

Phatic communication and Relevance Theory:

a reply to Ward & Horn*

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1. Introduction

In Žegarac & Clark (1999) we try to show how phatic communication can be explained within the framework of Relevance Theory. We suggest that phatic communication should be characterized as a particular type of interpretation, which we call ‘phatic interpretation’. On our account, an interpretation is phatic to the extent that its main relevance lies with implicated conclusions which do not depend on the explicit content of the utterance, but rather on the communicative intention (where ‘depends on X’ means: ‘results from an inferential process which takes X as a premise’). For example:

(1) *Situation*: Arthur and Pauline have quarrelled, and have not spoken to each other for two days. Over breakfast, Arthur says to Pauline:

There's a red gas bill.

In this situation, Arthur's utterance implicates that he is no longer as angry with Pauline as he had been. This implicature is a conclusion which crucially depends on the fact that Arthur has spoken to Pauline (i.e. on his communicative intention), rather than on the meanings of the words he has used. Thus, even if Arthur had said something like: ‘I don't want to speak to you ever again’, Pauline might still have taken the fact that he has addressed her as evidence of some degree of willingness on his part to make up with her (perhaps as an indirect invitation to make the first direct move towards a reconciliation). Our detailed analysis of a few clear-cut examples draws on the Relevance-theoretic notion of (mutual)

manifestness, which is defined in terms of degrees of disposition for belief representation, and on assumptions about the role of processing effort in utterance interpretation. We argue that this approach provides a good basis for explaining the following intuitive observations about phatic communication: (i) what the speaker has said matters less than the fact that something has been said; (ii) utterances can be more or less phatic; (iii) phatic interpretations become more likely when the social relationship between the interlocutors is in doubt.

Ward & Horn (W&H hereafter) are critical of our characterization of phatic communication. Allegedly, our methodology is non-empirical, our account relies too heavily on (our own) intuitions, our characterization of phatic interpretation is circular, we follow Relevance Theory in taking an omniscient view of communication, we reject the role of conventionalization out of hand (only to introduce it in disguise), and we are guilty of parochialism. W&H also claim that we characterize the phatic use of language as a form of covert communication, although, by defining phatic interpretations in terms of implicatures (i.e. implications which the speaker evidently intends to communicate) we explicitly reject this position. We want to clarify our approach and to explain why we disagree with their criticisms.

What is it that makes certain acts of communication tokens of a particular type of communicative behaviour, namely phatic communication? This question can be tackled in various ways. For example, phatic communication can be described by identifying systematic correspondences between (i) particular situational settings (e.g. having a casual conversation at a bus stop), (ii) aspects of linguistic (and paralinguistic) forms of communicative acts (e.g. the use of conventionalized expressions about particular topics, say, the weather) and (iii) the social function(s) of those acts in those settings (e.g. to avoid silence, to establish and maintain a good atmosphere of sociability, and so on). We do not dispute the usefulness of exercises of this sort, but we have some doubts about their explanatory value. The problem with existing accounts of phatic communication is that there is no logical argument going from a particular theory of communication to an

explicit and detailed characterization of this type of communicative behaviour. W&H seem to believe that such a theory can be derived on the basis of descriptions of the interlocutors' directly observable actions. They are wrong for two reasons. First, the assumption that theories can be derived from descriptions of data via inductive generalization is false, and it is well known to be false. Quite some time ago, Chomsky (1957: 49-60) famously remarked that the idea of a discovery procedure which takes the researcher from raw data to a theory of language, is unfounded and without precedent in other sciences. More recently, Searle (1996: 5) has pointed out that it is not possible to describe social institutions from an external behaviourist point of view, because the description of overt behaviour misses the underlying structures that make the behaviour possible (see Žegarac 1998). Second, the data for pragmatic analysis are not the directly observable features of a type of communicative behaviour, but rather, interpretations of observable behaviour. Let us expand on this. Human communicative behaviour is best explained in terms of causal chains consisting of some public representations (i.e. acts of communication which can be observed and interpreted) and some private representations, that is, thoughts and inferences, which are not observable, but which cause, and are caused by, the observable public representations (see Sperber 1996; Sperber & Wilson 1986/95). Now, W&H do not take issue with this view, but they do not seem to be aware of its implications for pragmatic research. This view entails that it is the interpretations of observable behaviour which are the data for pragmatic analysis. Since interpretations cannot be divorced from intuitions, the latter should play a major role in pragmatic research, rather than being banned from it.

2. The Data

W&H's main claim — that our methodology is non-empirical — rests on two ideas: (i) that our account is not based on a representative collection of naturally occurring data, and (ii) that it is non-falsifiable for a whole range of reasons. They seem to think that phaticness can

only be investigated fruitfully by first describing and classifying a vast range of (naturally occurring) data. They might have a case if phatic communication were not an already established and widely accepted technical term. But it is: pragmaticists, social psychologists, anthropologists and ethnographers of communication do not spend much time arguing about whether particular exchanges are, or are not, instances of phatic communication. Moreover, phatic communication is a rather intuitive technical term: one does not need years of training to develop a feel for distinguishing between phatic and nonphatic exchanges. For example, the (naturally occurring) data in (2) to (4) are easily recognized as phatic by anyone reasonably familiar with this term:

- (2) Vlad is jogging. Kostas, an acquaintance of Vlad's, is running in the opposite direction. They recognize each other, and as they approach, Kostas says: *Hi, how are you?*, without showing the slightest sign of slowing down. Vlad replies: *Hi*. They keep jogging (still in opposite directions).
- (3) Several people (who have never met before) have been waiting at a bus stop in North London for about twenty minutes. One of them walks some distance up the road to see if there is a bus coming. He then rejoins the others and says (facing another person who is also waiting impatiently): *No sign of a bus. I suppose they'll all come together*. She replies: *Oh yes. They travel in convoys*.
- (4) Peter and Mary are colleagues and they often come to work early in the morning. Peter usually makes lemon and ginger tea first thing in the morning and offers it to Mary. This has become a matter of habit, a kind of tacit convention between them. On Tuesday morning Peter says to Mary: *I've made tea. Would you like some?*

By virtue of what are (2)–(4) intuitively judged to be instances of the phatic use of language? Do they share some directly observable features with other acts of phatic communication? What might those features be? Certainly, (2)–(4) are a mixed bunch: (2) is an exchange of conventionalized greetings between people who are only superficial

acquaintances; (3) is a brief conversation between strangers brought together by a feeling of frustration at the length of time they have been waiting for a bus; and (4) is an offer of tea which has become a convention between two colleagues. It seems that what brings together acts of communication which are identified as phatic are not the resemblances between the public representations which instantiate them (i.e. descriptive similarities), but rather resemblances between the private, mental, representations which are among the causes and among the effects of those public representations (i.e. interpretive similarities). Hence, if people generally find the term phatic communication easy to grasp, it is because their intuitions latch onto the interpretations that certain acts of communication have for those who take part in them. W&H's remark that 'even traditionally-defined phatic communication (à la Malinowski) represents such a vast and disparate range of data that it would be extraordinary to find that it could be reduced to a corollary of R[elevance] T[hory]' (p. 556) misses this important point. What makes phatic communication a recognizable use of language is precisely the resemblance between the patterns of interpretation that underlie a 'vast and disparate range of data'. Therefore, an explanatory insight into phatic communication cannot be gained by looking at the directly observable data: they are as disparate as they seem. What is needed is an explicit characterization of the interpretive resemblances that bring the data together. Our attempt to provide such an account is located within the framework of a general theory of communication and focuses on a few clear-cut examples. Our approach does not attempt the reduction of a vast range of data to a corollary of Relevance Theory, but it does provide a way of reconciling the intuitive unity of phatic communication with the diversity of its manifestations.

W&H point out that our analysis is based on an unrepresentative sample of data, but they do not give any indication of what a representative sample might be, and they do not mention any criteria which could be used as guidelines in making a decision on the size of a representative corpus. Their objection echoes an all too familiar observation found in data-driven analyses of communicative behaviour. Consider the

following quote from a study of phatic communication:

Phatic discourse is too complex a phenomenon to be dealt with in only one book. Much has remained unsaid about it, many details have only been touched upon and require further investigation. Future research needs to concentrate particularly on the induction of other situational frames from instances of naturally occurring data. It seems desirable, for example, to gain more insight into party talk for a more comprehensive understanding of small talk. (Schneider 1988: 288)

Schneider's conclusion about the need for collecting and describing more data reads like a promissory note, and one which we have no reason to believe could be fully honoured. His data-based study fails to provide a coherent and theoretically well-motivated characterization of phatic communication, and, in the absence of a general theory of human communicative behaviour, a description of party talk will remain just that: a description of party talk. We believe that it is better to use a general theory of communication in order initially to explain a few clear-cut examples of a particular type of language use. If the account is plausible, then it ought to accommodate a vast range of other examples, and give rise to predictions which can be tested. Clearly, W&H do not find such an approach acceptable, but they also argue that our account of phatic communication is unfalsifiable for a number of other reasons.

3. Reliance on intuitive judgements

If we are right in claiming that phatic communication is an interpretive (rather than a descriptive) term, then the data for testing our analysis must invoke intuitions about interpretations of particular (phatic) exchanges. Therefore, all work in this field, including Ward's and Horn's, must rely heavily on intuitions. Interpretations are independent of, and evidentially stronger than, our conceptual account of phatic communication. Consequently, they can be used to test the analysis in a fairly straightforward way. If our characterization of phatic communication is right, it should be the case that for every act of communicative behaviour which is intuitively (i.e. pretheoretically)

categorized as phatic, its interpretation will be found to include a large proportion of implicated conclusions which do not depend on the explicit content of the utterance (what we call 'phatic implicatures'). A straightforward way to falsify our analysis is to find an example of a conversation which most or all people would consider phatic, but whose main relevance does not lie with phatic implicatures.

It is worth noting that our approach should be able to explain cases in which it is not intuitively clear whether an exchange is phatic. For example, consider what goes on when two or more people are being introduced:

(5) By chance, on his way home Billy meets a friend who is walking with a person he does not know. Billy's friend Jason says: *Hi, Billy. Have you two met before?* Billy: *Hi, I don't think so.* The 'stranger': *No.* Jason: *This is Mat. He teaches in the Philosophy Department. Billy is in Communication Studies. I thought you'd met before.*

(6) Just before a business meeting, A introduces two people from the company hosting a small group of businessmen from another company. A: *Good morning. This is Dr X, the vice-chairman of our company, and this is Mr Y, his deputy....*

The exchanges in (5) and (6) have some features often associated with phatic conversations. For example, what goes on is primarily socially significant, and the purpose of the introduction is to ease further communication between the parties being introduced. However, other features of phatic communication are conspicuously absent. For instance, typical phatic exchanges are not highly informative, but these clearly are. The linguistically communicated information in (6) is particularly important: much about communicating in a business meeting follows from knowing the status of the interlocutor. By contrast, in informal introductions, illustrated in (5), the main relevance may easily lie with phatic implicatures: people who have been introduced will feel more free to engage in further conversation, even if they have failed to take notice of

the name, profession, etc. of those to whom they have just been introduced (i.e. even if they have failed to pay much attention to the linguistic meaning of the utterance). What often distinguishes introductions in a business meeting and those in informal situations is the fact that phatic and non-phatic implicatures are more evenly balanced in the business meeting setting than in a casual encounter. Hence the pre-theoretical intuition that introductions in business meetings are somewhat less phatic than introductions in casual encounters. Examples like (6) present difficulties for descriptive studies of verbal interaction. The problem for the researcher who drew this type of example to our attention (Helen Spencer-Oatey) was precisely to spell out the insight – based on observation and description of naturally occurring data – that certain types of exchange appear to have interpretations which are simultaneously both phatic and non-phatic. Note that this observation is independent of, and therefore evidentially stronger than, our conceptual analysis of phatic communication. A desirable consequence of this is that observations based on the description of naturally occurring data can be used in testing our characterization of phatic communication. These examples show that we are in a position to give a natural account of the phatic and the non-phatic aspects of a given exchange. W&H dispute this:

Presumably the correct theory of pragmatics will not be determined by reflecting about how people process and interpret language ... ; it will require investigating what people actually do, in both naturalistic and experimental settings. (p. 560)

It is extremely naive to believe that ‘the correct pragmatic theory’ can be developed in the way suggested by W&H. They seem to assume that decisions about which of the communicators’ actions should be observed, and even decisions about experimental design, can be made on objective, pre-theoretical, grounds. It would be useful if W&H had given some examples of theories whose development has followed the path that they recommend for pragmatics. Of course, experiments and descriptions of naturally occurring data may lead to new theories, but they usually do

so when they are used to test existing ones.

It is particularly disappointing that W&H's argumentation falls so dramatically short of the standards that they set for others. For example, they take issue with our report of Laver's (1974) claim that 'speakers may only comment on, or inquire about, attributes of the hearer if they are social "equals" or "superiors"' (Ž&C, p. 340). W&H claim that this is 'entirely unsupported – and quite likely unsupportable – by either data or intuition ... If anything' they suggest, the opposite 'might be argued' (p. 560). First, our main concern here is not about whether W&H are right or wrong, but that they are prepared to advance arguments based only on their intuitions, while criticizing us for relying on ours. Second, although W&H object to our use of 'hedges' which they claim reduce our arguments to near vacuity, they use them quite freely themselves (e.g. 'quite likely', 'might be argued'). We return to our use of hedges in section 6.

4. Conventionalization

We have pointed out that, in principle, phaticness is independent of conventionalization. W&H took this to mean that we reject conventionalization as being irrelevant in analysing acts of phatic communication. But this is not what our claim means. Our point is that utterances may be interpreted as phatic even when their phaticness is not conventionalized. Moreover, utterances which are conventionally phatic may have non-phatic interpretations in some contexts. (It may also be worth noting that, intuitively, the expression 'conventionally phatic' is not pleonastic.) Therefore, phaticness needs to be explained independently of conventionalization. This does not amount to rejecting the importance of conventionalization out of hand. Let us consider the utterance in (4) in a different setting:

- (7) On Monday, A and B were in a meeting and had a heated argument. They haven't spoken to each other since then. On Tuesday morning A says to B: *I've made tea. Would you like some?*

On any characterization of phatic communication, the offer of tea in (4) is a phatic act. It is an expression of the speaker's generally favourable social attitude towards the hearer, rather than being motivated by the speaker's concern about the hearer having a sore throat, suffering from dehydration, and so on. Since the offer of tea has become a matter of habit, it is also conventionally phatic. Thus, if one morning B did not mention tea, it would be reasonable for A to wonder why B is not behaving in the usual way. On our account, A's utterance would be phatic in (7) (as well as in (4)): in this situation, the fact that A has initiated communication with B is far more relevant than the explicit content of A's utterance. Moreover, in (7), A's utterance would have a phatic interpretation regardless of whether there was an established convention between A and B in place. In fact, most utterances would be phatic in this situation. Thus if A were to ask B for a favour (by saying something like: 'Could I borrow your tippex?'), his utterance would still be phatic in the situation in (7) (but not in (4)).

The main point is this: W&H claim that only conventionalized phatic utterances should be considered phatic. If this were true, then (7) could not be considered phatic. Only A's utterance in (4) would be phatic, because in this case the fact that the offer of tea is conventionally phatic contributes to the way it is understood. On our account, the utterance is phatic both in (4) and in (7). We agree that it is possible to restrict the use of the term phatic to conventionalized phatic exchanges, but we believe that doing so comes at some considerable cost both to the description and to the explanation of the relevant data. On the side of description, the decision to restrict the term 'phatic' to conventionalized (types of) utterances/exchanges circumscribes rather narrowly the range of data that descriptive, empirical research is likely to focus on: the very possibility that some non-conventionalized utterances/exchanges might have a bearing on an account of phatic communication is ruled out by stipulation. On the side of explanation, possibly significant generalizations which hold over both conventionalized phatic utterances and others which are not conventionally phatic are bound to be overlooked. We have argued that such generalizations exist and we have

tried to show how they could be expressed in Relevance-theoretic terms. W&H's claim that, by proceeding in this way, we reject the notion of conventionalization out of hand is at best puzzling.

5. Manifestness

W&H criticize (our use of) the notion of manifestness on two counts. First, they wonder who is to decide what is manifest to whom. Well, assumptions are manifest to individuals to the extent that those individuals are capable of representing them mentally and of entertaining their respective representations of those assumptions as true or probably true. For example, W&H's response to our article provides both of us (and anyone else who reads it) with fairly conclusive evidence for the belief that they do not like our analysis of phatic communication. Second, W&H also point out that what may be sufficient evidence for one person may well not be for another. This is certainly true. Thus, their reply to our article makes some assumptions manifest only to one of us (e.g. only one of us finds in what they have to say sufficient evidence for the assumption that they are opposed to theoretical pragmatics in general). But neither of us is able to figure out why W&H think that this fact (i.e. that the same environment may make different sets of assumptions manifest to different individuals) is a problem for a theory which adopts this notion of manifestness, let alone why they think that it might entail an 'omniscient' view of communication. Second, the following argument emerges from their discussion:

- (i) [W&H wrongly assume that] mutual manifestness is the same thing as presumed shared knowledge.
- (ii) [W&H rightly assume that] Ž&C claim that assumptions can be manifest without being mentally represented.
- (iii) [W&H wrongly assume that] therefore, Ž&C contradict themselves, because all knowledge must be mentally represented.

Our square brackets make clear that this argument is flawed. We confine ourselves to pointing out only the most blatant mistakes. First, people often act on beliefs which are not true, but the term 'knowledge'

applies only to true beliefs (compare the expressions 'false knowledge' and 'false belief' – the former is technically contradictory, the latter is not). The Relevance-theoretic notion of manifestness is intended to apply to assumptions (i.e. representations capable of being true or false), and cannot be reduced to knowledge. Second, the notion of mutual manifestness was developed by Sperber and Wilson in an attempt to overcome problems with the notion of mutual knowledge. One of these problems is that mutual knowledge entails infinite regress of the type : each of two (or more) people knows that the other knows that the other knows that the other knows, and so on ad infinitum. An assumption is said to be mutually manifest to two (or more) people to the extent that each of them is capable of representing mentally the fact that the assumption in question is manifest to both (or all) of them. For example, consider what might happen when a meeting is cancelled and all the people who were originally invited to attend are sent a memo to that effect. The fact that a memo has been circulated may make the information about the cancellation manifest to all the individuals concerned to varying degrees. This information is very manifest to anyone who has read the memo. It is less manifest to anyone who has not yet read the memo but has glanced at it and noticed that it has something to do with the cancellation of a meeting. It is even less manifest to anyone who has simply noticed that a memo concerning the meeting has been circulated. Let us assume that everyone concerned has read the memo. On what grounds might one of these people decide whether to tell the others about the meeting cancellation? Clearly, this will largely depend on the extent to which it is manifest to her that the others have read or will read the memo. What matters here is not that each person should have evidence of the meeting having been cancelled, but that they should all have evidence of the others having evidence of its cancellation. Of course, the memo may contain information about which people it has been sent to, thus providing evidence about which people the meeting cancellation is likely to be manifest to. In this case, the memo itself contains information intended to make it more likely that its content will become mutually manifest to all those who receive it. The distribution of the memo has altered the

extent to which those who have paid attention to it are disposed to believe that they share the information in the memo with other people. Clearly, the term presumed shared knowledge is, at best, a fairly loose, informal equivalent of the Relevance-theoretic notion of mutual manifestness. Third, W&H claim that knowledge entails mental representation. Following Sperber & Wilson (1986/95) – and the vast literature on the subject which we do not cite, but which W&H do – two ways of using the term KNOWLEDGE can be distinguished: (a) the strong notion of knowledge, which does entail mental representation, and (b) the weak notion of knowledge, which subsumes all the things one is capable of representing mentally on the basis of already held true beliefs. For example, only in the weak sense of the term ‘knowledge’ can most of us be said to know that William Shakespeare never used a laptop computer. Although this is not an assumption which many people have reason to represent mentally, it is one which follows conclusively from what many of us know (in the strong sense). We find it surprising that W&H criticize Relevance theorists for not paying much attention to the (philosophical) literature on the problem of mutual knowledge. Sperber & Wilson (1986/95) discuss this problem in much detail, and Sperber & Wilson (1990) consider their notion of (mutual) manifestness in the context of other work on human cognition.

6. Vacuity, Circularity and Vagueness

According to W&H, we hedge virtually all substantive claims we make to the point of near vacuity. Some of the examples they cite to corroborate this criticism are obviously not to the point. For instance, when we claim that ‘The question *How are you?* is USUALLY not used to show the speaker's genuine interest in the hearer's welfare’ (Ž&C, p. 337; W&H, p. 560) we do not compromise our analysis. We merely take account of something that should be obvious: that the expression *How are you?* is SOMETIMES used to show genuine interest in the hearer's welfare. Other examples that they give may seem more

convincing: ‘But, in this context neither the phatic nor the non-phatic interpretation is LIKELY to be very salient’ (Ž&C, p. 332; W&H, p. 560). Our reason for including the hedging expression in this and other examples is that we are making observations about performance, rather than competence. Thus, a syntactician who points out that the string *The sat mat cat on the* is ungrammatical need not (and should not) use hedges such as ‘likely’: the ungrammaticality of *The sat mat cat on the* does not depend in any way on the circumstances in which this string is uttered. Of course, syntacticians also need to use words such as ‘likely’ when they talk about what speakers will or will not say. A pragmaticist dealing with the interpretation of actual communicative behaviour may be justified in using hedges like ‘usually’, ‘likely’, ‘typically’ and a few others, in order to indicate that the actual interpretation of a communicative act is sensitive to all sorts of unpredictable changes in the physical and mental environment of the interlocutors. W&H go much further and claim that when they are not hedged, our claims ‘verge on the circular’ (p. 561). They say (p. 561):

‘Phatic communication is communication which gives rise to, or is intended to give rise to, phatic interpretations’ (p. 346). And what is a ‘phatic interpretation’? An interpretation is phatic to the extent that it contains ‘phatic implicatures.’ And a ‘phatic implicature’ is a kind of ‘phatic implication’. AND SO ON [emphasis Ž&C].

In this quote, ‘and so on’ suggests that there is an endless list, or a list which eventually closes the circle and defines ‘phatic implications’ in term of phatic interpretations. In fact, this suggestion is false. We define the term ‘phatic implication’ as ‘a conclusion which does not depend on the explicit content of the utterance’ (p. 345).. AND THAT'S IT!

This line of criticism extends to our reliance on ‘vague, unanalysed, or poorly defined notions’ (p. 561). Allegedly our analysis relies on the notions of ‘linguistic complexity’ and ‘ease of processing’ which we characterize only with reference to our own intuitions. This, W&H claim,

is corroborated by the following quotation from our article: ‘the less effortful the interpretation of the linguistic expression is, the more suitable that expression will be for phatic communication’ (W&H, p. 562; Ž&C, p. 336). It should be clear from our text (pp. 335-336) as well as from the examples which follow this quote (‘How are you?’ ‘How are you today?’ ‘How are you these days?’ ‘How are you now that you've had the operation?’) that we meant: ‘OTHER THINGS BEING EQUAL, the less processing effort a linguistic expression imposes on the hearer, the more suitable it will be as a phatic, ...’. Moreover, it is difficult to see how W&H could disagree with our intuitions about linguistic complexity and ease of processing, given their own views on the relation between phaticness, conventionalization and processing effort. They concede that the use of a non-conventionalized phatic utterance ‘may well increase processing costs and generate implicatures’ (p. 557). So, they would have to agree that a minimal change in a conventional string may well present evidence of the speaker's intention to communicate more than the conventional meaning of the linguistically less complex formulaic expression. This would presumably entail extra processing effort. Therefore, W&H could hardly disagree with what we have to say without contradicting their own position.

7. Parochialism

What is the basis for W&H's allegation that our approach is parochial? They acknowledge that we cite the work of Malinowski, Jakobson, and Laver, but object to the lack of references to Argyle, Bihler, Firth and Halliday in our paper. Their reply to our paper begins with the following observation :

Recent work in Relevance Theory illustrates the coming of age of modern pragmatic scholarship in creating an environment in which a particular theory of pragmatics can be taken for granted, without explanation or justification, and an analysis of a phenomenon previously unaccounted for within that theory can be advanced. (p. 555)

We agree. Phatic communication had not previously been considered within the framework of Relevance Theory, and we have proposed a Relevance-theoretic account of this use of language. W&H are confused about the comparative importance of three points that they make. They disapprove of the lack of extensive references to previous scholarship (i) in our paper, (ii) in papers which investigate the implications of a particular framework for the analysis of a specific problem, in general, and (iii) in other work in Relevance Theory. We believe that their criticism of our referencing practice should be based, not on a sweeping generalization about research in Relevance Theory as a whole, but on a comparison of our paper with other papers in the same genre (i.e. papers which provide analyses of particular phenomena within a particular theoretical framework). W&H's dismissive comments on work in Chomskyan syntax suggest that they disapprove of this sort of research, but they fail to give any examples of the ways in which the practice that they criticize has proved counterproductive. They do not, for example, point out any ways in which our Relevance-theoretic analysis of phatic communication would benefit from the approaches of, say, Argyle or Halliday.

For readers interested in other recent approaches we included a reference to Schiffrin (1994). We do not see what would have been gained by also listing the names of some of the authors mentioned in Schiffrin's survey, as W&H do. To us, this would seem to be a rather pointless way of making our paper a little, and our bibliography a lot, bigger.

8. Conclusion

W&H acknowledge that our main aim was to characterize phatic communication, but they evaluate our work as if it were a descriptive study. They have a view of the relationship between theories and data which we do not share. This is at least partly responsible for a number of general and specific misunderstandings of our paper including the following: their criticism of our reliance on intuitions is misplaced; they falsely claim that we take an omniscient view of communication; they fail to appreciate the importance of the difference between the theoretical notion of mutual manifestness developed within Relevance Theory and the commonsense, intuitive concept of shared knowledge; they

misrepresent our views on the role of conventionalization in phatic communication; without giving any plausible arguments, they assert that our account is vague and vacuous, and they make unsystematic assumptions about the purpose of references to previous literature in new research.

A reply by Ward and Horn raises great expectations of relevance, based on the significant contributions they have each made to pragmatic research (Birner & Ward 1998, Horn 1989, Ward 1988). It is only to be hoped that this reply will not be their most substantial joint contribution to discussions of pragmatic theory.

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