

# Apocalyptic Relevance<sup>1</sup>

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**ABSTRACT:** In their book, *Relevance*, Sperber and Wilson make an important contribution towards constructing a credible theory of this unforthcoming notion. All is not clear sailing, however. If it is accepted as a condition on the adequacy of any account of relevance that it not be derivable either that nothing is relevant to anything or that everything is relevant to everything, it can be shown that Sperber and Wilson come close to violating the condition.

**KEY WORDS:** Corroboration, implication, inference, paraconsistent logic, relevance, relevant logic, rule of adjunction.

There are reasons to be wary of treating relevance as a propositional relation.<sup>2</sup> Though it is in many ways the best account produced to date, I fear that the contributions of Sperber and Wilson<sup>3</sup> fall foul of trouble that afflicts other propositional approaches.

Perhaps the most dramatic difficulty that a theory of relevance can run into is that it makes apocalyptic provisions for its target concept: It makes it true either that nothing is relevant to anything or that everything is relevant to everything or, twice-over, some dangerous approximation thereto. Apocalypse is so undesirable a consequence that it seems as natural as it is necessary to impose as an adequacy condition on any would-be theory of relevance the requirement that:

(AC1:) A theory of relevance should not imply that relevance is (or is approximately) apocalyptic.

My limited task here is to establish that the account of Sperber and Wilson violates AC1.

Sperber and Wilson define relevance for ordered pairs,  $\langle P, C \rangle$  where P is an assumption or belief and C a context, itself a conjunction of beliefs. So defined, theirs is another basically propositional approach. I must here mention that I have taken the liberty of recasting the account propositionally, with conjunction for set theoretic-union. This might turn out to be a liberty that does violence to their account. However, I mean to begin this way with a strategic purpose in mind. Doing so will facilitate critical evaluation, as we will see. The principal

claim of the theory is:

*Relevance.* A belief is relevant in a context if and only if it has some contextual effect in that context.<sup>4</sup>

An assumption or belief has a contextual effect in a context when it strengthens or reinforces a belief contained in that context, when it contradicts a belief contained in that context and thus forces an “erasure”, or when it licenses implications. Contextual effects can in each case be likened to changes of mind. Degrees of confidence are raised or lowered, beliefs are contradicted and erased, or new beliefs are derived.

As it happens, both strengthening and contradiction are analyzable in terms of contextual implications,<sup>5</sup> so I shall concentrate my remarks upon it. Contextual implication is defined in the following way. Where P is a belief and C a context and Q a further belief,

*Contextual implication.* P contextually implies Q in context C iff (i)  $P \wedge C$  non-trivially implies Q; (ii) P does not non-trivially imply Q; and (iii) C does not non-trivially imply Q.<sup>6</sup>

Here, then, the central idea is that upon new information P into a given inventory of commitments C, an implication is sanctioned of some further assumption Q, where Q couldn't be got either from C alone or from P alone.

It is necessary to say something about the idea of non-trivial implication, which drives the definition of contextual implication.

*Non-trivial implication.* P logically and non-trivially implies Q iff when P is the set of initial theses in a derivation involving only elimination rules, Q belongs to the set of final theses.<sup>7</sup>

For their part, “[e]limination rules ... are genuinely interpretative: the output assumptions explicate or analyse the content of the input assumptions.”<sup>8</sup> Further, “[o]ur hypothesis is that the human deductive device has access only to elimination rules, and yields only non-trivial conclusions...”<sup>9</sup>

Elimination rules contrast with introduction rules. Consider the familiar rule of v-introduction, illustrated by

[A] 1. P  
2.  $\therefore P \vee Q$       1, v-introduction

This can be compared with the equally familiar rule of  $\wedge$ -elimination, illustrated by case [B].

[B] 1.  $P \wedge Q$   
2.  $\therefore P$       1,  $\wedge$ -elimination

It becomes clear at once that the difference between the sort of rule reflected in [A] and the sort of rule reflected in [B] lays an entirely unconvincing claim on the distinction between triviality and non-triviality. This might lead us to think that the account of contextual implication is ruined. But this would be over-hasty, and in fact quite wrong – the upgrading of a semantical quibble to a substantial complaint. For it is open to Sperber and Wilson to suppress all

reference to triviality and non-triviality and to re-write the definition of contextual implication as follows.

*Revised contextual implication.* P contextually implies Q in context C iff (i)  $P \wedge C$  implies Q; (ii) P does not imply Q; (iii) C does not imply Q; and (iv) the implications cited in (i) to (iii) employ elimination rules only, that is, they are implications whose consequents (purport to) explicate the content of their antecedents.

While disposing of the problem of non-triviality, it may be objected that Revised Contextual Implication runs into difficulties of its own. In particular it might be thought that content containment is relevance, plain and simple. So revised, Contextual Implication comes to this: that P contextually implies Q in C just when  $P \wedge C$  relevantly implies Q, P does not relevantly imply Q and C does not relevantly imply Q. So far, so good. But notice that Revised Contextual Implication is disqualified from service in the definition of relevance itself. For it is a corollary of the Relevance condition that

P is relevant in C if there is some Q relevantly implied by  $P \wedge C$  but not by either P alone or C alone.

The circularity, it might be said, is evident and irritating and it spreads to the Relevance condition in an obvious way.

The objection doesn't hold up. It is quite true that, for some relevance theorists, content-containment or topical overlap is relevance. But it is not relevance in the theory of Sperber and Wilson. It is linked to their relevance, and it shares certain intuitions captured by their relevance, but it is not relevance by their lights. So the circularity is only apparent.

The real trouble lies elsewhere. For let  $C = \{Q\}$  and let P be any arbitrarily selected belief, and let P and Q be logically independent of one another. Then

1.  $P \wedge C$  non-trivially<sup>10</sup> implies Q.  
By the specification of C and the  $\wedge$ -elimination rule.
2. P does not non-trivially imply Q.  
By the independence condition.
3. C does not non-trivially imply Q.  
For Q doesn't non-trivially imply itself. Why? Because  $\Phi \vdash \Phi$  is not an elimination rule. It is a redundancy rule.

Whereupon we have it that every such arbitrary P is relevant in every such context C. Apocalypse nearly enough, and lax fulfillment at best of AC1. For every pair of logically independent propositions the one is relevant to the other in any context containing one or the other alone. The problem is averted, of course, if one forbids contexthood to unit sets. Perhaps the authors intend such a restriction, never mind that they don't provide it. I mean to persist with the difficulty, because Sperber and Wilson seem to have provided other resources for dealing with it.

Though it turns on a somewhat technical matter, a further comment is required. Contextual implication is also held to be synthetic implication.<sup>11</sup> A

synthetic implication is one using only synthetic rules of derivation,<sup>12</sup> and a synthetic rule takes not one but “two separate assumptions as input”. Thus the rule of  $\wedge$ -elimination on which our counter-example rests, “which takes a single conjoined assumption as input, is an analytic rule, and *modus ponendo ponens*, which takes a conditional assumption and its antecedent as input, is a synthetic rule.”<sup>13</sup> So, in its employment of  $\wedge$ -elimination – an analytic rule – our counter-example to relevance-promiscuous contextual implication will be thought to have failed. In fact, however, there are two reasons to resist this rejoinder.

*First*, the putative distinction between analytic and synthetic rules is muddled. For the implication of  $Q$  by  $P$ ,  $P \rightarrow Q$  will be held to be synthetic whereas the implication of  $Q$  by  $P \wedge (P \rightarrow Q)$  will be held to be analytic. And there, it may be said, one finds no difference that makes a difference. (But see below.)

*Second*, even if the above objection is misconceived, the general form of a contextual implication is one in which we have the implication of  $P$  by  $Q \wedge C$ , that is, an implication from a single (conjunctive) premiss. So by the reasonings of the above rejoinder, the putative contextual implication is not a synthetic implication and hence, not a contextual implication after all. Given that the entire category of contextual effects, with reference to which relevance is defined, is bound up with the idea of non-empty contextual implications, we would have it that nothing is relevant to anything. Apocalypse, and again AC1 is failed.<sup>14</sup>

We’ve arrived at a critical juncture. I have presented the interaction between  $P$  and  $C$  as the conjunction of  $P$  with a conjunct of  $C$ , since  $C$  itself I’ve represented as a conjunction. This guarantees that the result of connecting  $P$  with  $C$  is a “single assumption”, not “two separate” ones. This suffices to block the derivation of any synthetic implication from the conjunction of  $P$  with a member of  $C$ , wherewith all possibility of contextual implication is foreclosed, and the account of relevance is ruined. It is precisely here that my formulation by way of conjunction rather than set theoretic union is a liberty which does the theory the great violence of destroying it.

Should my reformulation be abandoned? Suppose that we were to introduce the class of *prima facie* contextual implications. A *prima facie* contextual implication of  $Q$  by  $P$  in  $C$  is an implication of a  $Q$  which is not a member of  $C$  from the union of the unit set of  $P$  and the set of some assumption in  $C$ , say  $\{R\}$ , such that  $\{P\} \cup \{R\}$  implies  $Q$  and neither  $P$  does nor  $R$  does; or there is an implication of  $Q$ , from the conjunction of  $P$  and  $R$ , where neither conjunct implies  $Q$ . The class of *prima facie* contextual implications is thus the union of the class of contextual implications got according to Sperber’s and Wilson’s formulation and the class of contextual implications, so called, got by way of my reformulation. *Prima facie* contextual implications thus comprehend both synthetic implications and their nonsynthetic counterparts.

If we stick with the Sperber and Wilson formulation of contextual implication, then we must say that *prima facie* contextual implications won’t serve for relevance. They fail the syntheticity condition. This is troubling. Take any pair  $\{X, Y\}$  of *prima facie* contextual implications, where  $X$  is a synthetic implication

and Y is a nonsynthetic counterpart. X and Y are logically equivalent. If we wish to reserve the name of contextual implication proper for those *prima facie* contextual implications that are also synthetic, we are met with the following question: why isn't contextual implication closed under logical equivalence?

Lots of things could be said. One might declare oneself a holist and explain that it is reason enough to reject the closure of contextual implication under equivalence that in so doing you spare the theory of relevance the nuisance of apocalypse. After all, many reflective and capable people think that *confirmation* is not closed under equivalence and that this is the interesting moral of the Raven Paradox. But, apart from what an undisciplined holism will allow you to do in principle with a theory under attack, I myself think it fair to say that rejecting closure under equivalence is not just there for the asking, especially when there is good independent reason for thinking that it should not be rejected. In the present case, the independent good reason is the demonstrable validity of the rules of adjunction and simplification. If adjunction and simplification jointly show, as they do, that the premiss-sets of our two counterpart *prima facie* contextual implications are logically equivalent, then one would think that since these are implications of a common conclusion from different but logically equivalent premiss-sets constructed out of the same propositional atoms, then from the point of view of their logic the two implications will be equally interesting. If the one is interesting by virtue of its possession of the property of contextual implicationhood, it would seem that the other would be made interesting in the same way. So it begins to appear that the move to reject closure under equivalence is *ad hoc* and slightly desperate.

If we are to refuse closure under equivalence, it is desirable that we find a reason for doing so other than that not doing so will wreck the account of relevance. It may be that human reasoners who add new beliefs or accept new commitments into their commitment stores do not in the general case bother to conjoin the new belief with any commitment already present. Though the rule of adjunction is a valid rule of deduction there may be evidence to suggest that, by and large, people don't avail themselves of it and, in particular, that they don't trouble with it when doing so would introduce a step unneeded for the success of the reasoning underway. A reasoner who did routinely employ adjunction could, perhaps, be said to be performing beyond the norm and somewhat less efficiently than the others. Perhaps he is a bit of a fuss-budget or maybe he just likes cashing his deductive entitlements, for the fun of it as it were.

Now suppose that this is right empirically, that is, that by and large people don't use adjunction. If that is how it does happen, it certainly gives no normative offence. This might lead us to say one or other of two things.

(A) Adjunction is not after all a good rule. The present logical equivalence claim collapses and the question of the closure of contextual implicationhood under equivalence no longer arises.

Objection (A) takes the high ground. (B) operates lower down.

(B) Logical equivalence aside, human beings do not in the general case behave as if

adjunction were a good rule (for they eschew its use) and this does their inferences no damage in the general case. In as much as the syntheticity constraint of Sperber and Wilson mimics the human reasoner's preference for inferential efficiency, though that might not be reason to reject the equivalence, it is a reason to reject closure under it.<sup>15</sup>

(A) raises the question of whether adjunction is a valid rule. Some paraconsistent logicians – da Costa and Rescher and Brandom, for example – argue that adjunction is not valid;<sup>16</sup> and that would seem to give some comfort to Sperber and Wilson. It is not enduring comfort, however. The disaffection with adjunction turns on a confusion. It is the confusion of implication with inference. What shows their conflation to be a mistake is that implication is monotonic and inference is not. Any valid implication  $A \rightarrow B$  remains valid under arbitrary augmentation of its antecedent:  $C, A \rightarrow B$  is valid for any choice of  $C$ . Such is not the case with inference, even deductive inference, so-called. A good inference “Since  $A$ ;  $B$ ” does not stay good under all such augmentations, as witness. “Since not- $B$ ,  $A$ ;  $B$ ”.<sup>17</sup> It may well be that adjunction is not an exceptionlessly good rule of inference, but denying its validity for implication is going too far.<sup>18</sup> Actually this is not quite right. In some paraconsistent logics, implication is *not* monotonic in the sense here described. But the logic underlying the Sperber and Wilson “deductive machine” is indeed monotonic, that is, monotonic for implication. If it were to their purpose to collapse the present distinction between inference (which is non-monotonic) and implication (which, classically, is monotonic), they would need to jettison their underlying logic in a non-trivial way. As things stand, they show no signs of wanting to do this; so the distinction between inference and entailment is preserved.

Even so, Sperber and Wilson might say that they possess all the motivation they need. If adjunction is not a correct rule of inference, certainly not as a non-optional rule, then it can be seen that our two counterpart *prima facie* contextual implications reflect non-equivalent inference strategies, one of which is at a minimum optional (and may not be universally correct), and the other of which is mimicked by the syntheticity requirement. And in so saying we defer to possibility (B). Contextual implicationhood can now be likened to a property of inference strategies, not of implications as such, and it seems that the closure question is answered negatively, since the inferential strategies are not equivalent.

We need to take care. *Which* inference strategies aren't equivalent? Let the schema

$A, B \vdash [?]$

represent a state of affairs in which an inferer is deciding what move, if any, to make from the input  $A, B$  (and it only). For the case at hand it suffices to consider three options.

- (a)  $A, B \vdash A \wedge B$
- (b)  $A, B \vdash \{A\} \cup \{B\}$
- (c)  $A, B \vdash [\text{nothing}]$

It might be thought that (a) and (b) are equivalent under the description, “putting A,B together” or some such thing, expressed by inessentially different notations. But this can’t be right. The locution “ $A \wedge B$ ” is grammatical in the context “Since A, B,...,” but “ $\{A\} \cup \{B\}$ ” is not. The dispute between the “conjunctive” formulation and the “set theoretic” formulation of the Sperber Wilson definitions is somehow misconceived. The real issue is between (a) and (c). Here there are clear differences, since (a) charts an inference and (c) does not; (c) represents an inference foregone, if you like. Logical equivalence also fails. For although there is no possible world in which (c) is a correct inference and (a) not (trivially), there is a possible world in which (a) is a correct inference and (c) not.

Don’t we now have motivation enough to deny closure under equivalence to contextual implicationhood, considered now as a property of inference strategies? No closure, because no equivalence.

It is worth noting that, whatever else we might think of it, this is a motivation that has no expression in the theory itself. Relevance, and contextual implication too, is defined over sets of propositions. The definitions involve no parameters about what goes on in an inferer’s head or in his “inference organ” wherever that might be. Although the canonical form of a synthetic implication might be said to, and clearly does, simulate what goes on in the inference organ of an efficient inferer in the general case, none of this is given formal definitive admittance into the theory itself. Relevance is not there a property of inference strategies; it is forwarded as a property, pure and simple, of propositional relationships. It is defined over the propositional elements abstracted from the din and swirl of inferential practice.

It is easy to see why Sperber and Wilson might not wish to bring to the front and centre the routines of human reasoners. There are costs to be borne. One is the risk of complicating the theory and of getting what you say about human reasoners wrong (an easy thing to do). The other is that you crimp the simple generality of the formal account of relevance. An attraction of these definitions is that P is relevant in a set of propositions C, then it is so timelessly, so to speak, independently of whether or not there are any reasoners or any inference organs, or ever were. The definitions promise simple generality, but they don’t yield the nomicity got from counterfactuality. P contextually implies Q in C just in case the union of {P} and {R}, for some R in C, implies Q and neither P implies Q nor R implies Q. It is nowhere formally specified that for a human reasoner, H, were C to be (part of) his present commitment store and were P added to C, as here indicated, than H would conclude that Q, or should. The necessary and sufficient conditions don’t provide for this, nor should they. Even if H were an ideal inferer, if R is not-P, then though  $\{P\} \cup \{R\}$  does imply arbitrary Q, inferring Q is precisely what H will not do.

Sperber and Wilson accommodate such a case differently. This is a case of commitment-contradiction and it calls for handling by procedures other than

contextual implication. Sperber and Wilson are describing an abstract model of cognitive optimality. We are to think of this model as programmed to respond to inputs diachronically and differentially. Some things are done first; others are done later or not at all. In the present case, the model will attempt the contradiction/erasure routine. Upon addition of  $P$  to  $C$ , where  $C$  contains  $R$  [ $\text{=not-}P$ ], the model will stop and then scrutinize  $P$  and  $R$  for strength and will reject the less strong if it can find it. And so on. What the model will *not* do is draw the offending contextual implication of arbitrary  $Q$ . The implication is true in the model, by the definition of contextual implication, and relevant in the model by the definition of relevance; but it is also *not* relevant in the model, since the model refused to draw it. So which is it?

The point at which you secure the benefit of simple generality afforded by the definitions is precisely the point at which, simultaneously, is lost the rationale for imposing a constraint the whole motivation of which is that it reflects how reasoners' minds work. Why impose it if the definition, thus constrained, won't tell, with consistency, when something is a contextual implication for a reasoner?

Perhaps it can fairly be said that Sperber and Wilson should not be encouraged to have it both ways. Either let the account proceed without giving formal expression to what reasoners do; or let it proceed the other way. If the former course is taken, simple generality is bought at the cost of motivating the synthetic constraint. If the latter course is taken, the motivation is revived, for now the definitions are part of a theory of inference, not implication, but the definitions misdescribe or underdescribe inferential performance.

If you do things the first way, the needed deductive apparatus is already supplied. It is given in the standard theory of deduction – the theory of implication. If you do things the second way, the needed deductive apparatus is not supplied; it doesn't chance just to be lying about. The theory of deductive inference needs to be produced, not borrowed – for there is little there to be borrowed.<sup>19</sup> My own view is that relevance wants theoretical development in something like this second way. I say "something like" in order to register a reservation about *deductively* inferential preoccupations in the theory of relevance (and also the theory of inference more generally).<sup>20</sup> I shall shortly take up the reservation, but just now I want briefly to raise two further objections against the Sperber and Wilson account as we actually have it, the account in which relevance is defined timelessly over proposition-sets.

One is that no proposition is relevant to anything it entails. For let  $P$  entail  $Q$ . Then for any context  $C$ ,  $\{P\} \cup C$  entails  $Q$ , but since  $P$  alone already entails it,  $P$  is not relevant to  $C$ , for any  $C$ . Though there is no reason to think that this is an unintended consequence of their account, it is, I think, strikingly counterintuitive.

The second worry is that the account provides that, for a great many propositions  $P$ ,  $Q$  if  $P$  *explains*  $Q$ , then  $P$  cannot be relevant to  $Q$ . Let  $Q$  be a proposition stating the facts of radioactive decay, and let  $P$  be the Schrödinger equations. It is widely believed that since the derivation of  $Q$  can be got from  $P$  then  $P$



qualifies as a good explanation of Q. (It is a further question whether the putative referents of P, the theoretical entities of the Schrödinger equations, actually exist, but this is an inessential complication here). Although P may be said to explain Q in C, it cannot be said to be relevant to it, for any C in which Q already appears, since wherever Q does appear it trivially implies itself.

I ask now if an account of relevance were to take formal notice of how human reasoners operate, wouldn't it be unduly restricted if it confined itself to the routines of deductive inference? We might test this question by asking whether it would be reasonable to hold the theory to a further condition of adequacy:

(AC2): For any response fulfilling Grice's maxim, "Be relevant", the truth of the assertion, "That response was relevant" is preserved under the translation of the word 'relevant', therein, into the theory's semantic provisions. That is, "That response was relevant" will remain true under the theory's interpretation of 'relevant'.

Sperber and Wilson might protest. What justifies the decision to hold the theory of relevance to *Grice's* account of it? In Grice's work, "[e]ssential concepts are left entirely undefined. This is true of *relevance* for instance: hence appeals to the 'maxim of relevance' are no more than dressed-up appeals to intuition".<sup>21</sup> Except for the 'dressed up' part, I agree with this entirely and, more importantly, so does Grice.<sup>22</sup> In no sense does what Grice produced in "Logic and Conversation" in 1967 qualify as an account, an analysis or a theory of relevance. So there can't be any question of Sperber and Wilson having to conform their theory to Grice's. The object of AC2 is not to get Sperber and Wilson to make Grice happy.

Grice appeals to intuition. Is this objectionable? Are Sperber and Wilson saying that it's objectionable? It is not clear, but it is a good question.<sup>23</sup> It is true that AC2 turns on intuitions in a modest way. It invites judgements about what we would find it natural to characterize as relevant responses in certain contexts. (*Not*, "Responses relevant according to the relevance theory of H.P. Grice"; rather responses "relevant according to the intuitions of Dan Sperber and Deirdre Wilson, John Woods and Joe Blow" – and these are intuitions about the relevance of responses, not about the meaning or analysis of the word 'relevant'. So I am not persuaded that AC2 is an indefensible check to impose on a theory of relevance.)

Every theory of relevance that I know of fails this condition. That fact alone should give us pause, since it is some evidence that the condition may be unrealistic. We might lower our sights and proclaim AC2 a desideratum. In any event, I now want to show that the Sperber and Wilson account fails it, whatever it is.

Here is a case. Harry stops Peter in the hallway of a building and asks, "Where is the office of the Rector, please?" and Peter replies, "You're standing in front of it." Did Peter fulfill the maxim, "Be relevant"? I cannot easily imagine that he did not. But it is easy to see that "You're standing in front of it" doesn't qualify as relevant in the sense of Sperber and Wilson. It carries the needed information all by itself. AC2 is failed. (But see just below on strengthening.)

Another case. Harry is helping with dinner. His task is to condimentize and toss the Neopolitan spaghetti. Sarah hands him the beaker of measured olive oil. She didn't hand him the corkscrew or the distributor cap from the Porsche. Did her response fulfill the maxim? Was what Sarah did, as opposed to what she didn't do, the relevant thing to have done? I don't doubt that it was. But there is no way of preserving that intuition in the theory of relevance. It can't even be expressed there.

Someone might say. "The expectation of AC2 is unrealistic; it is too much for any single theory of relevance to bear". I say: produce the appropriate limitation theorem, and then we'll see.

We might also propose to hold theories of relevance to further conditions. One such is AC3 which is just like AC2, except that instead of speaking of judgements of compliance with Grice's maxim, here we speak of honoring the van Eemeren and Grootendorst relevance conditions on appropriateness of response in conversations of conflict resolution. (Here, too, these are not conditions on the analysis of relevance; they are conditions on the appropriateness of responses.) One such rule is "stage relevance": make your contributions in such a way that they are appropriate for the stage at which the conflict resolution exercise presently rests.<sup>24</sup>

Peter and Harry are arguing about the theory of evolution, Peter pro and Harry contra. Peter opens. He briefly describes the evolutionary account and gives some indication of its attractions. Harry follows with a brief account of creationism and presents some reasons for liking it. Peter now says, "Okay. You win. Creationism is true". Here stage relevance is utterly failed (under all reasonable assumptions). Peter's concession comes at the wrong place in the argument.

It was, as we say, not a relevant move to make at that juncture. But relevant it is for Sperber and Wilson, since Peter's concession corroborates something already in Harry's commitment-store, *provided* that Sperber and Wilson will allow for corroboration as strengthening. Corroboration is a difficult notion,<sup>25</sup> but if ever we did allow that P, as uttered by me, counts as some corroboration of P as held-true by you, then P would be relevant to P on the Sperber and Wilson account. But, on the contrary, I think that relevance is decidedly anti-reflexive. Sperber and Wilson think so, too, or imply that they do. But they derive this result at high cost. They do not allow that corroboration *is* strengthening whenever in the form "X said that P; and Y also said (or holds) that P."<sup>26</sup> And this seems to me to be over-severe.

Further trouble announces itself. Any denial of an opponent's thesis at any stage of an argument always begets Sperber-Wilson relevance. Let C be any context containing P. Then the union of {not-P}, the opponent's denial, and {P} is an inconsistent set and requires "erasure". (This is too strong a consistency requirement for human reasoners, but let that pass). But the whole point about disputes is that denying an opponents thesis erases neither it nor your own. Denials made by me do not, just so, get lodged in my opponent's commitment store if the proposition denied is already there. Saying so does not make it so.<sup>27</sup>

In the end, the timelessly propositional formalities of Sperber and Wilson leave unsuccessfully motivated their single most important constraint with the near apocalyptic consequence that almost nothing is relevant to anything. On the other hand, a theory like theirs which did motivate the constraint would need to be, among other things, a theory of deductive inference beyond the mere fragment of it that underwrites that particular constraint. Yet as it stands, the Sperber and Wilson account is a theory of deductive inference, which mis-manages its own definition of relevance and contextual implication. Even if it were not so, and we were able to read the pages of *Relevance* as supplying or sketching an adequate theory of deductive inference, it would fail AC2 and AC3, and so it would undershoot reasonable targets for a theory of relevance (again, justification of the low aim needs a limitation theorem). Finally, the theory as it stands produces further consequences too counterintuitive for comfort.

The work of Sperber and Wilson is certainly nothing to be grouchy about. If I have given the contrary impression these past several pages, I disavow it now. Sperber and Wilson have broken new ground in important and lasting ways; they stepped forward when others wouldn't or couldn't. The failure to make relevance a high priority for theory has been a disgrace, especially in philosophy.

As I say, it is not that I think that the views of Sperber and Wilson are fundamentally wrong; in fact they are fundamentally right in their primary insight: information is relevant when it invades a context and matters there or, to subdue the circularity of 'mattering', does some work there.

Relevance, their way, is precisely three kinds of operation that a deductive device is taken to perform: responding to contextual implications, to strengthening and to contradiction. The deductive device in its full operation yields a description of communication and cognition. But the relevance-operations are, I fear, all too few and too much of a single "logical" type for the size of the large task at hand. That is one worry. Another is that the necessary and sufficient conditions imposed on the relevance-procedures seem not to be honoured by the cognitive device. This is not a refutation of those definitions. They could still be held as procedural ideals which the device will have difficulty with; and the longer and more interesting story of greater or less fidelity to them could be told in the larger theory of communication and cognition. At the end of the theoretical day, perhaps one could adjust the necessary and sufficient conditions. But done this way, the account of relevance is top-down: Say a limited bit about relevance, then let it loose in the broader theory of communication and cognition, and see how it fares there. My preferences are for a more bottom-up approach. This means seeking to collect a very wide range of what one is prepared to take as relevance-phenomena in the life of communication and cognition, and then see if they will collect under a more or less unified theoretical paradigm. If they do so collect, then you'll end up with a richer account of relevance itself, which is just what a theory of relevance should aim for.

Whether top-down or bottom-up, the approach must in any event make some sense of what goes on when we communicate and cognize. The two approaches can be expected to crisscross at numberless junctures, and they do.

In the end it would appear that *Relevance* is not primarily (or even much of) a theory about relevance, notwithstanding the authors' testiness about Grice's leaving the key notion of relevance all "dressed-up" but well short of a theory. What Sperber and Wilson actually said was that Grice didn't define relevance. They do define it. But in doing what they have done, they haven't done what Grice acknowledged rather wistfully he hadn't done either. This was to produce for relevance an analysis or, as I would prefer, a theory, although theories like to have analytical definitions when they are available.

It is important to emphasize that in not giving a theory of relevance, I am not saying that Sperber and Wilson have given only a one-and-a-half line definition of it and have then moved on to other things. No, the definitional linkages are more substantial than that and it is also correct to say that ultimately the whole enterprise *is* about relevance. But how can the whole enterprise be about relevance and yet not be a theory of relevance? It can happen this way. Suppose that you wanted to make good on something else than Grice didn't do, and acknowledged that he hadn't done. This is to produce a *compliance model* for Grice's four maxims. In a compliance model, devices are specified enabling the Grice co-operator to discern what a speaker has said, to determine how he (the hearer) is to solve the interpretation problem, especially the problem of what additional information he (the hearer) must furnish and to whom he must attribute it, and what inferences, including "conversational implicatures" he needs to draw.<sup>28</sup> Now it is also true that Sperber and Wilson attempt to show that Grice's four maxims reduce to one, namely "Be relevant", though their "Be relevant" will be richer than his. But the account that they give is not an analysis of relevance, except sketchily and by the way; it portends an account of what you must do to obey the maxim. Their enterprise reminds me of the relevant logicians of yore. Some people thought that they were giving an analysis of relevance. They were not. They offered propositional variable-sharing as a necessary condition on relevance, which in turn was offered as a necessary condition on entailment. The relevance of relevant logic didn't know the orderly comforts of definition. Relevant logicians didn't give the toss of a button about relevance. What they cared about was the classical theorem that a contradiction entails everything. They abjured this theorem and sought technical relief. Whereupon "relevance" logic (or as some would say, relevance "logic").

Of course, the similarity with what Sperber and Wilson are up to is not strong. You can learn nothing about relevance from relevance logic, and you can learn lots more than that from Sperber and Wilson. You could learn more still if it were true that something like a successful compliance-model for "Be relevant" counted as an implicit definition or analysis of the concept of relevance. Such is an interesting and broadly contested idea as such, but I am prepared to concede the possibility. Even so, there is much more to relevance than Grice's relevance, as, once again, Grice himself admits. Another way of saying this is that AC2 is

at best only a necessary condition on the analysis of relevance.

I want to see if theory can do more, but this is for another time.<sup>29</sup>

## NOTES

<sup>1</sup> This paper is detached from a much larger work in progress which was presented in summary fashion, under the title "Agenda Relevance", at the Conference on Relevance, McMaster University, June 1991. This part arises from an ISSA Lecture at the University of Amsterdam in March 1990. For their valuable comments, I thank Francisca Snoek Henkemans and Tjark Kruijger in Amsterdam and Ralph Johnson at the McMaster Conference.

<sup>2</sup> See, for example, John Woods, 'Sunny Prospects for Relevance?'

<sup>3</sup> *Relevance*.

<sup>4</sup> *Relevance*, p. 122.

<sup>5</sup> *Ibid.*, pp. 108–115. For example, contextual implication generalizes to strengthening via synthetic implication (p. 108) and contradiction and erasure are handled by strengthening and synthetic implication (p. 114). These matters are discussed below.

<sup>6</sup> *Ibid.*, pp. 107–108. Here, too, I deviate. Sperber and Wilson speak not of the conjunction of P and C, but of the union of the unit set of P with C, which itself is a set. I return to this point shortly.

<sup>7</sup> *Ibid.*, p. 97.

<sup>8</sup> *Idem.*

<sup>9</sup> *Idem.*

<sup>10</sup> For ease of exposition, I reinstate the triviality idiom.

<sup>11</sup> *Op. cit.*, p. 109.

<sup>12</sup> *Ibid.*, p. 104.

<sup>13</sup> *Idem.*

<sup>14</sup> I admit to being unclear about the definitional likeages among contextual implication, strengthening and contradiction/erasure. Perhaps the objection should be softened. If contextual implication goes then the heart of the account of relevance goes with it. This doesn't yield apocalypse, but it is a near thing in any case.

<sup>15</sup> Thus there *would* be difficulties "if we were trying to develop an optimal *logical* system. But we are actually trying to model a *cognitive* system". *Relevance*, p. 111.

<sup>16</sup> Newton C.A. Da Costa, 'On The Theory of Inconsistent Formal Systems'.

<sup>17</sup> Non-monotonic "logics", which often make the same confusion, are a big part of the AI research programme. See the special issue of *Artificial Intelligence*, 13 (1980). Early non-monotonic insights can be found in Michael Scriven's discussion of "normic" statements. See 'Truisms as the Grounds for Historical Explanation', in Patrick Gardner (ed.), *Theories of History*.

<sup>18</sup> It might be said that if there are cases in which non-monotonicity considerations did cast doubt on adjunction, they would not cast doubt on if-augmentation via set-theoretic union, and that this might be further reason to approve the Sperber and Wilson syntheticity constraint. No, both devices stand or fall together in the theory of implication.

<sup>19</sup> There is, to be sure, a burgeoning literature. In addition to the work on non-monotonicity, already cited, there has been a productive flurry in psychology. To date more issues have been raised than settled. See, for example, P.N. Johnson-Laird: 'Reasoning Without Logic', in T. Myers *et al.* (eds.), *Reasoning and Discourse Processes*, and the references therein.

<sup>20</sup> There is a great deal of useful discussion of matters other than deductive inference in Sperber and Wilson. Their book is appropriately sub-titled, "Communication and Cognition". It may well be that much of what they say about these other things could be

integrated with a theory of relevance. But as it is presently set up, it is unsurpassing difficult to see how they could get into the theory.

It is also true that the theory has a lot to say about the computation of confirmation. But this is done by the system's "deductive device". *Relevance*, pp. 110–111.

<sup>21</sup> *Relevance*, p. 36.

<sup>22</sup> H.P. Grice: 1975, 'Logic and Conversation', in Peter Cole and Jerry L. Morgan (eds.), *Syntax and Semantics*.

<sup>23</sup> For a discussion of the role of intuition in analytic philosophy, see L.J. Cohen: 1986, *The Dialogue of Reason*.

<sup>24</sup> van Eemeren and Grootendorst, 'The Relevance Problem in the Analysis of Argumentative Texts'.

<sup>25</sup> See George N. Schlesinger, 'Why a Tale Twice-Told is More Likely to Hold'.

<sup>26</sup> *Relevance*, p. 109.

<sup>27</sup> Again, compare the definition. P has the contextual effect of commitment-contradiction in C iff for any  $Q \in C$ ,  $\{P\} \cup \{Q\}$  is an inconsistent set.

<sup>28</sup> Sperber and Wilson, 'Mutual Relevance and Knowledge in Theories of Comprehension'.

<sup>29</sup> Woods, "Agenda Relevance".

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