The Matter of Chance

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D. H. MELLOR

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For my parents.

For the friends I have made while at Pembroke College Cambridge;
who made writing this book compatible with sanity
and sometimes necessary; whom I have ill repaid.

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Preface

I OWE MUCH to many people for what I say in this book. Specific debts are acknowledged in the text. In discussing other views I use direct quotation where possible, to avoid attacking straw men or claiming tacit credit for what is not mine. I have no further interest in academic patents, and have gone rather to clear expositors than to first authors of views. But I apologise if I have overlooked a due acknowledgement, and should be glad to be told of it. References are given in the Harvard system to a list at the end of the book. This also lists pertinent works I have read but not referred to. It is not otherwise comprehensive on any topic I discuss.

Many deeper debts are less traceable. I have discussed all these topics with many people and gained much from their remarks. Professors Braithwaite, Kneale and Mackie have taken a particularly careful and critical interest and made me face problems I should not have seen unaided. I may not convince them, but I hope they may have helped me to convince others. I am further indebted to the private comments, as well as to the published work, of Professors Körner and Giere and Drs Ian Hacking and John Wilson on topics central to the book. The first two chapters, written last, owe much to the patient criticism of Paul Teller.

I have many general philosophical debts. Professors Feigl and Brodbeck introduced me to the philosophy of science; Gerd Buchdahl has persistently encouraged me in it; Mary Hesse's and Jonathan Bennett's astringent interest has been a great curer of complacency; the stimulus of my colleagues, teachers and students of philosophy in Cambridge, has left my limitations no excuse. Their stimulus is largely responsible for what may be of interest in this book. The mistakes are all mine.

My most practical debts are to Verna Cole, who typed the book, Charles Jardine, who computed figures 3 and 4, David Papineau, who checked the references, Harold Frayman, who compiled the index, and my father, who thought up the title.

I should like to have repaid my debts with a good book, but there it

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is. Some material, mostly in chapter 6, first appeared in *Philosophy of Science* **32** (1965), 105–22; **33** (1966), 345–59; **34** (1967), 1–9. Some material mostly in chapter 7 first appeared in *The British Journal for the Philosophy of Science* **16** (1965), 209–25; **17** (1967), 323–6; **18** (1967), 235–8; **20** (1969), 366–71. Some material, mostly in chapters 1 to 4, first appeared in 'Chance', published in the *Aristotelian Society's Supplementary Volume* **43** (1969), 11–36, and is reprinted by permission of the Editor. © The Aristotelian Society 1969. I owe thanks to the editors and publishers of these journals for permission to use this material again.

<div align="right">D. H. MELLOR</div>

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Introduction

> There are certain common privileges of a Writer, the Benefit whereof, I hope, there will be no Reason to doubt; Particularly, that where I am not under-stood, it shall be concluded, that something very useful and profound is coucht underneath.
>
> Swift. *A Tale of a Tub*. Preface

IT MAY BE THOUGHT rash or superfluous in the Cambridge of Venn, Keynes, Ramsey, Fisher, Jeffreys, Braithwaite and Hacking, to write another philosophical book on probability. The present state of the subject, however, is neither so good as to make it superfluous nor so bad as to make it entirely rash. My project is in any case 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. Inductive probability and the deep problems of confirmation, induction and acceptance that involve it I mention only to show how little chance bears on them. That may serve indirectly to forward the solution of these problems, by limiting them, and I would claim no more than that for this work.

I assume some familiarity with the existing philosophical accounts of chance to which I refer. The ingredients of the present theory are in the literature, but they have hitherto been no more than half baked. The test of the theory is how much sense it makes of what professional usage shows to be thought true of chance. This usage seems to me to embody four important assumptions that other theories cannot make simultaneous sense of: that chance is objective, empirical and not relational, and that it applies to the single case. The chance of a radium atom decaying in the next ten minutes is as objective and empirical a matter as its mass, as little relative to evidence, and as much an attribute of one as of many statistical trials. Frequency theory makes no sense of the single case, personalist theory no sense of chance's objectivity, and classical and logical theory no sense of its empirical and non-relational character.

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The present theory takes most if not all chances to display dispositional properties which I call 'propensities'. The idea is not new; the name of the property is taken from Popper (1957). But the details of the theory and its defence against obvious objections are new. My chief concern is to give an acceptable account of the nature of propensity and of its relations to chance and to other dispositional properties.

I start by adopting the positive contribution of the personalist theory of probability. Statements of chance *inter alia* express degrees of partial belief, e.g. in the decay of a radium atom. Arguments are given for the existence and dispositional nature of partial belief and its appropriate relation to full belief. I then present the rationale for one betting measure of partial belief in more than usual detail to support the claim that it is measurable. This is all preliminary to the main claim that chance statements assert some degrees of belief to be made more reasonable than others by objective empirical features of the world. It is contingent that the world has such features; I argue only that nothing in the characterisation and measurement of partial belief excludes further empirical constraints of rationality upon it.

The constraints are then located in propensities, which are such standing dispositional properties of things as the *bias* of coins and dice, the *half-life* of radioelements and the *death risk* of people. The latter examples are given in some detail to support my claims about scientific usage and to show how naturally propensity theory accounts for it. I have deliberately avoided references to quantum theory, for a number of reasons. First the rôle of probability in the foundations of the theory is controversial. But secondly, if the theory provides rational partial beliefs in the happening of macroscopic events, the measure of these will be probabilistic. We may take any such objective chances as displays of macroscopic propensities independently of their microscopic explanation. I do not take the less theoretically "fundamental" nature of my examples to make them less secure cases of scientific knowledge. I have argued elsewhere (Mellor, 1968) against the all-embracing ontological pretensions of physical theories of the very small.

These examples are followed by a discussion of possible objections to propensity as a disposition. I attend in particular to imprecision and inexactness in scientific concepts and show that propensity is not peculiar in these respects. From an account of the sources of inexactness in explanatory dispositions I derive the regulative principle of connectivity, that two physical systems cannot differ in just one such

property. When this principle is applied to propensities some plausible classical chance distributions can be derived. I then compare these derivations with related classical and fiducial arguments.

Finally the implications of propensity theory for determinism and the status of natural laws are gone into. 'Determinism' here refers not to the absence of free will but to the thesis that events are governed by non-statistical laws. In this connection and generally throughout the book I address myself only to problems that bear on the relation of chance to other scientific concepts; I am not satisfied that the problems of free will do bear on this relation.

For the same reason I have felt free to adopt controversial positions without argument where the controversy would affect my discussion only in its terminology. For example, objective and realist terminology is used throughout the book. I write as if scientific knowledge is objective and as if such properties as mass, length and temperature exist. The wholesale dissent of subjectivists and nominalists from these presuppositions ought not to bear substantially on the theory here presented of the particular relation chance has to these other concepts.

One or two miscellaneous matters of notation need remarking. I use single quotation marks to form the names of terms and sentences and double quotation marks for other purposes. (In particular I use single quotes where I wish to discuss an author's remarks rather than merely to reiterate them.) Equations and other symbolised statements to which further reference is made are numbered consecutively within each chapter. In representing conditional statements I use ' $\rightarrow$ ' except where the material conditional is explicitly intended. It should be read 'if... then...', however that is further analysed.