THE PROBLEM OF THE SINGLE CASE

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PREFACE

Frequency theories of probability, such as those of Reichenbach (1949) and von Mises (1957), are often criticised for their apparent inability to make sense of the application of a notion of probability to a single instance. If this is the worse failing of such accounts - as seems to be widely believed - then an obvious strategy is to look for some other account of the application of probability to a single case; an account compatible with, and designed to supplement, a frequency interpretation of sentences in which an occurrence of a term such as 'probable' does not refer to a single case.

This strategy would be bound to fail, if it could be assumed that the relation between general-case and single-case applications of probability is the same as that between a non-probabilistic generalisation and one of its instances: i.e., the relation of instantiation of a universal quantifier. For there is no non-trivial way to construe a statement about relative frequencies in certain general classes which will give it the form, 'For all *x*, *Fx*'. However, there seems to be no reason to make this assumption.

Such an assumption would perhaps constrain the interpretation of probabilistic generalisations more than that of statements which apply probability to a single case. It would require such generalisations to be of a universally-quantified logical form, but single-case statements only to be such as to be able to be universally quantified. Even so, in its absence the single case still seems the proper starting point for a philosophical account of probability. The goal of such an account, above all, ought to be to describe and explain the role of probability and related notions in guiding our behaviour. But any context in which a probability judgement is *used* is - *inter alia*, perhaps - one of single-case application. So an early task of a theory of probability ought to

be to attempt to understand such an application.

Beyond this rather vague goal, however, it remains unclear as to what the real issues are for an account of single-case probability. It is not even clear what distinguishes 'single-case' from 'general-case' applications of probability, given that it cannot be assumed that the relation between the two matches that in the non-probabilistic case. So in Chapter 1 I try to locate the central aspects of 'the problem of the single case'; and to characterise the problem in terms which depend as little as possible on the viewpoint of any particular proposed solution. I argue that the central issue concerns the interpretation of sentences of the form (or paraphrasable in the form) *Pq*, where *P* is a probabilistic sentential operator ('It is probable that ...', for example), and *q* is an appropriate sentence. (I call sentences of the form *Pq* 'SP sentences'.)

Although few, if any, existing accounts of probability actually identify a problem in exactly these terms, it is usually possible to see how such an account will approach the issues I thus raise. In Chapter 2 this procedure enables me to outline a range of proposals based on existing accounts. These have in common the assumption that SP sentences are truthconditional, their utterance amounting to a particular kind of *assertion*. In this chapter I also identify a problem common to all accounts which make this assumption, and exhibit the ways in which in solving it most such accounts rely on a notion of *rationality*. Some do so more directly than others, and this provides the basis of a useful classification of the accounts mentioned in this chapter (with one exception) into two classes.

Chapters 3 and 4 take these classes in turn, criticising the accounts concerned. There is particular reference to their dependence on a notion of rationality, and to the role of and grounds for the claim that SP sentences are truthconditional. Some of the arguments in

these two chapters are exemplified in Chapter 5, which is a criticism of some of D. H. Mellor's views on probability.

In Chapter 6 I suggest an alternative view, which rejects the assumption that SP utterances are assertions, holding instead that they stand directly to partial beliefs as assertions do to full ones. This view escapes the objections raised in Chapters 2, 3 and 4 to truth-conditional accounts of SP sentences, but has to meet others. Among these is an objection raised (in slightly different forms) by Geach and by Searle, to a general strategy for the interpretation of certain classes of utterances, to which the proposal here (for the class of SP utterances) is at least closely related. I defend the proposal against this objection in Chapter 7. Although here I am concerned only with the interpretation of SP utterances, it seems to me that the argument is applicable more generally, to defend similar approaches to other types of utterance against the line of objection raised by Geach and Searle.

In Chapter 8 I consider some further problems for the proposal of Chapter 6, these ones being specifically related to the fact that it is an account of sentences involving *probability*.

Finally, in Chapter 9, I return to the notion of rationality, exhibiting the ways in which this notion bears on the view I am advocating. The resulting picture seems to me to be more satisfactory than those to which the accounts outlined in Chapter 2 give rise. In this chapter I also mention some of the questions left open by the present account, particularly to do with the treatment of general-case probability, and its relation to the single case.

This dissertation is the result of my own work, and includes nothing which is the outcome of work done in collaboration. I have tried to acknowledge specific debts for arguments and ideas in notes of reference - which are numbered consecutively in each chapter, and listed at the end of the chapter concerned. Less specific debts, and

similar or closely related arguments of which I am aware, I have tried to recognise in separate bibliographical notes, which follow the notes of reference, if any, at the end of the appropriate chapter. (In notes of both kinds, and very occasionally in the text itself, entries in the bibliographical list at the end of the dissertation are referred to by such forms as 'Reichenbach (1949)'; the date given is the date of the edition referred to.)

Such individual references cannot reflect the extent to which I have been influenced by - indeed, introduced to the philosophy of probability by - the writings of F. P. Ramsey and of D. H. Mellor. My debt to Hugh Mellor for this and for help and encouragement in philosophy over several years is poorly repaid by Chapter 5.

I am also indebted and grateful to Jonathan Cohen, whose comments came at exactly the right time to influence Chapters 7 and 8; to Christie Hamilton, Richard Healey and Frank Jackson for comments and encouragement at various stages; and to Deborah Best and Eileen Price, for locating many typographical mistakes.

1. LOCATING THE PROBLEM.

What are the tasks for an account of single-case probability? And to what extent is it possible to set them out in terms which are neutral between different possible approaches? Theory-neutrality is a relative matter: a statement neutral between a given pair of theories may not be so between these two theories and a third. But if we wish to compare rival accounts of single-case probability, we should at least be able to say what is at issue in terms which are neutral between all the accounts we want to consider; for otherwise, what grounds could we have for thinking that these 'rival' theories are in any way incompatible?

I think that a strongly theory-neutral characterisation of the problem of single-case probabilities is most likely to begin with some class of the sentences, or utterances, of ordinary language - of English, for us. For some such class, identified in syntactical terms, it will be agreed that its syntactical characterisation marks a distinction of deeper, non-syntactical significance. And it will be noted that English speakers characteristically utter such sentences in situations of a certain kind. The fact that there is such a particular kind of situation, for a given class of utterances, seems to be entailed by the claim that the class in question is of more than syntactical significance. This is because, roughly, there would otherwise be a difference of meaning without a difference of use - the impossibility of which seems an uncontroversial aspect of the principle 'meaning is use'.

More specifically, since an utterance is in some sense an effect of its speaker's state of mind, a non-syntactical difference between types of utterance should be matched by a difference between the kinds of state of mind from which such utterances result. Otherwise there

would be a significant difference of effect without a significant difference of cause. It is true that in general a difference of effect may result instead from a difference of prevailing circumstances; but it is not as though what needs to be explained in a case like this is something like the special features of utterances made by speakers with heavy colds. Note that to claim that a non-syntactical distinction between types of utterance should be matched by a distinction between associated mental states, does not involve denying that certain syntactical distinctions should also be matched in this way.

The differences of states of mind which are thus associated with non-syntactical differences of utterance will usually, if not always, also be revealed in differences of non-linguistic behaviour - or at least of non-linguistic behavioural dispositions. Thus if two types of utterance U_a and U_b result from mental states M_a and M_b, and M_a and M_b are such that for no speaker is there any possible circumstance in which it would make a difference to his non-linguistic behaviour whether his mental state were M_a or M_b, then it is difficult to see what grounds there could be for regarding M_a and M_b as distinct (and hence U_a and U_b as different in some non-syntactical respect). How could we tell that a speaker was using U_a and U_b correctly, rather than associating U_a with M_b, and U_b with M_a?

In other words, if a person claims that a syntactical characterisation of a class of utterances U marks a distinction of deeper, non-syntactical significance, he should be prepared to describe associated distinctions at two other levels: one distinguishing a particular kind of mental attitude, or state; the other marking a particular kind of non-linguistic behaviour. He should thus admit the following three questions:

1.1 What is the distinguishing non-syntactic characteristic of an

utterance of class *U*, in virtue of which *U* is a significant sub-class of the class of all (English) utterances?

1.2 What is the characteristic feature, or component, of the state of mind of a speaker who makes an utterance of class *U*?

1.3 With what characteristic behaviour or behavioural disposition is the state of mind identified in *1.2* associated, other than (the disposition to make) utterances of class *U*?

These questions should be answered in such a way as to permit an answer to a fourth:

1.4 What is the nature of the connection between the state of mind identified in *1.2* and on the one hand, the making of an utterance of class *U*; and on the other, the behaviour (or behavioural dispositions) identified in *1.3*?

Suppose a number of theories agree that a class *U* is of more than syntactical significance. Then these four questions constitute a problem which is posed in terms which are neutral between these various theories, so long as none of them disputes the assumptions about language, mind and behaviour on which the questions rest. Thus for sufficiently restricted *U*, at least (though perhaps not, for example, if *U* is the class of all English utterances), *1.1* - *1.4* are theory-neutral relative to all but the most far-ranging disputes about the nature of the utterances concerned.

These four questions are applicable to any class of utterances which is held to be of more than syntactical significance. However, there is no guarantee that the problem they comprise, with respect to any such class, is related to existing disputes; or that this problem does not have an obvious solution. The cost of such a general formulation of the problem may be that many of its instances are uninter-

esting. Fortunately the case which is relevant to single-case probability does not seem to be of this kind; here appropriate instances of *1.1 - 1.4* seem to set out a problem closely related to existing disputes (and one whose solution is by no means obvious).

It seems to me that the most significant class of utterances for which *1.1 - 1.4* form part of the problem of single-case probability, is that of utterances of the form *Pq*, where *P* is a probabilistic sentential operator, and *q* is a truthconditional sentence. There are many forms of probabilistic operator, from the most vague and non-numerical - 'It is probable that ...', 'It is unlikely that ...', and so on - to the most numerical and precise - 'There is a 74% chance that ...', etc. The requirement that *q* be truthconditional is intended mainly to exclude sentences to which ordinary usage does not attach probabilistic operators (questions, for example). Whether it is exactly the right criterion for this purpose need not concern us here; it does not seem too weak, and if it is too strong it will nevertheless simplify the discussion in a useful way.

q does not have to be a singular sentence. In this respect the term 'single-case probability' is a little misleading. Roughly speaking a 'single case' here is anything which could form the basis of a bet. This includes the truth of general propositions. (The fact that it would be impossible to settle a bet on certain types of generalisation is not relevant here; many singular bets are also undecidable, for various reasons. In any case, the truthconditionality of the more unverifiable kinds of generalisation is in doubt.) Thus 'It is likely that all galaxies contain intelligent life' meets the criterion (subject to the truthconditionality requirement); but 'All galaxies probably contain intelligent life', where 'probably' occurs within the scope of the universal quantifier, does not.

As this example illustrates, ordinary usage often does not make

a distinction between a pair of sentences, only one of which is actually of the form *Pq*. 'All galaxies probably contain intelligent life' would usually have the same sense as 'It is probable that all galaxies contain intelligent life'. It will be safe to ignore such cases, so long as it is not a theory-dependent matter which such pairs are equivalent. I shall assume that general agreement between competent speakers is enough to ensure that this is so. We may thus assume, in effect, that everything which can be paraphrased in the form *Pq*, has been.

It does not seem possible to precisely define the notion of a probabilistic sentential operator. However, given speaker agreement as to which sentential operators are equivalent, it would in principle be possible to investigate utterances of the form *Pq* equivalence class by equivalence class - step by step, in other words, at each stage looking at just those utterances whose sentential operator is equivalent to some given operator. Since we have to assume speaker agreement at other points, it would cost us little to fall back on this technique. But it is far simpler to assume that it is reasonably clear to all parties whether a given operator is probabilistic, and hence to deal with all such operators in one go.

I shall call an utterance (sentence) of the form *Pq* - where *P* and *q* are as defined above - an *SP utterance (sentence)*; and a context in which such an utterance is used an *SP context*.

The fact that the class of SP utterances is of more than syntactical significance is in one sense a consequence of the assumption that if P_1q is an SP sentence and P_2 is a sentential operator equivalent to, or interchangeable with, P_1, then P_2q is also an SP sentence. This assumption has the effect of including in the class of SP sentences all merely syntactical variants of any given SP sentence. (The notion of equivalence here, which we have assumed to be backed by speaker agree-

ment, is a non-syntactical one.) However, it doesn't follow that the class of SP sentences is homogeneous with respect to whatever gives it non-syntactical significance. The class might contain very distinct sub-classes, with the members of different such sub-classes having little in common. Because such sub-classes could be investigated individually it would not be disastrous if this were so, but I shall assume it is not. More precisely, I shall assume that general agreement as to which operators are *probabilistic* (which agreement we have already assumed) marks the presence of a non-syntactical feature common to all SP utterances.

The case for regarding SP utterances as central to single-case probability is as follows: the *application* of probability and related notions occurs in contexts in which an agent is uncertain whether some state of affairs holds - whether some proposition q is true, let us say; and in which he believes it is relevant to his interests whether it is the case that q. In such a context an agent will generally express interest in whether 'it is probable that q'. His assessment of his position will depend on to which SP utterances involving q he is prepared to assent. If he takes q to be relevant to his choice of a course of action, then his behaviour will also depend on which such SP utterances he is prepared to make, or assent to. If for example he is prepared to say, 'It is probable that q', and has to decide whether to act in a way which will be as much to his advantage if q is true as it will be to his disadvantage if q is false, then he would normally act in the manner in question. There is no other class of utterances in which 'probable' and related terms occur which is so closely linked to the application of the notion of probability. An SP utterance seems the direct expression of the judgement which precedes an application of this kind.

I shall assume that an argument along these lines is acceptable

to all the standard approaches to single-case probability, and hence that there is at least a *de facto* agreement that SP utterances form a class of central importance to the understanding of this notion. (Note that this assumption does not entail that all approaches agree that the sentential operator form best represents the *syntax* of these utterances. We have seen that some such utterances, at least, can be paraphrased in other forms; one of which might be held to be more basic, in some sense, than the operator form. However, the assumption of speaker agreement as to which paraphrases are acceptable allows us to discuss one form only - and the operator form is at least convenient.)

It follows that with respect to the class of SP utterances, questions *1.1 - 1.4* constitute a central problem for an account of single-case probability. It will be convenient to be able to refer directly to this particular instance of these questions. Thus:

1.5 What is the distinguishing non-syntactical characteristic of an SP utterance, in virtue of which such utterances form a significant sub-class of the class of all (English) utterances?

1.6 What is the characteristic feature of the state of mind of a speaker who makes an SP utterance?

1.7 With what characteristic behaviour or behavioural disposition is the state of mind identified in *1.6* associated, other than with (a disposition to make) SP utterances?

1.8 What is the nature of the connection between the state of mind identified in *1.6* and on the one hand, the making of an SP utterance; and on the other, the behaviour (or behavioural disposition) identified in *1.7*?

To ask these four questions is to ask for a description of the key linguistic, mental and (non-linguistic) behavioural components of an

SP context, and of the connections between these components. I shall therefore call *1.5 - 1.8* the *descriptive aspect* of the problem of single-case probabilities.

I am going to argue that most existing accounts of single-case probability have considerable trouble in meeting this descriptive requirement (though trouble of a kind which tends to be overlooked). The purpose of the above development has been not only to exhibit an aspect of the task of these accounts in such a way as to enable this difficulty to be made apparent; but also to show that it will be difficult to defend such accounts on the grounds that the supposed problem is formulated in terms that they need not accept. The descriptive aspect of the problem of the single case seems strongly theory-neutral.

A second important component of the problem of single-case probabilities is the *justificational* (or *explanatory*) *aspect*. It is one thing to describe the mental and behavioural features of a typical SP context; another to justify, or explain, the fact that we have such mental states and behave in such a way (in a particular such context, or in general). For example, if in answer to *1.6* it is said that a particular kind of belief gives rise to our behaviour, both linguistic and non-linguistic, in an SP context, then a central component of the justificational problem will be the question: how do we come to hold beliefs of this kind, and with what sort of justification? The required justification may be more or less general: a justification of the *practice* of adopting such beliefs in certain circumstances, or of an *individual* such adoption. Our main interest in these justificational questions here will be in showing that the views we are criticising are not significantly better able to provide answers than the alternative account which we support.

As *1.1 - 1.4* indicate, the pattern of a unified description of the linguistic, mental and non-linguistic behavioural aspects of a type of context which has initially been characterised in terms of its linguistic component, is by no means confined to the SP case. A commonplace example is the simple model of assertion, belief and resulting action. In this case *1.1 - 1.4* are taken to have answers along the following lines:

1.9 The distinguishing non-syntactical characteristic of a member of the relevant class of utterances is that it is an *assertion*.

1.10 The state of mind characteristically associated with the assertion that *q* is the full belief that *q* - of which the utterance '*q*' (or 'It is the case that *q*') is a result, in some sense, in appropriate circumstances.

1.11 The behaviour characteristically associated with the assertion that *q* is whatever the speaker believes will be in his best interests, if it is the case that *q* (or whatever *will be* in his best interests if it is the case that *q*, if it can be assumed he is not mistaken about this).

1.12 A full belief that *q* is associated with such behaviour because such a belief *is* - *inter alia*, perhaps - a disposition to behave in this way. Such a mental state gives rise to the assertion that *q* in virtue of a habit the speaker has acquired in learning the language; he has learnt to say that *q* when he believes that *q*, and when the circumstances are in other ways appropriate.

In so far as we shall make use of this model, I think it will be best to ignore its deficiencies, on the grounds that since we are concerned with the special problems in understanding SP contexts, the simplest policy is to try to avoid more general problems. But it will

be useful to mention one idealisation on which the model rests: the notion of a full belief. No one is ever so confident of the truth of a proposition as to act as if it were true, no matter how severe the consequences of being wrong. So no one ever possesses the behavioural disposition which the model associates with a full belief. Unless it can be identified in some other way, there is strictly no such mental state.

However, there do seem to be mental states such that faced with an ordinary range of choices and consequences, a person will always act as though a given proposition were true. We can call such a mental state an *effectively full belief*. Where precision is important (and sometimes as a reminder) I shall use this term, but otherwise I shall continue to refer to 'full beliefs'. Note that since a person's range of choices and consequences may vary from one context to another (in which the truth of a given proposition is relevant), a belief which is effectively full in one context may not be so in another.

We shall see that other than simply by way of analogy, there are two quite distinct ways in which the model of *1.9 - 1.12* may be held to relate to answers to *1.5 - 1.8*. It may be said either that *1.5 - 1.8* concern a special case of the assertion model, or that the latter model is itself a special or limiting case of a model answering *1.5 - 1.8*. Either view might hope to satisfy what we may call the *limiting case constraint* on *1.5 - 1.8*: the recognition that towards the limiting cases of very high and very low probabilities, SP contexts somehow merge into, or approximate to, some other kind of context, in which no mention need be made of probability. It is difficult to be precise about the nature of this constraint. Its theory-neutral basis, even more than that of the descriptive aspect itself, is an observation about ordinary language: in particular, that

ordinary speakers tend to drop talk of probability altogether, in preference to using expressions involving extreme probabilities. An account of the single case should presumably admit some explanation of this fact; but there seems to be considerable scope for different kinds of explanation.

In summary, I have argued that one of the tasks of an account of single-case probability is to answer questions *1.5 - 1.8*. The theoretical presuppositions on which these questions depend seem to be sufficiently general, and sufficiently remote from the notion of probability itself, to be acceptable to all actual accounts of the single-case. This impression is supported by the observation that the pattern of description required by *1.5 - 1.8* is well-recognised elsewhere, for example with respect to the notion of assertion.

2. *TRUTHCONDITIONAL SOLUTIONS, AND THE NEED FOR RATIONALITY.*

How do existing accounts of probability answer questions *1.5 - 1.8*? With respect to *1.5*, there are certainly important differences among the popular theories; but in one very significant way, most accounts agree. It is held, in effect,[1] that SP utterances should be characterised as a particular kind of *assertion*, to be distinguished from other assertions by their subject-matter - by the fact that they are assertions about probabilities. The differences come in attempting to explain what it is to say something 'about probabilities'. Different candidates are offered as the central feature of the semantics of this agreed class of utterances - as what provides their *truth conditions*, as it is said.

In this chapter I want to outline the main such *truthconditional* accounts, as they apply to *1.5 - 1.8*. I want to exhibit a problem which they all share; and to show how in attempting to solve it, most such accounts rely on a notion of *rationality*, in one way or another. Later on I shall argue that this dependence on such a notion is a major weakness of the accounts concerned.

The first task is to distinguish the main forms of the truth-conditional view. It will be useful to do so in three main groups. (The overlaps to which this gives rise are an indication of the tendency for accounts of probability to reach the same conclusion from different points of view.)

The members of the first group have in common a notion of *objective chance*. The common claim is thus that in certain situations all physically possible outcomes have such chances. The sentence, 'It is probable that *q*', for example, is true if and only if the objective chance of an outcome such that *q*, is significantly greater than the

chance of an outcome such that not *q*. Variants of this view depend on different interpretations of the notion of objective chance. Thus:

2.1 The *hypothetical limiting frequency* variant: the objective chance that *q* is *what* the relative frequency of outcomes such that *q* in the class of all outcomes *would* tend towards as a limit, if the given situation were to be repeated indefinitely.

2.2 The *tendency* version of the propensity view: the objective chance that *q* is a measure of the strength of the tendency for an outcome such that *q* to occur in the given situation.

2.3 The *range* variant: the objective chance that *q* is a measure of the number of outcomes such that *q* in the class of all equiprobable possible outcomes of the existing situation.

2.4 The *objectively reasonable degree of partial belief* view: the objective chance that *q* is the unique degree of partial belief in the occurrence of an outcome such that *q* which is made reasonable by the existing state of affairs.

The second major group of truthconditional accounts have in common the view that SP utterances are assertions about certain *partial beliefs*. Assuming there is no relevant dispute about what a partial belief is, variants of this approach differ with respect to which such beliefs, actual or hypothetical, provide the required truth conditions. The simplest position is the most subjective:

2.5 The *actual partial belief* view: an SP utterance is an assertion about the speaker's own state of mind ~ 'It is probable that *q*', for example, means the same as 'I have a strong partial belief that *q*'.

A standard objection to *2.5* is that nobody's actual degrees of partial belief satisfy the probability calculus. The equally standard

reply is that the calculus formalises an *ideal* way of arranging one's partial beliefs, so as to achieve what is called *coherence*.[2] Roughly speaking this amounts to ensuring that one's degrees of partial belief are not such as to lead to certain loss against a sufficiently clever betting opponent (on the assumption that betting quotients are chosen in accordance with one's partial beliefs)[3]. The probability calculus is said to be an axiomatic description of the constraints on degrees of belief which follow from this requirement. Nevertheless, on this view an SP utterance refers not to some ideal belief, but only to the actual belief of the speaker.

The following view is different in this respect, and in the nature of what is held to be the primary ideal:

2.6 The *rational belief* view: 'It is probable that *q*' means the same as 'Given the existing evidence, a rational agent would have a strong partial belief that *q*'.

To illustrate the difference between coherence and what is involved in *2.6*, suppose I believe that all past *A*'s have been *B*'s, and have no reason to think that the next *A* will be different. In the sense intended by *2.6* it would be irrational of me to believe that the next *A* will not be a *B*, but it would not be incoherent.

2.6 admits several variants, depending on an ambiguity in the notion of *existing evidence*. The more important readings are as follows:

2.6.1 'The evidence of which I am actually aware ...'.

.2 'The evidence reasonably accessible to me ...'.

.3 'The evidence in principle accessible to me ...'.

.4 'The objective present state of affairs ...'.

.5 'The total state of affairs, past present and future ...'.

There are further possible distinctions and elaborations. We can perhaps

ignore *2.6.5*, the 'God's eye view' alternative, but *2.6.1 - 4* all give distinct and non-trivial interpretations of *2.6*. Note that some of these interpretations - particularly *2.6.3* and *2.6.4* - offer plausible readings of *2.4*, which is also ambiguous, though not as much so as *2.6*.

The third major approach to truthconditions for SP sentences is based on the notion of a *probability relation*, so that SP utterances are literally assertions that a certain relation holds between certain entities. Variants depend on different accounts of this relation (and to some extent of the entities between which it is said to hold). Thus:

2.7 The *logical* version: the relation in question holds between a set of propositions (or, given sufficiently strong conjunction, a single proposition) on the one hand, and a single proposition on the other. It is a generalisation of the relation of logical consequence, holding in any particular case at some strength *s* between 0 and 1. (Alternatively *strength* can be made the third term of a standard triadic relation).

2.8 The *rational relation* version: here the relation holds between the same kinds of entities as in *2.7*, but is to be understood as 'A full belief in all the propositions ... makes reasonable a partial belief of degree ... that ...'.

If we denote the assertion that some such probability relation holds with strength *s* between a set of propositions *Q* and the proposition *p* by '*R(Q,p,s)*', then for any given *s* the truth of *R(Q,p,s)* depends on both *Q* and *p*. Yet for any given probabilistic operator *P*, the truth of an SP sentence *Pp* seems to depend only on *p* - in any given context, at any rate. How then can a sentence of the form *R(Q,p,s)* provide the truth-conditions of an SP utterance?

The standard solution is to take an SP utterance to make an implicit reference to the existing evidence. Thus 'It is probable that *q*'

is said to be conventionally elliptical for something like 'For some high *s*, if *Q* is a set of propositions describing the existing evidence then *R(Q,q,s)*'. A consequence is that *2.7* and *2.8* have the same kinds of sub-interpretations as *2.6*, in virtue of the ambiguity of the notion of existing evidence. With this addition *2.8* is in any case essentially the same account as *2.6* of the truth conditions of an SP utterance.

Although it doesn't do justice to individual accounts,[4] I think this summary does exhibit quite accurately the main strategies for a truth-conditional approach to *1.5*. The major groupings reflect three of the most dominant beliefs about probability: that it is somehow objective, that it is connected with degrees of confidence, or partial beliefs, and that it is in some sense relational. As we said earlier, a tendency for these different intuitions to lead to the same conclusions is revealed in the overlaps - in the similarity between *2.4*, *2.6* and *2.8*.

It might seem there should be a grouping corresponding to the intuition that probability is related to frequency. But except in the form of *2.1*, frequency accounts do not in themselves offer an interpretation of *single-case* probability utterances. It is true that such accounts are usually accompanied by an account of the single case, but if this is to be truthconditional it must apparently fall into one of the groupings we have described.

Assuming a truthconditional answer to *1.5*, let us now consider *1.6*. The claim that an SP utterance is an *assertion* permits the corresponding choice of *(effectively) full belief* as the category of mental state required by *1.6*. Just as SP assertions are said to be distinguished from others in virtue of their subject-matter, these SP full beliefs will differ from other full beliefs in that they are about single-case probabilities (or in other words, about whatever provides the truth conditions

of SP sentences). Let us call this

2.9 The *full belief* option for *1.6*.

Each of the truthconditional accounts *2.1* - *2.8* has a corresponding elaboration of *2.9*, describing the characteristic content of an SP full belief. But is there any alternative to *2.9* itself? It seems not, for if SP utterances are a particular kind of assertion - as a truthconditional account holds - then *2.9* follows directly from the assertion/full belief/ relevant action model of *1.9* - *1.12*. It is difficult to see how such an account can deny that what is *stated* to be the case by an SP utterance can be *believed* in the way associated with any other assertion (and usually is so believed, by a person making such an utterance).

Given *2.9*, however, what are we to make of the references to *partial belief* by many of the accounts *2.1* - *2.8* - and of the widespread feeling that someone who says 'It is probable that *q*', for example, indicates a certain degree of confidence that *q*? (This feeling is the basis of *2.5*, but is not confined to such a subjectivist viewpoint). Moreover we shall see that partial beliefs are very helpful in answering part of *1.8*. So there are good reasons for wanting to include partial beliefs in the answer given to *1.6*.

There is one way of doing so, for a truthconditional account: to say that someone who makes an SP utterance characteristically has *both* a corresponding (effectively) full belief (that it is probable that *q*, e.g.), and a corresponding partial belief (that *q*). Let us call this

2.10 The *partial belief* option for *1.6*.

It will be simplest to take *2.9* to exclude *2.10* - i.e. to say that *2.9* admits just the full belief, and not the associated partial belief.

I think we may assume there is no relevant disagreement about

question *1.7* - about the nature of the non-linguistic behaviour characteristic of an SP context. For our purposes I think it will be sufficient to consider only betting behaviour - which in any case is usually seen as the most straightforward kind of single-case application of a probability judgement[5]. If we are unable to give a proper account of betting cases, we are unlikely to be able to deal with more general ones. And in betting cases all accounts of single-case probability seem to agree on certain things: in particular, that if an agent claims 'There is a probability *p* that *q*', he will in general (and *should*, perhaps) choose a betting quotient *p*, if given the opportunity to control the odds, but not the size of the stake or the direction, of a compulsory bet that *q*.

If there is no disagreement about *1.7*, then the only variable in *1.8* is the answer already given to *1.6*. In other words, what is involved in *1.8* for a truthconditional account depends just on whether it has chosen option *2.9* or *2.10*. Moreover in either case such an account will presumably say that an SP utterance itself characteristically results from the corresponding full belief, in the same way that any assertion does so. So the question is just the connection between the non-linguistic behaviour (or behavioural dispositions) characteristic of an SP context and the state of mind identified in *2.9* or *2.10*.

Here *2.10* has a distinct advantage. A partial belief can be taken to *be*, roughly speaking (and *inter alia*, perhaps), a disposition to a certain characteristic form of behaviour. The betting behaviour we have mentioned will then be a striking (if somewhat artificial) display of this disposition. In general the disposition will be displayed by a person's acting in the way that someone would who assigned expectations to his possible actions by means of a certain calculation involving his degrees of belief and perceived utilities, and chose the course of action with the highest expectation[6]. The disposition associated with a full

belief - described in *1.11* - is a special case of this one. Notice that the use of partial beliefs and the notion of coherence to provide a subjectivist interpretation of the probability calculus, depends on the link between such beliefs and a disposition to certain betting behaviour being as close as this. If it can't be taken for granted that people will bet in accordance with their partial beliefs, then it can't be claimed that someone with incoherent partial beliefs will accept the bets required for a Dutch book.

Thus partial beliefs give *2.10* an answer to *1.8* which is directly analogous to the corresponding part of *1.11* (which it generalises, in fact). In contrast the full belief option, *2.9*, has an awkward problem at this point. Why should the belief that it is probable that *q*, say, give rise to just such-and-such a betting behaviour with respect to whether *q* - in addition, note, to the kind of behaviour associated with any full belief, with respect to whether it is probable that *q*? What is it about the subject-matter of these particular full beliefs that ensures this particular pattern of behaviour?

Although *2.10* avoids this difficulty in answering *1.8*, essentially the same problem re-emerges at another level; for why should the full belief from which an SP utterance results be accompanied by the 'corresponding' partial belief? Why should someone who believes that it is probable that *q* also have a strong partial belief that *q*?

Thus the advantage of partial beliefs here is short-lived - which is perhaps not surprising, given that they have been characterised in terms of their behavioural consequences. The problem can be stated entirely in behavioural terms, as concerning the connection between two different kinds of disposition. For convenience I shall usually assume *2.10* and hence take the problem to concern the connection between two levels of belief; truthconditional accounts are not thereby disadvantaged. Such an account thus seems bound to say that a person who holds a

full SP belief will (usually, at the very least) also hold a corresponding partial belief. The *link problem*, as I shall call it, is to explain why this is so.

At this point a natural suggestion is that the two beliefs are really the same mental state, which happens to have two different characterisations. But the trouble is, for a truthconditional account, that it is not clear why these characterisations should not apply to different entities (and why, if so, such entities should occur together). Such an account takes the full belief that it is probable that *q* to be an instance of the general notion of a full belief, and so needs only an account of the truth conditions of 'It is probable that *q*' to complete a characterisation of this mental entity - relying on a standard account of the general features of a full belief. There is thus no evident need to refer to the corresponding strong partial belief that *q*, and hence no obvious reason to regard it as really the same mental entity as the full belief. The problem is compounded if a truthconditional account takes the line that a person's belief that it is probable that *q explains* his high degree of confidence that *q* - the presence of a mental state cannot explain itself; but the source of the problem is not the tendency of such accounts to take this line.

It thus seems that a solution to the link problem will need to rely on some feature of the truth conditions assigned to SP sentences, and we might expect the different accounts *2.1* - *2.8* to propose different solutions, some more satisfactory than others.

The subjectivist option, *2.5*, seems to have the simplest solution. If 'It is probable that *q*' means the same as 'I have a strong partial belief that *q*' (or 'I am confident that *q*'), then to believe that it is probable that *q* is just to believe oneself to have a strong partial belief that *q*. So if it is impossible to be wrong about one's own

beliefs - given that one asks oneself what they are, in a particular case - then it is impossible to have the full belief without the corresponding partial one. Perhaps for many of our partial beliefs we don't have the associated full belief (because the question whether we have the partial belief has never occurred to us) but this doesn't matter - the converse implication is the important one.

The claim that it is impossible to hold mistaken beliefs about one's own present state of belief need not necessarily rest on a principle of infallible introspection. It is enough that if we do ever misjudge one of our existing beliefs, from that time on our newly acquired second-level belief itself leads us to hold the matching first-level belief, and therefore - unconsciously - to abandon our previous partial belief.

I do not want to discuss this proposal here, however. It seems to me that (in this truthconditional form) subjectivism is refuted on other grounds, and is the least plausible of the options *2.1* - *2.8*. I shall mention some of the objections in later chapters.

I shall call options *2.4*, *2.6* and *2.8*, which refer to 'rational' (or 'reasonable') degrees of partial belief, the *rational personalist* accounts. Such theories have an approach to the link problem which is similar to that of the subjectivists. The details depend to some extent on which of the sub-interpretations arising from the ambiguity of the notion of existing evidence is involved.

Thus under *2.6.1*, to believe that it is probable that *q*, is to believe that the evidence of which one is actually aware makes reasonable a strong partial belief that *q*. So if *(i)* I believe it is probable that *q*, and *(ii)* whenever I believe that the evidence of which I am aware makes reasonable a certain belief I actually hold that belief, then it follows that *(iii)* I have a strong partial belief that *q*.

I think the best argument for *(ii)* is roughly as follows. If I were regularly to claim to believe that my evidence made reasonable a certain belief, and yet that I didn't hold that belief, it would appear that either I was simply lying about my beliefs, or I was misusing the term 'reasonable'. There is some latitude: in a particular case I might be claiming that some strictly empirical evidence supports a certain belief, which is at odds with my intuitive judgement; then it is simply the fact that I have not made reference to my total evidence (including my intuitions) which allows an apparent violation of *(ii)*. Or by 'is made reasonable' I might intend 'is prescribed by the accepted theory', or something similar,[7] indicating that I think accepted theory is in error in this case. But in general if I use the term 'reasonable' at all with respect to beliefs, it is a condition of my being taken to be doing so in the accepted sense that I actually hold the beliefs I claim to be reasonable.

The same kind of argument is available to rational personalist accounts which rest on stronger interpretations than *2.6.1* does of the notion of existing evidence. The principle *(ii)* needs to be reformulated, but is justifiable much as before. Indeed *(ii)* itself is almost sufficient, because since the move from *2.6.1 to 2.6.5* involves steadily increasing bodies of evidence, it would be odd for someone who believes say 'The evidence in principle accessible to me makes reasonable a strong partial belief that *q*' - i.e. the interpretation of 'It is probable that *q*' under *2.6.3* - not to also believe the interpretation under *2.6.2* and *2.6.1*.

Thus in virtue of their account of the truth conditions of SP utterances, rational personalists seem to have plenty of material with which to attempt a solution to the link problem. No doubt there are difficulties for the proposed solution, but I don't want to discuss them here (I am not trying to defend rational personalism, and I think it can

be attacked more effectively at other points).

I shall call the remaining options from *2.1* - *2.8* - i.e. *2.1*, *2.2*, *2.3* and *2.7* - the *objectivist* accounts of single-case probability. Here, and from now on, the term 'objectivist' simply marks the fact that these accounts are not personalist, in referring to beliefs.

How is an objectivist to approach the link problem? One suggestion might be that it is somehow inconsistent to have a full SP belief, understood in objectivist terms, but not the corresponding partial belief. We have seen that given certain assumptions, both subjectivists and rational personalists have an argument on these lines. But let us take the hypothetical limiting frequency option, *2.1*, as an example: then to believe fully that it is highly probable that *q*, is to believe that if the present situation were to be repeated indefinitely, the limiting relative frequency of outcomes such that *q* would be close to 1. A person who holds this belief, but also believes that on *this* occasion the outcome will not be such that *q*, is not being inconsistent. Moreover it does not seem that a person who professed such beliefs would thereby indicate a lack of grasp of the notion of limiting relative frequency (as seemed to be the case with respect to the notion of rationality in the rational personalist case).

The position is much the same for objectivist accounts other than *2.1*. With its generalisation of the notion of logical consequence *2.7* might seem to be a special case, in that it will have available an accompanying generalisation of the relation of inconsistency. However, since even a highly probable outcome need not occur, pairs of propositions which are inconsistent in this generalised sense need not have false conjunctions. Even if it is a strong (but not deductive) consequence of the present state of affairs that the outcome of the existing situation will be such that *q*, it may nevertheless be *true* that the

outcome will be such that *not-q*. So the fact that a pair of beliefs is inconsistent simply in this generalised sense, doesn't seem in itself a reason why we shouldn't believe both. It might be objected that the usual notion of inconsistency doesn't in itself provide such a reason, either. But at least here we have an idea how an account might go, in terms of the disutility of false beliefs, for example. And whatever the problems with the details of such a justification for logical constraints on belief, there are further problems with the extension to probabilistic constraints. It is these further problems we are concerned with here. It seems that *2.7* is not significantly better equipped to deal with these problems than other objectivist accounts. A generalised notion of logical consequence is not in itself an aid to a solution of the link problem in terms of a consistency requirement, in the usual sense of 'consistency' - even in the indirect way in which such a solution seemed possible for subjectivist and rational personalist accounts.

So an objectivist must take another line. I think there are two possible approaches. One - the most popular, I think, in so far as existing such accounts consider the link problem - is to say that it would be *irrational* of someone who had the full belief that it is probable that *q*, not to also have a strong partial belief that *q*; and that most of us are rational in this respect, which explains the fact that we generally do have the partial belief whenever we have the corresponding full one. I shall consider this approach in more detail below. The alternative, I think, is to claim that the reason people generally associate this pair of beliefs, holding the partial one whenever they hold the full one, is that they have acquired the habit of doing so, in learning to speak the language. I shall ignore this approach for the moment, but return to it in Chapter 4 (where I shall argue that it leaves little content to the claim that SP utterances are truthconditional).

We are now in a position to see the central importance of a notion of rationality to most truthconditional accounts of SP utterance. Let us recap a little. In Chapter 1 we characterised an aspect of the task of an account of single-case probability, as to answer questions *1.5 - 1.8*. In this chapter we began by outlining different approaches to *1.5*, which had in common the assumption that SP utterances are a particular kind of assertion. We then looked at *1.6 - 1.8*, and saw that in virtue of this assumption all these approaches face a similar problem, in justifying a link between two parts of their overall picture. We saw that it is at least arguable that subjectivist and rational personalist accounts have the means to secure this link, but that no corresponding move is available to the remaining - objectivist - accounts. We have just suggested two alternatives, one of which is based on the claims that the link is a rational one to make, and that most language-users are rational in this respect.

Notice that what rests on these claims, for an objectivist who takes this line, is not an argument that this link *does* exist, in practice - that people who believe (effectively fully) that it is probable that q also have strong partial beliefs that q. This much can be established empirically (or so an objectivist should claim). It is rather an *explanation* of this fact. An objectivist's position is like that of a zoologist who has noticed that all animals with hearts also have kidneys; the need is for an explanation of this regularity.

Rational personalists make a different use of the notion of rationality. In this case the truth conditions offered for SP assertions explicitly mention rational behaviour - an SP assertion is said to be about the degree of partial belief it is *reasonable* to adopt, given the existing evidence. The fact that rationality enters such an account at such an early point means that the rationality of ordinary language-users does not have to be separately assumed - or so it seems.

These uses of rationality should both be distinguished from another role the notion plays in accounts of single-case probability, in what we called (p.1:8) the justificational aspect of the problem of the single case. Thus whereas the above uses concern the *use* (for the objectivists) and the *content* (for the rational personalists) of an SP full belief, the justificational aspect looks at the rationality of the *adoption* of such a belief in the first place - at the question, for example, as to why, given certain beliefs about something other than the probability of q, we should infer that it is probable that q.

Although these three uses of rationality are distinct, the last one is closely related to each of the other two, in virtue of the fact that objectivists and rational personalists will both need to consider the justificational aspect of the problem of the single case. For an objectivist the connection takes the form of the condition that the compound inference from certain evidential beliefs, to an SP full belief, and hence to the corresponding partial belief, should be reasonable as a whole. If we call the central feature of an objectivist's truth conditions for an SP sentence an *objective chance* (i.e. modifying slightly the usage of *2.1 - 2.8*, so that *2.4* is not an objective chance account, but *2.7* is), then the argument is as follows: the objectivist wants to claim both that *(i)* if there are such objective chances, then the *downward* inference (from the full SP belief to the associated partial belief) is a rational one; and that *(ii)* if there are such chances then the *upward* inference (from evidential beliefs to the full SP belief) is also a rational one. *(i)* and *(ii)* presumably entail that *(iii)* if there are such chances then the *combined inference* from evidential beliefs to the relevant partial belief is likewise a reasonable one (since it amounts to taking two reasonable steps in succession). So if the combined inference is unreasonable, there are no such chances - or so the objectivist must claim. Hence the objectivist will regard the upward and downward infer-

ences as related by the condition that their composition itself be a reasonable inference (in whatever sense they themselves are held to be so).

There is an analogy here to scientific theories. To take a simple example, suppose that on some observational basis *OB* we accept a theoretical description *DS* of the structure of a certain substance, and are hence able to deduce that this substance will burn with a yellow flame (*YF*) - all this in terms of some atomic theory *AT*, and such that *YF* is not part of *OB*. Then the belief that the given substance has structure *DS* seems closely analogous to the objectivist's full SP belief, and *AT* to the theory that there are objective chances. In this case we may argue that *(i')* if *AT* is true then a belief in *DS* justifies a belief that *YF*; that *(ii')* if *AT* is true then a belief that *OB* justifies a belief that *DS*; and hence that *(iii')* if *AT* is true then a belief that *OB* justifies a belief that *YF*. So if the combined inference turns out to be unjustified, *AT* is false.

However, this analogy does not seem to help an objectivist to clarify the sense in which his downward inference can be shown to be rational, or justified. In the scientific case it is the fact that within *AT*, *YF* is a logical consequence of *DS*, which gives us *(i')*. Because its consequent is a partial belief (whose correctness depends on its *strength*, as well its *content*), the objectivist's downward inference cannot be straightforwardly deductive; and nor does it seem to be given by an indirect consistency requirement (in the way in which subjectivists and rational personalists seemed to be able to secure their downward inferences). So although the scientific example closely matches the structure of the objectivist model, the nature of the key downward inference is very significantly different.

For a rational personalist, the upward inference passes from a set of evidential beliefs (*EB*, say) to the belief that the existing evidence

makes reasonable a certain degree of partial belief in a certain proposition. The beliefs *EB* concern evidence of which a person who makes such an inference takes himself to be aware, but we have seen that under most of its possible interpretations the term 'existing evidence' has a wider reference. However since a person who takes himself to be justified in believing that the existing evidence in one of the wider senses makes reasonable a certain partial belief, will apparently believe the same thing with respect to all narrower senses, we can concentrate on the upward rule for the narrowest case - i.e. that in which 'the existing evidence' does have the sense of 'the evidence of which I am aware'.

There is a close connection between this upward rule of inference, and the combined rule in the objectivist case; because we may think of the rational personalist's reading of 'It is probable that *q*' as 'The best combined rule, applied to (a true belief as to) the existing evidence, leads to a strong partial belief that *q*'. So given that we hold evidential beliefs *EB*, the difference between applying the rational personalist's upward rule, and applying what we take to be the best combined rule, is that our conclusion in the former case *describes* our conclusion in the latter one.

This connection, and difference, can be illustrated in our scientific example. The rational personalist's reading of 'It is probable that *q*' corresponds to 'The reasonable thing to infer from (a true belief as to) the observational data, is (a belief) that this substance will burn with a yellow flame'. Essentially this describes the result of applying what we take to be the proper rule of inference, in the circumstances. We occasionally use such constructions to distance ourselves from an assertion. 'The evidence shows you are mistaken', we say, rather than 'You are mistaken' (referring to the evidence, though not describing it - that's another step). But notice how odd it is to say 'The evidence as a whole indicates that you are mistaken, but you are not', or 'The

observational data enables us to infer that this substance will burn with a yellow flame; I wonder whether it will?'. Statements like these only make sense when the evidence referred to is thought to be incomplete, or otherwise in doubt. If not, then they involve the same kind of mistake as would be made by someone who disputed the rational personalist's downward rule of inference.

It might seem that objectivism and rational personalism correspond to realism and instrumentalism, respectively, in the traditional dispute about the status of scientific theories; but this is not quite right. A truly instrumental construal of 'It is probable that *q*' would regard it not as an assertion, with truth conditions, but as some sort of move, or stage, in a procedure of inference. Rational personalism corresponds instead to what we might call meta-instrumentalism, a view which takes theoretical statements to *describe* the structure of the proper system of inference from observation to prediction. This view shares with instrumentalism (or strictly with instrumentalism plus a 'this is the best instrument' clause) the opinion that the whole dispute is captured in the question 'What is the best form of inference in this domain?'. In contrast the realist admits not only this question (put as 'What is the best combined rule of inference'), but also a question as to the nature of the underlying state of affairs, in virtue of which the former question has one answer rather than another.

It is the fact that objectivists admit a question of the latter kind which gives rise to their special problem with the notion of rationality - to their interest not only in showing why and in what sense their proposed form of downward inference is *rational*, but also in assuming that ordinary agents are rational in this respect, on the whole. Note that unless an objectivist can show that any speaker who uses SP utterances has a grasp of the relevant notion of rationality, this assumption will require the claim that even speakers without the

grasp of such a notion, *are* rational in making the downward inference. We have seen that rational personalists, on the other hand, merely need to claim that speakers learn to be rational in the process of learning the proper use of the term (or of related terms, such as 'probable' itself). This advantage is perhaps offset by the need to claim that ordinary users of SP utterances do, in effect at least, have a grasp of the notion of a rational degree of partial belief; I shall return to this consequence of rational personalism in Chapter 3.

The main tasks of this chapter have been firstly to show how the assumption that SP sentences are truthconditional leads most accounts of single-case probability to rely on some notion of rationality, in order to deal with simply the descriptive aspect of the problem of the single case (i.e. with simply *1.5 - 1.8*); and secondly to exhibit the different forms this reliance may take. This use of rationality has often been taken too uncritically, I think, with the effect of concealing some of the deficiencies of the accounts concerned. In the next two chapters I shall try to bring these faults to light. At the same time I shall argue that the truthconditional accounts we have seen not to rely on rationality in answering *1.5 - 1.8* are not satisfactory alternatives. The latter accounts are the subjectivist one, *2.5*, and any objectivist account which takes the second of the two lines we suggested (p.2:13) towards the downward inference - namely that it is a habit speakers acquire in learning the language associated with SP contexts. We have not yet discussed this line, but it may seem similar to the rational personalist's defence of the downward rule. This impression is quite justified, but we shall see that the similarity counts more against the rational personalist than in favour of the objectivist.

Notes

1. This qualification is important: few, if any, accounts actually see their main task as to answer *1.5* - rather they tend to ask 'What is (a) probability?', for example. This claim therefore relies on the procedure of considering *1.5* in the light of what such accounts do say.
2. See de Finetti (1964), p. 111, n. (e), for example.
3. For the details see Mellor (1971), Chapter 2, for example.
4. For the reasons mentioned in n. 1 above, and in the bibliographical notes below.
5. And as a close analogue of more natural situations; as Ramsey says (1978, p. 85), 'Whenever we go to the station we are betting that a train will really run, and if we had not a sufficient degree of belief in this we should decline the bet and stay at home'.
6. The point of this roundabout way of putting things is to avoid claiming that acting on a partial belief involves *consciously* performing this calculation. The calculation itself follows the formula $E_A = \sum u_x.p_x$, where E_A is the expectation assigned to the action A, u_x is the utility of the outcome x of the action A, p_x is the agent's degree of belief that x will occur (given that A does), and the sum ranges over all the outcomes of A which the agent regards as possible.
7. Cf. Ramsey (1978), p. 96, n. 1.

Bibliographical Notes

The views outlined in *2.1* - *2.8* do not correspond exactly with existing accounts of single-case probability, and not only because *2.1* -*2.8* are simply sketches of possible approaches: there is also the reason mentioned in n. 1 above, the related fact that not all accounts

of probability distinguish its single-case use to the same extent, and the fact that even in so far as existing accounts do follow *2.1 - 2.8*, they may contain elements of more than one of these options (quite apart from the overlap between *2.4*, *2.6* and *2.8*). But with these qualifications, views along the lines of most of these options are not hard to find.

Thus for *2.1* see Hacking (1965), and Popper (1957, 1960). The terminology of *2.2* relies on a distinction drawn by Mellor (1971, pp. 68-70). Mellor rejects this view, partly on the grounds that it is liable to reduce to *2.1*, which he has already rejected. But it is perhaps closer than *2.1* to the remark of Peirce (1931, Vol. 2, #664) that a 'die has a certain 'would-be', ... quite analogous to any *habit* that a man might have'. *2.3* is the classical view, developed by Laplace (1951). As we shall see in Chapter 5, Mellor's (1971) contains a strong element of *2.4*.

2.5 is the adaption to the needs of *1.5* (and the assumption that SP utterances are truthconditional) of the subjectivism developed by de Finetti (1964), and adopted by Savage (1954), for example. In versions corresponding to the weakest and strongest readings of 'existing evidence' *2.6* is hard to find, but otherwise its manifestations are those of *2.4* and *2.8*. For *2.7* see Keynes (1921); though some of Keynes' remarks (such as those on which Ramsey (1978, p. 65) comments) invoke rationality in a way which suggests *2.8*. This option is also suggested, for example, by Carnap's account of 'logical or inductive probability' ('probability$_1$') in (1963), #25.

I have taken the notation of *upward* and *downward* rules of inference from Sklar's (1979). However I shall use the term 'downward rule' differently: when a distinction is important, for the strictly single-case inference from a full SP belief to the associated partial belief about a individual instance, rather than for the inference from an SP

belief to a belief about relative frequencies in a yet-to-be-observed class of cases. The latter inference is often called the *direct inference*, and is the basis of rejection rules for SP beliefs. There is a close connection between this inference and the former one, and in general it will *not* be important to make a distinction. So I shall usually use the term 'downward inference' indiscriminately, for both. Sklar uses it just for the direct inference.

3. *RATIONAL PERSONALISM.*

A *rational personalist* believes that an SP utterance is an assertion, and that its truth conditions are the same as those of a corresponding assertion about the degree of a certain partial belief which is made reasonable by the existing evidence. Thus 'Given the existing evidence, it is reasonable to have a strong partial belief that *q*', or something similar, is said to paraphrase 'It is probable that *q*'. The dependence of such an account on an appropriate notion of rationality is quite explicit.

Such a paraphrase only supports the claim that SP utterances are truthconditional if it is agreed that the rationality ascription involved is itself truthconditional. In this chapter I want to present several arguments for declining to agree. However, I want to begin with a brief argument against the paraphrase itself.

Rational personalism reduces talk about probabilities - in so far as it takes the form of SP utterances, at least - to talk about reasonable degrees of partial belief. And it does so in a rather strong sense, so that a person who claims that it is probable that *q*, is *thereby* claiming that the existing evidence makes reasonable a high degree of partial belief that *q*. This is a much stronger kind of reduction than is involved in standard physicalist reductive accounts, for example. It doesn't follow from the usual physicalist claim that a desk is in some sense *really* a collection of atoms, that in saying 'My desk is supporting my elbows' I am myself saying that a collection of atoms is supporting my elbows. Only a very naive physicalist account would fail to accommodate the evident fact that it doesn't take a grasp of the concept of an atom - under that name or any other - to speak competently about desks.

The rational personalist's reduction needs to be stronger for at

least two reasons: firstly because the proposed solution to the link problem rests on the claim that a person who believes it is reasonable to adopt a certain belief, and uses the term 'reasonable' correctly, will adopt the belief in question. This claim would be of no relevance to the link problem if the full belief associated with an SP utterance was not in itself a belief about rationality (i.e. a belief that it is reasonable to adopt a certain partial belief). Secondly, and more importantly, rational personalism is not simply an account of what else is true, when a given SP utterance is true; it claims to be an account of the characteristic feature of the *meaning* of SP utterances. If the account is correct then a person knows the meaning of 'It is probable that *q*', only if he knows that this sentence is true if and only if given the existing evidence, it is reasonable to have a strong partial belief that *q*. So it would be impossible to know the meaning of SP utterances without a grasp of the idea of a partial belief, and of such a thing being made reasonable by a certain body of evidence. In contrast, no physicalist would claim that I don't know the meaning of 'The desk is supporting my elbows' unless I am acquainted with the notion of an atom.

The fact that rational personalism depends on such a strong reduction leads to the following difficulty: the making of SP utterances seems a less sophisticated linguistic activity than that of referring to partial beliefs, or to mental states in general. There seems no obvious reason why a community should not have developed the former activity but not the latter one (or why such a group couldn't in principle be established, by ensuring that its members were not taught certain parts of our language). But a rational personalist must either deny these possibilities, or admit that in such a case there would be some other account of the distinguishing non-syntactic feature of an SP utterance. In the latter case it is difficult to see how such an alternative account could fail to extend to our own linguistic activity, so

as to give us an alternative to rational personalism itself.

This argument would be reinforced if it could be shown that some actual English speakers use SP utterances in the ordinary way, and yet have no understanding of the notion of a partial belief being made reasonable by a body of evidence. But it is not enough to argue that many English speakers do not understand the term 'partial belief'. A rational personalist could reasonably reply that such speakers understand the notion of a partial belief, but under some other name - 'degree of confidence', for example. Instead it needs to be argued that there is some such group of speakers who have no grasp whatsoever of any relevant notion of mental state. It was the fact that referring to mental states seems a much less basic linguistic activity than making SP utterances which was the basis of the above argument.

Note that this line of argument also applies to the subjectivist truthconditional account, *2.5* - in fact even more so than it does to rational personalism. Subjectivism requires not only that users of SP utterances have a grasp of the notion of partial belief, but also that they are able to apply this notion reflexively, so as to describe their own states of mind. Even more than simply referring to mental states, this awareness of one's own present state of mind as a possible subject of discourse, seems too sophisticated a linguistic ability to be an essential part of the making of SP utterances.

However, putting aside this objection to the rational personalist's use of 'Given the existing evidence, it is reasonable to have a strong partial belief that *q*' as a paraphrase of 'It is probable that *q*', let us turn to the question as to whether the proposed paraphrase is itself truthconditional. I want to present three lines of argument against taking it to be so.

Rational personalism is a reductive account of the truth cond-

itions of SP utterances. However, the question it answers about SP utterances - i.e. *1.5* - arises again for the class of rationality ascriptions which it takes to describe the truth conditions of SP utterances. Thus

3.1 What is the distinguishing non-syntactic characteristic of such an SP rationality ascription?

Here the rational personalist faces a dilemma: if he is prepared to do without a further reductive account in answer to *3.1*, then it is not clear on what grounds he can insist on one for *1.5*; but a reductive approach to *3.1* faces two severe problems.

One problem is that the natural ways of attempting to explain the relevant notion of single-case rationality either parallel objectivist accounts of single-case probability, or explicitly refer to probability. There is a hypothetical limiting frequency approach, for example: 'is reasonable' equals 'would be more successful than other policies if this situation were to be repeated indefinitely'. Similarly there is a tendency approach, a logical relation approach, and perhaps a range approach. And there is the line that 'is reasonable' corresponds to 'is most *likely* to be successful'. The existence of these parallels is not surprising, given that they rely on the same kind of facts about ordinary English as give rational personalism its plausibility. But to admit the need to explain rationality in any such terms, rather undermines its function as a secure base for a reductive account of single-case probability (and one which is preferable to objectivist accounts).

The second problem for a reductive approach to *3.1* concerns the rational personalist's justification of the downward rule of inference. We suggested (pp.2:10-11) that a rational personalist might claim that the operation of the downward rule is a key display of a correct grasp of the relevant notion of rationality - so that a person who consist-

ently claims that it is reasonable to adopt certain partial beliefs, and yet fails to adopt them himself, simply reveals that he hasn't understood what 'reasonable' means (or that he is lying, of course). This strategy depends on there being no sensible question left over as to *why* the habit of making such an inference should be associated with a grasp of this term. To avoid the re-emergence of the link problem in this new form, it needs to be claimed that to acquire an understanding of (the relevant use) of 'reasonable' *is* - among other things, perhaps - to acquire such a habit. But this claim seems inconsistent with a reductive approach to *3.1*, for if the meaning of an SP rationality ascription is explained in terms which don't mention rationality itself, then these terms will suffice to say what a grasp of the meaning of 'reasonable' consists in, without mention of the habit of downward inference.

Let us therefore suppose that rational personalism settles on the other horn of the above dilemma, claiming that SP rationality ascriptions have no *non-trivial* truth conditions - no truth conditions which are statable other than in terms of rationality itself. One variant of this claim would regard such ascriptions as theoretical statements, whose meaning is to be understood in terms of the rules for their acceptance and rejection. Alternatively, the required notion of rationality might be said to be an intuition, a grasp of which is revealed by a correct use of the upward and downward rules of inference linking it to other notions.

As we said, it is difficult to see why someone prepared to accept such an account shouldn't have done so for SP utterances in the first place. But although this counts against the rational personalist, it is not an objection to this kind of move in general, as a way of construing SP utterances as truthconditional. In order to find out what form a stronger objection could take, we shall need to look more closely at

the notion of a truth condition.

The most widely-known use of this notion is that of the programme to explain meaning in terms of truth conditions. It is commonly said that to know the meaning of a sentence is to know what would have to be the case for it to be true. From this it is held to follow that a systematic account of the truth conditions of the sentences of a given language, in terms of their structure and components, will give such an account of their meaning. This programme is intended to apply in the first place only to those meaningful expressions of the language in question whose standard utterance amounts to an assertion - to the making of a statement that the relevant truth conditions hold. It is recognised that there are other kinds of meaningful expressions in natural languages; questions and imperatives, for example. But it may be claimed that even in these cases there is a truthconditional core, which in part determines the meaning of such an expression.

We are concerned with the question whether SP utterances and the rational personalist's rationality ascriptions fall into the truthconditional class (for English) or into the class of exceptions. And we are supposing that in order to avoid the problems we have raised for the alternative line, our rational personalist has agreed that such rationality ascriptions have no non-trivial truth conditions. What kind of consideration could now be relevant?

One way to get some idea, I think, is to envisage treating as truthconditional a class of utterances agreed not to be so, such as the class of propositional questions - i.e. of utterances of the form 'Is it the case that *q*?' (*Qq*, for short), where *q* is a truthconditional sentence. Suppose we suggest that such an expression is an assertion. We say that although its truth conditions cannot be stated by means of any other expression (except one which ordinary usage takes to be

equivalent to *Qq*, such as 'Is it true that *q*?'), it states roughly what is believed by someone for whom it is relevant but uncertain whether *q*. We agree that English conceals the assertive character of these utterances, and therefore propose a modified language (*Q-English*) which differs from English in two respects: it treats the sentence 'It is queried whether *q*' as an (optional) alternative to *Qq*; and it allows that *Qq* may be said to be true or false, just as 'It is queried whether *q*' may be. Thus 'It is true that is it the case that *q*' asserts what 'It is true that it is queried whether *q*' asserts, and both sentences are true just when *Qq* is. Such truth ascriptions are useful particularly in expressing agreement with ('asking the same question as') a previous speaker; and also when there is quantification over a class of sentences of the form *Qq*, enabling them all to be asserted ('asked') at one go. Thus if Q_n is (a name for) the sentence 'Is it the case that *n* is a prime number?', the utterance 'Q_n is true, for every natural number *n*' performs a function which in English is most closely approximated by the question 'Which natural numbers are prime?'. Similarly 'It is false that *Qq*' asserts the same as 'It is false that it is queried whether *q*'. Either utterance is appropriate for a speaker for whom it is either not uncertain or not relevant whether *q*, and explicitly withdraws *q* from the questions at issue. Such an utterance is thus particularly useful in disagreeing with a previous speaker; and, like the truth ascription, has a use in cases of semantic ascent.

On the relation of Q-English to English, and the truthconditionality of the relevant portions of each, we seem to have three options: we can say that English questions of the form *Qq* are truthconditional after all (and that their appearance is hence deceptive); we can say that it is the appearance of the sentences of the form 'It is queried whether *q*' of Q-English which is deceptive, and that such sentences are not really truthconditional; or we can say that the difference between

the two languages is more than just a superficial one, in that the relevant portion of Q-English is truthconditional, but not that of English. The first option conflicts with the well-established view of the nature of propositional questions. The third seems to me implausible, particularly given that Q-English itself contains expressions of the form *Qq*, as well as the equivalent expressions 'It is queried whether *q*'. So the second option seems the most acceptable.

If the query ascriptions of Q-English are not really truthconditional, I think we should expect this fact to be revealed in the way in which this language associates the terms 'true' and 'false' with such expressions. For presumably it must be something more than the possibility of replacing this segment of Q-English with the corresponding part of English itself which reveals the lack of truthconditionality. This is particularly clear if we imagine trying to convince a speaker of Q-English that 'It is queried whether *q*' is not a genuine assertion. Unless we can point to some internal feature of his language which justifies our claim, this person will feel that we have not ruled out the first and third options above. That is, he will feel free to claim either that *our* questions are really assertions, this fact being concealed by the peculiar form of words we use in making such statements; or that there is something which can be asserted in Q-English but not in English.

So what is it about the way in which Q-English associates the terms 'true' and 'false' with expressions such as 'It is queried whether *q*' which distinguishes these sentences from genuinely truthconditional ones? If we can answer this, I think we shall be in a position to examine the claimed truthconditionality of the rational personalist's SP rationality ascriptions.

I think the key is the relational nature of the state of relevant uncertainty. We saw that which questions I ask - what, in Q-English, I

declare to be queried - depends roughly on what things I am uncertain about, and on which such things I take to be relevant to me. And this in turn depends on what I already know, and what my interests are. So if I claim 'It is queried whether *q*' and you reply 'That's false', you may intend no criticism of me, but simply be expressing a different viewpoint. Hence the exchange should not be seen as me claiming that something is the case, and you denying it. If anything has been asserted at all, it is apparently that *for me* it is relevantly uncertain whether *q*, and *for you* not relevantly uncertain whether *q*.

Suppose we systematically construe query ascriptions in this way, making explicit the relativisation to the speaker's viewpoint. We shall then find that the above exchange does not conform to the ordinary use of the notion of falsity: since you are not *denying* that *for me* it is relevantly uncertain whether *q*, it is not correct, in the ordinary sense, for you to say 'That's false' in answer to my claim that it is queried whether *q*. The relativised claim is false in the standard sense only if I am wrong about the relevance to me of, or my uncertainty as to, whether *q*. So this proposal does not preserve the way in which Q-English applies the term 'false' to query ascriptions; or does so only at the cost of admitting a non-standard use of this term, different from its use in association with the notion of a truth condition.

Suppose now that we try to avoid this difficulty by introducing an objective justification for 'asking a question'. 'It is queried whether *q*' is thus to be understood as 'It is objectively uncertain and objectively relevant whether *q*' (we might say that *q* is 'objectively in question'). This move is motivated by the desire to interpret the response 'That's false' to the utterance 'It is queried whether *q*' as a standard denial of what the utterance asserts to be the case. However, the proposal faces the following dilemma: the more it objectifies that

to which the query ascription is held to be relativised, the more situations it will admit in which it is quite implausible to say that the speaker is actually referring to (and relying on) this objective base; while the more it reduces the level of objectivity of the base, the more it will allow situations of the difficult kind, in which the response 'That's false' doesn't indicate a criticism of the original speaker, but simply the possession of a different point of view.

A more detailed examination would show, I think, that there is no way to resolve this dilemma - no base to which to relativise query ascriptions which escapes both kinds of problem. And it is this which shows that the query ascriptions of Q-English are not really truthconditional, it seems to me. I now want to argue that SP rationality ascriptions are subject to the same dilemma.

The relational character of many rationality claims is well-recognised; what is rational from your point of view needn't be so from mine, and the disagreement may give neither of us grounds to criticise the other. As rational personalists acknowledge, the simple claim that it is reasonable to adopt a certain partial belief is relational in just this way. What partial belief it is reasonable for me to adopt depends on what my evidence is. If your evidence is different there may be a formal disagreement between us, in that we are not both prepared to say, for example, 'It is reasonable to have a high degree of partial belief that *q*'.

As in our query example, to maintain that such rationality ascriptions are truthconditional it is necessary to exclude these non-critical disagreements. This can be attempted by taking such utterances to make implicit reference to the evidence on which they are based. Rational personalists make this move in any case (for different but related reasons); in fact they go one stage further, in making *explicit* the reference to evidence. But to which evidence? We saw in Chapter 2

(p. 2:3, in particular) that there is a range of possibilities, from the evidence of which the speaker is actually aware at the time of speaking, to a very strong 'God's evidence' alternative. Which alternative, if any, is such that the resulting relativised rationality ascriptions function in discourse in the same way as the (allegedly elliptical) utterances they are supposed to represent?

At the subjective extreme, the difficulty is that disputes about the truth of an utterance of the form 'The evidence of which I am aware makes reasonable a partial belief of degree *d* that *q*' seem to bear no direct relation to the situation in which two speakers each make such a claim, differing only in the degree of partial belief that *q* that each says to be reasonable. Since it is the latter kind of 'dispute' which according to this version of rational personalism is involved when two people ascribe different probabilities to the same proposition, the account faces the following problem: it must either concede that the ordinary language practice whereby one speaker declares another's utterance to be *false* in such a context, is of no significance with respect to the standard use of the notion of falsity; or acknowledge that it fails to give an adequate account of such ordinary usage. In the latter case it concedes that some further account is needed; while in the former one it denies the significance of just that feature of language which ought, it seems, to be its own primary motivation. For if the ordinary language practice of ascribing truth and falsity to SP utterances does not involve the standard senses of these notions - the senses associated with the notion of a truth condition - then what grounds are there for expecting a truthconditional account of such utterances?

As we turn to more objective evidential bases this problem becomes less significant. Situations become rarer in which two speakers at the same time and place can have different evidential bases, in the

chosen sense. If it is held that SP utterances make implicit reference to the *reasonably accessible* evidence, for example, then you and I can only be referring to different bodies of evidence when we ascribe different probabilities to something, if what is reasonably accessible to you is not what is reasonably accessible to me. There are such situations (in any reasonable sense of 'reasonably accessible'), but they are less common than situations in which it is the evidence we actually have on hand which differs.

However, as this source of difficulty becomes less significant another one becomes more so. As in the Q-English case, the more objective we take the relevant evidential base to be, the more situations we admit in which the evidence that a speaker actually has - and (in the SP case) the evidence on which he bases his partial beliefs and hence his associated behaviour - is not the objective body of evidence, in the chosen sense, for the context in question. Moreover, this fact about his evidence is one of which the speaker himself can be aware; he does not himself have to believe that his actual evidence - the evidence on which he acts - is the evidence objectively available to him. Consider for example the doctor who says 'Your operation has probably been successful - we could find out for certain, but since the tests are dangerous and unpleasant, it is better to avoid them'; or the driver who explains 'This is probably the quickest route - unfortunately we can't afford the time to stop and make sure'. These are plainly cases in which the speaker knows that the evidence objectively available (or 'in principle accessible') exceeds that on which he or she is relying; and therefore cases in which the probability ascription cannot be held to be relativised to such an objective base.

The fact that a range of alternative positions is for different reasons unacceptable at either end does not entail that there is not some satisfactory middle position. However, an acceptable middle

position would be ruled out by a demonstration that there is an overlap between the ranges of applicability of the two objections we have raised. In fact, if it is agreed that the range of possibilities forms a linear progression, then it is enough to show that there is one of its members which is both open to the subjective objection and such that any more objective position is open to the objective one. This will show that no position escapes both.

It seems to me that the reasonably accessible evidence option is of this kind. We have seen above that it is open to the subjective criticism because, so to speak, what is reasonably accessible to you may not be to me (if you have access to classified information and I don't, for example). But any more objective position will admit situations in which the evidence on which we act is (at most) the evidence which is reasonably accessible to us, rather than the evidence comprising this more objective base.

Thus it appears that no member of this range of alternatives is satisfactory. This seems to me to be a strong objection to the rational personalist position (and to the objectivist one, as we shall see in Chapter 4); though an objection it is easy to overlook, in failing to take seriously the ambiguity in the notion of *the existing evidence*.

The subjective criticism here is based on the observation that ordinary usage does not associate the terms 'true' and 'false' with SP utterances (or rather, does not always do so) in the way that it would if it used these terms in the sense associated with the notion of truthconditionality, and rational personalism's rationality ascriptions were accurate paraphrases of SP utterances. It is worth noting that subjectivists face much the same problem. Your reply 'That's true' to my 'It is probable that q', if it shows anything about partial beliefs at all, indicates that *you* have a strong partial belief that q - not that you think that *I* do (as the combination of the subjectivist

reading of 'It is probable that *q*' and the assumption that 'true' is used here in its standard sense would require). This is a well-known objection to this form of subjectivist interpretation of SP utterances.[1]

We have supposed (p. 3:5) that to avoid the problems we raised for a reductive account of the truth conditions of SP rationality ascriptions, it has been agreed that such utterances have no non-trivial truth conditions. We suggested two slightly different accounts of the meaning of such utterances, in line with this agreement: that they acquire meaning in virtue of the rules for their acceptance and rejection; and that their meaning is grasped intuitively, a correct grasp being revealed in correct use of these upward and downward rules. I now want to raise two points, in clarification firstly of what such a move achieves (and fails to achieve), and secondly of its possible grounds of support.

Firstly, there is an important sense in which such an account of the meaning of SP rationality ascriptions doesn't tell us why we *ought* to adopt the partial beliefs which such utterances describe as rational, or reasonable. Some care is needed to characterise what is left unexplained here, and the ethical analogy is helpful. It is open to someone who, like G. E. Moore, takes the notion of *goodness* to be an intuition, to say that the proposition that we ought to do what is good is analytic - i.e., roughly, that the good *is*, simply, that which we ought to do. But there remain questions as to how we come to have such a concept at all, and as to what value it is to us; and someone who asks 'Why ought we to do what is good?' with these kinds of issue in mind, will find it quite unhelpful to be told that he is asking for an explanation of an analytic truth.

In the case of SP rationality ascriptions, similarly, accounting for their meaning as an intuition, or in terms of the rules for their

acceptance and rejection, will leave unexplained the value to us of such an intuition or combination of rules - and hence the fact that we make such rationality ascriptions at all. (The 'hence' here relies on the assumption that the existence of any sufficiently significant linguistic feature, is in some sense a result of the usefulness of some function the feature in question performs). Moreover, given that non-trivial truth conditions for such ascriptions have been rejected, the fact that these utterances are being treated as truthconditional would seem to be able to play no part in these explanations. It will be the usefulness of the upward and downward rules in combination which provides the basis of an explanation, and irrelevant whether these rules are held to supply truthconditional meaning to an intermediate stage. And in the absence of reductive truth conditions, there is no basis in the meaning of a rationality ascription for an argument to *establish* the usefulness, in some sense, of the upward and downward rules in combination.

If the claim that SP rationality ascriptions are truthconditional can play no part in an account of how we have come to use such expressions, what other grounds could support it? Not ordinary usage, apparently: we have just seen that the way in which 'true' and 'false' are often applied to such expressions counts against the truthconditional view; and the Q-English example suggests that the fact that an utterance has the superficial form of an assertion - i.e. that it has, or can be paraphrased in, the form 'It is the case that ...' - is not a reliable guide to its true status.

Another suggestion might be that the upward or downward rules associated with SP rationality ascriptions 'transfer' truthconditionality. Thus if the upward rule, say, were deductive, then a judgement as to the truth of such an ascription would change only if the evidential beliefs on which it was based were later judged to be false. This

connection might ensure that 'true' and 'false' are applied in the same sense to these rationality sentences as to those describing the evidence (whose truthconditionality, let us assume, is not in doubt). In other words, if it were agreed that from *true* evidential beliefs the upward rule invariably leads to a disposition to make *true* rationality ascriptions, there would be some reason to think that 'true' is used with the same sense in both contexts.

However, the rules for accepting and rejecting these rationality ascriptions are non-deductive, at some stage at least - a rational personalist who holds that statistical generalisations are universalised SP sentences will say that the inference from the general to the single case is deductive, but in this case the lack of a deductively valid rejection rule distinguishes these generalisations from non-statistical ones. SP rationality ascriptions thus seem to be sufficiently isolated from those parts of language whose truthconditionality is not in doubt (in this context, at any rate), for it to be entirely an internal matter whether these utterances are truthconditional themselves. Whatever the reasons for regarding various other types of utterances as making assertions, these expressions need to be separately assessed. The same goes for SP utterances in general, I think; and I shall return to this point in Chapter 4.

Notes

1. And to analogous accounts of certain other types of utterance, such as the suggestion that 'It is right that *q*' asserts that the speaker approves of the fact that *q*; see Moore (1967), pp. 37-39, and Blackburn (1980), who mentions this case as analogous to the present one.

4. OBJECTIVISM.

In Chapter 2 we characterised an *objectivist* account of single-case probability as one which takes an SP utterance to be an assertion about an objective state of affairs, other than the strength or the rationality of a partial belief. We adopted the general term 'the objective chance that *q*' for the basis of an objectivist's truth conditions for such an assertion (with respect to the probability of *q*). Under *2.1 - 2.3* and *2.7* we noted several possible forms of such an account.

We saw that an objectivist is likely to rely on a notion of rationality to solve what we called the *link problem*. That is, an objectivist needs to explain why a person who holds an effectively full belief that it is probable that *q*, say, will also have a strong partial belief that *q* (or if partial beliefs are not regarded as significant, as *2.9* has it, then why such a person will have the non-linguistic behavioural dispositions characteristic of an SP context). It needs to be explained how the inference from the full belief is such a natural one that the difference between the full belief and the associated partial one is often overlooked. We saw how it follows from an objectivist's interpretation of an SP context that there is such a gap. The proposed explanation was that the required inference is a rational one, and taken for granted because, in this case at least, we all take rational behaviour for granted.

It might seem that objectivists will face the same problems with the notion of rationality as rational personalists, and perhaps more besides. For a satisfactory objectivist account would appear to give a satisfactory rational personalist one, according to the rule that it is reasonable in the rational personalist's sense to have a certain

partial belief, if and only if in the objectivist sense it is rational to have a certain (effectively) full belief about objective chances, from which in turn it is rational to infer the partial belief in question. Thus objectivists seem to need an account of the rationality of their combined rule of inference - the rule which results from taking their upward and downward rules *in succession* - and such an account will apparently serve the purposes of a rational personalist, in explaining what it is for a partial belief to be rational, given a body of evidence. Hence whatever problems rational personalists experience with the notion of rationality are an indication of similar problems for objectivists.

However, this overlooks an important option which is open to an objectivist: to say that the notion of rationality in these contexts is not correctly applied in the first place to individual inferences, but to *types*, or *habits* of inference - application to individual inferences being derivative, and in principle always subject to revision as more information comes to light about the inference in question.

As an illustration, consider the application of the description 'generous' to a person's actions. It might be said that each individual such application relies on a general maxim - 'It is generous to give an unsolicited gift', for example - and is therefore liable to be revised, if some more specific maxim is found to be applicable. In this case the more general maxim need not be said to be falsified, so long as it is not construed as a universalised product of statements about single cases. A case in which the giving of an unsolicited gift does not seem generous does not in itself show that there is something wrong with the principle that it is generous to give such gifts, on this view - as it would if the principle were of the form: (x)(If x is the giving of an unsolicited gift, then x is generous). (On this view the latter expression is actually without sense, since it depends on the application of

of the term 'generous' directly to the single case). Thus the reason it doesn't now seem generous of me to have given you a chess computer is not that it has now turned out that the gift was solicited, but that it appears that I hoped that you would enjoy it so much that you would be distracted from your work; and that it hasn't yet become clear that I wanted to distract you not for my own advantage, but because I was concerned about your health; and so on. The story can be continued indefinitely, and at each stage the general maxim on which the current judgement relies is in no way undermined by the later discovery that some more specific maxim is applicable as well. The current judgement is the present *evaluation* of the given action. It is not a statement of fact, but the expression of an attitude; an expression which is licensed by (rather than a logical consequence of) the present most specific applicable maxim.

I shall say more about this way of treating SP rationality later on. For the moment the important point is that although it will not yield truth conditions for the individual ascriptions of rationality required by the rational personalists, this line may be sufficient for an objectivist claim that the upward, downward and hence combined rules of inference - to, from and via objective chances - describe rational *types* of behaviour. Then three problems seem to remain. One is that of explaining what it is for a type of behaviour to be *rational*, in the intended sense. Another is the problem of explaining the fact that actual agents do tend to behave rationally in this respect, habitually adopting and applying beliefs about objective chances in more or less the right way - the latter aspect here involves the link problem, the step from a belief about objective chance to the associated partial belief. And the third is that of showing that the proposed upward and downward forms of inference *are* rational, in whatever sense has been explained, given the proposed account of objective chance.

A natural way to approach the first problem, I think, is to try to explain 'rational' as 'on the whole advantageous', or something similar. There seem to be several possibilities, even given that we shouldn't slip single-case probabilities back into the account. Since we want a description applicable to a *type* of inference, there seems to be no need to invoke the repetition of a numerically identical situation - as *2.1* does, for example - or to rely in such a strong way on a paraphrase of hypothetical form. It is an important question whether a thoroughly actualist such account is possible, but I don't want to discuss it here.

If rationality is in the first place a property of types, or habits, of inference, it is plausible to approach the second problem in terms of social and biological explanations for the possession of certain such habits by ordinary speakers. The natural objection to this move - that it doesn't allow for disputes as to whether some particular inference is *really* the rational one in the circumstances, even given that it is the one we are lead by force of habit to make - is now ruled out, as resting on a mistaken view of the nature of an ascription of rationality. The closest we come to such a dispute, on the present view, is a case in which a given inference instantiates one habit but conflicts with another; then the conversational process by which it is established that one or other habit takes precedence, may take the form of a dispute as to whether the given inference is 'rational' (or more precisely, the form that such a dispute would take, if there were really a matter of fact here to dispute about). On this view it is not the reasonableness of its instances which makes a habit a good one, but in a sense the other way round. A speaker's use of the term 'reasonable', as a description applicable to a single case, indicates his possession of a related habit - and the fact that speakers have certain such habits but not others seems to be in some way a result of

the advantages of possessing these habits.

It is the third problem which seems most difficult, I think, at least at first sight. The objectivist is being asked for an explanation of the rationality of the upward and downward inferences, in the sense in which rationality has been explained, in terms of his notion of objective chance. He is being asked, in effect: How would someone who was acquainted with this notion of objective chance, but who had somehow lost the habits of inference involving chance shared by the rest of us, be able to tell that these types of inference would be advantageous? If such a person held beliefs about relative frequencies, say, is there any way in which we could persuade him to adopt 'corresponding' beliefs about chances, and reject non-corresponding ones? If he held beliefs about chances, could we persuade him to adopt the corresponding partial beliefs? Could we even justify our own readiness to make such inferences?

As an example of the kind of problems which arise, suppose we have convinced this person that from the frequency evidence he should infer that it is very likely that there is a certain chance that q. In order to induce him to adopt a high degree of partial belief that there is this chance that q, we still need to persuade him that from a belief that it is very likely that p, in general, he should infer a high partial belief that p. So the upward rule depends on the downward one.

The objectivist's most plausible line seems to be to say that the upward and downward rules are what gives meaning to objective chance ascriptions. Just as the theoretical sentences of scientific theories are held to pick up their meaning by virtue of the rules for their acceptance, rejection and application, so in this case using and grasping the meaning of objective chance ascriptions amounts, essentially, to using the proper upward and downward rules, in correct association. So there is no question of not using these rules, for someone

who 'speaks the language' at all. Any proposed such question is at best terminological, and otherwise nonsensical. The person whose lack of these habits of inference seemed to present a problem cannot be said to have grasped the notion of objective chance. Correct use of sentences referring to such chances is simply inseparable from the activity of making these inferences. It is the entire linguistic package which is rational, in that it is advantageous, on the whole - and that explains why we have evolved it.

We looked at an analogous proposal from rational personalists in the previous chapter. Before that, in Chapter 2 (p. 2:13), we mentioned this same proposal from objectivists, as a means of securing specifically the downward rule (in order to solve the link problem); but we deferred considering it, in favour of the more popular proposal involving rationality. We now find that if this more popular approach is to avoid some of the difficulties we raised for rational personalism in Chapter 3, it is likely to rely on the proposal to which originally it seemed an alternative.

This proposal seems to me quite a promising one, but not an approach which will give us truth conditions for SP utterances. Thus notice that if our objective chance ascriptions are held to have truth values in the standard sense, then because the acceptance and rejection rules are not deductive, it may turn out that we have accepted a false chance statement and rejected a true one - even if we are not in error about the evidence. If we have any justification whatsoever for thinking that this is not the case with respect to the chance statements we presently accept, it rests on the fact that these rules are *likely* to give us the right answer. Here we encounter the classic circularity problem. In order to understand why it is a problem, it is important to understand what is at issue.

The objectivist is claiming that SP utterances are to be inter-

preted, and given truth conditions, in terms of objective chances; and that the meanings of sentences about objective chances are to be understood in terms of the rules for their acceptance and rejection. Let us ignore the feature of these rules which leads to the circularity problem for the moment, and suppose that everyone adopts and rejects straightforward dispositions to make SP utterances in appropriate circumstances, when confronted with certain kinds of evidence - about relative frequencies, say. Thus confronted with evidence that a particular coin has landed heads on 90% of the many occasions on which it has been tossed, everyone unreservedly rejects a disposition to say 'There is a 50% chance that this coin will land heads at the next toss'. Is this enough to show that SP utterances are truthconditional?

I don't think so. There are many kinds of situation in which an awareness of some 'evidential' state of affairs is habitually associated with the adoption or rejection of a disposition to make utterances which are not truthconditional. If I observe water dripping from the ceiling I will be disposed to say 'Where is that coming from?' (in appropriate circumstances - if there is someone present who might know, say). And if I then observe, or otherwise learn, the source of the water, I will lose this disposition.

Is there some characteristic of such a habit of forming and rejecting dispositions to utterance which reveals that the utterances concerned are truthconditional? Being an invariant habit does not seem enough. Some such habits are deductive inferences, and this does seem a sufficient condition for truthconditionality - but it is doubtful whether the deductive character of such a habit can be established, without it first being shown that the utterances to which it leads *are* truthconditional. In any case, when such a habit is clearly not deductive, even this slender prospect is not available.

Thus the circularity problem is a difficulty for objectivists

firstly in that it exhibits the non-deductive nature of the inferences by means of which objective chance ascriptions are accepted and rejected. But it also leads to a greater difficulty, once an objectivist takes the line that a grasp of the meaning of such ascriptions is revealed in correct use of these inferences. For in this case it is presumably important that we should be able to tell whether someone is using these rules correctly - and be able to explain their use to someone who doesn't already use them, in order to give him a grasp of the notion of objective chance. But if he doesn't already understand the notion of *likelihood*, what use will it be to tell him that given such and such evidence, it is likely that there is a high chance that *q*? And in the case of someone who is already prepared to make this inference, unless we can tell whether he is using 'likely' in the same sense as we do, how are we to know that he is really taking the same inference as we do to define the notion of objective chance?

I don't think it will do to say that 'likely' picks up its meaning as part of the same package, in the adoption of the same set of rules of inference. This move faces a dilemma: either it treats expressions of the form 'It is likely that *q*' as truthconditional, with meanings to be explained as those of objective chance ascriptions are, in terms of the rules for their acceptance and rejection; or it treats these expressions differently from objective chance ascriptions, either as truthconditional but with meanings explained differently, or as non-truthconditional.

Under the first option - treating 'It is likely that *q*' in the same way as chance ascriptions - the acceptance rule for 'It is likely that *q*' depends on a speaker's ability to recognise when it is the case that *q*; very roughly, he should accept that it is likely that *q* when he has observed that it is the case that *q* in a majority of a range of similar cases. (Here we are ignoring the third level probabilities

which would appear in a proper account). So in the case of 'It is likely that there is a high chance that *p*', a speaker is required to recognise whether or not there is a high chance that *p* in similar situations. But how can this be possible, if one cannot grasp the meaning of 'There is a high chance that *p*' without committing oneself to inferences whose conclusion is the very sentence - 'It is likely that there is a high chance that *p*' - a grasp of whose meaning depends on this act of recognition?

The second horn of the dilemma - treating the meaning of 'It is likely that *q*' differently from that of objective chance ascriptions - seems to escape this vicious interdependence. However if there is a satisfactory way of treating likelihood ascriptions, other than as assertions whose meaning is given by the rules for their acceptance and rejection, then what prevents us treating all SP utterances in the same way? (Ordinary usage seems to make no such distinction).

These difficulties which stem from the circularity problem are peculiar to chance; there are no such problems in the theoretical cases in science in which sentences are held to acquire meaning in terms of the rules for their acceptance and rejection.

Another problem peculiar to chance is the justification of the strictly single-case use of the downward rule, in the inference from an effectively full belief that there is a certain chance that *q*, to the corresponding partial belief. This should be distinguished from the rule whereby beliefs about chances are rejected, although the two are closely related. It follows from the single-case rule 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 in which *q* holds will be close to the chance that *q* (or strictly, to what one believes the chance to be). This fact, a consequence of Bernoulli's Limit Theorem,[1] is the basis of the rejection rule; i.e., roughly, that

if in a large sample of independent cases the relative frequency of cases such that *q* differs markedly from the presently accepted chance that *q*, the belief that there is this chance that *q* is likely to be false, and should be rejected. However, it is not clear that the rejection rule justifies the strictly single-case one. Even if a grasp of the meaning of an objective chance ascription does consist in a disposition to accept and reject such ascriptions according to the proper rules, it still needs to be explained why someone who accepts such a statement as true should adopt the corresponding partial belief. It may be that if he were to adopt some *other* degree of the relevant partial belief, such a person would be lead (by the argument above) to reject his present belief about chances; but why should he adopt *any* degree of the partial belief, given simply that he holds the full one?

In other words, the claim that objective chance expressions obtain their meaning in virtue of the rules for their acceptance and rejection, does not appear to be sufficient to secure the single-case use of the downward rule (and hence to solve the link problem). For this purpose it seems necessary to add that a person has not grasped the meaning of such expressions unless he does apply the downward rule in individual cases. There is no such additional requirement in the scientific cases of this kind (where there is no corresponding gap between the rejection rule and the rule of single-case application).

I have been raising objections to the proposal that the objectivist's upward and downward rules are what gives meaning to objective chance ascriptions. In the present context this suggestion arose from the proposal that in order to escape the rational personalists' problems with the notion of rationality, objectivists should claim that the term 'rational' applies in the first instance to types, or habits, of inference - and only derivatively to individual inferences, in

virtue of what is known about the types to which they belong.

It seems to me that the move to interpret SP rationality in this way is a good one, but that an objectivist doesn't take it far enough, in hanging on to a truthconditional view of SP utterances themselves. For one thing an objectivist is thus lead to reject the apparent widespread substitutability of 'It is probable that ...' for 'It is reasonable to be confident that ...', and vice versa, saying that only the former expression indicates an assertion of a matter of fact. This is an implausible move, and not simply on ordinary language grounds. For if there is a viable notion of objective chance, it would seem to give us the following reductive truth conditions for SP rationality ascriptions: it is reasonable to have a degree *n* of partial belief that *q*, if and only if there is an objective chance *n* that *q*. An objectivist can hardly *deny* that it is to this notion of rationality that the ordinary use of the term 'rational' (or 'reasonable') refers, in SP contexts; at best he can claim that the equivalence isn't such as to provide a *definition* of the required notion of rationality.

In any case, an objectivist who admits such a notion of single-case rationality will not be troubled by all our objections to the rational personalists' use of such a notion. Thus since he does not claim that an SP utterance is an assertion as to the rationality of a certain partial belief, the objectivist will not be concerned by the possibility of a group of speakers able to use SP utterances but not to refer to mental states. And he has no reason to deny that reductive truth conditions for SP rationality ascriptions should refer to objective chances.

However, our objection based on the relational nature of SP rationality ascriptions does seem to apply equally to objectivist accounts. If SP utterances are held to make implicit reference to the body of evidence on which they are based, this evidence needs to be

characterised in a way which is both sufficiently objective to avoid the conclusion that in many SP contexts the terms 'true' and 'false' are not associated with SP utterances in the pattern associated with the notion of a truth condition; and yet sufficiently subjective to avoid the redundancy argument, that the evidence on which we actually base our probability ascriptions and associated actions, is very often much less than that to which SP utterances are being said to make reference (so that an adequate account of our actual behaviour will make redundant the proposed objectivist account). Just as in the rational personalist case (p. 3:13), there seems to be no way to characterise the evidential base of an SP utterance so as to satisfy both requirements.

Our last objection to rational personalism in Chapter 3 dealt with the claim that although SP rationality ascriptions have no non-trivial truth conditions, they are nevertheless truthconditional, and acquire their meaning in virtue of the rules for their acceptance and rejection. We pointed out that since these rules are not deductive they cannot be seen as 'tranferring' truthconditionality. In effect we have already extended this argument to objectivism, in our discussion of the claim that objective chance ascriptions acquire meaning in the same indirect way.

We also argued in Chapter 3 that if the meaning of an SP rationality ascription is explained in this way, then the claim that such expressions are truthconditional can play no part in an explanation of the fact that we have a use for such a combination of upward and downward rules. This argument also seems to extend to the present case: in the absence of a reductive account of the truth conditions of an objective chance ascription, there seems no way in which the objectivist's claim that his combined rule of inference has an intermediate truthconditional stage, can bear on the usefulness of the rule itself.

We have mentioned above the objectivists' suggestion that there is an analogy between SP expressions and the theoretical sentences of scientific theories. This claim is a common and persuasive one, and it is tempting to see it as defusing the above arguments, in two ways: *(i)* to compare the claim that nothing turns on whether SP utterances are said to be truthconditional to an instrumentalist argument that nothing turns on whether there are 'really' theoretical entities; and *(ii)* to see the objection to truthconditionality based on the relational nature of SP utterances as no more significant than the supposed failure of the law of bivalence resulting from presupposition, particularly in theoretical contexts. But I think these comparisons are ill-founded.

For one thing, as we noted above (p. 4:9), there is an important difference between chance 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 are deductive so long as the theoretical sentences concerned are truthconditional. Hence these cases avoid any analogue to the circularity problem which, as we saw, plagues objectivist accounts.

Moreover, in the chance case the acceptance and rejection rules lead to the adoption or rejection of a belief as to a particular *value* of the chance concerned. It is thus misleading to compare chance itself with theoretical entities, whose existence is in question in the application of rules for the adoption and rejection of theories. There is no set of possible observations which would leave chance ranked with ether and phlogiston, as a theoretical entity whose time had passed. At most, for an objectivist, it is an empirical question whether there are chances other than 0 and 1; and even this question is only entertained at the cost of admitting that in many ordinary SP contexts SP utterances do not refer to objective chances (i.e., in all those contexts in

which there is no question of an empirical discovery that ordinary speakers are *wrong* to use the SP utterances they do - in fact to use SP utterances at all).

We have seen that objectivists, like rational personalists, face the following dilemma. If in order to make chance effectively non-relational they objectify the base to which SP utterances are held to refer, they invite the redundancy argument, that an account of the many SP contexts in which we act on evidence less than this objective base will also deal with the only cases in which such an objective chance account can be useful - those in which our actual evidence coincides with the objective evidence. If on the other hand chance is said to be relative to evidence which at least sometimes differs from speaker to speaker, then the ordinary business of agreeing and disagreeing with SP utterances does not in general amount to describing them as 'true' and 'false' in the standard sense. So in this case also the truthcond-itional interpretation is redundant, in a way.

If an objectivist takes the former option, then the redundancy argument provides a sharp contrast with instrumentalism. The argument rests 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 objectivist gives this term). There is no corresponding argument against electrons, say, because there are no situations to which electron theory is not applicable, which both exhibit the kinds of features which electron theory explains in situations to which it is applicable, and are such that an adequate explanation of these features would automatically extend to the class of 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 - Occam's Razor - is very much weaker.

If an objectivist takes the latter option, admitting that chance is significantly relational in order to deal with all SP contexts, then proposal *(ii)* for defusing our arguments against truthconditionality is relevant. How is the failure of strict truthconditionality due to the relational nature of chance different from the failure of bivalence due to presupposition?

For one thing, it is not clear that presupposition does lead to a failure of bivalence - that a sentence whose presupposition is false is neither true nor false, rather than simply false. Dummett has argued against this view (which is Strawson's[2]). Arguing that 'the roots of the notions of truth and falsity lie in the distinction between a speaker's being, objectively, *right* or *wrong* in what he says when he makes an assertion'[3], Dummett asks 'whether there is a place for a convention that determines, just by the meaning of an assertoric utterance of a certain form, that, when all the relevant information is known, the speaker must be said neither to have been right nor to have been wrong'[4]. 'It seems clear that there is no such place', he says.[5]

If Dummett is right only the presupposition case against strict truthconditionality is affected - our argument, from relational chance, is not. For if SP utterances are taken to make reference to the speaker's actual evidence (or to some other somewhat subjective base), we need not claim that there is any gap between such a speaker's being *right* or *wrong* - or between his speaking *truly* and speaking *falsely* - in the standard senses of these terms. The argument is just that no one does use the standard senses in such a context (and hence that from the point of view of such an objectivist account the truthconditionality of SP utterances is not reflected in this aspect of ordinary usage). In referring to another speaker's SP utterance as true or false, or as right or wrong, we consider in effect whether *we* would be prepared to make the same utterance; notwithstanding that if we were to do so, we

would be referring to our own evidence, rather than that of the other speaker. Only if our evidence coincides with that of the other speaker does this way of using 'true' and 'false' conform to the truthconditional pattern.

This way of using 'true' and 'false' 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 own purposes simply by saying 'That's true' or 'That's false'. In cases of presupposition this is impossible: if your conversation were to presuppose Evolution and mine Creation, there would be nothing in common on which such a linguistic device could operate. (This is related to the point that what is in doubt for an objectivist is the value of a chance, not the applicability of the notion itself).

Although contexts of presupposition will therefore not exhibit the same kind of non-standard use of the notions of truth and falsity as chance ones, they may involve some other non-standard use. Dummett recognises this, but describes it as 'the notion of falsity ... being understood as more restricted than that of an assertoric utterance the speaker is wrong to make'.[6] So he doesn't acknowledge the possibility of such contexts in which the notion of a speaker's being *wrong* is also non-standard, in which a speaker is not said to be *wrong* by those who do not share his presupposition (unless what he says is seen as mistaken from his own point of view, perhaps). There seem to be scientific cases of this kind, particularly in which a number of separate theories apply to a given range of phenomena, each theory explaining some of the facts better than any of the others. But in such a case few people are committed to one particular theory; most will work in terms of whichever is most useful for the situation at hand, and there will be a

tendency to deny that what is said describes the 'real' state of affairs. The lack of truthconditionality is thus quite open (and seems to provide no significant objection to Dummett's account).

These cases are very different from SP contexts, where no one switches from one body of evidence to another and back again, ascribing chances on the basis of each. Moreover, it is in the most mundane of SP contexts we rely on the most subjective evidence, and in which the lack of truthconditionality is hence most pronounced. Scientific cases of this kind are at the opposite extreme, most distant from everyday contexts.

It may be that there are more significant presupposition contexts in which both 'false' and 'wrong' are used non-standardly. If so, they will be analogous in this respect to SP contexts (as interpreted by an objectivist account which takes SP utterances to refer to a more or less subjective base of evidence) - though the nature of the non-standard use will differ. But since it will not be in some trivial way that the utterances involved lack truthconditionality, the possibility in no way undermines our argument in the SP case. On the contrary, it allows that there may be other contexts in which the truthconditional view is too easily taken for granted.

If there are such presupposition contexts, the expressions involved will function as truthconditional whenever a group of speakers share the relevant presupposition. In this case the non-standard use of 'false', 'wrong' and so on coincides with the standard one. The same is true for SP utterances (when a group of speakers share the same evidence), and incidentally for our query ascriptions of Q-English (Chapter 3). It no more shows that we should expect a truthconditional account of SP utterances in general than it does for query ascriptions or sentences embodying such presuppositions. The apparent truthconditionality of such cases should be explicable in terms of the general

account, rather than the other way round.

Thus the supposed analogy between chance and the theoretical entities of science, exploited by objectivists in several ways, seems to be a poor one. And although the claim of a similarity between SP contexts in which speakers rely on different bodies of evidence, and contexts of presupposition, fares a little better, it also turns out to be of little use in defending objectivism.

Objectivism thus seems no better off than rational personalism, having to meet almost the same objections to truthconditionality. And we have seen that the one account from *2.1 - 2.8* which falls into neither category - i.e. *2.5*, the subjectivist option - is not a satisfactory alternative. So there is a case for looking further afield. In Chapter 6 I shall introduce an approach which rejects the assumption that SP utterances are a particular kind of assertion. Before that, in the following chapter, I want to try to balance the abstract nature of the argument so far with an example of its application to an actual account of single-case probability.

Notes

1. Or Law of Large Numbers, as it is often called; see de Finetti (1964), pp. 124-7, for example.
2. Strawson (1950), and (1952), pp. 175-179.
3. Dummett (1978), p. xvii.
4. ibid., p. xviii.
5. ibid., p. xviii.
6. ibid., p. xviii.

Bibliographical Notes

The view that the notion of rationality, or reasonableness, applies in the first instance to habits of inference, rather than to single inferences, is advocated by Ramsey (who says he takes it from Peirce); thus, 'Following Peirce we predicate (reasonableness) of a habit not of an individual judgement' (Ramsey, 1931, p. 199; see also Ramsey, 1978, pp. 96-98).

Accounts which treat chance as a theoretical concept, implicitly defined by its acceptance and/or rejection rules, include those of Braithwaite (1953, Chapter VI) and Levi (1967, pp. 197-204). 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'.

This account of chance is criticised by Sklar (1979), who argues that the proposal that chance statements acquire meaning in virtue of their role in a theory - and particularly in virtue of acceptance and rejection rules - undermines the ontological significance of the notion of chance. Our argument (p. 4:12; and for the rational personalist case, p. 3:15) that given such an account of chance, the claim that SP sentences are truthconditional can play no part in explaining the usefulness of the combined rule of inference, is similar. Sklar regards his conclusion about chances (so defined) as analogous to such 'instrumentalistic' claims as Quine's of the underdetermination of radical translation, in semantics; and Reichenbach's of the undetermination of geometry (Sklar, 1979, p. 412). Note that these forms of instrumentalism are different from (and better established than) that with which we contrasted our objections to this kind of account of objective chance (pp. 4:13-14).

5. *MELLOR'S CHANCES.*

In this chapter I want to consider the views on single-case probability of D. H. Mellor. The choice is not an arbitrary one: Mellor's account is one of the most sensitive to the problems of the single case. It is also one of the more recent accounts, given that its original formulation[1] has very recently been supplemented.[2]

Mellor claims to make sense 'of chances being objective, empirical and not relational, and applying to the single case'.[3] In our terminology, however, his account often seems more rational personalist than objectivist. Thus the 'main claim' is 'that chance statements assert some degrees of belief to be made more reasonable than others by objective empirical features of the world'.[4] Mellor admits propensities, but not as themselves a variety of objective chance (as propensity accounts along the lines of *2.1* and *2.2* have it). Mellor's propensities are dispositions to display, in appropriate circumstances, chance distributions over possible outcomes - i.e. in effect at least, distributions of degrees of rational partial belief.

Among the problems from Chapter 3 to which Mellor's account thus seems subject, is that of saying what it *means* to assert that a particular degree of partial belief is *reasonable*, in a given (single) case. We suggested that one way to avoid this problem is to argue that such single-case rationality ascriptions have no *non-trivial* truth conditions, but to claim that they are truthconditional nonetheless, picking up their meaning via the rules for their acceptance and rejection. Mellor does not take this line, however. Rather, in a key argument, he offers a characterisation of the notion of rationality which is involved in such ascriptions.

The characterisation depends on the following argument, itself designed to show that when chance is 'specified as a fact justifying

degrees of belief', it 'will relate to frequency in the way we know it does'[5]:

> Suppose a degree p of belief in a coin landing heads is justified by knowing that p is its chance of doing so. To get what follows from this supposition about the frequency of heads on repeated tosses, I appeal to the laws of large numbers. By these it follows in particular that as high a degree p' of belief as we like, short of 1, is justified in any frequency proposition F of the following form: in enough such tosses, the frequency of heads would be within δ of p, where δ is any positive real number, however small.
>
> This very high degree p' of belief in the propositions F looks as if it needs another chance to justify it; and if that were so, there would be an arguably vicious regress. But p' does not need a chance to justify it, as the following argument shows.
>
> In Chapters 1 and 2 of (Mellor (1971)) I argued that a degree of belief increasing towards 1 must turn into a full belief before it gets there. ... For a belief to be justified, therefore, its justified degree need not be 1; it need only be a degree sufficiently close to 1. This being so, it follows that as the justified degree of a belief tends to 1, it will turn into justified belief somewhere before it gets there. Where it does so will depend on context, but that is immaterial here, since the laws of large numbers can get the justified degree of our belief in any F as close as any context could conceivably require. So whatever the context, our assumption justifies us in simply believing every F, and hence in believing that, in a sufficiently long run, the frequency of heads would come indefinitely close to the chance p. ...
>
> Chances specified as facts justifying degrees of belief do therefore entail the corresponding hypothetical long run frequencies.[6]

Relying on this argument, Mellor offers the following account of 'what it is for a degree of belief to be justified by a fact about an event'[7]:

> The sense of justification we require has to be factual rather than, say, moral: an outcome of the coin toss is to some degree to be expected, not to be approved or deplored. Yet justification here is not a matter of making a belief true, since truth applies to the content of a belief, not to its strength. There needs to be some other mode of justification by facts which relates specifically to

to the degree of a belief rather than its content.

> To see what this mode of justification is, consider ... the connection between betting and degree of belief ... . I argued in (Mellor (1971)) that people's choices of coherent betting quotients (CBQs) show how strong they think their beliefs are, provided they suppose the betting situation to be restricted in specified ways in order to exclude any effect on their choice of attitudes other than the belief whose strength is to be measured. I then used the entailment just established to show that only at a CBQ = p can I know that I would break even in a long enough run of bets on coins landing heads when their chance of doing so is always p. Now under the restrictions needed to make my CBQs measure my belief, ... breaking even is the best result I could possibly hope to know of. So I have a plain gambling rationale for choosing this CBQ in that situation; and hence for having the degree of belief which, in that situation, this CBQ measures.[8]

The way in which Mellor talks about the relation between chance and partial belief in these passages perhaps makes his view seem more objectivist than rational personalist. This is not entirely a terminological quibble, for we have seen that the two approaches depend on a notion of rationality in different ways. However, given that Mellor admits a question as to the nature of the notion of rationality, or justification, which is relevant to SP contexts, I think we can ignore the classification issue here.

Notice that Mellor's account of the relevant notion of justification relies on an argument in which the notion itself plays a central role. It is not obvious that the circularity is vicious, but I think it is.

The laws of large numbers are consequences of the standard axioms of the probability calculus, and are hence satisfied in any domain which provides a model of these axioms. So in the domain of coherent partial beliefs, in particular, someone who believes to degree p that a given coin will land heads when tossed should, for coherence, hold an appropriately high degree of partial belief that the frequency of heads

in a given large number of such tosses will be very close to *p* (if he holds any belief at all about this frequency, at least, and if he regards the tosses as independent; all this can be made more precise). In other words, the requirement of coherence suffices to ensure that someone who holds a partial belief will, if the matter arises, adopt the corresponding effectively full belief that in the sufficiently long run, the frequency of whatever it is would come indefinitely close to the degree of the partial belief. In a gambling context of the kind Mellor describes, such a person will believe that so long as he always chooses the CBQ corresponding to his degree of belief, he will break even in the long run.

Mellor certainly doesn't want to say that any partial belief is justified, so long as the person holding it arranges his beliefs coherently. So in Mellor's sense, justified belief must provide something more than a full belief in breaking even in the long run. What it provides, he says, is the *knowledge* that one would do so - a *true* and *justified* effectively full belief.

The claim that a justified partial belief provides *knowledge* about the corresponding long run, rests on the main argument, connecting chance 'specified as a fact justifying degrees of belief' with hypothetical long run frequency. This argument runs with an as yet undefined notion of justification. Although this procedure is not necessarily objectionable in itself, it is crucial to the argument that an effectively full belief which is *justified* in this undefined sense, is also justified in the sense standardly associated with full beliefs (whatever sense that may be), and is therefore true. As we have seen, if a partial belief is said to be justified if it coheres with the rest of the holder's beliefs, the limiting argument licensed by the laws of large numbers shows that the corresponding full belief in a hypothetical long run frequency will be justified in the same sense; but this

is no guarantee that it is true, or, for example, causally justified.

Hence it needs to be established that the argument is dealing with a notion of justification which, so to speak, behaves properly in the limiting cases. But how is this possible, if the conclusion of this argument is required in order to define the notion of justification for a partial belief?

The required behaviour in the limiting cases might be taken as a theoretical assumption of an account which says that the relevant concept of justification is a theoretical one, with no reductive definition. We have noted moves of this kind at several points in previous chapters. Here the difficulty is that it leaves justification defined, effectively, in terms of the hypothetical long run frequency; or in terms of whatever it is that is held to justify - in the sense in which *justified* true belief is knowledge - a true belief with respect to the long run frequency. And these are options which Mellor explicitly rejects:

> What has a disposition to produce a long run frequency of heads on other, mostly nonexistent, tosses to do with my prospects of getting heads on this actual toss? It is no use doing what Hacking (1965, p. 135) does with his "frequency principle", namely in effect defining the concept of justification or support to be such that this disposition supplies it. That just provokes the question: why should I adopt for this toss the degree of belief that is justified in that sense? And to that question I know of no sufficient answer.[9]

In other words, if the required notion of justification is introduced as a theoretical concept, anchored by the assumption that it corresponds to the standard notion in the limiting cases, it leaves open what is then the central question: why ought we to believe what is justified, in this theoretical sense?[10]

Mellor draws an analogy between knowledge of chances and knowledge of colours. The above passage continues:

> We cannot get to justified degrees of belief starting from frequ-

> encies, actual or hypothetical, or from dispositions to produce them. The only way to start, as with colour, is by specifying the fact by the degree of belief it is supposed to justify. And if we do that, the frequencies, as we (have seen), ... take care of themselves.[11]

Specifying single-case probabilities by the degrees of belief they justify, or make reasonable, is the essence of rational personalism. Its great strength, as we saw in Chapter 2, and as Mellor emphasises, is to avoid the objectivists' problem with the downward rule; to avoid the question: why should we adopt partial beliefs, and act, in accordance with our estimates of chance? However, as we also saw, the cost of this move is to shift the burden of providing truth conditions for SP utterances on to a notion of single-case rationality. In Chapter 3 we argued, in effect, that it is doubtful whether this notion is capable of bearing this weight.

The analogous step in the colour case is simple in comparison. The fact that there is a (more-or-less) invariant causal connection between an object's being of a certain colour and the resulting state of belief of a normal observer gives a relatively clear single-case sense to the notion of a colour belief being justified. The close association between 'justified' and 'true', said of full beliefs, depends on the single-case application of the notion of justification involved.

It seems to me that Mellor offers no adequate alternative sense of justification for the chance case. The sense he does offer, in the argument we have discussed, seems unsatisfactory.

In view of the proposed analogy between chance and colour, it might seem plausible that the adoption of partial beliefs is also a causal matter. Thus the claim would be that standard humans react to certain kinds of situations in standard ways, by adopting certain partial beliefs. Given that standard humans are 'programmed' in a particular way, their adoption of these beliefs is a direct result of

the relevant features of the situations concerned.

I think this is on the right track. However, it will not allow rationality (or justification) ascriptions to be taken as reporting, or *describing*, such a causal inference. To read 'A degree *p* of partial belief that *q* is justified' along the lines of 'A standard human is lead to adopt a degree *p* of belief that *q* in these circumstances' will simply invite the question: is it reasonable to behave in the standard way in this case? In the colour case the analogous 'A standard human would see this as green' prompts an analogous question: is it *really* green? In both cases the question would be senseless, or trivial, if the state of belief resulting from the causal inference were *about* the inference itself, in the way proposed.

The main difference between the two cases is that only in the chance one is there a serious proposal leading, as we have seen, to the claim that the beliefs and utterances which result from the causal inferences concerned are about these inferences. This proposal - an offshoot of the rational personalist programme - was a reaction to doubts raised on several grounds as to the truthconditionality of SP utterances. If objections were to be raised to the truthconditionality of colour ascriptions, this difference might vanish. It would then turn out that colour ascriptions cannot be made truthconditional by being taken to be about the effects of situations on the minds of standard humans. Just as with chances, such a proposal does not adequately represent the standard use of colour ascriptions, which might therefore be taken be taken to be non-assertive expressions of 'colour attitudes' towards objects.

This doesn't mean that the only difference between chances and colours is that, so to speak, we are at present objecting to the truth-conditionality only of utterances about the former. It is rather that the case against truthconditionality is much stronger for chances than

for colours.

We have seen that it is the relational character of single-case probability which casts most doubt on the truthconditionality of SP utterances. The resulting proposal that such expressions make implicit reference to 'the existing evidence' faces a dilemma: if the notion of the existing evidence is taken subjectively, the resulting truthconditional expressions fail to represent the way in which the notions of truth and falsity are associated with SP utterances in use; but if 'existing evidence' is taken objectively, there will be many situations in which SP utterances depend on evidence of a less objective level. In the latter case the proposed truthconditional account faces a redundancy problem: if some other account can make sense of contexts in which SP utterances depend on evidence less than this objective base, what is to prevent us using this other account, rather than the truthconditional one, in cases in which the levels of evidence coincide?

Mellor takes the latter course, and makes it clear from the start that he is not offering an account of all SP contexts:

> My project is ... limited. I am concerned only with statistical probability, which I call 'chance'. The chances of coins landing heads, of people dying and of radioactive atoms decaying concern me; the probabilities inconclusive evidence perhaps lends to hypotheses on these and other matters do not.[12]

As the base of evidence on which a chance ascription depends, Mellor takes the extent of what is causally available, to a being of our limited 'conceptual and perceptual abilities'.[13] This is sufficiently objective to avoid the situation in which two speakers in conversation are referring to different bodies of evidence: if two speakers are in sufficient causal contact to enable them to converse, then presumably what is causally available to one is causally available to the other. Hence Mellor is able to claim that chance is non-relational. However, there are many SP contexts in which the evidence on which a speaker

relies, and on which he bases some action, is less than is causally available to him (we gave examples in Chapter 3, p. 3:12). If an adequate account can be given of our behaviour in these contexts - as presumably it can - why should it fail to deal with the contexts to which an account such as Mellor's applies?

Mellor seems to feel that this argument goes the wrong way: 'Inductive probabilities are, I suspect, all descended from chances', he says.[14] He offers the following argument:

> Perhaps however there does not need to be a chance of snow, only an inductive probability of it relative to truth-making facts about temperature and pressure? Not so. Bayesian decision theory does need chances, as Jeffrey ... has remarked. Suppose I am a smoker deciding whether to give up the habit because of my fear of cancer. For me, smoking "dominates" not smoking, i.e. I shall prefer to smoke whether I have cancer or not. But I also know I should very much prefer not to have cancer, and I think cancer much more probable if I smoke. In short, the degrees of my relevant beliefs and desires make the theory tell me to give up smoking. And so I should, but only if what justifies the degrees of my conditional beliefs are propensities, not merely inductive probabilities. Since I prefer to smoke in any case, I would be a fool to quit if my smoking were merely better evidence than my not smoking for the hypothesis that, whether I smoke or not, I shall get cancer. Quitting can only be justified if it is an action which will cause a change in my prospects, namely a reduction in the probability of my getting the disease. But a probability which has causes is a part of the physical world, not merely part of inductive logic; that is, ... a chance. In other words, inductive probabilities are not enough to make sense of ... Bayesian decision theory. The work objective probability has to do there can only be done by chance.[15]

I don't want to defend inductive probabilities as a means of supplying truth conditions to SP utterances. But I do want to dispute that objective chances are essential to decisions of the smoking kind. Consider the following case: I have been arrested for robbery, in circumstances which leave me no defence. I know I will be convicted, and would like as short a sentence as possible. A sympathetic court

official, in charge of scheduling cases, offers me a choice of Judge A or Judge B. Judge A, he tells me, gives a short sentence (in the event of conviction) in every second case on his list; Judge B, in every third case. He explains that although he could find out where I will be on the list of whichever judge I choose - and hence whether I will get a short sentence - it would give the game away to make the necessary enquiries, and although I would have a short sentence, he would lose his job. So I have to choose without this extra information. Obviously I should choose Judge A.

Whichever judge I choose, however, it is only a matter of objective chance whether I get a short sentence in the trivial sense that my chance of doing so is 0 or 1 - if 'objective chance' is taken in Mellor's sense, at any rate. On the evidence causally available it is effectively certain whether or not I get a short sentence, once I have chosen my judge; and there is no guarantee that in choosing Judge A I increase my chance of a short sentence - I may make it 0.

The example rests on the fact that not all the evidence causally available to the court official and me is in fact available to us, given the practical constraints of the situation. So it might be objected that chance ascriptions refer implicitly not to the evidence causally available *in principle*, but to what is available *in practice*. The trouble with this is that it is too subjective; it allows for situations in which two speakers in conversation are referring to different bodies of evidence. The problems this leads to for truthconditionality, we have already seen.

A different move, perhaps, would be to claim that in such a case we make use of objective chances as a useful fiction, though recognising that they are not strictly applicable. This runs into very serious difficulties, however.

For one thing, in the supposedly analogous cases on which this

claim will depend - cases in which scientific theories are held to be used a useful fictions - it seems important that some explanation be available, at least in principle, of the fact that a theory which is acknowledged to be false is nonetheless useful. Such explanations are of several kinds. Sometimes the false theory provides a close approximation, in some sense, in a certain range of cases, even though it has been replaced in general by a theory which deals with these cases and others besides; then it is the new theory which justifies the continued use of the old one in a restricted range of cases. Newtonian mechanics is the best-known example of such a superseded theory. In other cases a theory may depend on assumptions which are known from the start to be false, but which are regarded as useful idealisations; the assumption of continuity in the treatment of fluids is an example. The fact that such an assumption does not lead to serious error seems usually to be explicable in terms of the theory in virtue of which the assumption is regarded as false (in the fluid case, in terms of molecular theory - particularly the extreme smallness of molecules in relation to ordinary fluid bodies).

No account of these kinds seems possible in the chance case. There seems to be no relevant sense in which my choice of Judge A approximates to one based on a real objective chance (in Mellor's sense). And since my position on the court list of whichever judge I choose determines whether my chance of receiving a short sentence is 0 or 1, the objective chance account does not justify my acting in ignorance of this position.

It might be said that my decision approximates that of someone faced with the same choice, but whose position on the list of the judge he chooses is not causally available to him even in principle. Assuming that such a person has no greater chance of receiving one position than another on the list of whichever judge he chooses, his objective chance

of a short sentence is 1/2 if he chooses Judge A, and 1/3 if he chooses Judge B. My decision closely resembles this person's, the claim would be, so I should act in the same way.

But why 'should' I do so? If the account is to parallel the scientific cases in which false theories are useful fictions, and is to support Mellor's claim that chance is indispensable in making such decisions, then chance must somehow explain why I should behave in this way. Now we have seen that objectivists have trouble with the question to which this one presupposes an answer, as to why one should choose betting quotients (for example) in accordance with one's estimate of the *actual* chance. We have seen also (pp. 5:5-6) that Mellor recognises the importance of the latter question, and that the rational personalist aspects of his account are an attempt to provide a solution. But we have argued that he fails to establish an adequate notion of rationality, or justification. This continuing difficulty with the downward rule suggests it is rather unlikely that chance can play a crucial role in the present case, which involves action on less than an estimate of the *actual* chance.

If this impression is mistaken, moreover, such a 'useful fiction' account is in great danger of being too successful. If it establishes unreservedly that it is reasonable for us to behave as someone should for whom the evidence available in principle, is just the evidence we have in fact, it will run into an objection raised by Ayer to certain types of relational and frequency accounts of probability: that it is unclear why, given the choice, we should prefer probability judgements based on larger amounts of evidence.[16] No matter how little evidence we have, we can imagine someone to whom no more is available, even in principle. If it is therefore reasonable to act in the way that this imagined person should, according to what are for him the objective chances, then why should we ever take the trouble to obtain more

evidence? More general second-level evidence might indicate that the more first-level evidence we pay attention to, the more chance we have of being successful (in some sense) in our resulting behaviour. But why should this concern us, if we have yet to be convinced of the advantages of taking into account more than our present (first-level) evidence?

Thus it seems that objective chances, in Mellor's sense, are no help in explaining and justifying my choice of Judge A rather than Judge B. Yet this simple piece of Bayesian decision theory differs significantly from Mellor's smoking example only in the nature of the obstacle which prevents the agent concerned obtaining more information relevant to his decision. The smoker knows that smokers encounter carcinogens more frequently than do nonsmokers, and wishes to avoid such contact himself. But he doesn't know whether if he continued to smoke he would actually encounter such substances in sufficient quantity to give him cancer (or whether he would do so - or has already done so - even if he now gives up smoking). The prisoner knows that people tried by Judge B receive long sentences more frequently than those tried by Judge A, and wants to avoid one himself. But he doesn't know whether he would in fact do so even if he went before Judge B, or fail to do so before Judge A. (Note that it would be easy to add to the latter example something analogous to individual variation in susceptibility to a given carcinogen). There seems to be no difference between the two cases sufficient to show that objective chances play an essential role in guiding and justifying our behaviour in the former one, given that they play no such role in the latter.

We have been discussing a passage which seemed to indicate Mellor's response to our redundancy argument - i.e. to the claim that since there is presumably some adequate account of contexts in which SP utterances depend on less than an objective base of evidence, and such

an account can be expected to deal with the remaining SP contexts as well, there is no need for a separate objective chance account (and hence that if only objective chances enable SP utterances to be construed as truthconditional, the truthconditional view is itself redundant). Note that this claim does not commit us to denying that there are contexts in which SP utterances *appear* to be truthconditional, in various ways; and in which terms such as 'probability' and 'chance' appear to have objective reference. On the contrary, we have argued that what most reveals the lack of truthconditionality of SP utterances is the dependence of many of them on subjective bases of evidence - in which case (as we have noted, p. 4:17) it is to be expected that when a group of speakers share a common base of evidence, their SP utterances will appear truthconditional. Such speakers will associate the terms 'true' and 'false' with SP utterances in what, given an objectivist or rational personalist reading of these utterances, will seem to be the standard way - the way associated with the notion of a truth condition. In such a context it will not be necessary to admit, for example, that the response 'That's false' to an SP utterance constitutes not a criticism of the previous speaker, in the usual sense - not a standard *denial* of what has just been asserted - but rather the expression of a different point of view.

There seem to be contexts in which such a common base of evidence is the norm, and in which the subjective character of SP utterances is therefore well hidden. It is these contexts which have appeared to require a notion of objective chance, in order to explain the meaning and use of the SP utterances involved; or at least to admit such a notion, in the absence of some of the difficulties of other SP contexts. I have suggested, however, that the relation of these contexts to more subjective ones is that of special to general cases. If so, then the search for an account of objective chance seems misconceived:

what is needed is an account of the general case, from which an account of the special cases can be expected to follow.

Moreover, even if we put aside the redundancy argument there are still problems to do with the class of contexts which are supposed to involve objective chances. For one thing, the most plausible cases are usually held to be scientific ones. But it is important to bear in mind that many of the expressions used in scientific contexts in which terms such as 'chance' and 'probability' occur are not SP expressions, but rather statistical generalisations of some kind. There is no way to paraphrase 'A radium atom has a 50% chance of decaying in 1622 years' in the form 'There is a 50% chance that *q*', where *q* is a well-formed sentence, for example. It is possible that such generalisations can be given truth conditions other than in terms of objective chances (in terms of frequencies, say). So the apparent truthconditionality of such expressions cannot be taken as an argument for objective chances in the same way as that of SP utterances. It also has to be shown that no other account will do the job (over and above a similar demonstration in the SP case - and, we should perhaps add, an argument in both cases that *apparent* indicates *real* truthconditionality).

A more serious difficulty for an account such as Mellor's is that it is not clear that in all contexts in which a group of speakers share a common base of evidence, with the result that their SP utterances appear truthconditional, this common base lies at the same level of objectivity as in other such contexts. In fact it is not difficult to see that such contexts differ a great deal in this respect. A group of doctors discussing a patient's chances of survival may all be relying on the same evidence; but know that if another piece of equipment was available, say, their estimate might be quite different. The same patient's friends might share another body of evidence, and dispute his chances on that basis. In general when a given event or proposition

instantiates various different descriptions or generalisations, different theories may ascribe it different chances. In the context of any one such theory the relevant SP expressions will function truthconditionally, but their doing so does not provide evidence of a real objective chance. For which theory-relative chance would it be?

In other words, the objectivism of usage which results from the fact that a group of speakers may base their SP utterances about a certain matter on the same body of evidence, does not in itself seem to indicate that such speakers are referring to non-relational objective chances, as Mellor proposes. Features of many, if not all, such contexts actually seem to preclude this interpretation. At this point our previous remark, that expressions such as 'A radium atom has a 50% chance of decaying in 1622 years' are not of SP form, is relevant. It is tempting to fall back on such expressions as examples of the use of chances which are secure from the objection that a given event may be given different chances under different descriptions. But they are secure from this objection precisely because they are statistical generalisations rather than SP expressions. And hence they provide no support for the claim that objective chances are needed to account for our usage and behaviour with respect to the single case.

This kind of problem is not peculiar to Mellor's view of chance, but plagues all the standard accounts. A hypothetical long-run frequency view, for example, relies on the idea of the repetition of the situation to whose outcome a chance is to be applied. But as is often pointed out, what counts as a repetition depends on how the situation is characterised. Saying what is to be included in the characterisation is just the problem of specifying the relevant level of evidence. However Mellor has an unusual response to the point we have just raised: he allows for 'an event instantiating more than one statistical law, attaching different chances to the same result'.[17] 'There is nothing paradoxical in

one outcome of an event having two chances', he says, 'even if it is uncommon. All it means is that two degrees of the same belief could both be objectively justified; and that can be so even if no one could actually have them both at once.'[18]

I think the difficulty with this proposal is Ayer's objection, which we have noted above (pp. 5:12-13). If an event has different chances under different characterisations, in what sense can it be better to adopt a partial belief corresponding to one rather than another? And what motive can there be for preferring a chance based on more evidence to one based on less? We have seen that it won't do to rely on a second-level chance ascription to answer these questions, because while the answers are in doubt we have no reason to heed the evidence on which such a second-level ascription would be based. It doesn't follow that no satisfactory answers exist; but it seems there can be none in terms not available to a frequentist, say, for whom the parallel question is 'Why should we act on the basis of the most complete available frequency evidence?'. Mellor in particular claims to do better than the frequentists, in avoiding this kind of question. But he can only do so if chances are unique and non-relational - a state of affairs which we now see receives far less support from the apparent truthconditionality of much of the ordinary use of SP expressions than might at first be supposed.

I want to end this chapter with some remarks about a respect in which Mellor's account seems more in accord with the view I shall be supporting than are most accounts of single-case probability. Mellor says that the reason 'it has proved so hard to frame an acceptable account of objective chance' is that 'people naturally feel that, if chance is objective, it must make true beliefs with some characteristic content', 'supposing that the only objectifying job facts can have to

do is making beliefs true'.[19]

> But this is not so, as we may see in ethics. An objectivist there will naturally give truth conditions, such as: '*X* is good' is true if and only if *X* is good. This truth condition, like that for chance, is not trite; to give it, the objectivist has to think there are facts such as *X* being good, and that *X*'s having some other properties would make that fact obtain. But he is not thereby committed to thinking that this fact's role is to make true a belief whose content is that *X* is good. Obviously not: its role is objectively to justify a mental state quite different from belief, namely approving of *X*. The content of all the relevant beliefs about *X* is entirely non-moral. Morality, like chance, has no subject matter of its own. It may be objective nonetheless; beliefs are not the only mental states capable of objective justification. Failure to see this underlies two views in ethics which correspond closely to frequency and subjective views of chance. One tries to provide a distinctive content for the belief that *X* is good; e.g. that *X* promotes human happiness. The other sees that this misses the whole point of morality (since it remains an open question whether one should approve of promoting human happiness or anything else), and concludes that there is no objective goodness at all.
>
> Properly to recognise both objective goodness and objective chance, we need to extend our conception of facts as suppliers only of true beliefs.[20]

Note, firstly, that views to which Mellor is objecting here can agree that the *ultimate* role of a chance fact is to justify a partial belief. Objectivist views which recognise the question 'Why does a full belief about a chance make reasonable a corresponding partial belief?' (as all such views should) do so because they recognise that the key role of chance facts is in some sense to guide our partial beliefs and hence our actions. The issue here is thus whether there are really full beliefs about chance facts, as a step in the process whereby such facts carry out this function. Mellor is apparently denying that there are such beliefs. I think he is right about this, but it is a view which seems to me to be difficult to reconcile with other aspects of his

account.

One problem is this: given that we do say things like: 'There is a chance of 1/2 that this coin will land heads when I next toss it', Mellor apparently has to claim either *(i)* that such an utterance does not make an assertion about one of the objective chances whose existence he accepts; or *(ii)* that it does make such an assertion, but that what it asserts cannot be believed to be the case in the usual way. If *(i)*, then what could be the evidence for thinking that there are objective chances (given that apparent assertions about such things are in fact to be taken as examples of some kind of non-assertive idiom)? But if *(ii)*, then what notion of belief could be involved? And what notion of assertion? On the usual view (summarised in *1.9 - 1.12*, pp. 1:9-10), an assertion characteristically results from, and is a key display of, a corresponding effectively full belief. Moreover, if there are objective chances then someone who is prepared to make an assertion such as the above one will presumably exhibit the non-linguistic behaviour which would ordinarily be taken as displaying the disposition in which the full belief associated with this assertion would consist. That is, roughly, such a person will behave as if there is a chance of 1/2 that the coin will land heads; he will be willing to bet a large stake for a small return that there is this chance, for example, and in general will behave in whatever way is to his advantage if there is such a chance. What non-arbitrary grounds - other than an acceptance that there are really no such things as objective chances - could there be for denying that he actually has the belief of which this behaviour is the display, according to the usual picture of belief? Of course we would expect such a person also to have the disposition characteristic of a partial belief of degree 1/2 in a result of heads. Saying *why* he should hold the latter disposition, given that he holds the former one, is for an objectivist the task of justifying the downward rule of

inference (of solving the link problem, as we called it in Chapter 2). It doesn't help in doing so to refuse to call the former disposition a belief.

Mellor says that 'for me to think there is a chance of heads is ... for me to have a degree of belief in heads and to think it justified ... by some fact about the coin toss'.[21] So to *say* there is a chance of heads is presumably to express this partial belief and to say, at least implicitly, that it is justified. It is remarks like these which give Mellor's account its rational personalist character. Rational personalism demonstrates that so long as there is an adequate notion of single-case rationality, or justification, SP utterances and beliefs - including those which mention chances - can quite well be construed as assertions and beliefs about rational partial belief. Mellor thinks that there is such a notion of justification (we have seen that he takes pains to define it), and at least at some points seems close to the rational personalist use of it. So from his point of view his rejection of beliefs about chances 'with some characteristic content' seems unecessary. Thus although I am in sympathy with the move itself, I think it fits uneasily with other features of Mellor's account.

Notes

1. Mellor (1971).
2. Mellor (1979); this paper is Mellor's reply to Salmon's (1979) discussion review of Mellor's (1971).
3. Mellor (1971), p. 175.
4. ibid., p. xii.
5. Mellor (1979), p. 15.
6. ibid., pp. 16-17.

7. ibid., p. 17.

8. ibid., pp. 17-18.

9. ibid., p. 15.

10. The argument of Mellor's we have criticised here is a restatement of an earlier one (1971, pp. 160-164), in response to objections from Salmon (1979, pp. 10-16). One of Salmon's objections (pp. 14-16) appears similar to ours. However it seems that Salmon is criticising Mellor's derivation of second-level justified partial beliefs from the laws of large numbers; whereas we are objecting to the next step, on the grounds that it needs to be shown that the sense in which 'justified' is being applied to partial beliefs is such that increasingly strong such partial beliefs do approach a *justified* full belief (where the term here has the sense standardly associated with full beliefs).

11. Mellor (1979), p. 15.

12. Mellor (1971), p. xi.

13. Mellor (1979), p. 28.

14. ibid., p. 1.

15. ibid., p. 23.

16. Ayer (1957), and (1963), Ch. 7.

17. Mellor (1979), p. 29.

18. ibid., pp. 29-30.

19. ibid., p. 21.

20. ibid., pp. 22-22.

21. ibid., p. 19.

6. *PARTIAL ASSERTIONS.*

The previous chapters have been critical but not constructive. We have argued against an assumption which seems to underlie most existing accounts of single-case probability, but have not suggested a view which escapes these arguments. I want now to outline such an account. This approach, not surprisingly, will face objections of its own. I shall try to defend it against some of these here and in later chapters.

The views we have been criticising had in common the assumption that SP utterances are a particular species of assertion, as the basis of their various answers to *1.5*. And it is this assumption we have been aiming to criticise, more than the particular accounts which rely on it. So an alternative account should reject this assumption, placing SP utterances in some other linguistic category.

In order to see what this category should be, I think it will be helpful to observe that language is among other things a means by which a mental state in one person can give rise to a certain mental state in another person - the nature of the latter mental state depending on the nature of the former one. This kind of connection occurs in non-linguistic ways as well - for example when X's desire to hop causes him to do so, which results in an awareness of X's hopping in the mind of Y. But language seems to be distinguished particularly by its use of a system of conventional signs, the conventions employed by the speaker needing to be understood by the hearer for the connection in question to be established.

Even when this condition is satisfied, by no means all utterances connect a pair of mental states in a straightforward way. Many things can go wrong: there may be no hearer; the speaker may be insincere, or talking unconsciously (as in sleep); the hearer may not trust the

speaker, or his judgement; and so on. We shall see below that the possibilities depend to some extent on the nature of the utterance concerned. However, in general it seems to make sense to ask of a given utterance what mental states it *would have* causally connected, if all the necessary conditions had been satisfied.

The importance of this move is that it promises to provide a means of classifying utterances in terms of a classification of mental states: the category of an utterance will depend just on the categories of the ordered pair of mental states which the utterance in question would, in ideal circumstances, causally connect. It is true that this approach is limited by several factors. It seems too dependent on access to a speaker's intentions to be applied as a primary classification of a language we don't speak ourselves, for example. And it relies on an ability to classify mental states independently of the types of utterance to which they characteristically lead (and from which they characteristically result). But these limitations do not restrict certain uses of this technique for classifying utterances - which seems, in particular, to be a useful approach to SP utterances.

It is not obvious that this technique will give us categories of utterance corresponding to more usual classifications. But if making possible causal links between mental states of different minds is the fundamental role of language, then it would not be surprising if differences with respect to this role turn out to underlie what have been seen as the significant divisions of the class of all utterances. This seems to be the case, most importantly, for the class of assertions, for which the relevant pair of mental states is a pair of effectively full beliefs with a common content. That is, when all the conditions are satisfied, X's *assertion* that *q* to Y is the means by which X's belief that *q* gives rise to Y's belief that *q*; if X hadn't made the assertion, Y wouldn't have come to have that belief at that time (and given that

X is speaking sincerely, and so on, he wouldn't have made the assertion if he didn't have the belief that *q*).

In these terms, we have offered three main types of argument against the view that SP utterances are assertions. One involved exhibiting the difficulties which result from the admission of full beliefs about probabilities, as well as the corresponding partial beliefs. Another consisted in criticising certain proposed accounts of the characteristic content of such a full belief. And the third involved arguing that various grounds which might be thought to show that there is some such content, do not in fact support this conclusion.

If these arguments have any force, then a natural suggestion, in view of this characterisation of an assertion, is that SP utterances characteristically transfer partial beliefs, rather than effectively full ones. If the conditions are right, X's utterance of 'It is probable that *q*' is the means whereby his high degree of partial belief that *q* gives rise to a high degree of partial belief that *q* on the part of Y. It is the partial belief that *q* that the utterance transfers, rather than - as the assertive view would have it - a full belief that it is probable that *q*.

By recognising only the partial belief in an SP context, this proposal avoids the difficulties which stem from the admission of an associated full belief. But doing without the full belief may seem a dangerous move, given that we have apparently taken mental states to be well-defined without reference to the nature of the utterances to which they characteristically give rise (so as to classify these utterances in terms of their associated mental states). For suppose it turns out that there *are* well-defined full beliefs such as the belief that it is probable that *q*. The proposal will then be shown to be false.

The short answer to this is that we are not really taking mental states to be so independent of their linguistic expressions. Rather we

are taking advantage of their close relation to their linguistic manifestations to classify some of the latter in terms of the mental states with which they are associated. The reason for doing so, in this case, is that the notion of a partial belief is much more widely accepted and understood than that of the category of utterance we are proposing; so there is a clear advantage in defining the utterance in terms of the belief. This is not to say it is possible to decide what mental state is present in a given instance without at the same time deciding the category of associated utterances. On the contrary, it may be impossible to decide the mental state if a possible category of associated utterance is overlooked - as the present case may illustrate. Lack of attention to forms of utterance other than assertion seems to have largely prevented the development of accounts of single-case probability without full beliefs.

Roughly speaking, a full belief that *p* is displayed by a person who acts as if *p*, and is prepared to assert that *p* in appropriate circumstances. Now an objectivist who thinks there is a high chance that *q*, will take his own behaviour with respect to the proposition that there is a high chance that *q* to fit this full belief model. This is the view of someone who has already bought into the scheme, however. It no more shows the correctness of the assertive view of SP utterance than the fact that there are people who believe in the reality of absolute space (and who therefore take ordinary spatial terms to refer to absolute space), for example, shows that it exists. If the argument seems more plausible for beliefs about chance than for space, it is because in the former case a view of a part of mind (and a part of language) is at stake, and our views about that seem more privileged than our views about space. But the impression is a mistake, as I think the availability of an alternative description of our mental states and behaviour in SP contexts will show. Such an alternative scheme will not

accommodate all the second-level beliefs of proponents of rival schemes about their own mental states, but it no more needs to do that than a relationist account of space needs to preserve the truth of existing beliefs about absolute space. In either case the most that is required is an explanation of how people could have come to hold what are being said to be mistaken beliefs.

We need some notation for the view we are suggesting. What are we to call this category of non-assertive utterances? It is sometimes said that SP utterances are qualified assertions. There seem to be two ways in which this can be taken. In one sense a qualified assertion is itself an assertion, though a different one from that to which the qualification is applied. Qualification thus amounts to transforming one assertion into another, for example by the addition of a sentential operator. In the other sense, which is closer to what we want, 'qualified' has the sense of 'weak', or 'partial' (or perhaps 'hedged'), so that a qualified assertion is not itself an assertion - it transfers no full belief, even in ideal circumstances.

The expression 'guarded assertion' is also sometimes used. But it seems more suited to the situation in which a person has an effectively full belief (that q, say), but in circumstances which make it advisable to be very cautious about expressing this belief, or to be careful not to be misunderstood in doing so. At least in the former case it may be useful to pretend that one's degree of belief that q is less than it actually is, by making the utterance appropriate to the sincere expression of some partial belief that q. But even if a hearer adopts a partial belief that q as a result of this utterance, the utterance has not been the means of a transfer of a partial belief that q, because the speaker had no such belief. So the utterance here is not of the type we are interested in (except as a degenerate case - a case in which not all the conditions of tranfer are satisfied).

I propose to use the term *partial assertion* for an utterance of the suggested category. It has the disadvantage that such an utterance is in some way a part or component of an assertion, occurring in isolation. But it matches the term 'partial belief', and will do as well as any, given that there seems to be no generally accepted existing term.

It would be a mistake to conclude from the lack of an existing term that there are no such utterances - as much a mistake as, say, it would have been to conclude from the lack of a term (in English) for a category of egg-laying mammals that there are no such animals. As we have stressed above, we have no more direct access to a final linguistic taxonomy than to such cases as the zoological one. The fact that in the linguistic case the instances to which such a taxonomy must apply are close at hand is a doubtful advantage; it is correspondingly difficult to treat our own linguistic behaviour as an object of enquiry. There is a difference between the linguistic and zoological cases in that we might take a linguistic taxonomy to be normative, and hence add new forms of utterance to our language; whereas no such thing is possible in the the zoological case (genetic engineering and selective breeding programmes aside, perhaps). But this possibility is not relevant to the present case.

What are the conditions for a partial assertion to be the means of transfer of a partial belief? Not surprisingly (if an assertion is to be a special or limiting case of a partial assertion) they are similar to those for an assertion to be the means of transfer of a full belief. Here the major condition on the speaker's side is that he be sincere - that he actually believe what he says. Strictly speaking this condition needs to be referred to a particular language, since a speaker may believe what his utterance says in one language but not what it says in another. On the hearer's side, the first condition is that he understand the language the speaker is using, and, in terms of

this language, correctly identify what has been said. The second major condition is that he regard the speaker as a reliable source of information on the matter in question; to do so he must both believe him to be sincere, and trust his (i.e. the speaker's) judgement on whatever it is. We can think of X's trusting Y's judgement on the matter of whether q, as X being disposed to believe that q, if he believes that Y believes that q. A third condition on the hearer is that he not already have the relevant belief when he hears the utterance in question.

Thus, roughly, an assertion that q is an utterance which is (or would have been) the means by which the speaker's effectively full belief that q gives (would have given) rise to a hearer's full belief that q, if and only if these conditions are (had been) satisfied (with respect to the speaker, hearer and utterance in question, and to the full belief that q).

This is not a precise characterisation. For one thing it admits as assertions that q utterances which are much more besides (conjunctions of this assertion with others, for example). Perhaps such cases could be excluded by the condition that there be no utterance which in the circumstances would also have resulted in the hearer adopting the belief that q, but would have caused fewer other changes in his mental state. But no doubt the characterisation is imprecise in other respects. However, it will do to allow us to introduce the notion of a partial assertion, in a way which exhibits its relation to the notion of assertion itself - which is what we want it for.

We could have said that partial assertions express partial beliefs, as assertions express full ones; but this would have depended on the notion of the *expression* of a belief. Even if it is clear what this amounts to in the case of a full belief, I don't think we can take for granted its extension to partial beliefs. So we have looked at what

the expression of a full belief achieves, in ideal circumstances, and are using that as the basis of our extension to partial beliefs. And we are interested in categorising SP utterances, rather than in finding a rule for deciding whether any given utterance is a partial assertion. Hence so long as we are clear as to the differences, if any, between the ideal circumstances for an assertion and a partial assertion, we needn't be concerned about some imprecision in our account of what these circumstances have in common.

It seems to me that the only significant difference concerns the requirement that the hearer be disposed to accept the speaker's judgement on the matter in question. In the partial belief case X will only rely on Y's judgement as to *q* if as well as trusting Y's assessment of Y's evidence as to whether *q*, he doesn't think he has any better evidence himself. In the limiting case of an effectively full belief this condition reduces to the one we have described above, because in trusting Y's judgement that it is effectively certain that *q*, X effectively rules out the possibility that he might have different evidence than Y, supporting a different conclusion.

With this qualification, the ideal circumstances for a partial assertion parallel those for an assertion. A partial assertion of degree *d* that *q* can thus be said to be an utterance such that if and only if these circumstances exist (with respect to the speaker, hearer and utterance in question, and the partial belief of degree *d* that *q*), it is the means whereby its speaker's partial belief of degree *d* that *q* gives rise to the same belief to the same degree on the part of the hearer.

This characterisation of partial assertion rests on a very 'external' view of language. It makes no use of the fact that utterances are (usually, at least) voluntary, intentional acts. And this

fact about language might seem to support the following objection to the notion of a partial assertion: because linguistic behaviour is intentional (with a few irrelevant exceptions, such as involuntary cries of pain), we can only say what we can intend to say. But no one can have intended to make partial assertions, if - with the possible exception of a few philosophers - no one has been aware of the notion. And hence ordinary SP utterances cannot be partial assertions.

However, there is a clear difference between *(i)* being able to intend to perform an action of type *A*, and *(ii)* having a language sufficiently powerful to *say* that one intends to perform an action of type *A*. A child who has not learnt the word 'hop' is not therefore unable to hop, or to do so intentionally. And someone who doesn't know what an assertion is, is not necessarily unable to make assertions. Linguistic acts are not in this respect different from acts of other kinds.

The feeling that linguistic acts are different in this respect might be encouraged by the tendency of the developers of the notion of an *illocutionary act* - in the first place J. L. Austin,[1] and later, for example, John R. Searle[2] - to concentrate on the large class of such acts for which we do have names in English. In fact Austin and Searle both recognise that there may be such acts for which we do not have names; as Searle says, 'The act may have been so special and precise in its intent that none of the existing words can quite characterise it exactly'.[3] Even so, Searle (at least) doesn't seem to envisage illocutionary acts which differ as much from any for which we do have a name as partial assertions do from assertions. Accommodating partial assertions is more than a matter of making slight adjustments to existing categories.

Suppose the term 'partial assertion' is introduced, with the meaning we have given it. Will someone who says 'I partially assert to high degree that q' and 'It is highly probable that q' be performing

the same speech act in each case (except in the trivial sense that different words are used)? It seems not, because it seems that the former utterance is much more subjective, more 'about the speaker', than the latter. But this is a feature of some of the utterances used to perform many types of illocutionary act. As Austin and Searle both argue,[4] constructions such as 'I promise that ...', 'I state that ...', and 'I assert that ...' are not (or are only exceptionally) used to say something about the speaker.

The last example is particularly relevant here, because there are constructions for making assertions whose apparent objectivity parallels that of SP utterances: 'It is the case that *q*', for example; and perhaps (though there would be more dispute about this), 'It is true that *q*'. For a partial assertion there is apparently nothing which parallels the simplest way to assert that *q*: to say simply '*q*'. But, as Austin points out,[5] to assert that *q* by saying '*q*' one needs to say it with the correct emphasis and tone. Otherwise one may be, for example, asking whether *q* (i.e. saying '*q*?'). There seems to be no reason why degrees of partial assertion shouldn't be performed in a more sophisticated version of the same manner. In fact to some extent this is already possible in English, as when our doctor says 'You've got at least ten years', and we judge from the tone of voice that he or she is not entirely confident of this prognosis (and hence, if we trust his or her judgement, adopt a corresponding partial belief ourselves). In theory the devices used to convey degrees of confidence in such cases could presumably be extended to handle much more precisely defined partial beliefs. But since the numerical theory underlying the ordering of such precisely defined degrees of belief is quite a recent development, it is not surprising that we in fact employ much less fundamental linguistic devices for this purpose: the superficially objective language of chance and probability.

Thus the notion of partial assertion seems to fit quite naturally into the approach to language in terms of speech acts and illocutionary force. In this framework the present dispute concerns the illocutionary force of an SP utterance. What illocutionary act do we perform, in saying 'It is probable that *q*'? In so far as it can be represented in terms of this approach at all, the assertive or truthconditional view will say that we make a statement, whose propositional content depends to some extent on our use of the construction 'It is probable that ...'. The alternative view we have outlined here claims that our use of such a construction modifies the illocutionary force (of '*q*', that is) but not its propositional content.

Why are these views incompatible? Why shouldn't we say that the same utterance may perform more than one illocutionary act, each associated with a different propositional content? I think the simplest answer, from our point of view, is that we make no such claim. We have argued in earlier chapters that the assertive view is unacceptable, and have now proposed an alternative. If a proponent of the assertive view admits this alternative, but suggests that SP utterances have a dual illocutionary role, he simply makes things tougher for himself: he now has not only to meet our original objections to assertive accounts, but to do so in the light of his admission that there is an alternative account available. We, on the other hand, have no reason here to dispute the general claim that an utterance may perform more than one illocutionary act; but say that we have already given our reasons for not regarding SP utterances in particular in this way (i.e. our reasons for denying the existence of one of the proposed roles).

The claim that SP utterances are not assertions is a counter-intuitive one; at least it is counter-intuitive to me, and I suppose it is to others, since otherwise it would surely have been more popular.

It is the fact that on analysis I find the alternatives worse which leads me to endorse this initially implausible claim. But it would be reassuring to be able to dispel the counter-intuition. To do so it would be necessary to identify its components, so as to say on what mistaken views it rests. This seems a rather difficult task, but I think we can distinguish one or two strands.

Perhaps the most important thing is that ordinary language permits all sorts of constructions which seem to treat SP utterances explicitly as assertions: 'The doctor stated several times that my chances of recovery are excellent', 'He asserts it is probable he will escape before Christmas', and so on; plus many related constructions, such as 'What did he say?', which may refer to SP utterances, and which reinforce the impression that there is a certain 'something' which *is* what is said, or asserted, by someone who makes such an utterance. Important among these constructions are those which apply the terms 'true' and 'false' to SP utterances - 'That's false', said in response to such an utterance, for example.

However, notice that although these are features of ordinary language, it is not the ordinary language user whose intuitions comprise the assertive view of SP utterances. That view is an interpretation of these features by specialists, who, unlike the ordinary user, possess relatively sophisticated notions of assertion, belief, and so on. It is a serious mistake to see the interpretation as as much a 'given' of ordinary usage as these features which it claims to interpret. Rather the interpretation results from the application to the case of SP utterances of a model of linguistic behaviour which (let us say) has been adopted in the light of its success elsewhere. It is not surprising that a specialist's intuitions should be guided by this model, nor wrong that they should be. But such intuitions are no better than the model itself is, in any particular case.

A person who denies the applicability of this model to the case of SP utterances should be prepared to offer an explanation of the features of ordinary language we have noted. But at first sight, at any rate, this doesn't seem an impossible task. The ordinary use of words such as 'assert', 'state', 'true' and 'false' precedes their more specialised use by philosophers, and so there is no obligation to explain every instance of the former kind of use in terms of the latter. To suggest otherwise would be like arguing that if zoologists, overlooking bats, had adopted the term 'winged vertebrates' for the category of birds, it would be necessary to explain the acceptability in ordinary usage of 'Bats have wings' in terms of the similarity of a bat to a bird (which is wrong because the use of this sentence doesn't depend on the speaker's having encountered birds, or his knowing what a 'bird' is). Moreover, there seems no greater obligation to make a significant category of all those things which in ordinary language can be said to be 'asserted' (or 'true'), than there is to make one of the class of animals which have wings. We can no more be certain that a useful taxonomy of linguistic behaviour will be revealed at such a superficial level, than we can be in the zoological case.

The partial assertion account is able to claim that these features of ordinary language are part of the apparatus language provides for describing and responding to the partial assertions of others (and of ourselves at different times). The task is thus to exhibit the conventions governing the use of 'assert', 'true', and so on, in connection with SP sentences. For the reasons we have explained, it is no objection to this programme to point out that this ordinary usage does not itself employ the term 'partial assertion' (or any equivalent term).

Similarly, the fact that we often say such things as 'He believes it is probable he will escape before Christmas', does not show that we

have beliefs about probabilities. A proponent of the partial assertion interpretation of SP utterances is free to say that such constructions are the means we use to report the partial beliefs of others - a means whose indirectness (compared to 'He has a strong partial belief that he will escape before Christmas') reflects the fact that neither 'partial belief' itself nor any equivalent term is very much in ordinary use. Lacking such terms, language makes do by treating SP utterances as assertions, and applying the forms appropriate to any assertion: in this case, constructions involving the notion of belief. It thus achieves much of what it would if it did have such terms, and the underlying process is difficult to detect. But it is revealed by certain conceptual problems - the link problem, in particular - and by the fact that at least when this process applies the terms 'true' and 'false' to SP utterances, the resulting usage is non-standard, in view of the relational nature of SP utterance.

Thus if the partial assertion account is to be rejected, it must be on more substantial grounds. In the next two chapters I shall try to block some possible lines of attack. Partial assertions are still very much on trial, of course; but I think the evidence against the alternative ought at least to ensure that they get a fair one.

Notes

1. Austin (1975).
2. Searle (1969).
3. Searle (1968), p. 417; for similar remarks see Searle (1969), p. 70, and Austin (1975), pp. 68-72.
4. Austin (1975), pp. 6, 78-82; Searle (1969), p. 33.
5. Austin (1975), p. 74.

Bibliographical Notes

The partial assertion approach to SP sentences is similar to that of Toulmin (1950), who says that 'there is no special "thing" which all probability-statements must be about, simply in virtue of the fact that they *are* "probability-statements"' (p. 50); and that 'to say "Probably *p*" is to assert, guardedly and/or with reservations, that *p*' (p. 61). However, I think Toulmin's use of the term 'guarded assertion' is unfortunate: firstly for the reason we have already given (p. 6:5), and secondly because it seems quite inappropriate for SP utterances other than 'It is probable that *q*' (and certain very similar ones). Thus is 'It is unlikely that *q*' a very guarded assertion that *q*? Or a guarded assertion that not-*q*? And what is 'There is a 50% chance that *q*'? I think it is much preferable to characterise SP utterances in terms of their role in tranferring partial beliefs (i.e., as partial assertions).

Toulmin's view seems to have received little support. Mackie (1973, Ch. 5) comments favourably on its application to certain cases. And Mellor, whose rejection of the opinion that 'if chance is objective, it must make true beliefs with some characteristic content' we have already noted (pp. 5:17-18), seems to have a view which has something in common with Toulmin's.

In his (1980) Blackburn supports the view (which he attributes to Ramsey) that 'judgements of probability ... are *projections* of our degrees of confidence in singular beliefs' (p. 1, my italics), rather than judgements about some matter of fact in the world. I think the partial assertion approach is close to Blackburn's, particularly in beginning with partial beliefs, yet recognising that SP utterances are not *about* such beliefs. But given the notion of partial assertion, I doubt whether we need Blackburn's 'projection' metaphor.

7. *AN OBJECTION FROM GEACH AND SEARLE.*

The view that SP utterances are partial assertions has analogues in other areas of discourse. Perhaps the best-known is the account of ethical statements of forms such as 'It is a good thing that *q*' as non-assertive expressions of approval, rather than as assertions whose content is to be explained in terms of some notion of objective goodness (or, as it might be put, the view that a construction such as 'It is a good thing that ...' alters the force of a sentence to which it is attached, without modifying its propositional content, or sense). There is a general objection to moves of this kind, which has been put forward by Geach, and later, in a somewhat different form, by Searle.[1]

The objection begins with the observation that accounts of this general kind characteristically propose an interpretation of just those sentences or utterances in which constructions of the relevant type - 'It is probable that ...', 'It is good that ...', 'It is true that ...', or whatever - are not part of any clause other than a complete sentence. The objection then notes certain other kinds of occurrences of such constructions, and argues that the proposed accounts are obliged, yet unable, to deal with these new cases.

Geach puts it as follows:

> There is a radical flaw in this whole pattern of philosophizing. What is being attempted in each case is to account for the use of a term "P" concerning a thing as being a performance of some other nature than describing the thing. But what is regularly ignored is the distinction between calling a thing "P" and predicating "P" of a thing. A term "P" may be predicated of a thing in an *if* or *then* clause, or in a clause of a disjunctive proposition, without the thing's being thereby called "P". To say, "If the policeman's statement is true, the motorist touched 60 mph" is not to *call* the policeman's statement true; to say, "If gambling is bad, inviting people to gamble is bad" is not to *call* either gambling or invitations to gamble "bad". Now the theories of non-descriptive performances

regularly take into account only the use of a term "P" to *call* something "P"; the corroboration theory of truth, for example, considers only the use of "true" to *call* a statement true, and the condemnation theory of "bad" considers only the way it is used to *call* something bad; predications of "true" and "bad" in *if* or *then* clauses, or in clauses of a disjunction, are just ignored. One could not write off such uses of the terms, as calling for a different explanation from their use to *call* things true or bad; for that would mean that arguments of the pattern "if *x* is true (if *w* is bad), then *p*; but *x* *is* true (*w* *is* bad); *ergo* *p*" contained a fallacy of equivocation, whereas they are in fact clearly valid.[2]

Geach does not mention non-descriptive accounts of 'probable' (and related words), but it seems clear that the partial assertion view is of the pattern whose 'radical flaw' he claims to be exposing. In any case, when Searle raises his similar objection he includes the case of 'probable', as well as some of the other cases mentioned by Geach:

In the classical period of linguistic analysis, philosophers often said things like the following:

The word "good" is used to commend (Hare).
The word "true" is used to endorse or concede statements (Strawson).
The word "know" is used to give guarantees (Austin).
The word "probably" is used to qualify commitments (Toulmin).

Each of these is of the pattern: "The word *W* is used to perform the speech act *A*". ...

Let us call this pattern of analysis the *speech act analysis*. Now, there is a condition of adequacy which any analysis of the meaning of a word must meet - and which the speech act analysis fails to meet. Any analysis of the meaning of a word (or morpheme) must be consistent with the fact that the same word (or morpheme) can mean the same thing in all the grammatically different kinds of sentences in which it can occur. Syntactical transformations of sentences do not necessarily enforce changes of meaning on the component words or morphemes of those sentences. The word "true" means or can mean the same thing in interrogatives, indicatives, conditionals, negations, disjunctions, optatives, etc. If it didn't, conversation would be

impossible, for "It is true" would not be an answer to the question "Is it true?" if "true" changed its meaning from interrogative to indicative sentences.

This is an obvious condition of adequacy, but the speech act analysis fails to meet it. There are two ways of construing the analysis and on either way it fails to meet this condition of adequacy. The crude way to construe it is to suppose that when the speech act analysts said, "*W* is used to perform act *A*" they meant every literal utterance of the word *W* is a performance of act *A*. If this is what they meant, it is too easily refuted, for even if an utterance of the sentence, "This is good", is a performance of the act of commendation, the utterance of the sentence, "Make this good" is not .... (S)o we must turn to a second, more sophisticated interpretation. Often the speech act analysts qualified their statements of the form "*W* is used to perform act *A*" by saying that the *primary* use of *W* is to perform act *A*. They were thus not committed to the view that every literal utterance of *W* is a performance of *A*, but rather that utterances which are not performances of the act have to be explained in terms of utterances which are.

More precisely ... the speech act analysts ... need to show ... only ... that literal utterances which are not performances of the act *A* stand in a relation to performances of *A* in a way which is purely a function of the way the sentences uttered stand in relation to the standard indicative sentences, in the utterance of which the act is performed. If they are in the past tense, then the act is reported in the past; if they are hypothetical then the act is hypothesized, etc. They need to show this, in order to show how the word makes the same contribution to each different sentence, while maintaining that the performative use is the primary use.

... But ... the speech act analysis of the ... words: "good", "true", "probable", etc. does not satisfy this condition. Consider the following examples: "If this is good, then we ought to buy it", is not equivalent to "If I commend this, then we ought to buy it". "This used to be good" is not equivalent to "I used to commend this", ... etc. Similar counterexamples will refute the speech act analyses of "true", "know", "probable", etc.[3]

Geach and Searle thus differ in their defence of the central claim on which this line of objection depends: the claim that any such non-descriptive performative account (to use Geach's term) is obliged

and yet unable to offer an account of certain types of occurrences of the words to which it is held to apply. Geach appears to argue for a very strong version of this central claim, namely that a non-descriptive account is obliged to provide the *same* interpretation of the word in question when it occurs in an *if* or *then* clause, as it does of the word's occurrence in a complete indicative sentence (and no lesser clause). This is impossible because in a case of the former kind such a word is not used to '*call* something "P"', as Geach puts it. But unless this obligation is fulfilled, Geach argues, such an account will be reduced to saying that the use of the same word in these different contexts reflects a mere ambiguity. And this would have the consequence that instances of *modus ponens* involving this word would fail, 'whereas they are in fact clearly valid'.

On the other hand Searle recognises, in effect, that a weaker version of the central claim will be sufficient to support the objection. It is enough that there be *some* sense in which a performative account is obliged to interpret the relevant class of occurrences of the word in question, and yet in which it is unable to do so. It is not essential that the interpretation thus required be of the same kind as that provided of the initial class occurrences of the relevant word - i.e., of the occurrences Geach describes as 'calling something "P", in which the word is not part of any clause other than a complete present-tensed indicative sentence.

Let us begin with Geach's argument for the strong version of the central claim. Can a performative account reconcile its inability to provide the same interpretation of a clause occurring as the antecedent of a conditional as it does of that clause standing alone, with the validity of *modus ponens*?

The first thing to note is that a performative account is in any case likely to reject the standard view of what the validity of many

argument forms, including many instances of *modus ponens*, consists in. Thus (concentrating on the probability case), if 'It is probable that *q*' is being said to be non-truthconditional, the validity of

7.1 If it is probable that *q* then *r*
It is probable that *q*
Therefore *r*

cannot, at least in the standard sense, be a matter of the truth of the premisses guaranteeing the truth of the conclusion. So a view such as our partial assertion account seems obliged to offer some non-standard account of validity. If it is unable to do so, that will be a strong objection to the view itself. But if it can do so, it will be justified in assessing Geach's objection in terms of this non-standard approach; there will be no obligation to meet Geach on his own ground.

This observation does not necessarily make things any easier for an account such as ours, but it does suggest that Geach's objection would be better phrased in another way: assuming there is no disagreement as to which inferences *are* correct, the problem for such an account is not that the change in the role of the clause 'It is probable that *q*' from the first to the second premiss of *7.1* makes the inference *invalid*, in the standard sense; but rather that it is not clear in what other sense such an account can distinguish correct from incorrect inferences, and classify *7.1*, in particular, as *correct*. In this form the argument is less an objection than a challenge - a challenge to produce a suitable non-standard notion of validity. Moreover, this challenge arises directly from the claim that the second premiss of *7.1* is non-truthconditional; the changing role of the clause 'It is probable that *q*' is almost a red herring. After all, the problem would be no less difficult (and the resulting inference no less correct) if by convention we always replaced this clause by some other form of

words, whenever it would otherwise occur as the antecedent of a conditional - even though there would then be no clause repeated in the premisses of *7.1*.[4]

It might seem that we can produce an account of validity applicable to inferences such as *7.1* by making use of the fact that SP utterances function as truthconditional in contexts in which speakers share a common body of evidence. That is, we might claim that such an inference is valid if and only if it would be valid in the standard sense, if the SP clauses involved were truthconditional - or if each were replaced in all its occurrences by a truthconditional clause. Then the fact that SP sentences behave in some contexts as if they were truthconditional will perhaps guarantee that this criterion classifies as valid the right class of inferences. True, it will not count as valid those inferences which depend essentially on the properties of probability itself, such as that from 'It is probable that *q*' to 'It is improbable that not-*q*'. But because such inferences do not have a form which is recognised as valid more generally, they do not give rise to Geach's argument; any account of probability needs to explain the validity of such inferences, and given that they lack a valid general form, truthconditional accounts have no obvious advantage.

Nevertheless, the validity of inferences which reflect the special features of the probability calculus does need to be explained, and the fact that this criterion is unable to do so is one reason for looking for a more substantial account. Another is that the criterion rests on what we have claimed is a derivative feature of SP sentences: their ability to function as truthconditional in contexts of shared evidence. We should expect an explanation of validity in terms in keeping with our primary view of the nature of SP utterances, rather than one which depends on a secondary characteristic (the interest of which lies in its connection with the truthconditional view, which, for

SP utterances themselves, we have rejected).

But the most important reason for looking for a more substantial account than this criterion is that it is no more than a criterion: the fact that it works needs to be explained. We cannot say that the correctness of *7.1 consists in* the fact that if *s* is truthconditional, the inference

7.2 If *s* then *r*
s
Therefore *r*

is valid in the standard sense; for the question remains as to why we should use the inferences picked out by this criterion (even assuming that the corresponding question has been answered for inferences valid in the standard sense).

As a first step towards a more substantial account of the nature of the validity, or *correctness*, of *7.1*, we shall need an interpretation of its first premiss; an interpretation compatible with our characterisation of SP utterances themselves, as partial assertions. We characterised the notion of partial assertion in terms of the tranfer from a speaker to a hearer of a partial belief. Is a similar move now possible to explain the role of an utterance of the form 'If it is probable that *q*, then *r*'? Is there a characteristic mental state, or attitude, associated with the making of such an utterance?

The key notion we need, I think, is that of a *disposition to infer* (or, more conveniently, an *inferential disposition*). We need to say that among the mental states a person may have, are ones which amount to a readiness to adopt a certain consequent mental attitude, if and when one comes to adopt a certain antecedent attitude. We might call such a disposition a *habit* of inference; but note that the number of times such a habit can operate is limited by the number of separate

occasions on which a person can adopt the particular antecedent mental attitude in question. Many such 'habits' are employed no more than once.

Thus for the conditional 'If *s* then *t*', the antecedent and consequent mental attitudes of the associated inferential disposition will be whatever attitudes are characteristically associated with the utterances '*s*' and '*t*' alone, respectively. If *s* is the sentence 'It is probable that *q*' and *t* (and *q*) are truthconditional sentences, then the antecedent attitude is a strong partial belief that *q*, and the consequent one is an effectively full belief that *t*.

It is important not to claim that a conditional *reports* the speaker's possession of such a mental disposition. If that were so the conditional would be truthconditional - would be *about* the speaker's state of mind - and in particular would be false if and only if the speaker did not have such an inferential disposition. But in practice what a hearer indicates by saying 'That's false' in response to such an utterance, is not that he doesn't believe that the original speaker has the inferential disposition in question, but that (even having heard the utterance) he doesn't have it himself. Similarly 'That's true', said by the hearer, indicates that he does (now, at least) have the relevant inferential disposition himself. So the cost of claiming that a conditional reports its speaker's possession of the corresponding inferential disposition, is to be forced to claim that the terms 'true' and 'false' are applied to such utterances in a non-standard way - and this despite the fact such utterances are thus being said to be truthconditional. (This point is essentially the same as one we raised - pp. 3:13-14 - against *2.5*, the subjectivist truthconditional interpretation of SP utterance).

For the same reason it is important not to interpret the conditional 'If it is probable that *q*, then *t*', along the lines of 'If I were

to partially assert (to high degree) that *q*, then I would assert that *t*'. This reading seems to be what a suggestion of Dummett's in reply to Geach's objection amounts to (for the probability case, which Dummett, like Geach, does not himself refer to).[5] Note that to reject this reading is not to deny that someone who has the disposition to infer from a high degree of partial belief that *q* to a full belief that *t*, would, if he were to partially assert to high degree (and sincerely) that *q*, be at least willing to assert that *t*; it is simply to say that the utterance 'If it is probable that *q* then *t*' does not *state* that this is the case.

On Dummett's reading, a conditional whose antecedent is an SP clause comes out as a sentence whose utterance amounts to an assertion about the speaker (in so far as a conditional with truthconditional antecedent and consequent is truthconditional itself, at any rate). This fact leaves the reading open to the following objection from L. J. Cohen: if Dummett's suggestion is to be extended to the case of probability, then there should be a use for a construction

> meaning 'If I were to assert (agree) guardedly that *A*, then I should assert (agree) that *B*'. But this would not be a use paraphrasable by 'If it is probable that *A*, then *B*'. For though it happens to be true that if I were to assert (agree) guardedly that it will be cloudy this afternoon I should also assert (agree) that I am excessively cautious in my weather predictions, it is not true that if clouds are probable this afternoon I am excessively cautious.[6]

On our view, however, the conditional 'If I were to assert guardedly that *A*, then I should assert that *B*' is associated with a disposition to infer from a belief that one is asserting guardedly that *A*, to a belief that one is asserting (or will assert) that *B*. There is nothing to prevent someone from holding this disposition, but not a disposition to infer from a high degree of partial belief that *A* to a belief that *B*; and it is this latter disposition which we associate with the cond-

itional 'If it is probable that *A*, then *B*'.

Cohen makes a further point, that on Dummett's reading there would be no obvious use for 'If it is probable that *A*, then I should prefer not to assert (or agree) guardedly that *A*'; whereas there is such a use, along the same lines as 'Even if it's true that *A*, I would prefer not to say so'. Our view handles this in much the same way, I think. But there seem to be more difficult cases of a similar kind. Thus suppose I think I am a consistently bad judge of horses, and hence say 'If it is probable that Proper Name will win the 3.15, then I am bound to be confident that he will not do so'. On our reading this ought to be associated with a disposition to infer from a high degree of partial belief that Proper Name will win the 3.15, to an effectively full belief that one has a high degree of partial belief that he will not do so. Now this disposition is not unintelligible, but it seems much less likely that I should actually have it, than that I should feel justified in asserting the conditional with which it is supposed to be associated. Surely I can think I am a poor judge of horses, and express this belief in this way, without being disposed to adopt incorrect beliefs about my own mental state.

The unusual nature of this conditional is revealed in other ways. Significantly, a *modus ponens* argument in which it appears seems to be valid only if it is unusable. Consider

7.3 If it is probable that *q*, then I am confident that not-*q*
It is probable that *q*
Therefore I am confident that not-*q*

Suppose the argument is valid (in the standard sense). It is not usable (for me) unless it is at least possible that there are circumstances in which I take both premisses to be true. But in any case in which I take the second premiss to be true (i.e. in which I think it is

probable that *q*), the conclusion is false; and hence, because the argument has been assumed valid, at least one of the premisses is false. So if the argument is valid there are, and can be, no circumstances in which it justifies my adoption of a true belief.

It would therefore seem to be no great loss to concede that in certain cases of this kind, *modus ponens* is not valid. However, the immediate problem is to show that our reading of conditionals (and particularly of conditionals containing SP clauses) is not undermined by the existence of such deviant examples. The way to do so, I think, is to refer to a factor we have so far ignored, namely the characteristic effect of a conditional utterance on a hearer's state of mind.

In Chapter 6 we characterised assertions and partial assertions as utterances which, if the circumstances are appropriate, are the means by which a certain belief on the part of a speaker gives rise to the same belief - i.e. a belief of the same content and the same degree - on the part of a hearer. We outlined the major conditions necessary for such a transfer to take place (and saw that these are slightly more involved for a partial assertion than for an assertion itself). The question now is whether the notion of an inferential disposition provides the basis of a similar characterisation of the class of conditional utterances.

The simplest possibility is that if the circumstances are appropriate, a conditional utterance is the means of transfer of just such an inferential disposition. It might seem that this won't do, on the grounds that even if the speaker of such an utterance characteristically has such a disposition, what a hearer will adopt is not the same disposition, but a belief about the speaker: i.e. that he has the disposition in question. But this is a mistake. It is analogous to the claim that what the hearer of an assertion '*q*' characteristically adopts is the belief that the speaker believes that *q*, rather than the

belief that *q*. In either case a hearer may adopt a belief about the speaker's state of mind - and may adopt no other relevent belief, if he is not inclined to follow the speaker's lead on the matter in question. But in ideal circumstances a hearer of the assertion '*q*' adopts a belief that *q*, and a hearer of a conditional utterance adopts the particular inferential disposition whose possession by the speaker is indicated by the fact of the utterance - or so it seems to me.

The ideal circumstances, under which a conditional utterance will be the means of tranfer of an inferential disposition, have a pattern which is familiar from the cases of assertion and partial assertion. On the speaker's side the main condition is sincerity, which here is a matter not only of having the relevant inferential disposition, but also, in general at least, of having neither the antecedent mental attitude nor an attitude incompatible with the consequent one (there may be exceptions to this requirement, such as when a conditional is used as a step in an argument). On the hearer's side, apart from the general condition that he understand what the speaker says, the major conditions are the pair that amount to trusting the speaker: i.e. the conditions that the hearer should believe the speaker sincere, and believe him reliable on the matter in question. In the case in which either the antecedent or the consequent is an SP clause, believing the speaker reliable is for the hearer not only a matter of trusting the speaker's judgement; it is also necessary that the hearer should not take himself to have relevantly different evidence from the speaker's on the SP matter in question. (We saw that this condition is the respect in which the circumstances for a partial assertion to result in the transfer of a partial belief are different from those in which a full assertion results in the transfer of a full belief).

We are now in a position to make sense of our deviant conditional 'If it is probable that Proper Name will win the 3.15, then I am bound

to be confident that he will not do so'. If this were a normal conditional, and the circumstances we have just outlined were present, its utterance would lead a hearer to adopt a disposition to infer from a high degree of partial belief that Proper Name will win the 3.15 to an effectively full belief that the speaker is confident that Proper Name will not win the 3.15. And this does seem to capture what the speaker of such a conditional would want to convey, even if he cannot be taken to have the same inferential disposition himself. The case seems rather like that of the person who says, despairingly, 'I am incapable of believing anything'. We cannot take this to be both a sincere expression of belief, and true; yet it is not difficult to think of a role for such an utterance. Such cases are made possible by the existence of general conventions, whose very stability enables sense to be made of certain kinds of deviations from the general pattern. Note that in the conditional case such a deviation depends on the speaker and hearer referring their SP talk to the same base of evidence - otherwise when the hearer says, 'I see, so if it is probable that Proper Name will win the 3.15, then you have a low degree of partial belief that that will be the case', he is not expressing what the speaker has wanted to convey.

We needed an account of conditionals in order to be able to say in what the validity, or correctness, of the inference *7.1* (p. 7:5) consists, for a view such as ours. And although we have really done no more than indicate an approach to conditionals, in line with our characterisation of assertion (full and partial), I think we have said enough to suggest an account of validity. Consider

(i)	If *p* then *q*	*(ii)*	If *p* then *q*
	p		*q*
	Therefore *q*		Therefore *p*

What distinguishes *(i)* from *(ii)* is that only in the case of *(i)* do the mental states characteristically associated with the utterance of (sentences of the form of) the two premisses together guarantee the presence of the mental state characteristically associated with the utterance of the conclusion. This holds not only for truthconditional *p* and *q*, but for any type of utterance whatsoever, so long as its associated mental attitude is such that it can occur as the antecedent or consequent of an inferential disposition. (This seems to be a very wide class indeed. I don't want to investigate its boundaries here, but we should hope that they at least include all the types of sentences which ordinary usage treats as the antecedents or consequents of conditionals.) In particular, it holds when *p* or *q* is an SP clause.

Thus we have an account which enables us to say in what respect *(i)* is a significantly different form of inference from *(ii)*; and to say why people do in fact, on the whole at least, make inferences of form *(i)* but not of form *(ii)*. This account is applicable to the special case in which *p* or *q* is an SP clause. And in producing it we have indicated the connection between an SP clause standing alone and the use of the same clause as the antecedent or consequent of a conditional (this will be important in meeting Searle's objection).

However, we have glossed over a very important point: in what sense do the mental attitudes associated with the sincere utterance of the premisses of *(i) guarantee* the presence of the attitude associated with the utterance of the conclusion? It is tempting to say that it is physically impossible for a person to have the former attitudes but not the latter one. But if that were so it would apparently be impossible for a person not to believe all the logical consequences of his beliefs. And it seems we often fail to make even such simple inferences as *modus ponens*, particularly when we have some motive for not believing the conclusion. Thus I know that if I haven't heard by the fifteenth, I

haven't been short-listed, and that I haven't heard by the fifteenth; but I can't bring myself to believe (fully, at least) that I haven't been short-listed. Perhaps a case such as this simply illustrates that we often profess things we do not fully believe, and that the degree of confidence which will induce us to do so varies from context to context (depending, among other things, on the relevance to us of the matter in question). But even if this is so, it is clear that we often simply overlook a consequence of our beliefs.

The notion of an inferential disposition is thus an idealisation not only in that, for various reasons, a person who utters a conditional may not have the 'associated' disposition, but also in that, these reasons aside, the dispositions to inference that people have are very imperfect. Their effectiveness seems to depend on several factors, but particularly on the extent to which the disposition and its antecedent mental attitude are consciously held (at the same time). However I don't want to investigate these factors here (or the question as to what it is for a mental attitude to be 'consciously held'). But it is worth noting that the notions of belief and partial belief are themselves idealisations in various respects (some of which we have already observed); and that their usefulness as part of an explanatory model of aspects of our behaviour depends in practice on these idealisations. So there is a respectable precedent for our present use of the notion of an inferential disposition.

Thus we may claim that the difference between validity and invalidity, though not accurately marked in the thought processes of any actual person, is revealed in the way we have described in an ideal model; to which our actual thought processes do in important ways approximate. This proposal does not alter the non-standard character of the approach to validity, which continues to provide the basis of a reply to (our reworked version of) Geach's objection, in the way we

have indicated.

However, we have so far ignored another important question: does this approach characterise as valid, or correct, just the same class of inferences as are held to be valid in the standard sense (when, where necessary, premisses and conclusions such as 'It is probable that *q*' are assumed to be truthconditional)? And the answer is 'No'. The following inferences, for example, are both valid in the standard sense[7]:

(iii) *p*
Therefore if not-*p* then *q*

(iv) If *p* then *q*
Not-*q*
Therefore not-*p*

But there is apparently no reason why a person who believes that *p* (or has whatever other mental attitude is associated with *p*) should be bound to have a disposition to infer from a belief that not-*p* (or from whatever mental attitude is associated with not-*p*) to a belief that *q* (or to whatever mental attitude is associated with *q*). Similarly, why should a person who is disposed to infer from a belief that *p* to a belief that *q* and who also believes that not-*q*, be bound to believe that not-p? The approach we have suggested thus seems to classify both *(iii)* and *(iv)* as incorrect forms of inference. In the case of *(iii)* this may be no bad thing; it is often held to be a defect (of certain accounts of the conditional) that such inferences are admitted. But surely we shouldn't classify *(iv)* as invalid.

I think this apparent difficulty stems largely from a mistaken view of the nature of the approach we have suggested. The mistake - which admittedly we have said nothing to prevent - is to see it as offering a criterion for deciding of *any* given form of inference, whether or not it is valid, or correct. It actually offers the basis of an *explanation* of the *acceptability* of certain forms of inference, which are taken to be acceptable in ordinary usage. Thus while it

claims to account for the correctness (in ordinary usage) of *7.2* and *7.1*, it does not - or rather, should not - extend this to a general claim with respect to any form of inference ordinarily judged to be acceptable. For it may be that some forms of inference rely on some more complex mental process than the operation of the inferential dispositions associated with conditional utterances. For example, it may be that many speakers possess the general habit of transposing their inferential dispositions (at least when the antecedent and consequent mental attitudes are both an effectively full belief; we shall see in Chapter 8 that there is a good reason for not doing so when either is a partial belief). Then the presence of this habit, and the acceptability of *7.2*, will explain that of *(iv)*. And while it is an important question where such a habit comes from, I don't think it is one which need concern us here.[8] It does not seem crucial to the task we have been facing, namely that of finding an approach to validity which does not rely on the assumption that all the premisses and the conclusion of the inferences concerned are truthconditional. Moreover, it seems to be the kind of question which any account of validity will face, at some point - and on a par with questions as to the justification of deductive inference.

In summary then, we have seen that Geach's argument, at least when construed as an objection to partial assertions, seems misdirected. It attempts to apply to the inference *7.1* a notion of validity which depends on the assumption that its premisses and conclusion are truthconditional; a notion which the partial assertion view must therefore reject. But the objection thus raises the question as to what non-standard account of validity is available, on such a view. We have now seen that there seems to be the basis of such an account - or at least of an explanation of the acceptability of certain forms of inference - in an account of the states of mind characteristically associated with

conditional utterances. This account of validity faces various problems, but seems sufficient to indicate how there can be a relation analogous to the relation of logical consequence, between sentences which are not truthconditional.

Geach's insistence that the two occurrences of 'It is probable that *q*' in the premisses of 7.1 should not have 'a different explanation' thus turns out to be irrelevant; the notion of validity with respect to which it would be relevant is inapplicable. But this insistence also camouflages an important issue: that of the real connection between the two occurrences. The existence of this connection is the core of Searle's objection; to which we should now turn.

Firstly, it is appropriate to ask how our analysis of the use of conditionals with SP clauses as antecedents or consequents, meets Searle's requirement that 'the analysis of the meaning of a word (or morpheme) must be consistent with the fact that the same word (or morpheme) can mean the same thing in all the grammatically different kinds of sentences in which it can occur'.[9]

We have offered the following account of the connection between the occurrence of an SP clause as (say) the antecedent of a conditional and its occurrence as a complete sentence: an SP utterance characteristically results from a speaker's possession of a certain partial belief; an utterance of the corresponding conditional characteristically results from a speaker's possession of a certain inferential disposition, the antecedent mental attitude of which is this same partial belief. The connection between the partial belief and the disposition of which it is the antecedent attitude is reflected, by convention, in the relation between the clause in question as a complete utterance and the same clause as the antecedent of a conditional utterance. In other words, there is the basis of a rule associated with the conditional

form, such that someone who knows this rule, and who understands a given sentence (i.e. who associates the correct mental attitude with this sentence), will understand and be able to use a conditional in which this sentence occurs as antecedent, even if he has never previously encountered such a conditional.

The recognition that there must exist some such rule ought to be the basis of Searle's objection. For it is only such rules which give significance to the repetition of the same words (or morphemes) in different grammatical contexts. For example, if language were finite, in the sense that every utterance had to be chosen from a finite list, then in principle every occurrence of a member of the list as a part of some other member could be eliminated, simply by the invention of some new word. Repetitions would therefore be of no more significance than the existence of namesakes. So the potentially infinite character of language gives point to an objection such as Searle's. It is seen that language can only be potentially infinite (and yet comprehensible by a finite intelligence), if there is a finite system of rules for constructing new well-formed expressions from old ones and from the finite stock of words. A proposed analysis of some part of language is thus unacceptable if it is unable to make sense of such rules.

However, although repetitions would be insignificant if language were finite, it does not follow from the fact that language is infinite that all - or indeed, any - repetitions are significant. It is possible to invent languages in which transformations according to context ensure that the same 'meaning' is expressed by different words in different contexts; and in which surface repetitions are therefore insignificant.[10] So even if it is unlikely that actual languages are of this kind, it is useful that Searle's objection can in principle be grounded in another way: on an empirical investigation of the ability of speakers to understand and use sentences which they have not prev-

iously encountered, just so long as they are familiar with the component words and structures of such sentences. (Note that this ability might be evident even in the speakers of a finite language, if it were large enough to make impractical the task of grasping its sentences individually). It is possible, in principle, to establish empirical facts of the following general form: in language L, speakers who are familiar with a sentence (or word, or construction) *x*, and with a sentence-forming function F( ), are able to understand and use the sentence F(*x*) even if they have not previously encountered it. The common ground between Searle and those he calls the speech act analysts, on which his objection must rest, seems to consist in facts of this general (broadly syntactical) form.

Our account of conditionals seems to make good sense of the relevant facts of this kind. It takes familiarity with the conditional structure to be a matter of having learnt the association of conditional utterances with inferential dispositions, the antecedent and consequent attitudes of which are the mental states associated with the antecedents and consequents of the conditionals in question. There is thus a general pattern associated with the conditional form, whose application in a particular case depends in a straightforward way on the conventions governing the use of the antecedent and consequent of the given conditional, when these clauses stand alone as complete sentences.

In defence of Searle, it might be said that the partial assertion view is not a speech act analysis, and so isn't the kind of view he is objecting to. We have no direct interest in settling this claim - only in showing that there isn't an objection on the lines of Searle's to the partial assertion proposal. But note that our emphasis on the mental attitudes characteristically associated with various utterances is not incompatible with an account which stresses the speech acts

performed in the making of such utterances. There can be no difference in the illocutionary acts performed, without a corresponding difference in the characteristically associated mental attitude. And speech acts would be pointless if they didn't have at least the potential to produce a change in the mental state of hearers to whom they are directed. So although some differences between utterances with respect to their associated mental attitudes will be explained in terms of differences of *sense*, rather than differences of *force*, there seems to be a reply to Searle in explicitly speech act terms, parallel to ours. The complex speech act performed by an utterance of 'If it is probable that *q*, then *r*' (say) will, as Searle rightly requires, be systematically related to that performed by 'It is probable that *r*', the connection being signalled by the conditional construction of the former sentence. It is true that the connection will not be as simple and direct as either of the alternatives that Searle suggests; and also that the nature of the connection - and the nature of the speech act performed by the conditional - will need to be explained in something like the mental state terms we have used; but these facts do not seem to count against the claim of such an account to be a speech act analysis.

The indicative conditional is not the only type of construction on which Searle bases his objection. He mentions several others, including interrogatives, negations, disjunctions, optatives and past-tensed constructions. I don't propose to deal with these here in as much detail as we have the conditional, but I shall indicate in each case the form of an account which seems to be consistent with our general approach, and which seems to meet Searle's requirements, to the extent that these are well-founded on the kind of grounds we have outlined. No doubt various possible objections will be left unanswered, but I hope at least to show that Searle's argument very much under-

estimates the resources of this kind of programme.

Taking negation first, how are we to explain 'It is not the case that it is probable that *q*'? (The awkward construction here is to make clear that the negation operator attaches to the entire clause 'It is probable that *q*'). With what mental attitude is such an utterance characteristically associated? And does it relate to the attitude associated with 'It is probable that *q*' itself in a way which can be taken to be a function of the use of the negation construction?

We want to know, in effect, under what circumstances a person will ordinarily deny that it is probable that *q*. Under what circumstances will a person say 'That's false' in response to the statement 'It is probable that *q*'?

The most usual such case is that in which a person has an attitude which conflicts with the attitude characteristically associated with the utterance of 'It is probable that *q*' - i.e. with a strong partial belief that *q*. That is, a case in which a person has some other degree of partial belief that *q*. However, it seems we should allow for the possibility that an agent may have grounds for rejecting a strong partial belief that *q*, without settling on any other degree of belief that *q*. So we should say that most generally, the utterance of 'It is not the case that it probable that *q*' is associated with the attitude of *rejection* of a strong partial belief that *q*. And the effect of the negation construction, applied to a given sentence, is to produce a sentence whose associated mental attitude is the rejection of the attitude associated with the initial sentence.

Having said this, it is important to note that there is little ordinary use for the application of a negation operator in this way. 'It is not probable that *q*', for example, usually has the sense of 'It is improbable that *q*' - which is associated with a weak partial belief that *q* (or strong partial belief that not-*q*), rather than with simply

the rejection of a strong partial belief that *q*. And in the limiting case, 'It is not the case that *q*' seems to be always associated with an effectively full disbelief that *q* (or full belief that not-*q*), and never with the mere rejection of a full belief that *q* (which would leave open the possibility of a partial belief that *q*, as well as a full belief that not-*q*).[11] Let us call this *strong negation*, as opposed to the earlier *weak negation*. Thus we find that ordinary usage always intends strong negation in negating sentences employed to make full assertions; and usually does so in negating partial assertions. Note that this fact does not help Searle's objection. Strong negation is just as capable as weak negation of being signalled by certain constructions; and our kind of account is able to deal with either.

These two types of negation give rise to two kinds of disjunction. Thus suppose we say that the mental state characteristically associated with 'It is probable that *q* or it is probable that *r*' is the same as that associated with 'If it is not probable that *q*, then it is probable that *r*'. The latter attitude is an inferential disposition, whose consequent mental attitude is a strong partial belief that *r*; and whose antecedent attitude is either a rejection of a strong partial belief that *q*, or a strong belief that not-*q*, depending on whether the negation in the antecedent of the conditional concerned is taken to be weak or strong. If we call the resulting disjunctions 'weak' and 'strong' correspondingly, it is apparent that an inference can be drawn from a strong disjunction less often than from a weak one (because a strong negation supports a weak one, but not conversely). Just as in ordinary usage negation of utterances employed to make full assertions is always strong, so is their disjunction. And similarly ordinary disjunction of SP sentences seems to be usually, if not always, strong. Note that if there are weak SP disjunctions, then there might be 'mixed' disjunctions, strong in one direction and weak in the other -

particularly in cases in which only one disjunct is an SP clause ('Either *r*, or it is probable that *q*').

Disjunctions seem to be symmetric - the order of the disjuncts seems to make no difference. But we have seen that the transposition of a conditional, although perhaps admitted in some cases in virtue of some habit, does not have the same status as the conditional itself. So this account of the mental state associated with a disjunction needs to be modified. In the above case it is not merely the state associated with 'If it is not probable that *q*, then it is probable that *r*', but the conjunction of this state and that associated with 'If it is not probable that *r*, then it is probable that *q*'. (In the case of weak disjunction this modification would be necessary even if transposition were available, because double weak negation is not equivalent to no negation at all).

Interrogatives and optatives differ from the kinds of sentences we have considered so far in that they do not, even in ideal circumstances, transfer to a hearer the same mental attitude whose possession by a speaker has lead to their utterance; or at least it is not essential to their role that they should ever do so. Thus if we describe the mental attitude most characteristically associated with the utterance of 'Is it the case that *q*?' as *wondering whether q*, it may be that such an utterance will lead a hearer to wonder whether *q*. This is most likely when a speaker is mistaken in thinking that the person to whom he speaks knows whether *q*. But it is easy to imagine a community who never make this mistake, yet for whom the activity of asking such questions has just as much point as it does for us. In contrast, if a group of speakers used the utterances we know as assertions in such a way that they were never the means of transfer of a belief, we should say that they were not being used as assertions at all; the activity of

using them would have some other point. Similarly, the utterance 'I wish it were the case that *q*' might lead a hearer to desire that *q*, but it is not essential to its role that it should ever do so.

With respect to the question 'Is it probable that *q*?', note that it is appropriate only if the speaker refers his SP beliefs (to do with *q*) to the base of evidence he takes the person to whom he directs the question to possess. His asking the question is a sign that he takes this to be the relevant evidence. (This is so, at least, in cases in which the speaker is actually trying to find out whether it is probable that *q* - i.e. is actually seeking to adopt a partial belief with respect to *q*. The question might also be used as in an exam, so that the speaker is interested specifically in the hearer's attitude to *q*; and may have a different attitude, based on different evidence, himself.) So when the person asked says 'Yes' or 'No', his implicit SP utterance rests on the same base of evidence as the original speaker's would, if he were to say, accordingly, 'I see, it's probable (not probable) that *q*'. This explains why although in general A's claim that it is not probable that *q* need not contradict B's statement that it is probable that *q*, if A asks B whether it is probable that *q* he can expect to receive an answer to the question he asks - and not one to some different question concerning B's viewpoint. Such a question indicates that the context is effectively truthconditional, in the sense we have noted (p. 4:17).

This point aside, the following argument from Searle seems misdirected (Searle presents it for the case of 'true', but apparently with the intention that it should apply to the other cases he mentions, including that of 'probable'):

> The word ('probable') means or can mean the same thing in interrogatives as in indicatives .... If it didn't, conversation would be impossible, for 'It is (probable)' would not be an answer to the question 'Is it (probable)?' if ('probable') changed its meaning

from interrogative to indicative sentences.[12]

For one thing, conversation could easily do without question and answer pairs in which significant words occur in both - for example by adopting the convention that questions asked in English should be answered in Japanese, and vice versa - though less extreme measures would suffice. For another thing, there is no guarantee that when the same word does occur in both, it 'means the same thing', in any ordinary sense. This is shown by actual examples such as, 'Is it mine?', 'No, it is mine'; and by the possibility of conventions under which the meanings of words would vary according to whether their context was indicative or interrogative. So the claim that in a given such question and answer pair a certain repeated word 'means the same thing' in both occurrences, requires an argument applicable to the given case; an argument to show, for example, that the relevant word somehow makes the same contribution to the meaning of the question as it does to that of the answer. Searle offers no such argument, for the above case.

On the other hand, the approach we have been suggesting is able to offer roughly the following account. A person who asks 'Is it probable that *q*?' is characteristically indicating that he wants to be guided in his adoption of a certain mental attitude with respect to *q* by the corresponding attitude of the person to whom the question is directed. The word 'probable' indicates that the mental attitude concerned is a strong partial belief; the speaker wants to know whether to adopt such a belief that *q*. Similarly, the word 'probable' in the answer 'It is probable that *q*' indicates that the mental attitude being expressed by this speaker is his partial belief (that *q*). This connection explains our intuition that it is not a mere accident that the same word occurs in both contexts.

Note that on this view, asking 'Is it probable that *q*?' is not asking whether some state of affairs holds, in the way that we are used

to thinking of 'Is it the case that *r*?', when *r* is truthconditional. Similarly with optatives: 'I wish it were probable that *q*' is not wishing that some state of affairs would hold, in the familiar sense. This raises the question as to what counts as satisfaction of such a wish. We seem bound to say that the wish is satisfied when the speaker comes to have a strong partial belief that *q*; and yet this seems wrong, in that it doesn't allow a distinction between the real satisfaction of the wish on the one hand, and the speaker's believing the wish to be satisfied on the other.

The solution is to make it clear that in such a case the sentence 'The wish is satisfied' is no more truthconditional than 'It is probable that *q*' itself. Thus if *W* is the sentence 'I wish it were probable that *q*', we must say that the utterance '*W* is satisfied' indicates, but does not *state*, that the speaker has a strong partial belief that *q*, just as 'It is probable that *q*' would; the former utterance is appropriate in certain circumstances. Hence X's utterance of '*W* is not satisfied, but Y believes that *W* is satisfied' is appropriate when, *inter alia*, X has a strong partial belief that not-*q* (or, if the negation is taken to be weak, a rejection of a strong partial belief that *q*), but believes that Y has a strong partial belief that *q*. '*W* is not satisfied, but I used to think that it was satisfied' is handled similarly. And '*W* is not satisfied, but I believe that *W* is satisfied' is appropriate for a person who has a strong partial belief that not-*q* (or who rejects a strong partial belief that *q*), but who mistakenly believes that he has a strong partial belief that *q*. Such a state of mind is not inconceivable, even if we should expect that being lead to make this utterance would be enough to make such a person realise his mistake. The assertion condition of the first clause of the utterance - i.e. that the speaker have a strong partial belief that not-*q* (or that he reject a strong partial belief that *q*) - is not incompatible with the

truth condition of the second clause, which is that the speaker believe he does have a strong partial belief that *q*. But if '*W* is satisfied' *meant* 'I have a strong partial belief that *q*', then its negation would be an assertion that the speaker did not have such a partial belief, and hence the assertion condition for the clause '*W* is not satisfied' would be that the speaker believe that he does not have a strong partial belief that *q*. And this is incompatible with the truth condition of the clause 'I believe that *W* is satisfied'.

Thus in denying that *W* is a wish for some state of affairs, in the usual sense, and hence denying that there is some state of affairs whose holding would constitute the satisfaction of *W*, we give ourselves the means to distinguish between the claim that *W is* satisfied, and the claim that one *believes* that *W* is satisfied. The distinction parallels that between 'It is probable that *q*' and 'I believe it is probable that *q*' - where the first utterance *indicates* a strong partial belief that *q*, and the second *reports* one (in the indirect way that ordinary usage allows, given that the term 'partial belief' is not in common use[13]). Note that we also have a straightforward explanation for the fact that 'It is probable that *q*' is a way of indicating the satisfaction of 'I wish it were probable that *q*', very similar to our explanation of the corresponding fact about questions and answers.

Let us now turn to sentences of the form 'It was probable that *q*', which, of the various examples Searle mentions in his objection to the speech act analysis, is the most difficult to explain in terms of our general approach. Note, firstly, that such a sentence is almost always used not in isolation, but in contrast to a present-tensed assertion (full or partial) that *q* - an implicit one, very often. Thus: 'It was probable that Proper Name would win' ('But he didn't', or 'And he did', being understood); or 'It was probable that Proper Name would

win tomorrow, but it is now improbable'. More rarely a past-tensed SP clause provides the grounds for an assessment of the present probability of some past event: 'It was probable that Reagan would win, so he probably did'. It might seem that there are many cases which are not of these kinds, such as 'It was (or used to be) improbable that a child would survive to the age of five' - but this is not a single-case probability sentence. SP utterances are closely tied to our present expectations (about states of affairs which may themselves be past, present, future, or of no temporal location), and hence to our present behaviour. Since we are unable to affect our past behaviour, we have no direct interest in actual or hypothetical past expectations. Hence our lack of use for past-tensed SP utterances in isolation. (Note that in contrast, 'It will be probable that *q*' is generally taken to imply 'It is probable that *q*'; a fact which is perhaps more readily explained on our view than by various objectivist accounts, which seem committed to making sense of objective probabilities changing over time).

The significance of our lack of use for past-tensed SP utterances in isolation is partly that it means we have no need to associate such a sentence with some past partial belief, actual or hypothetical; there is always a relevant present belief. And because it suggests that such sentences have a rather specialised use, it suggests that we are free to give a rather complex interpretation of such an utterance, without having to meet the objection that it is unlikely that a very common usage has such a relatively complex basis.

I propose to interpret 'It was probable that *q*' as, in the most general case, equivalent to

7.4 For some *r*, if it (ever) were the case that *r* then it would be probable that *q*, and it was the case that *r*.

By 'equivalent to' here I mean that the mental attitudes character-

istically associated - in the sense we have been using - with utterances of the two sentences concerned, are the same.

The first important qualification, however, is that *7.4* will generally represent only what is conveyed to a hearer, in ideal circumstances, by 'It was probable that *q*'; and not the state of mind of the speaker. For there will generally be some *r'* of which the speaker believes

7.5 If it (ever) were the case that *r'* then it would be probable that *q*, and it was the case that *r'*.

(The most significant case in which *7.5* may not be what the speaker believes seems to be that in which his grounds for the utterance 'It was probable that *q*' are that he has heard someone else, whom he takes to be reliable, making the same utterance; then, if he doesn't know on what past evidence this person is relying, his attitude will go by *7.4* rather than 7.5). So in the usual case the speaker's characteristic mental attitude consists in the combination of a disposition to infer from a belief that *r'* (is true at a particular time) to a strong partial belief that *q* (is true at a corresponding time), with a belief that it was the case that *r'* (at some particular earlier time). Note that such a mental attitude does not commit the speaker to a strong partial belief that *q* is true at a time corresponding to the present time; only the additional belief that it is at present the case that *r'* would do so. (How all this is best put depends on whether *r'* and *q* are taken to be tensed, and if so, on how tensed utterances are treated by an account such as ours; but I would prefer not to discuss these questions here).

Although in the usual case an utterance of 'It was probable that *q*' is an indication that there is some relevant *r'* such that the speaker believes that it was the case that *r'*, the utterance is not in

any sense an assertion that it was the case that *r'* - just as, as we have seen, 'It is probable that *q*' is in no sense an assertion about the speaker's evidence. But whereas in the latter case a hearer who takes the speaker to be reliable will adopt a strong partial belief that *q*, in the former one there is no simple corresponding attitude. However, in many contexts speakers share common beliefs about the relevant past states of affairs - i.e. they have the same beliefs, and take it for granted that they do so - so that the hearer already believes that it was the case that *r'*, and knows that *r'* is the state of affairs relevent to the speaker's utterance of 'It was probable that *q*'. In such a case, if the hearer takes the speaker to be reliable, the utterance will give rise to the same mental attitude on the part of the hearer as it results from on the part of the speaker.

Otherwise, in the most general case, a trusting hearer will adopt only the mental attitude of which *7.4* is an appropriate expression. This complex attitude involves a sub-attitude of a type we have not yet discussed: that associated with the existential quantifier. I think we may construe this in terms of negation and the universal quantifier. The universal quantifier seems relatively easily dealt with, as associated with a habit of adopting (or a disposition to adopt) an attitude *At*(*x*), with respect to any *x* believed to be in the range of the quantifier in question. The nature of *At*( ) depends on the form of the scope of the quantifier. In the case of 'All participants in the lottery will probably lose their money', for example, *At*(*x*) will be a strong partial belief that *x* will lose his money.

We want to say that an utterance of the form 'For some *x*, *S*(*x*)' is characteristically associated with the same mental attitude as the corresponding utterance of the form 'It is not the case that for all *x*, not *S*(*x*)'. But we have distinguished two kinds of negation (p. 7:23); which one is applicable to each of the two contexts here?

Strong negation depends on the existence of a mental attitude opposite to that associated with the sentence being negated - for example on a strong disbelief (or weak partial belief) that q in the case of 'It is not probable that q', and on an effectively full disbelief that q in the case of 'It is not the case that q'. There seems to be no such opposite mental state to the kind of habit associated with a universal quantifier, unless it is the habit of *never* doing whatever it is. But a person may think that it is not the case that all businessmen are rogues, say, without being disposed to never think that a businessman is a rogue. So the first negation in 'It is not the case that for all x, not $S(x)$' is a weak one.

If the second negation is strong, then this utterance is characteristically associated with a rejection of the habit of adopting, for any x, the opposite mental state to that characteristically associated with the utterance '$S(x)$'. If $S(x)$ is the sentence 'It is probable that $q(x)$', for example, then this attitude amounts to the rejection of the habit of adopting, for any x, a strong partial disbelief that $q(x)$ (or a strong partial belief that not-$q(x)$). However a person may reject this habit, without there being any x with respect to which he holds a strong partial belief that $q(x)$ (and is thus prepared to say, 'It is probable that $q(x)$'). This will be so for a person who thinks that for all x, there is a 50% chance that $q(x)$, for example. So if the second negation is taken to be strong, 'It is not the case that for all x, it is not probable that $q(x)$' cannot be taken to be equivalent to 'For some x, it is probable that $q(x)$'.

I therefore take both negations in 'It is not the case that for all x, not $S(x)$' to be weak; and hence take 'For some x, $S(x)$' to be characteristically associated with a rejection of the habit of rejecting, for any x, the mental attitude characteristically associated with the utterance '$S(x)$'.

In the case in which $S(x)$ is truthconditional, the claim that the second negation here is weak seems to conflict with our earlier (p. 7:23) observation that ordinary usage applies only strong negation to sentences used to make full assertions. However, the conventions governing truthconditional contexts seem to ensure that in this case the second negation can be taken to be strong: that if $S(x)$ is truthconditional, then a person will only reject the habit of disbelieving that $S(x)$, for every x, if he does think there is some x such that it is the case that $S(x)$. The possibility that would otherwise count against strong negation - that he might merely think that there is some x such that it is probable that $S(x)$, say - is taken to be ruled out by the nature of the context. (It is hard to say exactly what conventions ensure that this is so; but the law of excluded middle seems to play an important role).

Applying all this to the case that we are interested in, we thus take *7.4* to be characteristically associated with the rejection of the habit of rejecting, for propositions r, the combination of the disposition to infer from a belief that r to a strong partial belief that q, with the belief that it was the case that r. Note that rejecting the combination of two mental states amounts to rejecting their conjunction, not to the conjunction of the rejection of one with the rejection of the other.

The relative complexity of the mental states we thus associate with past-tensed SP utterances seems to me to be no objection to our general view. We have seen that the complexity results from the relation of the evidence on which the speaker relies to the context of utterance - from the fact that it is not usually evidence which the speaker would now use. The hearer's relation to this evidence is generally different, so there is further complexity - the details depend on what base of evidence the hearer has in common with the speaker. Now if

such utterances were a common and fundamental part of ordinary usage, we might expect a much simpler account than this. But we have seen that there is relatively little use for such sentences. When they are used, moreover, it seems that they are often a source of confusion - people fail to distinguish 'It was probable that *q*' from 'It is probable that it was the case that *q*', for example; or are reluctant to accept such a claim as 'It was probable that he would win, but he lost'. In the latter case 'It seemed probable that he would win, but he lost' is often preferred; an utterance which has a much more straightforward analysis, in terms of the speaker's previous partial beliefs, and perhaps those of his peers - or in terms of his beliefs about these partial beliefs, to be precise. So if anything ordinary usage provides a certain amount of evidence that a past-tensed SP utterance is a rather complex construction.

It should be emphasised that the mental states we are taking to be associated with utterances need not be consciously held - and certainly not in the sense that speakers should, if asked, be able to report their possession of these attitudes. If that were so, language would be impossible unless it had the means (and its speakers had the ability) to refer to all its associated mental attitudes. Our own language seems at present very limited in this respect. We have already had to invent a term or two ('inferential disposition', for example), even to deal with very common types of utterance.

The more we have to characterise new kinds of mental state, the more we are likely to find that our classification of such entities depends on some prior classification of the utterances with which we are taking these mental states to be associated. We saw in Chapter 6 (pp. 6:3-4) that this is likely to be the case, but that it doesn't undermine the use we were then making of the notion of the mental

attitude associated with a type of utterance, which was to introduce the notion of partial assertion in terms of partial belief. In this case the mental state is relatively familiar, and so does serve to characterise the type of utterance. In this chapter we have made a different use of associated mental attitudes: they have enabled us to give an account of the connection between the use of an SP clause as a complete sentence and its uses as a component of various more complex sentences - and thus, I think, to meet the objections of Geach and Searle. This use does not depend on an ability to classify associated mental states without a prior classification of the corresponding utterances.

It is worth emphasising that our use of the notion of the mental state characteristically associated with an utterance does not commit us to claiming that *every* utterance of a particular sentence signifies that its speaker has the mental state we have said to be *characteristically* associated with such an utterance. It doesn't rule out insincere SP utterances, for example; or many kinds of specialised uses of these and other utterances, in which the use of an utterance doesn't conform to its most characteristic pattern. At most we need to claim that these specialised uses rely on the characteristic one - that they would not be understood by someone who did not understand the characteristic use, in particular. (Our sentence 'I am incapable of believing anything' - p. 7:13 - is a minor example of such a specialised use).

Underlying this approach is roughly the following view of language. Learning a language is a matter of acquiring habits, particularly of two basic kinds: habits of making utterances whose form depends on various aspects of one's mental state; and habits of changing one's mental state in various ways, in response to various features of utterances one has heard. These habits are general in form, and many of them operate in the production or use of a single utterance. Their use of

particular words and other syntactical components is conventional, but not without restrictions: it is essential that the conventions be such as to provide syntactically-based rules to enable speakers to generate, and hearers to utilise, utterances which they have not previously encountered. Neither a speaker's production of an utterance as a result of his possession of a certain mental state, nor a hearer's utilisation of that utterance to produce a particular mental state, is a conscious decision - or indeed a decision at all. This is not to deny that some or all of the mental states involved may be consciously held (whatever this amounts to), but just that there need be a conscious belief of the form 'I have this mental state, so I should make such-and-such an utterance' (or 'I have heard this utterance, so I should adopt such-and-such a mental attitude'). There may well be cases in which utterances do in part result from, or produce, beliefs of this form - but then these beliefs simply form part of a complex mental state, whose connection with the utterance in question is not itself mediated by another conscious attitude. Recognition of this point is essential, if we are to avoid an objection based on the observation that most of the mental states to which this account will need to refer are not ones of whose existence most speakers are - or any speakers need be - aware; or even ones for which ordinary language has names.

Finally, this view might seem too committed to the existence of complex mental states, prior to the ability to speak a complex language. On the contrary, it would thus be claimed, our ability to adopt various complex mental attitudes (if not to adopt even simple ones), depends on our possession of a language 'in which to think'. I think such an objection is unlikely to be successful. For one thing, the notion of thinking in a particular language seems far less applicable to unconscious mental states than to conscious ones. More importantly, the view we have taken seems consistent with the claim that mental

complexity, and the capacity to hold certain complex mental attitudes, develops alongside linguistic ability. It is true that such an account would have to allow for a certain innate capacity for complex linguistic and mental activity, but any approach seems bound to admit this much.

Notes

1. Geach (1960); the argument is repeated in his (1965). And Searle (1969), pp. 136-141.
2. Geach (1960), p. 223.
3. Searle (1969), pp. 136-9; Searle gives the following references for the views to which he is objecting: Hare (1952), Ch. 2, Strawson (1949), Austin (1946), and Toulmin (1950).
4. Given that the same form of words does occur in both contexts - and that we have no reason to think this is accidental - we should expect this fact to reflect a non-syntactical connection, of which an account of the validity of *7.1* will need to take note. But until we know what such an account will look like, when it cannot be assumed that the premisses are truthconditional, we have no reason to think that the relevance of this connection will be any less obvious than it is on the standard view.
5. Dummett (1973), pp. 351-54.
6. Cohen (1977), p. 29, n. 19; I am grateful to Jonathan Cohen for bringing this to my attention.
7. If the conditional is interpreted appropriately, at any rate.
8. Partly for the reason that the habit of transposition does not seem to apply to the dispositions associated with conditionals containing SP clauses. This, incidentally, is one reason why it seems better to rely on such a habit to give us *(iv)* than to attempt to

claim that inferential dispositions are transposable by nature.

9. Searle (1969), p. 137.

10. To illustrate, suppose that a language L_1 is built up from the atomic sentences s_1 and s_2 by means of the operations 'Not' and 'And'; and that the sentences of L_1 are systematically listed as t_0, t_1, t_2, ..., in such a way that knowing the form of t_i, it is possible to work out that of t_{i+1}. Let L_2 be defined from L_1 by the rules *(i)* To express what would otherwise be expressed by 'Not t_i' say 'Not t_{i+1}'; and *(ii)* To express what would otherwise be expressed by 't_i and t_j' say 't_{i+2} and t_{j+2}'; these rules to be applied repeatedly in building up sentences of multiple complexity. Thus if s_1 is t_0 and s_2 is t_1, what is expressed by '(Not t_0) and t_1' in L_1, is expressed in L_2 by 't_{n+2} and t_3', where n is the index in the given list of 'Not t_1'. Interpreting a sentence of L_2 is a matter of applying these rules in reverse, in the order dictated by the structure of the sentence concerned, and then of rebuilding the sentence in L_1. But in L_2 itself, occurrences of s_1 and s_2 in complex sentences are of little more than accidental significance.

11. We are relying on this fact in writing 'not-q'. Its explanation is perhaps that only evidence that not-q could comprise grounds for rejecting an effectively full belief that q - though this would not be so if there were non-relational objective chances, for then evidence that the chance that q is other than 1 would comprise such grounds.

12. Searle (1969), p. 137.

13. Cf. pp. 6:13-14. We can say 'I am confident that q', which has a very similar sense to 'I believe it is probable that q'; the fact that one is sometimes more appropriate than the other seems explicable largely in contextual terms - because a discussion is, or is not, couched in terms of probabilities, for example.

8. FURTHER PROBLEMS.

CONDITIONAL PROBABILITY

Jonathan Cohen raises the following objection to 'guarded assertion' theories of probability:

> ...(A)ny analysis for the term 'probable', that is to allow interpretation of the mathematical calculus of chance as a logic of probability, must at least elucidate a certain well-known fact. This is that the following three expressions do not necessarily have the same truth-value for particular A, B and n, viz.: p(B,A) = n, p(A → B) = n, and A → p(B) = n. And the guarded-assertion theory is inherently incapable, on its own, of elucidating this fact, because all three expressions can function equally well as forms of guarded assertion where the truth of A is the only known or assumed evidence.[1]

Let us read 'partial assertion' for 'guarded assertion'. Is the partial assertion view of SP utterances able to meet this objection? And more generally, how does this view relate to the mathematical calculus of chance, or probability?

Let us firstly consider the pair of expressions 'p(B,A) = n' and 'A → p(B) = n'. The fact that these expressions may have different truth-values (for particular A, B and n) is supposed to be revealed by examples in which p(B,A) is not equal to p(B), and in which A is contingently true. Now 'p(B,A)' is generally read as, and intended to formalise, 'the probability of B, given A'. Thus if 'A → p(B) = n' is read as 'If A, then there is a probability n that B', then the non-equivalence of these expressions is revealed by a case in which for some n_1 and n_2 ($n_1 \neq n_2$): *(i)* the probability of B, given A, is n_1; *(ii)* the probability of B is n_2; and *(iii)* it is the case that A. For in such a case, the claim is, the assumption that *(iv)* if A, then the probability of B is n_1, leads to a contradiction.

However, the partial assertion account denies that *(ii)* and *(iv)*

are truthconditional, and seems likely to do the same for *(i)*, when it offers an interpretation of such a sentence. It therefore doesn't admit a notion of equivalence here based on speaker-independent truth-values, but only the following substitute: it says that two sentences are not equivalent if and only if there are circumstances in which a speaker acquainted with both will be prepared to use (or assent to) one but not the other. The relevant question is thus whether, on our account, a speaker can have the mental attitudes associated with each of *(i)*, *(ii)* and *(iii)*, at the same time. We know that this is not so for *(ii)*, *(iii)* and *(iv)* (for an ideal speaker, at any rate[2]), because the belief associated with *(iii)* and the inferential disposition associated with *(iv)* together guarantee a partial belief of degree n_1, rather than n_2, that B. So only if the situation is different with respect to *(i)*, *(ii)* and *(iii)* does this objection point to a significant difference between *(i)* and *(iv)*.

The fact that we do not yet have an interpretation of *(i)* in line with our general account prevents us from *demonstrating* that *(i)* is just as incompatible as *(iv)* is with *(ii)* and *(iii)*. Instead, we have to ask whether there are any actual cases in which speakers do assent simultaneously to sentences of the forms *(i)*, *(ii)* and *(iii)*. It seems to me that there are not, at least so long as 'The probability of B is *n*' is an SP sentence, which is the case that concerns us here.

If it is true that ordinary usage doesn't significantly distinguish between *(i)* and *(iv)*, then we may take the mental attitude characteristically associated with *(i)* to be the same as that associated with *(iv)* - i.e., a disposition to infer from an effectively full belief that A to a partial belief of degree n_1 that B.

There seem to be two strands in the usual rejection of this equivalence. One is the argument we have just looked at. About this, note that the problem arises essentially with respect to *(iv)*. If

probability ascriptions are truthconditional, then someone who asserts 'If A, then probably B', and admits that it may be that A, seems unable - consistently - to claim also that it is not probable that B; for this claim, with the initial assertion, entails that not A. On the other hand 'The probability of B, given A' seems to have an interpretation given by

8.1 $p(B,A) = p(A \& B)/p(A)$.

Under this interpretation, 'The probability of B, given A, is high' is not incompatible with 'The probability of B is low', for a person who admits that it may be that A. The claim that the probability of B is low does seem to impose *some* constraint on what may be consistently claimed about the probability of A, for a given high p(B,A) - we shall see why this is later on - but it does not require that A be said to be false.

The second strand in the usual rejection of the equivalence of *(i)* and *(iv)* seems to be the view that the former is associated with 'the odds at which (a person) would now bet on (B), the bet only to be valid if (A) is true'; whereas the latter indicates 'the degree to which (a person) would believe (B), if he believed (A) for certain'.[3] And these may be different, in general, 'for knowledge of (A) might for psychological reasons profoundly alter (such a person's) whole system of beliefs'.[4] I think our characterisation of the conditional in *(i)*, in terms of the notion of an inferential disposition, largely avoids this problem. There seems to be no reason why we shouldn't allow that such a disposition may be overridden, in certain cases, by a disposition of some other kind - say by an agent's disposition to commit suicide, if he comes to believe that A, to give an extreme example. Because 'If A then probably B' is not an assertion *about* the speaker's state of mind - indeed, not an assertion at all, in the

strict sense - its sincere utterance is not incompatible with the existence of such an overriding disposition.

However, the question remains as to why someone who has the disposition to infer from a full belief that A to a strong partial belief that B, should accept certain odds but not others on a conditional bet that B (the bet only to be valid if A is true). If we wish to identify *(i)* and *(iv)*, and associate *(i)* with the choice of such conditional betting odds, we shall have to provide a rather strong answer to this question.

Ideally, a willingness to accept certain odds on an appropriate conditional bet will turn out to be equivalent to - or at least a part of - our behavioural characterisation of the relevant inferential disposition. We have not yet offered such a characterisation; if anything, we have simply relied on the notion that someone who has such an inferential disposition will exhibit the behaviour associated with its consequent mental state (or at least will be disposed to do so), whenever he exhibits (or is disposed to exhibit) the behaviour associated with its antecedent mental state. But it is important to recognise that such inferential dispositions have a use, and are revealed, not only when they are 'actualised' in this way, but also in hypothetical reasoning. And since a person's choice of conditional betting odds seems to depend on such reasoning, I think we have good grounds for expecting the kind of inferential disposition associated with *(iv)* to be revealed in the choice of such odds. Indeed, a good indication that there is this connection is the fact that if a person chooses certain odds on a bet that B, conditional on A, but then different odds on a straight bet that B, when A turns out to be the case, we do think his behaviour calls for explanation - we do look for the 'psychological reasons' behind it. Moreover the fact that such an explanation typically concentrates on the way in which the 'knowledge of A might ...

alter such a person's whole system of beliefs' (p. 8:3), shows that we do generally take it for granted that the source of what needs to be explained is not, or is only indirectly, some feature of the person's mental state prior to his coming to believe that A; and so is nothing so straightforward as the lack of a significant connection between his choice of conditional betting odds and the mental state in virtue of which he is prepared to assent to *(iv)*.

Thus I think we are justified in taking a willingness to assent to either *(i)* or *(iv)* to be associated with the choice of a conditional betting quotient n_1 on a bet that B, conditional on A; and hence in taking *(i)*, as well as *(iv)*, to be characteristically associated with a disposition to infer from an effectively full belief that A to a partial belief of degree n_1 that B.

In thus denying that the standard use of 'If A, then probably B' supports the contraposition, 'If it is not probable that B, then it is not the case that A', we deny that the former 'If ... then ...' is the truth-functional conditional. Since we are regarding 'It is probable that B' as non-truthconditional, we are bound to deny that a conditional with this clause as its consequent is truth-functional in the standard sense. But this fact does not in itself commit us to rejecting contraposition. We might interpret 'If A then probably B' as 'Not-A or probably B' - and, as in Chapter 7 (p. 7:24), associate this disjunction with the mental state consisting of two inferential dispositions: that from a belief that not-not-A to a strong partial belief that B, and that from a strong partial disbelief that B (or a rejection of a strong partial belief that B, if the negation is taken to be weak) to a belief that not-A.

The reason we have little or no use for this disjunction is that when 'It is probable that B' is the conclusion of some argument or chain of reasoning, the base of evidence on which it rests always

includes any premisses on which the conclusion rests. But if 'It is not probable that B' is to be used as a premiss in the transposition of such an argument, it must rest on a smaller base of evidence; otherwise it would presuppose what it is supposed to refute. So, in effect, the disjunction 'Not-A or probably B' runs together two different uses of 'It is probable that B' - uses which differ in depending on different bases of evidence. We have noted before that to treat a belief as a full one is to treat as irrelevant changes of evidence - to the extent that we are sure that A, we are sure that there is no evidence showing that not-A - and this explains why contraposition is permissible of the conditional 'If A then B', associated with the disposition to infer from an effectively full belief that A to an effectively full belief that B.

If *(i)* is equivalent to *(iv)*, and the value of the probability of B, given A, is correctly specified by *8.1*, then it needs to be explained why someone who claims 'If A, then the probability of B is n' should also agree that $p(A \& B)/p(A) = n$. More exactly, it needs to be explained why a person should be prepared to claim *(a)* 'If A, then the probability of B is n', *(b)* 'The probability of A & B is n_1', and *(c)* 'The probability of A is n_2', only if $n = n_1/n_2$; or if not why a person *should* believe *(a)*, *(b)* and *(c)* only on this condition, at least why people generally *do* do so.

It seems to me that the proper account will be of the latter kind. Roughly, it will explain this fact about the way in which we arrange our mental attitudes, as resulting from the nature of the general habits which lead us to adopt the particular mental attitudes associated with *(a)*, *(b)* and *(c)*. We have not discussed the form of these habits, or the form of their antecedent mental attitudes (i.e., the attitudes given which these habits lead us to adopt the mental states associated with such utterances as *(a)*, *(b)* and *(c)*). But to

illustrate the present point, let us suppose that the antecedent attitudes are general beliefs about relative frequencies, and that the habit concerning *(c)* is to adopt a partial belief of degree n_2 that A, if A instantiates some general type such that one believes that $100n_2$% of propositions instantiating this type are true. (This is really a great over-simplification). Then the habit concerning *(b)* is to adopt a partial belief of degree n_1 that A & B, if A and B instantiate general types such that one believes that $100n_1$% of propositions of the form p_A & p_B are true, where p_A and p_B are of the type instantiated by A and B, respectively. (This is also over-simplified: if A and B are of the general forms A( ) and B( ), what is important is the relative frequency of true propositions of the form A(x) & B(x), not of the form A(x) & B(y)). And the habit concerning *(a)* is to adopt a disposition to infer from an effectively full belief that A to a partial belief of degree n that B, just when the habits associated with *(b)* and *(c)* would lead to partial beliefs of degrees n_1 and n_2 that A & B and A, respectively, and $n = n_1/n_2$.

The fact that people posses habits of these kinds will explain the fact that people *are*, generally, prepared to claim *(a)*, *(b)* and *(c)* only if $n = n_1/n_2$. The question then becomes, why do people have such habits? It seems to me that the most plausible approach is an evolutionary one. These habits are part of our linguistic and conceptual inheritance. That explains why we have them, while the fact that they form part of this inheritance is to be explained in terms of their general usefulness (and of several circumstantial factors, such as the capacity of human minds to function at this level, and perhaps the existence of certain linguistic and conceptual preconditions for the development of the language and concepts of probability).

I shall say a little more about this kind of explanation in Chapter 9 (though not particularly with respect to its present use).

Its present use, to recap, is to account for the fact that the use of the expression 'The probability of B, given A, is ...' (formalised as 'p(B,A) = ...') is constrained by *8.1*, given that we are claiming that in practice it is equivalent to 'If A, then the probability of B is ...'. I think this sketch of an explanation is enough to dispel the feeling that our equivalence claim leaves something mysterious about this constraint.

We are now in a position to observe the sense in which both 'p(B,A) = *n*' and 'A → p(B) = *n*' support a kind of weak contraposition. It results from the fact that p(A & B) ≦ p(B), so that p(B,A) = p(A & B)/p(A) ≦ p(B)/p(A). So p(A) ≦ p(B)/p(B,A), so that for fixed p(B,A), there is an upper bound on p(A) which decreases with p(B). In particular, when p(B,A) is close to 1, then p(A) is virtually bounded above by p(B) - which gives standard contraposition in the limiting case: if p(B,A) = 1 and p(B) = 0, then p(A) = 0.

The partial assertion account will interpret and justify the inequality 'p(A & B) ≦ p(B)' as it does the equality 'p(B,A) = p(A & B)/p(A)'; that is, as a formalised description of a general characteristic of the way in which we arrange our partial beliefs, which holds because - and in so far as - we adopt such beliefs according to certain rules of inference. The resulting inequality 'p(A) ≦ p(B)/p(B,A)' will also be interpreted in this way - i.e., roughly, as a formalised description of a pattern into which our partial beliefs tend to fall. Note that our beliefs may be generally constrained in this way without our being aware of the fact, and of the reason for it; and therefore without our having a corresponding rule of inference, say from a disposition to infer from a belief that A to a strong partial belief that B (i.e. what we would normally report as a belief that the probability of B, given A, is high), and a strong belief that not B, to a strong belief that not A. This is also true of

non-probabilistic inferences: as we saw in Chapter 7 (pp. 7:16-17), the inferential disposition associated with 'If A then B' does not in itself guarantee the transposed disposition, but may do so for someone who has the habit of transposing such conditionals. We now see that the source of such a habit may be an awareness of the formal properties of rules of inference whose own justification lies at a more basic level (though we should note that such an awareness might well accompany a mistaken view of the *origin* of these formal properties).

Let us now turn to the expression 'p(A → B) = *n*'. To start with, let us suppose that '→' indicates the material conditional (and that A and B are truthconditional). Then 'A → B' is itself truthconditional, and what it expresses can be believed, fully or partially, in the usual way. And it can be bet upon, which is the best way to exhibit the difference between 'p(A → B) = *n*' and 'p(B,A) = *n*' (or 'A → p(B) = *n*', so long as '→' is *not* taken truth-functionally, but rather in the way we have suggested). Thus suppose I have the chance to place a bet that if the Vicar is struck by a thunderbolt, he will survive - it being specified that the conditional is to be taken materially. Although I think the Vicar would be unlikely to survive such an accident, I also have a high degree of partial belief that the antecedent will not be realised; that the Vicar will not be struck. Hence I have a high degree of partial belief that the bet will be won, and I shall thus be prepared to accept a high betting quotient, staking a large amount for a small potential return. And I shall be prepared to say 'There is a high probability that A materially implies B, where A is the proposition that the Vicar will be struck by a thunderbolt, and B the proposition that he will survive'.

If on the other hand I am offered a conditional bet that the Vicar will survive, the bet only to be valid if he is struck by a thunderbolt, I shall only accept at a very low betting quotient,

corresponding to the very low degree of partial belief that the Vicar has survived, which I am disposed to infer if I come to believe that he has been struck. I shall thus be prepared to say 'If the Vicar were struck by a thunderbolt, there is very low probability that he would survive'; or 'The probability that the Vicar will survive, given that he is going to be struck by a thunderbolt, is very low'.

In examples such as this the partial assertion account is able to exhibit the difference between (the ordinary language expressions represented by) 'p(A → B) = *n*' and 'p(B,A) = *n*' (or 'A → p(B) = *n*'). Now Cohen's objection is that 'all three expressions can function equally well as forms of (partial) assertion where the truth of A is the only known or assumed evidence'.[5] This is so, roughly, because

8.2 p(A → B) = p(not-A or B) = p(not-A) + p(B) - p(not-A & B)

(given that '→' is the material conditional). Thus for a group of speakers who take p(A) = 1, so that p(not-A) = 0,

8.3 p(A → B) = p(B).

In other words, the constraints of coherence are such that a person who has an effectively full belief that A and a certain degree of partial belief that A → B, should - for coherence - have the same degree of partial belief that B. If such a speaker thinks that a hearer also has the full belief that A, and that the hearer knows that the speaker believes that A, he will take 'There is a probability *n* that A → B' to be a suitable expression of his partial belief of degree *n* that B. This utterance may be preferable to 'There is a probability *n* that B', for example if the speaker wishes to conceal his partial belief that B from a second hearer, who (the speaker believes) does not know that the speaker believes that A.

However, this kind of thing is not confined to contexts involving

probability. Suppose you and I both know that a third person, X, was seen in the vicinity of the bank, soon after it was robbed; but that X doesn't know we know this. I am certain that if X was in the vicinity at the time it was he who robbed the bank, and therefore certain that he did it. To express this belief to you in the presence of X, without letting him know that I'm certain that he did it, I may say 'If X was in the vicinity of the bank at the time, he is the person who robbed it' (or better still, 'Either X robbed the bank, or he was nowhere in the vicinity at the time'). But the existence of this kind of case does not show that a speech act analysis is unable to make sense of the difference between asserting that B and asserting that A materially implies that B; or between the latter speech act and that which expresses a disposition to infer from a full belief that A to a full belief that B. It is simply that these differences are not clearly revealed in this kind of case - rather as the difference between 'Hesperus is a planet' and 'Phosphorous is a planet' is not revealed in a community of speakers who take it for granted that these names refer to the same thing, for example.

The same is true in the probabilistic case. The partial assertion account admits a clear distinction between 'It is probable that A materially implies that B' and 'If A then probably B' (or 'B, given A, is probable'; note again that since this account denies that 'It is probable that B' is truthconditional, it is unable to make sense of a demand for an interpretation of 'A materially implies that probably B'; cf. pp. 8:5-6). But it is free to admit that in certain cases, such as when A is taken for granted, this distinction is not revealed in the use of these expressions.

Having said this, it is important to note that it is very rare indeed for 'It is probable that if A then B' to be used in the sense of 'It is probable that A materially implies that B'. Instead the former

sentence is almost always equivalent in use to 'If A, then it is probable that B' (and hence, as we have seen, to 'B is probable, given that A'). This is shown, for example, by our rejection of an inference from *(i)* 'It is very unlikely that the Mayor will come for lunch' to *(ii)* 'It is probable that if the Mayor comes for lunch, he will have forgotten to shave'. If *(ii)* were of the form 'It is probable that A materially implies that B', then *(i)*, and the fact that p(A materially implies B) = p(not-A) + p(B) - p(not-A & B) $\geq$ p(not-A), would entail *(ii)*; yet obviously we don't take such an inference to be valid.

Ernest Adams has presented many similar examples.[6] And David Lewis has shown that under very weak assumptions *no* interpretation of '$\rightarrow$' can reconcile such examples with the claim that 'It is probable that if A then B' is of the form 'It is probable that $(A \rightarrow B)$'.[7] However, neither Adams nor Lewis draws the conclusion that we simply don't have any ordinary use for sentences ascribing probabilities to indicative conditionals; rather each concludes, roughly speaking, that the assertability of such a conditional goes by the conditional probability of its consequent, given its antecedent, and not by its own absolute probability. Hence each faces the task of explaining this fact. Now this conclusion does not seem to be available to an account such as ours, at least as a distinct conclusion from the claim that ordinary usage simply doesn't ascribe probabilities to indicative conditionals; this is so, roughly, because we do not have a notion of meaning, other than in terms of assertability conditions. But this distinction seems no loss. It is true that we ought to explain *why* ordinary usage behaves in this way, but this seems a simpler task than the one that Adams and Lewis set themselves.

Indeed, if indicative conditional utterances are to be understood as being - most characteristically, at any rate - expressions of

inferential dispositions, and as non-truthconditional, then it is difficult to see what role there could be for an utterance of the form 'It is probable that (A → B)'. Such an utterance would presumably have to be associated with a partial inferential disposition; but how can a constant disposition come by degrees? (And we certainly shouldn't associate such an utterance with an inconstant disposition - say, to *usually* infer a belief that B from a belief that A). So if this view of conditionals is correct, it is not surprising that ordinary usage does not ascribe probabilities to conditionals, except perhaps to material ones. And although, as we have seen, the partial assertion view can make sense of an ascription of the latter kind, the scarcity of such ascriptions in ordinary use seems at least partly explained by the fact that we rarely use the conditional form of the material conditional. We tend to use the disjunctive form instead, to which probabilities do seem to be ascribed in the standard way - i.e., in particular, so that as p(A) increases, so does p(A or B). If there are any uses of the material conditional in conditional form, and it turns out that even in these cases probabilities are not ascribed to the conditional, then we shall have to say there is simply a convention not to use the form 'It is probable that if A then B' in this way. But such a convention is perhaps to be expected, given the much more widespread use of this form with respect to non-material conditionals, as equivalent to 'If A, then probably B'. This more widespread use perhaps results from a general syntactical convention, allowing an occurrence of 'probably' within a sentence to be replaced by an earlier occurrence of 'It is probable that ...'.

In summary, we have argued that a partial assertion account has no need to make sense of a difference between 'B, given A, is probable' (or 'The probability of B, given A, is *n*') and 'If A, then it is probable that B' (or 'If A, then the probability of B is *n*'). It is able to

distinguish between 'It is probable that A materially implies that B' (or 'There is a probability *n* that A materially implies that B') and either of these earlier expressions, but seems to have very little need to do so. In practice 'It is probable that if A then B' usually, if not always, is used so as to be equivalent to these other expressions.

INTERPRETING THE CALCULUS

A question raised by the above discussion is in what sense does the partial assertion view provide an interpretation, or a model, of the mathematical calculus of probability? In what way ought it to do so, and does it succeed?

The probability calculus is unlikely to be much of a constraint on an account of probability, unless it is based on what such an account needs to interpret: the forms and structures of ordinary usage involving 'probability' and related terms. So let us assume that the probability calculus formalises what ordinary usage seems to take to be true of probability. Then it would appear that no matter how an account of probability *interprets* the calculus, it is bound to do so fully and without deviation, so as not to conflict with ordinary usage.

I think this requirement is too strict, however. Standard representations of the probability calculus embody the assumption that probability is a *property* (of events, or propositions, or whatever). This assumption is not a given of ordinary usage, but an introduced tenet of the interpretations in question. In so far as the calculus depends on it, the calculus may be rejected by a view of probability which rejects this assumption.

We saw something of this kind in the previous section. We saw that the assumption that 'It is probable that B' is truthconditional supports the transposition of 'If A, then it is probable that B'; and

hence the inference from this conditional and 'It is not probable that B', to 'Not A'. The need to accommodate this inference leads in turn to the distinction between this conditional (or 'A → p(B) = *n*') and 'B, given A, is probable' (or 'p(B,A) = *n*'). This distinction is unnecessary, according to a partial assertion account; and, we claimed, finds no support in ordinary usage.

In earlier chapters, we have seen that SP utterances function as truthconditional in contexts in which a group of speakers share a common base of relevant evidence. In such a context the notions of truth and falsity are associated with SP sentences in a way which matches the standard use of these notions - the use associated with the notion of a truth condition. So if the assumption that SP sentences are truthconditional is reflected in standard presentations of the probability calculus, we should expect this to be revealed in cases of mixed evidence. These might arise in several ways: the perspectives of people with different evidence might be run together; the perspectives of the same person at different times might be combined; or a hypothetical perspective might be joined to an actual one (or to a different hypothetical one). The inference from 'If A, then it is probable that B' and 'It is not probable that B' to 'Not A' is a case of the last kind; when 'It is probable that B' occurs as the consequent of this conditional, the evidence on which it rests includes the hypothesis that A. We have seen that in this case ordinary usage seems more easily reconciled with a partial assertion account than with a truthconditional one.[8] We should hope that this is true in cases of mixed evidence in general; or at least that there are none which can only be made sense of by a truthconditional account.

At any rate, we should expect the standard probability calculus to accurately reflect the use of SP sentences in contexts of shared and fixed evidence. In these cases the assumption that such sentences are

truthconditional should have led to nothing inexplicable by a partial assertion account. More precisely, the calculus should in these cases formalise the rules governing the use of SP sentences - in the way, for example, that the propositional calculus formalises the rules governing the truth-functional connectives (in their application to truth-functional clauses). Neither calculus need formalise *actual* usage, which doesn't always conform to these rules.

There is an important qualification, however. The numerical probability calculus enables us to make SP utterances which were not available to us before it was developed - any utterance of the form 'There is a probability *n* that A', for example. And it might seem that given that the whole numerical calculus has been developed under the assumption that there are real probabilities - properties or relations of some kind - such numerical SP sentences can only be made sense of under this assumption. If so, then it will surely be very hard to reject at least a 'theoretical construct' account of objective probability - for haven't such utterances become almost indispensable?

Not so much as might first appear, I think. It is important to keep in mind that we are talking about *single-case* probability here. A sentence such as 'There is a 0.023% chance that a radium atom will decay within a year' is very precise, numerically, but it is not an SP sentence. We seem to be much more reluctant to make precise numerical SP ascriptions than we are to make such statistical generalisations. It is common for SP ascriptions to be criticised for 'meaningless' precision.

To the extent that we do use numerical SP sentences, I think there is the following explanation, consistent with the partial assertion approach: the development of numerical probability has given us the means to express - and perhaps the means to hold - a very much greater range of effectively distinct degrees of partial belief than

was previously possible. This range is not only ordered, but also provided with a measure, by the association of probabilities with the real or rational numbers from 0 to 1. Note that we do not therefore need to claim that a single person can have more than a finite number of effectively distinct degrees of belief; any more than the use of real or rational numbers in the labelling of temperatures, say, requires thermometers capable of recording an infinite number of distinct temperatures. A numerical statement of probability, like a numerical statement of temperature, has a certain implicit vagueness. Hence in any given context there are only a finite number of effectively distinct such statements. It is irrelevant here whether probability is objective.[9]

It might be said that people often make numerical SP utterances without the understanding necessary to have the 'associated' disposition to (non-linguistic) behaviour - the understanding necessary to make a decision to accept certain corresponding betting odds and reject others, for example. Since it is this disposition which is supposed to indicate a person's possession of the corresponding partial belief, it would seem that having numerically-defined degrees of partial belief is a more difficult activity than the (sincere) utterance of numerical SP sentences. If so, then surely the primary account of SP utterances cannot be in terms of associated partial beliefs.

I have two answers to this. Firstly, I think it overestimates the extent to which people lack the behavioural associations of their SP utterances. It would be unusual for someone acquainted with the language of probability not to prefer a favourable outcome to depend on a proposition he takes to be more probable than on one he takes to be less probable. But secondly, and more importantly, what cases there are of this kind simply seem to exemplify a very common type of linguistic phenomenon: the ability of speakers to learn to use a sentence or

construction in a certain range of appropriate circumstances, without having grasped its full meaning and proper associations. Repetition of the utterances of figures of authority is a common kind of case. It occurs not only with utterances of apparently assertive form, but also, for example, with questions: a person may learn, say, that in the present climate the question, 'Is your monetary growth target compatible with a fall in the rate of inflation?' is a good one with which to try to embarrass a political opponent, even though he (the speaker) has no idea what a monetary growth target is (and therefore can hardly be said to wonder whether such a thing is compatible with a fall in the rate of inflation).

The partial assertion account characterises SP utterances in terms of their role in a certain kind of 'core' context. It is no objection to this account that such utterances have uses in other kinds of context, so long as these uses are in some way derived from the core use. These imitative cases clearly satisfy this condition.

If it is agreed - with the above qualifications with respect to cases of mixed evidence, numerical precision, and so on - that the probability calculus formalises the ordinary use of SP sentences, then a further question arises: why is it so? Why does the use of such sentences have the structure of (what we refer to as) the probability calculus, rather than some other structure?

For the partial assertion account, in view of the fact that it takes SP utterances to be, standardly, expressions of partial belief, essentially the same question arises at the level of belief. Why are our partial beliefs governed by constraints which are formalised in the probability calculus? Answering the question at this level will automatically answer it at the level of utterance. And at the level of belief, it seems to me that the question has largely been answered by

the subjectivists, in their development of the notion of coherence.[10] This is not to say that we arrange our beliefs coherently because we realise that we *should* do so; but rather that this notion enables us to understand what is wrong with an arrangement of partial beliefs which doesn't satisfy the probability calculus, and hence gives us the beginnings of a (more-or-less evolutionary) explanation of our development of constraints such that our beliefs do conform to the calculus, on the whole. However, it is not at all clear how the details of this explanation should go; and with the exception of a few remarks in the next chapter, I shall not try to provide them here.

DISPUTES ABOUT PROBABILITIES

Suppose I say, 'It is probable that A', and that you disagree, saying, 'No, you're wrong, it is not probable that A'. It is natural to think that we are disagreeing as to some objective state of affairs. But in Chapters 3 and 4 we saw that in view of the relational nature of such utterances, this natural interpretation of such exchanges can only be maintained if SP utterances depend on a sufficiently objective base of evidence, or in contexts in which all participants in fact share a base of evidence at a more subjective level. This observation led to a central difficulty for truthconditional accounts of SP utterance.

If we are to reject truthconditional accounts, however, then whatever alternative theory we propose should be able to account for such exchanges. What is the point of such behaviour, if not to settle some objective question?

A dispute about an objective question is settled when the participants come to share the same belief about the matter concerned. Analogously, the partial assertion account should claim, a dispute of the above kind is settled when the participants come to have the same

partial belief, about whatever it is whose probability is at issue. Now the degree of a person's partial belief that A (if any), is a function of the various dispositions he holds to infer such beliefs from certain evidential beliefs (i.e., dispositions corresponding to his accepted rules of inference to single-case probabilities), and of his relevant evidential beliefs themselves. So it is conceivable that an SP dispute could arise because the participants accepted different rules of inference. But since such a dispute would be impossible to settle unless appeal could be made to some common set of more basic rules of inference, this is not the interesting case. In general an SP dispute will arise from the participants' possession of different evidential beliefs, and will be settled when they come to have the same such beliefs.

The normal course of an SP dispute is for the participants to make explicit the evidence on which their SP claims rely. Ideally one or other participant will come to recognise that the other's evidence is superior in some respect, and will accordingly adopt a modified belief. In practice various things can go wrong, of course. Some piece of evidence may itself become contested; then this new dispute will need to be resolved before the original one can be. Or it may turn out that the participants have conflicting evidence, so that both are led to reject their original partial belief, but not to adopt any other.

On the whole, however, the benefits of increasing one's evidence seem to provide a plausible explanation of the existence of such dispute behaviour. It is not that people have these disputes because they are conscious of the resulting advantage, but rather that identifying the benefit of such behaviour seems to give us the basis of an explanation of its development as part of our linguistic activity. It enables us to say what purpose such behaviour serves.

Finally, note that objectivists are bound to accept that it is

better to base probability judgements on more evidence than on less. So they too ought to be prepared to explain the benefits of doing so. In other words, our account of the purpose of SP disputes does not rest on a claim that truthconditional theories can reject. It might be suggested that only a truthconditional account can explain *why* increasing one's evidence is beneficial; but we saw in Chapters 3 and 4 that the more defensible of such accounts cannot support this kind of explanatory load.

Notes

1. Cohen (1977), p. 30.
2. In the sense outlined in Chapter 7, pp. 7:14-15.
3. Ramsey (1978), p. 82.
4. ibid., p. 82.
5. Cohen (1977), p. 30.
6. Adams (1965), (1966).
7. Lewis (1976).
8. Though truthconditional accounts can also avoid the distinction between 'A → p(B) = *n*' and 'p(B,A) = *n*', which ordinary usage seems not to recognise, by making explicit the reference of probability statements to evidence - i.e., in effect, by treating all probabilities as conditional - and observing that when such a statement occurs as the consequent of a conditional, the evidence to which it refers includes the assumption of the antecedent.
9. Thus see Mellor (1971), Ch. 6; Mellor discusses this matter from an objectivist viewpoint.
10. See Kyburg and Smokler (1964), for an introduction to coherence.

9. A RETURN TO RATIONALITY.

One of our major concerns in earlier chapters was to distinguish the different ways in which most truthconditional accounts of single-case probability rely on a notion of rationality, and to argue that this dependence is unsatisfactory, on several grounds. Since then, in Chapters 6, 7 and 8, we have been presenting and defending an alternative to such truthconditional accounts. In this final chapter I want to look at rationality (as it relates to single-case probability) in the light of this alternative theory; and to argue that the partial assertion account escapes serious problems with respect to this notion. I doing so I shall try to exhibit some of the more general questions with which the present dispute about single-case probabilities - i.e., essentially, *1.5 - 1.8* - is connected.

In Chapters 3 and 4 we saw that one problem for truthconditional accounts of SP utterance, given that many of them depend on a single-case application of the notion of rationality, is that this notion is no more obviously explicable in truthconditional terms than the notion of probability itself. Obvious ways of attempting to specify truth conditions for a rationality ascription of the relevant type - for example, an utterance of the form, 'It is reasonable to have a high degree of partial belief that *q*' (where *q* is truthconditional) - seem either to amount to one of the ways of construing objective probability ascriptions, or to actually refer to a notion of probability. Yet it is difficult to see how someone who accepts the need to give non-trivial truth conditions for SP sentences, can consistently deny that similarly non-trivial truth conditions are needed for such corresponding rationality ascriptions.

Similarly, it would be odd to reject truthconditionality for SP utterances and yet to try to retain it for the corresponding ration-

ality ascriptions. For one thing, such a move would conflict with the widespread substitutability of 'It is reasonable to belief that ...' (or 'It is reasonable to be confident that ...') for 'It is probable that ...'. More importantly, the rational personalist programme seems to demonstrate that sentences of the form, 'It is reasonable to have a partial belief of degree *n* that *q*', if truthconditional themselves, are an adequate basis for a truthconditional account of SP sentences.

Thus the partial assertion account must associate such rationality ascriptions with something other than an effectively full belief that it is reasonable to have a certain partial belief. The simplest possibility is that a rationality ascription is associated with the same mental attitude as its corresponding SP utterance. Then 'It is reasonable to have a strong partial belief that *q*' (or 'It is reasonable to be confident that *q*') is characteristically an expression of a strong partial belief that *q*, just as 'It is probable that *q*' is. Other features of the context will presumably decide which utterance is the more appropriate.

One objection to this suggestion is the claim that it is quite conceivable - indeed quite common - for someone to believe it is reasonable to be confident that some state of affairs *q* obtains, without actually thinking that it is probable that *q*. And conversely, isn't it possible to think that *q* is highly probable, and yet to think it is quite unreasonable to be confident that *q*? (Thus, 'I'm almost sure the cat will be back, though there's really no good reason to think so').

I think this kind of claim is overrated. It is common in discussions of the nature of knowledge, where it is used in an attempt to identify a difference in mental state between someone who knows that *q* (or who thinks he does) and someone who merely believes that *q*. Thus to think that one knows is said to be to believe (and presumably to *think*

one believes) *and* to think this belief justified; whereas (it is claimed) it is quite possible to believe that *q* without thinking that one is justified in doing so.

This claim has a weak sense, in which it is indisputable, but also irrelevant: it is clearly conceivable for someone to be confident or certain that some state of affairs holds, without ever having heard of the notions of justification and rational belief (unless the state of affairs in question itself concerns these notions, of course). Such a person partially or fully believes whatever it is, but does not believe he is justified in doing so. He has no thoughts at all about justification. But this tells us nothing about the difference between probability and reasonable partial belief, or between full belief and knowledge.

In the strong sense, the claim is that it is possible for someone to be confident or certain that *q*, and yet to think that he is not justified in holding this belief. I doubt whether this is so. It seems to me that the cases which appear to be of this kind are better explained in other ways. Roughly speaking, we say that a belief is justified when we take it to instantiate some general kind, with respect to which we hold the general rule that (other things being equal) beliefs of this kind are justified. If we believe that few straying pets ever return, for example, we may operate the general rule such that if we learn that *x* is a pet who has just disappeared, we shall be disposed to say that we are justified in being confident that *x* will not return. (Though it seems that our actual rules are of much greater generality than this). This rule will go hand in hand with one which leads us to *be* confident that *x* will not return, when we learn that *x* is a pet who has disappeared. Generally speaking we shall apply these rules in a particular case unless we have some more specific rule which overrides them; if we believe that Tiddles always returns, for example, this will

take precedence over these more general rules, when it is Tiddles who disappears. However, it may happen that the more specific rule is one of which we are not conscious, a rule which we would find it difficult to put into words, and therefore to appeal to, if called on to justify the belief to which it leads. The most specific rule we can *appeal to* is the more general one - in this case the one which justifies the belief that Tiddles will not return. So we are inclined to acknowledge that this belief is justified, even though we don't hold it ourselves. Conversational constraints oblige us to recognise generally accepted rules, and we are unable or unwilling to argue the case for the more specific rule from which our actual belief - that Tiddles will return - results.

Moreover, note that although such circumstances separate 'I am confident that ...' from 'It is reasonable to be confident that ...', the construction 'It is probable that ...' stays with the latter expression. 'I am confident that Tiddles will return, though really it is probable he won't' is much more readily understood - in much the same way as 'I am confident that Tiddles will return, though it is really reasonable to be confident that he won't' - than 'It is probable that Tiddles will return, though it is reasonable to be confident that he won't'.

In other words, not only are cases in which a person's stated degree of confidence differs from what he claims to be the reasonable degree of confidence, of doubtful significance; but also such cases seem to be ones in which the stated degree of confidence differs from the claimed probability. The connection between 'It is reasonable to be confident that ...' and 'It is probable that ...' is hence reinforced by such cases. At the same time, the claim that 'It is probable that q' is characteristically an expression of a strong partial belief that q, is not significantly undermined. As we have already noted (p. 7:35),

it is possible to acknowledge situations in which such an utterance does not result from this partial belief, without rejecting the view that the central use - the use which determines the meaning of such an utterance - is the expression of such a partial belief. One such non-standard situation is the one we have described, in which a speaker's agreement that it is probable that *q* (or that it is reasonable to be confident that *q*) is a recognition of a generally accepted rule of inference, which for the speaker himself is overridden by some other rule; this latter rule being one that the speaker is not able or not prepared to *state*. 'I agree that it's probable that *q*, though I can't bring myself to be confident that *q*' has the often useful effect of avoiding a dispute as to whether it is probable that *q*, without deceiving a hearer as to the speaker's own attitude to *q*.

So the claim that the single-case rationality ascriptions associated with SP contexts are characteristically linked to the same partial beliefs as their corresponding SP utterances is more secure than it first appears. Moreover, it is important to bear in mind that unlike most truthconditional accounts of SP utterance, the partial assertion view does not depend on the existence of a notion of rational partial belief, and hence needs only to interpret whatever ordinary use there is for constructions such as 'It is reasonable to believe that ...'. This use may be very much less than would result from the powerful notion of rationality required by other accounts. And its interpretation seems more a matter of answering such questions as, 'In what circumstances is "It is reasonable to be confident that *q*" more appropriate that "It is probable that *q*"?', than of trying to explain the meaning of 'It is *reasonable* to believe that ...'.

The most important difference between 'It is reasonable to be confident that *q*' and 'It is probable that *q*' seems to be that the former is more defensive than the latter. The former is

appropriate when a speaker feels that his authority with respect to *q* is being challenged, and presents an expression of his partial belief that *q* which reminds a hearer that if he wishes to reject the belief, he must reject the evidence or the inferences on which it is based. By such an utterance a speaker hopes to indicate that he does not stand alone, that he follows widely-accepted beliefs and conventions, which must be challenged if the belief he expresses is to be disputed (though there is always an element of bluff: a speaker can also be challenged on the grounds that he is mistaken about these beliefs or conventions). However, note that 'It is reasonable to believe that ...' cannot be *paraphrased* by 'Widely-accepted conventions lead to the belief that ...', or by anything of the kind. The force of the expression, its connotations of approval according to some impersonal standard, depend on its not referring to any particular set of conventions.

This is all I have to say about the use of single-case rationality ascriptions in SP contexts. It seems to me that the partial assertion account is free to treat the role of such ascriptions as a minor one. Ordinary usage seems compatible with such a treatment. The emphasis usually placed on such a notion of rationality in discussions of single-case probability appears to be very largely a response to the needs of the various truthconditional accounts, rather than a reflection of an important ordinary use.

However, there is another way in which questions of rationality are relevant to accounts of single-case probability. In earlier chapters we referred to the various upward and downward rules of inference, to and from beliefs about single-case probabilities, which are a part of the different truthconditional theories of SP utterance. We saw that objectivist accounts - those such theories which rest on some notion of objective chance - tend to rely on rationality to secure their downward

inference (from a belief about the chance of some event or state of affairs, to the corresponding partial belief). Rational personalists, on the other hand - who construe SP utterances as statements about the rationality of certain partial beliefs - seem to obtain the downward rule in a more straightforward way; but only because they have already invoked rationality, in giving truth conditions to SP sentences.

All truthconditional accounts take the upward rules to license inferences from beliefs about observed relative frequencies, or whatever other evidence is held to be relevant to the values of single-case probabilities, to beliefs about such probabilities. We noted that such accounts have the option of taking these rules to be in two steps: a non-deductive inference from observed frequencies (say) to a universally quantified sentence about single-case probabilities; followed by the deductive instantiation of this universal quantifier, to yield an SP sentence.

For rational personalists, the conclusion of an upward inference is true in virtue of the rationality of the inference from the evidential beliefs in question to the corresponding partial belief - i.e., it is correct to infer from an evidential belief B that it is reasonable to have a partial belief of degree n that q, if and only if it is reasonable to infer from the belief B to a partial belief of degree n that q. We saw that this latter inference corresponds to the result of combining the objectivist's upward and downward inferences; and that it is unlikely that the rationality of the objectivist's downward inference can be established in isolation from that of the upward one - it seems to be the combined inference which must be shown to be rational, in the first place.

However, we saw that objectivists seem to have a strategy which involves denying that the required notion of rationality is applicable to the single-case, at least in such a way as to yield truthconditional

rationality ascriptions. This consists in treating the upward and downward rules as part of a package that speakers acquire, in learning to understand and apply the language of probability; and in claiming that this package can only be judged as a whole, and not with respect to some property of its individual applications. Single-case rationality ascriptions (to these inferences and to the beliefs which result from them), in so far as we have use for them, are thus to be regarded as non-truthconditional expressions of attitudes, as evaluations resulting from the speaker's general evaluative maxims (these in turn being related to his beliefs about the relative usefulness of various types of behaviour).

However, we saw that this strategy seems to leave no substance in the claim that SP utterances are truthconditional. In any case, it was unclear why someone who acknowledges the lack of truthconditionality of such ascriptions of rationality to degrees of confidence as do occur in ordinary usage, should want to insist on truthconditionality for SP utterances themselves. Indeed, we saw that an objectivist possesses all the ingredients for an objective standard of assessment applicable to single beliefs, in the formula: 'A partial belief that *q* is *reasonable* if and only if its degree corresponds to the objective chance that *q*'. True, if this is a *definition*, rather than a *derived* equivalence, then it will be no use in explaining our use of the downward rule; but the fact that what seems such an obvious equivalence - at least from the objectivist's point of view - has an inconvenient logical status, is a rather poor reason for denying that it is true. (This formula defines 'reasonable' for a partial belief; note that the 'reasonableness' of the inference from the full belief that there is a chance *n* that *q*, to the partial belief of degree *n* that *q*, follows according to the principle that it is always reasonable to adopt a reasonable belief - i.e. that an inference is reasonable if its conc-

lusion is, given its antecedent).

Thus although it is open to objectivists to argue that the upward and downward rules are linguistic 'facts of life', subject to evaluation only as a whole, and not with respect to their individual instances, this strategy is at best an incongruous addition to the objectivist view. It seems to leave the central feature of such a view - a concept of objective chance - not only as an unsupported and ineffectual notion, but as a positive embarrassment, given the ordinary association between ascriptions of probability and ascriptions of rationality to degrees of confidence.

In denying that SP utterances are truthconditional, a partial assertion account denies, in effect, that the objectivists' combined rule of inference has a significant division into upward and downward rules. It rejects the view that there is a half-way stage in the application of the combined rule, consisting in an effectively full belief about an objective chance. Since it is this stage which seems to conflict with the strategy of treating such rules of inference as linguistic 'facts of life', the strategy is much more plausible for a partial assertion theory than it is for objectivism.

We have seen that both objectivists and rational personalists are likely to take the upward rule of inference to be a two-stage one, the intermediate stage being a generalisation about single-case probabilities. A partial assertion account also seems likely to admit an intermediate stage at this point, at least if it is going to make sense of the ordinary use of statistical generalisations; but it is not clear what form this stage should be said to take. There seem to be at least two possibilities.

The alternative which most closely parallels the corresponding step in truthconditional accounts, is to associate the intermediate stage with the result of applying a universal quantifier to an SP

clause. In Chapter 7 (p. 7:31), we suggested that 'For all *x*, it is probable that *Fx*' is characteristically associated with a disposition to infer (or habit of inferring) a strong partial belief that *Fx*, with respect to any newly-encountered *x*. I think it is open to a partial assertion account to say that such dispositions are the intermediate stage in the inferences from which our partial beliefs result: certain beliefs about observed frequencies (and perhaps other kinds of evidence) lead us to adopt certain such dispositions, which in turn lead us to adopt the partial beliefs from which SP utterances result.

At first sight an advantage of this approach is that what follows the intermediate stage is deductive; or at least has the partial assertion view's substitute for deductive validity, in the fact that the disposition associated with 'For all *x*, it is probable that *Fx*' *guarantees* the partial belief associated with 'It is probable that *Fa*' (for anyone who is aware that *a* lies in the range of the quantifier 'For all *x*, ...'). Against this advantage, however, must be set the fact that a given case may instantiate several such dispositions, each supposedly leading to a different partial belief, unless the rules under which these dispositions are inferred from evidential beliefs can ensure that this situation will never arise. At some stage in the inference from evidential beliefs to partial beliefs, different, perhaps conflicting, evidential beliefs must be 'processed', to yield the resultant on which the final partial belief depends. If this is to happen at the step from evidential beliefs to dispositions of the above kind, the rules governing this step will need to be very much more complicated than at first appears. Each such rule will need to take into account all the relevant evidence, and the resulting dispositions will be liable to adjustment every time there is a change of evidence. Only in this way will it be guaranteed that the available evidence never gives rise to conflicting dispositions both applicable to a

single case.

It is true that these problems also arise for most truthconditional accounts of single-case probability, which must not allow different parts of a given body of evidence to give rise to conflicting probabilities for a single proposition. (The possible exceptions are accounts which make explicit the reference to evidence of SP statements, and which incorporate a rule to select as the basis for action and partial belief the particular statement which refers to all the available evidence). But it would be pleasing if a partial assertion account could do better. So it is worth trying to relocate the stage at which the 'processing' of conflicting evidence is said to take place.

One way to do so is to weaken the disposition held to be associated with the intermediate stage in the inference from evidential beliefs to partial beliefs, so that it becomes a disposition to infer (say) a strong partial belief that *Fa*, for suitable *a*, only if there is no conflicting disposition, over which the given one does not take precedence. This has the disadvantage that either the sentence 'For all *x*, it is probable that *Fx*' is no longer available to express the stronger disposition, where this might be appropriate, or there is no simple way of expressing this weaker disposition. In the former case there are likely to be problems with constructions involving the universal quantifier, such as the existential one; while in the latter case we would seem to lose the connection between the generalisations actually used in probabilistic contexts and the intermediate stage in the inference from evidential beliefs. So although perhaps neither difficulty is conclusive, I think it may be better not to construe the intermediate stage in terms of such dispositions, but rather in terms of beliefs about relative frequencies in appropriate classes.

The picture will then be roughly this: beliefs about observed relative frequencies, and perhaps about other things, lead us to adopt

beliefs about relative frequencies in certain general classes. We then follow a system of rules in using these general beliefs as a guide in the adoption of partial beliefs about individual cases. Certain conflicts of evidence can arise at the first stage; the percentage of B's in two samples of A's may differ, for example. But the conflicts which arise because a given individual belongs to more than one class with respect to which such general beliefs are held, are dealt with at the second stage. The rules we apply favour certain such beliefs at the expense of others, in a way which depends particularly on the relations between the various classes concerned. Thus if class A contains class B, 80% of A's are C's, and 20% of B's are C's, then other things being equal we will adopt a degree of belief of 0.2 rather than 0.8 that a given B is a C. But there is no guarantee that these rules decide every such conflict; in some cases conflicting general beliefs may simply cancel each other out, leaving us without guidance as to what partial belief we should adopt.

If such inferences to and from general beliefs about relative frequencies are to be regarded as linguistic 'facts of life', as habits we all acquire (more or less perfectly, perhaps) in learning to use the language of probability and related notions, then two kinds of question arise. There is a descriptive one: what are the actual forms of these various inferences? And an explanatory one: why do we have these forms of inference rather than others? (Note that just the same kind of questions also arise if we take the intermediate stage to involve the above dispositions, rather than such beliefs about relative frequencies).

I have very little to say about these questions. With respect to the first, it is worth noting that the descriptive approach is not ruled out by the existence of disputes (among statisticians, for example) as to the merits of various rules of inference involving

relative frequencies. If such a dispute can be settled, there must exist agreed constraints on such rules of inference; these constraints will be the linguistic 'facts of life'. If there are no such constraints there is no real dispute - only a confusion resulting from different linguistic practices making use of the same words. Assuming that there is such a common basis, however, it may turn out that different groups have exploited its potential in different ways. Statisticians may habitually employ complex derived rules of inference of which most speakers are unaware, for example.

Concerning the second question, the natural approach is to say that we have these forms of inference because they work, because they enable us to be more successful in our dealings with the world than we would be without them (and perhaps more successful than we would be with any alternative set of rules). But such an answer will need to be expressed with care, at several points. If it claims to offer an evolutionary account of the fact that we use such rules, then it ought to suggest a mechanism associated with the historical development of language, whereby innovations can be adopted or retained in virtue of their advantages. It is unclear how much scope such an account can, and should, allow for deliberate additions to linguistic practice by speakers themselves.

For another thing, such an approach should presumably be able to establish whatever claim it makes for the usefulness of our existing rules without itself relying on these rules - particularly if the claim is that these rules are more successful than alternative sets. Within this constraint, it would be possible in principle to examine a large class of actual uses of these inferences, and to show not only what contribution our use of them makes to the outcome of each case, but also, perhaps, what would have been the outcome had other rules of inference been employed; though here it might turn out that some such counter-

factual claims rest on generalisations which cannot be justified without use of the inferences whose advantages we are trying to establish. If a plausible evolutionary mechanism can be exhibited, then the fact that we have developed our actual rules of inference may be evidence that these rules have 'worked' in the class of actual past instances of their use. But if such conclusions are to be generalised to future cases, or even to hypothetical past ones, an inductive inference will be required, of the same type as the inference from observed to general relative frequencies.

An interesting question is whether if the inductive step is taken for granted, it is possible to provide any stronger justification for the inference from beliefs about general relative frequencies to single-case partial beliefs; whether it is possible to go further than an argument of the form, 'This type of inference has been useful in the past, so it will be useful in the future', to provide a justification for such inferences which is applicable to a single instance.

At first sight the justification may seem obvious: if I believe that 50% of A's are B's, then if I am in the habit of adopting a partial belief of some degree n less, say, than 0.5 that a newly-encountered A is a B, my corresponding choice of odds will lead to my losing money if I continually bet that the A's I encounter are B's; or at least so I should believe, given my belief about the relative frequency of B's among A's. This argument is deceptive, however. From *(i)* '50% of A's are B's', what follows is *(ii)* 'A person who agrees to a total stake s at betting quotient n on a bet that a is a B with respect to *every* a in A, will lose an amount $Ns(0.5 - n)$, where N is the total number of A's'. It is not clear what relevance *(ii)* has for someone who can be certain he will not be betting on more than a small number of A's. At best it seems to show that if a person has no idea how often he will want to bet that an A is a B, at a given stake, if he

wants a consistent policy for all such cases, and if he wants to avoid admitting as a possible situation any case which he can tell in advance would result in a loss for him, he must adopt the policy of setting betting quotients equal to the proportion of A's which are B's. Moreover, if a person accepts this argument for the general betting policy, it is not clear that it doesn't bypass the usual connections with partial beliefs. 'I see the argument for choosing a betting quotient of 0.5 that this A will be a B', such a person might say, 'But why should I have a partial belief of the same degree that it will be so; why should I have any particular partial belief about it?'. There is thus a danger that the argument operates at the wrong level, and justifies the wrong thing. The problem arises from the attempt to locate a notion of justification applicable to the inference to an individual partial belief, and hence to the individual partial belief itself; a notion such that a rational agent can be said to adopt such a partial belief in virtue of perceiving it to be *justified*. We have suggested instead that agents adopt such beliefs in virtue of their possession of certain habits of inference; that these habits can be justified only as a whole, and not with respect to their individual applications; and that in so far as the individual beliefs which result from these habits are said to be justified (or 'reasonable', or whatever), this is a specialised expression of the belief concerned, and therefore itself a result of the speaker's possession of the relevant habit of inference.

In any case, it is these kinds of questions which provide the partial assertion account's closest approach to the issues of rationality which, as we have seen, are so important for truthconditional accounts of single-case probability. We can see that they are far less central to the partial assertion view than similar issues are to the latter accounts. It seems to me to be an advantage of this view that it

separates these questions concerning the origins and advantages of certain linguistic practices, from essentially descriptive questions about these practices, such as we raised in Chapter 1 (in the form of *1.5 - 1.8*).

If the intermediate step in a typical inference from evidential beliefs to a partial belief is a belief about relative frequencies, there are important questions to do with the precise form of this intermediate belief. For example, is it concerned with a relative frequency in an *actual* class, or in some sort of ideal or hypothetical class? And is it a limiting frequency, or a simple class ratio?

I don't want to try to answer these questions here. However it seems to me that freed of the task of providing truth conditions for SP sentences, the actual class ratio alternative is more plausible than it has usually appeared. For example, the most common objection to the use of actual frequencies in accounts of probability is that the proportion of actual cases may not equal the chance. Thus a coin which is to be tossed once and then destroyed has a chance of 1/2 of landing heads, even though it does so on either none or all of the occasions on which it is tossed. But on the partial assertion view the utterance, 'There is a chance of 1/2 that this coin will land heads on the coming toss' does not *state* any class ratio; it merely expresses a partial belief, held in virtue of the speaker's belief that in some relevant class of tosses to which he takes this one to belong, 50% result in heads. It is true that the speaker wouldn't be justified in using this utterance - that is, wouldn't have the associated partial belief - according to the partial assertion view, if he believed he knew the outcome of this particular toss already; but ordinary usage wouldn't have him do so. When a speaker's belief about an actual class ratio conflicts with what other beliefs would lead him to expect, the former belief takes precedence in determining his partial beliefs with respect to members of

that class, and hence his SP utterances (unless the latter beliefs lead him to question the former one, as when our belief that a coin is unbiased leads us to doubt someone's claim to have tossed it heads fifty times in a row).

We have already noted that whatever mental attitude is said to form the intermediate stage in the inference to partial beliefs, it ought to be such as to be plausibly associated with the types of utterance which appear to characterise this stage; that is, with the various forms of probabilistic generalisation. As in the SP case, there is a certain amount of scope for claiming that ordinary usage has come to embody, to some extent, the assumption that probability is some kind of entity, or property. Even so, it is not clear whether an actual class ratio account can meet this condition.

An advantage of the actual class ratio, I think, is that since it does not require the notion of a limit, it does not require that individuals be ordered in any way. The notion of a limiting frequency seems an unnecessary burden, taken over from single-case frequency accounts.

However, there is a disadvantage of the actual class ratio account, from our point of view, which concerns a limit of another kind. The limiting case of a belief that *n*% of A's are B's, as *n* approaches 100, is a belief that all A's are B's. In Chapter 7 we took the mental attitude characteristically associated with the utterance 'All A's are B's' to be a disposition to infer that *a* is a B, with respect to any newly-encountered *a*, and not simply a belief that the relative frequency of B's in the class of A's is 100%. So if we now claim that the intermediate stage in a typical inference to a partial belief is a belief about relative frequencies, we admit a kind of discontinuity in the limiting case. This appears to be a violation of the limiting case constraint we mentioned in Chapter 1 (p. 1:10).

I am not going to attempt to settle this issue here. For one thing, it is by no means clear what the real issues are for an account of general-case probability. The subject seems to require the kind of analysis we have tried to provide for the single case, in Chapter 1. However, I think we have taken the cases in the correct order. Generalisations would be irrelevant if it were not for their application to individual cases, and the study of their forms should hence be based on a proper understanding of the associated single case. True, it might turn out that a proposed account of the single case could be shown to be unacceptable, on the grounds that it could not be linked to any satisfactory account of the general case; but we have found no reason to think that the partial assertion theory is in this position.

Bibliographical Notes

On the view that rationality is a characteristic of habits of inference, see the bibliographical notes to Chapter 4 (p. 4:19).

A view according to which probability judgements rely on beliefs about relative frequencies has been developed extensively by H. E. Kyburg, particularly in his (1961) and (1974). However, Kyburg takes the probability of a statement *s* to be a logical relation between *s* and a body of such beliefs. Subject to the qualifications in the bibliographical notes to Chapter 2 (pp. 2:20-21), his account thus falls into our category *2.7* (and I think that some of our objections to truth-conditional accounts thus apply to it). Nevertheless, I think Kyburg's theory gives a good indication of the kind of account of probabilistic generalisation which might accompany a partial assertion view of the single case.

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