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SCIENTIFIC REASONING-FROM POPPER TO BAYES

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Reasoning and Probability

The major problem in the philosophy of science is to establish when scientific theories should be accepted or discarded in the light of evidence obtained. The word "probable"often arises in this context: has a hypothesis become more or less probable in the light of certain new evidence? The same word is also used about events in the physical world: how probable is it that a spun coin will come down heads, that the die will land showing a six, that it will rain tomorrow, and so on?

What exactly is the connection between these two notions of *subjective* and *physical* probability? We know how to attach numbers to many physical probabilities, and there is an elaborate mathematical apparatus for dealing with them. Yet somehow it seems absurd to link this to subjective probabilities. We are satisfied that the probability of an ordinary coin coming down heads on being spun is exactly 0.5, but how could we ever establish that the probability of the theory of evolution, say, was some number like 0.95? There must be a link of some kind, however, or otherwise why should we be so content to use the same vocabulary for both?

The Principle of Indifference

A link that is often invoked is that of the "Principle of Indifference". It is most useful in calculating physical probabilities, and tacitly underlies the sort of probability calculations we are taught in our schooldays. We have no reason to suppose any difference between one side of a coin and another, so the probability of either side landing uppermost is 1/2. The six sides of a die seem all the same, so the probability of throwing any one of them is 1/6. If we know that a box contains a mixture of white and coloured balls, we guess initially that the probability of drawing a white one out is 1/2.

That last example shows how we can slip easily from a physical principle of Indifference to a subjective one. If there are n hypotheses to choose from, the probability of any one of them being true is 1/n. That seems an excellent conclusion at first sight, but already our suspicions might be aroused. Suppose we had been told instead that the box contained white, red and blue balls? The probability of selecting a white one, according to the Principle, drops to 1/3. Yet the situation could be exactly the same: the probabilities seem to change rather mysteriously depending on just how the state of affairs is described.

Supposing that odd little matter could be resolved, what does the Principle of Indifference tell us about the probabilities of scientific hypotheses? It is useful to introduce here the philosopher Nelson Goodman's "Grue Paradox"(See Goodman, 1954). This begins with the statement, "All emeralds are green", which we take to be a scientific hypothesis, confirmed by the discovery of green emeralds, falsified by the discovery of any of a different colour. (To make this work, we must pretend that being green is not a *defining* property of an emerald: we recognise an object as an emerald by some method which has nothing to do with its colour.)

The property "grue" is now defined as "green until midnight on the 31st December 1999, and blue thereafter". Clearly, until the date mentioned in the definition, the discovery of a green emerald supports the hypothesis "All emeralds are grue" just as well as "All emeralds are green". There are any number of other dates that could be used to define other properties, and thus an infinite number of hypotheses that are confirmed by any observation. Following the Principle of Indifference, we can only conclude that the probability of any scientific hypothesis is zero, since we can concoct any number of alternatives. Thus "Every action has an equal and opposite reaction until 14th March 2046 and no reaction thereafter".

The Logic of Scientific Discovery

This is the title of a work by the most influential philosopher of science, Karl Popper. It was first published in German (*Logik der Forschung*) in 1934. Its English translation did not appear until 1959, and underwent many revisions and additions by its author. I am using the sixth revised impression of 1972.

In the view of many practising scientists, if not philosophers, Popper's achievement in this work was to lay to rest the terrible problem *of induction*, which had haunted scientists' worst nightmares for centuries. How can we know *for certain* that the sun will rise tomorrow? How can we be sure that the law of gravitation will continue to hold? No one had ever succeeded in providing a *logical* reason for these beliefs. If there was a "Principle of Induction", which told us how to reason from like events in the past to like events in the future, how could we be certain of the truth of that principle? Only, it seems, by induction itself: a vicious regress seems to lurk behind the problem of induction.

Popper made the problem disappear in a puff of smoke. There is no such thing as induction, he claimed: we can never logically justify a scientific hypothesis, but only *falsify* one that has been put forward. Newton's law of gravitation failed to predict irregularities in the motion of the planet Mercury, and was thus falsified. Einstein's law did predict them, and is thus to be preferred.

Where hypotheses come from in the first place is a matter for psychology rather than logic. It does not matter - intuition or guesswork will do for formulating hypotheses. It is up to scientists to try to falsify them afterwards. Theories should always be subjected to the most rigorous tests in the search for falsification. Thus

Newton's gravitation was tested in an extreme condition: on the planet nearest to the Sun, where the gravitational field is much stronger than elsewhere in the Solar System.

Theories which make surprising predictions are also to be preferred, for those predictions provide ways for them to be falsified. Einstein's law of gravitation predicted that light rays passing near the sun would be deflected by an amount different from Newton's law, but it passed the test.

Unfortunately, Popper was under the spell of the Principle of Indifference. If the prior probability of any theory is zero, what is the point of testing it? Why should surprising predictions be preferred? Nothing we can do will make the theory other than quite impossible, according to that Principle. A good deal of *The Logic of Scientific Discovery* is concerned with an analysis of probability, and various attempts at a theory of *corroboration*, which would make some theories preferable to others even if they were all impossible.

None of these ideas could be made to work. Although they seemed to reflect the scientist's instinctive way of working, Popper's views came to resemble ethical pronouncements on the methods of science, rather than logical ones. Although Popper as a young man was, in the words of A. J. Ayer, "still tolerant of criticism"(1977, p164) he later became quite immune to it. Maybe the failure to come to terms with probability was the reason for this.

There are also some other problems with Popper's philosophy, not directly related to subjective probability. One of these is connected with the well-known "raven paradox" of Hempel (*Vide* Hempel 1965). If two hypotheses are logically equivalent, then confirmation of one must equally be confirmation of the other, one would imagine. But consider the hypothesis "All ravens are black". (As in the case of the green emeralds above, it must be assumed that being black is not a defining property of a raven.) This hypothesis is logically equivalent to "All not-black things are not-ravens", which should mean that my computer mouse, for instance, which is grey, and thus not-black, and is certainly a not-raven, is a good test of the hypothesis "All ravens are black". Certainly paradoxical, though not fatally so: a smooth tongued orator might be able to convince one that grey computer mice do indeed confirm the hypothesis that all ravens are black.

Much more serious is the problem of whether it is *ever* possible to falsify any hypothesis. An excellent example of this is pointed out by Feyerabend (1975). It seems perfectly obvious to us nowadays that lunar eclipses are caused by the earth coming between the moon and the sun. How could anyone have ever doubted this? Some early Greek thinkers did, and not for any foolish reason. They observed that occasionally a lunar eclipse can be seen just around the time of sunset, when both the moon and the sun are visible in the sky together. There could hardly be more convincing evidence against the theory. Of course now we explain away the phenomenon with theory of refraction in the atmosphere. Objects which are in fact below the horizon still appear to be above it, thanks to the bending of light rays through the atmosphere.

This kind of situation poses a really severe problem for Popper's programme. Given evidence which is inconsistent with a theory, we may choose to reject the evidence and not the theory. It is always possible to do this, and sometimes it may be the right thing to do.

Popper tried to explain this by invoking a certain amount of *convention* or *intersubjectivity* in the scientific method. Refutations are accepted or rejected according the general agreement of scientists. This comes inevitably back to scientific method as ethics rather than reasoning.

The solution to this conundrum lies, as we shall see, in a different way of dealing with subjective probabilities. But first it is necessary to discredit once and for all the Principle of Indifference.

Will the Sun Rise Tomorrow Morning?

Suppose that box contains an unknown mixture of white and black balls. Ball after ball is removed from the box (whose contents are invisible from outside), and they all turn out to be white. What is the probability that the next one will also be white? If there were n balls in the box to begin with, one can imagine all the possible sequences of white ones and black ones. In each of the n places in the sequence, there is a choice of putting either a white or a black ball. This gives 2^n possible sequences altogether. The Principle of Indifference suggests that we regard each of these as equally likely to be the one in the box. In that case, clearly, the probability of drawing out black ball from the box after no matter how many white ones is precisely 1/2, because in "constructing" any possible sequence there was always a free choice of putting a white or a black ball in the next place. Note that the probability of the colour of the next ball does not depend on n.

This last feature is important, since it enables us to compare the risings of the sun on successive mornings to the drawing of balls from the box. Each day is a drawing of a ball: a white ball is analogous to the sun rising, a black ball to the sun not rising. We have no idea how many days are going to be in the sequence altogether, but this does not matter to the probabilities for tomorrow. But the result is a bit disappointing: surely we think that the chance of the sun rising tomorrow morning is considerably more that 50:50.

Going back to the balls in the box, the situation we would like to have is one where the more white balls are drawn, the greater is the probability of the next one being white. A formula like the following would be agreeable. On the very first draw the probability of a white ball emerging should indeed be 1/2. If the first ball is white, however, suppose the probability of a second white one rose to 2/3, adding one to top and bottom of the fraction. The probability of a third white ball would be 3/4, and in general, if the sequence has so far consisted of r white balls, the probability of a subsequent white one will be $(r+1)/(r+2)$.

An agreeable result, certainly, but the Principle of Indifference seems to have been left behind. However, the great French mathematician Pierre Simon de Laplace showed in an essay published in 1820 that the (r + 1)/(r + 2) formula can be reconciled with the Principle of Indifference. It turns out that if we proclaim our indifference not to a particular sequence, but to the *number of white and black balls* in a sequence, then the (r + 1)/(r + 2) result follows. There is only one sequence which is all white, but n containing just one black ball. This means that with with the new kind of indifference, any particular sequence containing just one black ball has only 1/nth the probability of being in the box as the all white sequence. The all white and the all black sequences are the most likely to be in the box, the probability of each being 1/(n + 1), as there are n + 1 different possible numbers of white or black balls in asequence, ranging from 0 to n. Laplace used his formula to calculate that the probability of the sun rising the day after his calculation was 1826214/1826215, impressively close to unity, even though it looks as if Laplace was using the relatively recent date of somewhere in 4004 BC, calculated by biblical scholars, for the first sunrise of all.

The proof of the relation between (r + 1)/(r + 2) and indifference to number requires rather more arithmetic than anything else in this paper, so I omit it here. Those who are not prepared to take it on trust can apply to the author.

The logician John Venn called the (r + 1)/(r + 2) formula "Laplace's Rule of Succession"(see Venn 1888), and in criticising it, pointed out how many philosophers had been seduced by it, including de Morgan and Jevons. In more recent times we can cite Rudolf Carnap as one who has succumbed to its charms (see Carnap 1950).

The reason for this may be the relative complexity of the derivation of the Rule from the axioms of probability (the argument is set out in full in Howson and Urbach, 1993, pp 55 -58). This makes it look far more impressive. In my simplified argument above, the Rule was produced from thin air to begin with, then related to sequences, a much simpler procedure which also makes clearer the sheer arbitrariness with which the Principle of Indifference is applied. If the result of being indifferent to some property is not to our satisfaction, then we cast around for something else to be indifferent to until we come up with a formula we approve of.

The Rule of Succession gives a rather strange picture of induction, too. Is it only because the sun has risen so many times in the past that we feel certain it will rise tomorrow? Surely our knowledge of the way the solar system works has something to do with our confidence.

The Dutch Book Theorem

I can't resist using that rather charming name for what should more properly called the Ramsey-de Finetti Theorem, after its two independent discoverers (see Howson and Urbach, 1993, p 79). No doubt it derives from times of conflict between England and the Netherlands, like "Dutch treat" and "Dutch courage": a "Dutch book" is a series of bets on which the foolish punter who accepts them is bound to lose overall. The reason for the name will become clear as we proceed.

What the theorem shows is that *rational* bets must observe the axioms of probability. It provides the proper link between objective and subjective probability, which the Principle of Indifference failed to do. The theorem will be proved first, and then its implications considered.

Consider the process of making a casual bet. Person A says to person B, "I bet you 50p that it'll rain on our day off." B is not pessimistic, and accepts: if it rains, B pays A 50p, if it does not, A pays B the same amount. In a more formal situation, A may pay a bookmaker B a sum of money **pS** in return for a promise by B to pay a sum **S** if a certain horse wins a certain race. Bets of all kinds may be represented in a canonical form as follows:

h	payoff to A
T	S-pS
F	-pS

Here A is betting on a *hypothesis* **h**: that it rains on a certain day, or that a certain horse wins a certain race. If the hypothesis turns out to be true (T), then A receives a sum **S** less the initial amount **pS** paid out. If **h** is false (F), then A loses the initial **pS**: this becomes a negative payoff. In the case of the 50p bet, no money changed hands originally, but clearly the result is the same as an initial payment by A to B of 50p, with a return (**S**) of £1 should it rain on the day. The value of **p** in that case is taken to be 1/2, and such casual bets will surely be judged "fair" if the actual chance of rain is about 50:50, and neither A nor B has a particular advantage.

Of course a judgement about the value of **p** is a matter of belief, and one is entitled to one's own beliefs. All the same, for a rational person, even beliefs must together be *logically consistent.* This means, for one thing, that the value of **p** in the above *payoff matrix* must lie between 0 and 1. For suppose **p**<0: in that case, A's payoff is positive whether **h** is true or false, which clearly is not fair to B. If **p**>1, then A's payoff is negative whether **h** is true or false: now B has the unfair advantage.

Suppose now that A simultaneously makes *two* bets about **h**: one *on* **h**, as above, with what we shall call *betting quotient* **p**, and another *against* **h** with betting quotient **q**. The second bet will have the following payoff matrix:

h	payoff to A
T	-qS
F	S-qS

For this time, A pays out a sum **qS** in the hope of receiving **S** if **h** is *not* true. When the two bets are summed, we get the following situation:

h	**payoff to A**
T	**(1 -p -q)S**
F	**(1 -p -q)S**

Both lines are the same in this case, so at first sight it seems impossible to avoid the same positive or negative payoff to A, with unfairness to one or the other party in either case. There is just one way to avoid this: to make sure that **(1-p-q)** is zero, so that neither party gets anything whatever the outcome. This happens when **q** is equal to **1-p**, and reminds us of the familiar result that if the probability of an *event* **h** is **p**, then the probability of **not-h** is **1-p**.

A further example. Suppose A bets on a hypothesis **a** with betting quotient **p**, and on another hypothesis **b** with betting quotient **q**. The combined results would be as follows:

a	**b**	**payoff to A**
T	T	S-pS + S-qS = 2S-pS-qS
T	F	S-pS-qS = S-(p+q)S
F	T	-pS + S-qS = S-(p+q)S
F	F	-pS-qS = -(p+q)S

Except for the first line, where **a** and **b** are both true, there is a clear pattern of a bet with betting quotient **(p+q)**, which is won if either or **a** or **b** is true, and lost if they are both false. Now there are many cases where two hypotheses are *mutually exclusive*: they cannot both be true. Take, for example, "this ink is blue" and "this ink is red". These can of course be both false: the ink may be green. It appears that if there are two mutually exclusive hypotheses, when we can ignore the top line of the table, and if the betting quotients on them are **p** and **q**, then the rational betting quotient on their disjunction is **(p+q)**.

This needs a check to be sure, however. If somebody bets simultaneously both *on* and *against* **a or b**, with the same sum **S** involved, the only fair payoff can be zero. Suppose the bet on has betting quotient **(p+q)**, and that against has quotient **r**. The results of the combined bet would be as follows:

a or b	**payoff**
T	S-(p+q)S-rS = S(1-p-q-r)
F	-(p+q)S + 1-rS = S(1-p-q-r)

The quantity in brackets can only be zero if **r** is **1-(p+q)**, confirming that the only rational betting quotient on **a or b** is **(p+q)**.

After successfully dealing with **a or b**, at least in the mutual exclusion case, we might wonder about **a and b**. What should the rational betting quotient be, on *both* hypotheses turning out true? The **a or b** case looks as if it provide a clue at first sight, for when **a or b** is false, **not-a and not-b** must be true. Clearly, then, the rational betting quotient on **not-a and not-b** must be **1-(p+q)**. On the other hand, we have here rather a special case, as **a** and **b** are not *independent* of each other: they cannot both be true.

Suppose that the rational betting quotient on **a and b** is **r**. If we make a bet with **S** as unity on **a and b**, the result will be as follows:

a	b	payoff
T	T	1-r
T	F	-r
F	T	-r
F	F	-r

Suppose now that we simultaneously make a bet *against* **a**, choosing **S** in such a way that the net payoff will be zero when **a** is false. The net result will be a kind of bet on **b** only, which comes into effect when **a** is true.

If the rational betting quotient against **a** is **(1-p)**, then we stand to gain **pS** when **a** is false. We choose **S** so that **-r+pS=0**, or in other words so that **S=r/p**. The complete table works out as follows:

a	b	a and b	not-a	total payoff
T	T	1-r	-(1-p)r/p	1-r/p
T	F	-r	-(1-p)r/p	-r/p
F	T	-r	p.r/p	0
F	F	-r	p.r/p	0

The appearance of the total payoff is certainly that of a bet on **b**. If we use **q** for the rational betting quotient on **b** as before, then the table shows that **q=r/p**, or **r=pq**. This is familiar from the realm of objective probability. The probability of throwing a four with one die *and* with a second is 1/6 x 1/6, or 1/36, we all remember.

However, this result is only so when we are considering *independent* events, like the throwing of two dice. Not all events or hypotheses are independent: for example, the probability that a certain person is a Catholic is much higher *given* that he or she is British, say, rather than Japanese. Similarly with a die: given that an even number has been thrown, the chance that it is four is now 1/3 rather than 1/6, since the die has just three even numbered faces.

The table above shows a situation like this: the bet on **b** only takes place when **a** is true, and **a** and **b** may not be independent. Some extra notation is useful at this point: let us use **p(a)** and **p(b)** to denote the rational betting quotients on **a** and **b** respectively, and **p(b/a)** to denote the betting quotient on **b** *given* **a**. The results of the above table can now be expressed as:

p(a and b) = p(a) x p(b/a)

An exceedingly similar table, so similar that I can safely leave readers to work it out for themselves, would result if we balanced the loss on **a and b** with a bet against **b** instead of **a**. This time we would find:

p(a and b) = p(b) x p(a/b)

Using the same notation for the case of **a or b**, which was worked out before, gives:

p(a or b) = p(a) + p(b), provided a and b are mutually exclusive

Of course in the previous discussion we have generally put **q** for **p(b)**, and **p** for **p(a)**. The latter usage should not confuse, since in the new notation, **p** is always followed by some item in brackets.

It has already been noted that in the case where **a** and **b** are mutually exclusive, **p(not-a and not-b)=1-(p(a)+p(b))**. Only the notation has been reformed in this formula. According to what has been discovered, it is also the case that

p(not-a and not-b) = p(not-a) x p(not-b/not-a)

Going back to the previous **p**, **q** notation for simplicity, we can combine these two formulae and conclude

p(not-b/not-a) = (1-p-q)/(1-p)

This conclusion can be checked by the method that has now been perfected, that of cunningly chosen simultaneous bets. We begin with a bet *on* **not-a and bot-b**, choosing **S** as unity, for simplicity:

a	b	payoff
T	F	-(1-p-q)
F	T	-(1-p-q)
F	F	p + q

Now we make a bet *on* **a**, so that the net payoff when **a** is true is zero. In other words, **S** must be chosen so that:

-(1-p-q) + S(1-p) = 0

This equation gives **S= (1-p-q)/(1-p)**, and the table of all the bets is:

a	b	not-a and not-b	a	payoff
T	F	-(1-p-q)	(1-p).(1-p-q)/(1-p)	0
F	T	-(1-p-q)	-p.(1-p-q)/(1-p)	-(1-p-q)/(1-p)
F	F	p+q	-p.(1-p-q)/(1-p)	1-(1-p-q)/(1-p)

The last column shows the betting quotient to be exactly what had been expected.

Actually this kind of cross-checking is otiose: all the formulae of probability theory may be derived as theorems from the following four axioms:

Axiom 1 $p(x) \geq 0$

Axiom 2 p(x and not-x) = 1

Axiom 3 p(x or y) = p(x) + p(y), if x and y are mutually exclusive

Axiom 4 p(x and y) = p(x) x p(y/x)

Here x and y are any events.

Once these axioms are established, nothing more is needed. The arguments above have dealt with them all except Axiom 2, which is very straightforward. "x and not-x" is an example of a *tautology*, something which always turns out true. Clearly anyone who bet on such a thing with a betting quotient **p** less than 1 would always collect the sum **S(1-p)**, which would be unfair. If **p** was greater than 1, there would inevitably be a loss, equally unfair.

The Dutch Book Theorem has been proved: we have shown that rational betting quotients must obey the axioms of probability. This establishes a proper connection between objective and subjective probability, one which does not lead us into the absurdities of the Principle of Indifference or the infamous Rule of Succession. A hugely important consequence of this liberation is that we are not led to the conclusion that the probability of any scientific theory must be zero, since it is but one of an infinite number of different possible theories. Before we consider just what this means, it is necessary to explore one theorem of probability in more detail.

Bayes' Theorem

It has already been shown that **p(a and b) = p(a) x p(b/a)** and that **p(a and b) = p(b) x p(a/b)**. If we now put **h** in place of **a** and **e** in place of **b**, we can combine the two formulae and obtain:

p(e) x p(h/e) = p(h) x p(e/h), or equivalently, **p(h/e) = p(h) x p(e/h)/p(e)**

This, essentially, is all of Bayes' Theorem, named after the 18th century clergyman and mathematician Thomas Bayes. The change of letters was merely to have **h** as a mnemonic for *hypothesis*, and **e** for *evidence*. Let us apply the theorem to Popper's philosophy of science.

From Popper to Bayes

The basis in reasoning for Popper's claims about the conduct of science now falls into place. Let us examine Bayer' formula in detail:

p(h/e) = p(h) x p(e/h)/p(e)

To say that evidence falsifies a theory means that **p(e/h)** is zero: given the hypothesis, the probability of the evidence zero. As mentioned above, we are no longer forced into the position of saying that a hypothesis has an initial probability of zero: **p(h)** can be guessed at in the light of our experience or *background knowledge*. The equation above shows that provided **p(e)** is not zero, but **p(e/h)** is, then **p(h/e)** does become zero, whatever **p(h)** was. The hypothesis has been falsified. If **p(e/h)** is not zero, then **p(h/e)** just *changes* according to the evidence **e**. Whatever our initial guess about the probability of the hypothesis, further experience increases or decreases it until some equilibrium is reached.

If certain evidence is *predicted* by a hypothesis, then **p(e/h)** is unity. It is clear why we should test the most surprising predictions of a theory: to say that evidence is *surprising* means that **p(e)** is low, and that in turn means that **p(h/e)** will increase a good deal if the prediction turns out to be the case, according to the equation above.

Now that Popper's general points have been justified, let us turn to the paradox of the ravens. Remember the problem is to find the reason why the discovery of a black raven tends to confirm the theory that all ravens are black, while the discovery of a non-black non-raven seems to make no difference.

We will use the expression **RB** to mean a black raven, and **-R-B** to mean a non-black non-raven. Bayes' theorem tells us that

p(h/RB) = p(h) x p(RB/h)/p(RB) and also that **p(h/-R-B) = p(h) x p(-R-B/h)/p(-R-B)**

Consider the term **p(RB/h)** This must be equal to **p(R/h)**, since given the hypothesis "all ravens are black", anything that is a raven *must* be black. Furthermore, **p(R/h)** is itself equal to **p(R)**, as the probability of some object being a raven is quite independent of the hypothesis. The first equation above simplifies to

p(h/RB) = p(h) x p(R)/p(RB)

The change in the probability of the hypothesis on the discovery of a black raven depends on how surprising that evidence is. If this was new hypothesis, it might be quite surprising, so **p(RB)** would be low, and the probability of the hypothesis would be increased quite a lot. Now consider the term **p(-R-B/h)**. The hypothesis says that anything that is not black cannot be a raven, so this term is equal to **p(-B/h)**. Being not black is itself independent of the hypothesis, so the term is equal to **p(-B)**, and the second equation above becomes

$$p(h/-R-B) = p(h) \times p(-B)/p(-R-B)$$

Since most things in the universe are not ravens, **p(-B)** is virtually equal to **p(-R-B)**, so clearly the discovery of a non-black non-raven makes virtually no difference to the probability of the hypothesis, as required.

We now turn to the problem of the lunar eclipse at sunset. Here **h** is the hypothesis that lunar eclipses are caused by the earth coming between the sun and the moon, while **e** is evidence that at sunset eclipses the earth is *not* between the sun and the moon. We can take **-e** to be evidence that the earth only *appears* not to be in between: there is some explanation like refraction for this. Bayes' theorem gives:

$$p(h/e) = p(h) \times p(e/h)/p(e) \text{ and also } p(h/-e) = p(h) \times p(-e/h)/p(-e)$$

In the first equation, if **p(e)** is taken not to be unity, then since **p(e/h)** most certainly is zero, the probability of the hypothesis drops straight to nothing. On the other hand, if we have accepted the theory of refraction, then **p(e)** itself becomes zero, and the first equation tells us nothing. In that case **p(-e)** becomes one, and since **p(-e/h)** must also be one (if the hypothesis is true, it can only be an illusion that the earth is not between the sun and the moon), and the second equation tells us that the probability of the hypothesis is unchanged by the evidence.

Conclusion

I think it has been clearly shown that given the validity of the Dutch Book

Theorem, then Bayes can come to the rescue of Popper' s programme. In a future paper, I hope to apply these results to hypothesis in linguistics. One interesting point is the great store placed by linguists in the value of *independent evidence*. Oddly in Bayes' theorem there is no special place for this: evidence is only valued by its probability. There is room for argument here.

Bibliography

Ayer, A.J. (1977) *Part of My Life* (Collins)

Carnap, R. (1950) *Logical Foundations of Probability* (University of Chicago Press)

Feyerabend, P. (1975) *Against Method* (NLB)

Goodman, N. (1954) *Fact, Fiction and Forecast* (The Athlone Press)

Hempel, C.G. (1965) *Aspects of Scientific Explanation* (Free Press)

Howson, C. and Urbach, P. (1993) *Scientific Reasoning: the Bayesian Approach* (Open Court, 2nd ed.)

Popper, Karl R. (1972) *The Logic of Scientific Discovery* (Hutchinson 6th revised impression)

Venn, J.(1888) *The Logic of Chance* (Macmillan, 3rd ed.)