adopt a *model-theoretic* account of validity, on the assumption that I- and E-rules determine the truth-conditions of the logical operators. For suppose  $\wedge$ -I and  $\wedge$ -E are truth-preserving. Then,  $\alpha \wedge \beta$  is true iff both  $\alpha$  and  $\beta$  are, i.e.  $\wedge$  denotes the truth-function it does. Following Hodes (2004) and MacFarlane (2005), inferentialists may thus distinguish between the *sense* of a logical constant, whose grasp is constituted by a willingness to infer according to

of validity, albeit a more complex one than the one considered, and criticised, by Field and Etchemendy. Following Prawitz (1985) and Prawitz (2006), let an argument, a step-by-step deduction, be *closed* if it has no undischarged assumptions and no unbound variables, and let us say that it is *open* otherwise. Let an *immediate subargument* of a closed argument  $\Pi$  be an argument for a premise of  $\Pi$ 's last inference rule, and let us call an argument *canonical* if it ends with an introduction rule, and it contains valid arguments for its premises.<sup>49</sup> Finally, let us assume that a set of justification procedures  $\mathcal{J}$  for transforming non-canonical arguments into canonical arguments is available: one can always reduce arguments ending with an application of an E-rule to arguments whose last step is taken into accordance with one of the I-rules of the main logical operator of the argument's conclusion.<sup>50</sup>

With these assumptions in place, the validity of an argument  $\Pi$  with respect to its set of justification procedures  $\mathcal{J}$  may be defined as follows (Prawitz, 1985, pp. 164-165). If  $\Pi$  is a *closed* argument,  $\langle \Pi, \mathcal{J} \rangle$  is valid if and only if either (i)  $\Pi$  is in canonical form and each immediate subargument  $\Pi'$  of  $\Pi$  is valid with respect to  $\mathcal{J}$ , or  $\Pi$  is not in canonical form, but it can be transformed into an argument for which (i) holds, by successive applications of the operations in  $\mathcal{J}$ . If  $\Pi$  is a *open* argument, on the other hand,  $\langle \Pi, \mathcal{J} \rangle$  is valid if and only if all closed instances  $\Pi'$  of  $\Pi$  that are obtained by substituting for free parameters

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its basic introduction and elimination rules, and its *referent*, e.g. the truth-function it denotes. Indeed, they may have independent reasons for doing so. Classically, for instance,  $\alpha \vee \beta$  (" $\alpha$  or  $\beta$ ") and  $\alpha \ddagger \beta$  (" $\alpha$  and  $\beta$ ") have the same truth-conditions. Yet,  $\vee$  and  $\ddagger$  intuitively differ in some aspect of meaning (MacFarlane, 2005, § 6.2). Inferentialists may argue that  $\vee$  and  $\ddagger$  have different senses, which are specified by their different I- and E-rules. Closer to our present concerns, they may take the truth-conditions of the logical operators to select the class of *admissible models*—models in terms of which validity can be defined in model-theoretic terms. Thus Vann McGee: "[t]he rules of inference determine truth-conditions. The truth-conditions together [...] determine the logical consequence relation" (McGee, 2000, p. 72). For discussion of whether rules can determine truth-conditions in this sense, see e.g. Carnap (1943), Smiley (1996), Rumfitt (2000), Garson (2001), Garson (forthcoming), Garson (2013), Murzi and Hjortland (2009) and Woods (2013).

<sup>49</sup>Thus, for instance, the arguments below are both canonical, but the argument on the left is open, since it has an undischarged assumption,  $\alpha$ , and the argument on the right is closed, since it does not contain undischarged assumptions or unbound variables:

$$\frac{\alpha}{\alpha \vee \beta} \qquad \frac{\begin{array}{c} [\alpha] \\ \mathcal{D} \\ \beta \end{array}}{\alpha \rightarrow \beta},$$

where  $\mathcal{D}$  is a derivation.

<sup>50</sup>This assumption is effectively equivalent to Dummett's Fundamental Assumption.

closed terms and for free assumptions closed arguments for the assumptions, valid with respect to an extension  $\mathcal{J}'$  of  $\mathcal{J}$ , are valid with respect to  $\mathcal{J}'$ . In short: the validity of whole of logic is reduced to the primitive validity of a small set of intuitively valid inference rules.

Since the foregoing definition makes no reference to particular logical systems, Etchemendy's and Field's objections do not apply (Prawitz, 1985, p. 166).<sup>51</sup> Still, proof-theoretic accounts of consequence, and inferentialist approaches to logic more generally, face a number of—more serious—objections.

To begin with, while the rules of intuitionistic logic are harmonious, standard formalizations of classical logic typically aren't (Dummett, 1991; Prawitz, 1977; Tennant, 1997). For instance, the classical rule of double negation elimination

$$\text{DN} \frac{\neg\neg\alpha}{\alpha}$$

is not in harmony with the standard rule of negation introduction:

$$\begin{array}{c} [\alpha]^i \\ \vdots \\ \neg\text{I}, i \frac{\perp}{\neg\alpha} . \end{array}$$

The harmonious rule of negation elimination is the following *intuitionistic* rule:

$$\neg\text{-E} \frac{\alpha \quad \neg\alpha}{\perp} .$$

This rule, unlike its classical counterpart, allows us to infer from  $\neg\alpha$  precisely what was required to assert  $\neg\alpha$ : a derivation of  $\perp$  from  $\alpha$ . But, then, double negation elimination is left, so to speak, in the cold. Intuitionists such as Dummett, Prawitz and Tennant have taken this observation to show that classical rules such as double negation elimination are not logical, and that the logical rules we should adopt are those of *intuitionistic logic*, i.e. classical logic without the Law of Excluded Middle ( $\alpha \vee \neg\alpha$ ), double negation elimination and other equivalent rules.

This argument is also problematic, however. For while it is true that *standard* axiomatisations of classical logic are not harmonious, a number of non-standard axiomatisations *are* harmonious. In particular, classical logic can be shown to be

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<sup>51</sup>For a critical overview of proof-theoretic accounts of consequence, see also Schroeder-Heister (2006). For an different and original proof-theoretic approach, see Schroeder-Heister (2012).

as proof-theoretically respectable as intuitionistic logic provided rules are given both for asserting and for *denying* complex statements (Rumfitt, 2000; Incurvati and Smith, 2010), where denial is taken to be a primitive speech act distinct from the assertion of a negated sentence (Parsons, 1984; Smiley, 1996). The negation rules for classical negation are then as harmonious as the intuitionistic ones: they allow one to deny  $\neg\alpha$  given the assertion of  $\alpha$  and *vice versa*, and to deny  $\alpha$  given the assertion of  $\neg\alpha$  and *vice versa*. Alternatively, harmonious axiomatisations of classical logic can be given in a *multiple conclusions* setting (Read, 2000; Cook, 2005). Sequent calculi axiomatisations of classical logic are exactly alike, except that classical sequent calculi allow for sequents with multiple premises *and* multiple conclusions. In turn, such sequents can be plausibly interpreted as saying that one may not assert all the antecedents and deny all the succedents, where, again, assertion and denial are both primitive speech acts (Restall, 2005).<sup>52</sup>

A second objection, recently advanced by Timothy Williamson, aims at undermining the inferentialist account of understanding, that to understand a constant  $\$$  is to be disposed to infer according to  $\$-I$  and  $\$-E$  (Williamson, 2003, 2006, 2008, 2012). The objection simply consists in pointing to the existence of *deviant logicians*: competent English speakers who reject some standardly accepted basic rules. For instance, according to Vann McGee, Niko Kolodny and John MacFarlane, *modus ponens* is subject to counterexamples, and hence invalid (McGee, 1985; Kolodny and MacFarlane, 2010). Likewise, Field, Priest and others have argued that, in view of Curry's Paradox, Conditional Proof should be rejected.

If we understand these theorists as proposing a change of our inferential practice, and if at the same time we assume that their understanding of 'if' is exactly like ours, we are effectively presented with living counterexamples to the inferentialist account of understanding. The issue is complex, and we cannot hope to settle it here. We limit ourselves to observe that inferentialists could (and should) reject Williamson's assumption that the theorists in question have exactly the same understanding of 'if' as the majority of English speakers (even if they reject instances of basic rules such as *modus ponens*). Intuitionists, for instance, perfectly know the meanings of *both* classical and intuitionistic negation. They might even concede that negation in English has largely a classi-

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<sup>52</sup>For a technical introduction to multiple-conclusion logics, see Shoesmith and Smiley (1978). For a recent criticism, see Steinberger (2011).

cal meaning, and that in most contexts the difference between the intuitionist and the classical meaning will go unnoticed, so that it *seems* right to attribute to an intuitionist an understanding of classical negation. However, it doesn't follow from this that intuitionistic and classical negation have the same meaning: on most accounts of intuitionistic and classical semantics, they don't. Pace Williamson, it seems to us that intuitionists possess two concepts of negation, but choose, on theoretical grounds, to use only one.<sup>53</sup>

Williamson might appeal to a form of *semantic externalism*, and retort that it isn't up to individual speakers to decide what English words mean (Williamson, 2006, 2008). But the inferentialist will now object that individual speakers can still modify the meaning of words *in their own idiolect*, a seemingly coherent notion the incoherence of which is presupposed by Williamson's argument. They will insist that Williamson is committed to crediting intuitionists *exclusively* with an understanding of classical negation, vitiated by a bizarre tendency not to assert instances of the Law of Excluded Middle. Yet, it seems more appropriate to rather credit intuitionists (also) with an understanding of intuitionistic negation, which *explains* why they refrain from asserting such instances.<sup>54,55</sup> Similarly for Williamson's original case involving 'if' and other logical expressions.

Francesco Paoli and Ole Hjortland's contributions fall squarely within the proof-theoretic tradition. Both papers deal with the problem of making sense, within an inferentialist framework, of *structural rules*, i.e. rules in which no logical vocabulary figures, such as Structural Contraction

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<sup>53</sup>Prawitz (1977) and Dummett (1991) actually deny that classical negation has a coherent meaning. But this seems to be an overstatement (Rumfitt, 2000; Read, 2000; Murzi, 2010).

<sup>54</sup>For more objections to the inferentialist account of understanding, see Casalegno (2004). For a recent inferentialist reply to both Casalegno and Williamson, see Boghossian (2012).

<sup>55</sup>It might still be maintained that, even setting aside the difficulties faced by the inferentialist account of logic, the proof-theoretic account of logic itself faces problems of its own. One objection has it that the account *undergenerates*. For instance, it might be argued that, while Fermat's Last Theorem is intuitively a consequence of the axioms of second-order Peano Arithmetic, there is no guarantee that there exists a canonical proof of the theorem, so that proof-theorists may not be in a position to say that the theorem follows from the axioms of second-order Peano Arithmetic, contrary to intuition (Moruzzi and Zardini, 2007). Another objection would target the very notion of a *canonical argument*. It may be argued that the Prawitz's definition of consequence requires a problematic distinction between canonical and non-canonical ways of establishing *atomic* sentences. But are we to canonically establish, say, that Silvio Berlusconi has won the 2010 regional elections in Italy? Neither objection seems particularly damaging. The first objection is highly speculative: while it is true that we don't have positive reasons for thinking that there isn't a canonical proof of Fermat's Last Theorem in second-order Peano Arithmetic, we don't have positive reasons for thinking that there isn't such a proof either. As for the second, proof-theorists may be able to circumvent the problem by simply stipulating that all acceptable ways of establishing atomic statements count as canonical.



$$\text{SContr} \frac{\Gamma, \alpha, \alpha \vdash \beta}{\Gamma, \alpha \vdash \beta}.$$

If proof-theoretic accounts of consequence and of the meaning of logical expressions are only able to justify I- and E-rules, isn't there a lacuna in such accounts? In "Verbal disputes in logic: against minimalism for logical connectives", Ole Hjortland attacks Paoli's *minimalism for logical constants*, the idea that logical expressions have two kinds of meaning: a *local* one determined by their I- and E-rules (or left and right rules, in a sequent setting), and a *global* one, which is identified with the totality of the theorems and rules one can prove given the I- and E-rules for the relevant constant *together with the structural rules of the logic* (Paoli, 2003). In "Semantic minimalism for logical constants", Paoli responds to Hjortland's objections. Finally, building on D'Agostino and Floridi (2009), Marcello D'Agostino provides in "Analytic inference and the informational meaning of the logical operators" an informational semantics for the logical operators consistent with what he calls a Strong Manifestability Requirement, to the effect that "any agent who grasps the (informational) meaning of the logical operators should be able to tell, in practice and not only in principle, whether or not s(he) holds the information that a given complex sentence is true, or the information that it is false, or neither of the two".<sup>56</sup> Among other things, the account aims at vindicating the thought that logically valid inferences are *analytically* valid, i.e. valid in virtue of the meaning of the logical expressions.<sup>57</sup>

## 4 Truth-preservation and Semantic Paradox

We now turn to the question whether we can consistently assert the seeming truism that valid arguments preserve truth—arguably the driving thought underpinning the model-theoretic account, at least in its representational interpretation. It turns out that focusing on models obscures to view an important

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<sup>56</sup>The need for a manifestability requirement for understanding has been famously defended by Dummett in a number of works. See e.g. Dummett (1973a,b, 1976, 1978, 1991). For a defence of a weaker manifestability principle, see Prawitz (1977). For a more recent defence, see Tennant (1997). For a recent criticism of the principle (and on revisionary arguments based on it), see Byrne (2005) and Murzi (2012).

<sup>57</sup>The question whether semantics should be informational is intimately tied to the question whether SContr above holds—an issue we'll deal with in the next section. For the time being, we simply observe that, if the information that  $\alpha, \alpha$  is distinguished from the information that  $\alpha$ , SContr no longer obviously holds (Slaney, 1990; Mares and Paoli, 2012).

fact: that, on commonly held assumptions, the claim that valid arguments preserve truth is inconsistent, and indeed trivial.

Our starting point, then, is the ‘only if’ part of Modal:

(VTP)  $\langle \Gamma \therefore \alpha \rangle$  is valid only if, necessarily, if all  $\gamma \in \Gamma$  are true, so is  $\alpha$ .

In short: if an argument is valid, then, if all its premises are true, then its conclusion is also true. Intuitive as it may seem, this claim, on natural enough interpretations of ‘if’ and ‘true’, turns out to be highly problematic. Both Field and Jc Beall have observed that VTP almost immediately yields absurdity via Curry-like reasoning in most logics—call this the Triviality Argument (Field, 2008; Beall, 2007, 2009). Moreover, Field has argued that, by Gödel’s Second Incompleteness Theorem, any theory that declares all valid arguments truth-preserving must be inconsistent—call this the Unprovability of Consistency Argument (Field, 2006, 2008, 2009b,a). Either way, we can’t coherently affirm that valid arguments preserve truth, or so the thought goes.<sup>58</sup>

Both arguments require two main ingredients: that the conditional occurring in VTP detaches, i.e. satisfies *modus ponens*, and the *naïve view of truth*, viz. that (at the very least) the truth predicate must satisfy the (unrestricted) T-Scheme

$$(T\text{-Scheme}) \text{Tr}(\ulcorner \alpha \urcorner) \leftrightarrow \alpha,$$

where  $\text{Tr}(\dots)$  expresses truth, and  $\ulcorner \alpha \urcorner$  is a name of  $\alpha$ . Both assumptions lie at the heart of the leading contemporary *revisionary approaches* to semantic paradox.<sup>59</sup> For simplicity’s sake, let us focus on arguments with only one premise. We can then try to affirm VTP in the object-language, by introducing a predicate  $\text{Val}(x, y)$  which intuitively expresses that the argument from  $x$  to  $y$  is valid. Then, VTP may be naturally represented thus (see Beall, 2009):

$$(V0) \text{Val}(\ulcorner \alpha \urcorner, \ulcorner \beta \urcorner) \rightarrow (\text{Tr}(\ulcorner \alpha \urcorner) \rightarrow \text{Tr}(\ulcorner \beta \urcorner)).^{60}$$

<sup>58</sup>Shapiro (2011) labels the claim that VTP must be given up the *Beall-Field thesis*.

<sup>59</sup>These include recent implementations (e.g. Brady, 2006; Field, 2003, 2007, 2008; Horsten, 2009) of the *paracomplete* approach inspired by Martin and Woodruff (1975) and Kripke (1975), as well as *paraconsistent* approaches (see e.g. Asenjo, 1966; Asenjo and Tamburino, 1975; Priest, 1979, 2006a,b; Beall, 2009). Paracomplete approaches solve paradoxes such as the Liar by assigning the Liar sentence a value in between truth and falsity, thus invalidating the Law of Excluded Middle. Paraconsistent approaches solve the Liar by taking the Liar sentence to be both true and false, avoiding absurdity by invalidating the classically and intuitionistically valid principle of *Ex Contradictione Quodlibet*.

<sup>60</sup>Strictly speaking, this should be expressed as a universal generalisation on codes of sentences, but, for the sake of simplicity, we won’t bother.

As Field and Beall point out, V0 entails absurdity, based on principles accepted by standard revisionary theorists (Field, 2006; Beall, 2007; Field, 2008; Beall, 2009). Simplifying a little, one notices that V0 entails

$$(V1) \text{Val}(\ulcorner \alpha \urcorner, \ulcorner \beta \urcorner) \rightarrow (\alpha \rightarrow \beta).$$

Next, one assumes that one's semantic theory  $T$  implies the validity of a single-premise version of the *modus ponens rule*:

$$(VMP) \text{Val}(\ulcorner (\alpha \rightarrow \beta) \wedge \alpha \urcorner, \ulcorner \beta \urcorner).$$

Hence V1 in turn entails the *modus ponens axiom*:

$$(MPA) (\alpha \rightarrow \beta) \wedge \alpha \rightarrow \beta.$$

But MPA generates Curry's Paradox, i.e. MPA entails that you will win tomorrow's lottery (Meyer et al., 1979; Field, 2008; Beall, 2009). The only additional ingredient we need is the claim that "conjunction is idempotent," i.e. that  $\vdash \alpha \leftrightarrow \alpha \wedge \alpha$ .<sup>61</sup> The principle is intuitive enough, but not as innocent as it might seem at first sight: it(s right-to-left direction) presupposes the validity of the structural rule of contraction

$$\text{SContr} \frac{\Gamma, \alpha, \alpha \vdash \beta}{\Gamma, \alpha \vdash \beta},$$

an observation to which we'll return below.<sup>62</sup> Since VTP and VMP jointly entail the paradox-generating MPA, it would appear that revisionary theorists can't consistently assert that valid arguments preserve truth.<sup>63</sup> Field (2008, p. 377) and Beall (2009, p. 35) accept the foregoing argument, and consequently reject the claim that valid arguments are guaranteed to preserve truth.

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<sup>61</sup>In a nutshell, the reasoning is as follows. We let  $\pi$  be a Curry sentence equivalent to  $\text{Tr}(\ulcorner \pi \urcorner) \rightarrow \perp$ , and substitute  $\pi$  and  $\perp$  for, respectively,  $\alpha$  and  $\beta$  in MPA. We then get

$$(\pi \rightarrow \perp) \wedge \pi \rightarrow \perp,$$

which, given the T-Scheme and the way  $\pi$  is defined, entails

$$\pi \wedge \pi \rightarrow \perp.$$

The idempotency of conjunction now yields  $\pi \rightarrow \perp$  which, courtesy of the T-Scheme, now entails  $\pi$  and, via *modus ponens*,  $\perp$ .

<sup>62</sup>For more details, see Beall and Murzi (2013) and Murzi and Shapiro (2012).

<sup>63</sup>See Beall (2007), Beall (2009, pp. 34-41), Shapiro (2011, p. 341) and Beall and Murzi (2013).



itself, up to equivalence, that it validly entails that you will win the lottery:

$$\vdash \pi \leftrightarrow \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)$$

Let  $\Sigma$  now be the following derivation of the further theorem  $\text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)$ :

$$\frac{\frac{\pi \vdash \pi \quad \vdash \pi \leftrightarrow \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)}{\pi \vdash \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)} \rightarrow\text{-E} \quad \pi \vdash \pi}{\frac{\frac{\pi, \pi \vdash \perp}{\pi \vdash \perp} \text{SContr}}{\vdash \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)} \text{VP}} \text{VD}$$

Using  $\Sigma$ , we can then ‘prove’ that you will win the lottery

$$\frac{\frac{\Sigma \quad \vdash \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)}{\vdash \perp} \text{VD} \quad \frac{\frac{\Sigma \quad \vdash \pi \leftrightarrow \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)}{\vdash \pi} \text{VD} \quad \vdash \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)}{\vdash \perp} \rightarrow\text{-E}}{\vdash \perp} \text{VD}$$

We have proved on no assumptions that you will win the lottery! This is the Validity Curry Paradox, or v-Curry Paradox, for short.<sup>66,67</sup>

It might be objected that the problem doesn’t affect *logical* validity, where ‘logic’ is first-order classical logic. For notice that subderivation  $\Sigma$  above doesn’t establish the argument from  $\pi$  to  $\perp$  as *logically* valid, for two reasons. First, this subderivation relies on a substitution instance of the *logically invalid* biconditional proved by the Diagonal Lemma, viz.  $\pi \leftrightarrow \text{Val}(\ulcorner \pi \urcorner, \ulcorner \perp \urcorner)$ . Second, it uses VD, and, it might be objected, surely such a rule isn’t logical.<sup>68</sup> However, these objections simply show that the v-Curry Paradox is not a paradox of *purely logical* validity,<sup>69</sup> and it seems to us that there are broader notions of validity

<sup>66</sup>The paradox is sometimes referred to as the Beall-Murzi paradox (see e.g. Toby Meadows’ contribution to this volume), owing to its recent discussion in Beall and Murzi (2013). The paradox is much older than that, however. For some historical details, see Beall and Murzi (2013). For some recent discussion of the paradox and related issues, see e.g. Ketland (2012), Mares and Paoli (2012) and Cook (2013). Essentially the same paradox is discussed in Carrara and Martino (2011), who observe that naïve provability also gives rise to a v-Curry paradox. For more discussion on paradoxes of naïve provability, see Priest (2006b, p. 238).

<sup>67</sup>The logical resources used in the derivation of the v-Curry paradox are quite meagre. Beyond Id and SContr, all is needed for deriving the Validity Curry is that VP and VD be valid. We should stress, however, that the rule of Cut, and hence the transitivity of deduction, is literally built into VD. For more discussion, see Murzi and Shapiro (2012) and Murzi (2014).

<sup>68</sup>Field (2008, §20.4) himself advances versions of this line of argument, while discussing what is in effect a validity-involving version of the Knower Paradox resting on NEC\* and T\*. See especially Field (2008, p. 304 and p. 306).

<sup>69</sup>A recent result by Jeff Ketland shows that purely logical validity *cannot* be paradoxical.

than purely logical validity.<sup>70</sup> For instance, the objections don't obviously apply to Modal. In this sense, at least intuitively, the arithmetic required to prove the Diagonal Lemma is valid and VD is validity-preserving.

To be sure, it might be insisted that there is no coherent notion of validity distinct from purely logical validity. But it seems to us that the distinction must be made, for at least two reasons. First, it might be argued that there are clear examples of arguments that are valid, albeit not logically so. A first (admittedly controversial) example is given by the  $\omega$ -rule:

0 has property  $F$   
 1 has property  $F$ ,  
 2 has property  $F$ ,  
 ...  
 Every natural number has property  $F$ .

Many would think that the rule is intuitively valid.<sup>71</sup> Yet, the rule is invalid in first-order logic.<sup>72</sup> Less controversially, other examples of valid but not logically valid arguments include analytic validities such as the following:

$$\frac{x \text{ is a brother}}{x \text{ is male}} \quad \frac{\Phi(0) \quad \forall n(\Phi(n) \rightarrow \Phi(S(n)))}{\forall n(\Phi(n))},$$

where  $S(x)$  expresses the successor function. To be sure, one might point out that such rules would be logically valid if we held fixed the interpretation of 'brother', 'male', 'successor' and numerals. However, the dialectic here is a familiar one: intuitively invalid inferences such as  $\langle \text{Leslie was a US president} \therefore \text{Leslie was a man} \rangle$ , and intuitively invalid sentences such as 'There are at least

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Ketland (2012) proves that Peano Arithmetic (PA) can be conservatively extended by means of a predicate expressing logical validity, governed by intuitive principles that are themselves derivable in PA. It follows that purely logical validity is a consistent notion if PA is consistent, which should be enough to warrant belief that purely logical validity simply *is* consistent.

<sup>70</sup>Several semantic theorists, including revisionary theorists such as Field and Priest, resort to non purely logical notions of validity. For instance, Field (2007, 2008) extensionally identifies validity with, essentially, preservation of truth in all ZFC models of a certain kind, thus taking validity to (wildly) exceed purely logical validity. Likewise, McGee (1991, p. 43-9) takes logical necessity to extend to arithmetic and truth-theoretic principles.

<sup>71</sup>Examples include Tarski (1983, p. 411), Etchemendy (1990) and McGee (2011, p. 33).

<sup>72</sup>First-order logic is *compact*: an argument is valid in first-order logic if and only if some *finite* sub-argument is valid.

two numbers', would thereby be declared logically valid (Etchemendy, 1990).<sup>73</sup>

Second, validity arguably has a role to play in our epistemic lives. Harman (1986, p. 18) suggests that ordinary reasoning is partly governed by the following principles:

*Recognised Implication Principle.* One has a reason to believe  $\alpha$  if one recognises that  $\alpha$  is implied by one's view.

*Recognised Inconsistency Principle.* One has a reason to avoid believing things one recognises to be inconsistent.

Similarly, Field advocates the existence of a connection between validity and correct reasoning, this time framed in terms of degrees of belief. Where  $P(\alpha)$  refers to one's degrees of belief in  $\alpha$ , Field's principle reads:

(F) If it's obvious that  $\alpha_1, \dots, \alpha_n$  together entail  $\beta$ , then one ought to impose the constraint that  $P(\beta)$  is to be at least  $P(\alpha_1) + \dots + P(\alpha_n) - (n - 1)$ , in any circumstance where  $\alpha_1, \dots, \alpha_n$  are in question. (Field, 2009b, p. 259)

Roughly: if  $\alpha$  entails  $\beta$ , then one's degree of belief in  $\beta$  should be no lower than one's degree of belief in  $\alpha$ .

Both Harman's and Field's principles are, we think, plausible. But neither principle is especially concerned with *logical validity*. Plainly, ordinary speakers usually don't distinguish between valid sentences and *logically* valid sentences (Harman, 1986, p. 17). Arguably, by 'valid' they mean something in between 'truth-preserving' and 'warranted in virtue of the meaning of the relevant expressions'. If that's correct, if the principles apply at all, they must apply to validity. As Harman puts it, "since there seems to be nothing special about *logical* implications and inconsistencies, ... there seems to be no significant way in which *logic* might be *specially* relevant to reasoning" (Harman, 2009, p. 334). Logical validity is not specially relevant to reasoning. But validity arguably is.

It might be objected that, if the foregoing considerations are correct, we have just shown that non-purely logical validity is threatened by paradox, and hence incoherent. However, this simply would not follow. Murzi (2014) shows that

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<sup>73</sup>Again, we don't have space here to fully defend the claim that Tarski's account of validity cannot handle analytic validities. This is a large issue related to the even larger issue whether, as forcefully argued in Etchemendy (1990, 2008), Tarski's account *undergenerates*. For more discussion on analytic validity and the issue of undergeneration, see e.g. Priest (1995, p. 288) and Etchemendy (2008, p. 278 and ff).

validity, just like truth, is neither definable nor expressible. Yet, it would be a mistake to infer from this that validity is incoherent, just like it would be a mistake to immediately conclude that *truth* is incoherent from Tarski's Theorem. Both conclusions require *argument*: more specifically, an argument to the effect that no alternative solution can be forthcoming.

In any event, the standard treatments for truth—essentially, the hierarchical one and the revisionary one—are also available in the case of validity. Validity, just like truth, can be *stratified* (Myhill, 1975; Whittle, 2004; Beall, 2013). Then, one can truly claim that valid arguments preserve truth, although such a claim will never encompass absolutely all levels of validity and truth—not a surprising conclusion, if one thinks that rejecting absolutely general quantification is the key for solving the semantic, and perhaps set-theoretic, paradoxes.<sup>74</sup> Or, just like for truth, one can weaken some of the logical principles involved in the foregoing paradoxical derivations, such as SContr or the transitivity assumption built into VD (Shapiro, 2011; Ripley, 2011; Zardini, 2011, 2012; Beall and Murzi, 2013).<sup>75</sup> Then, the Triviality Argument, the Validity Curry Paradox and, arguably, the Unprovability of Consistency Argument are blocked (Murzi and Shapiro, 2012).

In “Logical consequence and conditionals from a dialethic perspective”, Massimiliano Carrara and Enrico Martino critically investigate *dialetheist* treatments of Curry's Paradox, i.e. treatments within a framework for countenancing true contradictions. Toby Meadows' pioneering “Fixed points for consequence relations” generalises Saul Kripke's fixed-point techniques (Kripke, 1975) to contraction-free logics weak enough to invalidate Validity Curry and related validity paradoxes.

## 5 Concluding Remarks

We hope to have shown that the debate about logical consequence is still very lively. We also hope to have cast some doubts on Field's contention that, in view of the problems faced by the modal, model-theoretic and proof-theoretic accounts of validity, validity is undefinable and must be considered a primitive notion. Our conclusion is not very exciting: there are several notions of validity,

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<sup>74</sup>See §2 above.

<sup>75</sup>Simply invalidating Conditional Proof, as in common revisionary treatments of Curry's Paradox here won't do, since the rule isn't involved in the Validity Curry derivation. For details, see Beall and Murzi (2013).



perhaps equally legitimate. And their standard definitions are not obviously inadequate, in spite of more or less recent arguments to the contrary.

It might still be objected that one doesn't need to argue against existing accounts of validity in order to conclude that validity cannot be defined. In a recent paper, Field (2013) offers a different argument for the indefinability of validity. His argument is in effect a version of Moore's Open Question Argument:

[A]ny ... proposal for a *definition* of 'valid' is subject to Moore's Open Question Argument: a competent speaker may say "Sure, that inference is *classically* valid, but is it *valid*? ... Even an advocate of a particular logic ... should recognise a distinction between the concepts of 'classically valid' and 'valid', given that it's hard to seriously dispute what's classically valid but not *so* hard to seriously dispute what's valid. (Field, 2013)

Whatever model- or proof-theoretic account of validity one might offer, it will be always possible to ask whether proof- or model-theoretically valid arguments are also genuinely valid.

The argument is correct, as far as it goes. But, we think, it doesn't go very far. An observation by MacFarlane helps us identify the problem. As MacFarlane observes, that "truth in all set-theoretic models is not conceptually equivalent to logical truth ... is indeed trivial" (MacFarlane, 2000b, p. 5). Rather, advocates of MT will hold that logical truth is conceptually equivalent to *truth in all structures*, or, as MacFarlane puts it, *truth on all possible interpretations of the language's non-logical terms*. And Field's Open Question argument does nothing to challenge *this* equivalence. Similar considerations, we take it, apply to proof-theoretic validity. We conclude that more needs to be shown that validity is a primitive concept governing our inferential and epistemic practices.

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## Abstracts

Corine Besson (University of Sussex)

“Logical truths and their instances”

*Abstract:* Classical logic counts sentences such as ‘Alice is identical with Alice’ as logically true. A standard objection to classical logic is that Alice’s self-identity, for instance, is not a matter of logic because the identity of particular objects is not a matter of logic. For this reason, many philosophers argue that classical logic is not the right logic, and that it should be abandoned in favour of free logic—logic free of existential commitments with respect to singular terms. In most standard free logics, sentences such as ‘Alice is identical with Alice’ are not true. This paper argues that this objection from existential commitments is somewhat superficial and that there is a deeper reason why ‘Alice is identical with Alice’ should not be considered a logical truth. Indeed, a key fundamental thought about the nature of logic is that a logical truth is true in virtue of its logical form. The fundamental problem I raise is that a sentence such as ‘Alice is identical with Alice’ appears to not even be true in virtue of its logical form. Thus this paper argues that given that such a sentence is not true in virtue of its logical form, it should not be counted as logically true. It moreover argues, on the same grounds, that even the sentences which free logicians regard as logically true shouldn’t be regarded as logically true. So in this sense free logic is no repair to classical logic.

Massimiliano Carrara and Enrico Martino (University of Padua)

“Logical consequence and conditionals from a dialethic perspective”

*Abstract:* Dialetheism holds the thesis that certain sentences are *dialetheias*, i.e. both true and false, and devises several strategies for avoiding trivialism, the (classical) consequence that *all sentences* are provable. Two such strategies are aimed at invalidating one of the most direct arguments for trivialism, viz. Curry’s Paradox: a proof that you will win the lottery which only resorts to naïve truth-principles, Conditional Proof (CP), *modus ponens* (MPP) and the standardly accepted structural rules. The first strategy simply consists in observing that the most well-known dialetheist logic, sometimes referred to as the Logic of Paradox (LP), invalidates MPP. The second strategy consists in rather taking one

of the primary senses of ‘if’ to be captured by an *entailment* connective which does not validate CP. We argue that both strategies prove problematic.

Marcello D’Agostino (University of Ferrara)

“Analytic inference and the informational meaning of the logical operators”

*Abstract:* This paper elaborates on some ideas put forward in Marcello D’Agostino and Luciano Floridi’s “The enduring scandal of deduction. Is propositionally logic really uninformative?” and provides a more detailed exposition of an “informational semantics” for the logical operators. In this semantics the meaning of a logical operator is specified solely in terms of the information that is *actually possessed* by an agent. We show that the informational meaning of the logical operators that arises from this semantics is consistent with a *strong manifestability requirement*: any agent who grasps the (informational) meaning of the logical operators should be able to tell, in practice and not only in principle, whether or not s(he) holds the information that a given complex sentence is true, or the information that it is false, or neither of the two. This informational semantics also allows us to draw a sharp demarcation between “analytic” and “synthetic” (i.e. non-tautological) inferences in propositional logic, which defies the empiricist dogma that all logical inferences should belong to the first class. It also provides the means for defining degrees of syntheticity of logical inferences that may be related, on the one hand, to the “cognitive effort” required by an agent to recognize their validity and, on the other, to the computational resources that need to be consumed for this task.

Ole T. Hjortland (Munich Center for Mathematical Philosophy)

“Verbal disputes in logic: against minimalism for logical connectives”

*Abstract:* Quine’s famous meaning-variance thesis has it that when a classical and a nonclassical logician argue about a logical law, say, the law of excluded middle, the apparent disagreement is a ‘mere verbal dispute’. Here we explore a popular response to the meaning-variance thesis, minimalism for logical connectives, as developed for example by Hilary Putnam, Susan Haack, and more recently Francesco Paoli. We use a new variant of Quine’s argument — a meta-Quinean argument — to show that the minimalist’s position is ultimately

untenable. We then outline an alternative response to the meaning-variance thesis along structuralist lines.

Toby Meadows (NIP, University of Aberdeen)

“Fixed-points for consequence relations”

*Abstract:* This paper provides a way of dealing with paradoxes associated with consequence relations via a generalisation of Kripke’s fixed point technique. In particular, we focus on Beall and Murzi’s paradox, although the framework outlined should have more general application. We first attempt to locate this problem among the existing corpus of semantic paradoxes. We then examine two canonical approaches to the issue and conclude with an inductive construction which, in some fashion, goes beyond those approaches.

Vittorio Morato (University of Padua) “Validity and actuality”

*Abstract:* The notion of validity for modal languages could be defined in two slightly different ways. The first is the original definition given by S. Kripke, for which a formula  $\alpha$  of a modal language  $L$  is valid if and only if it is true in every actual world of every interpretation of  $L$ . The second is the definition that has become standard in most textbook presentations of modal logic, for which a formula  $\alpha$  of  $L$  is valid if and only if it is true in every world in every interpretation of  $L$ . For simple modal languages, “Kripkean validity” and “Textbook validity” are extensionally equivalent. According to E. Zalta, however, Textbook validity is an “incorrect” definition of validity, because: (i) it is not in full compliance with Tarski’s notion of truth; (ii) in expressively richer languages, enriched by the actuality operator, some obviously true formulas count as valid only if the Kripkean notion is used. The purpose of this paper is to show that (i) and (ii) are not good reasons to favor Kripkean validity over Textbook validity. On the one hand, I will claim that the difference between the two should rather be seen as the result of two different conceptions on how a modal logic should be built from a non-modal basis; on the other, I will show the advantages, for the question at issue, of seeing the actuality operator as belonging to the family of two-dimensional operators.

Francesco Paoli (University of Cagliari)

## “Minimalism for logical constants”

*Abstract:* In “Quine and Slater on paraconsistency and deviance”, I defended a minimalist account of meaning for logical constants as a way to ward off Quine’s meaning variance charge against deviant logics. Its key idea was that some deviant propositional logics share with classical logic the operational meanings of all their connectives, as encoded in their sequent calculus operational rules, yet validate different sequents than classical logic? therefore, we can have genuine rivalry between logics without meaning variance. In his PhD Thesis *The Structure of Logical Consequence: Proof-Theoretic Conceptions*, Ole Hjortland levels several objections at this view. The aim of this paper is to address these criticisms, highlighting at the same time the rôle played by logical consequence in this version of semantic minimalism.

Gil Sagi (Munich Center for Mathematical Philosophy)

## “Formality in logic”

*Abstract:* In this paper, I discuss a prevailing view by which logical terms determine forms of sentences and arguments and therefore the logical validity of arguments. This view is common to those who hold that there is a principled distinction between logical and nonlogical terms and those holding relativistic accounts. I adopt the Tarskian tradition by which logical validity is determined by form, but reject the centrality of logical terms. I propose an alternative framework for logic where logical terms no longer play a distinctive role. This account employs a new notion of *semantic constraints*. The paper includes some preliminary definitions and results in the new framework.

Jack Woods (Bilkent University)

## “Logical indefinites”

*Abstract:* The best extant demarcation of logical constants, due to Tarski, classifies logical constants by invariance properties of their denotations. This classification is developed in a framework which presumes that the denotations of all expressions are definite. However, some indefinite expressions, such as Russell’s indefinite description operator  $\eta$ , Hilbert’s  $\epsilon$ , and abstraction operators such as ‘the number of’, appropriately interpreted, are logical. I generalize the Tarskian



framework in such a way as to allow a reasonable account of the denotations of indefinite expressions. This account gives rise to a principled classification of the denotations of logical and non-logical indefinite expressions. After developing this classification and its application to particular cases in some detail, I show how this generalized framework allows a novel view of the logical status of certain abstraction operators such as ‘the number of’. I then show how we can define surrogate abstraction operators directly in higher-order languages augmented with an  $\epsilon$ -operator.