Conversation and Conditionals Again: Lewis, Jackson and Appiah on Pragmatic Defenses of the Equivalence Thesis.

by

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ABSTRACT

CONVERSATION AND CONDITIONALS AGAIN:
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Call the claim that the truth-conditions of the ordinary indicative conditional are just those of the material conditional the Equivalence thesis (ET). I argue (1) that ET is appropriately treated as a theoretical claim which must meet the demands of observation and logic in company with other associated claims; (2) that ET thus viewed is a plausible thesis in the context of Lewis' and Jackson's recent pragmatic accounts of the assertability of indicative conditionals; and (3) that Appiah's criticisms of these accounts fall short of supporting his conclusion that there is no reasonable alternative to a non-truth-conditional semantics of assertability. A Gricean generalization of Jackson's pragmatic account is briefly sketched.

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Introduction

This paper is in the service of two theses. One is a reasonably uncontroversial methodological principle, the other is a position rather more beleaguered these days. I shall be arguing, against recent comment, that there are respectable reasons for continuing to say that the truth conditions of the ordinary indicative conditional are just those of the material conditional. This is a claim which Frank Jackson has dubbed the Equivalence thesis.[1] It is the embattled position; in what follows I shall, for convenience, be calling it ET. Its defense will involve presentation of evidence in its favor, consideration of problems involved in its endorsement, and the invocation of some generally accepted criteria for theory choice which, I shall argue, count strongly in its favor. Less controversially, I shall be noting that language has many different functions and that it is helpful to keep this in mind when we try to find order among our conflicting linguistic intuitions.

A preliminary glance at the over-all shape of this undertaking may be helpful. I shall, in what follows, be giving critical attention to one particular attack on ET--Anthony Appiah's criticisms of recent attempts by David Lewis and Frank Jackson to defend ET by invoking rules presumed to govern ordinary discourse. In "Conversation and Conditionals", (1982),[2] and "Lewis on the Material Conditional", (1983),[3] Appiah tells us that both of these ventures are in pretty bad shape. One of them has a failing "fatal for a whole class of . . . [such] approaches;" he is sceptical about the other (Appiah, 1982, p. 327). Appiah concludes from this that we have no choice but to give up on a truth conditional account of the indicative conditional (which, for convenience, I shall be symbolizing hereafter as '$(p \rightarrow q)$'). It is, Appiah tells us,

> just a prejudice that all interesting semantics is to be done in terms of truth conditions. . . . We can explain what people are doing when they utter conditionals by way of the assertability rule [$(p \rightarrow q)$ is assertable when $Pr(q/p)$ is adequately high]. . . . Why do we not rest content with that?

(Appiah, 1982, p. 338)

If Appiah is correct, ET is, of course, untenable. I shall be arguing that Appiah is not correct but suggesting that his criticism is nevertheless useful in that it calls attention to canons of theory choice which are easily lost to view amidst technical discussions of ET and its problems.

Despite appearances, Appiah's conclusion is not based on the claim that there is an attractive semantics of assertability close at hand--which, of course, there is not--but, rather, on what he takes to be the unavailability of any reasonable alternative. I think that Appiah's pessimism is misplaced and that on two quite different scores. First, he seems convinced that it is only by virtue of a pragmatic defense that ET has any respectable claim on our credence. I think this is quite mistaken, that while ET has some well-known counter-intuitive consequences, there are consequences of rejecting the thesis that are at least as hard to take. So the first thing I shall want to be arguing is that the claim that $(p \rightarrow q)$ is true just when $(p \supset q)$ is true has a good deal more going for it, even in isolation, than Appiah seems to acknowledge. That ET is a peculiarly fragile thesis is, I think, an illusion fostered very considerably by a mistaken view of what we have on our hands in the case of claims about the relationship between formal logic and natural language. I shall be urging that despite temptations so to view it, what we have here is not a conflict between unnatural tidy-making proclivities of formal logicians, on the one hand, and untrammelled and incorrigible linguistic intuitions, on the other. Rather, I shall suggest, we confront a tension between two sets of compelling intuitions, beliefs and inferential practices. What is at issue, I shall propose, is the goodness of competing theoretical accounts of these phenomena. I shall be urging that our decision should, in this case, be guided by the same sorts of considerations which we take to be relevant in other cases of theory choice.

Secondly, I should like to suggest that even if we grant that a pragmatic solution to the puzzles raised by endorsement of ET would be welcome, Appiah is surely prematurely pessimistic about our chances of working out such a solution. That there is no very simple theory currently available does not seem to me adequate grounds for concluding

that no satisfactory story is possible. Indeed, given the complex requirements to which our linguistic intuitions must of necessity be responsive, it would be surprising if a simple theory were available. I shall urge that the current status of pragmatic theory should encourage rather than discourage attempts to make use of its resources and that in conjunction with such a theory ET is a persuasive claim.

Doing this will involve a combination of argument and fairly straightforward exposition. The main arguments are presented in the first and last chapters; elsewhere, critical comments are restricted to the numbered endnotes (as are first citations of sources). Derivations appear in the Appendix and are indicated by (Appendix).

In Chapter 1, I shall be concerned with establishing that, despite current unpopularity, ET has considerable plausibility as a theoretical claim and is by no means susceptible of easy dismissal. Chapter 2 reviews some major challenges to ET, including P.F. Strawson's consequentialist account of ordinary indicative conditionals and a variety of more or less well-known counterexamples to ET. Chapter 3 introduces Paul Grice's influential conversationalist defense of ET and some extensions of this strategy. Chapter 4 takes up a more recent challenge--Ernest Adams' proposal for a thoroughgoing revision not only of the semantics of conditionals but of criteria for inference schemes as well. In this chapter we also confront some unobvious but far-reaching consequences of endorsing Adams' program, in the form of Lewis' triviality theorems. Chapter 5 lays out two defenses of ET intended specifically to counter Adams--David Lewis' neo-Gricean quantified conversationalist approach and Frank Jackson's SET, a proposed augmentation of standard conversationalist theory.

In Chapter 6 we are, at last, ready to look in some detail at Appiah's objections to these two proposals. I shall be arguing here that Appiah's criticisms are not persuasive and that they fail to support his conclusion that we should reject ET and endorse a semantics of assertability. I propose, instead, that in company with a pragmatic theory general enough to take into account a variety of functions of discourse, ET deserves very serious consideration.

Chapter 1.
ET as a Theoretical Claim

There is little doubt that the Equivalence thesis is currently considered a difficult position to defend. Mackie has characterized it as a paradoxical view[3]; Hunter finds it a scandal that what he takes to be a central, non-truth-functional sense of 'if' "has not yet been caught in any interpreted formal system with an adequate metatheory."[4] Indeed, belief in the falsity of ET has been described by one writer as something of a philosophical orthodoxy,[5] a view which is supported by the entry under 'If' in the Encyclopedia of Philosophy:

> Many contemporary philosophers. . . use the locutions 'if p, then q' and 'p only if q' in the sense of 'p⊃q' and have therefore established a new use for them. . . . [I]t is important to see that when 'if p, then q' is used in this way, it does not strictly express a conditional at all--or if it does express a conditional, it is a conditional at the vanishing point.[6]

In this chapter I shall, therefore, be largely concerned with clarifying the question of what sort of claim ET is and arguing that, appropriately viewed, it is a thesis which we have good reason to take seriously.

Let me begin by saying that it is rather tempting to see the debate over ET along the lines of some sort of opposition between law and order, on the one hand, and natural unruliness, on the other. Even Strawson, whose account of the relationship between natural language and formal system is full of subtle nuance, occasionally seems to open the door to some such potentially misleading contrast.[7] Formal logic, Strawson tells us, is characterized by clear, explicit, precise rules which we deliberately establish in the service of system and elegance (Strawson, p. 232). Natural language, on the other hand, is full of complexity and ambiguity; we come upon its rules by reflection on our practice and such rules are characterized by fluidity and imprecision in the distinctions they involve (Strawson, p. 231).

Systematic simplicity, he tells us,

> while admirably exemplified by the truth-functional system, is exemplified not at all by a veridical account of the maze of logical uses through which we unhesitatingly thread our way in our daily employment of the customarily related conjunctions (Strawson, p. 93).

Thus,

> The formal logician. . . might be compared with a man ostensibly mapping a piece of country of which the main contours are highly irregular and shifting. . . [P]assionately addicted to geometry. . . he insists on using in his drawings only geometrical figures for which rules of construction can be given; and on using as few of such rules as he can. Naturally, his maps will never quite fit (Strawson, p. 58).

It is, I think, important to match Strawson's remarks here with the targets to which they are germane. Metaphoric contrasts of this sort are, for example, prophylactic against the temptation first to substitute the study of formal languages for that of natural ones (because formal languages reveal their secrets to us more readily) and then to assume that what we find out about our formal system must be true of natural language. There is no doubt that the study of ordinary speech is, as Strawson insists, "more complicated and less tidy than the study of formal systems," and it is equally unarguable that, as the old joke reminds us, we do not have the luxury of searching only where the light is best (Strawson, p. 231). Thus, that the truth-conditions of $(p \supset q)$ are what they are (because of properties of the propositional calculus) provides, in itself, no warrant whatsoever for the claim that the truth-conditions of $(p \supset q)$ are the truth-conditions of $(p \rightarrow q)$. In this regard, Strawson's picture is helpful.

Moreover, Strawson's imagery reminds us that it is unlikely that any formal logic will, in and of itself, provide the theoretical apparatus for explaining everything interesting about competent participants' ordinary language inferences or their judgments of truth, just as it is unlikely that any formal grammar, in and of itself, will provide the theoretical apparatus for predicting everything interesting

about competent speakers' linguistic performances or their judgments of grammaticality. Because the surface phenomena to which we have access (and which may, in fact, be the focus of our interest) are generally the resultant of many different sorts of determinants--limitations of short term memory as well as grammatical rules, information processing biases as well as logical structures--we are not going to observe precisely what the rule set, by itself, might predict. In this sense, neither the logician's nor the linguist's maps will ever quite fit, and it is well to be aware of this.

Insofar as they help us to account for what we observe, however, we may still insist that rules formulated by the logician or the linguist have a legitimate place as theses in our over-all account of inferential practice or linguistic performance. To say that such rules are simpler than the phenomena which we are trying to understand can thus, in itself, be no objection to the rules but is merely a remark about the nature of theory. It can hardly count against physics that the laws of classical mechanics are compellingly tidy whilst my office is a mess. Strawson is, thus, both helpful and potentially misleading, I think, when he talks about "the stylized, mechanical neatness of the logical system"(Strawson, p. 81). We must at least be careful how we read disclaimers like the following.

> It will not do to reproach the logician for his divorce from linguistic realities, any more than it will do to reproach the abstract painter for not being a representational artist; but one may justly reproach him if he <u>claims</u> to be a representational artist (Strawson, p. 81).

While there is a sense in which there is, as Strawson tells us, no way to "give the exact and systematic logic of expressions of everyday speech; for these expressions have no exact and systematic logic," there is also a sense in which exact and systematic rules may quite reasonably be said to be the rules of ordinary inferential discourse--a logical system L is the logic of such discourse just in case L meets demands of theoretical adequacy in helping us construe such phenomena as competent speakers' judgments of truth and inferential validity (Strawson, p. 58).

I shall, therefore, assume in what follows that it can hardly count against ET as a thesis of our over-all theoretical account of competent speakers' inferential practices and logical judgments that the rules of truth-functional logic are clear, economical and elegant whilst the observable phenomena in this domain are notably complex. The question to which we must address ourselves is whether ET does what any acceptable thesis of any good theoretical structure must do--be, in company with the rest of the structure, useful in construing the phenomena with which we are concerned.[8]

What I should like to emphasize here, therefore, is the claim that ET gives us a good first approximation to the data--it is, in some central cases, much more consonant with intuition and usage than is its denial. To deny ET requires us to make some deeply counterintuitive moves: we must, it would seem, give up either some substantial logical commitments, some firmly ingrained inferential practices or some very plausible metalinguistic beliefs.

For the first--that rejection of ET brings in its wake rejection of some very fundamental logical commitments--Clark offers the following argument, which he says he owes to Geoffrey Hunter (Clark, "Ifs," p. 35). It is, he tells us, intuitively plausible that

(C): If the conjunction p&q entails r, then p entails that if q then r.

It is also non-controversial that

(M): ((p⊃q)&p) entails q.

But substituting 'p⊃q' for 'p' and 'p' for 'q' in (C), we have it immediately that

(E): p⊃q entails that if p then q.

To reject (E) would seem to require us to reject either (M)--modus ponens for the material conditional--or (C). Nobody seems willing to abandon the former and, in Clark's words, the latter seems reasonable until someone produces counter-examples forcing us to abandon it. [9]

Since it is generally agreed that anyone who is willing to say that 'if p then q' is willing to say that (p⊃q), this amounts to showing that rejection of ET requires rejection of at least one logical principle to which we are strongly attached.

Ordinary inferential practice would seem to incline us similarly.

Stalnaker's remarks are at least relevant here--the more so, perhaps, because he has been no friend of ET.

> 'Either the butler or the gardener did it. Therefore, if the butler didn't do it, the gardener did.' This piece of reasoning--call it the direct argument--may seem tedious, but it is surely compelling. Yet if it is a valid inference, then the indicative conditional conclusion must be logically equivalent to the truth-functional conditional.[10]

Finally, not only our inferential practices but what might be termed our metalinguistic (or perhaps metalogical) intuitions would seem also to side with ET. Competent speakers talking about the truth of sentences and the validity of inferences seem inclined to endorse derivations of the following sort.

(1) Let's suppose that we know it is the case that either p is not true or q is true. [$-p \vee q$]

(2) Now let's suppose that p is true. [p]

(3) And surely, you'll grant me that that is the same as saying that p is not not-true? [$--p$]

(4) So look back at (1). Remember: Either p is not true or q is true, and so if p is not not-true. . .? Right! q is true. [q]

(5) Which means we can say that if p is true then q is true. [$p \rightarrow q$]

(6) And if we consider where we started, we can say that if it is the case that either p is not true or q is true, then it is the case that if p is true then q is true. [If $(-p \vee q)$ then $(p \rightarrow q)$][11]

Once started, it is difficult to see where it is possible to withhold assent. We thus have our competent speaker endorsing the only debatable part of the logical equivalence of '$\rightarrow$' and '$\supset$'.[12]

Let us recall that the point of this all is not to prove that $(p \rightarrow q)$ has the same truth-conditions as $(p \supset q)$: it is questionable what that would mean.[13] It is, rather, to suggest that we have strong intuitions which count in favor of their equivalence and that

abandoning this thesis thus requires either giving up some deeply rooted practices or coming up with new explanations of why they are still reasonable. It is only against this background, I think, that it makes sense to look at the puzzles which endorsement of ET brings in its wake.

It is to the puzzles associated with ET that we now turn. In the light of what we have just seen, why is Appiah so ready to give up on ET? Let us postpone for the moment consideration of the question of why he thinks the alternative to ET must be a non-truth-conditional semantics, and simply ask what difficulties appear to bar endorsement of what would seem, so far, to be a creditable enough thesis. Here, at least initially, we shall be traversing rather a well-trodden bit of terrain.

Chapter 2.
Objections and Counterexamples to ET

I shall not review all of the worries which have arisen with regard to ET but, rather, confine discussion to one branch of what is rather a ramifying tree. (It is, however, the branch on which Appiah is sitting.) What we shall note is that in addition to the very general objection to taking the rules of formal logic--any formal logic whatsoever--to be the rules of ordinary language, there are other, more fine-grained, objections which have been raised against the claim that, in particular, the truth-conditions of $(p \rightarrow q)$ are those of $(p \supset q)$.

Three sorts of arguments, more or less closely related, have been made against this thesis. The first, presented most notably by P.F. Strawson, simply invokes speakers' intuitions that the conditions which render $(p \supset q)$ true do not guarantee the truth of $(p \rightarrow q)$. A second argument relies on speakers' tendency to reject a variety of natural language arguments which, under ET, would seem to be instances of deductively valid inference forms. Finally, a third line of argument calls on speakers' judgments that such logical properties as tautologousness, consistency and equivalence may not survive translation from formalisms to natural language under the assumption of ET. We shall consider each of these in turn.

Let us begin with Strawson's well-known objection.

There is a sense, Strawson tells us, in which the logician's rules can, at least, "touch ordinary usage at some vital points" (Strawson, p. 58). Sentences which play the same role in inferences can be gathered together as having the same logical form; rules for the inferences allowable with sentences of such logical form can be drawn up. It is just this possibility which gives force to his criticism of ET in particular. Identification of $(p \rightarrow q)$ with $(p \supset q)$ is not only misleading, he tells us,--as any such identification of this sort would have to be--but definitely <u>wrong</u>, for

> the ordinary conjunction, in its standard or primary use, does not conform to a logical rule which holds for

> the truth-functional constant with which it is identified, and conversely (Strawson, p. 78).
>
> . . . . . .
>
> [O]ne of the sufficient conditions of the truth of a statement of material implication may very well be fulfilled without the conditions for the truth (or reasonableness) of the corresponding hypothetical statement being fulfilled; i.e., a statement of the form 'p⊃q' does not entail the corresponding statement of the form 'if p, then q' (Strawson, p. 83).

Specifically, Strawson tells us, for an ordinary English indicative conditional to be true, there must be grounds for believing that there is some connection between the truth of its antecedent and the truth of its consequent. We are entitled to (p→q) when, for example, this connection is logical or linguistic in nature. (Y): 'If he is a younger son then he has a brother', for example, says that 'He is a younger son but he does not have a brother' is self-contradictory. That is, to
assert (Y) is to claim that there is a relationship of logical or linguistic consequence between its constituents. By itself, however, the logical formulation 'He is a younger son ⊃ he has a brother' makes no such claim. To translate (Y) correctly, Strawson counsels, we would have to represent it formally as

'He is a younger son ⊃ he has a brother' is logically necessary (Strawson, pp. 34 ff.).

We are also entitled to (p→q), Strawson tells us, when the connection which holds between antecedent and consequent is empirical rather than logical. Not all reasonable inferences are linguistic; a step in reasoning can be justified, "not by linguistic rules but by the way things habitually happen in the world" (Strawson, p. 37). We may say such things as 'The butter has been standing on the work surface all morning, therefore it will spread easily.' When we are not sure that something is true but have reason to believe that from its truth something else would follow by such a causal connection, we are, Strawson thinks, also entitled to (p→q): 'If the butter has been standing on the work surface all morning, then it will spread easily.'

Again, however, this is not what is said by $(p \supset q)$, which simply denies the conjunction of p and $-q$.[14]

This characterization of a very important class of ordinary 'if...then...' statements has been termed the consequentialist position (Mackie, p. 67). Aune, following Strawson, formulates it as follows.

> The most general remark one can make. . . is that what is stated (granted, supposed, alluded to) in the if-clause purports to provide a condition of what follows in the sense that if the hypothetical is true, sound, or tenable, the apodosis would constitute a reasonable inference from the protasis [if-clause]. Thus, if one knew or had good reason to think that the hypothetical is sound or true, one could justifiably argue, given the premise p, 'p, so q', and supposing it to be a known fact that q, one could account for q by affirming 'q, because p' (Aune, p. 128, my stress).

If (ignoring a certain casualness here with regard to what constitutes an explanation) we accept this claim that a necessary condition for $(p \rightarrow q)$ being true, sound or tenable is the existence of a relation of consequence or causal connection between p and q, then we must reject ET, for a formula in which '$\supset$' is featured never requires such a relation to hold in order to be true.[15]

Strawson and others have thus argued from speakers' intuitions about the conditions under which isolated indicative conditionals are true or false that there is something more required of us by $(p \rightarrow q)$ than by $(p \supset q)$ and that the something more is a matter of causal or consequential connection. A case against ET has also been argued from the tendency of competent speakers to jib at certain natural language arguments involving conditionals despite the fact that translations of these arguments under the assumption of ET are logically impeccable. The most notorious case is, of course, that of the so-called fallacies of material implication. Although an argument from q to $(p \supset q)$ or from p to $(-p \supset q)$ is deductively valid, ordinary language reasoning along these lines is notably unpersuasive: 'I'll ski tomorrow' does not seem to require us to accept 'Therefore, I'll ski tomorrow if I break my leg today' nor does 'I'll have a second cup of tea' appear to entitle us to

'Therefore, I'll die if I don't have a second cup of tea.[16], [17]

Observations of this sort would seem to fly in the face of the claim that $(p \rightarrow q)$ and $(p \supset q)$ are true under precisely the same circumstances.

Similarly, it does not seem that a denial of $(p \rightarrow q)$ is standardly interpreted truth-functionally as equivalent to p and -q, as ET would appear to require. Cooper, for example, points out that although $-(p \supset q)$ entitles us to p,

It is not the case that if the peace treaty is signed, war will be avoided

is not normally taken as entitling us to

The peace treaty will be signed.

Moreover, since $-(p \supset q)$ also entitles us to -q, ET warrants the inference

It is not the case that if we follow that road we will reach the city

therefore,

We will not reach the city,

which also seems to be a pretty dubious hypothesis about the conduct of ordinary discourse. With a bit more ingenuity yet, it can be pointed out that since $-(p \supset q)$ entitles us to p, it also entitles us to $(-p \supset r)$ for any r, including q, and thus the application of ET to negated conditionals warrants the inference

It isn't true that if he breaks a mirror he will have bad luck

therefore,

If he dosn't break a mirror he will have bad luck.

Finally, since $-(p \supset q)$ entitles us to -q, it also entitles us to $(--q \supset r)$ for any r, including p, which is to say, it entitles us to $(q \supset p)$, and hence, under the assumption of ET, we may conclude from

It is not true that if she is over forty she is still young

to

If she is still young she is over forty (Cooper, pp. 197-8).[18]

That competent speakers do not respond to negated indicative conditionals in the way in which classical logic bids us respond to $-(p \supset q)$

has seemed to Cooper and others to be important evidence against ET.

Finally, even though we seem to have some idea of what the extrasystematic analogues of tautology, consistency and equivalence might be, (to use Haack's handy phrase), these logical properties do not always survive translations certified by ET. Thus, for example, $(-p \supset (p \supset -p)$ is a tautology and we might thus expect its translation under ET to seem true under any circumstances (in the same way that 'Either he's coming or he's not coming' cannot be gainsaid). But competent speakers do not seem to accord such status to, say, 'If she didn't do it if she did, then she didn't'. As Cooper points out, this sounds muddled at best(Cooper, p. 203). Similarly, according to formal logic, the conjunction $(p \supset q) \& (p \supset -q)$ is perfectly licit. We are, however, made distinctly uneasy by 'If John loses he will try again, and, if he loses he will not try again', which sounds somehow inconsistent or self-contradictory (Cooper, pp. 199-202).

Finally, there is the failure of at least some ordinary indicative conditionals to withstand transformation into what ought to be their logical equivalents on the assumption of ET. A speaker who will, for example, happily endorse 'If Carter wins, it will not be by a large margin' will predictably <u>not</u> accept what would appear to be the logically equivalent 'If Carter wins by a large margin, he will not win'; to say 'Even if it rains it will not pour' does not seem to commit a speaker to 'If it pours it will not rain'. Such failures of contraposition would seem, equally, to count against ET.

It was consideration of cases of this sort, Cooper tells us, which led him to the conclusion that

> once one got beyond the simple inference patterns discussed in logic textbooks. . . .[o]ne might almost as well flip coins as use classical logic to try to predict which English arguments would seem reasonable and which not! (Cooper, p. vii)

The advocate of ET has, of course, a response to this which is both straightforward and time-honored. When somebody says that $(p \rightarrow q)$ is not true under circumstances where ET requires it to be true or that an argument in which $(p \rightarrow q)$ occurs is not valid even though ET certifies it as valid, it is possible to reply that the person who offers such

objections simply has mistaken beliefs about the truth of the sentence or the validity of the argument. There is, after all, no a priori warrant that we are perfect reasoners, accepting all of the genuine consequences of our beliefs. Indeed, investigation of inferential errors has become an increasingly popular enterprise among cognitive psychologists. We have, they tell us, rather a pronounced tendency to lump together indiscriminately a variety of 'negative' semantic properties--being untrue, invalid, not universal--into one category and various 'positive' ones--being true, valid or universal--into another. Ascription of one negative (or positive) property is thus likely to bring in its train ascriptions of other negative (or positive) properties. (An analogous information-processing bias in person perception is the familiar 'halo effect'.)

Over forty years ago, Janis and Frick confronted graduate students with syllogisms such as the following and asked them whether the conclusions followed logically from the premises.

> Many brightly colored snakes are poisonous. The copperhead snake is not brightly colored. So the copperhead is not a poisonous snake.
>
> All poisonous things are bitter. Arsenic is not bitter. Therefore, arsenic is not poisonous.
>
> Some Russians are idealists. All Bolsheviks are Russians. It follows, therefore, that some Bolsheviks are idealists.

Janis and Frick also asked the students to indicate, separately, whether they agreed or disagreed with each conclusion. Not surprisingly, they found that their participants' errors with regard to validity were more likely to be consonant with their personal opinions about truth than contrary to them: they were more likely to evaluate an invalid conclusion as valid when they agreed with it and to call a valid conclusion invalid when they disagreed with it than vice-versa.

More recently, Begg and Denny have summarized findings with regard to syllogistic reasoning as follows.

> The first principle, referring to quality, states that

> whenever the quality of at least one premise is negative, the quality of the most frequently accepted conclusion will be negative; when neither premise is negative, the conclusion will be affirmative. The second principle, referring to quantity, states that whenever the quantity of at least one premise is particular, the quantity of the most frequently accepted conclusion will be particular; when neither premise is particular, the conclusion will be universal.[19]

Thus, the following turn out to be popular errors.

Some M are P.
Some S are M.
Therefore, Some S are P.

Some M are P.
Some S are not M.
Therefore, Some S are not P.

Johnson-Laird and Steedman found the same sorts of "atmosphere effect" errors being made in Chicago, New York, Edinburgh, London, Padua, and Nijmegen.[20]

Finally, and closest to home, Wason had participants look at cards with the following symbols on them:

A M 6 3

They were told that each card had a letter on one side, a number on the other. Their task was to indicate which cards had to be turned over to determine the truth of the rule 'If there is a vowel on one side, there is an even number on the other side'. Almost without exception, participants chose either 'A' or 'A and 6'. [21]

In the light of such findings, one might either insist that we take to heart the fact that there are strong and widespread intuitions counter to the most unarguable truths of classical logic or one might, more reasonably, conjecture that there are reasons why people are apt in some cases simply to be misled about what logically follows from what. Nor is there any reason why we could not take this view even of our own judgments and try to tutor our own intuitions to conform more closely to our best beliefs on the matter.

Where ET and intuition part company, then, one response is to say that intuition is in error. There are two rather closely related objections to taking this tack which may, I think, be met in much the same way. First, it may be argued that while such a response might be appropriate in the case of conflicts between our intuitions and the deliverances of ET with respect to the validity of more or less complicated inferences--the sort which, for example, we find ourselves wanting to cast into logical notation--it is not persuasive when what we are dealing with are intuitions about the meaning of single, simple indicative conditionals. Surely, as competent native speakers, we cannot be wrong about something like <u>that</u>? Secondly, it may be objected that the errors located by cognitive psychologists are, after all, only occasional: granted, some errors are much more widespread than others but not everybody falls into the same trap. Rejection of the so-called fallacies of material implication, however, is well-nigh unanimous. Could we <u>all</u> be wrong about that? Does it even make sense to ask such a question? Both of these objections raise, in slightly different form, the question of whether ordinary usage might not have a special claim on our commitments.

I think the most relevant observation here comes from John Austin. Talking about our ordinary language locutions with regard to actions, he observed that they might invoke models and noted that

> [T]here is no necessity that the various models used in creating our vocabulary. . . should all fit together neatly as parts into one single, total model or scheme. . . . It is possible, and indeed highly likely that our assortment of models will include some, or many, that are overlapping, conflicting, or more generally simply <u>disparate</u>.[22]

First, then, if such local disconnectedness may be presumed to exist with respect to our model for human action, why not with regard to our model for the logical relationships involved in the set of basic connectives? If the meanings of these terms is given by something like their logical powers, then, to have an inconsistent model would be, arguably, to be (at the very least) in a position to change one's mind about what could be true of their powers and, thus, to change one's

mind about their meanings. Second, if, as Austin allows, we can make progress as a community in linguistically encoding more useful discriminations and generalizations, and developing better models for, say, human action, why might we not be capable of making progress (all of us, together) in getting a more consistent model of the logic of our language? Perhaps, to paraphrase Austin, our ordinary linguistic intuitions with regard to the relationship between $(p \rightarrow q)$ and $(p \supset q)$ are the first word but not the last word.[23]

There would thus seem to be no reason to object, in principle, to the claim that even competent speakers' intuitions with regard to the truth of conditionals or the validity of inferences in which they occur might be in error.

There is, of course, another, equally time-honored response to what sound to us like erroneous judgments. Rather than concluding that our informant is talking about what we are talking about and holding incorrect views on the subject, we can always decide that there is no disagreement between us because he or she is making perfectly defensible claims about something altogether different. As we shall see, it is possible to claim that when competent speakers tell us, contrary to the deliverances of formal logic on the assumption of ET, that an ordinary English indicative conditional is false or an inference invalid, we may decide that what they are <u>really</u> referring to is its unassertability or its failure to preserve assertability. It is not clear that there is much to be gained by such an exercise in charity. While it does obviate the necessity for saying that ordinary speakers are widely mistaken about the truth or falsity of their utterances, it confronts us instead with the need to see them as confused about what 'true' and 'false' mean. Moreover, the question of when we ought to assume differences in belief and when we ought to assume differences in meaning is itself problematic: it has been denied that there is, indeed, any fact of the matter to be established in interesting cases.[24]

Whichever tack we take, we shall, in any case, need to have a very persuasive account of how we come to be so systematically misguided in our intuitions about truth and validity (or so thoroughly confused with regard to the distinction between truth and assertability) where

indicative conditionals are involved. We shall need not only to be willing to be convinced that we are in error but helped to see how we got there. To use Rorty's homely example, part of what is involved in convincing the natives that madness is not the result of demonic possession is giving them a plausible account of how they came to think they saw demons.[25] Having disposed of the notion that linguistic intuitions are somehow incorrigible is part of the battle; we still need an explanation of how they come, in this particular case, to need such substantial correction. Such an account is, of course, just what is provided by Grice's well-known conversationalist defense of ET.[26] It is to this that we now turn.

Chapter 3.
Grice's Conversationalist Strategy

Grice accepts Strawson's observation that, in standard cases, at least, we are indeed inclined to feel that ($p \rightarrow q$) involves something over and above the truth of ($p \supset q$) and, specifically, that ($p \rightarrow q$) seems to commit us to what Grice calls the Indirectness Condition--the claim that there are non-truth-functional grounds for accepting ($p \supset q$).[27] He takes his task to be provision of an explanation for this intuition which does not require endorsement of Strawson's conclusion that ($p \rightarrow q$) and ($p \supset q$) must, therefore, differ in their truth conditions. Grice's strategy involves two major moves. First, he develops a general theoretical distinction between what, on the one hand, a competent speaker knows to be <u>said</u> by the use of an expression and what, on the other hand, such a speaker will recognize as being <u>implicated</u> by its use. For Grice, "what is said (in a favored sense of 'say')" is a technical term referring to the truth-conditional content of an utterance.[28] He uses 'implicate' in contrast to such logical terms as 'entail' or 'have as a logical consequence' (Levinson, p. 103). Thus, to distinguish what is (in this special sense) said from what is implicated allows us to see, in general, how an utterance may convey something which does not affect its truth or falsity.

Next, Grice gives an account of how ($p \rightarrow q$), in particular, can come standardly to implicate the Indirectness Condition. If Grice's proposal is accepted, our sense that ($p \rightarrow q$) conveys the claim that there is a connection between p and q can be acknowledged without our having to accept a difference in truth conditions between ($p \rightarrow q$) and ($p \supset q$).

To begin with, then, Grice asks us to distinguish between what is said and what is implicated. When we offer (P): 'She was poor but she was honest', for example, our statement requires for its truth only that she be both poor and honest. That it would not normally be offered (or accepted) unless we also believed that poverty and honesty were generally incompatible is, Grice tells us, a matter not of the truth conditions of (P) but of the implicature conventionally

associated with 'but': (P) is true if and only if she is poor and she is honest.

Implicatures may be of various sorts, according to Grice. On the one hand, there are entirely arbitrary associations between terms or locutions and their implicatures just as there are completely arbitrary associations between terms and the truth-conditions for utterances involving these terms. (There is, for example, no reason why 'but' should indicate a presumed incompatibility, beyond the brute fact that it does). Such cases are called conventional implicatures. On the other hand, there are cases where we can derive, as it were, what is implicated from our knowledge of the conventional content of the words used--what is said and what is conventionally implicated--the context of use, certain bits of background information and our familiarity with some very general rules governing (at least certain sorts of) discourse. Grice calls these rules conversational maxims and implicatures in whose derivation they figure essentially are called conversational implicatures.

It is our commitment to construe speakers' contributions as being in conformity with the requirements of discourse (unless we have clear signals to the contrary) which gives rise to conversational implicatures. Given an utterance, S, we regularly attribute to the speaker beliefs such that S would meet (at least some) such requirements were the speaker to hold these beliefs, have an intention to convey them and be in compliance with conversational maxims. The content of these presumed beliefs constitutes the conversational implicature of S.[29]

The conversational maxim of Quantity, for example, bids us offer enough information to be useful to our listener; the maxim of Quality requires that what we offer be factually satisfactory; the maxim of Relevance makes a demand which is obvious (albeit difficult to explicate satisfactorily). The maxim of Manner enjoins such virtues as clarity, brevity and orderliness. Unless given reason to believe otherwise, a competent language user expects conversational partners to be in accordance with these and construes utterances accordingly. Thus, if to my 'Is there any coffee left?' my partner replies 'There's plenty of cocoa', I routinely assume that this is a helpful, informative response and conclude that the coffee is all gone (but that

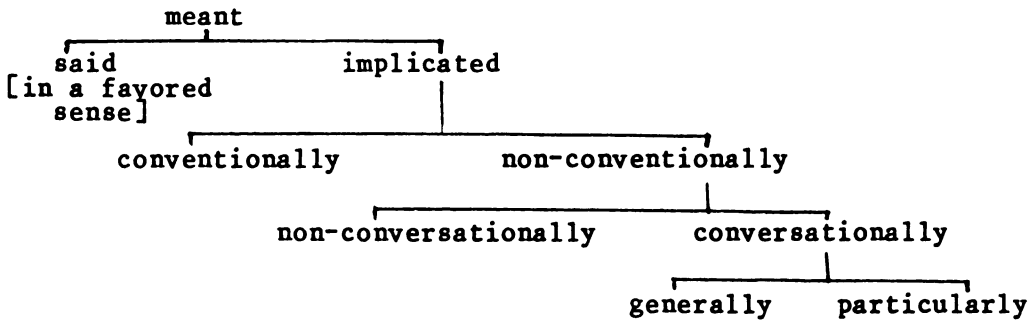
there is plenty of cocoa). Moreover, according to Grice, I am committed to extending such charity even to what might present themselves as distinctly deviant utterances--ones which, in his words, appear to flout conversational maxims. If, in response to my 'What do you think of Smith's work in philosophy?', my partner offers the conspicuously uninformative and irrelevant 'He bakes good bread', I ordinarily take myself to be confronted not with trash but with a puzzle. Was the utterance meant metaphorically? Is my partner in no position to offer reliably the news that Smith is a competent philosopher and hence--in accordance with the maxim of Quality and adherence to something like 'Nil nisi bonum'--offering this skimpy reply in expectation that I will infer that Smith is <u>not</u> notably competent? Conversational implicatures are often rather drastically underdetermined by the data; nevertheless, the exploitation of flouted maxims seems to play a significant role in natural language.

There is one final distinction to be made. When the hearer makes use, perforce, of information about a specific context of use in deriving an implicature, the conversational implicature thus generated is called a <u>particularized</u> conversational implicature. (The examples we have just considered are thus both instances of particularized conversational implicatures.) By contrast, in those cases where the implicature derives from what we might think of as standing requirements for information--demands and expectations which, in the nature of things, quite predictably attach to whole categories of discourse--the standardly resulting implicature is termed a <u>generalized</u> conversational implicature.

Grice's example here is the interest we may quite predictably be assumed to have in the existence of a relationship of ownership between a person and some object under discussion. It will, for example, generally matter to an interested listener whether the house out of which someone stepped was her own or someone else's. To offer 'She came out of a house', then, under conditions where it might be possible reliably to offer 'She came out of her house', would be, quite generally, to violate at least a couple of conversational maxims. A competent speaker might therefore be entitled to conclude that the potentially useful specification was not given because it could not be

offered reliably. Hence, 'She came out of a house' would standardly implicate that the house was not her own. There is, thus, a whole range of contexts in which use of the indefinite article carries a generalized conversational implicature of non-ownership. We do not, in such cases, have to derive the implicature for each member of the class but learn it as part of what we know about the use of indefinite articles and certain categories of situations.

The architecture of Grice's scheme is tidily displayed in the following figure, adapted from Levinson's text on pragmatics (Levinson, p. 131).



(It may be wondered what a non-conventional, non-conversational implicature would be like. It would be a meaning intentionally conveyed by the speaker by means of mutual knowledge and the hearer's recognition of the speaker's intention to convey this meaning by means of the hearer's recognition of such an intention rather than by conventional encoding in word or gesture <u>and</u>, in addition, no conversational maxim would be involved in generating the implicature. For example, a speaker might non-conventionally, non-conversationally convey the meaning 'I am finding this party terribly boring' by failing to hide a yawn which he/she knows his/her listener knows he/she could very well have hidden had he/she made a bit more effort so to do. If, on the other hand, both parties share the knowledge that the yawn could not have been voluntarily masked, it will at most be symptomatic of boredom without implicating it.)

It is clear that we need some guidance in discriminating among these kinds of communicational content (Levinson, p. 131). Grice tells us that there is no knock-down test of whether what we have on our hands is a conversational <u>implicatum</u> rather than merely "an element in

the conventional meaning of the sentence in question' (Grice, 1978, p. 115). He does, however, offer us what he terms hallmarks characteristic of conversational implicatures. We have already alluded to the first of these: conventional aspects of meaning are attached to terms purely arbitrarily whereas non-conventional aspects are derivable and, in particular, the derivation of conversational implicatures essentially involves maxims of conversation (or at any rate, an over-arching principle of conversational co-operation).

Second, unlike what is conventionally said or implicated, conversational implicatures can be <u>cancelled</u>, either explicitly, by some feature of their linguistic context, or implicitly, by some aspect of the non-linguistic context of discourse. Having answered the query about Smith's competence as a philosopher by commenting on his croissants, I may add 'I really don't know anything one way or another about his work in the field', thus explicitly cancelling the particularized conversational implicature that his work is unsatisfactory. If we happen to be standing on a dark street, knee deep in fog, when I say, 'She came out of a house,' the generalized conversational implicature that the individual in question came out of someone else's house is implicitly cancelled by the situational context. Cancellability is a second hallmark, then, of conversational implicatures.

Finally, conversational implicatures are independent of the precise wording of the utterance in question. With the exception of those special conversational implicatures which depend upon the manner in which information is given (e.g., in violation, say, of maxims dictating clarity, orderliness, brevity or the like), conversational implicatures cannot be avoided simply by choosing another turn of phrase. They are, in Grice's terminology, <u>non-detachable</u> (Grice, 1975, p. 74).

With this in mind, we can consider Grice's conversationalist defense of ET. The difference between $(p \rightarrow q)$ and $(p \supset q)$ to which Strawson draws our attention, Grice argues, is not a difference in what is said--in what makes $(p \rightarrow q)$ true--but, is, rather, a matter of the existence of a generalized conversational implicature associated (in the absence of cancelling conditions) with the ordinary English

conditional. To substantiate this claim, he first offers evidence that the Indirectness Condition (hereafter, IC) is, indeed, both cancellable and non-detachable. He then goes on to show that he can provide a plausible account of how just such an implicature might come standardly to be conveyed by the use of $(p \rightarrow q)$.

The implicit or situational cancellability of IC may be shown, Grice tells us, by its non-appearance in such contexts as puzzles and games, where 'If there is an even number on the front of the card then there is a vowel on the back', for example, conveys no suggestion whatsoever of a consequential relationship or connection between the antecedent and consequent. IC may also be cancelled explicitly, as, for example, by saying, 'I know just where Smith is and what he is doing, but all I will tell you is that if he is in the library he is working' (Grice, Lecture IV, p. 3).

The non-detachability of IC may be shown by its persistence across a variety of more or less idiomatic paraphrases--e.g., the suggestion of non-truth functional grounds for asserting 'If Smith is in London he is attending the meeting' is, Grice tells us, present equally when we say, instead, 'Either Smith is not in London or he is attending the meeting' (Grice, Lecture IV, pp. 1-2). Grice is willing to claim that even in a recitation of the deliverances of the appropriate truth table we may still detect the implication (Grice, Lecture IV, p. 2). Both the cancellability and the non-detachability of IC count toward our considering it as a conversational implicature of $(p \rightarrow q)$ rather than as a feature of what counts toward its truth or falsity, Grice argues.

Finally, we have noted that, on Grice's account, a distinguishing characteristic of a conversational implicature is the availability of a functional derivation: we must be able to see how the implication would be generated. Grice offers two separate accounts of how $(p \supset q)$ could come to have IC as a generalized conversational implicature. The first of these is the better known; Grice characterizes the second as deeper (Grice, Lecture IV, p. 6). I shall go through both. It may be noted that the two accounts begin differently, each presenting a distinct account of how $(p \rightarrow q)$ might come standardly to implicate the absence of truth-functional grounds for $(p \supset q)$. The two accounts then converge, agreeing that given the maxim of Quality, which forbids us to assert

anything in the absence of adequate grounds, assertion of (p→q) will come standardly to implicate the existence of non-truth-functional grounds for its assertion, which is just to say that it will implicate IC.

The first way of accounting for the fact that use of (p→q) implicates a connection between p and q--and, more specifically, a causal connection between them--goes like this. Suppose that the truth conditions of (p→q) are, indeed, those of (p⊃q). The conversational maxims dictate giving one's partner relevant information in as economical a form as possible. Hence, anyone who had truth-functional grounds for asserting (p→q)--i.e., anyone assured either of the falsity of p or the truth of q--would be better advised to assert whichever of these was available. Failure to assert either the stronger -p or q _tout court_ would thus come to implicate the speaker's lack of a purely truth-functional basis for the assertion of (p→q). Offered, then, by a speaker presumed to be in accordance with the conversational maxim requiring us not to assert that for which we have inadequate grounds, (p→q) would be a signal that the speaker had some _other_ ground for its assertion. Whence, to offer (p→q) would be to produce a generalized conversational implicature of some non-truth functional basis for its assertion--some causal or consequential connection between p and q. Thus, to offer 'If John is there then Mary is there' is needlessly verbose when we are either sure that John is not there or sure that Mary is there; in the absence of such assurance, we are entitled to field it only if we have some other reason to endorse the entire conditional. The most plausible reason is that we know some causal connection between John's presence and Mary's such that either John is not there or John is there and Mary is there. That such a connection exists is, thus, the implicature standardly conveyed to a competent language user.

Grice offers a second and, he believes, more satisfactory derivation of the implicature of the existence of such indirect evidence for (p⊃q) conveyed by use of (p→q). He argues that (p⊃q) is a logical formula particularly well suited for reasoning from the truth of p to q--i.e., for using modus ponens. It seems plausible that the deployment of a locution which has a particular use--a special _metier_ or _raison d'etre_--in preference to a logically equivalent expression

(especially, perhaps, a more basic one) might come standardly to convey the conversational implicature that conditions are, indeed, appropriate for just that use (Grice, Lecture IV, pp. 17 ff). We might thus expect the choice of (p→q) to have as a generalized conversational implicature that conditions are appropriate to using modus ponens--that is, (1) that there is a good chance that p is true, and (2) that we are in the dark as to the truth of q. (If we are already convinced of the truth of q, we are hardly in the market for a modus ponens argument and if p is false, we are in no position to carry it through.) If (1) and (2) are the case, however, once again we do not have truth-functional grounds for asserting (p→q) and would thus be in violation of the maxim of Quality unless we had non-truth-functional grounds for its assertion --that is, unless we knew of some connection between p and q. Hence, once again, assertion of (p→q) would be expected quite standardly to come to convey the implication of IC.

In summary, then, Grice has made a theoretical distinction between what is required for the truth of an utterance and what is conveyed by it and he has given reasons why (p→q), in particular, might come standardly to implicate the existence of uncertainty as to the truth of both its constituents and, thereby, the existence of some causal or logical connection between the truth of the antecedent and that of the consequent. Finally, he has taken pains to provide evidence that what (p→q) conveys over and above what (p⊃q) requires for its truth is a conversational implicature and not part of its truth-conditions. Confronting intuitions that our commitments are greater with respect to (p→q) than (p⊃q), Grice's answer is thus that there is, indeed, such a difference but that it is a matter of what is implicated by (p→q) rather than what its truth-conditions are.

We can, moreover, extend this account to take care of some of the other counterintuitive consequences of ET which we have noted. A competent speaker, it would seem, should not only avoid saying what is false but avoid implicating it as well: an utterance which conveys an implicature which is not factually satisfactory should be as inappropriate a contribution to the conversation as one which is downright false. There should thus be two logically distinct bases for finding an utterance unassertable (to use the customary term)--either

its own factual insufficiency or that of some one of its implicatures (if any). It is easy to see how these two aspects of assertability might become confused. A speaker who knew that an utterance S was somehow suspect might not be clear about whether S was unassertable-because-false or unassertable-because-conveying-a-false-implicature. Since the truth of an utterance is, on Grice's account, logically independent of the truth of its implicatures (if any), there is possibly a class of utterances which are true but which convey false implicatures. Such true-but-unassertable utterances are clearly prime candidates for being (erroneously) evaluated as false.

This account not only explains speakers' intuitions that a material conditional may be true whilst the corresponding ordinary English indicative conditional is not, but it also provides a way to cope with speakers' unwillingness to acknowledge as valid inferences which ought, under ET, to be so. Thus, confronting the so-called paradoxes of material implication, the canny conversationalist can point out that while the truth of q surely guarantees the truth of $(p \rightarrow q)$, there is no particular reason to expect it to underwrite its conversational appropriateness. A speaker who finds a premise assertable and a conclusion unassertable may quite understandably mistake this for an argument whose premise is true and whose conclusion is false and thereby (understandably but erroneously) label the inference as invalid.

What is more, the conversationalist hypothesis that rejection of a sentence may be construed as a rejection of its implicature rather than as an assertion of its negation can also help with the vexing case of negated conditionals. If an utterance can simultaneously convey two logically independent claims, 'It is not the case that. . .' (or some similar locution) might be used with respect to either one of them. It might thus be used either to negate something said or to reject something implicated. Given this option, (L) It is not the case that if he breaks the mirror he will have bad luck, is surely better understood as an objection to the implicature that there is a causal connection between breaking a mirror and having bad luck than as a negation of the conditional. On this reading, (L) does not require us to endorse 'He will have bad luck' nor 'If he doesn't break a mirror he

will have bad luck', which is just what our competent speaker would have insisted all along. In the case of negated conditionals, as Jeffrey and others have pointed out, speakers may best be heard as committing themselves to less than they appear to be saying (Jeffrey, 1981, p. 79).

It can easily be seen why Mackie has characterized Grice's proposal as an ingenious and forceful defense against Strawson's consequentialist claim that $(p \rightarrow q)$and $(p \supset q)$ have different truth conditions (Mackie, p. 77). Currently, however, advocates of ET face quite another challenge--a proposal by Ernest Adams that we abandon truth-conditional semantics for conditionals entirely. Interestingly, Adams also deploys Gricean conversationalist strategies on behalf of his program. We shall be looking first at Adams' work on the logic of conditionals and then at two recent responses to it by David Lewis and Frank Jackson.

Chapter 4.
Adam's Proposal and Lewis' Theorems

On Grice's account, as we have seen, formal logic is a correct, albeit incomplete, theory of ordinary language reasoning. Discrepancies between its verdicts and the intuitions of competent speakers are to be attributed to an understandable but essentially extralogical disinclination to trade in statements which would convey misleading implicatures. It is also possible to argue, however, as Ernest Adams has done, that when we discover competent language users rejecting inferences certified by formal logic, it is to the validity of the arguments themselves that we should turn our attention. Adams claims that various well-known discrepancies between the deliverances of logic and intuition are, in fact, attributable to misapplications of what he takes to be a very inadequate theory. [30] What Adams proposes, therefore, is a wholesale revision of the semantics of standard logic which will bring the judgments of the logician and the competent speaker into accord with each other. As we shall see, one cost of achieving such concordance is abandonment of anything even vaguely resembling ET.

Adams' proposal has two major components. The first is the replacement of standard semantic terms--'truth' and 'deductive validity'--by the analogues 'justifiable assertion' and 'reasonableness of inference'. Justifiability, in turn, is for Adams a matter of there being adequately high probabilities associated with statements. The second major component of Adams' proposal is that the probability to be associated with $(p \rightarrow q)$ be a conditional probability,

$$Pr(q/p) =_{def.} Pr(p\&q)/Pr(p).$$

Let us look at these two proposals in turn. First, Adams suggests that we cannot properly evaluate the sorts of arguments

with which we are concerned in terms of the standard criterion of deductive validity. He asks us, for example, to consider the problematic argument

(P) John will arrive on the 10 o'clock plane.

Therefore,

(C) If John does not arrive on the 10 o'clock plane, then he will arrive on the 11 o'clock plane.[31]

Is it fallacious?

According to the conversationalist account, as we have seen, the answer is that the argument is valid but is unlikely to appear so since it takes us from an assertable premise, (P), to an unassertable conclusion, (C). (C) is unassertable because, given (P), the speaker is in a position to offer

(-P) It is not the case that John will not arrive on the 10 o'clock plane

and under these circumstances it is misleading to offer the logically weaker (C) instead--the listener will standardly conclude both that there is a significant degree of uncertainty about (-P), which is not the case, and that there is some causal connection between (-P) and 'John will arrive on the 11 o'clock plane', which is also not the case. Our unease, the Gricean theorist would say, is due not to the invalidity of the inference in question but to our moving from what we might think of as a 'positively signed' premise to a 'negatively signed' conclusion.

Adams, however, looks at the case rather differently. Is the inference warranted?

> If we attempt to answer this question by applying the criterion of validity of formal logic,. . . we must ask: Is it logically possible for P to be true, but C false?
>
> To attempt to answer this question is to see that it has no clear answer. The reason is that the term 'true' has no clear ordinary sense as applied to conditionals, particularly to those whose antecedents prove to be false, as the antecedent of C must [be] if P is true. This is not to say that conditional statements with false antecedents are not sometimes <u>called</u> 'true'

> and sometimes 'false', but that there are no clear criteria for the applications of those terms in such cases. (Adams, 1965, p. 169)

This is, Adams tells us, an empirical claim:

> an assertion about the ordinary usage of the terms 'true' and 'false'. . . [which] can be verified, if at all, only by examining that usage (Adams, 1965, p. 169).[32]

From Adams' viewpoint, then, we are faced with a problem. In the absence of criteria for the truth or falsity of such sentences, what we need, he tells us, is some other way of characterizing the validity of inferences involving conditionals (Adams, 1965, p. 170). His proposal is a condition for reasonableness of inferences:

> If an inference is reasonable, it should not be the case that on some occasion the assertion of its premises would be justified, but the denial of its conclusion also [be] justified (Adams,1965, p. 171).

Intuitively, assertions are justified by an appropriately high probability that a bet on the statement in question would win; denials are justified by an appropriately high probability that a bet against the statement would win. In his more recent work on the subject, Adams offers an alternative probabilistic soundness criterion:

> it should be impossible for the premises of an inference to be probable while its conclusion is improbable (Adams, 1975, p.1).

Adams believes that his probabilistic criterion reflects, better than the classical truth-conditional criterion, both the strategy which competent speakers actually follow in evaluating arguments in everyday settings and the strategy which a rational agent should employ in reasoning.

The second major issue to which Adam addresses himself is the nature of the probabilities to be assigned to various sorts of compounded statements. He agrees with the conventions which are normally applied except in the case of conditionals. He tells us that although in the case of many conditional statements

> the conditions of settling bets are just as clear as

> they are on particular non-conditional statements. . . a bet that 'if p then q' is [in most cases] conditional--in force only if p proves true, and in that case winning if q is true, and losing if q is false (Adams, 1975, p. 175).

Adams therefore takes it to be intuitively plausible that the probability of an indicative conditional of the form 'if A is the case then B is' is a conditional probability (Adams, 1975, p.3). He is prepared to argue that 'appropriateness' considerations--what one <u>wants</u> of conditional conclusions--also support this assumption (Adams, 1975, p. 3).

Taken together, Adams' proposal for a probabilistic criterion of reasonable inference and his assignment of a conditional probability to $(p \rightarrow q)$ yield surprising verdicts about some classically endorsed inference patterns which involve conditionals.

The easiest way to see this is to present examples, making use of Adams' informal Venn diagram technique. Consider Figure 1.

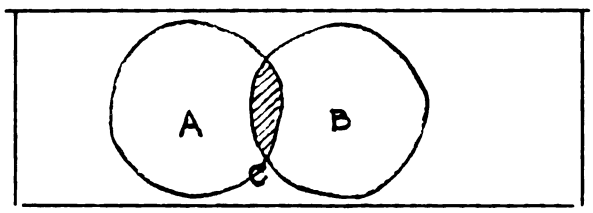


Figure 1.

Let the points within the rectangle represent all possible states of affairs and the subregions A and B represent those subsets of states of affairs in which statements A and B are true, respectively. Then the shaded area C represents the subset of possible states of affairs in which (A & B) is true and so on for various non-conditional statements compounded from A, B and their negations. Setting the area of the rectangle equal to 1, we may represent the probabilities of such statements being true by the area of their corresponding subregions. The probability of a conditional is not, of course, represented by an area within the rectangle but, rather, by a ratio between such areas.

> [T]he probability of the conditional $A \rightarrow B$ is identified in the diagram with the <u>proportion</u> of subregion A which lies inside subregion B. If most of region A lies

> inside region B, this is interpreted to mean that the probability of A→B is high, and if most of A lies outside of B the probability of the conditional is low . . . [I]f p(A)=0 then the probability of A→B is not defined (Adams, 1975, p. 10).

Such a diagram can be used, Adams tells us, to show that the so-called fallacies of material implication, for example, represent inference schemata which do not, in fact, meet the criterion of probabilistic soundness: we can construct a diagram which shows the premise as being highly probable and the conclusion as having a probability of 0.

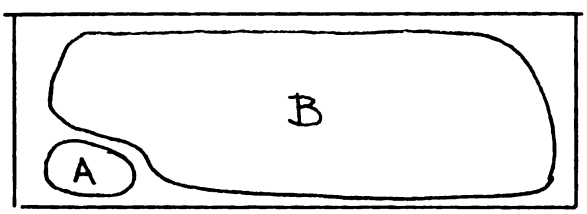


Figure 2.

According to Figure 2, while both B and -A are probable, (A→B) has a probability equal to 0 since none of the subregion A lies within B. To find an instance of such a probabilistic pattern, Adams tells us, we need propositions A and B such that

(1) A must be highly improbable;

(2) B must be highly probable;

(3) A must 'probabilistically exclude' B (Adams, 1975, p.11).

One such pair of statements would seem to be those we have already encountered in Jeffrey's astounding inference.

A = I break my leg today; B = I'll ski tomorrow.

No wonder, Adams would tell us, we reject the argument 'I'll ski tomorrow, therefore, if I break my leg today I'll ski tomorrow': it is not rational so to reason.

Having premises of less than total certainty and assigning conditional probabilities to conditional statements, then, can lead to radical divergence between classical and probabilistic soundness. Adams explains why this should be so. Let us define the uncertainty of p as

$$u(p) =_{def.} 1 - Pr(p)$$

It can be shown that probabilistic soundness parallels deductive validity neatly just so long as an inference involves nothing but non-conditional sentences and material conditionals. That is, just as a deductively valid inference schema cannot take us from true premises to a false conclusion, so the same pattern, applied to probabilistic premises, cannot take us from premises of any given degree of uncertainty to a conclusion whose uncertainty is greater than the sum of the uncertainty of those premises. As long as we avoid inferences with very many premises, then, we will not be taken from premises of high probability to conclusions of low probability. Thus, a deductively sound pattern will also be a probabilistically sound one.

When, however, we are dealing with a conditional conclusion ($p \rightarrow q$) whose probability is the conditional probability $Pr(q/p)$, the possible uncertainty of the conclusion relative to that of the premises increases. This is because the uncertainty of ($p \rightarrow q$) so construed will almost always be greater than that of the corresponding material conditional.

$$u(p \rightarrow q) = u(p \supset q)/Pr(p) \quad \text{(Appendix, a)}$$

Hence, a pattern of inference that would be rational to use with premises which were certainly true (and which would even be probabilistically sound on a truth-functional construal of the probability of ($p \rightarrow q$)) can carry us from probable premises to an improbable (conditional) conclusion. (Under these circumstances we can, in fact, be taken from premises whose probability is as close as we wish to 1 to a conclusion whose probability is as close as we wish to 0.)

Perhaps even more surprisingly, Adams points out that an inference schema which is both classically and intuitively acceptable may turn out to be probabilistically unsound, as in the case of contraposition. Consider Figure 3.

Now take the argument from ($B \rightarrow -A$) to ($A \rightarrow -B$). Most of B lies within -A so the premise is probable. None of A lies within -B, however, so the conclusion is not probable. If A = There will be a terrible cloudburst and B = It will rain tomorrow, one might well accept 'If it rains tomorrow there will not be a terrific cloudburst'

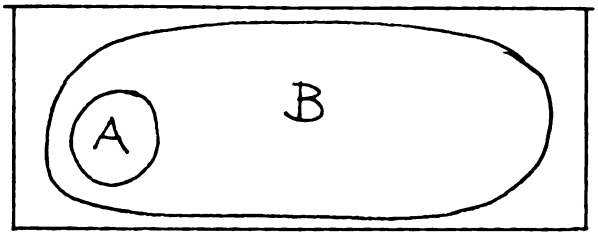


Figure 3.

but reject 'If there is a terrific cloudburst tomorrow, it will not rain.' Nevertheless, contraposition is not only a logically impeccable inference schema but, unlike the so-called fallacies of material implication, one which figures in ordinary argumentation as well.

At this point, it might appear that Adams has overshot his mark. It is one thing to field a proposal which shows that inferences normally rejected by competent speakers are, indeed, unreasonable; it is quite another to arrive at the conclusion that inference schemata which are both legitimated by standard logic and routinely applied by competent speakers fall short of our criterion of rationality. Adams' response to this is, interestingly, to invoke the Gricean doctrine of conversational implicature. Although it is possible, as we have seen, to construct probabilistic counterexamples to widely accepted schemata, he tells us, the conventions governing their use protect us from falling into the sorts of error which we have found to be possible.

> [I]f one accepts the probabilistic unsoundness of contraposition he is in a dilemma: what is to be made of all of the real life reasoning which seems to be of this form? It is too much to condemn it as irrational <u>simpliciter</u>. We will argue . . . that most such reasoning <u>is</u> rational, only, it is not rational <u>in virtue of being of the contraposition form</u>. Where such inferences are rational it is because further conditions are satisfied which usually obtain when persons are <u>told</u> propositions of the form B→A which are not part of the meaning of the proposition (Adams, 1975, p. 15).

Adams calls such conditions <u>conditions of partial rationality</u> (Adams, p. 18). If we figure out the circumstances under which an inference schema would not lead us astray, then, we should expect to find that

there are conversational constraints which ensure that it is used only under these conditions of rationality. To show that this is, indeed, the case, Adams shows us, first, when inferences involving conditional conclusions are, so to speak, safe.

> [T]wo rules relating uncertainties of premises to conclusion uncertainties throw light on partial rationality conditions. . . (1) The uncertainty of a factual conclusion of a truth-conditionally sound inference cannot exceed the sum of the uncertainties of the premises, whether or not the premises include conditionals. (2) The uncertainty of an indicative conditional equals the uncertainty of the corresponding material conditional divided by the probability of its antecedent.
>
> $[u(A \rightarrow B) = u(A \supset B)/p(A)]$
>
> Combining these we get: the uncertainty of a conditional conclusion of a truth-conditionally sound inference cannot exceed the sum of the uncertainties of the premises divided by the probability of the conditional's antecedent. Hence we can say roughly that if a truth-conditionally sound inference with [a] conditional conclusion has highly probably premises then its conclusion must [also] be probable <u>provided the conditional's antecedent is not highly improbable</u> (Adams, 1975, pp. 18-19).

Thus, in the case of contraposition, the uncertainty of the conclusion, $u(A \rightarrowtail B)$, cannot be greater than the uncertainty of the (single) premise, $u(B \rightarrowtail A)$, divided by the probability of A. We will not go from a probable premise to an improbable conclusion, then, unless the probability of A is very small, which is to say, unless $p(-A)$ is large. We are thus reasonably safe (and safely reasonable) so long as -A is not very probable. But, says Adams,

> [I]n the ordinary situation in which a speaker makes an assertion of the form $B \rightarrowtail A$ . . . it is unusual for -A to be by itself probable. Possibly it is misleading and in violation of conversational 'helpfulness maxims' for a

> speaker to say, e.g. 'if Jones attends the party then Smith won't' when he also believes 'Smith won't attend the party'. In such circumstances one feels the 'even if' locution to be the appropriate one, as in 'Smith won't attend the party, even if Jones attends' or 'there won't be a terrific cloudburst, even it it rains' (Adams, 1975, p. 21).

Adams thus uses Grice's strategy to support a position which is diametrically opposed to ET.[33]

Adams' proposal has been an influential one. It is generally agreed, even by theorists who otherwise differ in their views, that he is correct in his hypothesis that the assertability of ordinary English indicative conditionals can be predicted on the basis of the conditional probability with which he proposes that they be associated. Advocates of ET must now contend not only with the objections at which we have already looked but with what appears to be a complete alternative to their own program. As we shall see, however, there are reasons why it is not an unqualifiedly attractive alternative.

Adams observes that his proposal to adopt a criterion of probabilistic reasonableness and assign the conditional probability $Pr(q/p)$ to $(p \rightarrow q)$ is quite controversial at the present juncture--largely, one suspects, because its implications are radical (Adams, 1975, p. 3). Indeed, Adams has been the first to call attention to the departures from classical practice demanded by his views. These are of two sorts. The first (and generally less commented upon) involves syntactic revisions, the second, semantic ones. Let us look at these in turn.

First, we may note that, for Adams, a rule of inference is valid--universally probabilistically sound--if it cannot take us from probable (justified) premises to an improbable conclusion (one whose denial is justified). As we have just seen, a given rule may be probabilistically sound under special circumstances: a truth-conditionally sound inference is probabilistically sound so long as the antecedent of its conditional conclusion is not too improbable, for example. Adams thinks it plausible that recognition that a conclusion's antecedent is <u>not too improbable</u> may thus standardly function as a

'tacit premise' in real life reasoning (Adams, 1975, p. 19.). A premise of this sort cannot, however, be expressed in the symbolism of standard logic and therefore cannot figure among its rules. Nor, to look at the matter from a slightly different angle, can the standards of correct usage which, on Adams' account, corral everyday inferences within the bounds of partial rationality (e.g., the rule that governs application of 'even if') be represented formally in the notation of classical logic (Adams, 1965, pp. 191-2). Adams therefore thinks that systematic analysis of rational inference will require a 'syntactic generalization' of standard logic (Adams, 1975, p. 19).

The second radical consequence of Adams' proposal is, of course, its implications for traditional semantics. These go well beyond the obvious fact that Adams proposes to reject the hegemony of classical criteria of deductive validity and the use of standard truth conditions for conditionals. Adams makes clear, to begin with, the futility of what would seem at first glance to be an attractively moderate response to his proposal. Looking at the fact that truth-conditional soundness is closely related to probabilistic soundness in the case of inferences in which only factual propositions appear, we may imagine that there is still some way of assigning truth conditions to $(p \rightarrow q)$ consonant with Adams proposal which will preserve this neat parallel. Adams throws cold water on any such hope. It is, he tells us,

> hopeless to hunt for the 'right' truth-conditions for conditionals which can be used in testing the truth conditional soundness of inferences involving such propositions. . . .
>
> If the conditional probability measure for conditionals' probabilities is correct, and given other standard assumptions of probability theory, there is no way of attaching dichotomous truth values to conditionals in such a way that their probabilities will equal their probabilities of being true (Adams, 1975 p. 5, stress removed).

Adams presents a formal proof to this effect, which has been helpfully adapted by Carlstrom and Hill in their review of The Logic of Conditionals.[34] By showing that there exist two assignments of

probabilities to worlds and of truth values to statements p and q at those worlds such that the probabilities of $(p \rightarrow q)$ being true differ from each other under the two assignments whilst $Pr(q/p)$ does not so differ, they demonstrate that the conditional probabilities asociated with the indicative conditional cannot be probabilities of its truth (Appendix, b).

In another review, Edgington offers a more informal discussion of the same point.[35] Like Adams, she draws our attention to the fact that we can use areas of a Venn diagram to represent classes of states of affairs in which simple and truth-functionally compounded statements are true.

> If areas are made proportional to probabilities, the probabilistic relations between these propositions can also be read off. But $P(A \rightarrow B)$ is a <u>ratio</u>: the ratio of a probability that a state of affairs obtains in which 'A&B' is true, to the probability that one obtains in which 'A' is true. <u>There is no such thing as the class of state of affairs in which 'A→B' is true</u> (Edgington, p. 621, last stress added).

Conditionals, Edgington tells us, are not the <u>sorts</u> of things that can be true or false.

> Some conditionals may have probabilities 1 or 0, but there is no way of assigning dichotomous truth values to all conditionals (Edgington, p. 621).

But this finding has consequences that go beyond the interpretation of inscriptions like $Pr(p \rightarrow q)$. If we cannot assign probabilities of truth to $(p \rightarrow q)$, we cannot assign probabilities of truth to statements compounded from such conditionals, as, for example, $-(p \rightarrow q)$, $((p \rightarrow q) \vee ((q \rightarrow r))$, $(p \rightarrow (q \rightarrow r))$ and so on. Thus, as Lewis points out, if we nevertheless

> continue to permit unrestricted compounding of sentences by means of the usual connectives, so that the domain of our probability functions will be a Boolean algebra (as is standardly required) . . . we can no longer assume that these connectives always have their usual truth-functional interpretations, since truth-functional

> compounding of non-truth-valued sentences makes no sense.[36]

Even Edgington, who is sympathetic to Adams' project, considers this an area for further work (Edgington, p. 622).

> While '(A→B)&(C→D)' can be treated as a double assertion, and '(A→(B→C))' can usually be acceptably paraphrased '((A&B)→C)', there is no obvious treatment of (i) '-(A→B)', (ii) '(A→B)∨(C→D)' or (iii) '(A→B)→C)' . . . . Yet, with (i) and (ii), it is obvious what it is to deny that a conditional is probable (i.e. that a ratio of probabilities is high), or to say that either this or that ratio is high (Edgington, p. 621).

Edgington's suggestion here is that

> It may be necessary to ascend from merely expressing probabilities, to making statements about the probabilities expressed, to reinstate truth values and give a sense to negation and disjunction. . . . (Edgington, p. 622).

In the case of (iii), she notes, this will be a challenging project.

Lewis, on the other hand, pursues the possibility of giving up on talk of truth entirely. It might seem, he observes, that we could still develop a semantics without reference to truth were we willing to

> replace mention of equivalence, incompatibility, and necessity. . . by mention of . . .[their syntactic substitutes]: inter-deducibility, deductive inconsistency, and deducibility. . . .
>
> In this way we could describe the probability functions for our language without assuming that all probabilities of sentences, or even any of them, are probabilities of truth. We could still hold that assertability goes in most cases by probability, though we could no longer restate this as a rule that speakers should try to tell the truth (Lewis, pp. 135-6).

Even this alternative turns out to be unavailable, however. Lewis' own proofs show that given standard assumptions about

probabilities, probabilities of conditionals cannot be conditional probabilities for anything like a natural language. Lewis has shown that if, respecting the usual axioms of probability, we treat '→' as a non-truth functional connective such that Pr(p→q) = Pr(q/p) when Pr(p) is greater than 0, it will be the case that

(1) whenever Pr(p&q) and Pr(p&-q) are both positive, p and q will be probabilistically independent;

(2) there cannot be as many as three pairwise incompatible contingent statements in the language for which '→' is a connective (Appendix, c). Any such language, Lewis suggests, deserves to be characterized as trivial (Lewis, p. 132). In addition,

(3) the probability function in question can, at most, be four-valued, and thus is what Lewis calls a trivial probability function (Lewis, p. 134).

We are thus left with nothing but a truly drastic alternative to ET: indicative conditionals can only be thought of a

> non-truth-valued sentences, governed by a special rule of assertability that does not involve their nonexistent probabilities of truth (Lewis, p. 136).

Adams is undeterred by these consequences.

> [My] very tentative opinion on the 'right way out' of the triviality argument is that we should regard the inapplicability of probability to compounds of conditionals as a fundamental limitation of probability, on a par with the inapplicability of truth to simple conditionals. What is needed at the present stage is less mathematical theorizing than close examination of the phenomena of inference involving these problematic constructions (Adams, 1975, p. 35).

Adams concludes with the conjecture that

> an adequate theory will ultimately require just as radical a departure from the probabilistic 'conceptual scheme' as this scheme is itself radically different from the orthodox truth-conditional viewpoint (Adams, 1975, p. 35).

The defense of ET against Adams, then, is a game for high stakes

according to participants on both sides of the debate. It is not surprising that attempts have been made to neutralize this challenge. Two recent essays in this direction are David Lewis' quantified conversationalist approach and what Frank Jackson calls SET--ET in company with a pragmatic theory which goes beyond conversationalist doctrine. It is to these that we now turn our attention.

Chapter 5.

Lewis' Quantified Conversational Approach and Jackson's SET

Both David Lewis and Frank Jackson offer defenses of ET which, they believe, can accommodate Adams' observations about the conditions under which (p→q) is assertable. They are both pragmatic defenses, in the sense that they invoke speakers' and hearers' knowledge of what is implicated as well as what is said in order to explain discrepancies between intuitions about truth and validity of inference, on the one hand, and the deliverances of formal logic under the assumption of ET, on the other. It is the presumed deficiencies of these two theories in particular which lead Appiah to conclude that ET is an untenable thesis. Let us consider each of them in turn, therefore, before taking up Appiah's reasons for rejecting them.

First, then, Lewis thinks he can explain why the assertability of (p→q) tracks the conditional probability (q/p) within a Gricean framework. What he is proposing, he tells us, is a <u>quantified</u> conversationalist theory. On a Gricean account, the assertability of (p→q) can be eroded by either one of two factors. First, the assertability of (p→q) is diminished to the extent that (p⊃q) is unlikely to be true. Secondly, the assertability of (p→q) is diminished to the extent that our grounds for believing that (p⊃q) are purely truth-conditional. (It will be recalled that one of the generalized conversational implicatures of (p→q) is the existence of <u>non</u>-truth-functional grounds for believing that (p⊃q).) In particular, Lewis tells us, assertability is lessened to the extent that our grounds for believing that (p⊃q) consist very largely in our belief that -p. Let us call truth on account of a false antecedent 'vacuous truth' and the probability that a conditional has a false antecedent the probability of its vacuity. Let us refer to the probability that the antecedent of a conditional is <u>not</u> false the probability of its non-vacuity.

The assertability of (p→q) should thus be eroded to the extent that

1) the probability of vacuity, Pr(-p), is high, and,
2) the probability of falsity, Pr(p&-q), is a large fraction

> of the total probability of non-vacuity, Pr(p)--which is to say, when the ratio Pr(p&-q)/Pr(p) is high.

Lewis thinks that the product of these two factors is an appropriate measure of dimunition of assertability (LC, p. 137). Subtracted from the probability of (p⊃q), then, this product yields the required resultant measure of the assertability of (p→q). But, Lewis notes, making conventional assumptions about probability and simplifying, we find that

$$Pr(p \supset q) - [Pr(-p)Pr(p\&-q)/Pr(p)] = Pr(q/p).$$

And <u>that</u>, Lewis tells us, is why assertability goes by conditional probability (Lewis, p. 137).

In favor of his account, Lewis points out that diminished assertability due to misleading pointlessness is not an ad hoc property conjured up solely to take care of problems with regard to the assertability of conditionals but makes sense in the case of negated conjunctions as well. It is not, for example, appropriate to say dramatically of a coveted but perfectly edible mushroom 'You won't eat that one and live!' despite the high probability of truth of the statement.

> Its assertability goes not just by probability but by the resultant of that and a correction term to take account of the pointlessness and misleadingness of denying a conjunction when one believes it false predominantly because of disbelieving one conjunct (Lewis, p. 138).

Our unease in the face of such a statement does not tempt us to hypothesize some special non-truth-functional rule for negated conjunctions, says Lewis, nor should we be so inclined in the case of similarly unappealling indicative conditionals. In either case, what we assert can be true while its assertion is illicitly tricky and, in either case, this will quite predictably result in a highly unassertable statement.

Lewis' quantified conversationalist proposal may be thought of as a neo-Gricean response to Adams. Frank Jackson has more recently suggested that reliance on conversational maxims alone is insufficient when we attempt to account for the assertability of various sentences

of ordinary English. He proposes what he calls a supplemented Equivalence thesis or SET--an augmented pragmatic theory which, he thinks, gives a better explanation of discrepancies between linguistic intuition and the verdicts of logic under the assumption of ET. It is probably worth noting that what is supplemented in Jackson's scheme is not ET itself, which is still the claim that the truth-conditions of $(p \rightarrow q)$ are just those of $(p \supset q)$, but, rather, the pragmatic theory which is invoked to explain what might otherwise seem to be discrepant judgments about truth and validity of inference.

Jackson starts from the assumption that there is a prima facie case for ET.

> The circumstances in which it is natural to assert the ordinary indicative conditional 'If P then Q' are those in which it is natural to assert 'Either not P or, P and Q', and conversely. . . [T]he circumstances in which it is natural to assert 'Not both P and Q' are precisely those in which it is natural to assert 'Either not P or not Q'. We explain the latter coincidence of assertion conditions by a coincidence of truth conditions. Why not . . . hold that 'If P then Q' has the same truth conditions as 'Either not P or, P and Q'. . . [which]--given the standard and widely accepted truth functional treatments of 'not', 'or', and 'and'--amounts to . . . the thesis that $(P \rightarrow Q)$ is equivalent to $(P \supset Q)$ (Jackson, p. 565).

What we need, says Jackson, is an explanation of deviations from expectations based on this very plausible hypothesis. His strategy in providing such an explanation follows the general outlines of the pragmatic defenses at which we have already looked. Like Grice and Lewis, Jackson invokes a distinction between truth and assertability and, like Grice and Lewis, he derives rules of assertability from a functional analysis of discourse. Unlike Lewis, however, Jackson takes pains to differentiate his approach from what has become the standard Gricean strategy--a strategy which, he thinks, has disabling difficulties. Jackson believes that propriety of assertion cannot be explained solely by reference to conversational considerations of " high

probability, relevance, informativeness, and so on" (Jackson, p. 573). He tells us that he hopes to fill a gap in the standard way of trying to explain away the so-called paradoxes by taking into account an additional functional demand on discourse (Jackson, pp. 368-369). Jackson also prefers to replace talk of conversational implicature by reference to what is conventionally <u>indicated</u> or <u>signalled</u> (Jackson, p. 574).

Let me try to lay out Jackson's arguments as clearly as I can. Why, to begin with, does Jackson think that the usual conversationalist explanation will not do? He offers two quite different objections. The first has to do with the inadequacy of the standard account in predicting the assertability of various sorts of ordinary sentences, including indicative conditionals. The second criticism has to do with the inadequacy of the standard account in explaining our metalinguistic intuitions with regard to relationships between ordinary language expressions and logical formalisms. In neither case, says Jackson, can we make do with conversational maxims alone.

Let us take up the two objections in turn. Jackson points out that the standard conversationalist explanation of the so-called paradoxes of assertability invokes only two sorts of conversational considerations. We are not to say things which will lead our listeners astray and, within that limitation, we are to offer as much relevant information as possible. If this is really all that matters, Jackson notes, we should be able to predict when a relevant and adequately probable statement will be assertable by reference to the following simple rule:

> (S) Assert the [logically] stronger instead of the [logically] weaker (Jackson, pp. 566-67).

But, says Jackson, this rule does <u>not</u> reliably predict assertability. His suggestion is that it requires supplementation rather than replacement.

Jackson offers examples of a number of such failures of prediction. If (S) fully specifies the conditions for assertability of relevant and adequately probable statements (a proviso we shall simply understand from here on), we should, for example, never find $(p \rightarrow q)$ assertable when $-p$ is highly probable: both statements have just about

the same probability and -p is the logically stronger of the two. Nevertheless, as Jackson points out,

> (E) If the sun goes out of existence in ten minutes time, the earth will be plunged into darkness in about eighteen minutes time

is highly assertable.

Similarly, if (S) tells us everything we need to know about assertability, it should never be the case that (p→q) be assertable on the strength of the very high probability of q alone. Nevertheless, Jackson says, a speaker convinced that Carter will be reelected no matter who his opponent may be is entitled to say both 'If Reagan runs, Carter will be reelected' and 'If Reagan does not run, Carter will be reelected'.[37]

A third failure of (S) to predict assertability occurs when we consider that statements which are logically equivalent (and thus bound to have the same probability of truth) may not be equally assertable. Any friend of ET believes that

> ((-P & (P→R)) and (-P & (P→S)) are logically equivalent, both being equivalent to -P. But their assertability can differ sharply. 'The sun will come up tomorrow but if it doesn't, it won't matter' is highly unassertable, while 'The sun will come up tomorrow but if it doesn't, that will be the end of the world' is highly assertable (Jackson, p. 568).

In particular, all truths of logic are logically equivalent and their probability of truth is always equal to 1 yet some of them seem to be assertable under at least some circumstances--'Que sera, sera'--whereas others seem quite universally unassertable. That truths of logic are weak as can be does not help us figure out why some of them are assertable and others are not, says Jackson.[38]

Jackson's first objection, therefore, is that the standard conversationalist account does not have the resources to make reliable predictions about assertability.

Jackson's second objection is of a quite different order. On any purely conversationalist account, Jackson tells us, the relationship between the truth of p and the assertability of 'p or q', on the one

hand, must be the same as the relationship between the truth of p and the assertability of 'If q then p', on the other. The very same conversational maxims bridge the gap between truth and assertability in either case. That the truth of p entitles us to conclude to the truth of 'p or q' but does not underwrite its assertability should thus be just as much or as little puzzling to a competent speaker as that the truth of p entitles us to conclude to 'If q then p' without thereby rendering <u>this</u> an appropriate assertion. Our linguistic intuitions should thus be thoroughly symmetric with regard to the two cases. Yet we accept with equinamity the equivalence of 'p or q' and 'p∨q' while we choke on ET. A consideration of conversational propriety alone, Jackson concludes, leaves it a mystery why we--who are after all reasonably normal language users--find it so easy to swallow one thesis and so hard to swallow the other (Jackson, p. 569).[39]

In both of these regards, Jackson finds the standard strategy for dealing with the so-called paradoxes inadequate. Why, then, might we sometimes be justified in asserting the logically weaker of two relevant and more or less equiprobable statements? Jackson's answer is that we often also take into account what he terms the <u>robustness</u> of a claim--the extent to which its probability of truth is high and undiminished by receipt of <u>other</u> news. In particular,

> If we accept Conditionalization, the plausible thesis that the impact of new information is given by the relevant conditional probability, then 'P is robust with respect to I' will be true just when both Pr(P) and Pr(P/I) are close and high (Jackson, p. 569).

Jackson takes such robustness to be an important determinant of assertability. Thus, for example, Lewis' 'You won't eat that mushroom and live' is unassertable, Jackson thinks, not because it is misleading to field a negated conjunction on the strength of the high probability of falsity of one conjunct alone (whose negation could thus have been proffered <u>tout court</u>) but, rather, because of a failure of robustness.

> You take me to be providing information relevant to mushroom-eating pleasures, and so construct for yourself the following piece of practical reasoning. I won't eat that one and live. (Premise supplied by me.) I eat that

> one. (Premise you can make true.) Therefore, I won't live. The conclusion is undesirable, hence you are led to refrain from making the second premise true.
>
> Why were you tricked? The argument is valid, the premise I supplied does have a high probability, and you are able to give the second premise a high probability. But in order to infer the conclusion of a valid argument all premises need to be highly probable <u>together</u>; and if you were to make the second premise highly probable, the first premise (supplied by me) would no longer be highly probable. In the circumstances you were entitled to take it that not only was 'You won't eat that one and live' highly probable, [but that] it was also robust with respect to 'You eat that one'. My misdeed lay in asserting something lacking appropriate robustness (Jackson, p. 572-573).

The difference between a disjunction which is assertable and one which is not may, similarly, be a matter of robustness. If I read in the departmental newsletter that, just as I had expected, Mary won the prize and Margaret did not, it is improper for me to answer a query with (M) 'Mary or Margaret won the prize'. If, however, still believing, as before, that Mary is a shoo-in and Margaret a very unlikely candidate and knowing that they are the only two competitors whose names begin with 'M', I catch a glimpse of a memo on the Dean's desk just long enough to see that the winner's name begins with 'M', I may, it seems, appropriately offer (M). But if 'Assert the stronger' makes (M) unassertable in the first case, why not in the second? Surely, by my lights, (M) is still only marginally more probable than 'Mary won' and it is logically weaker. Jackson argues that it is the greater robustness of (M) in the second case that makes it assertable: were it to turn out that Mary did not win the prize, I would, in the first case, have to abandon the entire disjunction but not in the second case. I would merely have to readjust my beliefs about Margaret (or the selection committee). Jackson tells us that

> In general we are happiest asserting disjunctions which are two-sidedly robust. We most happily

> assert 'P or Q' when Pr(P∨Q), Pr(P∨Q/-P), and Pr(P∨Q/-Q) are all high (Jackson, p. 575).

Jackson's first point, thus, is that robustness is a factor in determining assertability. His next point is that there will have to be indications of what might be called the direction of robustness.

> Robustness is a relative affair. A highly probable sentence may be very robust relative to one possible piece of information and the opposite relative to another (Jackson, p. 573).

Jackson thinks that there are both implicit, contextual cues to the direction of robustness and explicit conventional signals. In the case of the mushroom gathering story, for example, situational factors suggest to us the sort of reasoning likely to be going on and this, in turn, determines the direction of the requisite robustness. We can also find cases in which information about robustness is explicitly signalled by a word or phrase. For example, 'nevertheless' conventionally signals the robustness of a claim with respect to what to what has immediately preceded it. Similarly, the use of 'p or anyway q' conventionally indicates what Jackson terms the merely one-sided robustness of a disjunction. We fly this flag, Jackson says, to avoid misleading our hearers into assuming the usual two-sided robustness (Jackson, p. 575).

Jackson is careful to note that such conventional signalling does not invade truth conditions (Jackson, p. 576). The truth-conditions of 'p or anyway q', he tells us, are just those of 'p∨q': " 'George lives in Boston or anyway somewhere in New England' is true if and only if either 'George lives in Boston' is true or 'George lives somewhere in New England' is true " (Jackson, p. 575).

Of most interest to us here is Jackson's claim that certain syntactic constructions also conventionally convey information about robustness and its direction. We have already seen that he suggests that using a disjunctive form not only conveys the adequate probability of the disjunction itself but also (unless cancelled) its robustness with respect to the negation of each disjunct taken separately. Jackson's third major claim, then, is the specific hypothesis that

> the indicative conditional construction . . . signals robustness with respect to its antecedent. Hence it is

> proper to assert (P→Q) when (P⊃Q) is highly probable and robust with respect to P, that is, when Pr(P⊃Q/P) is also high. But, by analogy with . . . '--or anyway--', the truth conditions of (P→Q) are those of (P⊃Q). . . .
>
> In the widest sense of 'meaning', (P→Q) and (P⊃Q) do not mean the same. But <u>their truth conditions are the same</u>--they agree in sense or literal content. The extra element is that in using (P→Q) you explicitly signal the robustness of (P⊃Q) with respect to P, and this element affects assertion conditions without affecting truth conditions (Jackson, pp. 576-577, my stress).

We have such a conventional signalling device, Jackson tells us, because it is likely to be of interest to us whether a conditional is robust with regard to its antecedent--this is a requirement for a modus ponens argument to go through.

> Although (P⊃Q), P, [therefore] Q is certainly valid, there is a difficulty about using it in practice. Suppose my evidence makes (P⊃Q) highly probable but that I have no evidence concerning P. Q is of interest to me, so I set about finding evidence for P if I can. The difficulty is that finding evidence that makes P highly probable is not enough in itself for me to conclude Q by <u>Modus Ponens</u>. For the evidence that makes P probable may make (P⊃Q) improbable. . . [W]e must distingush the <u>validity</u> of <u>Modus Ponens</u> from its <u>utility</u> in a situation where I know (P⊃Q) but do not know P (Jackson, p. 577).

Why should we be persuaded by this proposal that the ordinary indicative conditional construction is, in particular, a conventional indication of the robustness of the conditional with regard to the truth of its antecedent? Jackson argues that his theory <u>explains</u> relevant observations about competent speakers' judgments. By this he means that it predicts speakers' intuitions both about assertability and about validity of inference.

The first set of observations to which he draws our attention concerns the circumstances under which we are comfortable asserting

ordinary indicative conditionals.

> Ernest Adams has provided a simple formula governing our intuitions and the Supplemented Equivalence theory explains this formula. Adams has shown that the (intuitively justified) assertability of $(P \rightarrow Q)$ is given by $Pr(Q/P) =_{df} Pr(PQ)/Pr(P)$. . . We explain Adams' thesis as follows. On our theory, the assertability of $(P \rightarrow Q)$ will be the product of two factors: the extent to which $Pr(P \supset Q)$ is high and the extent to which $(P \supset Q)$ is robust with respect to P. But we have from the calculus [of probability] that $Pr(P \supset Q/P) = Pr(Q/P)$, and that $Pr(P \supset Q) > Pr(Q/P)$. Consequently both conditions are satisfied to the extent that $Pr(Q/P)$ is high Q.E.D. (Jackson, p. 580)(Appendix, d).

Jackson also thinks that his theory can go beyond the usual conversationalist dissolution of what we might think of as the paradox of dissent from conditionals. We have observed earlier that disagreement with $(p \rightarrow q)$ is not normally taken as committing the speaker to p and -q as ET would seem to dictate and that the standard conversationalist account of this is that the speaker's dissent is from what $(p \rightarrow q)$ implicates rather than from what it says. But, as Jackson points out, this leaves dissent from conditionals an inexplicably special case.

> Standardly you dissent from an assertion just when its subjective probability of falsity is high . . . The probable falsity of what may be signalled by an assertion is by and large irrelevant. You dissent from 'He is poor but happy' just when it is probable that he is either not poor or not happy, not when you dissent from the signalled contrast (Jackson, p. 586).

Why should conditionals be different? Jackson suggests that there is, in fact, a general rule governing circumstances under which dissent is appropriately directed at what is signalled rather than at what is said and that $(p \rightarrow q)$ falls under this proviso. In general, Jackson tells us, if S is a statement and C is what is indicated or conveyed (but not said) by asserting S, then when the truth of C is sufficient

for the truth or high probability of S, dissent from S will be taken to be an objection to C rather than a denial of S.

Jackson offers a non-conditional example. Consider the case

> when I say 'The winner of the election for club president will come from Tom, Dick and Harry'. What I say counts as true if any one of these three wins. But you won't dissent only if you think this improbable . . .[but also] because I left out George, and in your view George has the best chance after Tom. . . .
>
> The explanation . . . appears to lie in the peculiarly intimate relationship that obtains . . . between what is said and what is signalled. . . .[W]hat is signalled is sufficient for the high probability of what is said. In saying that the election is out of Tom, Dick and Harry, I signal that the high probability for me of the triple disjunction is robust with respect to the conjunction of the negations of any two of the disjuncts. . . .This is sufficient (by the calculus) for the high probability of the disjunction (Jackson, p. 587).

If the general rule Jackson proposes is correct--if the existence of a close connection between the truth of C and the truth or high probability of S makes it appropriate to take dissent from S as an objection to its associated implicature, C,--then dissent from $(p \rightarrow q)$ will standardly be heard as an objection to what it conveys rather than what it says because

> What is signalled by the assertion of $(P \supset Q)$ amounts to $Pr(Q/P)$ being high. This is sufficient for $Pr(P \supset Q)$ being high. So what is signalled is sufficient for the high probability of what is literally said. . . [and] dissent from $(P \rightarrow Q)$ may be prompted by the dissenter giving a low value to $Pr(Q/P)$ as much as by his giving a low value to $Pr(P \supset Q)$ (Jackson, p. 587).

In addition, Jackson thinks his theory explains our discomfort in asserting '$(p \rightarrow q)$ and $(p \rightarrow \neg q)$' even though $(p \supset q)$ and $(p \supset \neg q)$ can be true together: if he is correct, we cannot appropriately assert $(p \rightarrow q)$ unless

Pr(q/p) is adequately high and we cannot appropriately assert (p↛¬q) unless Pr(¬q/p) is adequately high but the axioms of probability preclude their being adequately high together.

Finally, Jackson invokes his proposal to explain why dissent from (p→q) is often expressed as the assertion of (p↛¬q). We may dissent from (p→q) either because Pr(p⊃q) is low or because Pr(q/p) is low. If Pr(p⊃q) is low, Pr(q/p)) will also be low. Hence, whenever we dissent from (p→q), Pr(q/p) is low. This, in turn, means that whenever we dissent from (p→q), Pr(p⊃¬q) and Pr(¬q/p) are high and thus that (p↛¬q) is assertable. And, says Jackson,

> this is just how it turns out in practice. If you dissent from 'If Fred went, he went by car,' you assent to 'If Fred went, he did not go by car' (Jackson, p. 588).

One strength of Jackson's pragmatic theory of assertability, then, is his ability to explain patterns of assent and dissent associated with conditionals in cases where they depart from expectations based on ET and standard maxims of conversation alone.

It will be recalled, however, that attacks on ET have involved not only intuitions about the assertability of particular conditionals under various circumstances but also the judgments of competent speakers with regard to the acceptability of arguments in which they occur. Adams' challenge, in particular, arose initially from his concern over such things as the so-called paradoxes of material implication. Jackson thus sets himself to coping with this family of puzzles as well.

What, for example, can be made of the notorious lack of appeal of the arguments from either ¬p or q to (p→q)? It is, says Jackson, easy enough to explain:

> Neither the fact that Pr(¬P) is high nor the fact that Pr(Q) is high is sufficient for Pr(P⊃Q/P) being high (Jackson, p. 581).

Jackson also turns his attention to other argument forms which are truth-conditionally valid but notoriously hard on the nerves. What, for example, is to be made of supposed failures of contraposition?

> The problem is not that it [contraposition] seems

> invalid stated in symbols; exactly the reverse is the case, as is evinced by its appearance in Natural Deduction systems. The problem is rather a certain class of apparent counterexamples like: 'If George works hard, he will (still) fail; therefore, if he passes, he won't have worked hard', and 'If Carter is reelected, it won't be by a large margin; therefore if Carter is reelected by a large margin, he won't be reelected' (Jackson, p. 581).

We are uneasy with these arguments, Jackson tells us, not because what was said fails to survive contraposition but, rather, because what was signalled fails to do so. Our linguistic intuitions, it would seem, are not discriminating enough to respond to this difference: we get the same sort of jolt from an argument that carries us from an assertable premise to an (adequately probable) but unassertable conclusion as we do from one that carries us from adequately probable premises to an inadequately probable conclusion. In support of this interpretation, Jackson offers two sorts of observations.

First, he cites instances where what is implicated by a locution fails to survive transformation into a logical equivalent without our feeling that this renders dubious a straightforward truth-functional interpretation of the ordinary language connective under consideration.

> These apparent counterexamples [against contraposition of $(p \rightarrow q)$] are paralleled by ones against the commutativity of '--or anyway--': for instance, 'It won't rain or anyway not heavily; therefore, it won't rain heavily or anyway it won't rain'. Despite this, . . .[we] give the same truth-conditions to 'P or anyway Q' as are standardly given to 'P or Q' (Jackson, p. 582).

Similarly, despite the fact that neither 'or at least' nor 'nevertheless' bears up well under commutation, we do not hesitate to assign standard truth-functional meanings to them as connectives.

Jackson also offers a second sort of evidence in support of his pragmatic dissolution of so-called failures of transposition. He points out that it is just the predicted pattern of probabilistically

appropriate signalling in the premise and probabilistically inappropriate signalling in the conclusion which seems to characterize problem cases in inferences involving conditionals.

> Putative counterexamples to Contraposition are all ones where $Pr(p \supset q/p) = Pr(q/p)$ is high, [making $(p \rightarrow q)$ assertable,] and $Pr(-q \supset -p/-q) = Pr(-p/-q)$ is low [making $(-p \not\rightarrow -q)$ unassertable].
>
> For example, the probability of Carter not being reelected by a large margin given he is reelected may be high when the probability of Carter not being reelected given he is reelected by a large margin is minimal [!]. Accordingly, we can explain our reluctance to assert 'If Carter is reelected by a large margin, then Carter will not be reelected' even when we are happy to assert 'If Carter is reelected, then it will not be by a large margin' in terms, not of the first being false and the second true, but in terms of what is signalled by saying the first being false and what is signalled by saying the second being true (Jackson, p. 582).[40]

Jackson has thus supported his proposal by evidence that it explains speakers' intuitions not only with regard to the assertability of particular conditionals but also with respect to the validity of patterns of inference and preservation of extrasystematic analogues of various logical properties. In each case, he believes he has dealt with discrepancies between such judgments and the deliverances of formal logic under the assumption of ET with greater success than could an orthodox conversationalist.

Both Lewis' and Jackson's proposals are attempts to accommodate Adams' persuasive thesis that the assertability of $(p \rightarrow q)$ tracks $Pr(q/p)$ without giving up ET. Appiah is unpersuaded by either of them. Their shortcomings, he believes, warrant abandoning the search for a truth-conditional semantics. In the following chapter we shall look at his objections.

Chapter 6.
Appiah's Criticisms

Before we pick up Appiah's criticisms of Lewis and Jackson it will, I think, be useful to review briefly the context in which I have suggested that they be considered. If I am correct, this is how things stand. We are considering the claim that the ordinary English indicative conditional, $(p \rightarrow q)$, has the same truth conditions as does the material conditional, $(p \supset q)$. It counts toward our endorsement of ET that, in company with some auxiliary theoretical considerations, it enables us successfully to construe the inferential practices and beliefs of competent language users. This, in turn, is simply to say that if such an ensemble is sufficiently helpful in making sense of the phenomena of interest, we may reasonably say that the truth conditions of the ordinary English conditional <u>are</u> those of $(p \supset q)$. (What else, after all, could be meant by such a claim?) To review this is, of course, just to remind ourselves that every term is a theoretical term, even the familiar predicates of everyday life, and that theoretical claims do not meet the world and the demands of logic singly but in company.

We may remain neutral, if we wish, as between two accounts of precisely how overall theoretical adequacy is related to the truth of individual claims: that it is so related follows either from a purely pragmatic view of truth, on which the two are taken to be the same thing, or from a correspondence view of truth which allows that goodness of theory provides us with evidence for the truth of its tenets. In either case, what we are we are engaged in is the evaluation of a theoretical framework which includes ET and, in particular, some auxiliary pragmatic theory, in a context of phenomena more or less successfully construed within that framework.

Such evaluation, in turn, requires invocation of canons of rational theory choice. Although we are (to put it mildly) in no position to lay these out exhaustively, some generally acknowledged candidates include the past track record of the theory in accounting for related phenomena, its conceptual economy, its cotenability with other well-regarded theories in the domain of interest and its promise

with regard to providing solutions to standing puzzles. Observations which are inexplicably inconsistent with theoretical prediction and/or reliance upon clobbered-together 'ad hoc' explanations of such anomolous cases are generally taken to count against a theory. Last, but by no means least, it is often urged that the relative attractiveness of competing theories in these regards also count in such decisions.

It is, I think, useful to keep this in mind as we look at Anthony Appiah's criticisms of Lewis' and Jackson's attempts to provide such a theoretical context for ET. For one thing, this way of looking at what is going on allows us to bridge the gap between Appiah's catalogue of supposed deficiencies in Lewis' and Jackson's work, on the one hand, and his concluding counsel that we bite the bullet with a good grace, on the other. It is only by assuming that Appiah is inviting us to assess the relative merits of two competing research programs that his criticisms could reasonably be seen as relevant to such a conclusion. This becomes apparent when we note what kind of argument Appiah is clearly <u>not</u> attempting to establish. He is not, for example, attempting to offer us a demonstration that pragmatic theory is, in principle, doomed to failure (in the way in which, for example, Chomsky's followers set themselves to demonstrate the utter impossibility of accounting for language acquisition with any finite state theory or in the way in which ethicists have argued against the possibility of deriving a theory of value from purely naturalistic considerations). Criticisms of this sort--arguments against the very possibility of success of some whole category of theory--lead in an instantly recognizable way to calls to abandon ship. Appiah does not offer this sort of argument, however. The route to his conclusion must, therefore, be a different one. It would be uncharitable to suppose that he envisages himself as persuading us with the induction that a pragmatic strategy didn't work for Grice nor for Lewis nor for Jackson so it's not going to work for anybody; more plausibly, we may suppose that Appiah is simply asking us to choose between Adams' program and that of the pragmatic defenders of ET on much the same grounds that we might be asked to choose between any two competing research programs.[41]

With this in mind, we may proceed to look at the reasons Appiah offers for urging rejection of the pragmatist program and then go on to ask whether he has been persuasive on this score.

Let us look first at Appiah's criticisms of Lewis. They fall into two main parts. Lewis, it will be recalled, wishes to show that the difference between the probability associated with $(p \rightarrow q)$ on the assumption of ET and the conditional probability which gives a measure of the assertability of $(p \rightarrow q)$ can be derived from Gricean conversationalist considerations. Lewis proposes that this difference --expressed as $Pr(-p)Pr(-q/p)$--represents a measure of dimunition in assertability due to the misleading pointlessness of conditionals whose probability of truth is very largely due to their almost-sure-to-be-false antecedents. Appiah argues, first, that Lewis' derivation of this difference from conversationalist considerations is unsatisfactory and, second, that even if it were,

> it would not provide a satisfactory explanation in terms of conversational implicature and consistent with the view that conditionals are material conditionals, of the fact that the conditional has the odd assertibility rule [Adams proposes] (Appiah, 1983, p. 29).

Appiah's argument against Lewis' derivation is a complicated one. These are, I think, its major themes. First, Appiah finds in Lewis' text two different characterizations of the conditions which ought to lessen the assertability of $(p \rightarrow q)$.

> On one hand he suggests that we ought not to assert the conditional $(A \supset C)$ if
> a) it is probable <u>but</u>
> b) its probability of truth consists mostly of its probability of vacuous truth.
> (Call this the first approach).
> On the other, he suggests that we ought not to assert it if
> a) it is probable <u>and</u>
> b) the probability $P(-A)$ of vacuity is high. . .and
> . . .the probability $P(-C\&A)$ of falsity is a large fraction of the probability $P(A)$ of non-vacuity.
> (Call this the second approach) (Appiah, 1983 p.29,

Appiah's ellipses).

Appiah argues that these two characterizations lead us to demonstrably different results. Second, Appiah notes that at least one plausible representation of the dimunition of assertability called for by the first of Lewis' readings of Gricean doctrine does not yield the required difference but, rather, a factor which can be shown to be unequal to it in all of the cases of interest to us. Thirdly, Appiah thinks that for the correction factor which Lewis offers to be capable of representing the relevant Gricean considerations, Lewis must endorse an untenable combination of views. Finally, Appiah thinks that there is pervasive difficulty with Lewis' notion of what it means to say that an agent believes S predominantly because of believing T.

Let me try to prune this a bit.

First, although it takes up considerable of Appiah's attention, I shall set aside the question of what the proper analysis of the notion of believing S predominantly because you believe T is (Appiah, 1983, p. 34). It is my impression that explication of Appiah's notion of analysis would take us a good deal further afield than we want to go. Moreover, whatever the results which Appiah seeks, the suggestion that our assessment of a theoretical program must wait on getting straight about our concepts of causality and explanation seems well worth resisting.

Secondly, I shall not pursue the question of whether Lewis really considered the second approach to be an explication of the first in the sense of being logically equivalent to it or whether (as seems more likely to me) what Appiah takes to be a second approach is simply Lewis' elaboration of what he said to begin with. Since the correction factor which Lewis finally presents includes the term $Pr(-q/p)$, I shall assume that he was not wedded to a pure vacuity version of the conditions which diminish assertability. (This seems the more likely because the maxim of quality is so central to Gricean doctrine that ignoring the question of falsity would seem curiously uncharacteristic of any worker within the tradition.)

Appiah's most interesting objection, it seems to me, concerns Lewis' right to the derivation of the correction factor which he actually proposes--the one that closes the gap between $Pr(p \supset q)$ and

Pr(q/p).

Appiah tells us that the

> intuitive Gricean idea of what is wrong with uttering a material conditional whose antecedent is false is [1] that the conditional is not only vacuous . . . but [2] that if it were <u>not</u> vacuous it would not be true (Appiah, 1983, p. 32, my stress).

The first of these considerations gives us the first factor in the correction--Pr(-p)--and to this Appiah raises no objection. What bothers him is how Lewis gets to represent [2], the fact that if (p⊃q) were not vacuous it would be false, by way of the second factor in the correction--Pr(-q/p), which is to say, by the ratio Pr(-q&p)/Pr(p).

> There is now an obvious question: why should the fact that my P(-C&A)/P(A) is large show that I think the material conditional would not be true if its antecedent were true?
>
> The answer must surely be that this ratio is equivalent to P(-(A⊃C)/A); and the reason that <u>this</u> captures the idea that the conditional would be disbelieved if the antecedent were believed. . .[must be] that probabilities should change by conditionalization. . . .[For if they do,] the agent would come to believe that the material conditional was false if he came to believe its antecedent was true just in case P(-(A⊃C)/A) is high (Appiah, 1983, p. 33).

But, says Appiah, given Lewis' views about (p→q), he is in trouble if he tries to derive his correction factor along these lines. More precisely, if Lewis avails himself of the attractive principle that beliefs change by conditionalization <u>and</u> continues to maintain that (p→q) has the truth conditions of the material conditional, he will find himself in the position of having to come up with an explanation of our "strong intuition. . . that if someone believes that if A then C, and comes to believe A (while having no other direct evidence about C), he will come to believe C." For, says Appiah, on Lewis' account, this intuition will now be incorrect (Appiah, 1982, p. 330).

The reason for this is as follows. If the conditional is a

material conditional then a speaker can assign ever so high a probability to ($p \supset q$)--and hence to ($p \rightarrow q$)--while Pr(q/p) is ever so low. But if beliefs change by conditionalization, then coming to believe p will not lead such a speaker to assign a high probability to q. It follows, says Appiah, that on Lewis' views, "someone <u>can</u> believe $A \supset C$ and not be so disposed that if he came to have evidence that A he would come to believe that C" (Appiah, 1982, p.330). And this is a problem for Lewis, says Appiah, because our intuitions strongly resist such a possibility.

> Anyone who has tried to teach propositional calculus to an intelligent student knows that it is a hard business getting people to accept that it is all right to believe a conditional purely on the grounds that its antecedent is false or its consequent true (Appiah, 1983, p. 331).

Appiah's first objection to Lewis, then, is that his derivation of the correction factor is deeply flawed. A dimunition factor based entirely on considerations of vacuity due to falsity of the antecedent cannot, by itself, give us the required correction; a factor which can give us the required correction reflects Gricean doctrine only when it is interpreted in a way which raises major difficulties for a theorist of Lewis' persuasion.

Appiah's second criticism of Lewis is that, even were its derivation to be acceptable, the correction factor would not do what he claims. Appiah notes that Lewis is relying on something like

> (R) Do not utter a sentence S in circumstances where, for some R,
>
> (a) R entails S, and S does not entail R
>
> (b) R is relevant
>
> and (c) R is assertible (Appiah, 1983, p. 332).

This rule, however, "does too much. . . . (R) rules out not only the assertion of conditionals with vacuous antecedents, but the assertion of those with vacuous consequents also" (Appiah, 1982, p. 333). Lewis has no explanation, Appiah argues, of

> why he does not [also] introduce a factor which discounts for the unassertibility of a conditional which should arise when its consequent is believed true (for C

[also] entails (A⊃C)) (Appiah, 1983, p. 35).

Appiah thinks that this problem infects any attempt to account for the vacuity of the conditional with vacuous antecedent in terms of a form of the maxim of quantity (Appiah, 1982, p. 334). Not only does Lewis' argument fail, he thinks, but so must a whole class of conversationalist defenses.

Although Appiah does not think that Jackson's defense of ET shares this weakness, he does not find it any more successful. Jackson is not going to get into the same trouble as Lewis because he is not relying exclusively on conversational principles to take up the slack between speakers' intuitions and the deliverances of logic under the assumption of ET. As Appiah points out, Jackson proposes that the assertability rule is a <u>conventional</u> and not a conversational constraint (Appiah, 1982, p. 335). There is no demand that conditionals with vacuous consequents behave in the same way as those with vacuous antecedents: each is governed by applicable local ordinances rather than both being constrained by the same general law. Jackson's difficulty, Appiah argues, is, rather, that his assertability rule dangles free from any essential connection with the truth conditions he postulates for (p→q). Not only do the truth conditions of (p⊃q) fail to play any essential role in explaining the assertability rule of (p→q), but, worse yet, there is no reason to believe that the truth conditions of (p⊃q) even <u>are</u> those of (p→q). Appiah argues this by contrasting the case of (p→q) with that of 'p but q', a parallel suggested by Jackson.

Jackson tells us, says Appiah, that what is conventionally signalled by use of the indicative conditional construction bears the same relationship to the truth-conditions of (p⊃q) as what is conventionally conveyed by 'but' bears to the truth-conditions of '&'. Just as an appropriately high probability of the truth of (p&q) is necessary but not sufficient for the assertability of 'p but q', so an appropriately high probability of the truth of (p⊃q) is necessary but not sufficient for the assertability of (p→q). So far, so good. There is, also, however, a

> striking disanalogy between the two cases. . . .
> [T]he meaning of 'but' can be broken up, so to speak, into two independent components. It must be the case

for 'S but R' to be assertible, both that

(a) the speaker believes that S and R, and that

(b) the speaker believes there is some conflict between the fact that R and some prior expectation (Appiah, 1982, pp. 336-7).

It is perfectly possible for (a) to hold without (b) being the case and vice-versa. When we look at the ordinary English indicative conditional, however, we do not find such independence: whenever $Pr(p \supset q/p)$ is high, so is $Pr(p \supset q)$. Appiah therefore concludes

> So far as I can see, the truth conditions on Jackson's view neither give you the logic of the conditional, nor play any essential role in explaining the assertibility rule. In the case of 'but', the truth conditions seem to be essential to explaining the assertibility rule. In the absence of even this role, the truth conditions are idle machinery (Appiah, 1982, p. 337).

Jackson might contend, Appiah tells us, that truth conditions really <u>are</u> essentially involved in the assertability rule for $(p \rightarrow q)$ insofar as

> we have a use for a form of words which expresses the fact that we not only believe the material conditional, but would continue to do so if we believed its antecedent true. But the state that we thus express is <u>believing the material conditional and being disposed to continue to do so even if we believed the antecedent</u>; and it is not at all clear why we should regard the truth conditions of the sentence we use to express <u>this</u> state as those of the material conditional (Appiah, 1982, p. 337).

That there is <u>reference</u> to the truth conditions of $(p \supset q)$ in our explanation of $(p \rightarrow q)$ should not lead us to conclude that the truth conditions of $(p \supset q)$ <u>are</u> those of $(p \rightarrow q)$, says Appiah (Appiah, 1982, p. 337).

Appiah finds these criticisms sufficient to ask us to abandon

pragmatic defenses of ET and take up with Adams' proposal instead, which is to say, to abandon truth-conditional semantics generally. Ought we to accept the bid? I have already suggested that I find Appiah's pessimism with respect to the prospects of pragmatic theory unwarranted and it is to this that I shall now turn.

Let me begin with what Appiah takes to be the more damaging of his two major criticisms of the quantified conversationalist approach.

> If we cannot account for the vacuity of the conditional whose antecedent is disbelieved in a way that does not make the conditional with the vacuous consequent unacceptable also, Lewis' argument fails (Appiah, 1982, p. 334).

Lewis himself draws our attention to this issue--a fact which Appiah gets around to acknowledging in his second article. First, Lewis thinks that it is just as well that his correction factor does <u>not</u> include an additional dimunition for vacuous consequents because, as it turns out, conditionals with almost-sure-to-be-true consequents appear to be quite adequately assertable under at least some circumstances. ('I'll probably flunk, and it doesn't matter whether I study; I'll flunk if I do and I'll flunk if I don't.') Why there should be such asymmetry of assertability, however, strikes Lewis as a puzzle.

> The best I can do to account for the absence of a marked dimunition in the case of the probable consequent is to concede that considerations of conversational pointlessness are not decisive. They create only tendencies toward diminished assertability, tendencies that may or may not be conventionally reinforced. In the case of the improbable antecedent, they are strongly reinforced. In the case of the probable consequent, apparently they are not (Lewis, p. 139).

This does not satisfy Appiah, who comments

> It is not enough to say simply that conversational pointlessness is not decisive; in the absence of an account of what <u>is</u> decisive we do not know why the assertibility of the conditional goes by the conditional

probability (Appiah, 1982, p. 334, my stress).

Clearly, Lewis' case would be stronger if he could suggest the sort of considerations which might be at work to diminish the corrosive effect on assertability of almost-sure-to-be-true consequents. One possibility close at hand is a suggestion by Jackson:

> Neither the fact that Pr(-p) is high nor the fact that Pr(q) is high is sufficient for Pr(p⊃q/p) being high [as it must be for (p→q) to be assertable]. The reason our reluctance is less marked in the case of asserting (p→q) on the basis of our certainty that q [than in the case of asserting it on the basis of our certainty that -p], is that Pr(q) being high together with p and q being probabilistically independent <u>is</u> sufficient for Pr(p⊃q/p) being high (Jackson, p. 581).

That is, if Pr(q) = Pr(q/p) and Pr(q) is high, Pr(q/p) is also high. But Pr(q/p) = Pr(p⊃q/p) and, as we have seen earlier, if Pr(p⊃q/p) is high, so also is Pr(p⊃q). Hence, if Pr(q) is high and p and q are probabilistically independent, (p→q) is both highly probable and robust with regard to p, which is to say, assertable.

Are we entitled thus to augment Lewis' theory with Jackson's? I cannot see why not. Lewis obviously envisages the operation of conversational constraints as only a part of the whole story and Jackson explicitly offers his hypothesis about robustness to supplement, not to replace, Gricean doctrines of conversational implicature. Why could not both sorts of constraints be at work simultaneously, producing a complex surface texture of differences in assertability? Moreover, I shall be suggesting shortly that there is a very natural way of seeing Jackson and Lewis as presenting complementary aspects of a generalized Gricean doctrine. I tend, therefore, to side with Lewis in believing that his inability to explain the asymmetry of assertability between conditionals with almost-sure-to-be-false antecedents and those with almost-sure-to-be-true consequents by way of (R) alone is not nearly so fatal a flaw as Appiah takes it to be.

What of Appiah's other major criticism, that Lewis cannot, without embarassment, give a properly Gricean gloss to the second component of

his correction factor? Appiah's criticism depends on two assumptions. The first is at least questionable; the second, I have already argued, is scarcely tenable. For the first, it will undoubtedly have been remarked that Appiah's derivation of Lewis' difficulties in this regard depends upon accepting the assumption that to believe that 'if p then q' is just to believe that $(p \supset q)$. Believing that $(p \supset q)$ is surely a necessary condition for believing that $(p \rightarrow q)$, but I am not at all convinced that it is the same thing. I have at least a suspicion that individuating beliefs is more complicated than that. Is the belief that Mary is poor but Mary is honest the same as the belief that Mary is poor and Mary is honest? What, precisely, does the belief that as a philosopher Smith is a good pastry chef amount to? And what do I believe when I believe that Charles has a heart of gold? I do not have any suggestions as to how we ought to characterize (analyze?) the belief that 'if p then q' or, more generally, how implicatures are related to beliefs but I doubt that the matter is as cut and dried as Appiah's treatment suggests.

Second, even if we accept Appiah's argument that Lewis' derivation of the second component of the correction factor really does bring him nose to nose with stubborn intuitions contrary to his commitments, why ought this to be so daunting? A conversationalist is, after all, in the business of providing explanations of discrepancies between the deliverances of logic under the assumption of ET and the linguistic intuitions of assorted students and colleagues. In this particular case, Lewis can, once again, appeal to Jackson's doctrine of robustness and say that the reason we are persuaded that someone who believes if p then q and comes to believe p will also come to believe q is that under the conditions where competent speakers offer and accept $(p \rightarrow q)$, the applicability of modus ponens is, so to speak, guaranteed. That is what the constraint on robustness is all about. Appiah can hardly object to this sort of explanation: we have already seen that Adams, whose program he recommends to us, has recourse to just such a conversationalist strategy in explaining why competent speakers regularly take to be valid inference forms (such as contraposition) which, on his account, are not probabilistically sound. (That is what conditions of partial rationality are all about.)

It is my impression that Appiah takes the (supposed) counterintuitive consequences of Lewis' commitments so seriously because he is assuming that there is something epistemically special about linguistic intuitions, especially, perhaps, his own. "I cannot myself find cases, he tells us, where I think it is plausible to suppose that someone believes that if A then C, where p(C/A) is low." (Appiah, 1982, p. 331). This may well be true but I do not see why we need conclude from it more than that either Appiah's treatment of believing that 'if p then q' is mistaken or that his linguistic intuitions have been powerfully shaped by the rules of discourse. In either case, Appiah's intuitions may be as much evidence for pragmatic claims as against them. I am therefore inclined to think that if Lewis really is in any trouble producing proper Gricean credentials for Pr(-q/p), it is not nearly so deep as Appiah thinks.

Neither of Appiah's major criticisms of Lewis, then, seems to me anything like lethal. They do, however, make Jackson's contribution to the cause seem more important than might originally have appeared to be the case. We should, therefore, take a careful look at Appiah's criticisms of his proposal.

Appiah tells us that he is sceptical about Jackson's approach. Perhaps by this he means that he does not find its difficulties as flagrant as those he detects in Lewis. Indeed, most of the space Appiah devotes to Jackson is to an exposition of his proposal rather than to detailed criticism of it. The criticism Appiah does offer seems directed primarily at the claim with which Jackson closes his article.

> Why not simply say the following about (P→Q)? We can distinguish truth conditions from assertion conditions. The truth conditions for (P→Q) are those of (P⊃Q) . . . . And the assertion condition for (P→Q) is that Pr(Q/P) be high. . . . End of story.
>
> My reason is that conjoining is not explaining. The problem is to explain one in terms of the other. . . . I have tried to show how a plausible thesis about (P→Q)'s truth conditions, namely the Equivalence thesis, can, in the light of the importance of robustness for

> assertability, explain <u>the</u> plausible thesis about (P→Q)'s assertion condition, namely Adams' thesis (Jackson, p. 589).

As we have seen, Appiah argues that Jackson has done nothing of the sort: whereas the truth conditions of (p&q) can be invoked to help explain the assertability of 'p but q', the truth conditions of (p⊃q), he tells us, are not essential to explaining the assertability rule for (p→q), and hence are idle machinery. Indeed, Appiah sees no reason to believe that the truth conditions of (p⊃q) are those of (p→q) in the first place. As we have seen, each of these criticisms involves contrasting the presumably unproblematic case of 'p but q' with the supposedly troublesome one of (p→q). In what follows, I shall be arguing that insofar as Appiah gives us any indication of (1) what must be the case for truth conditions to figure essentially in an explanation of assertability conditions, or, (2) what must be the case for a set of conditions to be the truth conditions for a type of sentence, he gives us no reason to believe that there is a difference in principle between the case of 'p but q' and that of (p→q). To be consistent, Appiah must either reject both accounts or neither.

Appiah's suggestion, it will be recalled, is that the first difference between the two cases is that we can, so to speak, decompose the meaning of 'but' into two independent components, whereas

> it follows from the assertibility rule. . . <u>alone</u> that the indicative conditional is assertible only if the speaker believes the material conditional (Appiah, 1982, p. 337).

Now this seems to me largely a matter of how you want to give the assertability rule in each case. If we wish, we may say that for 'p but q' to be assertable, it must be the case that

> (a) the speaker believes that (p&q), and
> (b) the speaker believes that the truth of p usually diminishes the likelihood of the truth of q, and
> (c) the speaker believes that (p&q) is robust with regard to p.

It surely now follows from the assertability rule alone that 'p but q' is assertable only if the speaker believes that (p&q), if the

assertability rule comprises (b) and (c). (If Appiah takes it to include all the constraints on assertability--(a) and (b) and (c)--the assertability of 'p but q' will, of course, guarantee that the speaker believes that (p&q) even in the form which Appiah gives above. But that would be silly.)

We now have an assertability rule for 'p but q' which is sufficient to guarantee that the speaker believe that (p&q). Presumably, Appiah would now find the truth conditions of (p&q) inessential to explaining the assertability of 'p but q'.

On the other hand, we may, if we wish, describe the assertability conditions for (p→q) as follows. It must be the case for (p→q) to be assertable both that

(a) the speaker believes (p⊃q) to be highly likely, and that

(b) the speaker believes that the probability of (p⊃q) is substantially undiminished by the truth of p.

Jackson would have no objection at all to treating the two conceptual components of robustness as distinct. (See, for example, his comments on p. 569.) Indeed, it is somewhat puzzling why he does not do so to begin with. We now have (a) and (b) independent of each other and, presumably, Appiah would now say that there is no greater bar to our finding the truth conditions of (p⊃q) essential to explaining the assertability of (p→q) than there is to finding the truth conditions of (p&q) essential to explaining the assertability of 'p but q'. There is thus no difference in principle on Appiah's account between the case of 'p but q' and that of (p→q) with regard to the eligibility of their truth conditions to play what he calls an essential role in explaining their assertability conditions.

We have, I think, dealt with Appiah's first criticism of Jackson--that even if the truth conditions of (p⊃q) were those of (p→q), they could not figure essentially in an explanation of the assertability rule for (p→q).

Appiah's second criticism is that Jackson has, in fact, no right to say that the truth-conditions of (p⊃q) are those of (p→q). On Jackson's account, says Appiah, the state that we express when we assert (p→q) is

(A): believing the material conditional and being

> disposed to continue to do so even if we believed the antecedent (Appiah, 1982, p. 337, stress removed).

But, Appiah tells us,

> it is not at all clear why we should regard the truth-conditions of the sentence we use to express <u>this</u> state as those of the material conditional (Appiah, 1982, p. 337).

It is even less clear (to me, at any rate), what Appiah would take to be appropriate grounds for making such an ascription. He offers without apparent objection what he calls the standard account of 'but', which has it that the truth conditions of

> (BUT) John is coming, but Mary is not

are that John is coming and Mary is not (Appiah, 1982, p. 336).

It would therefore appear that Appiah is willing to countenance the claim that the truth-conditions of 'p but q' are the same as those of (p&q). Surely, however, the state that we express by 'p but q' is something along the lines of

> (B): believing (p&q) and believing that q is usually unlikely given p and being disposed to continue believing (p&q) even so.

And (B) would seem as readily distinguishable from believing (p&q) as (A) is from believing (p⊃q). Why does Appiah see a problem in ascribing one set of truth-conditions but not the other? If there is a difference in principle between the two cases, Appiah does not tell us in what it consists. If, on the other hand, Appiah sees no difference and wishes to resist all such ascriptions of truth-conditions--a position which he may very well favor--he owes us an account of how he proposes to deal with the (by now equally standard) arguments for the distinguishability of assertability and truth conditions. At the very least, I do not think Appiah gives us grounds for thinking that the truth-conditions of (p⊃q) are any less plausibly the same as those of (p→q) than are the truth-conditions of (p&q) the same as those of 'p but q'.

Even if Appiah is not very forthcoming on the subject, it is, of course, still the case that we need at least some regulative notions of

what ascriptions of truth-conditions require. If the approach which I have been taking so far is correct, the answer to the question he raises--why should we take the truth-conditions of ($p \rightarrow q$) to be those of ($p \supset q$)?--is, as I have suggested earlier, quite simply that this ascription is the one made by the best available theory in the domain under consideration. That is, taken together, ET and the premises of an auxiliary pragmatic theory (along with the odd psychological proposition or two) provide us with the the best account we have of the inferential beliefs and practices of competent language users. I have already presented arguments that this ensemble does, indeed, allow us to account for a wide range of such phenomena. Two final considerations remain to be discussed, however. The first is the question of the status of alternatives to ET-plus-pragmatic theory; the second is its own prospects for future development. Both of these have been proposed as legitimate concerns in the context of rational theory choice. It is to these issues that we now turn.

If, as I have suggested, the acceptability of ET is a matter of the adequacy of the theoretical ensemble in which it plays a role and if that, in turn, is to be assessed by the criteria which count in any decision of this sort, then it becomes relevant not only how well ET _et cie_. are doing but also how well their competitors, if any, are faring. Let us assume that Appiah is correct in assuming that the most attractive alternative to ET is the claim that conditionals do not have truth-conditions (and a fortiori do not have those of the material conditional)--a central tenet of Adams' program. What seems to count most strongly in favor of this proposal is not any good argument on Adams' part to this effect--he simply invites the reader to note the lack of clear criteria for the application of 'true' and 'false' to conditionals with false antecedents and leaves it at that--but, rather, the collective force of his arguments that probabilistic soundness is the appropriate criterion for inference forms and that the measure to be associated with indicative conditionals in predicting their assertability is $Pr(q/p)$. Just as ET cannot be evaluated apart from the theoretical context in which it appears, so the proposal that ET is untenable is persuasive only to the extent that we find attractive an entire theoretical framework in which it is ensconced. In either case,

what we are doing is addressing a set of claims and a set of successes and problems.

If Adams' entire program is the relevant competition, then, how well is it doing? What bears noticing, I think, is the fact that Adams raises, but does not answer, the question of what a non-truth-conditional semantics would be like. He makes no bones about this: what lies ahead, he tells us, is likely to be nothing like what we have been doing and that is substantially all we know about it. Except that Adams relishes the prospect and Lewis clearly does not, they would seem to agree in reporting the current lack of any clear alternative to traditional semantics. We should, therefore, take seriously, I think, what Lewis characterizes as an inconclusive objection to endorsing the hypothesis of the non-truth-conditionality of indicative conditionals.

> [It] requires too much of a fresh start. It burdens us with too much work still to be done, and wastes too much that has been done already. So far, we have nothing but a rule of assertability for conditionals with truth-valued antecedents and consequents. But what about compound sentences that have such conditionals as constituents?. . . Either we need new semantic rules . . . or else we need to explain away all seeming examples of [such] compound sentences (Lewis, pp. 136-7).

If rationality in theory choice requires us to take into account the relative strength of competing theories, consensus that there is a lack of any well worked-out alternative or even a clear indication of how to get started on one would seem to count strongly in favor of ET-and-pragmatic theory.

In addition, I think it is also worth at least suggesting that Jackson's program may have theoretical virtues of which its author does not himself seem fully to be aware. Seen in a properly general and Gricean perspective, Jackson's notion of robustness seems capable of being an element of a much more general pragmatic theory than he indicates and, thus, of having count in its favor considerations of breadth and economy for which it might otherwise not receive credit. I should like rather briefly to sketch such a wider theoretical network.

One obvious question with regard to Jackson's notion of robustness is, of course, whether we can give a general explanation of the direction in which robustness must hold in particular cases. Jackson tells us, for example, that ($p \rightarrow q$) signals the robustness of ($p \supset q$) with regard to p because this is what is required for the very useful modus ponens argument to go through. He is, however, less forthcoming with regard to other cases. He tells us, for example, that we most happily field disjunctions when they are robust with regard to the negation of each disjunct but gives only the sketchiest of suggestions why robustness of this particular sort might be demanded. Having proposed 'p or q' and discovered not p, he tells us, we are not pushed to abandon the disjunction if it is appropriately robust. But why we should want (need?) to salvage the disjunction?

I think we can, without great difficulty, discern the shape of a general rule. Jackson himself is on the brink of so doing when, commenting on Lewis' unassertable (W): You won't eat that mushroom and live, he points out that (W) is misleading <u>given the sort of inference in which it is bound to be employed</u>. We are instantly put in mind of Grice's second derivation of the implicature of IC and his proposed new type of generalized implicature: i.e.,

> the implicature carried by the employment of certain forms of expression that those conditions are fulfilled which would <u>have</u> to be fulfilled if the form of expression were being employed for those purposes which constitute their <u>metier</u> or <u>raison d'etre</u> (Grice, Lecture IV, p. 17).

Locutions liable to enter into particular sorts of inferential reasoning--either because of context or because of their form--implicate, so Grice proposes, that all is well for that sort of inference. In the mushroom case, it is the likelihood that our listener will employ <u>together</u> two salient premises to draw a conclusion which, on this account, mandates the constraint (and thus generates the implicature) that these premises be cotenable (and makes it illictly tricky to assert them if they are not). We might want to say that in a parallel way 'but' and 'nevertheless' conventionally implicate that both conjuncts of the statements in which they figure are (despite

normal expectations to the contrary) available at the same time for such inferential use: their cotenability has been explicitly and conventionally underwritten. It might further be suggested that when we wish to advance a similar guarantee in the case of a state of affairs which, for all we know, may or may not hold, we use the 'even if. . .' locution: 'p even if q' implicates the availability of p for inferential use whether q holds or not, where p would, ordinarily, be unlikely in the case that q, and thus be assumed to be unavailable.

It does not seem too much to say that Jackson has (quite independently, to judge by his footnotes and citations) rediscovered Grice's second sort of conversational constraint and observed an interesting consequence of it which Grice himself did not notice. Because language serves to exchange information, we are governed by the maxims of conversation and, accepting their suzerainty, we derive the well-known conversational implicatures. Because language also serves as a structure which facilitates the drawing of inferences, we are equally bound by another set of constraints which, in their turn, give rise to additional implicatures. To use ($p \rightarrow q$) would, thus, standardly come to implicate not only IC, as Grice saw, but also the robustness of ($p \supset q$) with regard to p, as Jackson shows. (Jackson talks about 'signalling' or 'indicating' rather than implicating, but it is clear that what we have here may be considered a generalized implicature. It is, perhaps, not a generalized <u>conversational</u> implicature in what has become the standard sense of the term, however,--it does not invoke the usual conversational maxims--and Jackson's scruples on this score are certainly understandable in the light of widespread pressure to assimilate generalized conversational implicatures to conventional ones.)

We can also come to see, from this perspective, why 'p or q' implicates, as Jackson tells us, the robustness of the disjunction symmetrically with respect to the falsity of each disjunct, giving rise to the sense that it is wrong to offer 'p or q' on the strength of our certainty that p (or that q) alone. Let me take a moment to lay this out.

On Grice's view, 'p or q' takes its place beside (the presumably more basic) 'not both not p and not q' because we find ourselves faced

with a particular sort of inferential task--the need to answer various (affirmatively phrased) wh- questions ('Who killed Cock Robin?') and the necessity of making do in our planning with interim answers to such questions ('Well, we have several suspects. . .').

Under such conditions, says Grice, it is more economical typographically and perhaps in terms of concepts explicitly mentioned, to use the schema 'p or q' than to use 'It is not the case that both not p and not q' (Grice, Lecture IV, pp. 14-15). To use 'or' would thus come standardly to implicate that we are faced with just such a situation--that is to say, one in which we are unsure of the truth both of p and of q. Once the disjunction had come to convey this implicature of uncertainty with respect to both disjuncts, it would be misleading to field 'p or q' on the basis of certainty that p (alone) or that q (alone)--that is, on truth-functional grounds alone. If we are uncertain of the truth of p and uncertain of the truth of q, in turn, we do not have truth-functional grounds for the disjunction and hence, to be in accord with the maxim of quality, must have non-truth-functional grounds for it. We would thus come to see 'p or q' as capable of surviving the falsity of either one of its disjuncts, which is to say, as signalling symmetric robustness with regard to the falsity of each of its disjuncts. (We could equally have derived this from 'Assert the stronger', of course.)

Reference to the special use in reasoning for which a locution is developed and selected can also shed light on a puzzle to which Jackson calls our attention but which he does not (as far as I can tell) try to solve--the curious asymmetry between our intuitions with regard to the equivalence of '$\vee$' and 'or', on the one hand, and '$\supset$' and 'if. . .then', on the other. Grice had already noted its inability to explain a closely related asymmetry between 'or' and 'if. . . then' as a shortcoming of his first and better-known account of the derivation of the implicature of IC conveyed by the indicative conditional.

> That the account so far given does not go far enough is shown by the following objection. "The account you have so far given could be applied not only to 'if p then q' but also to 'either p or q'. It would, if accepted, explain why someone who advances either a conditional or

> a disjunctive would normally implicate that there are non-truth-functional grounds for saying what he has said. But there is an important difference between conditionals and disjunctives which remains unaccounted for, namely that whereas there seems to be no general difficulty in the idea that a disjunctive statement which has been advanced on non-truth-functional grounds can be confirmed truth-functionally, by establishing one of the disjuncts, the parallel idea with respect to conditionals is not acceptable; except perhaps in some very special cases, we do not regard the mere discovery that it is not the case that p, or the mere discovery that q, as confirming a statement that if p then q" (Grice, Lecture IV, pp. 6-7).

Grice tells us that he prefers his second account of how ($p \rightarrow q$) comes to carry the generalized implicature of IC because it allows him to account for just this asymmetry. His explanation of the difference with regard to our intuitions about confirmation in the two cases invokes the doctrine of the metier of a locution and goes as follows.

First, Grice suggests that we will be comfortable saying that a statement has been confirmed when we see that it could, indeed, have been put to the special use in reasoning for which it was evolved and selected.

> A disjunction, thought of as being put to 'planning' employment, is regarded as confirmed by establishing either disjunct; for this, after the planning has been done, will show that a disjunctive statement has done its job (we haven't wasted our time) (Grice, Lecture IV, p. 19).

On the other hand, in the case of a conditional,

> [T]o discover that '$p \supset q$' has the value TT [i.e., that both p and q are true] will show. . . [that] as employed in MPP either [it] has done its job, or would have done its job but for the 'accident' that the discovery that p did not precede the discovery that q. But to discover that not-p would be to discover that there was no job

> for it to do. . . and to discover merely that q, would leave it open whether there was a job for it to do (Grice, Lecture IV, p. 19).

Thus, unlike the case of $(p \vee q)$, where we welcome equally as confirmation either mode of truth-functional validation,

> Insofar as '$p \supset q$' is put to the employment which is its raison d'etre as a form of expression it is clear that we shall not be interested in some of its [truth-functional] validation possibilities (Grice, Lecture IV, p. 18).

Grice has thus given at least a tentative account of the coincidence of truth-functional validation and our intuitions with regard to confirmation in the case of 'or' and of the discrepancy between them when it comes to 'if. . .then'. This may, in turn, provide an explanation for the relative ease with which we accept the equivalence of 'or' and '$\vee$' and the unease we experience when confronted with ET.

If we consider the standard conversational implicatures, then, as only one sort of constraint on discourse, generated by only one of the functions of language, and, with Grice, entertain in addition the possibility that other functions of language may give rise, just as systematically, to other sorts of constraints, then Jackson's observations about robustness reveal themselves as fitting neatly into a more general pragmatic theory. If we are willing to see a connection even more generally between the conditions of felicity of speech acts and their implicatures--between, say, the requirement that the door be closed for 'Please open the door' to be happily fielded, on the one hand, and the existence of an implicature that the door is closed coming standardly to be conveyed by the request, on the other--then we can set Grice's theory into an even more extensive theoretical framework (Levinson, p. 105). That we can do so redounds to the credit of the thesis we wish to defend: it shows that it is part and parcel of a research program which promises to produce a coherent account of more and more phenomena by way of a nicely delimited set of initial presumptions and concepts. That we can extend Jackson's proposal in the manner I have suggested ought, I think, to lend support to ET in just the same manner that the explanatory promise of any theoretical

ensemble lends plausibility to each of its constituents.

I began this paper with the suggestion that, despite rumors to the contrary, there are respectable reasons to claim that the truth-conditions of $(p \rightarrow q)$ are quite standardly those of $(p \supset q)$. If goodness of theory counts toward the application of predicates such as 'is true' and 'has the same truth-conditions as', then I believe I have made a case for this suggestion.

FOOTNOTES

Footnotes

[1] Frank Jackson, "On Assertion and Indicative Conditionals," Philosophical Review, 78(1979), 565-689.

[2] Anthony Appiah, "Conversation and Conditionals," Philosophical Quarterly, 129(1982), 327-338.

[3] J.L. Mackie, Truth, Probability and Paradox: Studies in Philosophical Logic. (Oxford: Clarendon Press, 1973), p. 66.

[4] Geoffrey Hunter, Metalogic: An Introduction to the Metatheory of Standard First Order Logic. (Berkeley: University of California Press, 1973), p. xiii.

[5] Michael Clark, "Ifs and Hooks," Analysis, 32(1971), p. 33.

[6] Bruce Aune, "If," Encyclopedia of Philosophy, 1968 ed.

[7] P.F. Strawson, Introduction to Logical Theory, (London: Methuen & Co. Ltd., 1952).

[8] That at least some research programs in philosophy may usefully be regarded as competing in much the same manner and on very similar terms as those in science is a major thesis in Harold I. Brown, Perception, Theory and Commitment: The New Philosophy of Science (Chicago: University of Chicago Press, 1977). That the participants in the debate over ET are, in fact, adducing just the sorts of considerations that normally count in favor of theories will, I hope, become obvious in the course of this paper.

[9] Michael Clark, "Ifs and Hooks: A Rejoinder," Analysis, 34(1974), p. 83.

[10] Robert Stalnaker, "Probability and Conditionals," in Ifs, ed. W.L. Harper, R. Stalnaker, and G. Pearce (Dordrecht: D. Reidel Pub. Co., 1981), p. 193.

[11] I am indebted to H. Hendry for this.

[12] An intuitionist informant would, however, demur at the translation of 'not' by '-', since the truth conditions of intuitionist negation are quite a different matter from those of classical negation. (See D.C. Makinson, Topics in Modern Logic, (London: Methuen & Co. Ltd., 1973), pp. 68-69.) If, on the other hand, our competent speaker had been impressed

by Anderson and Belnap rather than by Brouwer and Heyting, we would have a different objection on our hands. Validity of inference, we would be informed, requires that premises be relevant to the conclusion being drawn from them. In particular, the inference from 'p or q' and 'not p' to 'q' demands for its validity that the disjuncts be relevant to each other. Thus, according to the relevance logician, 'or' has an intensional sense which is not captured by the classical 'v' with which we have represented it and our translation fails. (Susan Haack, Philosophy of Logic, (Cambridge: Cambridge University Press, 1978), pp. 199-200.)

[13] There have been attempts to show that ($p \supset q$) and ($p \rightarrow q$) do stand in a logical relationship to each other. Faris, for example, has proposed that they are 'interderivable' (J.A. Faris, "Interderivability of '$\supset$' and 'If'," in Logic and Philosophy: Selected Readings, ed. Gary Iseminger (New York: Appleton-Century-Crofts, 1968).) Haack summarizes his argument as follows.

> He assumes that a necessary and sufficient condition for the truth of 'If A then B' is condition E: there is a set S of true propositions such that B is derivable from A together with S. If '$A \supset B$' is true. . . there is a set of true propositions, namely, the set of which '$A \rightarrow B$' is the sole member, from which, with A, B is derivable; so E is satisfied, and 'If A then B' is true (Haack, p. 36, my stress).

Haack observes that there have been various objections raised against Faris' claim and offers a qualification of her own: Faris' argument, she tells us, depends heavily on a notion of 'derivability' which spans natural and formal languages in a somewhat irregular manner (Haack, p. 36).

[14] There is, Strawson believes, no natural language connective which performs this task. On the other hand, it has been

observed by Gardiner that if we take the four possible combinations of truth and falsity for two propositions

p&q p&-q -p&q -p&-q

> There exists in the English language. . . the means of eliminating every possible subset of alternatives from this exhaustive list. For instance, when we say 'not both p and q', we are eliminating the first alternative; when we say 'if p, then q', we are eliminating the second alternative; when we say 'either p or q', we are eliminating the first and fourth alternatives; when we say 'neither p nor q', we are eliminating the first, second and third alternatives. (W. Lambert Gardiner, Psychology: A Story of a Search (Belmont, Calif.: Brooks/Cole Pub. Co., 1970), p. 213.)

Gardiner seems to think that 'if. . .then' performs precisely this task.

15 Although Strawson seems to identify the meaning of (p→q) with its truth conditions, nothing in the discussion that follows hinges on such a position. ET is the thesis that the truth-conditions of (p→q) and (p⊃q) are the same, not that their meaning is the same. We can remain splendidly neutral with regard to the tenability of a truth-conditional account of meaning.

16 Richard Jeffrey, Formal Logic: Its Scope and Limits (New York: McGraw-Hill Book Company, 1981), p. 76.

17 Similarly, in urging that (p⊃q) does not mean what (p→q) ordinarily means, Aune not only tells us that

> 'p⊃q' means approximately 'not-p or q' or 'not both p and not-q,' and there is nothing very conditional about these statements

but goes on to object that

> if 'p' is true, then 'r⊃p' is true for any 'r' whatsoever. And to argue 'r, so p' amounts to committing a fallacy of relevance. . . . [T]he arguments 'The moon is made of green cheese, so

> Socrates was a philosopher' and 'Socrates was a philosopher because the moon is made of green cheese' are both jokes, even though 'Socrates was a philosopher' is true (Aune, p. 129).

This is pretty confused but one gets the general idea that what bothers Aune about ET is something along the lines of the so-called fallacies of material implication.

[18] William S. Cooper, Foundations of Logico-Linguistics (Dordrecht: Reidel Publishing Co., 1978), pp. 197-8.

[19] I.L. Janis and F. Frick, "The Relationship Between Attitudes Toward Conclusions and Errors in Judging Logical Validity of Syllogisms," J. Experimental Psychology, 33(1943) pp. 73-77; I. Begg and J.P. Denny, "Empirical Reconsideration of Atmosphere and Conversion Interpretations of Syllogistic Reasoning Errors," J. Experimental Psychology, 81(1969) pp. 351-354 cited in Robert L. Solso, Cognitive Psychology (New York: Harcourt Brace Jovanovich, Inc., 1979).

[20] P.N. Johnson-Laird and M. Steedman, " The Psychology of Syllogisms," Cognitive Psychology, 10 (1978), pp. 64-99, cited in R.L. Solso, Cognitive Psychology.

[21] P.C. Wason, " Reasoning About a Rule," Quarterly Journal of Experimental Psychology, 20(1968), pp. 273-281, cited in A.L. Glass, K.J. Holyoak and J.L. Santa, Cognition (Reading, Mass.: Addison-Wesley Publishing Co., 1979). (See also P.C. Wason and P.N. Johnson-Laird, Psychology of Reasoning (Cambridge: Harvard University Press, 1972).) When this task is presented in workshops on cognition, participants are apparently inclined to defend their erroneous answers with considerable persistence even in the face of correction. (John Furlong, informal communication.)

[22] J.L. Austin, " A Plea for Excuses," in his Philosophical Papers, ed. J.O. Urmson and G.J. Warnock (Oxford: Oxford University Press, 2nd ed.), p. 203.

[23] It is, of course, important to distinguish between the claim that we could all be mistaken about some such particular matter of truth or validity and the claim that we could all

be mistaken about almost everything of the sort. As Stanley Cavell puts it,

> The claim that in general we do not require evidence for statements. . . about what we are doing or about what we say [does not rest on a claim that we cannot be wrong] but only that it would be extraordinary if we were (often).

("Must We Mean What We Say?" in V.C. Chappell, ed., Ordinary Language: Essays in Philosophical Method (New York: Dover Publications, Inc., 1964, rpt. 1981), pp 87ff.)

[24] See, for example, Richard Rorty, "Realism and Reference," Monist, 60(1978), pp. 321-339.

[25] Richard Rorty," Mind-Body Identity, Privacy, and Categories," Review of Metaphysics, 19(1965), pp. 24-54.

[26] Presented in H.P. Grice's William James Lectures, delivered at Harvard University in 1967.
Part of this series has been published as " Logic and Conversation," in The Logic of Grammar, ed. Donald Davidson and Gilbert Harman, (Encino, California: Dickenson Publishing Co., Inc., 1975) and as Further Notes on Logic and Conversation, in Syntax and Semantics, vol. 9, Pragmatics, ed. Peter Cole (New York: Academic Press, 1978).

[27] H.P. Grice, William James Lectures, 1967, duplicated typescript, Lecture IV, p. 1.

[28] Stephen C. Levinson, Pragmatics. (Cambridge: Cambridge University Press, 1983), p. 97, note.

[29] Generation of an implicature also involves mutual knowledge on the part of conversational partners. As Levinson puts it,

> We can say that S and H mutually know p iff S knows p, H knows p, S knows that H knows p, H knows that S knows that H knows p, and so on, ad infinitum (Levinson, p. 113).

[30] Ernest W. Adams, The Logic of Conditionals (Dordrecht: Reidel, 1975), p. 31.

[31] Ernest Adams, "The Logic of Conditionals," Inquiry, 8(1965), p.

169.

[32] I think it is worth noting that Adams' argument here seems to depend upon the assumption, questioned earlier, that ordinary usage is the last word on meanings. I have already suggested that it makes better sense to say of items of ordinary language what we are inclined to say of more obviously theoretical terms--that their meaning is given by the network of beliefs and uses in which they occur. (I had to be told, for example, that 'odd' and 'even' had application to 0.) If this is the case, then 'true' and 'false' may quite reasonably be said to have meanings even in cases where competent speakers may not acknowledge them.

[33] Adams' concern in this regard is understandable. Among the widely used inference patterns which fail his test for universal probabilistic soundness are contraposition, disjunctive syllogism, hypothetical syllogism and conditionalization. It may be noted in this connection that he has thus to rely on Gricean considerations of appropriate usage and the avoidance of misleading assertion quite as heavily as does any conversationalist advocate of ET. Adams is frank in acknowledging that there is no obvious way to determine whether such conversationalist considerations are more reasonably invoked in favor of his proposal or ET.

> How to decide who is right?. . . For now we must simply adopt a position consonant with our ratio representation of the probability of a conditional. . . . Chapter III [on criteria for rational decision making] attempts to deal with these matters though even there nothing will be conclusively settled (Adams, 1975, pp. 20-21).

Those who find conversationalist explanations of purported failures of classical deductive logic under the assumption of ET to be disagreeably ad hoc should, I think, pay careful heed to Adams here.

[34] Ian F. Carlstrom and Christopher S. Hill, rev. of The Logic of Conditionals by Ernest W. Adams, Philosophy of Science,

45(1978), pp. 155-158.

35 Dorothy Edgington, rev. of The Logic of Conditionals by Ernest W. Adams, Mind, 87(1978), pp. 619-623.

36 David Lewis, "Probabilities of Conditionals and Conditional Probabilities," in Ifs, ed. W.L. Harper, R. Stalnaker, and G. Pearce, p. 135.

37 My own linguistic intuitions suggest that a speaker who actually believed that Carter would win no matter who his opponent was would be considered distinctly misleading in asserting 'If Reagan does not run, Carter will be reelected'. Surely, if I think that Smith is a competent philosopher who will do well on any topic, it would be misleading to offer 'If we stick to first order logic, Smith will do fine'. It would, of course, be appropriate to say 'If Reagan runs, Carter will be reelected and if Reagan does not run, Carter will be reelected' if the speaker wished to emphasize either the certainty of 'Reagan will be reelected' or its causal independence with regard to 'Carter runs'.

38 Jackson seems to be assuming here (and perhaps elsewhere) that conversational implicatures are derived only from the truth conditions of what is said. While this may be consonant with his own endorsement of a truth-conditional account of meaning, there is no reason to attribute this constraint to conversationalist theorists generally and good reason not to. Discussing Gricean implicatures, Levinson, for example, says

> Some quite detailed arguments can be given to show that all but the Manner implicatures must be read from the level of semantic representation, including some specification of logical form. They cannot be read off from uninterpreted surface structures, nor can they be inferred simply from the truth conditions of the sentence uttered (Levinson, p. 123).

Levinson's discussion of the need to go beyond truth conditions includes the observation that despite the fact that expressions of the forms (p) and (p & (p→p)) will have

the same truth conditions, their instantiations may very well not convey the same implicature. He asks us to compare, for example, 'It's done' with 'It's done and if it's done, it's done'. He observes that

> The latter alone has a distinctive implicature, roughly. . . 'It's no good regretting what has already happened' (Levinson, p. 124).

Grice suggests that because tautologies are deviant utterances their assertability will be evaluated in terms of the appropriateness of their implicatures rather than on the basis of what is said. While there is no simple explanation of how implicatures are generated by utterances that flout the maxims in so wholehearted a manner, we may expect that their semantic and logical character will play a part in the process. One possibility that occurs to me is that the very uninformativeness of truths of logic allows their deployment to signal that they are not being proferred in the service of conveying information but, rather, are intended to do something else. What this something else might be, in turn, might be a matter of the specific semantic content of the tautology. It seems, for example, that speakers often field truths of logic when they want to advise or console their listener or make salient some shared social standard. E.g., one way of construing the implicature of 'War is war' might be 'Stop protesting (grieving, struggling) because there is no reason to expect things to be any different'; one way of construing the implicature of 'Boys will be boys' might be 'Stop trying to change things; it's o.k. for males to act that way'. Truths of logic might thus be assertable when they perform an acceptable normative or solidarity-producing function. This is at least consistent with the observation that there is often a somewhat dismissive tone to their deployment--their use cuts off further debate or discussion (Levinson, p. 111). (You can't really argue with a Hallmark greeting card.) It is also consistent with Grice's observation that irony--a stance implicated, under the right

circumstances, by factually unsatisfactory utterances--conveys not only belief about some object but the speaker's disapproving attitude toward it. (For a discussion of these issues, see Grice, 1978 and Levinson, Chapter 3.) In general, by limiting his discussion to the rule 'Assert the stronger', Jackson seems seriously to underestimate the resources of a standard conversationalist strategy.

[39] Curiously, Jackson does not seem to offer a solution to this puzzle. On the other hand, as we shall see, Grice had already made a very close pass at it.

[40] If all we need to explain our intuition that this is not a valid inference is the observation that we have an assertable premise and an unassertable conclusion, it is not instantly evident why we should prefer Jackson's account over the Gricean one. Despite the assertability of the premise, the conclusion

(C) If Carter is reelected by a large margin, then Carter will not be reelected

is unassertable on standard conversationalist grounds because it improperly implicates IC--the existence of some causal or linguistic connection between its antecedent and its consequent. Not only does 'Carter is reelected by a large margin' fail to provide grounds for believing 'Carter is not elected' but it is linguistically incompatible with such a claim. If it makes us queasy to find ourselves implicating what is contingently false, we should not be surprised to discover that implicating what is logically false makes us pretty seasick. (Cf. 'If today is Monday then tomorrow is Thursday'.)

[41] Other participants in the debate seem to be comporting themselves in a similar manner. It is not the existence of knock-down arguments but a judgment of over-all theoretical promise that seems to be leading Edgington, for example, to characterize Adams' proposal as hammering nails in the coffin of material implication theory; both Cooper and L. Jonathan Cohen complain that Gricean doctrine has been evaluated

against a misleadingly restricted range of cases (L. Jonathan Cohen, "Some Remarks on Grice's Views About the Logical Particles of Natural Language," in Yehoshua Bar-Hillel, ed. Pragmatics of Natural Language (Dordrecht: Reidel Publishing Co., 1971)). Cooper comments, in addition, on what he takes to be the ad hoc quality of Gricean explanations of supposed paradoxes. One of the more surprising aspects of the debate over ET is, indeed, how much disagreement there can be over the relative virtues of competing approaches, given the very high degree of agreement over the facts of the case and the relevant formal arguments. Nobody seems to contest Lewis' triviality findings nor does anyone deny the awkwardness for ET of speakers' treatment of negated conditionals; the debate is, rather, over what we ought to make of all this. Perhaps because of their high regard for the objectivity and consensus characteristic of scientific undertakings, debates among philosophers in the analytic tradition tend to sound a lot like debates among scientists under similar circumstances.

APPENDIX

Appendix

a. Derivation: $u(p \rightarrow q) = u(p \supset q)/Pr(p)$

(1) $u(p \rightarrow q) = 1 - Pr(p \rightarrow q)$ Definition 'u(p)'

(2) $= 1 - Pr(q/p)$ Definition 'Pr(p→q)', (1)

(3) $= 1 - \frac{Pr(p \& q)}{Pr(p)}$ Definition 'Pr(q/p)', (2)

(4) $= \frac{Pr(p)}{Pr(p)} - \frac{Pr(p \& q)}{Pr(p)}$ Substitution

(5) $= \frac{Pr(p) - Pr(p \& q)}{Pr(p)}$ Simplification

(6) $Pr(p) = Pr(p \& q) + Pr(p \& -q)$ Calculus of probability

(7) $Pr(p) - Pr(p \& q) = Pr(p \& -q)$ (6)

(8) $u(p \rightarrow q) = \frac{Pr(p \& -q)}{Pr(q)}$ Substitution in (5), (7)

(9) $Pr(p \& -q) = Pr(-(p \supset q))$ Semantics of '(p⊃q)'

(10) $= 1 - Pr(p \supset q)$ Calculus of probability

(11) $= u(p \supset q)$ Definition 'u(p⊃q)'

(12) $u(p \rightarrow q) = \frac{u(p \supset q)}{Pr(p)}$ Substitution in (8), (11)

b. Here is a paraphrase of the version Hill and Carlstrom present.

Let A, B and (A→B) be statements true at some possible world W_1. Since there is no truth functional connective such that its probability of truth is p((A&B)/p(A), the truth values of A and B do not determine the truth of (A→B). Hence, there can be a world W_2 at which A and B are true and (A→B) is true and a world W_3 at which A and B are true but (A→B) is false.

Now consider two assignments of probabilities to worlds. Under one assignment, it is highly probable that either W_1 or W_2 will be the case and it is just about as likely that W_1 will be the case as it is that W_2 will be the case. Under this assignment, a sentence true at both worlds has a probability close to 1 of being true. Hence, under this assignment (A→B) (A→B) has a probability close to 1 of being true.

Now consider a second assignment of probabilities to worlds. This one is like the first except that the two worlds under consideration are now W_1 and W_3. Under this assignment, since it is true in only one of the worlds, (A→B) has a probability of about .5 of being true.

Under both assignments, the probabilities of A and of (A&B) are just about the same, and thus the conditional probabilities of (B/A) are also just about the same since Pr(B/A) = Pr(A&B)/Pr(A).

If the probabilities of truth of (A→B) differ from each other under the two assignments whilst the conditional probabilities of (B/A) do not, then the conditional probabilities associated with (A→B) cannot be its probabilities of truth.

c. This is a version of Lewis' proof that, given the standard axioms for a probability function, it is only for a trivial language that there can be a probability function Pr such that for any statements p and q, $Pr(p \rightarrow q) = Pr(q/p)$ when Pr(p) is greater than 0. The basic outline is that provided by Jeffrey (1981); it is clearer by virtue of suggestions by H. Hendry.

First we list some relevant theorems about conditional probabilities.

Definition. $Pr(p \rightarrow q) = Pr(q/p)$ if Pr(p) is greater than 0.

(1) Suppose Pr(p) is greater than 0. Then
$Pr(q/p) = 1$ if p implies q
$Pr(q/p) = 0$ if p implies -q.

(2) Multiplicative law.
$Pr(q\&r/p) = Pr(q/p)Pr(r/q\&p)$ if Pr(p&q) is greater than 0.

(3) Law of compound probability.
$Pr(q) = Pr(q/p)Pr(p) + Pr(q/-p)Pr(-p)$.

(4) Law of successive conditionalization.
If $Pr'(r) = Pr(r/p)$ and $Pr''(r) = Pr'(r/q)$, then
$Pr''(r) = Pr(r/p\&q)$ if Pr(p&q) is greater than 0.
(The result of successive conditionalization on two statements is the same as that of conditionalizing once on the conjunction of those statements.)

Now, suppose we take 'if' to be a non-truth-functional connective (as is, for example, 'because') such that Pr(q if p) = Pr(q/p) when Pr(p) is greater than 0. Then, interpreting '/' as 'if', we could go ahead and iterate '/' as we would any other connective, producing expressions like 'Pr((q/p)/r)'.

(This would be the same thing as being willing to write $Pr(r \rightarrow (p \rightarrow q))$, for example, if we think $Pr(p \rightarrow q) = Pr(q/p)$.)

If it means anything, $Pr((q/p)/r)$ is the outcome of conditionalizing twice and so, by (4) above, it is equal to $Pr(q/p\&r)$.

And if we can conditionalize twice, then we can invoke (3) and, substituting q/p for q and q for p, conclude that $Pr(q/p) = Pr((q/p)/q)Pr(q) + Pr((q/p)/-q)Pr(-q)$.

By (4), $Pr((q/p)/q) = Pr(q/(p\&q))$ and $Pr((q/p)/-q) = Pr(q/(p\&-q))$.
So $Pr(q/p) = Pr(q/(p\&q))Pr(q) + Pr(q/(p\&-q))Pr(-q)$.
But, by (1), $Pr(q/(p\&q)) = 1$ and $Pr(q/(p\&-q) = 0$.
So $Pr(q/p) = 1Pr(q) + 0Pr(-q)$.
Therefore, $Pr(q/p) = Pr(q)$, which means that p and q are independent.

Our first finding, then, is that

> We can treat '/' as a connective only at the cost of finding that whenever c(p&q) and c(p&-q) are both positive, p and q are independent. . . . Then unless c is so trivial a probability function as to make practically all statements independent of each other, '/' is no connective. . . . (Jeffrey, p. 84)

This, in turn, leads to the next trivialization result.

> '/' cannot be treated as a connective if c assigns positive values to even three pairwise unsatisfiable statements (Jeffrey, p. 85).

Let q, r, s be pairwise incompatible and let p be the disjunction $(q \vee r)$. Let Pr(q), Pr(r) and Pr(s) each be greater than 0.

(1) By the Kolmogorov axioms, $Pr(p) = Pr(q)$ if p and q are equivalent. Hence $Pr(p\&q) = Pr(q)$.

(2) We have just seen that '/' cannot be treated as a connective unless p and q are independent, which is to say, unless Pr(p&q) = Pr(p)Pr(q). Since, by (1), Pr(p&q) = Pr(q), we may substitute and say that '/' cannot be treated as a connective unless Pr(q) = Pr(p)Pr(q).

(3) Which is to say, unless Pr(p) = 1. (Dividing both sides by Pr(q), which we may do since, by hypothesis, Pr(q) is greater than 0.)

(4) Which is to say, unless Pr(-p) = 0. (By the Kolmogorov axiom which has it that Pr(-p) = 1- Pr(p).)

(5) But this is impossible because, since the three statements are, by hypothesis, pairwise incompatible, s implies -p and if s implies -p then (by yet another of the axioms of probability) Pr(s) must be equal to or less than Pr(-p). If Pr(s) is equal to or less than 0, it cannot be positive but, by hypothesis, it <u>is</u> positive.

So the assumption that there are as many as three pairwise incompatible statements with positive probability leads to a contradiction if we take '/' to be a connective. Thus, the only language which could have such a connective would be one with fewer than three contingent pairwise incompatible statements. As Lewis notes, we may justly call such a language a trivial one (Lewis, p. 132).]

d. We have already shown that $u(p \supset q)$ is generally less than $u(p \rightarrow q)$, which is to say that $Pr(p \supset q)$ is generally greater than $Pr(p \rightarrow q)$.

That $Pr((p \supset q)/p) = Pr(q/p)$ can be shown as follows.

(1)	$Pr((p \supset q/p) = Pr((-p \vee q)/p)$	Definition 'p⊃q'
(2)	$= Pr((-p \vee q) \& p/Pr(p)$	PL
(3)	$(-p \vee q) \& p$ is logically eqivalent to $(p \& q)$	PL
(4)	Thus $Pr((-p \vee q) \& p) = Pr(p \& q)$	(2),(3), Substitution
(5)	And $Pr((p \supset q)/p) = \frac{Pr(p \& q)}{Pr(p)}$	(2),(4), Substitution
(6)	$Pr(p \& q) = Pr(q/p)$	Definition, 'Pr(q/p)'
(7)	So $Pr((p \supset q)/p) = Pr(q/p)$	(5), (6), Substitution