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CHANCE

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In this paper I attempt to analyse statistical probability, which I call ' chance ', in terms of a dispositional property which, following Popper (1957, 1959b), I call ' propensity '. What is new is not the suggestion but the attempt at a serious analysis of what it entails. I start with a brief discussion of the subjective theory, to show that its analysis does not preclude further, objective, constraints on a person's coherent betting quotients. I turn then to the inadequacy of any frequency account of such constraints, to show the need for the alternative analysis given in the following sections.

1. *Subjective Probability*

No one doubts that people assign probabilities to possible future events, and base their behaviour on such assignments. Assignments may be imprecise, as when an event is said to be ' unlikely ', ' fairly probable ', ' almost certain '; they may be merely comparative, when one event is said to be ' more probable ' than another, even though no absolute assignment is made to either. The so-called ' subjective ' theory of probability analyses these assignments in terms of betting at certain rates on the happening of the possible events. So far the theory is acceptable, since it has been shown to be necessary and sufficient, for betting rates to be rational in an acceptably defined sense, that the corresponding betting quotients should satisfy the probability calculus (e.g. see Shimony (1955)). Such a betting quotient is called ' fair ' or ' coherent ', and for brevity I shall refer to it as a ' CBQ '. The CBQ analysis has, however, been misrepresented as implying the redundancy of a concept of objective probability (e.g. de Finetti (1937)). Since I include it here in the analysis of such a concept, this point must be dealt with first.

In fact, the CBQ analysis of a probability assignment does not bear at all on whether the latter is objective. It may be taken to propose a measure of the strength of belief a person, A, has in the realising of some possibility, X. A may, or may not, also be prepared to recommend a similar strength of belief in X to any other person in the same situation. If not, his probability assignment is subjective; if so, it is objective. The CBQ analysis applies in either case, if at all. So obviously we cannot infer disagreement between A and B *merely* from their adopting different CBQs on the same X, even if, for example, they share the same evidence about it. Their probability assignments may be subjective. But, equally, we cannot infer, from this obvious fact, that all probability assignments *are* subjective, and that A and B are never in disagreement.

If A's probability assignment is objective, his statement of it may, of course, be either true or false. It may be false either because X's true objective probability is not sufficiently similar to A's CBQ, or because X has no objective probability at all. More generally, it may be that, for some interval I in (0, 1), sufficient grounds can be provided why anyone in the given situation should adopt any CBQ in I rather than any CBQ not in I, and A's CBQ is not in I. Or it may be that there is no such interval I. It might even be that there are *no* objective probabilities, in which case all statements of objective probability assignments would be false. But this again does not follow from the obvious fact that the CBQ analysis applies equally to subjective assignments, which could still quite properly be made, since they raise no questions of truth or falsity (unless a man lies, or can deceive himself, about the CBQ he has adopted).

Thus the CBQ analysis does not make a concept of objective probability redundant. What would make such a concept redundant would be that there were, and were thought to be, no objective probabilities. Whether there are any objective probabilities is the question whether there are ever " sufficient grounds " for recommending to the public one CBQ rather than another. Such grounds may be, roughly, either logical or empirical. Probabilistic confirmation theory deals with the former; applied statistics, which is my present concern, with the latter. It follows that, given statisticians' agreed criteria of " sufficiency ", whether they are satisfied or not is an empirical, not a logical, question. There is no question either of proving or of disproving *a priori* the existence of chances i.e. the truth of statistical laws.

It is, of course, a legitimate exercise in philosophical scepticism to question statisticians' criteria, and so to tighten them that they are never satisfied. My object, however, is not to question but to analyse, and in particular to show the relation of these criteria to those used in applying other objective concepts in science. Hence I need not contest a general subjective view of *all* scientific concepts, including chance, since it need not affect, except in terminology, an account of the relations between them. As Hacking has observed in this context, " if all flesh is grass, kings and cardinals are surely grass, but so is everyone else and we have not learned much about kings as opposed to peasants " (Hacking (1965), p. 211). Borel, for example, characterises the " cases where it is legitimate to speak of the probability of an event " as " the cases where one refers to the probability which is common to the judgement of all the best informed persons " (Borel (1924), p. 50). This suggests a subjective view on which, when such common judgement is lacking, we must say always that there is no chance which the event has, and never that someone, however well-informed, is mistaken. But if Borel holds such a view, he does not hold it peculiarly of chance, since he admits that " the probability that an atom of radium will explode tomorrow is, for the physicist, a constant of the same kind as the density of copper or the atomic weight of gold ". This is precisely my view of the matter, and my general dissent from Borel's presumably subjective account, of all such instances of agreement among scientists, is not to my present purpose.

The same applies to de Finetti's redundancy argument against chances, that " while refusing to admit the existence of an objective meaning and value for probabilities, one can get a clear idea of the reasons, themselves subjective, for which in a host of problems the subjective judgements of diverse normal individuals not only do not differ essentially from each other, but even coincide exactly ". (de Finetti (1937) p. 99) For any scientific concept, one might equally have " a clear idea of the reasons . . . for which in a host of [situations of measurement] the subjective judgements of diverse normal individuals . . . coincide exactly ". One would not take this as a ground for " refusing to admit the existence of an objective meaning and value for " masses, electric currents and the like. In general, the truth of a scientific theory of perception, about how scientists come to agree in the opinion that they have objective knowledge of some kind, does not entail, or even suggest, that their agreed opinion is false. But if it did, chance would be in no worse a case than any other major scientific concept.

There is, however, a further speciousness in the particular subjective theory of how scientists come to agree on chances. Among the theorems of the probability calculus is Bayes' theorem, which may be taken here to relate a person's CBQ on an event X to a hypothetical CBQ on X conditional upon the happening of another event Y. A *Bayesian* is a person who, on learning of the happening of Y, adjusts his CBQ on X to equal this previously hypothetical CBQ. He is said then to ' conditionalise ' his probability assignments on the acquired evidence, which states that Y happens. Then, as Savage ((1954) p. 58) puts it, " in certain contexts, any two [Bayesian] opinions, provided that neither is extreme in the technical sense, are almost sure to be brought very close to one another by a sufficiently large body of evidence ". This is the fact cited by subjectivists as explaining how scientists, initially allowed by the subjective theory to have widely divergent CBQ's, are brought by the piling up of shared evidence into the close agreement that is observed in their chance assignments.

Now, not only does this account of Bayesian consensus fail to show that there is nothing to agree on, but the CBQ analysis fails in two important ways even to entail Bayesian consensus. First, the CBQ analysis is not a scientific theory about any person's actual betting quotients. It lays down canons of rationality which restrict betting quotients proffered simultaneously by one person to those consistent with the probability calculus. If a person's simultaneously proffered quotients violate this restriction, the analysis is not refuted; rather, his quotients are condemned as incoherent (de Finetti (1937) p. 111). The analysis can thus neither be confirmed nor be infirmed by evidence of actual behaviour. By the same token, it does not entail or explain actual behaviour as would a scientific theory of how scientists come to agree on an assignment of chance (or anything else).

Second, it is worth noting (cf. Hacking (1967) pp. 313–6) that, even if scientists' behaviour were coherent in the sense of the CBQ analysis, it still would not need to be Bayesian. Coherence applies only to a person's simultaneously proffered betting quotients, and a person cannot simultaneously proffer actual and hypothetical, conditional quotients on the same event. Put another way, a person cannot at once both have and lack a piece of evidence and hence make simultaneously the probability assignments proper to both these states. But then coherence cannot compel a person to change his betting quotients from time to time in a Bayesian, or indeed in any other, way. Just as, on the subjective theory, two people may have the same or widely different CBQ's on the same event, so one person may from time to time preserve or change his CBQ's on that event. That this is so is well illustrated by a subjectivist's recent suggestion that " there are cases in which a change in the probability assignment is clearly called for, but where the device of conditionalisation cannot be applied because the change is not occasioned simply by learning of the truth of some proposition. In particular the change might be occasioned by an observation ". (Jeffrey (1965) p. 154) But equally, of course, any change required by conditionalisation may be cancelled or modified to any extent by such effects of inconclusive observation. Whatever the other merits of this proposal (see e.g. Levi (1967b)), it shows at least the compatibility of coherence and non-Bayesian behaviour.

For all these reasons, the CBQ analysis fails to bear, as it has been thought to do, on whether we need an objective concept of statistical probability. However, one or two other arguments have been produced, purporting to exclude chance in particular from the set of admissible scientific concepts, and these must briefly be considered. For example, it is said that " any event whatever can only happen or not happen, and neither in the one case nor in the other can one decide what would be the degree of doubt with which it would be ' reasonable ' or ' right ' to expect the event " (de Finetti (1937) p. 113). Of this, construed as an argument, it is enough to observe that the premiss is a tautology and the conclusion is not. Or again, it is said that, " in the last analysis, each evaluation of probabilities different from 0 or 1 will surely be abandoned, for a well-determined event can only happen or not happen; an evaluation of probability only makes sense when and as long as an individual does not know the result [sic] of the envisaged event " (de Finetti (1937) p. 147). It might be that a person only has occasion to *make* evaluations of probability when he is in a state of ignorance, but it would not follow that that is what they express, or that they are otherwise senseless. When a coin has just landed heads ten times in succession, it might be uninformative and pointless to remark that the event is very improbable, but it would not be senseless; indeed, it would be widely agreed to be true.

Elsewhere, de Finetti seems to confuse chance with inductive probability, and to assume that rejecting the latter entails rejecting the former: " The old [frequency] definition cannot, in fact, be stripped of its, so to speak, ' metaphysical ' character: one would be obliged to suppose that beyond the probability distribution corresponding to our judgement, there must be another, unknown, corresponding to something real [i.e. a distribution of chances] and that the different hypotheses about the unknown distribution . . . would constitute events whose probability [i.e. inductive probability] one could consider. From our point of view these statements are completely devoid of sense." (de Finetti (1937) p. 141). In fact, of course, it is quite possible to accept objective chance distributions, while denying that the concept of support for hypotheses about them can be explicated probabilistically. Such, for example, are the views of both Popper ((1959a) p. 251) and Braithwaite ((1953) p. 120). Whatever may be the arguments against objective probabilities assigned by an inductive logic, they do not at all serve to show that there are no chances.

De Finetti is here discussing the case of a coin of unknown bias, and asserting that " one does not have the right to consider as distinct hypotheses the suppositions that this imperfection has a more or less noticeable influence on the ' unknown probability ', for this ' unknown probability ' cannot be defined ". This does not afford an argument for de Finetti's rejection of chance, but is rather an implausible consequence of it. If there can be chances, then scientists can be mistaken or ignorant about them, and may entertain hypotheses about unknown chances as about unknown densities and atomic weights (see p. 13). The case of a coin of unknown bias is merely a very plausible example of this and, again, de Finetti's attempts to explain how scientists tossing the coin come to agree on the bias would, even if successful, go no way to show that there was no bias to agree on. It is also worth noting Braithwaite's argument, I think correct, that de Finetti's account, of the common judgement in such a case, that tosses of the coin are " exchangeable events ", requires just as great a commitment to unknown probabilities (Braithwaite (1957) p. 8).

2. *Frequency*

The most potent subjectivist argument against the existence of chances has been the inadequacy of the frequency analyses which have become accepted as the standard accounts of chance. Philosophers have felt forced into subjectivism by elimination, just as they in turn feel that scientists have been forced into frequentist views. Thus Savage ((1961) p. 576): " Rejecting both necessary and personalistic [i.e. subjectivist] views of probability left statisticians no choice but to work as best they could with frequentist views." Among statisticians, the dominance of frequency views is such that, intoducing a recent Royal Statistical Society discussion, a symposiast naturally says: " The term *frequentist* applies to any analysis or analyst of the " objectivist " school, where . . . there is a tendency to interpret probability in terms of relative frequencies in large scale replication." (Aitchison (1964) p. 161). Similarly, for Savage, ((1954) p. 3) " objectivist views hold that . . . evidence . . . for the magnitude of the probability . . . is to be obtained by observation of some repetitions of the event, and from no other source whatever ".

Given this identification of objective with frequency views, it naturally follows for the subjectivist that " the difficulty . . . in any objectivistic view [is that] probabilities can apply fruitfully only to repetitive events . . . it is either meaningless to talk about the probability that a given proposition is true, or this probability can be only 1 or 0 " (Savage (1954) p. 4). This is indeed the crucial difficulty, which frequentists have too readily conceded. It seems to me essential to any adequate analysis of chance that it overcomes or avoids this difficulty, and the ensuing analysis does so.

The defect from which frequency accounts of chance suffer is too much operationalism; not, as de Finetti ((1937) p. 149) believed, too little. The concept of chance is too closely related to one particular method, namely measuring frequencies, of ascertaining chances. It is a commonplace, of course, among statisticians, that chance is a property of which " observed frequencies are to be thought of as measurements " (Loéve (1955) p. 5), but it does not follow that frequencies provide an acceptable definition of chance, any more than metre-rule operations provide an acceptable definition of length.

The attractive starting point for frequency analyses of chance is the relative frequency or proportion of G (i.e. items having the property G) in some finite population of F. Thus Braithwaite ((1953) p. 122): " On the assumption that the class β is neither the null class nor is an infinite class, . . . the probability of a β-specimen being an α-specimen can be identified with the proportion among the members of β of those which are members of α " (see also Russell (1948) p. 371). There are well-known difficulties in extending the frequency analysis to allow for possibly infinite populations (see Kneale (1949) Sections 32–3; Popper (1959a) Sections 50–65; von Mises (1957) p. 142), but they need not concern us: the principal difficulty arises even in the finite case which must, it seems to me, be at least a special case of anything that could reasonably be called a ' frequency ' analysis of chance.

This difficulty may be clearly seen as follows. Suppose a to have in fact the properties F_1, F_2, . . . where the corresponding classes (of things that are F_1, F_2, etc.) are all finite. Then, on any frequency view, the chance that F_1 is G is the frequency of G in F_1, the chance that F_2 is G is the frequency of G in F_2, and similarly for any other property F_i. In general, these frequencies, and hence these chances, will all differ. Which, if any, of them is to be ascribed to a? If none, how is chance, so defined, to fulfil its intended rôle of constraining the CBQ which it is appropriate to proffer on *this* individual, a, being G? If no one frequency can be picked out to fulfil this rôle, the definition, whatever it is of, is not of chance.

As Kneale (Körner (1957) p. 19) and Ayer ((1963) p. 200) have pointed out, this is the frequency analogue of a well-known dilemma facing those who hold probability statements to express a logical relation between two statements, say of a hypothesis and of inconclusive evidence for it.

The attempts of both frequency and " logical relation " theories of probability to escape this dilemma founder on the difficulty of setting a non-arbitrary limit to the amount of evidence, or closeness of definition of the reference class, to be used in the probability assignment. Even if the difficulty were overcome, the resulting definition would then rest on some consideration other than frequency or the logical relation holding between two statements. Frequentists have recognised this dilemma, with which they have dealt by denying that there is a chance that *a* is *G*. Thus von Mises ((1957) pp. 17–18) asserts that " we can say nothing about the death of an individual . . . It is utter nonsense to say, for instance, that Mr X, now aged forty, has the probability 0.011 of dying in the course of the next year. [Mr X] is . . . a member of a great number of other collectives . . . for which the calculation of the probability of death may give as many different values. One might suggest that a correct value . . . may be obtained by restricting the collective to which he belongs . . . by taking into consideration more and more of his individual characteristics. There is, however, no end to this process . . . we shall be left finally with this individual alone . . . the collective will cease to exist ".

But if frequentists deny that their definition applies to the single case, what does it apply to? What sense can be made of ' the chance that an *F* is *G* ' that denies sense to ' the chance that *this* *F* is *G* '? It seems to me that there are two kinds of situation which lend frequency analyses their plausibility. One is the kind of situation in which an *F* is selected, with each *F* having an equal chance of selection. It then trivially follows that the chance of the selected item being *G* is equal to the frequency of *G* in the class of *F*. Of course, this will not do as any sort of definition of chance, since the concept is presupposed in describing the selection device. But the situation is at least one in which, *given* the concept of chance, it is clear why frequency should be a measure of it, i.e. should warrant a particular CBQ on the possible event that the selected *F* is *G*.

The other kind of situation in which the frequency analysis appears plausible is that in which a population is generated by some device, as the human population is. Then some humans, F, are male, G, and others not. The frequency of G in the population of F may be explained in terms of the chance of the event that the creation of an F will also be a creation of a G. If this chance, p, is the same for each event, then it follows that there is a very high chance that the frequency, f, of G in a large population so generated will not differ by more than a specified small amount from the chance p.

In situations of both these kinds, where frequency in a population is closely connected with chance, some other item than the population is concerned, and there is some possible event, on the occurrence of which a person might be supposed to bet. In the one case the extra item is a sampling device, and the event is that the F sampled is G; in the other, the extra item is the generating device, and the event is, that the F generated is G. It is an irrelevant, but possibly misleading, fact that, in some cases, the sampling or generating device may itself be an F, as when it is a person who samples from, or breeds, a population of persons.

Where no such extra items are present, and no possible event is contemplated on whose occurrence a bet could be made, it is not at all clear that there is any use for the concept of probability. It is quite true that, in such cases, the frequency definition makes 'probability' a synonym for 'frequency', but it is precisely this feature that leads to the frequentist's dilemma. For in every finite population of F, F_1, F_2, etc. there is a frequency of G, and so the frequentist is obliged to suppose that there is a corresponding probability. But I have tried to suggest that no accepted inference from frequency to chance would be invalidated if this identification was abandoned; on the contrary, many accepted inferences to the single case would cease to appear irrational.

The abandonment of frequency views, of course, still leaves the positive task of analysing the relations of chance with other concepts including, where relevant, that of frequency. But this latter is not the difficult part of the task: the relation between chance and frequency, as my simple examples partly show, is quite well understood, except that it is not one of definition.

3. *Chances and Trials*

In our concern with chance, i.e. with statistical rather than inductive probability, we are concerned with some feature of the world, ascertainable by the methods of science, which provides the " sufficient grounds " referred to above (p. 12) for publicly recommending some CBQs rather than others on the happening of certain possible events. This analysis of the criteria for detecting such a feature is not, of course, empirical, but the criteria themselves are, in the last resort. As Ramsey ((1931) p. 189) put it, criticising Keynes: " Anyone who tries to decide by Mr Keynes' methods what are the proper alternatives to regard as equally probable in molecular mechanics . . . will soon be convinced that it is a matter of physics rather than pure logic." Ramsey, that is, does not deny that such " proper alternatives " exist as a feature of the world, which make it irrational for anyone aware of them to adopt some CBQs. What he denies is that any process of reflection unaided by empirical observation is competent to establish what these " proper alternatives " are. Which is not, of course, to deny that a science which is so competent can later be formalised by " applied " logicians. But that is not my concern. Equally, I am not concerned with the question of whether " sufficient grounds " constraining CBQs might be provided by a quantitative inductive logic, i.e. whether there are objective probabilities that are not chances. I am concerned solely with the empirical feature that forms the subject-matter common to all statistical sciences, and the questions are: of what kind of item is it a feature that, say, the chance that F is G is p, and how is this feature related to others which are also ascertainable by the methods of science?

From the discussion so far, it is at least clear that a situation in which it could be appropriate to apply the concept of chance is one in which the occurrence of an event is possible but not certain. Among such situations are those I instanced above, in which a population is being sampled or generated. Other well-known instances are the situations in which it is noted whether or not a person dies, or a radioactive atom decays, in a given period of time. In such a situation there will be two or more possible events, one or other of which is certain to occur. To apply the concept of chance is to say that some CBQs are more appropriate than others to the happening of these events.

I largely adopt Hacking's terminology. A situation to which the concept of chance can be applied is a " *trial* . . . each trial must have a unique *result* which is a member of a *class of possible results* . . . the possible results for each trial are mutually exclusive. A set of possible results on any trial will be called an *outcome*. A trial will be said to have outcome E when the result is a member of E." (Hacking (1965) pp. 13–14). A chance is normally assigned to an outcome, but it may for brevity also be assigned to a result, meaning by that the outcome whose sole member is the result.

The term ' trial ' too much suggests a contrived situation or experiment rather than one that occurs naturally. The concept of chance may be applied to either, as the examples of death and radioactive decay show. However, while not ideal, ' trial ' is an accepted term, and I adopt it as such rather than add yet another to the literature.

Given this terminology, I return to the question: to what should the feature that I call a ' chance ' be ascribed ? It is trivially true that an *outcome* " has " a chance; to ascribe a chance to an outcome is no more than to restate that there is an objective constraint on the CBQ reasonably proffered on the trial having that outcome. But the chance of its happening is not a property that can be ascribed to an event without taking it to be the outcome of a trial. Indeed it seems clear that it is the trial itself of which it is a feature that the chance of an event is p. Further, the pertinent feature of the trial is not just the chance of one outcome, but the distribution of chances over all the outcomes (which is in turn determined by the chance distribution over all the results).

There is, of course, normally no objection to saying that it is a property of a throw of a die that the outcome $\{\text{five}\}$ has a chance, say, of 0.3. On the contrary, this will be entailed by the chance distribution over all the results, just as that a rod has a length $>$ 20 cm. is entailed by its having a length of 30 cm. The point is that this usage must not be taken to license the inference that the chances of $\{\text{five}\}$ and of $\{\text{six}\}$ are distinct properties susceptible of independent explanation, perhaps by reference to each other. In the same way, the length of a rod being $<$ 40 cm. is not an extension of it independent of its being > 20 cm., and susceptible of independent explanation. Both are entailed by the length of the rod being 30 cm., and this is the single extension of it that is connected by laws with other properties and relations of the rod.

I need also to emphasise that, in contrast to Hacking ((1965) pp. 18–20), I ascribe chance distributions primarily to *trials* rather than to *kinds* of trials. It may be true, of course, that among our grounds for distinguishing two trials as being of different kinds will be the fact that they have different chance distributions over their results. For example, we might wish to distinguish between waiting a day and waiting a year, to see if a person dies, as being trials of different kinds, simply because the chance distribution differs in the two cases. This is as doubtful as that applying forces of 1 and 10 gm. constitute different kinds of trial of a thing's mass, but such a usage may be conceded for present purposes. But there is a danger is using the term 'kind' in this way. For while it is convenient to express this usage by ascribing the chance distribution to the *kind* of trial it characterises, it is tempting to use this ascription in return to deny the propriety of ascribing chance distributions to the trials themselves. This temptation is particularly strong for a frequentist. Something like a frequency of one possible kind of result can readily be ascribed to a kind of trial, namely its frequency in some class of actual or imaginary trials of that kind; but obviously such a frequency cannot be plausibly ascribed to any single trial of that kind (see section 2, above). Now this is only a fact about frequencies, which in itself merely makes the frequency analysis of chance implausible. While that analysis is in question, it cannot be claimed as a fact about chances, in support of the frequency view.

On the present analysis, the point of ascribing chance distributions to single trials is to express the fact that their whole function is to warrant certain CBQs on the possible outcomes of such a trial. One cannot bet on a *kind* of trial, since a kind of trial is not itself a trial, but one can adopt the same CBQ on any trial of a certain kind, because the term 'kind' is so used that this is warranted. This is all that talk of ascribing chance distributions to kinds of trial signifies, and it cannot in the least follow from it, as frequentists might wish, that they should not also be ascribed to individual trials of those kinds.

I conclude then, that the " feature of the world, ascertainable by the methods of science " (see p. 21) that warrants assigning some probabilities rather than others to the happening of certain events, is a chance distribution over the possible results of a trial, the set of whose possible outcomes includes the events in question. This feature is to be primarily regarded as a property of the trial, and derivatively of a kind of trials all of which have the same feature. But this entity, the trial, and this feature, the chance distribution, still bear an obscure relation to the other entities and features of science with which it is clear they are connected. I next introduce the concept of propensity in an attempt to clarify the relation.

4. *Propensity*

Many scientific properties are regarded as dispositions, and Popper ((1957) p. 70) has proposed that they should all be so regarded. The entities to which dispositions are ascribed are more or less permanent, being re-identified through apparent changes as the unchanging bearers of changing properties. The paradigms of such entities are the physical things of the common language, with which any science begins its investigations, and the paradigms of such properties are the dispositions we ascribe to people.

The point of a disposition is that it does not display itself all the time, but accounts for regularities of behaviour in repeated situations in which the entity is involved. Thus a person's generosity does not display itself continuously, but accounts for his regularly giving more than others in situations where giving may be expected. Similarly, the solubility of a chemical substance does not display itself continuously, and may never display itself at all, but is permanently available to account for the behaviour of the substance when put in a liquid. Dispositions may be quantitative as well as qualitative; for example, the inertial mass of an object may be regarded as a disposition to resist an applied force, which is ascribed to it whether a force is being applied or not.

The explanation of a regularity of behaviour in terms of a disposition invented solely to account for it would be trivially easy. We require, therefore, of a dispositional property, that it be so linked to other such properties and relations of the entity that it is properly ascribable on the basis of other regularities than the one it serves to explain. A drug's " dormitive virtu " that ise detectable also by smell or chemical analysis is a perfectly acceptable disposition. The links between the dispositional properties, that make them non-trivially usable in explanation, are clearly the laws into which they enter, however loosely these may be formulated. Thus a person's generosity is a function *inter alia* of his temper, a substance's solubility of its temperature, an object's mass of its volume.

These commonplace remarks, of course, leave many problems about dispositions unsolved. However, I think the concept of a disposition is familiar and clear enough to warrant its use in the analysis of chance. This suggestion is by no means original. It occurs in Peirce ((1932) Vol. 2, Section 664), is referred to by Braithwaite ((1953) p. 187), and has recently been strongly revived by Popper (1957, 1959b) and Hacking (1965) and adopted by Levi (1967a). But these authors' analyses of the suggestion seem to me to leave scope for improvement.

The suggestion is that the feature of the world I have called a ' chance distribution ', and ascribed to trials of a given kind, should be regarded as a dispositional property ascribed to more permanent entities. I follow Hacking ((1965) p. 13) in calling the entity a ' *chance set-up* ', and Popper ((1957) p. 67) in calling the disposition ' *propensity* '. Then Hacking characterises a chance set-up as " a device or part of the world on which might be conducted one or more *trials*, experiments or observations . . ." and identifies propensity (which he calls ' chance ') with what the chance distribution would be, would have been, or will be on such a trial. He draws an explicit analogy with such a disposition as the fragility of a glass ((1965) p. 10): " If a wine glass would break, or would have broken, or will break when dropped, we say the glass is fragile. There is a word for the active event, and another for the passive dispositional property." Now this analogy needs careful statement. The *fragility* is the dispositional property of the glass that it has whether or not it is being, or ever is, dropped; the *breaking* is the property of the " trial ", namely of the dropping of the glass, that the glass's fragility accounts for and which it therefore shares with *all* trials of the same kind, namely all droppings of fragile glasses. It is not immediately obvious, and is a matter for further enquiry, what, in the case of trials on a chance set-up, are analogous respectively to fragility and breaking.

The main point is that it cannot be the *result* of a chance trial that is analogous to the breaking of a dropped glass. It is true that the breaking may be regarded as the result of dropping the glass, but to warrant the ascription of a disposition, it must be supposed to be the *invariable* result. Other things being equal, if a glass does not break when dropped, that suffices to show that it is not fragile. But if propensity is to be analogous to fragility, the result of a chance trial is clearly *not* the analogue of the glass breaking since, in flat contrast to the latter, it must be supposed *not* to be the *invariable* result of any trial on the set-up. If it were so, the trial simply would not be of a chance set-up at all. Other things being equal, if a chance trial does not have any given result, that does not suffice to show that the set-up lacks the corresponding propensity.

If propensity, then, is a disposition of a chance set-up, its display, analogous to the breaking of a fragile glass, is not the result, or any outcome, of a trial on the set-up. The display is the chance distribution over the results, which we have taken in section 3 to be the feature of the world warranting some rather than other CBQs on the various outcomes. This feature is thus analysed in terms of a feature of a more familiar kind, namely a dispositional property of a persisting physical entity. But, it may still be asked, what advantage is gained from introducing the concept of propensity merely to analyse the concept of chance? Are we doing any more, as Kneale (Körner (1957) p. 80) suggested of Popper, " than [to] provide a new name for objective probability "? The answer lies in the remarks made at the start of this section about the non-triviality of ascribing dispositions in science. Where propensities are admitted in scientific theories, they must be connected with other dispositions in terms of which they admit of explanation. Thus it happens that indirect evidence can occur for the ascription of a propensity, as for any other disposition. Thus it also happens that the traditional gambling examples are not good ones, for there the interest is in the trial rather than in the set-up. There is not a serious science dealing with the propensities of coins and dice, although something is known about them. The point is that the terminology of trials and chances, originally devised to deal with gambling, is not so well suited to describing the rôle of statistical theories. The entities and properties involved are made to appear more different from those of non-statistical theories than they need to be.

5. *Propensities and Chances*

That the *result* of a trial on a chance set-up displays only a tendency and not a disposition is the source of a peculiar temptation to confound the disposition, propensity, with its display, chance. In general, a disposition may be characterised by describing that feature, often an event, of a trial whose happening constitutes a display of the disposition. But a statement ascribing the disposition does not entail that the characteristic event ever happens, because it does not entail that the disposition is ever displayed. To say that a glass is fragile is not to say that it will break, since it may never be dropped. The temptation to confound propensity with chance arises because this feature of disposition statements is shared with chance statements. A chance statement also deals with an event, on the happening of which it warrants some CBQ. If this CBQ is less than 1, the statement does not entail that this event happens, *even if the trial does.* But a propensity statement, being a disposition statement, further does not entail that the trial occurs. This lack of entailment must not be confused with that shown by a chance statement. For example, if the propensity statement is that a coin is unbiased, it fails to entail that the coin falls heads, not only because the chance of heads on a toss is less than 1, but also because it does not entail that the coin is ever tossed at all.

That Popper and Hacking confound chance with propensity is shown by their unfortunate reform of usage in the literature. Peirce attributed something like a propensity to a die ((1932) Vol. 2. Section 664): " the die has a certain ' would-be ' . . . a property, quite analogous to any *habit* that a man might have ". His subsequent account of this " would-be " is unacceptably frequentist, but at least it is ascribed to the die, just as an analogous " would-be " is presumably ascribable to coins and atoms. Popper and Hacking, however, noting correctly that the chance of heads on the toss of a coin, or of six on the throw of a die, depends on the way the trial is carried out, and in what surroundings, have included all these other features in the total " experimental arrangement " (Popper (1957) p. 67) or " set-up " (Hacking (1965) p. 13) to which the propensity is ascribed. I have adopted the terminology, but I now wish to disown some of its connotations.

In bringing about a situation that will display a disposition, it is often necessary to add something to the object to which the disposition is ascribed. For a solubility to display itself, some solvent must be added for the soluble substance to dissolve in. For a glass's fragility to display itself, a stone floor, say, must be added for the fragile glass to be dropped on. Call the description of what must be added, and how, to bring about the display of a disposition its ' operational definition ' (I don't make the operationalist assumption that each concept has a unique operational definition). Then the feature of the situation, brought about by applying an operational definition to an object, that warrants the ascription of a disposition to the object, is the display of that disposition. Thus the dissolving of the substance in the solvent, the breaking of the glass when dropped, are both such features. But these are not properties of the object, they are properties of the situation; and conversely, the ascribed disposition is a property, not of the situation, but of the object. Solubility is not a property of the mixing of a solid and a liquid, and fragility is not a property of the dropping of a glass.

Starting from the situation, or trial, the point may be put by saying that convention picks out some more permanent entity, involved in other situations of the same kind, to bear the disposition of which some feature of the situation is taken as a display. But though the choice of entity is conventional, it is by no means arbitrary. The entity must be capable of bearing other dispositions, i.e. of being involved in other kinds of situations, brought about by other operational definitions, so that their law-like connections will serve to explain features of many diverse situations. The physical things of our common language, with which sciences start their ontological collections, are entities of this sort.

The conventional element is well illustrated in the case of solubility. If only one solvent, e.g. water, is in question, the solubilities are ascribed to the different solids whose presence with water gives rise to mixing situations with different features. On the other hand, if only one solid is in question, but different liquids, the relevant disposition is ascribed to the liquid. Thus it is a notable property of *aqua regia* that gold is soluble in it. Where a variety of both solids and liquids is in question, the solubility, of course, is ascribed to an ordered pair and expresses a dispositional relation between them. Similarly, with a variety of floors, the dispositions of being hard and soft could be ascribed as a fragile glass respectively did or did not break when dropped on them.

The case of propensity is now clear. The warrant for ascribing propensity to a die or coin, rather than to the complete " set-up " present at the trial displaying the propensity, is that convention picks out this more permanent entity from others also involved in the trial. There are standard ways of tossing coins and throwing dice, which could be specified in an operational definition, but are normally understood, just as it is understood that solubility is solubility in water and fragility to dropping on a hard floor. The convention could be otherwise: if there were definite varieties of tossing devices, which affected the chance distributions of coins tossed on them, then such dispositions as that of being biased could be ascribed to them rather than to the standard coins, by the tossing of which their bias would be displayed.

In the trial, therefore, to which is ascribed the chance distribution over the possible results of heads and tails, there are present both the coin and the tossing device (and doubtless many other relevant items). The propensity displayed may be ascribed to either, according to convention; and the convention here is peculiarly arbitrary, since there is no well founded science with a network of laws, about either coins or tossing devices, into which the propensity can be fitted. The ascription of a propensity here either way may be taken to express a conviction that such a science is possible. What is clear, however, is that the propensity is *not* to be ascribed to the whole assembly of coin, tossing device, and their environs that is only present when the coin is actually tossed. To do that is to remove completely the point of ascribing a *disposition*, as something present whether or not it is being displayed; it is to confound propensity with the chance distribution that is the display of it, and hence indeed to make ' propensity ' no more than a new name for chance.

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That Hacking suffers from this confusion is clear both from his acceptance of Popper's notion of the " set-up ", and also from the example in which he says that " a piece of radium together with a recording mechanism might constitute a chance set-up " (Hacking (1965) p. 13). This is like saying that a glass together with a hard stone floor might be fragile, or that a solid with a bucket of water might be soluble, or that a fire together with a thermometer might be hot. I use the term ' set-up ' in such a way that a piece, or an atom, of radium is a chance set-up; a coin is a chance set-up, given standard tossing devices; a tossing device is a chance set-up, given standard coins. The distinction between propensity and chance is as essential as the distinction between a set-up and a trial on it.

6. *Propensities and Statistical Laws*

I conclude with a few remarks on the connexion of propensity with statistical laws, and the wide-spread fallacy that the truth of such laws is incompatible with causal explanation of the events they cover. Now, no one doubts that the fragility of a glass is connected with others of its dispositions, such as its shape, chemical composition and stress distribution, so that fragility is a function of these other determinables. The laws which collectively state this connection may be called ' fragility laws '. Moreover, once a fragility is ascribed to a glass, again no one doubts that this warrants a generalisation that, on every dropping of a certain kind, the glass will behave in a certain way. The point is that it is these latter generalisations over *trials*, warranted by singular disposition statements, that correspond to statistical laws, rather than the laws, such as the " fragility laws ", into which the dispositions themselves enter and which are generalisations over *objects*.

The generalisations warranted by dispositions tend not to be called ' laws ', although they clearly support subjunctive conditionals, because of their customary restriction to trials (e.g. droppings) on an individual (glass). " It is not indeed a peculiarity of statements which one takes as expressing laws of nature that they entail subjunctive conditionals: for the same will be true of any statement that contains a dispositional predicate. To say, for example, that this rubber band, is elastic, is to say not merely that it will resume its normal size when stretched, but that it would do so if ever it were stretched" (Ayer (1963) p. 229). But no one would suppose such a generalisation to hold who did not also suppose it to hold of any sufficiently similar individual, i.e. an individual of the same "kind", as defined by possession of the same determinate values of the connected determinables (see Schlesinger, (1963) Chapter 3. The principle of connectivity). So the truth of a further generalisation over all such individuals is always implicitly assumed in all such cases.

In particular, a singular propensity statement is "universalisable" over all chance set-ups of the same kind—all atoms of the same radioelement, all sufficiently similar coins or dice. Hence, so are the entailed chance distribution statements universalisable over all trials of the same kind. These universalised chance statements are statistical laws, as usually stated. So, to establish a propensity statement is to establish a statistical law, though *what* law will only be explicitly statable if the "kind-defining" propensity laws are known.

Now, a statistical law asserts of *each* trial of a certain kind that on that trial there is the stated chance p of some outcome. Thus, where a statistical law is put in some such common but misleading form as

$$\text{'}100\,p\ \%\text{ of } F \text{ are } G\text{'}, \tag{1}$$

this sentence must be taken to state that

$$\textit{all } F \text{ have a chance } p \text{ of being } G. \tag{2}$$

The statement, in short, is as much of a universal statement as is

$$\text{all } F \text{ are } G, \tag{3}$$

and the common distinction between "universal laws" and "statistical generalisations" can consequently be misleading.

The crucial distinction, in both the statistical and non-statistical case, is that between *lawlike* and *accidental* universals, between those that do, and those that do not, support the corresponding subjunctive conditional. (I don't take this as explaining the difference, but rather as a statement of the difference to be explained, for which 'lawlike' and 'accidental' serve merely as convenient labels.)

The distinction between accidental and lawlike statistical universals may be illustrated in a way that serves also to show that the latter do not, as is often supposed, presuppose the impossibility of " causal " explanation of the single events they cover. Consider the sentence (1)

$$\text{` } 100\, p \text{ \% of } F \text{ are } G \text{ '}$$

where this refers to some outcome of kind G on a trial of kind F on what is taken to be a chance set-up. It is claimed (e.g. by Ramsey (1931) p. 208) that this is not a statistical law if a difference can be found between those trials on which the outcome is G and those on which it is not. For a causal explanation has then been given of the outcome of each trial, on the basis of the deterministic (and, moreover, causal) laws

$$\text{All } F \text{ and } F^{\star} \text{ are G} \quad \ldots \quad \ldots \quad \ldots \quad (4a)$$

$$\text{All } F \text{ and } \sim F^{\star} \text{ are } \sim \text{G} \quad \ldots \quad \ldots \quad (4b)$$

Then, since each trial is either $F^{\star}$ or $\sim F^{\star}$, the " chance " of its being G is either 1 or 0. In particular, of *no* trial is it true that the chance of its being G is p, so the reformulation of (1) to state a statistical law, (2), is not warranted. Thus, it is argued, if any causal explanation can be given, i.e. if any $F^{\star}$ exists such that (4a) and (4b) are laws, (1) cannot state a statistical law. Conversely, if (1) does state a statistical law, no such $F^{\star}$ exists—and this is something that can never be known.

The argument, though persuasive, is fallacious, since it begs the question at issue. First, it is agreed that the sentence (1) makes a *true* statement: it is merely its lawlike status that is in question. Second, the true statement made by (1) is not deducible from the laws (4a) and (4b) unless a further premiss is added, namely that made by the sentence

$$\text{` } 100\, p \text{ \% of } F \text{ are } F^{\star} \text{ '} \ldots \quad \ldots \quad \ldots \quad (5)$$

But, now, what is the status of *this* statement? It is perfectly compatible with all that has so far been supposed that (5) states a statistical law and, if it does, so does (1). The truth, then, of a statistical law does *not* require the absence of causal (or other deterministic) explanation of the individual events.

It is the confusion of propensity with chance discussed in

Section 5 that can make this conclusion seem puzzling, and tempt one to deny it. Because a fluctuating propensity can be ascribed to a temporally persisting set-up from time to time, it is tempting to suppose that a fluctuating chance can be ascribed from time to time to a temporally extended trial. For example, consider the chance that a process of conception and delivery of a human child (F) will have the outcome that the child is male (G). The trial is temporally extended, and it is correctly observed that immediately prior to delivery there is an explanatory difference between all those trials that have outcome G and those that have outcome $\sim G$; namely, that the *unborn* child is either male (F^*) or not ($\sim F^*$). Of each class it is observed that the chance of any member yielding outcome G is either 1 or 0. Hence it is argued that in no case was the chance ever other than 1 or 0, the only objective feature of the trial is that the child is either G or $\sim G$, and all our statistical speech expresses is suitably quantified ignorance of the outcome in each case.

The fallacy here is to locate a chance (of a male birth) at some temporal point within the trial, which it seems plausible to do because the corresponding propensity *can* be so located. But the latter signifies something quite different, namely that if, *per impossibile*, the " ordered pair " of parents conceived a second child before the first was born, it too would have a definite chance of being born male. All that the attempt to locate a *chance* within a trial shows is that, on quite a different kind of trial, namely where an unborn child of either sex is delivered, the sex never changes. Hence, the chance that the child will persist in its sex through the process of delivery is 1. And all this entails that is pertinent to the original trial is that, whatever the chance is that the born child is male, there is the same chance that the unborn child is male. In short, the outcome, G or $\sim G$, of the original trial can be determined indirectly, by determining F^* or $\sim F^*$, before it can be determined directly.

It may still be argued that it must " in principle " be possible to predict the sex of an embryo, or the result of a coin toss, since there " must " be some prior difference between the acts of conception or of tossing that give rise to such different outcomes. But again the problem would remain of the status of the original statistical statement about the occurrence of acts of conception or of tossing of these two " kinds ". It is equally " in principle " possible that, however far back these sequences of causally connected events can be traced, the happening of one, $F^\star$, or other, $\sim F^\star$, event is itself the outcome of a chance trial. But the principles here being invoked are *a priori* principles of determinism and indeterminism respectively, and the matter is not an *a priori* one. The best, because the only, grounds for settling questions of determinacy in some area of science are whether the high-level laws and theories accepted in that area are statistical or not. If they are, there will be, I suggest, some set-up postulated, with a propensity; but causal explanations of the individual events that are the outcomes of trials on such a set-up will still be possible.

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