

## DOES 'PROBABLY' MODIFY SENSE?

Huw Price

### 1. Introduction

In English, sentences ascribing *probabilities* may be classed according to whether they have (or permit paraphrase in) the syntactical form  $Pq$ , where  $P$  is a sentential operator, containing the probabilistic reference, and  $q$  is a non-probabilistic sentence. 'There's a good chance of snow tonight' does so paraphrase, for example, but 'Green snakes are probably harmless' doesn't. It seems to me that this syntactical distinction has considerable significance: sentences of this operator form correspond to *single case* applications of probability. Briefly, this is because a single case probabilistic judgment, *inter alia*, is one which could form the immediate basis of a choice of betting odds; and any bet is a bet *that q*, for some sentence  $q$ . This explains my terminology: I use the term *SP sentence* (*statement, utterance, etc.*) for a sentence (*statement, utterance, etc.*) of this form.<sup>1</sup> More importantly, it motivates an enquiry as to the semantic role of a probabilistic sentential operator — the characteristic feature of the *meaning* of an SP sentence. For it is widely held that the major objection to frequency theories of probability is their inability to make sense of the single case. An appealing, if not popular, strategy is hence to retain frequencies where they work best, in 'the general case'; while seeking another, but compatible, account of the single case use of probability (i.e. of the meaning of SP sentences).

However, my present aim is not to defend frequencies, but to criticise a particular account of SP utterances: the view that such utterances have a distinctive *content*, or *sense*. This may justly be called 'the popular view'. Hence I shall be favouring the 'unpopular view', that a probabilistic sentential operator modifies *force*, rather than *sense*.<sup>2</sup>

The difference between the two views is nicely brought out by considering how each sees the relationship between the class of SP utterances and the class of assertions. Both may accept the familiar model of assertion, full belief<sup>3</sup> and relevant action: roughly, the model which takes an agent's *assertion*

<sup>1</sup> 'SP' for 'single case probability'.

<sup>2</sup> An early proponent of the unpopular view was Toulmin (1950); more recent sympathisers include Ellis (1979), Blackburn (1980) and Forrest (1981).

<sup>3</sup> 'Full' in contrast to 'partial'. The notion of a full belief is an idealisation: no one is ever so confident of a belief as to act as if it were true, no matter what the consequences of being wrong. But a belief may be *effectively* full, relative to some given context, in which there is a limit to the (positive and negative) utilities of the outcomes which may result from an agent's acting as if it were true. The availability of this weaker notion renders harmless the useful practice of using the stronger one.

that  $q$  as an indication of a (full) *belief* that  $q$ ; and takes such a belief to be revealed by a person's acting as if  $q$ , whenever (in the light of their desires) they think that it makes a difference to the outcome of their actions whether  $q$ . The popular view regards SP utterances as a particular type of assertion, expressing beliefs with a particular kind of content. The unpopular view, on the other hand, takes SP utterances to be a class of which assertions may be a special case; and to express not full but *partial* beliefs. 'It is probable that  $q$ ' is thus associated not with a full belief that it is probable that  $q$ , but with a strong partial belief that  $q$ . Both sides agree on the kind of behaviour to which probabilistic beliefs give rise (at least in certain canonical cases, such as choice of betting odds). For the unpopular view, this behaviour can be said to result directly from the partial beliefs concerned, in a manner which generalises the behavioural consequences normally associated with full beliefs. But for the popular view there is a problem at this point. Why should the full belief that it is probable that  $q$  give rise to such behaviour (in addition, note, to the type of behaviour standardly associated with a full belief, in contexts in which the outcome of a course of action depends on whether it is probable that  $q$ , rather than just on whether  $q$ )? Without loss of generality, this may be taken to be the question as to why the full belief that it is probable that  $q$  should be accompanied by a strong partial belief that  $q$ . I call this 'the confidence problem'.

It is useful to distinguish two broad classes of popular views: those which interpret 'It is probable that . . .' along the lines of 'It is reasonable to have a strong partial belief that . . .'; and those which do *not* employ the notion of partial belief, in explicating the typical content of an SP sentence.<sup>4</sup> We may call these the 'rationalist' and the 'objectivist' approaches, respectively.

In Section 2 I show how, for slightly different reasons, the confidence problem pushes both objectivists and rationalists towards a view — now quite widespread — that SP utterances are theoretical statements, to be interpreted in terms of their acceptance and rejection rules. In 3 I exhibit a constraint on such a view, arising from the non-deductive nature of these connecting rules. This leads (in 4) to the question as to what remains of the claim that SP utterances are assertions, possessing a distinct content. Noting that the natural appeal at this point is to ordinary usage, I argue that in one central respect this counts against the popular view. Finally, in 5, I defuse a possible objection, by pointing out some disanalogies between SP sentences and theoretical statements in science.

## 2. The Need For Confidence

Why should someone who believes that it is probable that  $q$  be confident that  $q$ ? Because it is *rational* to be so, is the natural answer. An argument

<sup>4</sup> These categories are not exhaustive: there is also the *literal subjectivist* view, which takes an SP utterance to *report* its speaker's possession of a partial belief. But this view is easily refuted, by the observation that that it makes no sense of ordinary disputes about probabilities.

hardly seems needed. Isn't it obviously foolish not to be confident that  $q$ , in these circumstances?

The trouble is, it is *too* obvious. It seems *absurd* for a person to believe that it is probable that  $q$ , without being confident that  $q$ . True, there are cases in which someone will profess to believe that it is probable that  $q$ , without displaying confidence; but similarly someone may claim to believe that  $q$ , without acting as if  $q$ . What would seem absurd in each case, would be to continue to maintain that a person had the relevant full belief, when their actions clearly demonstrated otherwise; i.e., in the probabilistic case, when their actions revealed that they lacked a strong partial belief that  $q$ .

This apparent absurdity has led some writers to see the inference from a full belief about a probability to the corresponding partial belief (the *downward inference*) as analytic.<sup>5</sup> If not, it ought at least to be explained how the connection can *seem* analytic. If such an explanation is to rely on the claims that it is *rational* to make the downward inference, and that ordinary speakers are rational, in this respect, the notion of rationality should be exceedingly commonplace, and well understood by the average competent speaker; and it must be obvious to the ordinary speaker that the downward inference is rational, in this sense. Only thus can the apparent absurdity of a violation of the rule of downward inference stem from the rule's rationality.

However, the prospects for such a notion of rationality are dim, to say the least. For one thing, the natural proposals themselves invoke *probability* (or related terms): 'rational' equals 'most *likely* to be successful', for example. Or they invoke some notion such as hypothetical limiting frequency, already invoked by certain objectivist accounts of probability. Either way, the confidence problem remains: why should a belief about probability, or about hypothetical long run frequencies, have a bearing on degrees of confidence, and hence on action in the single case. It is doubtful whether an account of the relevant notion of rationality can escape circularity of one of these two kinds. Notoriously, deductive inferences from probabilistic premisses yield probabilistic conclusions; while the non-deductive inference to degrees of confidence is what needs justification. Admittedly, it is not clear that these problems are insurmountable: some philosophers have argued otherwise.<sup>6</sup> But the fact that the issue is a difficult one itself suggests that no account which escapes these problems can be sufficiently evident to the ordinary speaker to explain the apparent analyticity of the downward inference.

Analytic accounts of the downward inference are thus increasingly attractive. One such, taking to heart the intuition on which the earlier suggestion was based, is the rationalist proposal: 'It is probable that . . .' simply *means* 'It is reasonable to believe that . ..'. Clearly, there is then no question of believing that it is probable that  $q$ , and yet doubting that it is reasonable to be confident that  $q$ . However, there *is* still a question as to

<sup>5</sup> See, for example, Mellor (1971), p. 67.

<sup>6</sup> For example, Mellor, in (1971), Ch. 8 and (1982); and Braithwaite, in (1966).

why people who believe that it is reasonable to be confident that  $q$ , are *actually* confident that  $q$ . What is it about the content of *this* belief, which leads people to adopt the corresponding degree of confidence?

Here, I think there are two possible moves. One is to attempt to explicate the notion of rationality, so as to display a decision procedure in which ordinary self-interested speakers can plausibly be held to indulge. But this faces the very problems which, as we saw, plague attempts to show that the belief that it is probable that  $q$  makes it reasonable to be confident that  $q$ . As in that case, this objection is perhaps not conclusive. But it does encourage another approach: to say that use of the downward rule is constitutive of a grasp of the meaning of the relevant rationality ascriptions. A person will thus be said not to know what 'It is reasonable to be confident that  $q$ ' means, unless in general when they profess to believe it, they *are* confident that  $q$ . (Let us call this 'contextual' rationalism.)

The appeal to rationality in accounting for the meaning of an SP utterance may now seem redundant. Why not be a contextual objectivist, saying simply that the habit of making the downward inference is constitutive of a grasp of the meaning of SP sentences, without making a detour *via* ascriptions of rationality?

Either way, it can be seen that the confidence problem, concerning the *use* of SP full beliefs, provides a strong motive for a particular account of their *content*. This account is incompatible with some popular versions of the popular view: i.e., those which offer reductive truth-conditions for SP statements (other than in terms of a contextually-defined notion of rationality). Suppose, say, that an SP statement is said to amount to a statement about hypothetical limiting frequencies. The content of an SP full belief will then be specified independently of the rules whereby such beliefs are adopted and applied. There will therefore be a substantial question as to why a belief of the specified content (i.e., about hypothetical long run frequencies) should be linked to evidential beliefs on one side, and to partial beliefs on the other, by just these rules. It will be conceivable for someone to have grasped the meaning of SP statements, and yet to be in doubt as to whether to adopt the standard upward and downward rules.<sup>7</sup> We have seen that it is unlikely that an appeal to rationality can dispel this doubt.

This doesn't mean that there is no role for a notion of rationality in an objectivist's account of the downward inference. On the contrary, objectivism, like any account of single case probability, should be consistent with some account of the rationality of the rules of inference which lead speakers from evidential beliefs, on the one hand, to the partial beliefs which determine their behaviour in probabilistic contexts, on the other. On the popular view, this is a two-stage *combined inference*: the *upward inference*, from evidential

<sup>7</sup> Indeed, a philosopher who thought that SP statements concerned long run frequencies, but who had yet to discover a satisfactory account of the rationality of the upward and downward rules, ought, for consistency, to be in just this position; to suspend the habits of making these inferences, until a justification for them could be produced.

beliefs to beliefs about probabilities (SP full beliefs); plus the downward inference (with which the confidence problem is concerned), from SP full beliefs to the corresponding partial beliefs. (The upward inference may then itself be divided, into an inference from evidential beliefs to a *general* belief about probabilities, and an inference of universal instantiation, from such a general belief to a singular SP full belief.) Hence if an objectivist says that the upward and downward rules of inference correspond to habits, the possession of which marks a speaker's acquisition of the concept of objective probability, the question remains as to the value of these rules (and thus as to the reason for the existence in language of such a concept).

The notion of rationality invoked here will not leave the contextual objectivist open to the criticism raised above. That stemmed from the attempt to base speakers' *use* of the downward inference on their grasp of its rationality. Here it is said rather that this use is a matter of habit, adopted in the process of language acquisition. Hence it need not be claimed that speakers understand the sense in which such a habit is rational, or of value. The view would be coherent, if these problems were as yet undreamt of. (But on the other hand, given that contextual objectivists reject reductive accounts of the distinctive truth-conditions of SP utterances, the claim that these utterances are assertions would seem irrelevant to the solution of these problems. Of importance will be the utility of the *combined* rule, rather than the question whether the upward and downward rules supply truth-conditions at the intermediate stage. And in the absence of reductive truth-conditions, the contextualists' analysis of the meaning of SP utterances provides no independent basis for a demonstration of the utility of the combined rule.)

The contextualist thus rejects a reductive account of the meaning of SP statements, claiming instead that their content is to be understood in terms of the rules for their acceptance and rejection, or application. Philosophers attracted to this position have often appealed to a supposed analogy between statements about probabilities, understood in this way, and theoretical sentences of scientific theories.<sup>8</sup> Here, as well as raising a usage-based objection to the popular view, I want to show that in fact it derives little support from a comparison between probabilities and the theoretical entities of science.

### 3. *Circularity and the Single Case Inference*

I noted earlier that objectivist attempts to base a solution of the link problem on a notion of rationality face a dilemma, resulting from the fact that the only substantial deductive consequences of statements about probabilities are further statements about probabilities. Either a notion of rationality itself mentions probability, thus rendering it useless in solving the link problem;

<sup>8</sup> See for example Levi (1967), pp. 197-204, and Braithwaite (1953), Ch. VI. Mellor (1971, p. 57) comments on Braithwaite's 'eminently reasonable approach', and notes the remark of Kyburg and Smokler (1964, p. 4) that 'most statisticians today hold views which, while not so formal and explicit as Braithwaite's, are not essentially different from his'.

or its connection with probability is such as to depend on a justification of the downward rule, which is what such a notion is supposed to provide.

At first sight, it looks as if contextualists face a similar problem, stemming from the fact that acceptance and rejection rules for SP statements tend themselves to be intrinsically probabilistic. Frequency evidence can show that it is *likely* that it is probable that  $q$ , but not simply that it is probable that  $q$  (where  $q$  is not itself probabilistic);<sup>9</sup> or that it is unlikely that it is probable that  $q$ , but not simply that it is not probable that  $q$ . Now if it is these rules which determine the meaning of SP statements, it is presumably important that we should be able to tell whether a person is using them correctly; and be able to explain their use to someone who doesn't already use them, in order to give them a grasp of the notion of objective probability. But if a person doesn't already understand the notion of *likelihood* (or equivalently, *probability*), what use will it be to tell them that given such and such evidence, it is likely that there is a high probability that  $q$ ? Or if someone already makes this inference, unless we can tell that they are using 'likely' in our sense, how are we to know that they are taking the same inference as we do to define the notion of objective probability.

It might be claimed that 'likely' acquires its meaning in the same way as 'probable' itself, in the adoption of the same set of rules of inference. But any frequency-based acceptance rule for 'It is likely that  $q$ ' will depend on a speaker's ability to recognise when it is the case that  $q$ . Roughly, one should accept that it is likely that  $q$  when one has observed that it is the case that  $q$  in a majority of a range of similar cases. (We are ignoring the higher level probabilities which would appear in a detailed account.) So in the case of 'It is likely that it is probable that  $q$ ', a speaker is required to recognise whether or not it is probable that  $q$  in each of a range of similar situations. And how can this be possible, if one cannot grasp the meaning of 'It is probable that  $q$ ' without adopting habits of inference whose conclusion is the very proposition — 'It is likely that it is probable that  $q$ ' — a grasp of whose meaning depends on this act of recognition?

It seems to me that any solution will depend on the difference between the strictly single case downward inference, and the inference which forms the basis of rejection rules for SP statements. It follows from the single case rule (i.e., that from an effectively full belief that there is a certain probability that  $q$ , one should infer the corresponding partial belief that  $q$ ) that with respect to a large number of independent cases one should have a very high degree of partial belief that the relative frequency of cases such that  $q$  will be close to what one believes the probability that  $q$  to be. This fact, a consequence of the Law of Large Numbers,<sup>10</sup> will be the basis of a frequency-based rejection rule: roughly, that if the relative frequency of cases such that

<sup>9</sup> Using 'likely' here, instead of 'probable', serves only to mark the distinction between second and first level probabilities. In particular, I shall not be using 'likelihood' in Fisher's special sense.

<sup>10</sup> See de Finetti (1937), pp. 124-127, for example.

$q$  in a large number of independent cases differs markedly from the supposed probability that  $q$ , this supposition is likely to be false, and should be rejected. But it is not clear that the rejection rule justifies the strictly single case inference. If not, then even if a grasp of the meaning of SP statements did consist in a disposition to accept and reject such statements (or the associated beliefs) according to the proper rules, it would still need to be explained why someone who adopts an SP full belief should also adopt the corresponding partial belief. It is true that if one were to adopt some *other* partial belief, one would be led by the Law of Large Numbers to an expectation conflicting with the evidence which had given rise to one's existing SP belief, and might thus be led to reject the latter belief. But why should a person adopt *any* partial belief, given simply that they hold the SP full belief?

Thus in the absence of reductive truth-conditions for SP statements, the single case rule seems crucial in handling the circularity inherent in frequency-based acceptance and rejection rules for SP statements. It yields a crucial criterion for a grasp of the meaning of terms such as 'probable' and 'likely': a person does not have such a grasp unless in general when they profess to believe, say, that it is probable that  $q$ , they do display confidence that  $q$ . Only thus can it be checked whether a person is using the proper acceptance rules for SP beliefs. It is no use simply asking them whether in the light of certain evidence they take it to be probable that  $q$ , because what we wish to know is whether they attach the proper sense to 'It is probable that . . .'. Similarly, teaching speakers the proper use of SP statements will depend, for the contextualist, on conveying an understanding of the relationship between SP full beliefs and degrees of confidence: speakers will learn that 'It is probable that  $q$ ', for example, is conventionally taken to indicate a strong partial belief that  $q$ .<sup>11</sup>

In the light of this, what is left of the view that SP statements possess distinct truth-conditions; that SP utterances are *assertions*, possessing a content of a distinct kind? Wouldn't it be simpler to say that SP utterances simply express partial beliefs, and are therefore distinguished by having a distinct *force*, rather than *sense*?

It is natural to think that the dispute between these views will be settled by appeal to ordinary usage. If there is a real difference between these positions (as the contextualist is bound to maintain), it will apparently be revealed in the inability of one or the other to explain some use of SP sentences in ordinary language. Whether or not this impression is, on *a priori* grounds, well-founded, it is supported by the existence of objections of this kind to the partial assertion proposal (and to analogous proposals in other areas).<sup>12</sup> Here I want to turn the tables, appealing to ordinary usage against the popular view.

<sup>11</sup> But not to *report* such a partial belief. It is a nice question just how this distinction is made apparent. Differences in acceptance and rejection conditions seem crucial.

<sup>12</sup> See particularly Geach (1960, 1965); and Searle (1969), pp. 136-141. I have attempted to meet these objections in my (1984).

#### 4. Contextual Truth Conditions: The Verdict of Ordinary Usage

The argument turns on a consequence of the relationship between SP judgments and the evidence on which they are based: the fact that two speakers may reach different conclusions about the probability of some proposition, without either having either misjudged his or her own evidence, or made a mistaken inference from that evidence. For speakers may simply have different bodies of evidence, each supporting a different judgement as to the probability of a given proposition. Elsewhere<sup>13</sup> I have noted that this does not happen with respect to assertions unqualified by a probability modifier, or another with the same property. In making an unqualified assertion speakers assert, in effect, that the matter is settled; that no further evidence could alter the verdict. They may be wrong, of course. But only if they have either misjudged the evidence, or been mistaken as to its bearing on the matter in question. Hence if two speakers endorse incompatible unqualified assertions, at least one of them is *mistaken*.

The fact that this need not be so in the case of SP utterances shows, I think, that an SP utterance is not strictly an assertion as to a matter of fact. For in claiming that a person has the facts wrong, we automatically claim that they are mistaken (even if we excuse the mistake, for example by saying that it is understandable, in the circumstances). To admit that it is possible for a person to get the facts wrong without being mistaken, would undermine any possible justification for a claim to be getting the facts right oneself. One cannot do better than to make no mistake; and yet this would no longer be sufficient to guarantee the truth of what one said.

The point can also be made as follows. If *truth* and *falsity* are properties of sentences, and '*p*' is the kind of sentence which can have these properties (not a command, for example), then "'*p*' is true' is itself an unqualified assertoric sentence, in the above sense: it simply says that the sentence '*p*' has the property of being true. Hence if two speakers disagree as to whether '*p*' is true, then at least one of them is mistaken in one of the ways mentioned. It is not possible that both have correctly judged their evidence, and correctly inferred their conclusion; for the two conclusions are incompatible. However, to disagree as to whether '*p*' is true is just to disagree as to whether *p*. If the former disagreement entails that one party is mistaken, so does the latter. So if truth and falsity are properties, their literal ascription must be confined to sentences not subject to the 'no-fault' disagreements characteristic of SP utterances. Accordingly, 'It is probable that *q*' cannot be said to possess truth-conditions; for it cannot have the property for possession of which truth-conditions *are* conditions.<sup>14</sup>

A natural objection here turns on attempting to eliminate the relational character of SP utterances, on which the argument depends, by taking such

<sup>13</sup> In my (1983).

<sup>14</sup> It doesn't follow that the ordinary application of 'true' and 'false' to SP utterances is simply mistaken; but only that in these cases (at least) the use of these terms does not amount to the ascription of properties to the utterances concerned. For more on this, see my (1983).

utterances to make implicit *reference* to the evidence on which they are based. 'It is probable that *q*' will thus be said to be elliptical for something on the lines of 'Given the existing evidence, it is probable that *q*'. This is plausibly held to be an unqualified assertion, in a way in which, for the above reason, the unrelativised utterance is not.

The trouble is, it is the wrong assertion. There are two reasons why this may be so, the choice depending on how the highly ambiguous phrase 'The existing evidence' is understood. Possible interpretations range from the highly subjective (and indexical), 'The evidence of which I am actually aware', to the excessively objective: 'The state of the universe, past, present and future'. At the subjective end of the scale, the problem is that the proposed analysis of an SP utterance does not make sense of actual disputes involving such utterances. If I disagree with your claim that it is probably going to snow, I am not disagreeing that given *your* evidence it is likely that this is so; but indicating what follows from *my* evidence. Indeed, I might *agree* that it is probably going to snow and yet think it false that this follows from your evidence. So the ordinary business of agreeing or disagreeing with SP utterances does not amount to describing as 'true' or as 'false' something of the form 'Given *my* evidence, it is probable that *q*'.

It might be thought that there is no difficulty here not explained by the indexicality of the reference to evidence. The indexical statements 'It is warm here' and 'It is cold here' are incompatible, in the sense that no one could correctly assent to both at the same time. But obviously both may be correct if said by speakers at different places or times. If SP utterances are interpreted as involving indexical reference to evidence, doesn't this consequence of indexicality explain how two speakers can endorse incompatible such utterances, without either being mistaken? If so, then clearly this property of SP utterances does not show that they are not assertions.

In fact there is a crucial difference between SP utterances and ordinary indexical statements. Imagine we are speaking by telephone, and you say 'It is warm here'. I will disagree with you just when I think that what you've said is false: i.e., when I think it's not warm where you are. Similarly I'll agree with you, and think you've spoken truly, when I think that it is warm where you are. But in the probability case I may disagree with you even if I think your evidence does support your assessment of the probabilities; and agree with you even if I think that it doesn't. In other words, a disagreement may exist in the probability case, even if both parties acknowledge that neither has incorrectly assessed his or her own evidence. There is no analogous possibility in the usual indexical cases.

It seems to me that this difference can be explained with reference to the behavioural consequences of belief. Whether an indexical belief is advantageous to a person holding it often depends on where or when or by whom it is held. For a person fond of swimming, for example, the consequences of assent to 'It is warm here' are likely to be appropriate in Canberra in summer, but disastrous in Cambridge in winter. But there is no corresponding dependence of the consequences of a given partial belief

on the evidence on which it is based. If I'm confident that Proper Name will win the 3.15, and bet accordingly, then I'll lose my shirt when he comes in last whether I've followed his form for years or picked him out with a pin. (It won't carry weight with my bookie that given *my* evidence, the horse was almost a certainty.)

There is thus the same kind of advantage in drawing attention to differences of degree between the partial beliefs of different speakers as there is in the case of non-indexical full beliefs. Everyone wants the full and partial beliefs which will best enable them to realise their desires. In the absence of indexicality, these beliefs are the same for different speakers (different areas of interest aside, of course). And in achieving these beliefs, two heads are better than one. So it is in everyone's interests to reveal differences, call in conflicting evidence, and try to reach agreement. Disagreeing with another speaker's utterance seems to me to be the institutionalised opening move of such a procedure. If so, then it is obvious why we should disagree with SP judgements which differ from ours, even when based on different evidence; and why we shouldn't necessarily disagree with an indexical judgement, even if it would be incompatible some present judgement of ours if (like that one) it were made by us, here, now.

So much for subjective readings of the phrase 'The existing evidence'. Towards the objective end of the scale, a different problem becomes significant. Suppose the phrase is read as 'The evidence accessible in principle', for example; and that SP utterances are accordingly said to make implicit reference to evidence of this relatively objective level. The problem is that many actual SP utterances are clearly based on evidence which does not lie at this level: consider the surgeon who says, 'Your operation has probably been successful. We could find out for sure, but since the tests are painful and expensive, it is best to avoid them.' The accessibility, in principle, of evidence which would override that on which the SP judgement is based, is here explicitly acknowledged. An account which takes SP utterances to be based on evidence of such an objective level is thus inapplicable to many actual situations in which SP judgements are made and applied.

To show that no interpretation of 'The existing evidence' escapes both problems, it is sufficient to show that there is some interpretation which is both vulnerable to the subjective problem, and such that any more objective interpretation is vulnerable to the objective one. The reading 'The evidence reasonably accessible to me' meets both conditions. There are situations in which the evidence reasonably accessible to one participant in a conversation differs from that reasonably accessible to another (on any reasonable reading of 'reasonable', at any rate). Yet clearly there are contexts in which speakers make SP judgements on evidence no more objective than that which is reasonably accessible to them.

Thus the contextualists' claim that SP utterances possess distinct truth-conditions runs into trouble with ordinary usage just where it might hope for most support: in connection with the ordinary use of the terms 'true' and 'false'. If SP utterances do not refer to evidence, the nature of the

disagreement between users of a pair of incompatible such utterances may be such that neither utterance can literally be said to be *true* or *false*,<sup>15</sup> for in disagreeing with such an utterance, one may not be justified in claiming that its speaker has made a mistake. If on the other hand SP utterances do implicitly refer to evidence, then either (i) the standard use of 'true' and 'false' in association with the sentences supposed to make this reference explicit bears no relation to the business of agreeing and disagreeing with the SP utterances of others; or (ii) by referring to evidence of an implausibly objective level, these sentences simply fail to represent many actual contexts in which 'true' and 'false' are applied to SP utterances.

In the last case, it clearly could not be established that *all* SP utterances are assertions, possessing distinct contents. It might be claimed that there is a subclass of SP utterances to which the contextualist account does apply. But then there would presumably be some account of the meaning of the remaining such utterances; and it is difficult to see what could prevent this extending to the supposedly assertoric subclass, to make redundant the contextualist account.<sup>16</sup>

### 5. *The Theoretical Analogy*

I have mentioned the suggestion that there is an analogy between SP expressions and the theoretical sentences of scientific theories. This claim is a common and persuasive one. It is tempting to think that it may be used to defuse the above arguments, for example by comparing the redundancy argument of the last paragraph to an instrumentalist claim that nothing turns on whether there are 'really' theoretical entities. But I think such comparisons are ill-founded.

For one thing, there is an important difference between the contextualist's probabilities and theoretical entities in virtue of the fact that in the theoretical case there are deductive links with non-theoretical sentences (or at least links which will be regarded as deductive, so long as the theoretical sentences concerned are regarded as truth-conditional). Hence there is no scientific parallel to the contextualist's dependence on the single case downward rule, noted above.

Moreover, in the probability case the acceptance and rejection rules lead to the adoption or rejection of a belief as to a particular *value* of the probability concerned. There is no set of possible observations which would lead us to discard probability, as theoretical notions such as ether and phlogiston have been discarded. At most, for the contextualist, it is an empirical question whether there are probabilities other than 0 and 1; and this question is entertained only at the cost of admitting that many ordinary and indispensable SP utterances do not refer to objective probabilities.

Indeed, if a contextualist takes SP utterances to make implicit reference

<sup>15</sup> At least in the sense required of these terms by the notion of a truth-condition.

<sup>16</sup> Note that the above argument stems directly from the relational character of SP utterances. I raised it here, having observed that only an appeal to ordinary usage seems able to decide the case for or against the contextualist. But it applies to all versions of the popular view.

to evidence, and construes 'evidence' objectively, then the redundancy argument provides a sharp contrast with scientific instrumentalism. That argument rested on the plausible assumption that there is some adequate account of our use of single case probability in contexts in which we rely on less than the objective evidence (in whatever sense the contextualist gives this term). This account can be expected to deal with the only cases in which the contextualist account would be useful: those in which our actual evidence coincides with the objective evidence. There is no analogous case against electrons, say, because there are no situations to which electron theory is not applicable, which (i) exhibit the features which the theory explains where it is applicable; and (ii) are such that an adequate explanation of these features would automatically extend to the situations to which electron theory does apply. Perhaps there are scientific theories subject to arguments of this kind, but the standard redundancy argument for instrumentalism is very much weaker.

On the other hand, if a contextualist admits (in order to deal with all SP contexts) that SP utterances refer to significantly subjective levels of evidence, then there is another kind of contrast with the possible grounds for a scientific instrumentalism. The same contrast exists if SP utterances are not held to make reference to evidence. In these cases our argument rested largely on the observation that one may disagree with an SP utterance without claiming that its speaker has made a mistake. A scientific parallel to these arguments would therefore require two speakers to have different observational evidence, leading them to conflicting theoretical statements. There are such situations, of course. But there is no reason to interpret them as we have the SP cases. In the scientific case, the fact that two observations have led to incompatible theoretical conclusions is taken as evidence that one or other set of observations is in some way mistaken; or that the theory itself is in error. There is nothing analogous to the 'blameless' resolution of differences with respect to SP utterances.

So at various points, including those at which the contextualist account needs most support, the analogy between probabilities and theoretical entities breaks down. A contextualist might therefore appeal to an analogy of a somewhat different kind, provided by the phenomenon of theoretical presupposition. It might be claimed that the dependence of SP utterances on evidence is comparable to the dependence of many statements, particularly in science, on such presuppositions; and that a no-fault disagreement about an SP utterance is comparable to the conflict which arises when people disagree as to whether a presupposition holds.

However, there are important differences between the two cases. The structure of disputes about SP utterances requires that different speakers share a common framework, not of evidence, but of the way in which they express their different points of view. It is because everybody talks in terms of probabilities that one speaker can adapt another's utterance to his or her own purposes simply by indicating agreement or disagreement. In cases of presupposition this is impossible: if your conversation presupposes the French monarchy, say, and mine the Republic, there is nothing in common on which

such a linguistic device could operate. But the no-fault disagreements characteristic of SP contexts depend on this device. My 'It is probable that  $q$ ' and your 'It is unlikely that  $q$ ' may depend on different evidence; but because each is expressed in probabilistic terms, we recognise a difference between us (and may set about resolving it, to common benefit). Moreover, the disagreement may arise *despite* my acceptance that your evidence supports your conclusion. Such a case is therefore quite unlike a conflict based on the alleged failure of a presupposition.

The case of presupposition does bear one significant similarity to that of SP utterance. When an assertion depends on a presupposition, doubts as to the applicability of the descriptions 'true' and 'false' in the case in which the presupposition is false, are irrelevant to the behaviour of the sentence in contexts in which all speakers take the presupposition to be true. Similarly, when a group of speakers can assume that they all appeal to the same body of evidence in making SP statements about a particular matter, then no-fault disagreements will not arise. It seems to me that it is the predominance of contexts of this kind which explains why partial beliefs are expressed by sentences which so closely resemble assertions.<sup>17</sup> But this predominance does not show that SP utterances *are* assertions: one counter-example is worth a hundred confirming instances. Of course, it should at some point be explained how a non-assertoric utterance can behave like an assertion, in so many contexts; but this does not seem an impossible task.<sup>18</sup>

*Australian National University*

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<sup>17</sup> Or rather, I think that the predominance of such contexts is what the resemblance of SP utterances to assertions ultimately consists in; see my (1983).

<sup>18</sup> For the beginnings of such an explanation, see my (1983).