Why conclusions should remain single

Abstract: This paper argues that logical inferentialists should reject multiple-conclusion logics. Logical inferentialism is the position that the meanings of the logical constants are determined by the rules of inference they obey. As such, logical inferentialism requires a proof-theoretic framework within which to operate. However, in order to fulfil its semantic duties, a deductive system has to be suitably connected to our inferential practices. I argue that, contrary to an established tradition, multiple-conclusion systems are ill-suited for this purpose. Multiple-conclusion systems fail to provide a 'natural' representation of our ordinary modes of inference. Moreover, the two most plausible attempts at bringing multiple conclusions into line with our ordinary forms of reasoning, the disjunctive reading and the bilaterlist denial interpretation, are both shown to be unacceptable by inferentialist standards.

Keywords: Inferentialism, multiple conclusions, proof-theoretic arguments.

1 Introduction

An argument leads to a conclusion, *one* conclusion, or so one would think. However, a number of logicians and philosophers have urged that the notion of argument and with it that of logical consequence be liberalized so as to include arguments containing any (finite) number of conclusions. In this paper I take issue with deductive systems that embody multiple-conclusion deducibility relations. At the very least logical inferentialists, I shall argue, should reject such systems.[1]

The plan is as follows. Sections 2 and 3 characterize the logical inferentialist's position and the constraints it imposes on the properties of acceptable deductive systems. We then explain why advocates of classical logic with inferentialist sympathies might be attracted to multiple-conclusion systems. The following two sections 4 and 5 argue *contra* Greg Restall that there are no episodes in our ordinary modes of deductive reasoning

[1] I believe that my conclusions bear on the work of a considerable number of authors, examples are Bostock (1997), Cook (2005), Dǒsen (1994), Hacking (1979), Kneale (1956), Kremer (1988), Read (2000) and Restall (2005). Even if not all of these authors explicitly commit themselves to inferentialism as I define it below, insofar as they all accord certain deductive systems a role in accounting for the meanings of the logical constants, my conclusions will be relevant to them.

that can be said to be more faithfully represented in a multiple-conclusion framework. We then present and defend an argument against the disjunctive reading of multiple-conclusion sequents (sections 6 and 7). Finally, an alternative bilateralist interpretation of multiple-conclusion sequents is considered and rejected (section 8).

2 The principle of answerability and the primacy of natural deduction

Why, then, is logical inferentialism incompatible with the use of multiple-conclusion proof systems? Logical inferentialism, we have said, requires a proof-theoretic framework within which it can be articulated. But not just any framework will do. A suitable framework must conform to certain constraints—constraints which, as it turns out, multiple-conclusion systems fail to meet. Let us begin, therefore, by characterizing the role of deductive systems in logical inferentialism with a view to identifying the demands that it imposes on proof systems.

Logical inferentialists hold that the meanings of the logical constants are determined by the role they play in deductive inference.[2] A constant's deductive behaviour, it is thought, is best represented in the form of explicitly stated schematic rules of inference within a deductive system. Whence the need for a deductive framework. Now, this much is perfectly compatible in principle with a radical type of conventionalism. According to this kind of *laissez-faire* inferentialism we can fix the meanings of the logical operators at will. Simply devise inference rules as you see fit and you will thereby fix the meanings of the logical symbols contained within them. Nothing prevents us from laying down new rules, of course. The mistake, however, resides in the idea that any formal game incorporating what appear to be inference rules will confer meanings on its logical symbols. Contrary to Carnap's amoralism about logic, adherence to inferentialism importantly constrains one's choice of proof-theoretic frameworks: the inferentialist must remain faithful to our ordinary inferential practice and so must oppose Carnapian promiscuity. Only such deductive systems fit the bill as can be seen to be answerable to the use we put our logical vocabulary to. It is the practice represented, not the formalism as such that confers meanings. Therefore, the formalism is of meaning-theoretic significance and hence of interest to the inferentialist only if it succeeds in capturing (in a perhaps idealized form) the relevant meaning-constituting features of our practice. It

[2] Inferentialism has been put forth as a doctrine about meaning in general, most famously in Brandom (1994). However, at present we will be concerned solely with the fragment of logical expressions. I will therefore use 'inferentialism' to designate the doctrine as it applies to this restricted class of expressions.

follows that the inferentialist position imposes strict demands on deductive systems. For future reference, let us summarize these demands in the form of the

> **Principle of answerability:** only such deductive systems are permissible from the inferentialist point of view as can be seen to be suitably connected to our ordinary deductive inferential practices.

A comment is in order at this point. To say that it is our practice that confers meanings on the logical particles is not to say that a system of logic amounts to a mere description of the actual use of the logical expressions. An inferentialist account of the meanings of the logical terms is compatible with the claim that there are norms which our actual use of those terms must respect. Although our rules of inference must bear witness to our practices, they simultaneously exert normative force. In this respect rules of inference are comparable to a grammar: beginning as a record of how we do in fact do things (or could do things), the grammar attains normative force telling us how we ought to do things. At the same time our rules of inference are constrained by the general principles that shape our account of meaning (demands of compositionality and of the finite stateability of our meaning-theoretic principles, for example). These principles are another source of normativity that weighs on our theory. Importantly, these principles of which the principle of harmony is an important example also constitute a corrective for our practice. We return to these points when we discuss the motivations that lead certain inferentialists to espouse multiple-conclusion systems.

Following its founder, Gerhard Gentzen, it has become customary in the inferentialist tradition to regard Gentzen-Prawitz natural deduction systems as the privileged proof-theoretic framework within which to carry out the inferentialist programme. Its alleged 'close affinity to actual reasoning' (Gentzen 1934/1969, p. 80), is thought to make natural deduction deserving of the honorific title 'natural', whereas other types of systems—in particular axiomatic systems of the Frege-Hilbert brand—are 'rather far removed from the forms of deduction used in practice' (ibid, p. 68) and are therefore inadequate codifications of our practice. Indeed, Dag Prawitz suggests that the analysis of the logical structure of proofs afforded by Gentzen's natural deduction system has a claim to being definitive in much the way that Turing computability has often been considered to be the definitive analysis of the notion of computation (cf. Prawitz (1971, p. 247)).

Moreover, natural deduction systems gain support, in a roundabout way, from an idea put forth by Michael Dummett (and then taken up by others, e.g. (Brandom 1994, p. 116–118)). The idea is that the natural deduction format of associating each logical expression with a pair of introduction and elimination rules constitutes a paradigm for how use-theoretic accounts of meaning in general should be structured. The bipartite format offers a way of bringing the seemingly unmanageable multitude of conventions and rules that characterize the use we make of the expressions of our language within the purview of a systematic theory. Dummett summarizes his approach—we might call it the *two-aspect model of meaning*—as follows:

> Crudely expressed, there are always two aspects of the use of a given form of sentence: the conditions under which an utterance of that sentence is appropriate, which include, in the case of an assertoric sentence, what counts as an acceptable ground for asserting it; and the consequences of an utterance of it, which comprise both what the speaker commits himself to by the utterance and the appropriate response on the part of the hearer, including, in the case of assertion, what he is entitled to infer from it if he accepts it (Dummett 1973, p. 396).

When viewed from this angle our inferentialist account of the meanings of the logical particles simply appears to be a particularly neat sub-component of a larger two-aspect meaning-theoretic framework that encompasses assertoric language use in general. From this perspective the logical fragment of language thus again seems to be very *naturally* codified in a natural deduction setting.

3 The naturalness of natural deduction in question

But the primacy accorded to natural deduction systems (at least *single-conclusion* natural deduction systems) has not gone unchallenged amongst authors with broadly inferentialist sympathies. The chief motivation for seeking alternative frameworks stems from a rejection of certain revisionist tendencies often associated with natural deduction-based inferentialism. Let me begin by saying more about the revisionist tendencies in question.

Inferentialists like Dummett, Prawitz and Neil Tennant have, within the context of the semantic realism/anti-realism debate, advanced well-known arguments against classical logic on the basis of their inferentialism and their commitment to the primacy of natural deduction-style representations of our inferential practices. The unifying thought is this. Because natural deduction inference rules specify the meanings of the logical operators, they must be subject to the same constraints that regulate any viable theory of meaning. Such general meaning-theoretic considerations are brought to bear on natural deduction systems in the form of constraints on the form of acceptable inference rules. For example, languages must be learnable, hence the rules characterizing the meanings of the logical constants must be schematically representable and finite in number. Language is molecular, so rules of inference must satisfy a certain complexity condition (see (Dummett 1991, p. 258, p. 283)). Or again, and this point is of particular importance here, logical expressions ought to be semantically well-behaved, therefore they must satisfy the constraints of harmony. The rules governing a logical constant are harmonious and hence semantically well-behaved (in our sense of the term) if the circumstances under which a sentence containing the constant in question as its principal connective may justifiably be asserted are in equilibrium with the deductive consequences of such a sentence. If we accept this inferentialist framework and implement it within a natural deduction setting, we find that the meanings of the classical logical constants do not pass muster, or so the argument goes. The rules concerning negation, in particular, fail to satisfy the principle of harmony.[3] I call arguments promoting revision of our logical practice in this way *proof-theoretic arguments*. If sound, such arguments establish that classical logicians fail to attach coherent meanings to the logical expressions and indeed that no system of logic stronger than intuitionistic logic can receive proof-theoretic justification.[4]

The question is whether a defender of classical logic can subscribe to the inferentialist assumptions upon which proof-theoretic arguments are premised while avoiding their revisionary conclusions. The key to the answer might reasonably be thought to lie in the anti-realist's choice of proof-theoretic framework.

In just this vein, a number of authors have attacked the choice of a natural deduction setting for tilting the balance in favour of constructive logics and thus towards revisionary conclusions. Far from being objectively the most *natural* way of representing the principles of inference we take to be binding for our inferential practice, it is argued, natural deduction comes with a built-in bias towards constructivist thought; natural deduction has been specifically tailored to privilege constructive modes of reasoning. As William Kneale puts it,

> Gentzen's success in making intuitionist logic look like something simpler and more basic than classical logic depends, as he himself admits, on the special forms of the rules he uses, and in particular on the requirement that they should all be rules of inference (Kneale 1956, p. 253).

If Kneale is right, proof-theoretic arguments are a fraud: the anti-realist would have

[3]Arguments of roughly this form have been proposed by Dummett (1991), Prawitz (1977) and Tennant (1997).

[4]Tennant has argued for even more thoroughgoing revisions of our logic; he endorses the adoption of the weaker intuitionistic *relevant* logic (Tennant 1987, 1997).

us renounce classical logic not on the basis of genuine meaning-theoretic shortcomings, but merely because of the aesthetically displeasing features it displays when squeezed—artificially—into an unbecoming proof-theoretic dress. Surely, the realist insists, the fact that classical logic fails to satisfy proof-theoretically articulated meaning-theoretic constraints must tell against the format chosen by the anti-realist—not against our time-honoured classical logic!

The question, then, is whether the advocate of classical logic can avail himself of a proof-theoretic framework more germane to classical modes of inference. And this is where multiple-conclusion systems enter the scene. Proof-theoretic arguments crucially rely on demonstrating that each of the possible ways in which the intuitionistic natural deduction system (NJ) can be extended to yield its classical counterpart (NK) violates at least one of the meaning-theoretic constraints: NK is obtained from NJ by adjoining to the latter one of the characteristically classical rules of inference—the law of excluded middle, *reductio ad absurdum*, classical dilemma, double negation elimination, etc.—all of which, the anti-realist claims, fall foul of meaning-theoretic requirements. And these alleged meaning-theoretic shortcomings are signalled by the conspicuous violation of the pleasing bipartite symmetry displayed by NJ once we adjoin to it one of the said classical rules of inference.

Not so in the case of the sequent calculus, Gerhard Gentzen's other innovation.[5] Having introduced the sequent calculus as a multiple-conclusion calculus, Gentzen observes that in this system it is possible to move between the classical variant (LK) and the intuitionistic one (LJ) simply by requiring that in the intuitionistic case, succedents be restricted to at most one formula.[6] As Gentzen immediately recognizes,

> the distinction between intuitionistic and classical logic is, externally, of a

[5]It should be noted that, although I make use of Gentzen's labels, I shall slightly depart from his presentation of sequent systems (Gentzen 1934/1969, p. 81). I take the *relata* of the relation denoted by ':' to be sets of statements rather than sequences. We may thus dispense with the structural rules of interchange and of contraction—rules that are irrelevant for our purposes. Also, I will allow myself to speak somewhat loosely of, say, '*the* intuitionistic natural deduction system' in the singular, even though there is, strictly speaking, a multitude of systems, all of which are adequate for intuitionistic logic.

[6]Standardly, 'succedent' is used to designate the set on the right-hand side of the sequent sign in order to distinguish it from the overall conclusion of the derivation, which is itself a sequent rather than a set. However, the use of the adjective 'multiple-succedent' to refer to systems that allow for succedents of cardinality greater than one is misleading, since there is but one succedent per sequent. 'Multiple-conclusion' fares no better, since in a sequent setting we take 'conclusion' to mean the *end sequent* of which there can only be one, even in a sequent setting that countenances multiple members in the succedent of sequents. Nevertheless, I will, by an *abus de langage*, often use 'multiple-succedent' specifically to designate sequent systems that allow for succedents with multiple members. I will also at times employ 'multiple-conclusions' especially in contexts where both multiple-succedent systems and other types of multiple-conclusion systems are at issue.

quite different type in the calculi LJ and LK from that in the calculi NJ and NK. In the case of the latter, the distinction is based on the inclusion or the exclusion of the law of the excluded middle [or any of the other rules mentioned] whereas for the calculi LJ and LK the difference is characterized by the restriction on the succedent (Gentzen 1934/1969, p. 86).

It is this 'external' difference that the classicist wishes to exploit in order to short-circuit the proof-theoretic argument. Sequent calculi, it is held, lend themselves rather more naturally to the formalization of classical logic than natural deduction systems (see e.g., Bostock (1997), Cook (2005), Hacking (1979) and Read (2000)). It is for this reason that the multiple-succedent sequent calculus offers a promising framework from the point of view of the inferentialistically-minded classicist. Not only does it provide an elegant formalization of classical logic, it also circumvents issues of non-conservativeness that plagued many classical natural deduction systems, a feature which has invited anti-realist criticisms to the effect that classical logic fails to attach stable meanings to the logical constants (Tennant 1997, p. 319).[7] Thus, if the formalization of classical logic afforded by the standard sequent calculi turns out to satisfy our principles of answerability, as well as being meaning-theoretically viable, the classicist could rightly claim to have neutralized the proof-theoretic argument and so to have successfully defended classical logic.

There is another feature of multiple-conclusion systems that recommends them especially to authors of both classical and inferentialist persuasions. The feature in question is that multiple-conclusion systems display a desirable categoricity property that standard natural deduction systems lack. Rudolf Carnap (Carnap 1943) showed that $\vee$ and $\neg$ (as characterized in natural deduction systems) are compatible not only with the standard truth-functional valuations, but also with certain deviant interpretations, suggesting that the rules in question fail to capture the full classical meanings of these connectives. Indeed, they fail to pin down any one meaning. However, as Carnap himself pointed out, the same is not true for multiple-conclusion systems; the move to multiple-conclusion formulations of classical logic enables us to ward off the unwanted non-standard interpretations.

[7]In natural deduction the addition of $\neg$ to certain classical fragments yields non-conservative extensions. For example, the introduction of negation to the implicational fragment $\{\supset\}$ makes available the previously unavailable derivation of Peirce's Law $((A \supset B) \supset A) \supset A$. The same is not true for the standard multiple-succedent sequent calculus.

4 Can multiple conclusions be found in nature?

So much for motivating reformulating the inferentialist position in a multiple-conclusion setting. The question now is whether the marriage of inferentialism with multiple conclusions makes for a coherent position. In other words, are multiple conclusions compatible with the demands on permissible deductive systems that the inferentialist is committed to? In particular, we must ask, do multiple-conclusion systems satisfy the principle of answerability?[8]

Now, it seems difficult to deny that multiple-conclusion systems constitute a departure from our ordinary forms of inference and argument. Arguments in 'real life' always lead to a unique conclusion. As Ian Rumfitt puts it,

> the rarity, to the point of extinction, of naturally occurring multiple-conclusion arguments has always been the reason why mainstream logicians have dismissed multiple-conclusion logic as little more than a curiosity (Rumfitt 2008, p. 79)

We must therefore ask whether the distortion of our practices resulting from treating premises and conclusions symmetrically constitutes a legitimate idealization. Crucially, in order to justify an idealization of this sort, the advocate of multiple conclusions must furnish an explanation of how conclusions involving multiple sentences are to be understood so as to square with our practice. However, as Gareth Evans has pointed out, the defender of multiple conclusions faces a fundamental interpretative difficulty here.

> We can assert a number of premises as a series '$A_1, A_2, \ldots, A_m$'. Each stage '$A_1, \ldots, A_i$' of this is complete in itself and independent of what may follow: the subsequent assertions merely add to the commitment represented by the previous ones. But if we tried to make a serial utterance '$B_1, B_2, \ldots, B_n$' in the way required for asserting multiple conclusions, as committing us to the truth of B_1 or of $B_2 \ldots$ or of B_n, we should be withdrawing by the utterance of B_2 the unqualified commitment to B_1 into which we had apparently entered at the first stage, and so on. The utterance will therefore have to be accompanied by a warning (e.g. a prefatory 'Either') to suspend judgement

[8]For simplicity and for their greater familiarity, I shall focus here on Gentzen-style multiple-conclusion sequent calculi. However, nothing in what follows hinges on this choice; our conclusions carry over *mutatis mutandis* to other types of multiple-conclusion systems like multiple-conclusion natural deduction systems of the type introduced by Carnap (1943) and Kneale (1956) and developed in Shoesmith and Smiley (1978).

until the whole series is finished, and we do not achieve a complete speech act until the utterance of the last B_j, duly marked as such. But this is much as to admit that the various B_j are functioning not as separate units of discourse but as components of a single disjunctive one (Shoesmith and Smiley 1978, p. 5).

Hence, if multiple conclusions are to play a role in the inferentialist story, we are almost inevitably led to interpreting the conclusions disjunctively, thereby in effect rendering the multiple-conclusion system into single-conclusion one. Let us call this the *disjunctive reading* of multiple conclusions.

Note that the principle of answerability precludes a purely formalistic reading of the sentences occurring in the conclusion as an alternative to the disjunctive reading. In particular, approaches that would seek to interpret the commas occurring to the right of the sequent sign as substructural operators as somehow 'contextually defined' by some or all the rules in the system are thereby ruled out.[9] Unless there is a way of showing how such a substructural story ties in with our ordinary practice, the multiple-conclusion setting would be condemned to the status of a mere artifice, which, though perhaps of mathematical interest, would be wholly devoid of meaning-theoretic significance and thus would be of no use to the inferentialist.

On the other hand, the disjunctive reading seems to be legitimized by the interderivability of A, B and $A \vee B$: the succedent of any sequent of the form $\Gamma : A, B$ can be transformed into a disjunction by a simple application of the $\vee$-introduction rule on the right. Conversely, we can transform any sequent of the form $\Gamma : A \vee B$ into a multiple-succedent sequent:

$$\textit{CUT}\ \dfrac{\Gamma : A \vee B \qquad \vee\text{-LI}\ \dfrac{A : A \qquad B : B}{A \vee B : A, B}}{\Gamma : A, B}$$

It is not hard to see that this procedure is readily generalizable to sequents whose succedents contain any finite number of formulas. Yet, before addressing the disjunctive reading directly, let us dwell for a moment on the question of the naturalness of multiple conclusions.

5 Arguments by cases

Greg Restall has challenged the mainstream view that multiple conclusions are not part of our natural deductive repertoire. He presents a putative example of naturally occur-

[9] Approaches of this type have been suggested to me in conversation.

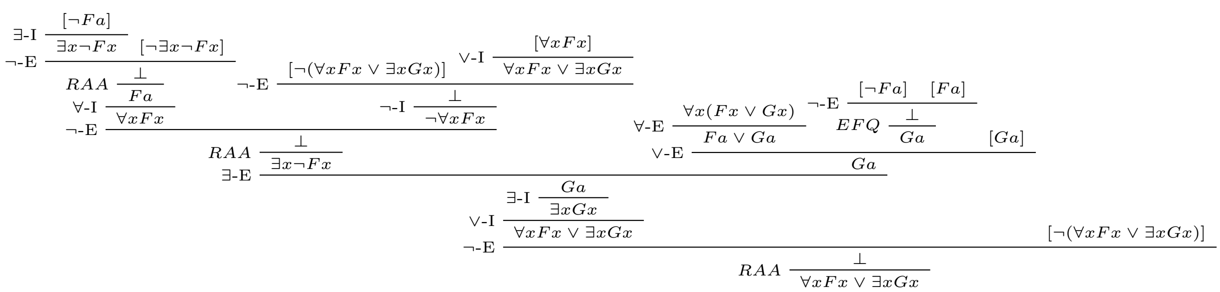
ring multiple-conclusion reasoning. The example in question is the classical argument from $\forall x(Fx \vee Gx)$ to the conclusion that $\forall xFx \vee \exists xGx$ (Restall 2005, p. 12). The most natural strategy, we may agree, is to argue by cases along the following lines: ‘Suppose everything is either F or G. Take an arbitrary object a. a must be either F or G. So we consider two cases: (i) a is F and (ii) a is G, etc.’. Restall’s claim is that our everyday reasoning insofar as it proceeds by arguments by cases of this type embodies a multiple-conclusion deducibility relation; or at least that such modes of arguing are best represented in this way. According to him, the following proof best captures our informal practice of case-based reasoning:

$$\begin{array}{c} \forall x(Fx \vee Gx) : \forall x(Fx \vee Gx) \\ \hline \forall x(Fx \vee Gx) : Fa \vee Ga \\ \hline \forall x(Fx \vee Gx) : Fa, Ga \\ \hline \forall x(Fx \vee Gx) : Fa, \exists xGx \\ \hline \forall x(Fx \vee Gx) : \forall xFx, \exists xGx \\ \hline \forall x(Fx \vee Gx) : \forall xFx \vee \exists xGx \end{array}$$

Admittedly the economy of the multiple-conclusion proof given here has a certain appeal, especially when compared with the significantly longer standard natural deduction derivations of the same result.[10]

But appearances are deceptive. While we may agree that everyday proofs frequently proceed by case distinctions, we certainly do not ordinarily consider the cases to be *conclusions* of any kind; Fa and/or Ga are not thought to *follow* from $\forall x(Fx \vee Gx)$. And indeed under closer inspection the phenomenon of ‘downward branching’, which Restall claims to be a feature of our ordinary reasoning, turns out to be no more than a disguised disjunction-elimination rule. In the move from the second to the third line (from $\forall x(Fx \vee Gx) : Fa \vee Ga$ to $\forall x(Fx \vee Gx) : Fa, Ga$) Restall makes use of the following

[10] The shortest natural deduction proof I could come with is the following rather more involved proof:



Tim Button kindly helped me in my search.

general rule—call it $\vee$-RE ('RE' for 'Restall's elimination rule'), which can be seen to rely on the aforementioned interderivability of right-hand side commas and disjunctions:

$$\vee\text{-RE}\ \frac{\Gamma : A \vee B, \Delta}{\Gamma : A, B, \Delta}$$

$\vee$-RE can easily be shown to be equivalent to the standard sequent calculus left-hand side disjunction introduction rule (given sufficient structural resources):[11]

$$\vee\text{-LI}\ \frac{\Gamma, A : \Delta \qquad \Gamma', B : \Delta'}{\Gamma, \Gamma', A \vee B : \Delta, \Delta'}$$

To see this, take the premises of $\vee$-RE ($\Gamma : A \vee B, \Delta$) and derive its conclusion with the help of $\vee$-LI:

$$CUT\ \frac{\Gamma : A \vee B, \Delta \qquad \vee\text{-LI}\ \dfrac{A : A \qquad B : B}{A \vee B : A, B}}{\Gamma : A, B, \Delta}$$

Conversely, we can derive $\vee$-RI from $\vee$-RE like so:

$$CUT\ \frac{CUT\ \dfrac{\vee\text{-RE}\ \dfrac{A \vee B : A \vee B}{A \vee B : A, B} \qquad \Gamma, A : \Delta}{\Gamma, A \vee B : B, \Delta} \qquad \Gamma', B : \Delta'}{\Gamma, \Gamma', A \vee B : \Delta, \Delta'}$$

Now, the equivalence is important because $\vee$-LI is of course strictly analogous to our customary $\vee$-elimination rule. Restall's non-standard presentation thus masks the fact that we already have a perfectly natural and well-understood way of formalizing proofs by cases of this kind: they simply take the form of subderivations from dischargeable assumptions within single-conclusion disjunction-elimination rules. As far as I can see, informal case-based reasoning is accurately represented by our natural deduction-style disjunction-elimination rule: plainly, provided that in both cases considered, Fa and Ga, we arrive at the same conclusion ($\forall x Fx \vee \exists x Gx$), we can assert the conclusion on the strength of any formula ($\forall x(Fx \vee Gx)$) that implies the disjunction $Fa \vee Ga$. It is thus hard to see in what sense it could be theoretically advantageous or more 'natural' to introduce ill-understood multiple conclusions that seem to be at odds with our ordinary notions of argument and consequence.

[11] Similarly, the move from $\forall x(Fx \vee Gx) : \forall x Fx, \exists x Gx$ to $\forall x(Fx \vee Gx) : \forall x Fx \vee \exists x Gx$ draws on a non-standard alternative of the standard disjunction-introduction rule. Given the structural rule of weakening (and contraction where one is dealing with multi-sets rather than sets) on the right, the two rules can also be shown to be equivalent.

And *even if* one chose to take this route, Restall's example would demonstrate only that natural reasoning involves only very limited range of atypical multiple-conclusion moves; as even advocates of multiple-conclusion logics admit, proofs by cases constitute 'at best a degenerate form of multiple-conclusion argument, for the different conclusions are all the same' (Shoesmith and Smiley 1978, p. 5).[12]

But that is not all. The lauded simplicity of the multiple-conclusion proof above, turns out to be the result of theft rather than of honest toil. The crucial move here is the one from $\forall x(Fx \vee Gx) : Fa, \exists x Gx$ to $\forall x(Fx \vee Gx) : \forall x Fx, \exists x Gx$. Provided that we adopt the disjunctive reading, the proof up to this point can be mimicked straightforwardly by a proof in natural deduction format.

$$\dfrac{\dfrac{\forall x(Fx \vee Gx)}{Fa \vee Ga}\,\forall\text{-E} \qquad \dfrac{[Fa]^1}{Fa \vee \exists x Gx}\,\vee\text{-I} \qquad \dfrac{\dfrac{[Ga]^1}{\exists x Gx}\,\exists\text{-I}}{Fa \vee \exists x Gx}\,\vee\text{-I}}{Fa \vee \exists x Gx}\,\vee\text{-E, 1}$$

Given that the parameter a is arbitrary (it does not appear in any of the undischarged hypotheses upon which the conclusion depends), we may conclude that $\forall y(Fy \vee \exists x Gx)$. It then remains to be shown that $\forall y(Fy \vee \exists x Gx) \vdash \forall x Fx \vee \exists x Gx$. The proof of this result is of course fairly routine in classical logic, but the point is that it is far from trivial; it is a result that requires *proof* and cannot simply be assumed. However, Restall's proof does just this: adopt the multiple-conclusion framework and you get the implication from $\forall y(Fy \vee \exists x Gx)$ to $\forall x Fx \vee \exists x Gx$ for free.[13]

[12]Note that also in Restall's proof both cases effectively lead to the same conclusion; simply, Restall's specifically tailored introduction rule allows him to dispense with an explicit restatement of the same conclusion in each case. The following equivalent formulation using the standard introduction rule for $\vee$ brings this out:

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\forall x(Fx \vee Gx) : Fa, Ga}{\forall x(Fx \vee Gx) : Fa, \exists x Gx}\,\exists\text{-IR}}{\forall x(Fx \vee Gx) : \forall x Fx, \exists x Gx}\,\forall\text{-IR}}{\forall x(Fx \vee Gx) : \forall x Fx \vee \exists x Gx, \exists x Gx}\,\vee\text{-IR}}{\forall x(Fx \vee Gx) : \forall x Fx \vee \exists x Gx, \forall x Fx \vee \exists x Gx}\,\vee\text{-IR}}{\forall x(Fx \vee Gx) : \forall x Fx \vee \exists x Gx}\,\text{Contraction}$$

[13]Indeed, with some minor fiddling we get the reverse implication as well:

$$\dfrac{\dfrac{\dfrac{\dfrac{\forall x Fx \vee \exists x Gx : \forall x Fx \vee \exists x Gx}{\forall x Fx \vee \exists x Gx : \forall x Fx, \exists x Gx}}{\forall x Fx \vee \exists x Gx : Fa, \exists x Gx}}{\forall x Fx \vee \exists x Gx : Fa \vee \exists x Gx}}{\forall x Fx \vee \exists x Gx : \forall y(Fy \vee \exists x Gx)}$$

Again assuming the disjunctive reading, the move in question corresponds to the following inference:

$$\frac{\Gamma : Fa \vee \exists x Gx}{\Gamma : \forall x Fx \vee \exists x Gx}$$

In other words, the universal quantifier in the conclusion is in effect introduced into a subformula within the scope of the disjunction operator. It is the latter disjunction operator, not the universal operator being introduced, that plays the role of the principal connective. As Peter Milne points out in his lucid explanation of the 'seemingly magical fact'—i.e. the rather surprising way in which sequent calculi encapsulate the transition from constructive to classical reasoning in the move from single to multiple succedents—this is a general feature of multiple-conclusion systems (Milne 2002). Given that conclusions are effectively disjunctively connected, right-hand side introduction rules license introductions of their operators into *subordinate* positions with respect to the disjunction operator which connects the formula containing the operator introduced with the remaining conclusions. That is, the right-hand side introduction rules for an operator $ do not simply state the conditions under which it is permissible to assert sentences that contain $ as their principal connective, multiple-conclusion systems allow for the same inferential moves even if the premises and the conclusion of the inference in question are embedded in disjunctions.

Take for instance the previously uncontroversial case of the $\wedge$-introduction rule. It now effectively takes the form

$$\wedge\text{-IR}\ \frac{\Gamma_0 : A\{\vee C\} \qquad \Gamma_1 : B\{\vee D\}}{\Gamma_0, \Gamma_1 : (A \wedge B)\{\vee(C \vee D)\}}$$

where the strings within braces combine with the formula immediately to their left to form disjunctions whenever there are formulas to take the place of C and D.[14] (In the limiting case where both slots are unoccupied, we of course find ourselves within a single-conclusion system.)

This raises a number of worries. First, it follows that multiple-conclusion systems make it impossible in general to characterize the meaning of any one logical constant in isolation. Someone innocent of the meanings of the logical constants should not be able to fully comprehend the (logically relevant) meaning of, say, 'and' without *already* having mastered the meaning of 'or'. The notion that it should be possible to acquire knowledge

[14] In general C and D could of course be sets of formulas. However, on the disjunctive reading adopted here we can equally well treat them as the disjunctions of their member formulas, since C and D are finite. This justifies our mode of presentation.

of the meaning of any one logical expression independently of any prior knowledge of the meanings of other logical expressions is known as the *requirement of separability* (Tennant 1997, p. 315). Clearly, a multiple-conclusion system (at least when explicitly interpreted disjunctively) must violate separability. That being said, Milne (2002, p. 523–525) is right to stress that the requirement of separability has received insufficient justification even from authors (like Tennant) who place considerable weight on it. Nevertheless, it is hard to see how it should be possible to formulate a coherent locally acting requirement of harmony without presupposing separability. For example, I know of no non-separable rules that admit of what Dummett calls a *levelling procedure* (which he assimilates with the notion of *intrinsic harmony* (Dummett 1991, p. 250)).

Second, the introduction of logical operators into subordinate positions is not generally acceptable from a constructive point of view. While some such forms of inference (e.g. from $\Gamma : A \vee D$ and $\Gamma : B \vee E$ to $\Gamma : (A \wedge B) \vee (D \vee E)$) are available also to the intuitionist as derived (though not trivially so) rules of inference, others, like the case of the universal quantifier, can only be justified by making essential use of specifically classical modes of inference. This raises serious worries of impartiality. If multiple-conclusion systems have, by their very constitution, a bias towards classical logic, such systems would prove unsuitable frameworks within which to settle disputes where the validity of classical principles are called into question.

Third, there is a more fundamental worry. We have seen that Restall's multiple-conclusion proof owed its attractive simplicity largely to the peculiar ∀-introduction rule to which it appeals. Moreover, we saw that the introduction rule in question is justified just in case—assuming once more the disjunctive reading—the following rule of inference (∀-I*) is legitimate.

$$\forall\text{-I}^* \ \frac{Fa \vee \exists x Gx}{\forall x Fx \vee \exists x Gx}$$

However, not only does this form of inference face the problem of impartiality, the inference also disguises potentially important inferential fine-structure. It lumps together several intuitively more primitive inferential moves into a single rule that presents itself as indecomposable. But this seems at odds with one of the fundamental tasks not just of the inferentialist but of logic as such: namely, the task of identifying the most basic forms of inference of which all other derived rules of inference can be shown to be a consequence.

We have thus seen that Restall's example of naturally occurring multiple-conclusion reasoning is unconvincing. The phenomenon of case-based reasoning is more simply and more naturally accommodated by applications of the standard disjunction-elimination rule. The apparent gain in simplicity involved in espousing a multiple-conclusion framework turned out to be illusory. We now turn to a further problem that besets multiple-conclusion-based inferentialism.

6 The argument from circularity

The disjunctive reading, we had said, treated the commas occurring to the right of the sequent sign as disjunctions. A sequent $A_1, A_2, \ldots, A_m : B_1, B_2, \ldots, B_n$ should thus be understood as saying 'Whenever $A_1, A_2, \ldots, A_m$ are assertible, so is the disjunctive sentence ⌜Either B_1 or B_2 or… or B_n⌝'.[15] In the previous section we have already encountered a number of difficulties faced by this interpretation. But there is more in store. While we can grasp the way in which the premises function jointly in the antecedent of a sequent without having any prior understanding of the meaning of conjunction, no such understanding of the conclusions is possible without already understanding the meaning of 'or'. Therefore the very format of the proof system requires us to have a prior grasp of the meanings of some logical constants. Dummett (1991, p. 187) makes this very point. His argument can be paraphrased as follows:

> The dispute between realists and anti-realists is recast as a dispute over the validity of certain fundamental logical principles. But disagreements about these matters must turn on questions of meaning; the meaning of the logical constants. Therefore a characterization of the meanings of the logical constants in question will be an indispensable preliminary for any such discussion. Moreover, by our inferentialist hypothesis, such a characterization is to be given within the confines of an interpreted proof system that codifies all meaning-theoretically relevant inferential relations. However, if the only possible (informal) interpretation of our proof-theoretic framework necessitates a prior understanding of certain logical operators, it will not be a suitable medium within which to settle questions of legitimacy of any of the principles containing the logical constants in question.

Call this the *argument from circularity.* According to the argument multiple-conclusion systems already fail at a fundamental level; they are incompatible with the very idea

[15] As Ian Rumfitt has pointed out to me, the fact that semantic ascent is involved already in stating how multiple-conclusion consequence is to be understood, suggests that in doing multiple-conclusion logic we are really doing meta-logic.

of inferentialism. If, as we said, the only plausible interpretation of such multiple-conclusion systems draws essentially on an understanding of the meanings of at least some logical constants, then such systems cannot play the role required of a proof-theoretic framework. After all, it is the very purpose of such a framework to provide an adequate means for specifying the meanings of the logical constants. On our reading of inferentialism, a system qualifies only if it yields a way of representing what it is a speaker has to grasp in order to be a semantically competent user of the expression in question. On this understanding of what, for the purposes of inferentialism, a proof system has to accomplish, multiple-conclusion calculi constitute a blatant failure, at least under the standard interpretation.

7 An objection and its rebuttal

Could it be, however, that the argument from circularity proves too much? Why is it the case that an understanding of the premises does not likewise require an antecedent grasp of the meaning of conjunction? And so why should single-conclusion systems not be in a similar predicament? After all, it is obviously not the case in general that A entails B and $A \supset B$ entails B. Only when the premises A and $A \supset B$ are taken to be *conjunctively connected* can they be said to jointly entail B. We thus have $A_1, A_2, \ldots, A_m \vdash B$ just in case we have A_1 and A_2... and $A_m \vdash B$. Moreover we can—as we did above for the case of disjunctively connected conclusions—establish the formal interderivability of $A, B : \Delta$ and $A \wedge B : \Delta$. Any sequent of the form $A, B : \Delta$ can be transformed into the sequent $A \wedge B : \Delta$ by an application of the left-hand side $\wedge$-introduction rule. Conversely, given $A \wedge B : \Delta$ we get:

$$\dfrac{\dfrac{A : A \qquad B : B}{A, B : A \wedge B}\,{\scriptstyle \wedge\text{-RI}} \qquad A \wedge B : \Delta}{A, B : \Delta}\,{\scriptstyle CUT}$$

Again it is easy to see how this result may be generalized to any number of premises. Should we not then, by parity of reasoning, conclude that single-conclusion calculi too necessitate a prior understanding of the meaning of conjunction? The result would be not so much a disproof of inferentialism as a wholesale disqualification of any proof system with multiple premises (so, in practice, any proof system whatsoever) from playing the role of a proof-theoretic framework.

Fortunately the inferentialist need not despair. For there is an important disanalogy between the conjunctive connection of premises and the disjunctive connection of conclusions. The difference is this. As already noted by Evans in the quotation above, asserting

A and asserting B is in a sense 'tantamount' (Dummett 1991, p. 187) to asserting $\ulcorner A$ and $B\urcorner$. We are not obliged to understand the meaning of 'and', as long as we know how to assert both A and B. This is not to say that there is no distinction to be drawn at all between asserting A and asserting B, on the one hand, and asserting $\ulcorner A$ and $B\urcorner$, on the other hand. The transition from one to the other requires, in both directions, the effecting of an inference, and it is a mastery of inferences following these patterns that constitutes knowledge of the (logically relevant) meaning of conjunction. This logical distinction notwithstanding, it makes no difference whether another person's claims are reported to me in the form of two separate assertions

> Henry said that aardvarks are nocturnal and he said that they are indigenous to South America.

or as affirming a single conjunctive proposition.

The same does not hold true in the case of disjunction. Here the distinction between

> Henry said that aardvarks are nocturnal or he said that they are indigenous to South America.

and

> Henry said that aardvarks are nocturnal or indigenous to South America.

is crucial. In the second case Henry speaks truly, since aardvarks are indeed nocturnal. In the first case, whether Henry speaks truly or not depends on which of the sentences Henry in fact asserted; he might be wrong, since aardvarks are not indigenous to South America. In other words the force marker for assertion distributes over conjunctions but not over disjunctions. Therefore we cannot, in this case, replace an understanding of the assertion of a disjunction by an understanding of the disjunction of assertions. Indeed, even if we could it would not be of much help. We simply cannot understand the disjunctive nature of the connection between conclusions save by invoking the notion of disjunction itself. Consider an argument of the form $A \vdash B, C$. Surely we cannot read such an argument as issuing an inference ticket to either B or C, whichever we choose, provided only that A is assertible. For if we are warranted, upon asserting A, in asserting either B or C at will, we are in effect warranted in asserting $\ulcorner B$ and $C\urcorner$—obviously this is a much stronger claim. The conclusion that we need to presuppose a notion of disjunction thus seems inescapable.

If we now ask which notion of disjunction we must presuppose, we find—as Tennant has pointed out (Tennant 1997, p. 320)—that multiple conclusions are intrinsically classical in that we do not in general know which of the disjuncts within the conclusion obtains.[16] We thus arrive at the conclusion that multiple-conclusion systems not only necessitate a prior grasp of the meaning of 'or', but that this meaning must be that of classical disjunction. As such, they do not constitute a suitable inferentialist framework. May the defender of single conclusions therefore rest his case? Not quite yet. Our arguments so far have relied on the assumption that there is only one acceptable interpretation of multiple-conclusion arguments: the disjunctive one. Therefore, if the realist could offer an alternative interpretation that avoided the same problems, there may yet be a way out for him.

8 Bilateralism—an escape route?

We have so far assumed that the only inferentialistically acceptable interpretation of multiple-conclusion systems is the disjunctive reading. Our informal deductive reasoning proceeds by the construction of arguments, and arguments lead from premises to a *single* conclusion. Therefore the only way in which a multiple-conclusion system can be matched with our ordinary practice is by interpreting it as a single-conclusion system with a disjunctively connected conclusion. This was Evans' point.

In order to escape the conclusions of the previous section while still being able to reap the benefits of multiple-conclusion systems the defender of classical logic must therefore devise an alternative interpretation. His task is that of rendering arguments (or sequents) in such systems intelligible without presupposing an antecedent grasp of the notion of disjunction or any other logical constants. How is this to be achieved? Is there any room here for the multiple-conclusion enthusiast to manoeuvre? If so, what shape might such an interpretation take?

One way of approaching the problem—the only contender I am aware of—is by invoking the notions of rejection and denial, thus operating in a bilateral framework.[17] The central idea is to introduce denial as a symmetric counterpart to the speech act of assertion. Similarly the notion of rejection may be understood as a negative mental attitude alongside the positive attitude of acceptance. I accept a statement if I judge it to be true; I reject a statement if I judge it to be *untrue*. Corresponding to the internal

[16] However, see Steinberger (2008) for a critical discussion of Tennant's argument. Whilst I still think that my argument against Tennant is essentially correct, the present paper should be understood to supplant the naively optimistic views about the prospects of multiple-conclusion systems advanced at the end of my earlier paper.

[17] These notions have been the object of recent work by a number of authors, e.g. Restall (2005), Rumfitt (2000), Smiley (1996).

states of acceptance and rejection, we have the outward manifestations in the form of the speech acts of assertion and of denial. The crucial point is that the speech act of denial and its associated mental state are taken to be conceptually (and according to Restall, also developmentally) prior to the (use of the) negation operator. It's one thing to deny a statement; it's quite another to assert its negation. Even if it turns out that the notion of denial should be construed so as to assimilate the assertion of the statement ⌜not-A⌝ and the denial of the proposition A (effectively identifying a proposition's being untrue with its falsity), as is the case with classical negation, the two are not 'the very same thing' (Smiley 1996, p. 1). In the first case we are dealing with a sign of illocutionary force; in the second case with a logical operator. The denial-theorist, who is all too aware of the difference, does not intend to replace the latter notion with the former. Even in the case of classical negation, there remains a residual difference between expressing assent to the negation of A and expressing dissent from the statement A, just as there is a difference between asserting ⌜A and B⌝ and asserting A and asserting B. The aim, rather, is to give a more complete account of our inferential practice and/or to give an inferentialistically satisfactory account of classical negation.

In the present context, however, the question is whether the availability of this new pragmatic tool also enables the classicist to give an alternative reading of multiple-conclusions without presupposing any familiarity with the meanings of the logical constants. And it seems as if such an interpretation is indeed available. We may interpret a sequent of the form: $A_1, \ldots, A_m : B_1, \ldots, B_n$ as follows:

> It is incoherent to assert each of $A_1, \ldots, A_m$ while simultaneously denying each of $B_1, \ldots, B_n$.[18]

Reading sequents in this way—call it the *denial interpretation*—allows us to do away with the disjunctive reading and thus appears to eliminate the problems associated with the conventional interpretation. However, before we may hope to have resolved the realist's difficulties, we must ask whether our new interpretation can be accepted by the inferentialist. This, it would appear, is doubtful.

Let us grant, for the sake of the present argument, that denial has a place as a speech act alongside assertion and that it has a central (perhaps symmetric) role to play in the determination of the meanings of the logical constants. We put to one side for the time being the question of the exact relation between the mental act of rejection and the linguistic act of denial and their respective functions in an account of meaning.[19] We

[18]Cf. (Restall 2005) and (Smiley 1998).

[19]All of these questions are disputed and may be settled in such a way as to provide grounds for ruling

also need not worry about what exactly is meant by denial (whether, for instance, it is appropriate to deny any statement one is not in a position to assert, or whether there may be statements that are neither assertible or deniable) or by 'incoherent'.[20]

We should note, however, that the denial interpretation constitutes a marked departure from the bilateralism of Smiley and Rumfitt. Smiley and Rumfitt seek to raise the notion of denial to the status of a speech act 'on all fours' with assertion (Smiley 1996, p. 1). The initially plausible idea is that both types of act have an equally important meaning-theoretic role to play. In particular, both are equally instrumental in fixing the meanings of the logical operators. What is needed, therefore, is a proof system that lays down a complete set of inference rules on the basis of both types of speech acts. And this is precisely what Smiley and Rumfitt deliver. The standard assertion-based rules regulating the usage of a given logical constant are supplemented with rules stating the inferences to which we are entitled by virtue of having denied statements involving the constant in question. Such systems lay down introduction and elimination rules for each logical constant specifying when a statement of that form may be denied as well as asserted. Importantly, however, both Smiley and Rumfitt are concerned exclusively with *single-conclusion* calculi; neither author seeks to employ bilateralism for the purposes of vindicating multiple-conclusion calculi.[21] Indeed Rumfitt explicitly repudiates such systems (see (Rumfitt 2000, p. 794–796) and (Rumfitt 2008)). Rather, he regards the bilateral framework as an *alternative* defence of classical logic from the proof-theoretic argument and as a solution to Carnap's categoricity problem. As such, it merits careful consideration. At present, however, our sole focus is the question of the legitimacy (from an inferentialist point of view) of multiple-conclusion calculi. Since Restall's use of the denial interpretation is, as far I am aware, the only sustained attempt at justifying multiple-conclusion systems by way of an bilateralist interpretation, we may focus our attention on it (leaving a discussion of the significance of Smiley- and Rumfitt-type single-conclusion systems for another occasion).

In what way, then, does Restall's denial interpretation deploy the notion of denial differently from Smiley and Rumfitt? For a start, all the statements in the antecedent of the sequent carry assertoric force; all the statements in the consequent are denied. A consequence of this is that Restall's explanation of the meanings of the logical con-

out the denial interpretation *ab initio*. See for example (Dummett 2002) and Rumfitt's reply (Rumfitt 2002).

[20]Note, however, that it is not clear on the face of it how the notion of coherence appealed to here could be cashed out without invoking substantive notions of truth and falsity objectionable to the inferentialist.

[21]True, the aforementioned (Smiley 1998) does allude to the denial interpretation. But it is his (Smiley 1996) paper that carries weight for our current discussion and no relevant mention of multiple conclusions is made there.

stants fails to conform to what we called the two-aspect theory of meaning. Unlike standard inferentialist accounts, Restall's approach does not explain the meaning of a logical constant in terms of the conditions under which a sentence containing the constant in question as its main connective may be asserted, and in terms of the deductive consequences of asserting the said sentence. Rather, the meanings of the constants are thought to be determined by the specific way in which they 'constrain assertion and denial'.

Let me explain. The denial-theorist holds that an account of meaning based solely on the notion of assertion is insufficient; a complete account *also* takes denial conditions into account (i.e. the conditions under which a statement made by means of a sentence containing the constant in question in a dominant position may legitimately be denied) and the consequences of denial (i.e. the consequences of denying such a statement). Therefore, the denial-theorist is committed to delivering a complete account of the assertibility conditions and the consequences of asserting *and*, on top of such an account, he promises to provide a similar account for denial. But clearly, Restall provides no such thing. All we learn is when it is inappropriate to deny a statement containing a given operator in a dominant position (relative to the statements that are simultaneously endorsed), and we learn when it is illegitimate to assert certain statements with the constant in a principal position while simultaneously denying certain others. Rather than offering us an explanation of the assertion conditions *and* the denial conditions for each of the connectives, Restall offers neither. Instead, Restall's account purports to elucidate the meanings of the constants in terms of their role in the interplay between assertions and denials.

The question is whether the account afforded by the denial interpretation is satisfactory from the inferentialist's point of view. Now, inferentialism, is a type of use-theory of meaning. Meaning is determined by use. And the relevant use consists in the licit core inferential moves involving a given logical constant. The inferentialist's task is thus to bring the manifold uses to which a logical expression may be put under a small number of rules. Put another way, the inferentialist must specify what a speaker has to know in order to be a competent user. And this is just what the two-aspect model of meaning in its natural deduction incarnation does: it tells us what knowledge of the meaning of a logical constant consists in by instructing us how it can be introduced, i.e. under what canonical circumstances we are entitled to assert a sentence containing it in a dominant position; and by telling us what the canonical deductive consequences of asserting such a sentence are. Denial theorists *à la* Rumfitt and Smiley, as we have seen, claim that an account needs to deliver even more than this; participating in the language game of logical inference requires in addition that the speaker have mastered the introduction and elimination rules governing denial. Whether or not the bilateralist's additional exigencies are justified need not concern us here. We do know, however, what, from the inferentialist's point of view, the controversy turns on. The question is whether the solely assertion-based account exhausts the meanings of the logical particles or whether the notion of denial is necessary to complete the story.

What seems perfectly clear, though, is that Restall's characterization of a connective's meaning in terms of how they constrain our assertions and denials does not succeed in fully characterizing the meanings of the logical constants. In order to do so it would have to provide, for each operator, a full inventory of the rules a (tacit) knowledge of which would constitute competence in the use of the operator in question. But the denial interpretation provides no such counsel to someone unacquainted with the meanings of the constants. Take the case of conjunction. Restall's account tells us that asserting a sentence $\ulcorner A \wedge B \urcorner$ (possibly along with a number of other sentences Γ) is incompatible with denying a set of sentences Δ provided that asserting A alone (along with the same sentences Γ) was already incompatible with denying the same set of sentences Δ (and the same for B). Moreover, it informs us that $A \wedge B$ cannot be coherently denied (possibly with other sets of sentences Δ and Δ') while asserting the sets of sentences Γ and Γ' given that A cannot be coherently denied (along possibly with Δ) while asserting Γ and that B cannot be coherently denied (along possibly with Δ') while asserting Γ'. None of this, however, is of any help to a speaker innocent of the meaning of 'and'. A knowledge of these clauses does not give us any guidance, for instance, as to when it is appropriate to use 'and'. For this, the speaker would have to know the assertion-conditions for 'and'. But the assertion conditions cannot be derived from the right-hand side introduction rule that they are standardly associated with so long, at least, as the rule and the schematic sequents figuring in it are understood in accordance with the denial interpretation. Nor can they be derived in any other way. Similarly, there is no way of determining the deductive consequences of asserting a conjunction or the denial-conditions of a conjunction. In short, the standard rules under the denial interpretation fail to give a complete account of the rules that regulate the inferential use that is constitutive of the meanings of constants. It follows that the denial interpretation fails by inferentialist standards.

This, of course, is not to dismiss the possibility that Restall's account might offer a potentially useful way of understanding the normative impact of logic as codified in multiple-conclusion systems. It does mean, however, that multiple-conclusion systems cannot be rehabilitated to meet inferentialist standards with the help of the denial interpretation.[22] Significantly, Restall himself appears to concede that the denial interpretation does not afford an acceptable reading of the relation of logical consequence:

> once one reads this turnstile as a form of *consequence* from X to Y, one must read X and Y differently—it is the *conjunction* of all X that entails the *disjunction* of all Y (Restall 2005, p. 8, fn. 3, the emphases are the author's).

Indeed, aside from the inferentialist objections already noted, it seems that the disjunctive reading does not adequately convey even rather basic features of the consequence relation. Take the example of the classical theoremhood of the law of excluded middle. On the denial interpretation $: A \vee \neg A$ would have to be rendered as 'It is incoherent to deny $\ulcorner A$ or not-$A \urcorner$'. But this surely is not what is intended; even the intuitionist can happily agree that it is incoherent to deny (every instance of) $\ulcorner A$ or not-$A \urcorner$. What the advocate of the denial interpretation owes us is a way of expressing that $\ulcorner A$ or not-$A \urcorner$ can always be correctly asserted, which is what the classical logician is after.[23]

9 Conclusion

We have argued that multiple-conclusion systems cannot reasonably be said to represent our ordinary modes of inference, not even the phenomenon of proofs by cases. Linking multiple-conclusion systems to our inferential practice thus requires that they receive a suitable interpretation. Two candidates presented themselves. The standard interpretation, the disjunctive reading, turned out to presuppose knowledge of the meanings of the logical constants, the very meanings the system was supposed to help convey, and thus proved untenable from an inferentialist perspective. The other option open to the classicist, the denial-interpretation, failed to fully characterize the meanings of the logical operators and therefore was equally found to be wanting. We conclude, therefore, that inferentialists should have no truck with multiple conclusions.

[22] As we noted earlier in this section, we have not ruled out that the notion of denial as it figures in single-conclusion systems of the kind proposed by Smiley and Rumfitt might still open the door to an effective classicist defence against proof-theoretic arguments. All we have argued here is that an appeal to the speech act of denial is of no help when it comes to reconciling multiple conclusions with inferentialism.

[23] I owe this point to Ian Rumfitt.

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