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SEMANTIC MINIMALISM FOR LOGICAL CONSTANTS

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Abstract

In [31], 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 [19], Ole Hjortland levelled 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.

Keywords

Logical pluralism; deviant logics; substructural logics; proof-theoretic semantics; logical consequence.

1. Quine's Challenge

Classical logicians and relevant logicians famously disagree about the validity of the truth table tautology $A \to (B \to A)$, sometimes referred to as the *a fortiori* law. One of the chief philosophical motivations behind relevant logic, in fact, is the urge to make a clean sweep of such supposedly paradoxical implicational principles, which are taken to offend common sense and to distort our vernacular use of 'if… then'. In the intentions of its propounders, relevant logic is an *alternative and better theory of implication*, yielding more accurate verdicts than classical logic (**CL**) about the behaviour of this concept: for example, compelling principles like transitivity, permutation and the like are in, the *a fortiori* law and other paradoxes are out. Similar considerations apply to the conflict between **CL** and intuitionistic logic, or between **CL** and orthomodular quantum logic. Each one of these 'deviant' logics has been introduced to offer an account of logical constants that rivals **CL**; whether or not the resulting theory can be considered superior to its Boolean opponent, we cannot deny that we are in the presence of an antagonism. So far, so good.

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It is well-known, however, that Quine has provided a different viewpoint on the issue in his *Philosophy of Logic* [38]. Suppose, to fix the context of our discussion, that Rita favours Anderson's and Belnap's **R** [2] as the logical system of her choice, while Charles is a partisan of **CL**. Despite appearances to the contrary, Quine surmises, Rita and Charles are not disagreeing about the validity of any logical principle, but are simply talking past each other. Rita may believe she is referring to implication when using the symbol '$\rightarrow$' in her logic, but in fact she is not — '$\rightarrow$' is what grammarians would call a 'false friend'. When trying to make sense of Rita's **R**-statements, indeed, Charles will encounter a far better candidate to interpret his own '$\rightarrow$': the defined connective $A \supset B = \neg A \vee B$, that obeys all the purely implicational theorems of two-valued logic (it may not satisfy modus ponens when one is arguing from arbitrary assumptions, but modus ponens is at least an *admissible* rule for this derivative implication). Guided by the 'Save the obvious' maxim, Charles will therefore find it more convenient to reject the homophonic translation[1] suggested by Rita and to match heterophonically his '$\rightarrow$' with Rita's '$\supset$'. This, together with the fact that **R** has the same theorems as **CL** in the $\{\neg, \wedge, \vee\}$-vocabulary, gives him the right to claim that there is no divergence between Rita's and his own account of negation ($\neg$), conjunction ($\wedge$), disjunction ($\vee$) and implication ($\supset$). But this is all there is to propositional logic. True, Rita's system also contains this weird additional notion that she denotes with the symbol '$\rightarrow$', but no conflict can arise out of predictions concerning this concept, because **CL** is silent about it. In Haack's terminology, **R** *supplements*, rather than *rival*, **CL** [17]. It yields no more an alternative account of propositional connexion than modal logics have to offer[2]. An analogous scenario explains away the supposed rivalry between **CL** and intuitionistic logic, where the Gödel translation (or one of its variants) allows the classical logician to retrieve his familiar notions in the reforming logician's discourse.

[1] In this paper I will talk a lot about *translations* between logics. There is a huge literature on the topic (in addition to the items directly referenced or discussed in the sequel, let me mention at least [34], [7], [23], [13], [14], [8], [9]), yet there is no single universally accepted definition of the concept. Of course, much depends on the definition of logic one is adopting. If we follow abstract algebraic logic in identifying (propositional) logics with pairs ($\mathbf{Fm}, \vdash$), where **Fm** is the absolutely free algebra of formulas of a given propositional language and $\vdash$ is a consequence relation on **Fm**, then a translation from $\mathbf{L}_1 = (\mathbf{Fm}_1, \vdash_1)$ to $\mathbf{L}_2 = (\mathbf{Fm}_2, \vdash_2)$ is usually taken as a map $t : Fm_1 \rightarrow Fm_2$ s.t. $\Gamma \vdash_1 A$ implies $t(\Gamma) \vdash_2 t(A)$ for all $\Gamma \cup \{A\} \subseteq Fm_1$, with $t(\Gamma)$ defined pointwise. A translation for which the converse implication holds is called *faithful* in [23] and *conservative* in [14]. We will keep most of our discussion on a more intuitive level, so as to avoid being overtly constrained by technical aspects that could cloud the main issues.

[2] Our rough-and-ready paraphrase of Quine's argument only shows that **R** can be seen as supplementing classical logic at the level of *theorems* — however, if we expand **R** by the so-called *Boolean negation* [12] the same argument can be lifted to the level of the respective consequence relations.

Quine's attack to nonclassical logics is particularly hard to defuse. Some participants in the discussion ([37], [29]) have remarked that no decent revisionary logic treats, say, implication in a way that is totally different from the classical one. Charles and Rita will always share a copious stack of logical principles that confer implication a common invariant 'core meaning'[3], and will therefore be able to recognise that connective as the same on both sides of the fence. However, this assumption has been called into question — Mortensen, for example, even claims that there is no single logical principle that holds across the whole spectrum of alternative logics in the literature [28]. Priest [35] [36] and Restall [40], instead, have imputed to Quine a confusion between logic as a theory of reasoning (logic_1) and logic as the subject matter of that theory (logic_2). With this disambiguation in force, it sounds no longer that plausible to say that changing the logic_1 is changing the subject; on the other hand, although it *is* plausible to say that changing the logic_2 is changing the subject, deviant logicians cannot be blamed for that error. However, Quine's *immanency thesis* for logical constants renders this distinction idle. For Quine, there is no subject matter beyond logical theory, no pretheoretical data to look at that constrain formalisation in the same way as experimental data would guide the construction of a physical theory. As a consequence, if we want to take our cue from Priest and Restall to demolish the Quinean objection in a non-question-begging way, we should address his immanency thesis directly, which may not be so easy to do.

In my [31], I tried the following tack. I distinguished two aspects of the meaning of a logical constant c in a given logic **L**: its *operational* meaning, whose comprehension amounts to knowing how to use c in inferential practice, and its *global* meaning, whose comprehension is manifested through the speaker's being able to assent to a correct inference involving c. The operational meaning is fully specified by the operational rules for c in a sequent calculus S for **L**, while the global meaning is specified by the class of S-provable sequents that contain c[4]. Now, it often happens that different propositional logics have the same operational rules for all their connectives, although they obviously validate different sequents. If we identify meaning *tout court* with operational meaning, therefore, we are in a position to claim that although the classes of provable sequents are different in each case

[3] Actually, while Putnam claims that the core meaning of implication is given by the *nonempty* set of principles satisfied by *all* the different logics on the market, Morton does not take a stand on the existence of such a set — he only contends that any connective that is sufficiently resemblant (in terms of logical properties) to classical implication has the right to be considered an implication.

[4] [31] has a further distinction between two aspects of global meaning, according as the phrase 'that contain c' is read as 'that contain c and no other logical constant' or as 'that contain c, possibly together with other logical constants'. In this paper, I will disregard this semantic bifurcation.

(and therefore our logics are genuine competitors), the connectives' meanings do not change across this particular range of logics. A change of logic, *pace* Quine, does not entail a change of subject. Genuine rivalry between logics is possible after all.

In greater detail, a *presentation* of a logic is a pair $\mathcal{L} = \langle L, S\rangle$, where L is a propositional language containing the connectives $c_1, \ldots, c_k$ and S is a cut-free sequent calculus based on L. The *similarity type* of a presentation $\mathcal{L}$, whose language contains the connectives $c_1, \ldots, c_k$, is the sequence of non-negative integers $\langle n_1, \ldots, n_k\rangle$, where for each $i \leq k$ the number n_i is the arity of c_i. Two presentations are *similar* if they have the same similarity type. Finally, if $\mathcal{L} = \langle L, S\rangle$ and $\mathcal{L}' = \langle L', S'\rangle$ are similar presentations and $\Gamma \Rightarrow \Delta$ is a sequent of S, the homophonic translation $t(\Gamma \Rightarrow \Delta)$ of $\Gamma \Rightarrow \Delta$ into L' is defined as follows ($r, n, m \geq 0$):

$t(p_i) = p_i$ for every variable p_i;
$t(c_i(A_1, \ldots, A_r)) = c_i'(t(A_1), \ldots, t(A_r))$ for every c_i in L;
$t(A_1, \ldots, A_n \Rightarrow B_1, \ldots, B_m) = t(A_1), \ldots, t(A_n) \Rightarrow t(B_1), \ldots, t(B_m)$.

The genuine rivalry criterion we informally introduced in the preceding paragraph is made precise below:

> (CGR) Let $\mathcal{L} = \langle L, S\rangle$ and $\mathcal{L}' = \langle L', S'\rangle$ be similar presentations with respective connectives $c_1, \ldots, c_k$ and $c_1', \ldots, c_k'$. $\mathcal{L}$ and $\mathcal{L}'$ are *genuine rivals* iff, for every $i \leq k$, c_i has in S the same operational rules as c_i' in S', but there is an S-provable sequent whose homophonic translation into L' is not S'-provable, or else there is an S'-provable sequent whose homophonic translation into L is not S-provable. If two logics have genuinely rival presentations, they are genuine rivals.

Observe that (CGR) is meant to be a *sufficient* criterion for genuine rivalry, not a *necessary and sufficient* one (the 'if' in its final sentence cannot, as argued in [31], be strengthened to an 'if and only if'). Also, remark that we do not require of some putatively competing logics that *all* of their presentations be genuine rivals; it suffices that *there exists* a pair of genuinely rival presentations for the criterion to apply. Verdicts obtained through (CGR) seem to confirm, in many cases, our naïve intuitions; for example:

- modal logics do not compete with **CL**, because we cannot come up at all with similar presentations thereof[5];

[5] As Hannes Leitgeb observed in conversation, it is well possible to present e.g. **S4** in the similarity type containing negation, conjunction, disjunction and *strict* implication. In that case, assuming we find a way to get (CGR) going, I would have no objection to viewing **S4** as a rival theory of implication with respect to classical logic (C.I. Lewis's viewpoint on the issue was more complex, but this is not the place to discuss his otherwise fascinating brand of logical pluralism).

- Quine's fictional logic in [38], identical with **CL** in all respects except that every occurrence of conjunction is replaced by a disjunction and vice versa, also fails to compete with **CL**, because classical disjunction and classical conjunction (its 'match' in Quine's fictional logic) do not have the same operational rules[6];
- many substructural logics, on the other hand, are rivals of **CL**, for it is possible to present them through sequent calculi having the same operational rules as **CL** but different structural rules (and, therefore, different provable sequents).

In particular, I maintained that **LL**, linear logic without exponentials and without lattice bounds, can be considered a genuine rival of **CL**.

2. Hjortland's Allegations

In his extremely rich and stimulating doctoral thesis [19], Ole Hjortland debates at length and with sympathetic tones the minimalist view expounded in the preceding section[7]. Although Hjortland expresses an overall agreement with the main features of this way out of the Quinean quandary, he does not refrain from posing a few problems that need the minimalist's attention. We select three of them for the purposes of the present discussion.

A. *The scope objection*. One can object to (CGR) as an excessively restrictive criterion for genuine rivalry, in that its scope is too limited. After all, it can be said, many interesting and well-motivated logics admit of no sequent formulation at all, or if they do, they do not necessarily distance themselves from **CL** in having a different set of structural rules. Are we not biased in favour of substructural logics in adopting (CGR)? Doesn't all this smack of an ad hoc strategy to shelter these logics against the meaning variance charge? To be fair, Hjortland does not seem to be too bothered by this thought, which he mentions only parenthetically:

> Granted, there are logics, and debates about logical revision, that outstrip change of the global meaning alone, but at least the minimalist can salvage a number of important revisionary debates (p. 169).

In particular, many logics — intermediate logics like Gödel logic, but also logics in the extended relevance family like **RM** — have found a convenient proof-theoretical formulation not via the sequent formalism, but by means

[6] I thank a referee for prompting me to clarify this point.

[7] More recently, Hjortland gives another interesting take on minimalism in his [21], a rejoinder to the present paper. Due to time constraints, it is not possible for me to address here the points raised in that paper.

of *hypersequents*, a generalisation of such independently introduced by Pottinger and Avron ([5], [27]). How are we to compare logics across the sequent-hypersequent divide? Are we not going to brush off interesting competitors of **CL** for what appears nothing more than a technical aspect of their proof theory, devoid of philosophical significance?

B. *The cut-off objection*. According to (CGR), in moving from **CL** to a substructural logic we do not change the subject, because the operational properties are the same in both logics. We have a real competition, though, because the structural properties[8] are different. The appeal of this view seems to depend, to a large extent, on the possibility of exhibiting a clear-cut divide between operational and structural properties. This, however, may be problematic:

> It is difficult to motivate any sharp demarcation between what count as structural properties and what count as operational properties. Just like the development of proof-theory itself, the identification and separation of structural properties is an open-ended process. It is fundamentally hostage to what sort of proof-system one is considering: sometimes what we believe to be a core feature of a logical constant is extracted and formulated as a structural property encompassing the entire system (pp. 176-177).

The distinction between operational and global meaning may be too dependent on the way we package the proof theory for our logic, rather than on intrinsic features of the logic itself (see also [30, p. 39]). A case in point is Kripke's version of the sequent calculus for **BCIW**, the purely implicational fragment of **R**, where the structural rule of contraction is built into the operational rules for implication as a technical device for proving decidability [2, §13]. Are we to conclude that the **BCIW** implication is not a genuine competitor of classical implication? If so, what about the standard formulation of the calculus, where the sole difference with **CL** can be traced back to the absence of the structural rule of weakening?

C. *The meta-Quinean objection*. There is a third, and potentially much more devastating leak in the minimalist account. We took it for granted that the operational meanings of all the connectives in a wide range of substructural logics is the same as in **CL**. We based this belief on the observation that the operational rules for the connectives look the same in all these cases. Can this resemblance turn out to be just a deceptive illusion? By way of illustration, let us look at the introduction rules for conjunction:

[8] Hjortland prefers to replace the dichotomy 'structural *vs* operational *rules*' by the more general dualism 'structural *vs* operational *properties*', where structural properties also embrace e.g. the management of side formulas in the sequent calculus under discussion. I completely agree with him on this count.

$$\frac{A, \Gamma \Rightarrow \Delta}{A \wedge B, \Gamma \Rightarrow \Delta} \quad \frac{B, \Gamma \Rightarrow \Delta}{A \wedge B, \Gamma \Rightarrow \Delta} (\wedge \mathrm{L}) \quad \frac{\Gamma \Rightarrow \Delta, A \quad \Gamma \Rightarrow \Delta, B}{\Gamma \Rightarrow \Delta, A \wedge B} (\wedge \mathrm{R})$$

On the face of it, we have the same rules in all these logics, whence the connective '$\wedge$' must *mean* the same thing across the board — or so the story goes. But wait a moment: does the *sequent separator* '$\Rightarrow$' refer to the same notion of consequence in **CL** and in substructural logics? It is all too natural to answer this question in the negative, because, for example, the classical notion abides by the whole set of structural rules (weakening, contraction, exchange), while its substructural rival must perforce restrict or drop altogether some of them. The preceding operational rules, then, may well look *typographically identical*, but this is not enough to guarantee identity of meaning. According to the standard Quinean objection, Rita's '$\rightarrow$' and Charles's '$\rightarrow$' were false friends; now, Rita (or perhaps Laura, who likes **LL** better than **R**) seems to have vindicated their semantical coincidence by pointing at the identity of their operational rules, but the wily Quinean strikes back at the metatheoretical level, simply observing that '$\Rightarrow$' is no less a false friend than its object language analogue.

Hjortland does not phrase his objection directly in these terms; however, as far as I can interpret the following remarks, he is pointing to that direction:

> We have mentioned [...] that there is, for example, a suggestive connection between monotonicity and weakening, between transitivity and cut. If we take such properties to be properties of logical consequence, it seems plausible that the properties are tied up with the content of a validity predicate, *Val*, ranging over arguments. The proposition expressed by '$Val(\ulcorner\langle\Gamma, A\rangle\urcorner)$' is, in other words, sensitive to which properties one ascribes to logical consequence.
>
> That would help us understand what an argument about structural properties is about, but it would reidentify the dispute as merely verbal. That is, two parties disagreeing over structural properties amounts to subscribing to different validity predicates. A problem becomes immediately apparent: If the original task was to circumvent Quine's meaning-change argument, we have now reintroduced the worry (pp. 178-179).

Adopting the terminology of a subsequent paper by Hjortland himself [20], meaning variance of the A-type (the innocuous and unavoidable meaning variance about consequence) entails, at least within this account, meaning variance of the B-type — the noxious change of subject deprecated by Quine.

The main bulk of this paper will be devoted to addressing the meta-Quinean objection. In the remainder of the present section, I will briefly try to counter the other two allegations.

As regards the scope objection, let me first of all observe, once more, that (CGR) is meant to be a *sufficient* condition for logical rivalry, and does not rule out other possible forms of logical deviance beyond that arising out of the structural-operational dichotomy[9]. It is easy, however, to recapture hypersequential logics in this framework. Every sequent calculus, in fact, can be lifted to a hypersequent calculus in a pretty algorithmic fashion. It suffices to reformulate each rule by adding side sequents where appropriate; for example, $(\wedge R)$ becomes:

$$\frac{\mathcal{G}\,|\,\Gamma \Rightarrow \Delta, A \quad \mathcal{G}\,|\,\Gamma \Rightarrow \Delta, B}{\mathcal{G}\,|\,\Gamma \Rightarrow \Delta, A \wedge B}\,(\wedge R^h)$$

where $|$ is the usual hypersequent separator and $\mathcal{G}$ is a metavariable for hypersequents. In addition, we have to empower the calculus with a standard set of *external* structural rules. As an example, the rule of external weakening is reproduced below:

$$\frac{\mathcal{G}}{\mathcal{G}|\mathcal{H}}\,(EW).$$

It is not hard to follow this recipe in such a way as to output a hypersequent calculus, say, for intuitionistic logic. If we do so, we can observe that intuitionistic logic and Gödel logic can be given hypersequent formulations with the same operational rules and trace back their divergence to a conflict over the following structural rule of *Communication*:

$$\frac{\mathcal{G}\,|\,\Gamma_1, \Pi_1 \Rightarrow \Delta \quad \mathcal{G}\,|\,\Gamma_2, \Pi_2 \Rightarrow \Sigma}{\mathcal{G}\,|\,\Gamma_1, \Gamma_2 \Rightarrow \Delta\,|\,\Pi_1, \Pi_2 \Rightarrow \Sigma}\,(Com)$$

thanks to which Gödel logic proves $\Rightarrow (A \rightarrow B) \vee (B \rightarrow A)$, while intuitionistic logic does not.

Moving on to the cut-off objection, I concede that the same logic can often be given different sequent presentations in which structural and operational rules split the bill in various ways. Remember, however, what was our starting point in addressing the whole issue. We envisaged a hypothetical cooperative dialogue game where, say, Charles and Laura were doing their best to understand each other's logical consequence statements. If they are in a position to set up their calculi in such a way that the homophonic translation works, as witnessed by the identity of the operational rules for the corresponding connectives, would there be any reason to reject it on the

[9] A reviewer observed that some authors (Michael Dummett, for instance) are perfectly happy with the idea that a logical dispute should turn on operational meanings, but on their view such disputes may nevertheless be substantive debates, susceptible of being resolved on principled grounds. Of course, defenders of this position owe us an explanation of the reason why these disputes are not merely verbal. Dummett [11] has a detailed story to tell about this, although I will not examine it here (cf. also [21]).

sole ground that *an alternative* presentation of either calculus (or both) destroys the match? Putting the same point in a slightly different way, it is correct to say that, perhaps for technical purposes, we often have some leeway in moving back and forth the structural-operational divide within the same calculus. This does not necessarily mean, however, that the divide itself is artificial; I would rather affirm that some presentations of a calculus — namely, presentations where operational rules are formulated in a way that is invariant across logics — may be less artificial than the rest. In any case, according to (CGR), for **L** and **L**′ to be genuinely rival it suffices that *there exist* genuinely rival presentations thereof[10].

3. Classes of Translations

While the scope objection and the cut-off objection have been given comparatively short shrift, two different, and lengthy, rejoinders to the meta-Quinean objection will be offered. In this section I will examine a more defensive argument, aimed at salvaging **LL** as a competitor of sorts with respect to **CL** independently of whether the meta-Quinean strategy succeeds. In the remainder of the paper, on the other hand, I will stay truer to the spirit of [31], trying to defuse the objection head-on.

Negro [30, pp. 11-12] proposes an interesting distinction between two types of conflict among logics. *Global* conflict is, roughly, genuine rivalry in Quine's sense. A deviant logic conflicts globally with **CL** if it can rightfully claim that it is dealing with the same logical constants, about whose behaviour it formulates, at least in part, different predictions. Such a revisionary account would probably aim not at a wholesale rejection of **CL**, but only at a fine-tuning of it — typically, at a rejection of some individual logical laws or inference rules. On the other hand, another nonclassical logician could believe that the classical operators are not only different from what she has in mind, but even *make no sense to her*. This distinct, *partial* conflict setting has as its natural upshot, on the deviant logician's part, an *en bloc* dumping of **CL**.

If we take into account this distinction, an escape from Hjortland's charge is forthcoming. Even if we grant that the meta-Quinean objection prevents **LL** from playing the rôle of a *global* competitor of **CL**, that would not entail that *partial* conflict is out of reach. If we can show that the classical connectives are nonsensical in the eye of the linear logician (either because they are ill-defined, or because they are ambiguous, or for whatever other reason), we could still view **LL** as a rival of **CL** in the sense depicted above.

[10] Hjortland suggested in conversation that such a 'liberal' existential quantifier may commit one to the ubiquity of genuine rivalry, obliterating the discriminatory power of the criterion. However, the above example of modal logics shows that (CGR) allows for cases of non-rivalry.

An analogy with natural languages shows, indeed, that our prospects in this regard look promising. It is almost a platitude to say that different languages categorise concepts in different ways. Sometimes a given language L can offer a more fine-grained categorisation than the language L', while in other cases this relation is reversed. For example, Italian has a single word ('tappeto') corresponding to the English nouns 'rug' and 'carpet'; whereas a competent English speaker can appropriately resort to either term by evaluating the relevant characteristics (size, portability, etc.) of the object to be categorised, these considerations are inessential in Italian, where a single, all-encompassing concept is available. We have a reverse situation with the English word 'fan', translatable into Italian as 'ventaglio' or 'ventilatore' according as it is a hand-held or a mechanical cooling device.

Offhand, I could not say if there exists a pair of natural languages L and L' such that L *consistently* categorises its concepts in a more fine-grained way than L' (most probably, there is no such pair). In logic, on the other hand, it is easy to exhibit such an example: **LL** consistently categorises its logical vocabulary in a more fine-grained way than **CL**. In fact, as argued at length in the tradition of substructural logic ([2], [39], [32]), in the perspective of **LL** every classical connective is ambiguous between an *extensional* and an *intensional* connective. We already recalled that conjunction has the following operational rules in the sequent calculus for **CL**:

$$\frac{A, \Gamma \Rightarrow \Delta}{A \wedge B, \Gamma \Rightarrow \Delta} \quad \frac{B, \Gamma \Rightarrow \Delta}{A \wedge B, \Gamma \Rightarrow \Delta} (\wedge \text{L}) \qquad \frac{\Gamma \Rightarrow \Delta, A \quad \Gamma \Rightarrow \Delta, B}{\Gamma \Rightarrow \Delta, A \wedge B} (\wedge \text{R})$$

However, the following pair of rules:

$$\frac{A, B, \Gamma \Rightarrow \Delta}{A \wedge B, \Gamma \Rightarrow \Delta} (\wedge \text{L'}) \qquad \frac{\Gamma \Rightarrow \Delta, A \quad \Pi \Rightarrow \Sigma, B}{\Gamma, \Pi \Rightarrow \Delta, \Sigma, A \wedge B} (\wedge \text{R'})$$

is classically equivalent to the previous one. In fact, using the structural rules of weakening and contraction, we can derive (∧L') and (∧R') in the original formulation of the calculus, and conversely, it is possible to derive (∧L) and (∧R) in the calculus which has the alternative rules as primitive.

A similar situation holds for disjunction, where we have the alternative pairs of rules:

$$\frac{A, \Gamma \Rightarrow \Delta \quad B, \Gamma \Rightarrow \Delta}{A \vee B, \Gamma \Rightarrow \Delta} (\vee \text{L}) \qquad \frac{\Gamma \Rightarrow \Delta, A}{\Gamma \Rightarrow \Delta, A \vee B} \qquad \frac{\Gamma \Rightarrow \Delta, B}{\Gamma \Rightarrow \Delta, A \vee B} (\vee \text{R})$$

$$\frac{A, \Gamma \Rightarrow \Delta \quad B, \Pi \Rightarrow \Sigma}{A \vee B, \Gamma, \Pi \Rightarrow \Delta, \Sigma} (\vee \text{L'}) \qquad \frac{\Gamma \Rightarrow \Delta, A, B}{\Gamma \Rightarrow \Delta, A \vee B} (\vee \text{R'})$$

It is important to observe that the equivalence proofs for the previous pairs of logical rules *rest essentially on the presence of weakening and contraction inferences*. If we ban these, as we do in **LL**, it matters whether we choose to introduce, say, conjunction by means of the pair (∧L)-(∧R) or of the pair (∧L')-(∧R') — and similarly for disjunction. In other words, classical conjunction (∧) and classical disjunction (∨) both split. We end up with:

- an extensional conjunction, or *meet* (⊓), defined by the rules (∧L)-(∧R) (henceforth rechristened (⊓L)-(⊓R));
- an intensional conjunction, or *fusion* (⊗), defined by the rules (∧L')-(∧R') (henceforth rechristened (⊗L)-(⊗R);
- an extensional disjunction, or *join* (⊔), defined by the rules (∨L)-(∨R) (henceforth rechristened (⊔L)-(⊔R);
- an intensional disjunction, or *fission* (⊕), defined by the rules (∨L')-(∨R') (henceforth rechristened (⊕L)-(⊕R).

For the sake of clarity, we reproduce hereafter a table with the terminology and notation used in the sequel for the full range of propositional connectives in **CL** and **LL**. Remark that there is no consensually established notation in the area, within which every single tradition tends to keep to its own set of symbols; also observe that in some cases the table below conflicts with the symbols used at the beginning of the paper, where we chose to adhere to more traditional notational conventions.

Classical conn. name	*Classical conn. symbol*
conjunction	∧
disjunction	∨
implication	⊃
Extens. **LL** *conn. name*	*Extens.* **LL** *conn. symbol*
meet	⊓
join	⊔
squiggle	⇝
Intens. **LL** *conn. name*	*Intens.* **LL** *conn. symbol*
fusion	⊗
fission	⊕
arrow	→

Summing up, we now have two candidates vying for our attention when we have to translate classical conjunction, and likewise for the other binary connectives. Although all these duplications might seem to inflate the similarity type of **LL** beyond measure, in the presence of negation (which is translated trivially, having just one counterpart in **LL**) and just one connective for each family, the remaining **LL** connectives are *definable* and can therefore be expunged from its primitive logical vocabulary. In order to attain a presentation of **LL** having the same similarity type as the usual presentation of classical logic, it is enough to pick one conjunction, one disjunction and one implication from our toolbox: provided we are careful enough not to select all of them within the same (extensional or intensional) family, this will be sufficient to recover the full expressive wealth of **LL**. If it is global conflict that we are after, we could then resort to one such *uniform* translation schema, mapping each classical binary connective to its extensional or its intensional **LL**-counterpart once and for all. The homophonic translation suggested in [31] as a way to make sense of the disagreement between classical and linear logicians was an example of uniform translation schema: we proposed to translate $\wedge$ as $\sqcap$, $\vee$ as $\sqcup$, and $\supset$ as $\rightarrow$, verifying that the result of our translation complied with (CGR). As we have seen, however, the meta-Quinean objection put this strategy on hold, at least until we find a way to justify this particular uniform translation schema as actually meaning-preserving.

We have an alternative, though. We could adopt a *non-uniform* translation schema, by means of which any single given *occurrence* of a classical connective is interpreted extensionally or intensionally depending on circumstances. This way of decoding the consequence statements in a different logical system appears even more intuitive. Non-uniform translation schemata have a stronger allure, at least in the context of natural languages. An Italian speaker listening to an English radio broadcast would probably interpret in a non-uniform way every occurrence of the word ‘fan’ (meant as the name of a cooling system) as ‘ventaglio’ or ‘ventilatore’ according to contextual clues, or whatever else. Similarly, it seems plausible to assume that Laura would give every occurrence of the ‘ambiguous’ conjunction connective in Charles’s discourse an intensional or extensional reading from time to time, as deemed appropriate by her. If there were a non-uniform translation schema t under which, for any provable formula[11] A of **CL**, $t(A)$ comes out provable in **LL**, Laura would have a strategy to maximise her agreement with Charles, at least at the level of accepted validities: given any two-valued tautology upheld by the latter, she would dexterously proceed to replace every connective occurrence therein by either its extensional or its intensional counterpart, with the aim of obtaining an **LL**-acceptable logical law. She would *disambiguate* Charles’s claims. If so, Charles would not utter anything false in the eyes of Laura; instead, his logical talk would

[11] For the sake of simplicity, in this section we will confine our discussion at the level of formulas. In the next section, we will consider the more general case of consequence relations.

appear to her *confused*, or systematically ambiguous. We would be home and dry, in that we would have reached a partial conflict situation.

This tactic, however, is wobbly. To see why, we need to introduce one further definition. Following Wojcicki [44] and Humberstone [22], we call a translation t between propositional logics *definitional* in case propositional variables are mapped by t to propositional variables. So, for example, the Gödel translation from classical to intuitionistic logic is not definitional, because each variable p is sent to its double negation $\neg\neg p$, while the translation of classical logic into **R** mentioned at the outset is definitional. A case can be made against non-definitional translations to the effect that they have nothing to do with the reinterpretation of the connectives of one logic in terms of those of another, but they simply 'extract information about one logic by finding it duplicated in however ingenious a disguise within another logic' [22, p. 441]. In particular, if Laura's non-uniform translation is to count as a disambiguation, it had better be a definitional one: the only move permitted to Laura should consist in replacing each classical connective occurrence by one of its linear logical counterparts. Unfortunately, however, we are precisely in the opposite scenario. The upshot of the Grishin-Ono translation of **CL** into **LL** (see e.g. [43, pp. 48-49]) can be roughly described as follows: to every theorem A of **CL**, we can constructively associate a formula $t(A)$ in the language of **LL**, such that: i) every occurrence of a binary connective in A is replaced in $t(A)$ by either its intensional or its extensional counterpart; ii) every occurrence of a propositional variable in A is replaced in $t(A)$ by one of its classical equivalents $p \sqcap 1, p \sqcup 0$; iii) $t(A)$ is a theorem of **LL**. It is evident that clause ii) makes t non-definitional. Interestingly enough, if we examine the definitional variant of the Grishin-Ono translation obtained by insisting that $t(p) = p$ for every propositional variable p, while leaving all of its remaining clauses untouched, classical recapture fails, as witnessed by a counterexample found by Patrick Allo [1, p. 73].

Let us now take stock and reassess the situation. The path is narrow. If we interpret classical logic into **LL** via an (appropriate) uniform translation and claim that there is global conflict between the two logics, we fall prey to the meta-Quinean objection, which commits us to establish that this translation preserves meaning. If we use the non-uniform, non-definitional Grishin-Ono translation, we just have an 'ingenious' embedding of **CL** into **LL**, not a disambiguation. The only available way out would be to look for a definitional and non-uniform translation, but we have just seen that the most obvious candidate fails to deliver partial conflict between our putatively rival logics. Actually, Allo argues that the very presence of classical tautologies whose definitional Grishin-Ono translations fail to be **LL**-provable points at another form of conflict between logics, namely "a disagreement about the correct consequence relation for a shared ambiguous language" [1, p. 75].

It remains to be seen to what extent this standpoint is immune from strengthened meaning variance arguments.

In sum, so far as the meta-Quinean objection remains in force, we cannot appeal to (CGR) to claim that our privileged uniform translation preserves operational meaning, and so global conflict remains out of reach. On the other hand, strategies aimed at partial conflict now look less convincing than they had at the beginning. It looks like we are hard-pressed to face the objection, a task I will no longer defer.

4. Logical Consequence and Meaning Variance

Recall that, according to (CGR), whenever two connectives in different logics have the same operational properties in appropriate sequent calculus presentations, they also have identical meanings. In turn, the operational rules for a connective c in a given sequent calculus can be viewed as methods to infer consequence statements containing formulas with c dominant from consequence statements containing their immediate subformulas. However, the meta-Quinean objection warns us that the consequence statements on which we are resting our case might involve different concepts of consequence in each situation. This claim calls for an elaborate reply, for which I need to analyse more carefully the notion of logical consequence that is at stake here.

Before coming to that, let me consider a possible way of dismissing the meta-Quinean objection flat out. A reviewer suggested that we could understand the rôle of the sequent arrow in operational rules as *schematic*, that is, as instructing us on which operator-specific deductive moves are permissible relative to *some* deducibility relation. If so, there would be no merely typographical identity and thus no meaning variance. This interpretation, however, may well reflect the external perspective of an outside observer, but does not do justice to the features of the dialogical situation envisaged by Quine. The parties in this debate have a *particular* deducibility relation in mind when they utter (or when they understand) the consequence statements that occur in the operational rules for their connectives, and if meaning has to be conferred upon them via these rules, it is hard to see how the classical and the nonclassical logician can fail to talk past each other.

Now that this shortcut has turned out to be unfeasible, let me start rather pedantically. It is well-known that according to Tarski [42], a *consequence relation* over a propositional language L is a relation $\vdash \subseteq \wp(Fm(L)) \times Fm(L)$ obeying the following conditions for all $A \in Fm(L)$ and for all $\Gamma, \Delta \subseteq Fm(L)$:

1. $\Gamma \vdash A$ if $A \in \Gamma$ *(Reflexivity)*;
2. If $\Gamma \vdash A$ and $\Gamma \subseteq \Gamma'$, then $\Gamma' \vdash A$ *(Monotonicity)*;
3. If $\Delta \vdash A$ and $\Gamma \vdash B$ for every $B \in \Delta$, then $\Gamma \vdash A$ *(Cut)*.

Not everybody was happy with this approach. Some logicians advocated a multiple-conclusion variant of Tarski's definition; other authors observed that considering formulas as the sole type of objects featuring in entailments was unduly restrictive; someone else was dissatisfied with the rôle awarded to sets and supported the choice of more sensitive tools (multisets, sequences, non-associative lists) as means for aggregating premisses; finally, the conditions of Reflexivity, Monotonicity, and Cut were challenged on the basis of their failure to account for several features of actual reasoning[12]. These suggestions deviate from the Tarskian orthodoxy to various degrees. The concept that suits best our needs, due to Avron [3] [4] and hereafter reproduced in full, differs from Tarski's notion under three sole respects: i) it is a multiple-conclusion relation; ii) possibly infinite sets of formulas are replaced by *finite multisets* of formulas, whereby it is no longer possible to 'contract' several occurrences of a same formula into a single one; iii) the Monotonicity postulate is dropped, and appropriate restrictions are placed on Reflexivity, to the effect that we can no longer freely add formula occurrences to the left or to the right of our turnstile. Since the multiple-conclusion formulation originally adopted by Avron is not essential for our present purposes, I will simplify it here to a single-conclusion version, so as to avoid blurring our comparison with Tarski's suggestion with an irrelevant feature. A *multiset consequence relation* over a propositional language L is a binary relation $\vdash$ between a finite multiset of formulas of L and a single formula of L obeying the following conditions for all $A, B \in Fm(L)$ and for all multisets Γ, Π of members of $Fm(L)$:

1. $A \vdash A$ *(Reflexivity)*;
2. If $\Gamma, A \vdash B$ and $\Pi \vdash A$, then $\Gamma, \Pi \vdash B$ *(Cut)*.

Observe that this more general formulation of Cut, which is equivalent to the standard one for Tarskian consequence relations, is made necessary by the replacement of sets by multisets, and by the absence of Monotonicity.

This notion, which I will adopt throughout the rest of this paper, is wholly abstract. The next step is to see how it specialises to our sequent calculus framework. Is there any consequence relation in this sense we can naturally extract out of a sequent calculus? One possible candidate immediately comes to mind. Logic students and logicians are constantly invited, even by Gentzen himself, to read sequents as entailments: for example, in the sequent calculus for classical logic, we have been taught to interpret intuitively a single-conclusion sequent $\Gamma \Rightarrow A$ as 'the formula A follows from the conjunction of the formulas in Γ'. Therefore, we might simply associate

[12] See [26] for a less cursory treatment of these aspects, and for an extensive list of references thereabout.

with a sequent calculus S of our choice over the language L — whether it is a single-conclusion or a multiple conclusion calculus — the following single-conclusion relation: if Γ is a finite multiset of L-formulas and A is an L-formula, $\Gamma \vdash_S^I A$ holds whenever $\Gamma \Rightarrow A$ is a provable sequent of S. This relation, sometimes termed the *internal consequence relation* of S [3], is easily seen to be a multiset consequence relation for any calculus that has all instances of $A \Rightarrow A$ among its provable sequents and where Cut is at least an admissible rule.

The above relation, however, is not the best pick if you have to prove that the sequent calculus for the logic you are interested in proves exactly the same entailments as the corresponding Hilbert-style calculus — a very natural move if, as it often happens, that logic was first introduced through a standard axiomatic system and you want to show that the sequent version you are considering is actually a sequent calculus for *that* logic. The problem, of course, is that internal consequence relations satisfy Monotonicity and contraction only if the corresponding sequent calculus has weakening and contraction rules, while the usual notion of proof from assumptions on which the derivability relations of Hilbert-style calculi are based incorporates Monotonicity and contraction just by definition. To avoid this shortcoming, a different multiset consequence relation, sometimes called the *external consequence relation* of S [3], is usually introduced. For Γ a finite multiset of L-formulas and A a single L-formula, let $\Gamma \vdash_S^E A$ hold whenever $\Rightarrow A$ is provable in the calculus obtained from S by adding as initial sequents all the sequents $\Rightarrow B$, for B in Γ, as well as Cut as a primitive rule. If you work in the sequent calculus for classical logic, the two relations can be easily shown to coincide. There are sequent calculi for substructural logics, however, where the internal and the external relation genuinely differ: for example, if S has no weakening rules, then $A, B \vdash_S^E A$ while it is not the case that $A, B \vdash_S^I A$.

Two questions, of course, immediately need our attention. First, how are we to philosophically interpret the distinction between internal and external consequence? And second, is it of any use to us? In particular, when we say that the operational rules for a connective c in a sequent calculus can be viewed as methods to infer consequence statements containing formulas with c dominant from consequence statements containing their immediate subformulas, are the corresponding notions of consequence and inference internal or external?

Once we stipulate that a consequence relation is a relation between a *multiset* of formulas and a formula, we assume a backgroundy theory of inference according to which we are not arguing from sentence *types*, but from sentence *tokens*, and repeat occurrences of a same sentence in your premisses can make a difference as to what conclusions follow from them. This informational view of consequence was presented in detail in [32], [26].

There, the internal consequence of a sequent calculus was given the following interpretation: to say that some conclusion A internally follows from the premisses in Γ means that, given the rules of the logic at issue, we can extract the information that A from the combined information provided by the sentences in Γ. It is the *horizontal* reading of consequence formally represented by the sequent separator '$\Rightarrow$'. The failure of structural rules is quite plausible for this reading: while it is fine to say that we extract the information that B by applying the information that $A \to B$ to the information that A, it is almost nonsensical to claim that we extracted the information that A by *applying* the information that A to the information that B, for B arbitrary.

What about external consequence? In the above-referenced papers, Ed Mares and I contend that it encodes the *preservation of the warrant to assert*. A externally follows from the premisses in Γ just in case Γ yields grounds for asserting A; said otherwise, if whenever we accept Γ we are committed to accepting A. The important point to be stressed here, I think, is that this is the kind of notion we have in mind when we affirm that the introduction rules of the connective c in a sequent calculus provide us with the meaning of c by specifying when we are in a position to infer a sentence where c is a principal connective from its auxiliary subsentences. Here, we look at the sequent rule and we are interested in the way its conclusion 'follows from' its premisses in the sense encoded by the *fraction line* that separates them; we do not look at the individual sequents in the rule, because we are not interested in the way their succedents 'follow from' their antecedents in the sense encoded by the sequent separator. For this *vertical*, external notion of inference, weakening and contraction make perfect sense. Although no one, to the best of my knowledge, framed it in the context of an informational theory of consequence, this distinction has been around for some time in the literature, in various guises: examples are Prawitz's dichotomy between the grounds for asserting a sentence and the assumptions on which we assert it [33, pp. 29-30], Dana Scott's duality between 'horizontal' and 'vertical' inference [41, p. 802], or Humberstone's interpretation of Smiley's distinction between rules of inference and rules of proof [24].

It is worth observing that the internal-exernal bifurcation parallels the intensional-extensional divide more closely than it might at first appear. At the level of *connectives*, in fact, classical logic is equivocal (in the perspective of **LL**) in that it ascribes to conjunction and disjunction the 'structural' properties of simplification, addition and idempotency ($A \wedge B \vdash A$, $A \vdash A \vee B$, $A \dashv\vdash A \wedge A \dashv\vdash A \vee A$) as well as (dual) residuation properties (e.g. $A \wedge B \vdash C$ iff $A \vdash B \supset C$). Ed Mares and I have argued that all kinds of fallacies ensue from this paralogistical attribution: paradoxes of implication [32] and *some* versions of the standard set-theoretical and semantical paradoxes [26].

These fallacies of equivocation can be solved once we acknowledge that, in **LL**, intensional conjunction and disjunction are (dually) residuated but fail the structural properties, while for extensional connectives we have the opposite situation. At the level of *consequence statements*, on the other hand, classical logic is equivocal in that it ascribes to the same notion of consequence the 'structural' properties of weakening and contraction and the residuation property encoded by the *deduction theorem*; other versions of the standard set-theoretical and semantical paradoxes can be traced back to this feature [26]. Again, we can account for these puzzles once we acknowledge that, in **LL**, internal consequence satisfies the standard deduction theorem in the form $\Gamma, A \vdash B$ only if $\Gamma \vdash A \rightarrow B$ (this is a direct outcome of the right introduction rule for implication)[13] but fails the structural properties of weakening and contraction, while for extensional connectives it is the other way around.

5. Parrying the Meta-Quinean Attack

Let us zero in on the meta-Quinean objection again. We have thrown in the idea that the operational rules for a connective in a given logic provide us with the meaning of that connective in the same logic, and that having identical rules for corresponding connectives across different logics bars the way to meaning variance. Yet, our meta-Quinean critic is alerting us on the fact that connectives are not introduced in an absolute vacuum, but with respect to a notion of consequence that *preexists* such an introduction. Belnap puts things well in an oft-quoted passage from his celebrated rejoinder to Prior's Tonk paper:

> Even on the synthetic view, we are not defining our connectives *ab initio*, but rather in terms of an *antecedently given context of deducibility*, concerning which we have some definite notions. By that I mean that before arriving at the problem of characterizing connectives, we have already made some assumptions about the nature of deducibility [6, p. 131].

Here, Belnap aims at defending his conservative extension criterion of logicality: the rules that are supposed to define a new connective *c* cannot introduce into the logic new entailments about formulas in the old vocabulary. But there is something more one can get out of this remark, something directly relevant to the issue we are debating. We have repeatedly said that the operational rules for *c* can be viewed as methods to infer entailments

[13] It is essential to remark that it is this form of the deduction theorem that we are talking about. Many external relations of substructural logics have some local, or even global form of the deduction theorem in the sense of abstract algebraic logic: $\Gamma, A \vdash B$ iff $\Gamma \vdash A \rightarrow B$ for some n, or $\Gamma, A \vdash B$ iff $\Gamma \vdash A \sqcap 1 \rightarrow B$.

containing $c(A_1, \ldots, A_n)$ from entailments containing $A_1, \ldots, A_n$. If the 'antecedently given context of deducibility' adumbrated by the words 'infer' and 'entailment' is not the same in **CL** and **LL**, meaning variance threatens us again. The task we have to carry out is to ensure that this context does not change in passing from one logic to the other.

Let me observe two things, before I proceed to tackle this aspect. First, it is not obvious what concept, exactly, Belnap has in mind when using the italicised phrase in the above quote. If c is the constant we are introducing by means of appropriate rules, should the antecedently given context of deducibility be taken as the *purely structural* fragment of the logic (the fragment that does not contain any logical constant at all) or as the entire c-free fragment, possibly containing other logical constants? Interpreters are not unanimous on that score. Garson [15], for example, favours the former exegesis, Humberstone [25] the latter. In part, this controversy depends on how strict you want to be in ousting nonconservativity from the scene: having to conservatively extend the variable fragment is surely enough to ban blatant offenders like Tonk, yet could be insufficient to resolve some subtler cases where the addition of c does not alter our available stock of structural rules, but simply licenses entailments in the old logical vocabulary that were not previously authorised. Here, I will stick to the looser interpretation: if every connective is separately vouched for preservation of meaning with respect to the connective-free language, there would seem to be no reason why a breach of the stronger requirement should arise. After all, we are dealing with cut-free sequent calculi, where phenomena like the nonconservativity exhibited by the classical Hilbert-style calculus with respect to its positive fragment simply cannot occur.

Second, other pluralistic views on the phenomenon of logical deviance, similar to the one expounded here, are equally affected by this threat. Kosta Došen [10], for example, sees operational rules for logical constants (in a sequent calculus or otherwise) as mere 'translation rules' from the metalevel of 'logical punctuation marks' — the various devices for aggregating premisses or conclusions, the turnstile — to the object level of connectives and quantifiers, and vice versa. Having an invariant set of such translation procedures, unaltered across different logics, keeps meaning variance from the door, while the essential difference between two logics resides in their respective sets of structural rules, or perhaps structural properties at large. However, it is exactly the presence of differences relative to structural rules that casts doubts on the fact that the 'antecedently given context of deducibility' with respect to which the common translation procedures are established has to be the same in each case. If I am translating a punctuation mark that serves, say, to bunch together premisses relatively to a different notion of deduction, who can prevent a Quinean revivalist from levelling her 'changing the logic is changing the subject' charge once more? Ian Hacking's [18] related viewpoint is similarly jeopardised. For Hacking, logical constants are characterised[14] through the operational rules of a sequent calculus. To ward off these characterisations from attacks along the lines of Prior's Tonk, however, we have to guarantee that they are *conservative*, and this can only happen if a number of formal criteria are met by the calculus we are working with: cut elimination, deducibility of identicals (all instances of $A \Rightarrow A$, for A arbitrary, must be provable from atomic instances of the form $p \Rightarrow p$, for p a variable) and elimination of weakening (weakening rules with atomic principal formulas must be sufficient to derive weakening rules with arbitrary principal formulas). In the absence of these requirements, one would not be 'defining logical constants in connection with some previous language fragment' (p. 298): Belnap's insistence on defining connectives against the backdrop of an antecedently given context of deducibility would come to nothing. But the standard set of structural rules, Hacking argues, uniquely characterises classical logic among logics over a classical language: so, what possible rôle is left for its aspiring rivals? Hacking has a fleeting but insightful comment on this:

> The structural rules formalize the "pure" theory of classical logical consequence. It is my contention, not developed here, that a nonclassical logic has a right to be called logic […] just if there is a different semantic framework, with respect to which some nonclassical structural rules are complete [18, p. 312].

And it is evident that we are dipping our toes again into the meta-Quinean quagmire.

There are two ingredients in my reply to the objection. I have already sketched the first one, but let me dwell a bit more on that, taking the paradigmatic example of classical conjunction. Part of its meaning, if we follow the view defended thus far, is given by its right introduction rule, according to which we can warrantedly assert the formula $A \wedge B$ (within a given context) provided we can warrantedly assert its subformulas A, B (within the same context). The same condition provides part of the operational meaning of the extensional **LL** conjunction $\sqcap$. But this is tantamount to say that the same aspect of the meaning of both constants is manifested through the fact that a conjunction *externally follows from its conjuncts* (within a given context). Dual considerations apply to the left introduction rule for conjunction, and thus to the other aspect of its inferential rôle (what consequences I am supposed to draw from the assertion of a conjunctive sentence). Operational meaning, as we have already observed, is hand in glove with preservation

[14] Hacking refrains from using the word 'define' because definitions can be interpreted as reductions of some *definiendum* to ideas that are better understood, and he concedes that sequent calculus operational rules serve no such purpose.

of the warrant to assert, and thus with external consequence. The qualification 'within a given context' is important, because the behaviour of side formulas obviously affects the operational rules we are considering (it makes the difference, for example, between the right introduction rules of meet and of fusion), but inessential to this specific aspect: what characterises (part of) the meaning of *any* conjunction is the fact that whenever I accept its conjuncts (whatever is the information extraction procedure that led me to accept them), I am committed to accepting the conjunction itself. The information extraction procedures at issue may differ from one logic to another, and, accordingly, the sequent separator marking internal consequence may well obey different rules; however, that meaning-determining schema does not vary across all logics whose conjunction fits the above vertical pattern. Although the sequent separator $\Rightarrow$ may be a false friend, the *fraction line* separating the sequent premisses from the conclusion in an operational rule is not.

Why is such a claim plausible? Here comes the second ingredient of my reply. Classical logic (seen as a multiset consequence relation) and the external consequence relation of **LL** coincide over the purely structural fragment: for any multiset Γ of propositional variables, $\Gamma \vdash_{\mathbf{CL}} p$ if and only if $\Gamma \vdash^{E}_{\mathbf{LL}} p$. If, as argued above, the external consequence relation is the meaning-assigning one, then classical connectives and linear connectives are introduced in terms of the same antecedently given context of deducibility. This does not mean, of course, that **CL** and **LL** agree about what inferences preserve the warrant to assert, as witnessed by the fact that their external consequence relations differ. Only, that this disagreement does not affect the prelogical language and the purely structural fragments of these logics. Hacking was not aware of this because he adopted a viewpoint that was commonplace at the time his paper was written, considering internal consequence as the sole concept of consequence that a sequent calculus could generate. If we go along with this assumption, then of course the only margin for variation we are left with, if we want to keep the operational rules untouched, is in the structural properties of the logic. But this assumption is unwarranted. As we have just seen, you can change your logic (your set of derivable sequents) without changing the subject (the operational properties of your connectives, the antecedently given context of deducibility in terms of which they are defined).

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