Notre Dame Journal of Formal Logic
Volume 22, Number 2, April 1981

Relevance: A Fallacy?

JOHN P. BURGESS

Introduction Responding to Harvey's theories about the circulation of the blood, Dr. Diafoirus argues (a) that no such theory was taught by Galen, and (b) that Harvey is not licensed to practice medicine in Paris. Plainly there is something wrong with a response of this sort, however effective it may prove to be in swaying an audience. For either or both of the allegations (a) and (b) might well be true without Harvey's theory being false. So Diafoirus' argument can serve only to divert discussion from the real question to irrelevant side-issues. The traditional term for such diversionary debating tactics is "fallacy of relevance".

In recent years this traditional term has come to be used in a quite untraditional sense among the followers of N. D. Belnap, Jr., and the late A. R. Anderson. (All citations of these authors are from their masterwork [1], and are identified by page number.) According to these self-styled "relevant logicians", it is items (IA) and (IIA) in the accompanying table that constitute the archetypal "fallacies of relevance".

Table

(I)	$\begin{array}{l} p \text{ or } q \\ \underline{\text{not } p} \\ q \end{array}$	(II)	$\begin{array}{l} \text{not both } p \text{ and } q \\ \underline{p \qquad\qquad\qquad} \\ \text{not } q \end{array}$
(IA)	$\begin{array}{l} p \vee q \\ \underline{\sim p} \\ q \end{array}$	(IIA)	$\begin{array}{l} \sim(p \,\&\, q) \\ \underline{p \qquad\quad} \\ \sim q \end{array}$
(IB)	$\begin{array}{l} p + q \\ \underline{\sim p} \\ q \end{array}$	(IIB)	$\begin{array}{l} \sim(p \circ q) \\ \underline{p \qquad\quad} \\ \sim q \end{array}$

Received May 28, 1980; revised July 23, 1980

(In the table $\sim$, &, and $\vee$ stand for truth-functional negation, conjunction, and disjunction, respectively.) These forms of argument, say Anderson and Belnap, are "simple inferential mistake[s], such as only a dog would make" (p. 165). The authors can hardly find terms harsh enough for those who accept these schemata; they are called "perverse" (p. 5) and "psychotic" (p. 417).

Needless to say, (IA) and (IIA), which can be traced back at least to Chrysippus, were not traditionally regarded as fallacious. The Anderson-Belnap notion of "relevance", whatever it may amount to, must be something quite different from the traditional notion, which "was central to logic from the time of Aristotle" (p. xxi). And yet the authors declare their so-called "relevant logic" to be a commonsense philosophy, in accord with the intuitions of "naive freshmen" (p. 13) and others who have not been "numbed" (p. 166) by a course in classical logic. Moreover, whereas other dissident logicians (e.g., intuitionists) hold that some forms of argument always accepted and used without question by mathematicians in their proofs are in fact untrustworthy, Anderson and Belnap are at some pains to explain (pp. 17-18 and 261-262) that *their* brand of nonclassical logic does *not* conflict with the practice of mathematicians, but only with the classical logician's *account of* that practice.

In view of the fact that everyday arguments and mathematical proofs abound in instances of (I) and (II), one may wonder how Anderson and Belnap could hope to reconcile their rejection of (IA) and (IIA) with the claim that their "relevant logic" is compatible with commonsense and accepted mathematical practice. The answer is that the authors believe that the ordinary-language argument patterns (I) and (II) should be represented as expressions of the "intensional" schemata (IB) and (IIB), which are relevantistically acceptable, and not of the "extensional" schemata (IA) and (IIA), which the relevantists reject.

The compound $p + q$ appearing in (IB) is supposed to be an "intensional disjunction" stronger than the truth-functional $p \vee q$ in that mutual relevance of p and q is required for its truth. This $p + q$ is not entailed by p, nor even by p & q, since it might be false even though p and q were both true (or even necessary). This happens in the case of irrelevant pairs such as p = "Bach wrote the Coffee Cantata" and q = "The Van Allen belt is doughnut-shaped" (p. 30). Dually, the $p \circ q$ of (IIB) is an "intensional conjunction", better called "cotenability" or "nonpreclusion", a compound weaker than the truth-functional p & q in that mutual irrelevance of p and q is sufficient for its truth. This $p \circ q$ does not entail p, nor even $p \vee q$, since it might be true even though p and q were both false (or even impossible).

The relevantists' claim that (IB) and (IIB) best represent (I) and (II) admits of two formulations: a stronger and a weaker. The stronger claim would be that the ordinary-language "or" and "and" literally *mean* + and $\circ$ rather than $\vee$ and &. The weaker claim would be that anyone *basing an argument* on the premise that p or q, or that not both p and q, will at least *be in a position to assert* that $p + q$, or $\sim(p \circ q)$ as the case may be. (The latter claim is weaker than the former because even if "or" and "and" *meant* $\vee$ and &, it might still be that arguments of form (IA) and (IIA) could always be avoided in practice because in any instance where one *might wish to argue* from $p \vee q$ or from $\sim(p \,\&\, q)$ the stronger premises $p + q$ and $\sim(p \circ q)$ would *be available.*)

The stronger of these two relevantistic claims seems quite untenable. True, Anderson and Belnap do make a feeble attempt (pp. 176-177) to argue that the ordinary-langauge "or" usually means + rather than v. Their argument, however, is scarcely original, amounting to no more than a repetition of the arguments that used to be used by P. F. Strawson and other Oxford philosophers in their diatribes against modern formal logic. To the serious objections against such Oxonian arguments that have emerged from H. P. Grice's work on conversational implicature the authors of [1] attempt no reply. In any case, even if the claim that "or" means + is credited with a certain intuitive plausibility, the same cannot be done for the claim that "and" means o. To be sure, Strawson and others have claimed that the meaning of "and" sometimes diverges from that of & (that in some uses "and" does duty for "and subsequently" or "and as a result"); but these divergences are always in the direction of something *stronger* than &, not something weaker. The relevantists themselves shrink from identifying o with "and". While asserting (pp. 344-345) that cotenability is an "analogue" that "in some ways . . . looks like" conjunction, they concede that "it isn't conjunction".

The untenability of the strong claim that everyday and mathematical instances of (I) and (II) are literally *meant as* instances of (IB) and (IIB) might already be thought to do considerable damage to the relevantists' claim to be espousing a commonsense philosophy of logic. My aim in this paper is to cast further doubt on that claim by presenting counterexamples to the weaker claim that everyday and mathematical instances of (I) and (II) can at least be *avoided in favor of* (IB) and (IIB). I will present some examples, taken from everyday life and mathematical practice, of arguments of the forms (I) and (II) which can be neither *read as* nor *replaced by* instances of (IB) and (IIB).

Examples

Background to Example 1: The game of Mystery Cards is played thus: The red and black cards from an ordinary deck are separated. One red and one black "mystery card" are set aside face down, without having been seen by any player. The remaining 25 red and 25 black cards are combined, shuffled, and dealt out to the players, whose object is to guess the mystery cards. The players take turns questioning each other. The player whose turn it is addresses the player of his choice asking a question of the form, "Is it the such-and-such red card and the thus-and-so black card?" If the player questioned has either or both of the cards named in his hand, he must answer "No"; otherwise he must answer "Maybe". Both question and answer are audible to all players. If a player feels ready to guess the mystery cards, then on his next turn, instead of asking a question he may make a statement, saying "It's the such-and-such red card and the thus-and-so black card!" He then looks at the mystery cards. If his guess is correct, he turns them face up and is declared winner. If wrong, he puts them back face down, exposes his own hand, and is disqualified from further play.

Admittedly this game is a dull one, but it exhibits in simplified form the principle at work in several more interesting games (e.g., the one marketed under the trade-name CLUE, the importance of which was pointed out to me by D. K. Lewis).

Example 1, Argument: During the course of a game of Mystery Cards, Wyberg hears von Eckes ask Zeemann, "Is it the deuce of hearts and the queen of clubs?" He hears Zeemann reply "No". Later in the game he manages to figure out that it *is* the deuce of hearts. He argues: It isn't both the deuce of hearts and the queen of clubs; but it is the deuce of hearts; so it isn't the queen of clubs. He goes on to use this information to win the game.

Example 1, Analysis: Let p = "The mystery red card is the deuce of hearts", q = "The mystery black card is the queen of clubs". Zeemann's hint is no more and no less than that $\sim(p \ \& \ q)$. Her statement is made on purely truth-functional grounds: She sees the queen in her hand. Her statement is not made on the basis of any "relevance" between p and q: The two mystery cards were chosen entirely independently of each other. Zeemann is justified in denying a truth-functional conjunction, but would not be justified in denying cotenability. Since the premise $\sim(p \circ q)$ is not available to Wyberg, his argument is an instance of (II) that can be neither read as nor replaced by an instance of (IIB).

Had Wyberg been a relevantist, unwilling to make a deductive step not licensed by the Anderson-Belnap systems E and R, he would have been unable to eliminate the queen of clubs from his calculations, and would have lost the game. A relevantist would fare badly in this game and others, and in game-like situations in social life, diplomacy, and other areas–unless, of course, he betrayed in practice the relevantistic principles he espouses in theory.

Background to Example 2: Dr. Zeemann has just been awarded her degree for a dissertation in number theory. Her main result is a proof that every natural number n either has a certain property $A(n)$ or else has a certain other property $B(n)$. As written up in her thesis, the proof is by induction on n, as follows:

> Case $n = 0$: We show that $A(0)$. [Here follows a proof.]
> Case $n = 1$: We show that $B(1)$. [Here follows a proof.]
> Case $n \geqslant 2$: We assume as induction hypothesis that either $A(n-1)$ and $A(n-2)$, or $A(n-1)$ and $B(n-2)$, or $B(n-1)$ and $A(n-2)$, or else $B(n-1)$ and $B(n-2)$. [Here follows a proof treating each of the four cases separately.]

She remarks that the famous d'Aubel-Hughes Conjecture would imply that $B(0)$, whereas the equally famous conjecture of MacVee would imply that $A(1)$, but reports that she has no light to shed on these old conjectures.

Commentary: Before proceeding, let us note that, following the universal practice of mathematicians, Zeemann has taken her proof that $A(0)$ to dispose of the case $n = 0$ of the general theorem that for all n, either $A(n)$ or $B(n)$. In other words, she argues from the premise $A(0)$ to the conclusion that $A(0)$ or $B(0)$. This is worth mentioning because relevantistically inclined writers have been known to claim that no one ever seriously argues from p to p or q. Indeed, in everyday conversation we are, in R. C. Jeffrey's words, "at a loss to know what the motive could be" for someone to pass from p to the longer and less informative statement that p or q. "Knowing the premise, why not assert *it*, rather than the conclusion?" However, in mathematics we often have good reason to say less than we know: We will assert less than we could about the cases $n = 0$ and $n = 1$ in order to incorporate these cases in a generalization holding for all values of n. Now the inference from $A(0)$ to $A(0)$ or $B(0)$ is only valid if "or" is taken as $\vee$ rather than $+$. Hence Zeemann's theorem must be formalized as $(n)(A(n) \vee B(n))$, not $(n)(A(n) + B(n))$. This means that any argument of form (I) in which the major premise is supplied by Zeemann's Theorem will be an instance of (I) that can be neither read as nor replaced by an instance of (IB). Let us proceed to examples.

Example 2a, Argument: Zeemann applies her work to give bounds to the number of solutions to Tiegh's Equation, thus:

> Tiegh himself has shown that the number t of solutions to his equation is $\leqslant 13$. Now a little elementary algebra shows that we cannot have $A(t)$. Hence by our main result, we must have $B(t)$. But no n with $5 \leqslant n \leqslant 16$ can satisfy $B(n)$, as is clear from some more elementary algebra. Hence $t \leqslant 4$.

Example 2a, Analysis: This is a typical mathematical argument of form (I). The premise $A(t)$ or $B(t)$ must be represented as a truth-functional, not an "intensional", disjunction. The "unknown" t might for all we know be equal to 1, and no "relevant" connection has been established between $A(1)$ and $B(1)$—indeed, as Zeemann herself reports, she has been unable to establish anything about $B(1)$.

Example 2b, Argument: Professor Wyberg has been working for years on the celebrated conjecture of von Eckes, but has got no further than showing that the conjecture follows from the assumption that $B(1)$, a result he considers not worth publishing. Just recently he has given up work on von Eckes' Conjecture in disgust, and has turned to other matters. In particular, he has just refuted an old conjecture of MacVee by proving that $\sim A(1)$. Now he reads an announcement of Zeemann's result. The details of her proof are not available—it takes *years* for theses to come out in print—but he recognizes the significance of her results. In particular, they enable him to prove von Eckes' Conjecture at last. He writes a set of notes, "A Proof of von Eckes' Conjecture", with the following structure: First comes his proof that $\sim A(1)$. Second comes a linking passage:

> And so we see that the MacVee Conjecture fails. Now Zeemann has recently announced the result that for all n, either $A(n)$ or $B(n)$. Hence we must have $B(1)$. We now proceed to put this fact to good use.

Third follows the derivation of von Eckes' Conjecture from $B(1)$.

Example 2b, Analysis: Since what is established by Zeemann is just $A(1) \vee B(1)$, not $A(1) + B(1)$, we have here another mathematical instance of (I) that can be neither read as nor replaced by an instance of (IB). It is a slightly atypical instance. Had he known the details of Zeemann's work, had he known that she actually proves $B(1)$ outright, Wyberg would surely have just cited the fact that $B(1)$ from her thesis, rather than give the roundabout argument that he did. But this is not to say that the proof of von Eckes' Conjecture that

Wyberg did give is *erroneous.* One must distinguish inelegance from incorrectness, as even the relevantists allow (p. 279). To sharpen the intuition here, suppose that six months after Wyberg, von Eckes himself notices that his conjecture can be derived from $B(1)$. Suppose further that von Eckes, unlike Wyberg, has access to a photocopy of Zeemann's thesis, and so knows that she has proved $B(1)$. Von Eckes then writes a paper, "Proof of a Conjecture in Number Theory", in which he cites the fact that $B(1)$ from her thesis and then proceeds to derive his old conjecture from $B(1)$ in a manner indistinguishable from that of Wyberg. In this situation, nobody in his right mind would say that von Eckes had produced "the first correct proof" of the conjecture; the honor of priority goes to Wyberg.

One afflicted with relevantistic scruples could not have argued as Wyberg did, but would have had to wait for the publication of Zeemann's work before claiming to have settled von Eckes' Conjecture. By that time the less scrupulous Wyberg and the better-placed von Eckes would already be contending for priority. A follower of Anderson and Belnap would not prosper in the world of contemporary mathematics–unless, that is, he sometimes conveniently "forgot" his philosophy of logic.

Conclusion No doubt the reader can construct further examples. One might consider, for instance, the case of a person who remembers that once upon a time he was told either that p or that q, but can't now remember which. Investigating a bit, he quickly establishes that $\sim p$, and so concludes that q. Such examples, I submit, show that, as far as negation, conjunction, and disjunction are concerned, "classical" logic (and with it the whole logical tradition from Chrysippus onwards) is far closer to commonsense and accepted mathematical practice than is the "relevant" logic of Anderson and Belnap.

One ploy the relevantist might use in trying to escape from our counterexamples may already have occurred to the reader. What if we take the "relevance" required for the truth of $p + q$ and the falsehood of $p \circ q$ not as something objective and absolute, but as something subjective and relative. We might then say this of the Mystery Cards, for example: In objective fact there is no connection between its being the deuce of hearts and its being the queen of clubs, the red and black cards having been chosen separately. In Zeemann's mind there is no such connection, her statement that it's not both being based solely on her knowledge that it's not the latter. But Zeemann's information establishes such a connection *for Wyberg,* so that he is in a position to assert what she was not, namely $p + q$. Hence his argument can be represented as a case of (IIB).

I doubt such a subjectivization and relativization of "relevance" offers a viable way out to followers of Anderson and Belnap. If (IB) and (IIB) are to cover *all* the instances of (I) and (II) in mathematics and everyday argumentation, "relevance" will have to be not just subjectivized but trivialized. *Any* grounds for asserting $p \vee q$ short of the simple knowledge that p, or that q, will have to be taken as sufficient grounds for asserting $p + q$: the statement by a reliable person that she either knows p or knows q though she isn't saying which; the knowledge that p holds for $m = 0$ and q for $m = 1$, coupled with ignorance as to whether $m = 0$ or 1; the simple recollection that one *once* knew

p or knew q though one has now forgotten which. (And paradoxically, the acquisition of *more* information could threaten one's right to assert $p + q$: if one's informant decides to provide more specific information, if the value of m is settled, if one's memory improves, one may suddenly lose the right to assert $p + q$.) Relevantism would reduce to the position that (IA) is valid when and only when one's grounds for asserting $p \vee q$ are something other than the simple knowledge that q. Such a position, however, looks suspiciously like a confusion of the criteria for the *validity* of a form of argument with the criteria for its *utility*, a confusion of logic with epistemology.

Indeed, some writers have been willing to dismiss the whole relevantistic movement as a simple case of confusion between the logical notion of *implication* and the methodological notion of *inference*. The following (unpublished) remarks of G. Harman on this point will bear quoting:

> By reasoning or inference I mean a process by which one changes one's views, adding some things and subtracting others. There is another use of the term 'inference' to refer to what I will call 'argument', consisting in premises, intermediate steps, and a conclusion. It is sometimes said that each step of an argument should follow from the premises or prior steps in accordance with a 'rule of inference'. I prefer to say 'rule of implication', since the relevant rules do not say how one may modify one's views in various contexts. Nor is there a very direct connection between rules of logical implication and principles of inference. We cannot say, for example, that one may infer anything one sees to be logically implied by one's prior beliefs. Clearly one should not clutter up one's mind with many of the obvious consequences of things one believes.
>
> Furthermore, it may happen that one discovers that one's beliefs are logically inconsistent and therefore logically imply everything. Obviously, one ought not to respond to such a discovery by believing as much as one can. Some philosophers and logicians [the reference is to Anderson and Belnap] have imagined that the remedy here is a new logic in which logical contradictions do not logically imply everything. But this is to miss the point that logic is not directly a theory of reasoning at all.

And indeed *if* "relevance" is taken to be something subjective and relative (according to the proposal discussed above), I do not see how the relevantists could escape Harman's charge that they confuse implication and (useful) inference.

I do not, however, believe that the authors of [1] understand by "relevance" something subjective. What little they tell us about the nature of "relevance" (e.g., pp. 32-33, where they quote with approval from several sources) strongly suggests that it is a matter of *meaning*. Certainly their commonest charge against classical logic (first raised on p. xxii and repeated *ad nauseum*) is that it ignores "intension" and meaning. Meaning, however, is something that, generally speaking, will be the same for Wyberg as it is for Zeemann. That relevance is meant to be a semantical, and hence impersonal, notion, and not a matter of individual psychology, is further suggested by the relevantists' criticisms of T. J. Smiley (p. 217), who is faulted for "epistemologizing" and "psychologizing" the logical notion of entailment. Thus if the authors of [1] intend by "relevance" something less than objective, they are highly remiss in failing to alert their readers to the fact; while if "relevance" is supposed to be impersonal, then the claim that the relevantistic position is (even in a weak sense) compatible with commonsense and accepted mathematical practice succumbs to the counterexamples presented above.

In closing, let me reiterate that 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. I have not been concerned with other rationales for developing these systems, nor with the possibility of imposing interpretations on them that were not originally intended by their authors. (It has been suggested, for instance, that some of the formalisms created by the relevantists might be useful in developing a logic of ambiguity, or of truth-in-fiction.) Workers in category theory, one of the least constructive branches of modern mathematics, have found certain technical uses for intuitionistic logic; but no one imagines that this vindicates Brouwer's philosophy of mathematics. Similarly, the discovery of serendipitous applications of some of the formalisms created by Anderson and Belnap would not justify the claim that their logical systems are accurate formalizations of current mathematical practice. Still less could it justify the abusive tone of their remarks about classical logicians.

REFERENCE

[1] Anderson, A. R. and N. D. Belnap, Jr., *Entailment: The Logic of Relevance and Necessity*, Princeton University Press, Princeton, New Jersey, 1975.

Department of Philosophy
Princeton University
Princeton, New Jersey 08544