Relevance in Reasoning

by

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Abstract

This essay explains an approach to relevantization of logical reasoning that seeks to maximize epistemic gain. It does so by retaining Disjunctive Syllogism, and making admissible only a restricted rule of Cut. The virtue of this approach is that one can show that the resulting relevant logic is adequate for mathematics and science. In the course of explaining this kind of relevant logic, we compare and contrast our approach with the Anderson–Belnap tradition.

1 Introduction

We advance here an unabashedly partisan view of how best to 'relevantize' a logic. The view is laid out as informally as possible, given the technical nature of the subject matter.

Here, we understand 'relevantizing' as the project of formulating a decent system of logic that does not endorse Lewis's First Paradox:

$$A, \neg A : B.$$

Such a system will be *paraconsistent*, in that it will allow for distinct inconsistent theories (within a given language). But it will not be *dialetheist*.

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That is, it will not allow for true contradictions. Dialetheism does not follow from (though, in order to avoid trivialization, it requires) a refusal to infer whatever one pleases from a contradiction.

We can pose our question as follows:

> *How best might one restrict the deducibility relation of a familiar system of logic (such as intuitionistic or classical logic) so as to avoid the First Lewis Paradox, but still provide all the proofs needed for mathematics and for the hypothetico-deductive method in natural science?*

We are not taking sides in the debate between the classicist and the intuitionist. We seek to enable the intuitionistic [resp. classical] mathematician to do intuitionistic [resp. classical] mathematics with just the relevant fragment of intuitionistic [resp. classical] logic. The relevantized restriction of each system is to be judged by reference to the original system that has been restricted. Once we have found the right restrictions, the resulting relevant systems ought to be as smooth and as elegant and as powerful as possible. These are of course evaluative notions. But we believe they can be made to apply, even in the eye of the logical beholder.

2 Some historical background

The method of relevantizing that is to be explained in this essay derives from work by the author beginning with [15] and [16], and further developed in [17], [19], [18], [20], [22], [23], [21], [24], [25] and [26]. It stands outside the so-called Anderson–Belnap tradition (for which, see [1]).

It does not fall within the scope of this discussion to examine the details of the Anderson–Belnap tradition. (A few more details will be provided in a later section.) I do need, however, to explain why this essay is devoted to an alternative approach. I take the very title assigned to me—*Relevance in Reasoning*—to be an invitation to discuss (what I judge, on balance, to be) a *viable* account of how mathematical and scientific reasoning can be 'relevantized' when it is logically regimented.

I believe that Burgess—building on the earlier critical contributions of Copeland [6], [7]—prevailed in his critique, in the early 1980s, of Anderson–Belnap relevance logic. This critique was laid out in his well-known *Notre Dame* exchange with Mortensen and Read. (See Burgess [3], [4] and [5], Mortensen [11], and Read [14].)

One must nevertheless hasten to point out that, however diverse and protean the Anderson–Belnap tradition might be (as remarked by Burgess

[5], p. 217), Burgess's critique was directed *only* against this tradition. In [3], p. 104, he wrote

> ...I have been concerned here solely with the original Anderson-Belnap account of "relevant" logic, and with their claim that their systems E, R, etc., are in better agreement with commonsense than is classical logic.

Again, in [4], p. 41, he wrote 'All relevantists agree in rejecting *disjunctive syllogism* ... ' (emphasis in original).

While that might have been true of all relevantists known to Burgess at the time of writing, it is so no longer. As we shall see presently, one can be a relevantist yet preserve disjunctive syllogism as an essential derived rule of one's logic. Although many relevantists (such as Anderson and Belnap) object to Disjunctive Syllogism, I do not. It strikes me, intuitively, as being in perfect order. In this I side with Burgess.

Mathematics would be crippled without Disjunctive Syllogism. There is no metatheorem showing that classical (or intuitionistic) mathematics survives any Anderson–Belnap style of relevantizing proofs of mathematical theorems from mathematical axioms, especially if one imposes the reasonable requirement that the formal relevantized proofs bear a natural structural resemblance to their informal counterparts in ordinary mathematical practice. Burgess's critique of Anderson–Belnap relevant logic in the *Notre Dame* exchange focussed clearly on this, its main shortcoming. In [5], pp. 222–3, Burgess observed that (then unpublished) work of R. K. Meyer

> ... demonstrates beyond doubt that standard mathematical arguments cannot be formalized relevantistically ... For half of the most famous theorems of elementary number theory (including the theorem that every integer is a sum of four squares) it seems that no relevantisitically acceptable proof is known, for all Meyer's work. For the other half of the most famous theorems of elementary number theory (including the theorem that no cube is the sum of two cubes) relevantistic proofs are available, but they cannot be regarded as formalizations of standard proofs. They involved carrying along caveats of the form "unless $0 = 1$", which one would never have heard from the lips of Fermat or Euler or Lagrange or Gauss or Louisville, and eliminating these caveats at the end of the proof by various manipulations. Meyer's manipulations are undeniably clever, but the very need for such cleverness demonstrates that relevantism conflicts with standard mathematical practice.

> It seems that a relevantist [in the Anderson–Belnap sense—NT] *must* be a revisionist, at least as far as mathematics is concerned ...

Exactly how the work of Meyer in question (presumably [10]) 'demonstrate[d] beyond doubt that standard mathematical arguments cannot be formalized relevantistically' is not altogether clear from what Burgess says. Since the time of Burgess's critique, however, there has been a highly significant, dispositive result in this regard. Friedman has definitively proved (see Friedman and Meyer [9]) that there are indeed limitations to relevant arithmetic: there is a strictly positive theorem of Peano arithmatic that cannot be proved in the relevant Peano arithmetic $R^{\#}$. ($R^{\#}$ is the closure of the Peano axioms under the first-order version of the Anderson–Belnap relevance logic R.)

We shall return in a later section to a comparison between the Anderson–Belnap approach and the one to be commended here. At this point we must prepare the ground for the latter.

3 What do we require of a logic?

We begin by asking a question that is seldom raised explicitly, even when departures from classical logic are being proposed. *What, exactly, do we require of a logic?* Logic is an instrument of reasoning, and is central to our intellectual investigations. But what are the exact uses that we need to be able to make of it?

These uses will concern three main logical properties or relations:

(i) 'genuine' logical consequences (where a claim that is falsifiable follows from a set of assumptions that is satisfiable);

(ii) logical falsehoods; and

(iii) logical truths.

We take our task to be that of *restricting* what can be proved, in either intuitionistic or classical logic. In each case, the relevant fragment will be contained in the non-relevant logic concerned. Moreover, because the systems of intuitionistic and classical logic have their respective notions of validity, and their own completeness theorems, we shall be able to speak interchangeably of deducibility or logical consequence; of logical truth or theoremhood; and of unsatisfiability or inconsistency.

We shall be eschewing the *non-relevant parts* of the deducibility (or consequence) relation in each of these logics. Fallacies of relevance will not be regarded as genuine logical consequences, even when they appear to count as logical consequences according to the standard, Boolean definition that involves preservation of truth under all interpretations. Because we use a sentential definition of object-linguistic logical consequence in the metalanguage:

> X logically implies φ if and only if every model that makes all of X true makes φ true also; i.e.
> $X \models \varphi \leftrightarrow_{df} \forall M[(\forall \psi(\psi \in M \rightarrow M \models \psi) \rightarrow M \models \varphi]$,

it is no surprise that the First Lewis Paradox comes out as a logical consequence in the object-language. The metalinguistic claim

> Every interpretation that makes A and makes $\neg A$ true makes B true also; i.e.,
> $\forall M[(M \models A \wedge M \models \neg A) \rightarrow M \models B]$

is, indeed, a metatheorem even in a *relevantized* metalanguage. The question of genuine logical consequence should rather be posed as follows: Is it possible, in the metalanguage, to deduce from the premises that A is true under interpretation M and that $\neg A$ is true under interpretation M, the conclusion that B is true under interpretation M? When the question is posed this way, the respective answers from the standard metalogician and the relevant metalogician will differ. The standard metalogician will say 'yes', the relevant metalogician will say 'no'.

We now devote a subsection of clarification to each of our requirements (i), (ii) and (iii) above.

3.1 *Ad (i): Genuine consequence*

Our logic should enable us to draw those conclusions that follow from whatever satisfiable assumptions (or axioms) we might adopt. Two areas of inquiry immediately come to mind. In *mathematics*, we should be able to derive all consequences of our axiom systems (assuming that these have models); and in the *natural sciences*, we should be able to derive all the empirical predictions that would follow from our scientific hypotheses taken in (consistent) conjunction with statements of boundary conditions, initial conditions, etc. In both these cases, we are considering (what we believe to be) satisfiable sets of assumptions, and are concerned to be able to deduce from them all falsifiable conclusions that follow from them. Let us devote the next two paragraphs to underscoring this last claim.

In the case of mathematics, we take our axioms to be consistent (hence, satisfiable); and we seek proofs of non-trivial *mathematical*, i.e. non-logical, theorems from them. It is in this sense that a non-trivial mathematical theorem is 'falsifiable': for any such theorem p, there will be *some* logically possible interpretation of the extra-logical primitives (the specifically 'mathematical' vocabulary) that will make p false. Of course, such an interpretation will not be one in which the mathematical axioms themselves are true.

In the case of natural science, we take our scientific hypotheses to be consistent with the statements of such boundary conditions and initial conditions as might be involved in an experiment to test the predictions derivable from those combined assumptions. Call the set of such statements Δ. Note that Δ is consistent. It is only when observations and measurements are made, yielding further statements (say Γ) treated as extra assumptions, that a contradiction might follow: $\Delta, \Gamma \vdash \perp$. Then, but only then, will the evidence Γ, potentially, falsify the hypotheses in Δ. (The famous so-called Duhem-Quine problem, of course, is to decide which statements in $(\Delta \cup \Gamma)$ to reject.) Note again that the predictions derived from Δ will be logically falsifiable statements. No natural scientist would ever wittingly produce a logical truth as a supposedly empirically 'testable' prediction of a scientific theory!

3.2 *Ad (ii): Logical falsehoods*

If in either case (mathematical or natural-scientific) the assumptions in question are *not* satisfiable, then we require only that our logic be able to reveal the fact: that is, that it furnish a proof of their inconsistency. Our logic should be able to tell us when a mathematical investigation would be fruitless to pursue, and when a scientific theory has run foul of the data.

3.3 *Ad (iii): Logical truths.*

Thus far we have focused on assumptions. But what about conclusions? We are interested in conclusions that follow from our assumptions (assumed to be satisfiable). But certain conclusions, called logical truths, have a nasty way of 'following' (in standard systems) from *any* set of assumptions. So there is nothing special about deducing them 'from' any particular satisfiable set of assumptions (let alone any particular *un*satisfiable one!). What is special about logical truths is that one needs no assumptions at all in order to establish them. So let us require of our logic no more than that. Let us insist, with regard to logical truths as potential conclusions, only that our logic should furnish proofs of them *from no assumptions at all.*

3.4 Some definitions

The set X of assumptions is unsatisfiable just in case the sequent $X : \emptyset$ is valid. And the sentence φ is logically true just in case the sequent $\emptyset : \varphi$ is valid. (We write φ for the succedent when the latter is the singleton $\{\varphi\}$.) We shall also define the useful notion of *perfect* validity: a sequent is perfectly valid just in case it is valid and has no valid proper subsequents. That is, every sentence on the left and every sentence on the right is needed for the validity of the sequent concerned.

3.5 Summary of methodological requirements

We can summarize our three methodological requirements thus far as follows:

1. If $X : \emptyset$ is valid, then there should be a proof of $X : \emptyset$.
2. If $\emptyset : \varphi$ is valid, then there should be a proof of $\emptyset : \varphi$.
3. If $X : \emptyset$ is invalid and $\emptyset : \varphi$ is invalid, but $X : \varphi$ is valid, then there should be a proof of $X : \varphi$.

Note that requirements (1) and (2) take care of the two extremes, logical falsity and logical truth, respectively. And requirement (3) takes care of what we are here calling 'genuine' logical consequence.

Moreover, (1) is all that is needed for the hypothetico-deductive method of testing empirical theories. In so far as *deductive* logic features in scientific reasoning, it is in the context of inferring predictions, or refuting theories *modulo* observational evidence (once certain predictions have been inferred). For the refutations, (1) suffices (with the observational evidence reckoned to X, along with the theory being refuted). For the derivation of predictions, (3) suffices, as noted above (since in such a case X does not contain any observational evidence that might refute it).

Finally, in the case of mathematical reasoning, one is interested only in two things: (α) being able to reveal that one's axioms are unsatisfiable, if indeed they are (a very rare occurrence); and (β) being able to deduce all interesting consequences of one's axioms, if indeed those axioms are satisfiable. Note that (1) takes care of (α). And in connection with (β), as already noted, no mathematician is going to find a *logical truth* to be an 'interesting consequence' of her axioms. She will be interested only in such consequences of her axioms as are not, themselves, logically true. Hence (3) takes care of (β). So, for the purposes to be served by a system of logic within mathematics, (1) and (3) suffice.

4 Cumulative deductive progress

There is another aspect of the deductive enterprise, however, which is of special importance. We need to be able to make *cumulative deductive progress.* In mathematics, for example, we have the practice of proving interesting lemmas as 'halfway houses' on the way to those deeper results that we dignify with the label 'theorem'. (From a logical point of view, of course, these psychological classifications are irrelevant.) We need, therefore, to be able to do the following:

1. prove a lemma ψ, say, from axioms X;
2. then prove a theorem φ, say, from the lemma ψ along with further axioms Y;
3. finally, conclude to φ on the basis only of $X \cup Y$.

In a natural-deduction setting, where proof-trees have their assumptions at leaf-nodes, and have their conclusions at their root-nodes, this two-step process can be pictured as follows:

$$\begin{array}{c} X \\ \Pi \\ \underbrace{Y, (\psi)} \\ \Sigma \\ \varphi \end{array}$$

The lemma ψ stands as the conclusion of the proof Π and as a premise of the proof Σ. It is an 'accumulation point' of the overall proof-tree. Proofs are defined in such a way that accumulations of proof-trees are themselves proof-trees. This is because, in the standard non-relevant systems, proofs are allowed *not* to be in normal form. Here, for example, the sentence ψ is likely to stand as the conclusion (within the natural deduction Π) of an introduction rule, and as the major premise (within the natural deduction Σ) of the corresponding elimination rule. So the 'grafting points' ψ could in general be maximal sentence-occurrences, and the overall proof of φ from $X \cup Y$ accordingly not in normal form.

In a sequent setting, where the nodes of proof-trees are labelled with the whole sequent that has been established thus far, the same cumulative effect is achieved by applying the rule of Cut:

$$\frac{\begin{array}{c}\Pi \\ X : \psi\end{array} \quad \begin{array}{c}\Sigma \\ Y, \psi : \varphi\end{array}}{X, Y : \varphi}$$

In general, dividing one's deductive work in this way into two stages—the first stage, Π, towards the lemma ψ, the second stage, Σ, away from it—enables one to reduce quite dramatically the overall length of proof. The combined length of the two proofs Π and Σ is in general much less than the length of whatever proof might codify an argument proceeding *directly* from $X \cup Y$ to φ without 'going through' the lemma ψ. Abnormality of the overall proof is the price one pays for the reduction in length of proof that results from interpolating the lemma ψ between one's axioms $X \cup Y$ and the sought theorem φ.[1]

That is all very well; but considerations of feasibility or tractability can serve at best to *explain* unjustifiable deductive procedures, rather than to justify them. Here is the explanation why the procedure of 'proceeding via lemma ψ' works, according to the pattern just given, in mathematics:

> Mathematics is (presumed to be) *satisfiable*. So any collection of our mathematical axioms is satisfiable. Moreover, φ is a logical consequence of $X \cup Y$ (and, presumably, an interesting one, i.e. one that is not a logical truth in its own right). Hence one should be able to deduce φ from axioms drawn from $X \cup Y$. Indeed, given that one has already deduced ψ from X, and deduced φ from Y and ψ, further deductive work would be superfluous: we already know, by virtue of these two proofs, and on the presumption that the overall set of premises $X \cup Y$ is satisfiable,

[1] In this connection, Harvey Friedman has achieved, in as yet unpublished work, some spectacular results about just how bad proof-length blow-up can be upon elimination of cuts when formalizing everyday mathematical proofs of some very accessible mathematical results. George Kreisel and Friedman, among others, knew in the 1960s about the *existence*, i.e. the possibility-in-principle, of such blow-ups; but it is only in Friedman's more recent work that these blow-ups have been shown to arise within ordinary mathematics, involving informal proofs of feasible length (but containing cuts). For another striking example, albeit one that was carefully contrived to serve this specific theoretical purpose, see Boolos [2].

that $X, Y : \varphi$ is valid.

We cannot, however, conclude that $X, Y : \varphi$ is *perfectly* valid, even if both $X : \psi$ and $Y, \psi : \varphi$ are perfectly valid. Courtesy of Cut, therefore, we push back the frontiers of deductively generated mathematical knowledge. But we are using a rather blunt instrument in doing so. For consider: *if* (admittedly, a big 'if') our axioms $X \cup Y$ are inconsistent, might we not, in applying Cut, miss this fact altogether, in charging ahead and inferring φ? Moreover, we do not need to consider possible inconsistencies in our axioms in order to see that we might still miss out on opportunities to be more economical with them. We might not realize, for example, that the grain of truth in ψ that is needed, along with our choice Y of axioms, to yield φ, might be considerably less weighty than the bushel that is ψ itself, inferred from the axioms X. In taking ourselves (courtesy of Cut) to have inferred φ 'from' $X \cup Y$, we might be missing the fact that φ follows from some *proper subset* of $X \cup Y$ (even if neither from X itself nor from Y itself).

Furthermore, it is not enough for the natural-deduction theorist[2] to respond by suggesting that one should simply normalize the overall proof of φ from $X \cup Y$ in order to prune away such assumptions as might not be needed for the resulting proof in normal form. If the system within which such normalization is carried out tolerates irrelevancies such as the First Lewis Paradox, then potential 'reductions in premises' can *still* escape detection!

5 Avoiding the First Lewis Paradox: Banning Dilution

If we wish to avoid the First Lewis Paradox, we have to disable its simplest proofs. Here is one, in the sequent calculus:

$$\dfrac{\dfrac{A : A}{A, \neg A :}\ \neg L}{A, \neg A : B}\ \text{Dilution}$$

There cannot be anything wrong with the rule of initial sequents $A : A$. Nor can there be anything wrong with the rule

$$\neg L \quad \dfrac{X : A}{X, \neg A :}$$

[2] An analogous remark would of course apply to the sequent-proof theorist.

for introducing a negation sign on the left of the colon.

There remains only one diagnostic possibility: the culprit here must be the final step of Dilution (on the right).[3] So, in the interests of relevance, Dilution (on the right) must be banned.

So too must Dilution on the left. Otherwise, we shall be able to prove the Lewis-type sequent $A, \neg A : \neg B$, which is just as objectionable, on relevantist grounds, as the sequent $A, \neg A : B$. If the arbitrary proposition B bears no connection to the premises involving A, then neither does its negation $\neg B$. With Dilution on the left, however, we can form the following otherwise unobjectionable proof:

$$\dfrac{\dfrac{\dfrac{A : A}{A, B : A}}{A, \neg A, B :}}{A, \neg A : \neg B}$$

In the context of the little example involving the First Lewis Paradox, the ban on Dilution means that the proof above would get only as far as

$$\dfrac{A : A}{A, \neg A :}\ \neg L$$

And isn't that just as it should be? It is, after all, a more informative result than $A, \neg A : B$. (This is an intuition to be explicated presently.) In general, given any sequent of the form $X : B$, it might be quite difficult to tell that B 'follows' from X by dint of X's inconsistency, rather than by dint of any genuine deductive connection between X and B. We can lose sight of this general point when looking at the Lewis sequent $A, \neg A : B$, because its antecedent is so obviously contradictory. But with generality we lose obviousness. And, I contend, it is better to know that X is inconsistent rather than to know only that X logically implies B. The sequent $X : \emptyset$ represents *epistemic gain* over the sequent $X : B$.

6 Epistemic gain in logic

What exactly is epistemic gain? It is a matter of learning that a *tighter* logical result holds than one had previously thought. Examples of epistemic gains made on the deducibility statement $A, B \vdash C$ might be

[3] Dilution is often called Weakening.

- One finds a proof of the sequent $A : C$, thereby learning that B is unnecessary as a premise;
- One finds a proof of the sequent $A, B : \emptyset$, thereby learning that one's premises are inconsistent;
- One finds a proof of the sequent $\emptyset : C$, thereby learning that the conclusion one was trying to prove 'from' the premises A and B is a logical truth.

A proof of any of these *proper subsequents* of the original sequent $A, B : C$ would represent epistemic gain. Knowing that $A \vdash C$ is better than knowing that $A, B \vdash C$; and likewise for knowing that $A, B \vdash \emptyset$ or knowing that $\vdash C$. The more one 'subsets down', on the left and/or on the right, while still achieving the turnstile, the stronger the logical result one has learned. Of course, one cannot subset down all the way, since there is no proof of the empty sequent $\emptyset : \emptyset$. So one will always achieve a local optimum in any course of 'subsetting down'. In an obvious sense, it will be a '(locally) strongest possible result'. It will be a valid sequent that has no valid proper subsequent. Remember that we are calling such sequents *perfectly valid*.

An easy corollary of a theorem to be stated below, in conjunction with the usual completeness theorems, is that every perfectly valid sequent in classical (resp. intuitionistic) logic has a classical (resp. intuitionistic) *relevant* proof.

7 Avoiding the First Lewis Paradox: Banning Cut

We have seen that Dilution is a source of irrelevance. But what about Cut? Cut is a structural rule in a sequent system that registers the (assumed) *unrestricted transitivity of deduction*. The orthodox statement of the unrestricted transitivity of deduction (in single-conclusion systems) has the form

If $X \vdash B$ and $Y, B \vdash C$ then $X, Y \vdash C$.

Correspondingly, the structural rule of Cut allows one to infer from two premise-sequents to a conclusion-sequent as follows:

$$\frac{X : B \quad Y, B : C}{X, Y : C}$$

If we can get rid of Cuts (as Gentzen's Hauptsatz says we can), why should we be worried about having the rule of Cut in our deductive system? The simple answer is that Cut too can be a source of potentially undetected irrelevance. For, consider the following two perfectly good proofs, of $\vee$-Introduction and of Disjunctive Syllogism respectively:

$$\frac{A : A}{A : A \vee B} \qquad\qquad \frac{\dfrac{A : A}{A, \neg A :} \qquad B : B}{A \vee B, \neg A : B}$$

Now, if we apply Cut to the sentence $A \vee B$, we obtain the unwanted Lewis sequent $A, \neg A : B$.

So: if we wish to avoid the First Lewis Paradox, we must either ban Disjunctive Syllogism or ban Cut. We wish to avoid the First Lewis Paradox. Therefore, we must either ban Disjunctive Syllogism or ban Cut. But we cannot ban Disjunctive Syllogism without unacceptable consequences for the codification of mathematical practice. My own methodological conclusion, therefore, is that *Cut* must be banned *as a rule within the system.* And I note the virtuous irony in the fact that I am arguing for this methodological conclusion by means of (a deontic version of) Disjunctive Syllogism:

> We must either ban Disjunctive Syllogism or ban Cut.
> We should not ban Disjunctive Syllogism.
> *Ergo*, We should ban Cut.

7.1 On how best to 'ban' Cut

To say that Cut must be banned is *not* to say that we shall thereby forfeit transitivity of deduction altogether. Our conclusion 'We should ban Cut' should really be written: *We should ban Cut in those situations where its use can allow irrelevancies to creep in to our reasoning. But we expect the vast preponderance of cuts in actual mathematical and scientific reasoning to be innocuous in this regard.* Making good on this informally expressed expectation is the main task before us.

Recall our earlier contrast between the statement of transitivity (which involved turnstile-claims) and the rule of Cut as a means of inferring one sequent from two others. As far as the transitivity of deduction is concerned, our relevant systems will still enjoy it—*where it counts.* For the following metatheorem holds for the relevant systems:

If $X \vdash B$ and $Y, B \vdash C$ then $[X, Y] \vdash [C]$.

(Remember that $[X, Y]$ ranges over subsets of $X \cup Y$, and $[C]$ over subsets of $\{C\}$. We shall also use '$X \vdash \bot$' as a synonym for '$X \vdash \emptyset$', and talk of being able to deduce absurdity ($\bot$) from X.) Another way of stating this metatheorem would be to say that Cut, though no longer applicable unrestrictedly (as it was in the non-relevant systems C and I), will nevertheless be 'gainfully admissible' in our respective relevant systems *CR* and *IR*.

This statement of *restricted* transitivity of deduction is all that one needs for the serious scientific purposes broached above. Those purposes, as we have already seen, are (1) to prove the inconsistency of any unsatisfiable set of assumptions, (2) to prove all logical truths from no assumptions, and (3) to derive all falsifiable consequences of any satisfiable sets of assumptions. There is nothing for a canon of deductive reasoning to accomplish over and above these three aims. By seeking to provide more, the classical Tarskian notion of consequence or deducibility is guilty of a misleading *over-provision*, which can blind one to epistemic gains that stand to be made by the logically more wary. *Without a rule of Cut*, one can still be assured of transitivity of deduction wherever it matters. Why, then, spoil that austere provision by adopting Cut as a rule? The only answer is: in order to achieve speed-up in the search for 'proofs'. But that is a merely pragmatic matter of tactics, of strategy, and of expenditure of time and energy. It has nothing to do with normative issues of justification.

There is no rule of Cut, then, within our relevant systems. And Cut in its orthodox unrestricted form is not even an *admissible* rule for either of the systems *IR* and *CR*. For in each of these systems we have

$P \vdash P \vee Q$ and $\neg P, P \vee Q \vdash Q$ but $P, \neg P \nvdash Q$.

The orthodox Deduction Theorem for logical calculi states that

(i) $X \vdash B$ only if $X \setminus \{A\} \vdash A \rightarrow B$; and
(ii) $X \vdash A \rightarrow B$ only if $X, A \vdash B$.

In *IR* and *CR*, the implication (i) holds. But the converse implication (ii) fails, for in these relevant systems we have $\neg A \vdash A \rightarrow B$ but not: $\neg A, A \vdash B$.

Notice that the two little proofs that were put together above for a final step of Cut are so degenerate that the inconsistency of the antecedent of the final sequent is obvious. But it is worth stressing once again that in general this will not be the case. One might have proved $X : B$ (with X consistent) and $Y, B : C$ (with Y, B consistent), while yet the final sequent $X, Y : C$ obtained by Cut has an *inconsistent* antecedent X, Y. The latter inconsistency, however, might go undetected; the Cut-applier might be unaware of it. Accumulating proofs for deductive progress, by means of Cut, brings with it the risk that the overall result is *relevantly unsound.* It might be that the conclusion C 'follows from' the set of premises $X \cup Y$ 'only because' $X \cup Y$ is inconsistent.

Our method of relevantizing can be thought of as a measure for logical quality-control. It is always in search of epistemic gain. It seeks to ensure that the results we can prove 'relevantly' are those whose conclusions do not follow from their premises only because of the joint inconsistency of those premises. Viewed another way, it seeks to ensure that the deductive progress afforded by the transitivity of deduction is *genuine* deductive progress. Pooling of assumptions must not get us consequences on the cheap. For such consequences are worth no more than the assumptions that they depend upon. Better to know that the pooled assumptions are inconsistent, than to continue in ignorance of this fact, and rely on any conclusion that has been 'deduced' from them.

The fact that Cut is not a rule in our relevant systems does not mean that someone possessed of relevant proofs

$$\begin{array}{c} X \\ \Pi \\ \psi \end{array} \qquad \begin{array}{c} \underbrace{Y, \psi} \\ \Sigma \\ \varphi \end{array}$$

would be at a loss to find a relevant proof of some subsequent of $X, Y : \varphi$. In complaining that the Rule of Cut is too crude an instrument of cumulative deductive progress, I do not intend to leave the reasoner unable to achieve the benefits of accumulation. In situations such as these, the sought relevant proof can be obtained as follows. (For perspicuousness of display, we switch here to natural-deduction mode.)

1. Graft Π on top of Σ as one would normally do. Call the resulting proof-tree Θ:

$$\begin{array}{c} X \\ \Pi \\ \underbrace{Y, (\psi)} \\ \Sigma \\ \varphi \end{array}$$

2. Normalize Θ:

$$\Theta \mapsto \begin{array}{c} Z \\ \Xi \\ \varphi \end{array}$$

   The resulting normal proof Ξ has conclusion φ, and its set Z of undischarged assumptions is a subset of $X \cup Y$.

3. Extract from this normal form Ξ a *relevant* proof, of either $\perp$ or φ, from some subset of Z.

That is to say, our metatheorem stated earlier:

If $X \vdash B$ and $Y, B \vdash C$ then $[X, Y] \vdash [C]$.

has a constructive proof. (Z will be the subset of $X \cup Y$ that makes the consequent hold.)

To be sure, step (2) can result in exponential blow-up. That is to say, the length of the normal proof produced as output in step (2) is bounded below by an exponential function in the length n of the non-normal, input proof—in the sense that, for any n, some input proof (not in normal form) of length $k > n$ produces an output proof (in normal form) of length $> 2^k$.[4] Step (3), however, can be completed in polynomial time. That is to say, there is some polynomial function $p(n)$ such that for all m, for all input proofs (in normal form) of length $< m$, the relevantized proof (of course, still in normal form) is produced in fewer than $p(m)$ units of time. Normalizing carries a heavy computational price, whether or not one is a relevantist. But at least going one step further and relevantizing adds negligibly to the computational costs involved. The potential epistemic gains come cheap at this price.

8 The metalogical upshot

Our motivation can be summarized, but now in the sequent calculus setting, as the requirement that we should be able to prove a Dilution Elimination Theorem *in addition to* the well-known Cut Elimination Theorem. That means that we must not rely on having Dilution in the system in order to show that the system admits of Cut-elimination.

[4] Indeed, Orevkov has demonstrated *hyper*exponential blow-up, by exhibiting a hyperexponential function $f(k)$ and a sequence of sentences C_k each of which has a non-normal proof of length linear in k, but no normal proof of length $< f(k)$. See Orevkov [12].

Gentzen's famous Hauptsatz, or Cut-elimination theorem, is that applications of Cut can be eliminated from any proof. Gentzen happened to formulate his logical rules in such a way that in eliminating cuts from proofs one would need to be able to take steps of Dilution in order to ensure that newly required applications of the logical rules would be formally correct. Perhaps that is why Gentzen himself never raised the question whether one could eliminate Dilutions from proofs, along with Cuts. Given his chosen way of stating the logical rules, the answer would have appeared to be an obvious negative. Since, however, we are avoiding Dilutions as well as Cuts—because these are sources of irrelvance—we shall need to formulate the logical rules in a subtly different way from Gentzen.

The motivating considerations behind our approach to capturing relevance determine not only the form of the rules for the logical operators, but also some very general restrictions on their applications in proofs. These restrictions are imposed in order to avoid Dilutions. Of course, if we can get rid of all dilutions in a proof, what we end up proving is some *subsequent* of the original sequent proved. But that is precisely what I call epistemic gain.

The relevantizing of both intuitionistic logic I and classical logic C is accomplished uniformly, by means of the same techniques. Thus considerations of relevance are orthogonal to the considerations of constructivity that lead one from classical logic C to its intuitionistic subsystem I.

We relevantize both C and I, then, by modifying the usual Tarskian assumptions about what is desired of a deducibility (or consequence) relation. If one needs a slogan to help with orientation, ours is a method of relevantizing 'at the level of the turnstile'. As a result, there will be some tweaking of the rules for the logical operators (in the natural deduction or sequent setting); but these tweakings are not such as to change their established meanings.

Before I state the rules of the system *IR*, it is worth mentioning two of its proven virtues. It suffices for intuitionistic mathematics[5] and for the hypothetico-deductive method in science.[6] So for all serious epistemic purposes (from an intuitionistic point of view), *IR* is a system that relevantizes without loss. Exactly analogous claims hold in the classical case. The relevant system *CR* suffices for classical mathematics and for the hypothetico-deductive method in science. So for all serious epistemic purposes (from a classical point of view), *CR* is a system that relevantizes without loss.

[5] Cf. [22].
[6] Cf. [18].

9 The system *IR*

9.1 Natural Deduction

We now set out the system of natural deduction for *IR*. We begin with three explanatory remarks, and an explanation of some new notational conventions.
(i) In all applications of the elimination rules stated below, the major premise *stands proud*; that is, it is not the conclusion of any rule.
(ii) No applications of the absurdity rule are permitted.
(iii) All elimination rules are in 'parallelized' or 'general' form, including those for $\rightarrow$ and $\wedge$.

We also need to explain the perhaps unusual notational conventions used below, involving boxes and diamonds appended to discharge strokes over assumptions used for the sake of argument. The *permissive construal* with a discharge-rule is that one is permitted to discharge any assumption of the indicated form, *if* one has used it. The permissive construal allows one to apply a so-called 'discharge'-rule even in cases where no assumption of the indicated form has been used. In such a case one could speak of 'vacuous discharge'. In our statement of the rules below, this permissive construal applies to the rules $(\neg I)$ and $(\rightarrow I)$. Here, permissiveness is indicated by a diamond appended to the discharge-stroke. For all the other discharge-rules, we adopt instead a construal according to which discharge is *obligatory*. Obligatoriness is indicated by a box appended to the discharge-stroke. Here, the affected rules may be applied *only if* at least one assumption of the indicated form(s) has indeed been used to obtain the sub-conclusion concerned. (For the parallelized version of the rule of $(\wedge E)$ given below, one must have used either A or B as an assumption, but not necessarily both.)

The graphic presentations of the following rules do not explicitly reveal an important structural fact. Construed as definitional clauses for the inductive formation of proofs, inference rules with more than one premise allow their immediate subproofs, in general, to have *distinct* sets of undischarged assumptions. For example, the rule of $\wedge$-Introduction, construed as a clause in the inductive definition of the notion 'Π is a proof of A from the set Δ of undischarged assumptions', would read as follows:

> If Π is a proof of A from the set Δ of undischarged assumptions and if Σ is a proof of B from the set Γ of undischarged assumptions, then $\dfrac{\Pi \quad \Sigma}{A \wedge B}$ is a proof of $A \wedge B$ from the set $(\Delta \cup \Gamma)$ of undischarged assumptions.

Such a clause allows the formation of the proof of $A \wedge B$ even if Δ is distinct from Γ. We adopt here the convention that distinct sets of assumptions will be presumed to be allowed, unless there is explicit mention to the contrary. And we shall see below that constraints to the contrary are what characterize the more intractable notion of proof in the Anderson–Belnap system R.

	INTRODUCTION	ELIMINATION
$\neg$	$\dfrac{\begin{array}{c}\Box\overline{\quad}(i)\\ A \\ \vdots \\ \bot\end{array}}{\neg A}\,(i)$	$\dfrac{\neg A \quad A}{\bot}$
$\wedge$	$\dfrac{A \quad B}{A \wedge B}$	$\dfrac{A \wedge B \quad \begin{array}{c}(i)\overline{\quad}\Box\overline{\quad}(i)\\ \underbrace{A\,,\;B} \\ \vdots \\ C\end{array}}{C}\,(i)$
$\vee$	$\dfrac{A}{A \vee B}$ $\quad$ $\dfrac{B}{A \vee B}$	$\dfrac{A \vee B \quad \begin{array}{c}\Box\overline{\quad}(i)\\ A \\ \vdots \\ \bot/C\end{array} \quad \begin{array}{c}\Box\overline{\quad}(i)\\ B \\ \vdots \\ \bot/C\end{array}}{\bot/C}\,(i)$ where neither A nor B remain undischarged
$\rightarrow$	$\dfrac{\begin{array}{c}\Diamond\overline{\quad}(i)\\ A \\ \vdots \\ B\end{array}}{A \rightarrow B}\,(i)$ $\quad$ $\dfrac{\begin{array}{c}\Box\overline{\quad}(i)\\ A \\ \vdots \\ \bot\end{array}}{A \rightarrow B}\,(i)$	$\dfrac{A \rightarrow B \quad A \quad \begin{array}{c}\Box\overline{\quad}(i)\\ B \\ \vdots \\ C\end{array}}{C}\,(i)$ where B does not remain undischarged

Some further explanatory comments are now in order.

9.1.1 Preventing dilutions from creeping in

In the rule of $\vee$-Elimination (proof by cases) the restriction means that the first case-assumption cannot be an undischarged assumption in the second case-proof, and the second case-assumption cannot be an undischarged assumption in the first case-proof. In the rule of $\rightarrow$-Elimination, the restriction means that the consequent B of the major premiss $A \rightarrow B$ cannot be an undischarged assumption in the 'minor' proof of A.

9.1.2 Liberalized proof by cases

The rule of proof by cases, or $(\vee E)$, looks unusual. By stating it graphically as we have, we are providing for the possibility that one of the case assumptions might lead to absurdity ($\perp$). We are then permitted to bring down as the main conclusion whatever is concluded from the *other* case assumption. Thus the rule as stated is shorthand for the following possibilities regarding the sequence *Conclusion of first case-proof, Conclusion of second case-proof, ergo Main conclusion*:

C, C, *ergo* C (including, as a special case, $\perp$, $\perp$, *ergo* $\perp$);

C, $\perp$, *ergo* C; and

$\perp$, C, *ergo* C.

Liberalizing proof by cases in this way is entirely natural, given how we reason informally. Suppose one is told that $A \vee B$ holds, along with certain other assumptions X, and one is required to prove that C follows from the combined assumptions X, $A \vee B$. If one assumes A and discovers that it is inconsistent with X, one simply stops one's investigation of that case, and turns to the case B. If C follows in the latter case, one concludes C as required. One does *not* go back to the conclusion of absurdity in the first case, and artificially dress it up with an application of the absurdity rule so as to make it also 'yield' the conclusion C.

9.1.3 Vacuous v. non-vacuous discharge of assumptions

Note that in the rules for *IR* above we make some unusual demands on the *discharges of assumptions* allowed by certain of these rules. By contrast with minimal logic, we retain the permissive construal only for (the first half of) the rule of ($\rightarrow I$). Neither minimal nor intuitionistic logic insists on non-vacuous discharge when applying the rule ($\neg I$). The intuitionist logician has a reason of sorts for this omission: any application of ($\neg I$) to infer $\neg A$ *without* having used A to obtain the preceding absurdity could simply be regarded instead as an application of the absurdity rule. The latter rule, however, is conspicuously absent from our relevant system *IR*, and for good reason: it leads to the First Lewis Paradox $A, \neg A : B$. Just as disagreeable is the negated-conclusion version of the paradox: $A, \neg A : \neg B$. Thus we have good reason to insist that $\neg I$ be applied only with non-vacuous discharge.

9.2 Sequent calculus

We follow the convention, when stating sequent rules for intuitionistic systems, whereby succedents are at most singletons. But we write the sequents as though their succedents, when non-empty, are single sentences. Thus $X : \{C\}$ is rendered as $X : C$. Another useful notation is $X : [C]$, which is ambiguous between $X : C$ and $X : \emptyset$. Likewise, $[X]$ will range over subsets of X.

The only **structural rule** is the rule of initial sequents

$$A : A$$

All other rules are **logical rules**, for introducing a dominant occurrence of the operator concerned on the *right* or on the *left* of the sequent.

RIGHT	LEFT
$\dfrac{X, A :}{X : \neg A}$	$\dfrac{X : A}{X, \neg A :}$
A not in X	
$\dfrac{X : A \quad Y : B}{X, Y : A \wedge B}$	$\dfrac{X : C}{X \setminus \{A, B\}, A \wedge B : C}$
	$X \cap \{A, B\}$ non-empty

$$\frac{X:A}{X:A\vee B} \qquad \frac{X:B}{X:A\vee B}$$

$$\frac{X,A:[C] \quad Y,B:[C]}{X,Y,A\vee B:[C]}$$

A not in $X \cup Y$, B not in $X \cup Y$;
conclusion-succedent empty
iff both premise-succedents are

$$\frac{X,[A]:[B]}{X:A\to B}$$

A not in X
and either A or B
occurs in the premise sequent

$$\frac{X:A \quad Y,B:C}{X,Y,A\to B:C}$$

B not in Y

10 The system *CR*

10.1 Natural deduction

The natural deduction system for *CR* is obtained by adding to *IR* the rule of classical *reductio ad absurdum*, subject, of course, to the requirement that the assumption for *reductio* really be used:

$$\begin{array}{c} \square\overline{}^{(i)} \\ \neg A \\ \vdots \\ \dfrac{\bot}{A}\,{\scriptstyle(i)} \end{array}$$

We still insist, as we did with *IR*, that major premises of eliminations stand proud. Thus no conclusion of classical *reductio ad absurdum* can be the major premise of an elimination.

The decision problem for theoremhood in *IR* (equivalently, deducibility from finite sets of premises in *IR*) is PSPACE-complete, just as it is for the parent system *I* of intuitionistic logic. The decision problem for theoremhood in *CR* is co-NP-complete, just as it is for the parent system *C* of classical logic.

10.2 Sequent calculus

The sequent calculus for the classical system of relevant logic is obtained in the usual way, by allowing succedents with more than one member.

THE SEQUENT RULES FOR *CR*

RIGHT		LEFT
$\dfrac{X, A : Y}{X : \neg A, Y}$		$\dfrac{X : A, Y}{X, \neg A : Y}$
A not in X		A not in Y
$\dfrac{X : A, Z \quad Y : B, W}{X, Y : Z, W, A \wedge B}$		$\dfrac{X : Y}{X \setminus \{A, B\}, A \wedge B : Y}$
A not in Z, B not in W		$X \cap \{A, B\}$ non-empty
$\dfrac{X : Y, A}{X : Y, A \vee B}$	$\dfrac{X : Y, B}{X : Y, A \vee B}$	$\dfrac{X, A : Z \quad Y, B : W}{X, Y, A \vee B : Z, W}$
A not in Y	B not in Y	A not in $X \cup Y$, B not in $X \cup Y$
$\dfrac{X, [A] : Y, [B]}{X : Y, A \rightarrow B}$		$\dfrac{X : A, W \quad Y, B : Z}{X, Y, A \rightarrow B : W, Z}$
A not in X, B not in Y		A not in W, B not in Y

Unrestricted cut fails to be admissible for *CR*, as it failed for *IR*. But the same compensating metatheorem holds for *CR* as for *IR*:

If $X \vdash B, Z$ and $Y, B \vdash W$ then $[X, Y] \vdash [Z, W]$

where we now allow for more than one sentence in succedents.

11 Contrast with the Anderson–Belnap approach to relevantizing

IR and *CR* are to be contrasted with competing systems in the Anderson–Belnap tradition. These other systems (such as the relevant logic *R*) differ from standard logic by having unusual, 'intensional' connectives, subject to more restrictive axioms (in a Hilbert-style proof system) and rules of inference (in a natural deduction or sequent setting) than the standard connectives. But on the Anderson–Belnap approach, the *deductive structure* of the relevantized logical system is in one important regard the same as in the non-relevant case. The Anderson–Belnap relation of deducibility admits of *unrestricted Cut*.

The Anderson–Belnap system *R*, like the systems of classical and intuitionistic logic, was originally presented as a Hilbert-style proof-system based on many axioms and one or two rules of inference. Since we have been using natural deduction in this essay, we present the system *R* here in that format, in order to facilitate comparison.

	INTRODUCTION	ELIMINATION
$\neg$	$\begin{array}{c} \square\text{—}(i) \\ A \\ \vdots \\ \dfrac{\bot}{\neg A}\,(i) \end{array}$	$\dfrac{\neg A \quad A}{\bot}$
$\wedge$	$\begin{array}{c} \Delta \quad \Delta \\ \vdots \quad\ \vdots \\ \dfrac{A \quad B}{A \wedge B} \end{array}$	$\begin{array}{c} (i)\text{—}\square\text{—}(i) \\ \underbrace{A\ ,\ B} \\ \vdots \\ \dfrac{A \wedge B \quad C}{C}\,(i) \end{array}$

$\vee$

$$\frac{A}{A \vee B} \qquad \frac{B}{A \vee B}$$

$$\begin{array}{c} \\ \\ \\ \underline{A \vee B} \end{array} \begin{array}{c} \Delta\,,\ \overset{\square\!-\!(i)}{A} \\ \vdots \\ \underline{C} \end{array} \begin{array}{c} \Delta\,,\ \overset{\square\!-\!(i)}{B} \\ \vdots \\ \underline{C}\,{}_{(i)} \end{array}$$
$$C$$

where neither A nor B remain undischarged

$\rightarrow$

$$\begin{array}{c} \overset{\square\!-\!(i)}{A} \\ \vdots \\ \dfrac{B}{A \rightarrow B}\,(i) \end{array}$$

$$\begin{array}{c} \\ \\ \underline{A \rightarrow B \quad A} \end{array} \begin{array}{c} \overset{\square\!-\!(i)}{B} \\ \vdots \\ \underline{C}\,{}_{(i)} \end{array}$$
$$C$$

where B does not remain undischarged

DISTRIBUTIVITY

$$\frac{A \wedge (B \vee C)}{(A \wedge B) \vee C}$$

CLASSICAL *REDUCTIO AD ABSURDUM*

$$\begin{array}{c} \overset{\square\!-\!(i)}{\neg A} \\ \vdots \\ \dfrac{\perp}{A}\,(i) \end{array}$$

We stress again that major premises for eliminations must stand proud, which ensures that no conclusion of classical *reductio ad absurdum* can be a major premise for an elimination.

We have indicated, in $\wedge$-Introduction and $\vee$-Elimination, that these rules can be applied only when the *same* set Δ of undischarged assumptions is employed in the two places indicated. Hence the need for the independent postulation of Distributivity—for the usual proof of Distributivity would involve *distinct* sets of undischarged assumptions when applying the usual versions of $\wedge$-Introduction and $\vee$-Elimination.

We are also requiring that the discharge operations indicated with boxes are to discharge at least one occurrence, and indeed all occurrences, of the assumption of the indicated form. For $\wedge$-Elimination, we require use either of the assumption A or of the assumption B, and permit use of both. All such assumption-occurrences, however, must be discharged by the application of the rule. Thus the system above can be described as having the *universal discharge requirement*. It does not allow 'partial discharge', that is, discharge of only some, but not necessarily of all, of the available occurrences of an assumption. In this regard our system differs from that of Prawitz [13].

Major premises of eliminations have to stand proud, with no proof-work above them. This ensures that every proof is in normal form.

Definition.
(i) If there is a proof of φ whose undischarged assumptions form the set Δ, then $\Delta \Rightarrow \varphi$.
(ii) If $\Delta \Rightarrow \varphi \in \Gamma$ and $\Gamma \Rightarrow \psi$, then $\Delta \cup \{\Gamma \setminus \{\varphi\}\} \Rightarrow \psi$.
(iii) $\Delta \Rightarrow \varphi$ only if this can be shown by means of (i) and (ii).

Our system of proof requires proofs to be in normal form. So we can speak of 'normal-form deducibility' in this system. Intuitively, $\Rightarrow$ is just the 'deductive closure' of the (possibly more limited) relation of normal-form deducibility. (We could achieve $\Rightarrow$ directly as the deducibility relation if we allowed partial discharge with the rules of $\neg$-Intro and $\rightarrow$-Intro. This is essentially what Prawitz does.) We can talk of $\Rightarrow$ as representing 'extended deducibility' in our ND-system.

Anderson and Belnap [1], pp. 340–1, provide two slightly different sets of axioms for R, for the generation of Hilbert proofs using only the rules
$(\wedge I)$ From A, B infer $A \wedge B$; and
$(\rightarrow E)$ From A, $A \rightarrow B$ infer B.
Every line in such a proof is a theorem; whence the rules $(\wedge I)$ and $(\rightarrow E)$ apply only to theorems. Clearly, $(\wedge I)$ is derivable in our ND-system for R, since we can take $\Delta = \emptyset$. Likewise, we can derive $(\rightarrow E)$; whence its applications in Hilbert proofs can be mimicked by extended deducibility in our ND-system. Finally, one can show that every one of the Anderson-Belnap axioms (*loc. cit.*) has a *normal* proof in our ND-system (with *universal* discharge!). Hence, every theorem of R is extendedly deducible in our ND-system.

Conversely, one can show that every extended deducibility in our ND-system for R can be captured as a theorem of the Hilbert-system for R in the following sense:

(i) if $\varphi_1, \ldots, \varphi_n \Rightarrow \psi$, then there is a Hilbert-proof of

$$\varphi_1 \to (\varphi_2 \to \ldots (\varphi_n \to \psi) \ldots)$$

(ii) if $\varphi_1, \ldots, \varphi_n \Rightarrow \bot$, then there is a Hilbert-proof of

$$\varphi_1 \to (\varphi_2 \to \ldots (\varphi_{n+1} \to \neg\varphi_n) \ldots)$$

Because of its restrictions on applications of rules, theoremhood in propositional R is undecidable (Urquhart [27]). The fragment obtained by dropping Distributivity is decidable, but the decision problem is at least ESPACE-hard.

11.1 Discussion

The practitioner of a system such as R might acquire the impression that it will be 'business as usual' on the deductive front. One will be able to prove lemmas from one's axioms, and then prove theorems from one's lemmas (plus perhaps other axioms), and put one's proofs together so as to get the theorems directly from the axioms used. The usual strategies of 'breaking down' a logical problem into a sequence of more manageable steps will therefore apply. One just has to find the right lemmas—the right interpolants—and the reasoning within one's favorite mathematical theory will surely be relevantizable.

Alas, this is mistaken. The effects of relevantizing by 'going intensional' on the connectives (and quantifiers) are rather disastrous. They leave the relevantized theory in a relatively much less complete state than the unrelevantized one. One loses not just proofs embodying fallacies of relevance, but also, in many cases, the very results established by such proofs—even when, from our point of view, those results do admit of proofs free of any fallacies of relevance. In the systems that place these intensionalist demands on relevance, it will often happen that familiar proofs in standard logic cannot be reconstructed so as to obey the relevantist strictures. Just as the intuitionist loses certain lovely classical proofs, and with them even the theorems that they establish,[7] so too the relevantist in the Anderson–Belnap tradition loses proofs, and, with them, theorems. Yet no mathematician proving such 'lost'

[7] Example: Every polynomial of several variables and integer coefficients assumes a least absolute value. The classical proof exploits the least-number principle. But any constructive proof would yield an algorithm for determining that least absolute value, thus violating the negative solution to Hilbert's tenth problem. (There is no algorithm for determining whether a given polynomial expression has a root.) See Friedman [8].

theorems in the original system would brook the accusation that she has committed some fallacy of relevance that, once recognized, deprives her of her entitlement to assert the theorem. Such a suggestion from the reforming relevantist logician would strike her as quite outrageous. Moreover, the relevantist (on the approach to relevantization which is being recommended here) can agree. The relevantist's aim should be so to regiment the mathematician's reasoning that its result is shown to be valid even by relevantist lights. Relevantizing is a matter of regimenting reasoning without epistemic loss—indeed, at times with epistemic gain.

Note that this is unlike the situation that is involved when disagreement arises between a classical mathematician and an intuitionist, over the validity of a piece of classical mathematical reasoning. The classicist will be convinced by a particular proof that employs strictly classical forms of reasoning (such as excluded middle), while the intuitionist (in the absence of any intuitionistic proof of the same result) will refuse to assert the 'result' that the proof (according to the classicist) establishes. Here a genuine doctrinal or epistemic difference arises over the issue of constructivity, which cannot be resolved. It is doubtful, however, that an analogous disagreement on the issue of *relevance*—between a mathematician using standard logic (embodying fallacies of relevance) and a mathematician committed to using only a relevant logic—will ever result in the former being prepared to make an assertion based on proof, and the latter being unprepared to endorse the same. This is because (or so I contend) the following explanatory conjecture is true:

> All intuitively convincing mathematical reasoning can be regimented without loss in the kind of relevance logic advocated here.

Thus what may look like a use of a fallacy of relevance in the course of a piece of mathematical reasoning is, upon closer analysis, no such thing. Intuitively, relevance is already 'built into' our deductive sensibilities, in a way that a predilection for constructivity, for example, is not. If our conjecture is true, it follows that the standard systems of logic, which embody fallacies of relevance, are guilty of 'over-provision'. Fallacies of relevance are never of any help in mathematical reasoning, and ought to be expunged from the systems of logic that are designed to regiment such reasoning. It is an interesting question why (from Frege onwards) our systematizations of deductive reasoning have allowed fallacies of relevance to count as formally correct and/or as semantically valid. I believe that it was because a certain kind of reflective equilibrium was struck, in which an overly succinct set of constraints was imposed on the relations of deducibility and consequence, at just that juncture when the object-language conditional was being teased apart from the metalinguistic turnstile. Within this reflective equilibrium, the price paid for unrestricted transitivity and time-saving accumulation of proofs was the admission of the fallacies of relevance. The moral of my story here is that this reflective equilibrium can be re-fashioned, and relevance regained. By being a little more attentive to what is really going on (and what we want to have going on) when we link proofs together, we can find a better way to characterize the structure of the turnstile relation uncolored by considerations of efficiency. We can fashion a system of proof that allows even more faithful regimentations of actual patterns of mathematical reasoning, even though mathematicians make profligate use of Cuts.

The deep reason why the Anderson–Belnap relevantist loses proofs, and, with them, theorems, is that their systems do not contain *disjunctive syllogism*:

$$A \vee B, \neg A : B.$$

This impedes their logical investigation of the various cases yielded by such disjunctive principles as the axiom of trichotomy:

$$x < y \vee y < x \vee x = y.$$

Such principles are indispensable in our axiomatizations of various branches of mathematics. The mathematician needs, like Chrysippus's dog, to be able to chase the truth down a different fork in the road as soon as it is discovered that a chosen fork is a dead-end. Without disjunctive syllogism, one cannot in general do that.

By contrast with the Anderson–Belnap 'intensional-connective' systems, the systems *IR* and *CR*, which I advocate as the respective *properly relevant* versions of intuitionistic and classical logic, preserve the whole stock of mathematical theorems—and indeed in each case from the very same axioms—in the respective kinds of 'non-relevant' mathematics. Non-relevant classical mathematics is the mathematics than can be derived from one's axioms by means of the full resources of non-relevant classical logic. That means that one can use the rule that allows one to infer any statement at all from a contradiction; one can use disjunctive syllogism; one can use the so-called 'paradoxes of implication'; etc. If a classical mathematician produces a 'non-relevant' proof of, say, Fermat's last theorem in some interesting arithmetical theory such as exponential function arithmetic (*EFA*), then my *relevantizing* classical mathematician will be able to prove Fermat's last theorem *relevantly*, using only axioms of *EFA* that had been used in the non-relevant proof. (*Mutatis mutandis* with 'intuitionistic' in place of 'classical'.)

12 The maxim of narrow analysis

Relevantizing brings benefits for automated deduction. In automated deduction, the system is given a queried sequent $X?\text{-}Y$, and seeks to construct a normal or cut-free proof of that sequent, without human intervention. The deductive problem is broken down into more manageable sub-problems by foreshadowed applications of logical rules; one does not seek, in automated deduction, to program the system to find interpolants. That is something that only human reasoners do.

We noted earlier that every perfectly valid sequent in classical (resp. intuitionistic) logic has a classical (resp. intuitionistic) *relevant* proof. This observation prompts the following maxim:

> Maxim of Narrow Analysis
> When given a deductive problem $X?\text{-}Y$ that admits of proof, always seek a proof of a minimal sub-sequent of $X:Y$ that admits of proof.[8]

Suppose one has a proof Π of the sequent $X : Y$. Suppose that one then discovers a new proof Π^* of some proper sub-sequent $X^*:Y^*$. Then one should be able to substitute Π^* for the old proof Π of the original sequent $X : Y$, without ever forfeiting (the results of) any proofs involving Π^* as a subproof. Making a subproof prove a tighter logical result should never result in having to forfeit larger proofs containing that subproof. This sounds obvious, and one feels it ought to be true.

In some systems of logic, however, one can be thwarted when trying to follow the maxim of narrow analysis as vigorously as one would like. In these systems of logic, if one substitutes in all one's proofs Σ the better subproof Π^* (of the stronger result) for the worse subproof Π (of the weaker result), then one does not in general preserve proofhood. Nor is one in general able to recover or restore the situation, by producing new proofs in the light of that substitution.

Some relevant systems (such as R) can be unreasonably finickity about the use one may make of assumptions for the sake of argument, especially with a rule like the rule of conditional proof. In a new would-be subordinate proof Π^* of a stronger result, some of those assumptions may be eschewed, or the subordinate conclusion may be of the wrong form ($\perp$ instead of the 'sought' conclusion B). When such Π^* is substituted to replace the subproof Π (of the weaker result) within some larger proof Σ, the result $\Sigma(\Pi/\Pi^*)$

[8] The proof-finding algorithms in *Autologic* are designed with this in mind.

may fail to be a proof, because the 'lost assumptions' or the changed form of the subordinate conclusion make some rule-application in the wider proof-context Σ illicit. And there might not be any way, in general, of capitalizing on the epistemic gain represented by the new subproof Π^*, and distributing its potential returns within the wider proof Σ.

13 Non-forfeiture of epistemic gain

This complaint cannot be levelled, however, against *IR* and *CR*. These systems, unlike other systems of relevance logic, are devoted to the non-forfeiture and potentially wider distribution of epistemic gains. *IR*, for example, satisfies the following principle:

> Principle of Non-Forfeiture of Epistemic Gain
> Let Π be a proof of the sequent $X : A$, occurring as a subproof of the proof Σ of the sequent $Y : B$. Let Π^* be a proof of some proper sub-sequent of $X : A$. Then $\Sigma(\Pi/\Pi^*)$—the result of substituting Π^* for Π in Σ—can be effectively transformed into a proof of some sub-sequent of $Y : B$. Indeed, the transformation can be effected in polynomial time.

Note that I am not saying that a strict gain via Π^* with respect to some subproof Π will always turn into a strict gain with respect to the overall proof Σ. In many cases it will; but in some cases the result established by (the transform of) $\Sigma(\Pi/\Pi^*)$ will be the original result $Y : B$.

Still, non-forfeiture of epistemic gain would be very good news. For we would never lose any of our erstwhile deductive knowledge by forgetting Π and remembering Π^* instead. And this cannot be said for many a rival system of relevance logic.

14 Summary

We have tried to show how to relevantize the two main logics—intuitionist and classical—without methodological loss. In the sequent setting, we ban dilutions and cuts. And we state the logical rules in a slightly more relaxed form, so that we can get by without the structural rules of Dilution or Cut. In the natural-deduction setting, we ban the absurdity rule and abnormalities, and keep exigent but well-motivated checks on how we have used assumptions for discharge. We also liberalize slightly certain logical rules, so that the loss of the absurdity rule is not crippling.

Elsewhere, I have shown how this process of relevantizing (in the intuitionistic case) brings the system of natural deduction and the corresponding system of sequent calculus into 'deep isomorphism'.[9] The relevant systems are also ideally suited for computational logic, since they allow one never to forfeit any epistemic gains that one might achieve in the course of solving deductive sub-problems. This approach to relevantizing salvages those 'local' forms of reasoning, such as disjunctive syllogism, that seem, intuitively, so indispensable for ordinary mathematical reasoning. It locates the issue of relevance at the level of the turnstile, and challenges certain orthodoxies as to what structural features of the deducibility relation it is methodologically necessary to preserve. The upshot is that the student of logic who is dismayed by the First Lewis Paradox can be assured that it is irrelevant to the deductive needs served by one's logic, whether one is an intuitionist or a classicist. We can make do without it, and stand only to gain by eschewing it. Logical rules suffice for our logical reasoning. We do not even need any 'structural' rules—except, of course, the rule of initial sequents $A : A$. That, however, is the case only in a sequent system. In natural deduction, the counterpart to this is that one is allowed to write down an assumption A for the sake of argument. It might be left standing as an undischarged assumption by the end of the proof, or it might be discharged on the way to the conclusion. Without such allowance, no proof would ever get started. So I don't propose to revoke *that* licence. I have banned a great deal; but then again, with no untoward effect at all. Our canon of deductive reasoning has been relevantized; but our powers of reasoning have been left intact.

Perhaps we should call the resulting position 'compassionate relevantism'.

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