EXPLANATION
AND
HUMAN ACTION

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Chapter One

Introduction

In daily life we succeed in accounting for our actions without recourse to general theories or statistical regularities. When we appeal to wants, plans, schemes, desires, intentions and purposes we render our actions intelligible to ourselves and our everyday auditors. More often than not, it would seem absurd to render our accounts explicit; they are implied in the ways we observe and describe behaviour. A student in my office reaches for a cigarette and matches, he strikes a match and lights the cigarette, inhales and exhales the smoke. It would not occur to me to accompany this set of observations of his actions with further comments designed to explain what he did. If I had to do so, I should appeal to his reaching for a cigarette as indicating a desire to smoke, and the rest of his actions as contributing to the same end. It would not occur to me or to my interlocutors to offer or demand general laws from which this action can be shown to follow, or regularities of which the connexion of this action and its motive would be an instance.

'Behavioural scientists' (i.e. psychologists and social scientists) and philosophers have put obstacles in the way of ad hoc explanations by demanding that any explanation lean on generalities for its support. When these demands of philosophers of science or 'methodologists' are taken seriously, as they are very frequently by psychologists and sociologists, theories are developed which meet the formal requisite of generality, but which pay the price for it rather heavily. For these theories are often redundant and platitudinous or totally irrelevant to the behaviour they are designed to explain. When we say that a man seeks food because he is hungry, or kills his father because he has been cut out of the will, or equally, when we say that men band together in economic or political enterprises and religious and social ceremonies because of their beliefs, needs, or roles, we are offering explanations of cases which do not require the support of general or theoretical statements. Often enough, of course, patient and detailed observation is necessary to describe adequately what we wish to explain; but no further research or generalizing technique is required to add an explanation to this description. It is true that men generally seek food when hungry, but it would hardly be necessary or relevant to establish or invoke this generalization in order to proffer the claim that hunger led me to cook my dinner or Jean Valjean to steal a loaf.
INTRODUCTION

But philosophers are inveterate generalizers and criterion-mongers, and ad hoc answers thus leave qualms. The qualms grow as we extend the discussion to more complicated questions. If someone asks me why I took up philosophy it might seem reasonably decisive to say that I became concerned with conceptual difficulties that stand in the way of empirical discoveries. But if an unflattering psychiatrist should suggest that I was temperamentally unfitted for the more arduous work of the laboratory or market place I should find it difficult to challenge him with my ad hoc reasons. In some sense my reason is the reason, yet in a sense that leaves open a host of alternative explanations.

Moreover, it is not only professional or professed scientists who find themselves ready and able to challenge ad hoc reasons. The accounts we give of our own behaviour are frequently challenged in an unanswerable way by wives and mothers and friends. A wife scorns her husband's account of his night of drinking (a business deal, of course) or his animated conversation with a pretty girl at a party (her intelligent conversation) as transparent rationalizations. But we don't know when to speak of his excuses as reasons and when as rationalizations. It may be that a new contract as well as a headache was the consequence of the evening's entertainment, and that the girl had brains as well as looks, though nothing is remembered of what she said. Quite outside the needs expressed by philosophers and scientists, to tidy up or generalize or verify our hypotheses, we do want to accept some reasons as appropriate or plausible or correct, and reject others as rationalizations. But we are in a quandary if we attempt to draw the line, or frame criteria which would enable us to draw the line, between reasons and rationalizations.

We are tempted in such circumstances to deny the propriety of reason-giving accounts altogether, and suppose that human nature must await, as the sociologists say, its Galileo. Much of the rationale of work in the behavioural sciences has to do with this sort of expectation, and so it is that psychologists and sociologists feel their actions justified if they spend lifetimes collecting statistics and framing indices, computing the mathematical values of correlations and devising hypothetical constructs: it will all bear on the Galilean revolution in the human sciences just as Brahe's tables of planetary motion contributed to Kepler's hypotheses.

Still, much of the time our reasons are not challenged. At least some of the time such accounts must stand; otherwise we should hardly have grounds for singling out some instances as rationalizations. If I said I enjoyed complicated fiction like that of Proust or Faulkner as an explanation for having their volumes on my shelves, yet when alone ferret out whodunits and scatological tracts, I should be rationalizing my possession of solemn novels. By the same token I should be giving an adequate account of my possession of whodunits and salacious paperbacks if I said I enjoyed them. So we distinguish between good and bad, or better and worse reasons; the problem is to see them formulated in such a way as to make clear why some of them are good and others are bad. And this is the difficulty.

How, then, can an explanation be adequate and yet have no implications beyond the case? One solution, quite popular among philosophers of history around the turn of the century, preserves ad hoc accounting, but does so by shrouding the business of explanation in mystery. According to the advocates of this kind of view, explaining human action is a matter of plumbing motives by means of a hypothetical experiment enabling the historian or sociologist to relive the actions of those whose lives he investigates. It is not at all clear, however accurate an account this may be of the historian's procedure, how successful divinations can be distinguished from the failures, and this seems to disqualify Collingwood and Croce, Rickert and Dilthey and Bergson, as responsible critics of the historian's, the social scientist's and the psychologist's trades.

The mystical and divinatory excesses of these philosophers have contributed much to the power of the opposite thesis that all explanation is general and theoretical in nature. Sometimes the advocates of this view follow a Humean line, according to which the strategy of any explanation consists in drawing an event under a law, without claiming that the law is descriptive. An adequate explanation, on either form of this account, is one that can be shown to extend the account beyond the case in question, which, in its usual form, is to predict. If either the inductive or the hypothetico-deductive strategies are taken as paradigm, something must be done about putative ad hoc accounts. Sometimes they are dismissed as irrelevant, being treated as rather trivial and uninteresting approximations of knowledge. At other times it is supposed that they disguise generalizations which could be brought forward in their defence, but which, because of their transparency or unimportance, do not need to be.

Neither of these alternatives, it seems to me, does justice to the business of ad hoc explanation. History, anthropology, journalism and our day-to-day observations attest the prevalence and importance of ad hoc accounts. We have, in fact, a rather rich knowledge of human nature which can only be assimilated to the generality pattern of explanation by invoking artificial and ungainly hypotheses about which we are much less secure than we are about the particular cases the generalizations are
introduced to guarantee. Moreover, when we move, as we shall in the
next chapter, to a review of attempts by psychologists and sociologists to
seek law-like explanations, we shall discover that these attempts normally
result in redundancy and platitude, or else are irrelevant to the behaviour
to be explained. Still further, it is not at all clear that theoretical or
inductive support is implicit in perfectly normal cases of physical explana-
tion. The paradigms of causal explanation, I shall suggest in Chapter III,
are observed collision, effort, and continuity, cases in which a correct
account can also be given ad hoc. The view that causal explanations depend
upon repeated observations of temporal succession derives its support
chiefly from an atomistic view as to what we may be said to observe, a
view which I hope to show cannot be intelligibly stated.

Physical interaction, of course, is not directly germane to explanations of
human action. But if we can attack successfully the major claim of the
generality thesis of explanation it may be possible to break the hold that
this thesis has had on philosophical thinking about explanation, and so
consider explanations of human behaviour on their own merits without
being distracted by the generality thesis and its supposed paradigm in
physics. The thesis I shall advance, in Chapter IV, and work out in detail
in the remainder of the book, is simply this: when we offer explanations of
human behaviour, we are seeing that behaviour as justified by the
circumstances in which it occurs. Explanation of human action is moral
explanation. In appealing to reasons for acting, motives, purposes,
intentions, desires and their cognates, which occur in both ordinary and
technical discussions of human doings, we exhibit an action in the light
of circumstances that are taken to entitle or warrant a person to act as he
does.

This view may help to clarify and even resolve a number of issues in
philosophy and the behavioural sciences. First, it may serve to straighten
out the role that generalizations do play in explaining particular human
actions, by considering the role of rules and aims in their logical bearings
on action. Second, it may contribute something to a view, much in
evidence nowadays, that what we observe depends as much on language
as upon eyes, on techniques as much as on events. To identify a piece of
behaviour as an action is already to describe experience by means of
moral concepts. This view has been obscured by the is/ought dichotomy
that plagues contemporary ethics, and so this discussion may lead to
some revamping of traditional conceptions of a subject which has been
the target for a good deal of abuse in the last hundred years. Third, it
may help to dissolve the paradoxical flavour of many of the comments of
contemporary philosophers on the subjects of consciousness, private
experience, purpose and intention, by showing that such terms exact
different requirements depending on whether they are employed to
describe, explain, or perform other linguistic tasks. Finally, I should at
least expect that the thesis offered in these pages might serve to counter
some of the more extravagant views denying a role to scientific method
in explaining human affairs. For this rejection of science often leaves us
with nothing but expletives and appreciations where we had hoped for
knowledge.

The behavioural sciences have become established features of academic
life. They play a crucial role in the political and social strategy of
governments. Experts in these disciplines are looked to for professional advice.
To oppose their influence is thus to find oneself opposed to intelligent
action in government and social policy. And if one denies to them the
right to frame hypotheses and form laws of human behaviour, one under-
cuts their status as scientists and experts.

But the truth, I think, is somewhat different. The intelligent and well-
formed rearing of children, treatment of the mentally ill, and fashioning
of policy is, no doubt, highly to be desired. But the observations pertinent
to these areas are piecemeal, the conclusions tentative, and dependent
upon a moral point of view. The machinery that behavioural scientists
bring to bear on human action is, in contrast, general and theoretical,
and in principle free of the context of moral discussion. Consequently,
I will suggest, their efforts pass by the problems to which they are
ostensibly addressed. Behavioural scientists are forced into a mistaken
view of their subject-matter as a result of their preoccupation with a
method they take to be necessary to any respectable inquiry.
Chapter Two

THE SCIENCE OF BEHAVIOUR

My object in this chapter is to describe how a conception of methodology has prevented sociologists and psychologists from offering significant accounts of human behaviour. This is necessary, to prepare the way for a rather different analysis of our manner of acquaintance with human doings by showing how methodology leads only to formulae for possible theories, but not to any genuine accounts of human behaviour. A sterile scholasticism has possessed the behavioural sciences, for which philosophers with their theories about the nature of science are very much responsible.

It might be supposed that the object of philosophers of science is to describe the moves scientists make in order to discover, invent, or verify hypotheses. But there is always the further note in the philosopher's account, which suggests that he is in a position to impose certain criteria and restrictions on what the scientist can do or say with propriety. Certainly psychologists and sociologists of a methodological bent have taken some philosophical writings in that spirit. Language, Truth and Logic, the Encyclopaedia of Unified Science, the writings of Reichenbach, Hempel and Bergmann have, among others, served as scriptures to the methodological exegeses of many psychologists and social scientists. They find in these writers sustenance for the view that all explanation consists in bringing phenomena under laws. Accordingly, all explanation will depend upon generality or scope, which ordinarily will take the form of successful forecast of new phenomena.

This story falls into two parts. The first states, as it were, the moral qualifications for any investigation, taking the form, for example, of the verification principle of meaning, or, earlier, Hume's dictum, 'no idea without a corresponding impression', or, still earlier, Bacon's insistence on the method of experiment. The second outlines the procedure for passing this test. Now there are points, and important ones, made by these demands. But they need to be understood in context. Bacon's demand for experiment, for example, needs to be seen as an argument against the appeal to scriptural or Aristotelian authority which is supposed to have characterized the science of the Middle Ages. The special cases of 'torturing nature' to find out her truth must be taken as illustrating his point, not constituting it. In the same way, Hume's appeal to induction needs to be seen as a polemic against the view that deductive reasoning is sufficient to establish the truth of any proposition. The verification principle needs to be viewed against the kind of philosophy that purported to describe how the world really is by invoking concepts that were suspiciously elastic or impossibly vague. All of these arguments are illustrated in ways that make the intended contrast more explicit. Difficulties arise when the vehicles for drawing the contrast are taken as embodying the rules for any investigation. The extreme case, employed to make the contrasting point, is taken as the standard or universal case, and thus the condition for any inquiry whatever.

The same might be said of more recent attempts to describe the nature of science. First, within the context of the claim that the meaning of a statement is its manner of verification, it becomes necessary to do something about laws which, because of their central role in science, ought to get full marks for empirical meaningfulness. If laws are treated as generalizations, however, they are not conclusively verifiable, for they cover any possible case of a certain sort, and thus include cases beyond those for which verification has been offered. So laws come to be treated as tools, ways of getting round reality in Schlick's phrase, or models that order the data, or mathematical devices that facilitate prediction. In much the same spirit these same descriptions of law-like statements are framed in contrast to Hume's inductive view of laws. It is claimed that laws are really hypotheses, from which consequences in the observable world can be deduced, and not inductive consequences of a host of observations. These remarks bring out an important feature of numbers of moves of physicists and biologists which tend to be overlooked by writers like Hume and Mill, who are anxious to avoid the a priori excesses of rationalist accounts of science. But these instructive contrasts do not entitle us to draw the moral that the methodologists in psychology and sociology tend to draw from them. Many scientific laws serve as bridges to predictions, or models which join otherwise disparate observations, or parts of deductive systems from which observations can be shown to follow. But it does not follow that anything counting as adequate description or explanation of phenomena must take this shape. To suppose so betokens an insensitivity to the content of an investigation as a result of a preoccupation with its form.

It might be said that before playing a game the rules must be laid down and understood. But imagine that the rules are laid down for a game before it has been decided whether we are to play with counters, nets and rackets, or cards, whether it is to exercise mind or body. To advance a theory of games under such circumstances would require saying something that would apply to any procedure and aim that one might
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conceivably want to employ or follow. To stretch so far, such a theory must accommodate itself to anything, and thus be tautologous, and so uninformative. To apply more specific prescriptions is to decide how to play a game when it is not clear what the aim or the equipment or the competitors in the game may be.

The sociologist, and the historian with scientific pretensions, are apt to take the philosopher’s polemics as prescriptions for carrying on their investigations. If, for example, they can produce any generalization at all, or any hypothesis that allows them to predict, they are prone to claim that they have accomplished something new and of significance in the study of society or of history. A classic case of the confusion of methodological propriety with new knowledge is F. J. Teggart’s study, _Rome and China_, in which he shows how Roman invasions of Armenia were invariably followed by barbarian uprisings on the lower Danube and Rhine, and Chinese imperial wars in the eastern Tien Shan by disturbances on the Danube between Vienna and Budapest. These coincidences occurred forty times during a period of 165 years. This is the generalization, which in turn is explained by appealing to the interruption of trade routes occasioned by the wars. Teggart’s analysis is designed to show that such an explanation could not have been given without the generalization. But the generalization does not in this case establish the explanation. Had we bothered to trace the chain of events in one case, we would have seen that the military movements created obstacles to a free flow of trade, and that these obstacles required the barbarians to move in order to restore their perishing economies. That such a chain of events was repeated indicates only that the dependence of the barbarian tribes on east-west trade continued through the whole 165 years. The explanation has its ties with a particular sequence, and any such sequence, of events. It had been given, in fact, before Teggart constructed his table of coincidence, as he fully acknowledges in his bibliographical data. It is only the view that all explanation is generalization that could make it appear that the barbarian movements had only been explained once the repeated sequence had been discovered.

Often the sociologist and the psychologist go further along the road to specifying the rules of the scientific game, by identifying the essential procedure of science with quite specific theories in it. Kepler’s laws have been especially popular in this respect. There is good reason for this, for the theory of planetary motions has the virtue of predicting from the laws and initial conditions the specific positions and motions of individual bodies. Since the psychologist feels that his study has a bearing on what individuals do in special circumstances, his hankering after general theory has often taken Keplarian shape. But Kepler’s laws scarcely illustrate any general truths about scientific procedure. They are, particularly with respect to their reference to individual bodies, rather unique in the catalogue of scientific theories. But this has not prevented many psychologists from supposing that their aim is to bring human behaviour within the laws of a Kepler-like universe.

The results of this preoccupation with methodology, i.e. with what is taken to be the proper and scientific form of any investigation, have been, in my view, disastrous in the disciplines investigating human behaviour. It has led to a formulation of methodological codes for investigation, in which everyone adds to or subtracts something from the code, but no one applies it. It has led sociologists and psychologists to design their studies in accordance with some conception of proper form and almost wholly without reference to the subject-matter in consequence the putative laws are often thinly disguised tautologies. It has sometimes led eager theorists to embrace symbols without the ghost of an idea as to the range of the variables or the function of the constants. To put it in a form acceptable to sociologists: methodological soundness is inversely proportional to factual significance. Triviality, redundancy and tautology are the epithets which I think can be properly applied to the behavioural scientist.

SOCIETY

Let us make the charges stick by appeal to cases in which methodological exactness replaces empirical significance. In a recent collection, Paul Lazarsfeld indicates the supposed advantages for our understanding of human doings that comes with a proper scientific technique.

It has been found that the more fire engines that come to a fire (x) the larger is the damage (y). Because fire engines are used to reduce damage the relationship is startling and requires elaboration. As a test factor (t) the size of the fire is introduced. The partials then become zero and the original result appears as the product of two marginal relationships; the larger the fire, the more engines and also the more damage. But I don’t suppose anyone would quarrel with the conclusion. But it is a rather unnecessary employment of the sophisticated techniques of mathematical or at least symbolic formulation of hypotheses. Lazarsfeld’s example may perhaps be intended only to illustrate the technique, and so such common or garden acquaintance with the world may only have been chosen in order to exhibit the technique without the distraction of

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the case. But it will appear, I think, that the technique is, for Lazarsfeld and for other sociologists, sufficient unto itself. Indeed, Lazarsfeld expresses himself as being rather satisfied with this outcome of his techniques, as if it had produced here something we didn't already know. Lazarsfeld's whole approach suggests that there is novelty enough in discovering that his techniques can produce the same results as those already obtained, quite convincingly, by more pedestrian methods. So much, for the moment, for the redundancy of the sociologist's account of human doings. But we can see now how the symbolizers in sociology come to be satisfied with their work. It is because their criteria for adequate theory derive wholly from a conception of the proper form of a theory and totally ignore its content or object.

Some of these equations are ambitious indeed. Dodd, for example, has been adequately excoriated for his symbolic writing of social processes, by Kaplan, but the gist of Kaplan's criticisms might be inserted here with profit. In the Dimensions of Society, Dodd offers the formula $S=(T;L;P)$, where $S$ means situation; $T$, time of duration; $L$, indication of characteristics; $P$, population; $;$, mathematical combination; and $;$, qualification factors. This appears to say, then, that a situation varies with respect to its duration, its characteristics, its geographical place, and its population, qualified by its social factors. One might say, surely, all of this goes into what we mean by a social situation. But it is not a law of social process, nor even, for purposes of further symbolization, helpful as a definition. The mathematical function $;$ remains uninterpreted. Perhaps we should read it as 'varies with', but we are totally in the dark as to whether it varies directly, inversely, arithmetically, geometrically, or in any number of other ways. At best we could only say, something like this might be the form that a law of social change would take, if it were possible to generate such a law. But this is a hypothetical perfectly consistent with the impossibility of such a law, or the possibility that it might take a quite different form. It bears much the same relation to the laws of science that La Mettrie's speculations about mechanical man bear to Newtonian mechanics. It is a kind of speculation which allows us to dwell on what a Newtonian science of man might look like, if there could be such a science, but it does nothing to decide whether there could be or not. At worst, it looks as if Dodd's formula is the result of an effort to leave nothing out, and not to be caught out in any way, an aim achieved by giving his law tautological elasticity.

There are many instances of the same sort of thing. In Sociology Today, Alex Inkeles offers another Dodd-like formula of even grander sweep and simplicity. It is: $(S)(P) = R$, where $S$ is the state of society, $P$ the personality factor, and $R$ the rate of social process. The emptiness of Inkeles' formula parallels Dodd's. In both cases a mathematical look is given the formula, without mathematical sense. It is not clear how these factors are to be quantified, nor what exactly their range of application is. But it does look, surely, as if one could say, whatever happens in society is a consequence of its state and the personality of its members. The verbal formula is all-inclusive and for that reason both true and unimportant. The symbols only serve to disguise the triviality.

These are perhaps sociological sports, but they illustrate something going on at the heart of the discipline. Methodology exercises its perverse influence by disguising itself as theory. The point of theory, sociologists are fond of repeating, is to order the data and open possibilities for liaison with new data. The point of methodology might be presumed to be statements like the preceding sentence, i.e. statements about the point of theories. But there is also a kind of scholastic intermediary, in which the sociologist lays down ground plans for analysis or chapter headings for books of as yet unknown contents. It is here that methodology keeps a foot in both the philosophical and the scientific doors, and does much to muddy both floors. Again from Sociology Today, hear what Parsons has to say about the ground plan for 'the systematic analysis of the structural components of large-scale and complex societies in terms of their relations to one another'. Parsons derives a scheme on the basis of two 'axes', the internal-external and the instrumental-consummatory. In a purely logical way 'problems or dimensions of a system structure' can be generated by the four possible intersections of the axes. Thus, where the external end of one axis intersects the instrumental end of the other, one has 'adaptation'; external-consummatory or goal attainment; internal-instrumental or 'pattern-maintenance and tension management' and finally, internal-consummatory or 'integration'. With some temerity, I translate these impressive rubries as follows: sometimes an individual adjusts to social pressures, at other times he manipulates other individuals and the institutions of his society to accomplish his ends, at still further times he attempts to reconcile external pressures and demands with his own desires and conceptions (e.g., sublimation?) and sometimes he achieves internal satisfaction (e.g., by a creative act?). Put in this way one wonders, what's the news? For surely the business of living has made us cognizant of the

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various strategies we employ, the satisfactions we achieve, and the pressures and demands made from without. That one can phrase these categories in technical jargon—presumably for the purpose of greater precision—and derive them from co-ordinating axes is only interesting if something follows from such terminology and organization that we did not know before.

What can be deduced from this scheme? In conjunction with some unspecified observations, it is discovered that the modes of social interaction are hierarchical, with the individual at the bottom and the nation at the top. (Parsons puts inverted commas around 'top' as if somewhat ashamed of his use of such a short and ordinary word.) It follows, for example, that "the decisions" [note again the embarrassed use of inverted commas] which bind larger and larger sectors of the social structure are made at progressively higher levels in the organizational system. Now we all know that federal law has wider scope than state law, that state law, applies also to towns, and that the rules a father lays down for his offspring do not apply to others' progeny. So Parsons' labours appear to be redundant. But worse, it is not quite clear what would count as higher levels in the organizational system except those bodies and corporations which make decisions of wider and wider scope, and so the law appears to be tautological. The theory is, so far, only a set of definitions whose usefulness in accounting for social phenomena is yet to be demonstrated.

Let us turn now to a special application of the theory. Parsons refers to it as an insight (p. 9), consequently I suppose it should be here if anywhere that we should see how the theory discloses new information. "The two main axes of differentiation . . . could also be identified in the generation and sex axes of the nuclear family." The roles of various members in the family can be talked about by reference to the internal-external axis, if we are organizing the family by generations; to the instrumental-consummatorv axis if we are thinking of differentiation by sex. I think this means that parents have authority over children, and that men tend to be the wage-earners. Once again, what's the news? Parsons' elaborate structure turns out to be a way of classifying the various interactions among individuals and groups, and any surprise arises only in that what we know already about human activities can be re-phrased in this terminology and classificatory system.

Now classificatory schemes may be useful adjuncts to scientific or scholarly inquiry; by a principle of ordering materials, they facilitate the location of any wanted item. But indices, tables of contents, or card catalogues, however ingeniously ordered and cross-filed, are not parts of scientific theories. (Except what is nowadays called Library Science: everything has to be a science.) In another sense, however, classification is an integral part of scientific theory. Linnaean family resemblances contribute to and are modified by evolutionary and genetic theories, the distribution of fossil and living types, and embryological development. But Parsons' scheme is not a consequence of theory; it is the theory. Consequently Parsons' pronouncements must be demoted from the status of theory to that of inventory. It is a system of arranging evidence so that it will be possible to locate the facts for which one is looking. But it is not clear that it is even successful in this role, for it is never quite clear what facts or items on his list Parsons is looking for.

There is, perhaps, yet a further point to taxonomy. For a classification is a way of reducing complex and confusing observation to something manageable simplicity. Parsons' system might be defended as the first gropings in the sociological field; it would be a mistake, then, to judge his taxonomy until it has either borne fruit or been replaced by something better. For the success of a taxonomic system cannot be judged beforehand. But even at its inception a taxonomic scheme, if it is to be more than an alphabetical or a subject inventory, should reveal unsuspected patterns; it should have, as it were, some theoretical standing. And this should show up in the consequences that Parsons draws from his system of concepts. Parsons attempts to show just such results of his taxonomy. He uses the concept of input-output to show (pp. 16 ff) the way in which a variety of social interactions can be accommodated to this model—in politics, the economy and education. Learning in the classroom can be regarded as output, educational 'deflation' can occur if students 'hoard' the output, and so on. The game could be carried on indefinitely. The question is, do we learn anything new about the educational process by applying this system, or is it rather that we understand the theory a little better when it is applied to a case that is wholly familiar? Parsons applies his theoretical or taxonomic superstructure to cases which are not illuminated in any way by the application. Quite the reverse; we understand the verbiage of the superstructure a little better when we see what count as instances of his terms and rubrics. In this sense the theory has no explanatory power; and as a description is unnecessarily complex.

The outrageous vocabulary clothes the essential barrenness of the theory. So:

... when the structure of the larger system is undergoing a relatively continuous process of change in the direction of increasing differentiation, the mechanisms involved in this change will, under certain circumstances, operate to dichotomize the population of units receiving the primary 'real' output of the focal system of
reference and to produce an orderly alternation of relative predominance of the two nearly equal parts (p. 32).

That is: given social change in a democratic society parties in power will tend to swing from liberal to conservative and back again. I do not know why such simpler formulations will not do in place of the bewildering complexity, unless it is that the terminological clouds the paucity of information. But one can see also how a sociologist might be led to such relatively arid pronouncements about social structure by virtue of just those considerations which trouble philosophers of science. Even when the particular sequence of events is better understood than the pattern or theoretical structure under which it is subsumed, an appeal to the theoretical structure is necessary in order to make our understanding of social processes fit a preferred account of explanation and description.

Many sociologists deplore Parsons' over-theorizing. Merton feels that what is needed first are laws of 'middle range', applicable to special segments or aspects of the social situation. With a firm foundation of such middle range laws, a more general accounting might be possible. Statements about society are needed, not Parsonian concerns with statements about the nature of sociological inquiry.

One such theory of middle range is to be found in George Homans' The Human Group. Homans wishes to generate a number of laws for the small group (in this case Hilltown, a small New England community) which may then be extended to apply to a suitable class of such groups. The 'variables' he wishes to join into laws are 'activity', 'interaction' and 'sentiment'. It will be my contention that his so-called laws are tautologies, sustained not by the evidence, but by the interdependence in meaning of the terms out of which his laws are formed.

An activity is any element of behaviour (pp. 34 ff.). This itself is somewhat obscure if it is taken in anything more than a commonsense way. For example, verbal interchange is an activity. But is the word or the sentence or the whole conversation the activity? In ordinary talk about the doings of men, this is not troublesome. What will count as an activity, what will count in other words as the unit of discourse, is made clear by the context. Whether I am talking to you about the motion of your arm in making a turn, or about the whole operation of turning the car, or the activity of driving, will ordinarily be made clear by the kind of questions I am asking you, or the kind of conversation we are having. But Homans wishes to establish laws expressing functional relationships

between activities and the other factors; consequently he needs more than contextual clarity.

Interaction is defined as the amount of shared activities, and sentiment as the liking of people for one another. The imprecision in the use of activity thus allows for a good deal of overlap between this concept and interaction. For many of our activities are shared by definition; they are social activities. Activity and interaction are inseparable without a criterion which would restrict activities to some sort of movements of the individual organism. Many of Homans' generalizations rely exclusively on this overlap. It is also very difficult to separate Homans' use of sentiment from his use of interaction. For the purpose of general laws, he needs a behavioural criterion for sentiment, for he can hardly rely on that kind of particular judgment we make as to the likes and dislikes of a person, which is a function of context and long acquaintance. But this behavioural criterion turns out to be suspiciously like that for identifying interaction. The mark of sentiment is the quantity of shared activities. Thus the way in which he defines the building-blocks of his theory provides the basis for the generation of a series of tautologies masquerading as empirical generalizations. Let us examine some of these.

(a) 'A man will do more [output, in sociologese] if he is interested in his work' (p. 102). It may be possible in ordinary talk and contexts to attribute a person's work to his enthusiasm for his task without running the danger of pleonasm. We read it in his style of work, his facial expressions and his avowals. But as these readings depend upon a kind of impressionistic and circumstantial evidence that a practising scientist would deplore, Homans is forced to interpret sentiments behaviourally and measurably, that is, by the amount of participation in the activity. Interest in work is identified by the amount of time spent in, or the intensity of, its pursuit. It is, then, not surprising that the amount of time spent at work increases directly with the amount of time spent working.

(b) 'If the scheme of activities changes, the scheme of interactions changes also' (p. 102). A man, let us say, plays golf with his business associates, but after his nefarious conduct of business affairs has been revealed and he is ruined we find him playing pool with a group of derelicts. His game is adjusted to his associates. This fragment of the larger generalization is a sociological truth. Games go with incomes. They are, if you will, status symbols. But it is not this or any assemblage of such observations that concerns Homans. These are the piecemeal observations which constitute amateur sociology, and have point when embedded in novels, newspaper columns, and travelogues. These are undoubtedly interesting observations, so long as one does not have to read a catalogue of them, that is, so

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1 Robert K. Merton, Social Theory and Social Structure (Free Press, 1957), pp. 5-11, passim.
long as they occur in a story. But to convert them into the kind of formula Homans desires, it is necessary to consider activities and interactions as variables bearing a constant relation to one another, and for which one may substitute any activity or instance of interaction. Such a formula either tortures the piecemeal observations of which it is made up, by exacting of them more than they can give, or degenerates into the pleonasm that one’s activities with ABC are distinct from one’s activities with DEF, in virtue of the fact that they are performed with different people. The first alternative is the familiar move to general formulae as a way of accounting for or describing human actions already well enough described and accounted for by piecemeal observations. The second reflects a concern over the appropriate way of defining action; it turns out that it is to be defined in terms of interaction.

(c) ‘Persons who interact frequently tend to like one another’ (p. 111). This statement tries to vault the horns of a dilemma. We all know very well that sometimes familiarity does breed contempt, and even hate. Consequently, if his thesis is factual, it is surely false.

But Homans uses interaction as the sign of sentiment, hence the connection between the interaction and likings of people for one another is tautologous. Homans is not offering this hypothesis in a ‘sometimes, but then again often not’ spirit, which would be the modest requirement for factual status, for on the following page he reformulates his thesis in a law-like manner: ‘If the frequency of interaction between two or more persons increases, the degree of their liking for one another will increase, and vice versa.’ The percentage of cases in which this hypothesis is likely to be false makes it a poor candidate for a law of social groups, and its universality is guaranteed only by depriving it of empirical significance.

(d) The next of Homans’ laws requires a new distinction, drawn between the ‘internal’ and the ‘external’ system. The external system (pp. 90–94) consists of those activities, interactions and sentiments that allow the group to survive in the environment. The internal system, by contrast, is characterized by behaviour that is an expression of sentiments ‘developed by the members of the group in the course of their life together’ (p. 110). With this increased arsenal of terms and distinctions Homans states the following ‘law’: Frequency of interaction in the external system varies directly with sentiment which in turn varies directly with interaction in the internal system. I think it is also Homans’ intention to give this formula a causal sense, that is, these circumstances follow each other in this invariant sequence, so that frequency of interaction (external) has frequency of interaction (internal) as a consequence. An instance might be the sharing of parties, lawn-mowers and whisky by men who also share workbench, office-space and tools. But so rendered the hypothesis is surely suspect. Of course we know people who do associate at home with the office personnel, but we know just as well of people who don’t. To save the generalization from such a disaster—one might call it the Pooh disaster, for, you will recall, Pooh is addicted to the solemn generalization: ‘Some do and some don’t’—Homans can only shore it up by defining his words in such a way that they no longer refer to independent factors in the situations allegedly described by the law. What will count as interaction in the internal system is determined by its bearings on interaction in the external system, and conversely. In this way the only case that will satisfy Homans’ concepts will automatically support his law.

(e) One final instance: People who interact frequently are more like one another in their activities than they are like other persons with whom they interact less frequently. Since activities are cases of interaction, this law too is tautologous. It might be claimed with some point that sharing specified activities (job, club) or neighbourhoods augurs other similarities, like religious or political beliefs. Political experience fortifies our belief in the relatively conservative politics of the wealthy. Such facts, as others that come to mind with each of Homans’ laws, are perfectly in order and defensible. But they neither require nor add up to the sorts of laws of universal scope that Homans finds necessary in order to carry out the scientific study of society.

At times Homans reaches through this structure of theory to actual descriptions of social behaviour. For example, he notes (p. 141) that the highest the rank of a person in a group the more nearly is his behaviour likely to conform to the standards of the group, although here too, seeing the generalization expressed in naked English gives us qualms. We think too readily of the many cases in which the boss dictates the moral code stringently to others but applies it rather loosely, if at all, to himself. The observations do not lack point by reason of the exceptions. It is in being forced to do duty as general laws that they are emptied of significance.

At the end, Homans voices Durkheim’s thesis that civilization decays through lack of social cohesion. This is the justification as he sees it of his enterprise in the meticulous analysis of small groups. For now the analysis bears an important relation to the preservation of society, a way of life, a civilization or culture. But the same difficulties beset him here in making a conceptual truth play the role of scientific law. Surely social cohesion and the prospering of a civilization go hand in hand. But not in the sense that the one is the harbinger of the other, but in the sense that the one in the other. We ought to be able to demand more from a science of society than this.
The Science of Behaviour

In writing the history of their subject sociologists are apt to see the following approaches succeeding one another. (1) Dynamic sociology, or the attempt to account for social change; (2) static sociology, or the analysis of social structure, samples of which we have just considered; and (3) neopositivism, a movement characterized by alleged improvements in the manner of observation of social phenomena, through the employment of questionnaires, statistics and other quantitative refinements. Practitioners of the last of these styles congratulate themselves on being sophisticated enough to shun sweeping generalizations and causal hypotheses, and substitute for them what is needed first, scrupulous observation. Theory without observation has an exceedingly bad reputation, of course, and this justifies many sociologists in pursuing what to the outsider might appear to be inconsequential details and arbitrary correlations. We shall see in due course that the apparent triviality of this research is real indeed, in the sense that our identification of social facts takes place in a way to which these precise methods have no relevance. In any case, since such writers eschew explanatory impulses, we can pass over them here.

We can also overlook so-called dynamic theories. The theories we have looked at so far are static theories. They show a morbid preoccupation with form at the expense of significant relation to social facts. This preoccupation has another side. Most social theorists of the past thirty years or so have wished to avoid the excesses of the dynamic school, in which the object was to explain or devise the laws of large-scale social changes. Now many of these dynamic theories were significant, but false. The supposed evolution of society from hunting through herding to tilling the soil is an instance. The wrecking of the fortunes of dynamic theories on the shoals of evidence led, on the one hand, to static theories with their redundant, trivial and tautological formulae, and on the other to new dynamic theories, which protected themselves against damaging evidence by much the same elastic and interchangeable use of concepts, which led to tautologies masquerading as laws in social static theory. Toynbee's curious intermixture of the concepts of religion and social order is an obvious case. But so also is the Marxist thesis. There is the tendency here to confuse a methodological directive, 'seek out the economic changes', and the substantive theory that all social change is due to changes in the mode of production. Every society exhibits some distinctive economic arrangements alongside equally distinctive cultural forms and processes; consequently by invoking the methodological principle as a law, the possibility of discovering disagreeable evidence is avoided.

Psychology of the Individual

Laws of social process characteristic of dynamic theories, then, show the same defects as the laws of social statics. They have been amply and ably criticized, by sociologists themselves, but with special force by Karl Popper in The Open Society and Its Enemies and The Poverty of Historicism. I shall not impede the progress of my argument by repeating his, though I should like parenthetically to endorse it. In any case, though social process theories have a certain popular vogue, because of their messianic and prophetic implications, they are regarded as poor relations by academic historians and social scientists, who preen themselves on their scientific emancipation from the apocalyptic tendencies of their forbears. Consequently I have limited my sketch of sociological theory to social statics.

Psychology of the Individual

Much the same story can be told of psychology. There are numbers of large scale theories of human development and personality. Some of these, like Freud's, are based on stages of erotogenous concentration, which, in conjunction with an elastic range of events taken as producing fixation at one of the stages, accounts for later-life miseries or profanities, frigidity or nymphomaniac, and perhaps even the choice of one's wife or model of car. Others borrow the terminology and form of physical theories which, in the case of someone like Lewin, attempts to reduce the study of personality to a study of geometrical relationships, much as this sort of reduction has taken place in mechanics. But Lewin's topological and vectorial representations are not theories, but models of what a theory ought to look like if it is to conform to the pattern set for scientific theory by Einstein. Psychologists object to Lewin's formulae on the ground that they fail to predict or disclose new information, and this is quite correct. But the same point might be put by saying that his views never come to grips with questions about human behaviour at all, but afford comments only on what a theory ought to be like if it is to count as a scientific theory of personality or human behaviour. Freud's oral, anal and genital stages come closer to what we can observe in human development and have, perhaps, something to say when stripped of the vaguely mechanical language in which they are clothed. But here we will see that the metaphors by which Freud stated his position are essential to its survival, and make his theory a rather special case which will require consideration in its proper place.

All of this, then, is merely introductory. Its aim is to provide the background against which a more numerous body of academic psychologists have staked out their scientific claims, and congratulated themselves that
they are not as their poor and speculative brothers, but 'hard' scientists. The question is whether they can fulfil their scientific programme without being reduced to methodological sterility, as in theories like Lewin's, or maintain some bearing on what can be observed of human doings without exceeding the bounds of their self-imposed scientific regimen. We shall need to answer this question by reviewing a number of methodological devices the psychologist supposes he must use to meet these aims.

A good deal of argument goes on in psychology between those who maintain that, ideally at least, psychological laws will be nomothetic and those who maintain that they must be idiographic. Sometimes this issue is clouded by a closely related controversy, in which a distinction is drawn between deterministic and probabilistic laws. So far as I can understand or summarize the different battles in this war, it would appear that nomothetic and idiographic laws are both determinstic. Perhaps it may be possible to combine the two dichotomies in the following way.

(i) A nomothetic theory is general in form. Its consequences describe or predict the events which follow if certain conditions obtain. Such a theory is deterministic in that, given the initial conditions, the consequences described must happen. The relationship between gravitational theory and the description of the acceleration of a falling body, or again, between the law of free fall and the fall of any body in a given gravitational field, would presumably be instances.

(ii) A probabilistic theory is also general in form, but its consequences do not record the occurrence of events, but the probability of their occurrence. The laws of elementary particle physics and the prediction from them of the likelihood of an energy jump is a standard example.

(iii) An idiographic theory is also law-like. But unlike a nomothetic theory, both the law and its consequences refer to specific events and specific objects. Kepler's laws of planetary motion is the standard instance.

Let us leave nomothetic and probabilistic models of theory formation aside until the next section and deal first with idiographic models. There is good reason for the demand for idiographic theories. In psychology one is supposed to be dealing with people, and explaining what they do. There are, says Gordon Allport, in Becoming, many areas of psychology where individuality is of no concern. What is wanted is knowledge about averages, about the generalized human mind, or about types of people. But when we are interested in guiding, or predicting John's behaviour, or in understanding the Johanian quality of John, we need to transcend the limitations of a psychology of species, and develop a more adequate psychology of personal growth.7

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7 Gordon Allport, Becoming (Yale University Press Paperbound, 1960), p. 23.

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The psychologist, then, takes on as his task the prediction of the next action of John. It is as if psychology has failed, or partially failed, if it succeeds only in framing statistical likelihoods that this or that type of person will do this or that sort of action in this or that situation. But, the argument goes on, to explain in any creditable way means to subsume the actions or events to be explained under laws. The wedding of this aim and this procedure requires laws for individuals, or at least laws which, in conjunction with initial conditions describing particular individuals, enable the psychologist to draw conclusions in the form of description and predictions of the behaviour of this or that individual. So Kepler's laws, as I have said, come to serve as the idiographic psychologist's ideal, for these laws do have the virtue of describing, and far in advance at that, the actual behaviour of specific, named bodies. Now Kepler's laws are rather unique in this respect, and so are apt to be misleading as models of appropriate scientific pursuits. It is perhaps idle to attempt to show that the psychologist's aim is logically misconceived in this respect. But the odds are certainly against him. In order to derive consequences about John's behaviour in the next minute, hour, or year from the laws of personality, the initial conditions which afford the bridge from laws to consequences must be extraordinarily complicated, for people and their separate histories are extraordinarily complex. Thus, if knowing John is knowing all about him, our knowledge will be so detailed and varied that we must have recourse in making our predictions, not to statistical regularities associating the situation with a wide range of uniform behaviour, but to an assessment of John as a person. Our very concept of personality will arise via assessment, and denote the composite picture of John or Mary pieced together by means of long study and observation. The psychoanalyst, to make use of one of Meehl's excellent examples, explains a woman's hallucinations of a raven on her pillow as a need to test her husband's love. He cannot have recourse to statistical formulations of the association of avian hallucination and a desire to test a husband's love. And the ways in which one could state general principles would be so general as to be vacuous. He must, because it is just this hallucination and this person which concern him, resort to other techniques which could be fairly characterized as hunches. Freud glorifies this notion as the 'resonance' of the analyst's unconscious with the patient's.8 What he is saying might better be put in the language

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of assessment: that kind of inference lacking formalized steps which can be rehearsed at will. And being unable to reproduce them, we are unable to elevate this collection of more or less informed hunches to the level of a method to be practised by any qualified member of a profession. It is not that there is no place for hunches, for understanding others, for assessment; these are indeed the tools we have at our disposal for the kinds of judgments we are inclined to make about individuals. We surely also want to admit that some people have a 'gift' for this sort of thing. Psychoanalysts like Freud and Sullivan, and perhaps others like Reik or Stekel, demonstrate constantly in their dealings with patients the ability to 'size-up' and predict the patients' behaviour.

But, as Mehl notes, there can be no genuine logic to such predictions or assessments, except in the actuarial analysis of their results. Psychoanalysts' claims might then be tested in the following manner. If any one is trained by special people in a special manner he will have significantly greater success in predicting individual behaviour than anyone else. There are, of course, no statistics of this sort except the dubious reports of analysts in congratulating themselves on their successes. The statistics that are available would lead one to suppose that training and theory have very little to do with the successful issue of prediction. In short the successes have more to do with the kind of individual making the predictions than with the method or theory they employ in making them.

In the Psychopathology of Everyday Life, Freud provides many examples of his flair in making shrewd guesses about the problems troubling people, from their mistakes and lapses of memory. In one case, for example, 'a young man of academic education' attempts to quote the line from Vergil: 'Exorari aliquis nostris ex ossibus ulter', but cannot supply the word 'aliquis'. On being asked for everything that came to his mind, the young man produced the following associations: a-liquis, reliques, liquidation, liquidity, fluid; and then: 'of Simon of Trent, whose relics I saw two years ago in a church in Trent', of the work of Kleinmulp, of an article entitled 'What St. Augustine said concerning Women', of a gentleman he had met who was 'an original type', and whose name was Benedict. At this point Freud interpolates: '... you give a grouping of saints and church fathers: St. Simon, St. Augustine, and St. Benedict ... Origenes ... and Paul in the name Kleinmulp.'

The man now thinks of St. Januarius and his blood miracle, a reference to the supposed liquefaction of the saint's blood, kept in a phial in a church in Naples, and of an occasion on which the miracle failed to take place on time. While soldiers waited for the liquefaction, the young man remembers, Garibaldi hinted to the priest that the miracle should take place. All of this leads the young man to 'an intimate matter' which he is reluctant to impart, but eventually says: 'I suddenly thought of a woman from whom I could easily get a message that would be very annoying to us both.'

Freud: 'That she missed her courses?'

The young man is appropriately astonished, but Freud notes, 'You prepared me for it long enough. Just think of the saints of the calendar, the liquefying of the blood on a certain day, the excitement if the event does not take place, and the distinct threat that the miracle must take place.' And so Freud triumphantly concludes, 'Indeed, you have elaborated the miracle of St. Januarius into a clever allusion to the courses of the woman.'

What can be gathered from this case? First, it is obvious that there is something to the interpretation. It has the virtues of logical consistency (provided one accepts the punning ground rules) and it appears to be borne out by the evidence. Though it may seem extraordinary or repellent that mistakes and omissions may have this meaning, it would appear difficult to doubt Freud's interpretation, for the man's testimony bears out the remarkable inferences. But the manner of inference is obscure, and for that reason it is apt to be called intuitive; the process of inference cannot be described. But if the inferential process cannot be described, it is no longer possible to decide whether an analyst is correctly using it. The tests of his insights are the results in particular cases. In place of a method which could be correctly or incorrectly applied, one is forced to depend wholly on the person making the inferences, and he is evaluated wholly in terms of his successes.

The force of these objections can be brought home more clearly by contrasting the previous case with another drawn from the same work (on pp. 35-38 of the Basic Works), which along with the aliquis case, Freud regards as an example of his skill, method and theory. But here the inferences are far-fetched and the punning connexions strained. Freud had been trying to recall the name Signorelli, but produced instead the names of two other artists, Botticelli and Boltraffio. The occasion of his lapse of memory was a conversation in which Freud and a travelling companion had been discussing, just prior to the difficulty over the matter of the fatalistic attitude of Turks living in Bosnia and Herzegovina toward death. A colleague, Freud reported, says of them that 'when one is

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10 The Basic Works of Sigmund Freud, edited and translated by A. A. Brill (Random House: Modern Library, 1930), pp. 41-44.
compelled to inform them that there is no help for the patient, they answer: "Sir (Herr), what can I say? I know that if he could be saved, you would save him."

At this juncture Freud says that he was about to call attention to a further characteristic of Turks living in Bosnia and Herzegovina, that they 'value sexual pleasure above all else', one of his colleague's patients telling him: 'For you know, Sir (Herr), if that ceases, life no longer has any charm.' Freud blocked this thought, he tells us, because of its associations with the theme of 'Death and Sexuality'. He was much affected at the time by a message he had received while staying at Trafoi, telling of a patient who had committed suicide 'on account of an incurable sexual disturbance'.

We are now invited to draw a punning connexion between Bosnia and the two obtruding names, Boltraffio and Botticelli, between Trafoi and Boltraffio, and between Signorelli, Herzegovina and the conversational references to 'Sir' in 'Sir, what can I say...' and 'For you know, sir...'.

Now the practice of punning is, in its humble way, a skill. One may be good at it (Freud perhaps), or hopelessly bad. Furthermore, one man may fall into punning habits while another man's chain of thoughts may operate on rather different principles. Thus it may be supposed that Freud has told us something about his own thinking habits, and has given in addition samples of his virtuosity in carrying off puns in a more or less dashing or felicitous manner. But it is not clear that a talent for puns will bear the theoretical load that Freud wishes to put upon it. Of course, the reason given for forgetting is not the mere capacity to weave a chain of words or thoughts out of punning connexions, but the fact that the puns lead ultimately to unpleasant recollections, thus showing why some member of the set of pun-linked terms has been forgotten through repression. The odd thing in the present case is that it is not clear why one name (Signorelli) could not be recalled, while others (Botticelli and Boltraffio), connected with the same force by the punning inferences to the objectionable thoughts, should obstruct themselves. What is left is the demonstration of a somewhat dubious punning skill in a case that is supposed instead to lay the foundations for a general theory of forgetting.

In the alius case we are prone to marvel at a turn of mind that can ferret out human problems and precautions from the slender evidence of a forgotten word. But in the Signorelli case we are more apt to marvel at the capacity to link up a fact, already known, with a chain of ingenious puns. But in both cases we are struck by a unique performance, rather than a process of inference from stable premises and by replicable methods.
the theory of Oedipus fixation. The boy, in fact, plays this game in a quite intelligent fashion with Freud, construing his own behavior in the light of what he is expected to say. The typical response of Hans is: 'If he thinks it, it is good all the same, because you can write it to the Professor.'

In the earlier Freud, of the Psychopathology, The Interpretations of Dreams and Studies on Hysteria, one sees a collection of often brilliant but ad hoc insights. Naturally, one supposes, there are other cases, not reported, in which the divinations went astray. Analysts are not prone to report their failures. In the successful cases, too, it is possible to see general patterns, tendencies and circumstances, in which people make slips, dream, or adopt strange habits and beliefs. But these themes do not as yet constitute anything so pretentious as a theory from which the observed slips, dreams and practices follow. That is, they do not afford a reliable basis for prediction, though they may serve as prima facie rules of interpretation. For the basis for prediction is still a study of the individual, so detailed as to defeat the application of laws stating uniform connexions of some kinds of experience with some kinds of behavior.

In the later Freud, the themes that emerge as common patterns in earlier divinations acquire theoretical standing. This change is required if psychoanalytic procedure is to live up to its scientific claims. So, for the hunches growing out of and tested in particular interviews, he substitutes laws of paralogical processes. These laws have the virtue of mechanical applicability; they can be learned in some unambiguous way by pupils of the analytic method. But applied in this spirit they defeat the kind of divining procedure, the pursuit of hunches, the shrewd guess, that gives point to the psychiatric interview. When an analyst attempts to convey what he has learned about the examination of patients, as Harry Stack Sullivan does in The Psychiatric Interview, for example, it is clear that he cannot tell the novice the exact circumstances in which a rule can be invoked, or an interpretation made. He can tell him that certain rules and certain cues are helpful, if applied with due care (and how is this to be defined?) to the case before him, but there is nothing here that has the clarity of rules governing experiments in physics or biology. Individual actions are the consequences of a whole history and all the complexities of the situation in which they occur. To invoke general laws presupposes a simple and general relation holding between limited numbers of variables, ignoring the differentia of individual cases. Consequently, if I wish to predict whether a given arrow from a given bow is going to hit its target, I will rely on my impression of the archer, and not on calculations which will include the wind velocity, the physical properties of bow and arrow, muscle, nerve and bone, and innumerable other properties which might, some years hence, fall into place in an enormously complicated formula from which I could predict precisely the outcome which occurred years before. Nor is a computer the answer here, for the problem is a matter of stating the exact nature of the relation among the many variables, and getting the values for them, and not merely calculating the consequences of the specific assortment of values. The difficulties have to do with programming, not using, the computer. We might say of the arrow thudding in the target that there are laws governing this occurrence. But we should not be able to say that the laws allow us to predict the precise consequence of any particular, but complex, situation.

The clinical use of psychiatric theory has the same disadvantages. If it is used as a way of predicting and explaining every remark, dream, or gesture, it blots out the idiosyncratic features of the case which in fact allow perspicacious description and prediction of particular actions or particular individuals on particular occasions. Laws of the paralogical process may, of course, relate to the actions of an individual. And so, to the clinician, theory may provide the rough outlines, the limits, of his approach. But employed in this spirit psychiatric theory consists of rules of thumb to be tentatively applied and, if useless, discarded. As rules of thumb, they cannot be used as determinants of what happens, but only as surmises that might make what happens intelligible, or as instructions to look out for certain things. The status of the theory itself is another matter which will concern us later. But it must be in this entirely flexible way that it applies to the clinical work.

If a baseball player wishes to improve his batting average, he does not go to the physicist whose laws govern what happens when a bat hits a ball, or to the physiologist, whose expertise entitles him to say something about the movements of his muscles as responses to the stimuli acting upon his eye. He goes instead to the batting coach who, though entirely ignorant of these theories, has the long observation and practice which enables him to diagnose the batter's particular difficulties. It is like this in clinical practice: experience and detail enable some psychiatrists, some writers, some husbands, some mothers to have something perspicacious to say about particular cases. Idiographic theorists are unhappy with this situation, just because it is both successful and ad hoc; it is not governed adequately by laws and criteria which would give it status as scientific

11 Collected Papers III, p. 215. There are two excellent critiques of the Little Hans case, the first by H. J. Eysenck, in the Spring 1961 issue of Innuity, the second in Philip Rieff's study, Freud: The Mind of the Moralist (Viking Press, 1959). The upshot of both critiques is that Freud has over-explained the case, and has been joined in this enterprise by the very clever boy.

The role of the variable is not difficult to understand. It might be supposed, for example, that a direct relation holds between number of trials and speed of solution of a maze. When the experimental evidence does not support such a supposition, one turns to rather more complicated mathematical techniques. As long ago as 1885 Ebbinghaus was introducing logarithmic equations in place of simple proportionality. In the same spirit, Hull's complicated formula relating 'habit strength' to trials, \( sHr = M(1 - e^{-kt}) \), introduces the base of natural logarithms (e) as a constant enabling the law to fit the observed data.\(^{14}\) It has no empirical or theoretical meaning in itself. Psychologists seem to be in agreement that variables can be introduced at will, if they facilitate a more generalized description or prediction. Nothing beyond pragmatic advantage hinges on the employment of such techniques. Consequently they have no explanatory significance, and do not raise particular problems for us. The construct is another kettle of fish. In standard sorts of experiments it is found that rewards, punishments, trials, and so on, correlate with learning speed. Now two attitudes toward such correlations could be taken. A thoroughgoing behaviourist might insist that the whole story is to be found in these correlations. So Thorndike's law of effect, according to which an organism tends to repeat actions that lead to pleasurable consequences, could be reformulated in terms of reinforcement, according to which the consequences which count as reinforcing are just those which show up in correlation with learning and other behaviour tendencies. If Skinner's reformulation does not ring true it is perhaps because we know, at the start of an investigation, the sorts of consequences which would count as reinforcing, and it is odd to think of a rigorous operational redefinition of concepts and an experimental test of reinforcement hypotheses leading to such platitudinous conclusions.

I shall return in a moment to strict behaviourism. For the present, I wish to consider the views of those who regard the correlations of responses with stimuli (however considered) as the basis for the elaboration of theoretical links joining them. Sometimes, perhaps, links of this kind are felt to be necessary as a result of a mentalistic prejudice. A complete account of behaviour must surely show the mediation of thoughts and intentions, motives and desires, sensations and perceptions, between external stimuli and observed behaviour. Tolman's generous allowance of mentalistic variables would be an instance. Such theories wish to assign a place to a concept like motive, for example, which both redefines

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\(^{14}\) Taken from E. R. Hilgard, *Theories of Learning* (Appleton-Century Crofts, 1956), p. 372. The other variables are: \( M \) = upper limit of habit strength, \( t \) = trials, \( k \) = constant expressing learning rate.

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the term in the language of physical occurrence and still allows it the force of a 'mental' concept. I shall postpone treatment of this use of such concepts, partly because R. S. Peters, in *The Concept of Motivation*, and many other recent philosophers, have argued the case for the logical impropriety of such theories with great effect, and partly because it will suffice to see them against a different interpretation of mentalistic concepts later on (Chapter VI). In any case, I do not believe that the mentalistic howler is committed as often by psychologists as Cartesian myth-hunters among philosophers pretend. The drift toward constructs has more to do with the attempt to avoid platitudes in the formulation of S-R connexions than with fixation on a Cartesian model of thinking. The typical laws of learning, for example, are strikingly like observations any one of us could have made without benefit of research. Animal trainers and parents take for granted in their day to day dealings with animals and children that behaviour can be altered or conditioned by rewards and punishments. It is built into the concept of pleasure that people will repeat actions with pleasurable consequences; we should regard them as peculiar if they did not. Thorndike’s law of effect is only a more pompous rendering of this platitude. We are also quite aware of the value of repetition, recency, association and all the other devices that psychologists supposedly have shown to relate to the learning process. In another day these were perhaps thought of as mnemonic devices, techniques facilitating learning in this and that case, though not to be interpreted as laws determining behaviour. And so psychologists have tried to suggest that their measures of habit strength, reinforcement, motive, and all the rest are not simply descriptions of clockable behaviour, which are discovered to have precisely the relation to learning that we already knew, but that they are indirect measures of other elements in the organism, elements which, in day to day psychologizing, ordinary men would never have stumbled upon. Such a concept is drive, another, as psychologists have appropriated the term, is motive. It is this desire for explanatory novelty, seconded by writings in the philosophy of science, that play up the role of theoretical constructs in scientific explanation, which accounts, I believe, for the prevalence of such constructs in psychology. The question is whether they do the job for which they are intended, i.e., whether they explain behaviour.

In some contexts, notably Freudian theory, psychologists of learning are rightly suspicious of the concept of drive. Their objection is that libido or psychic energy is put forward as a quantitative notion, yet remains essentially non-measurable. In contrast, they point with pride to their own experimental measurements, which have converted drive from a shadowy and occult force into an indirectly measurable concept. It is like, they say, the contrast between the Epicurean and the modern atom.

But there are difficulties. Atoms, electrons and other particles are described in a language common to observed movements as well, that is, velocity, direction of movement, position and spin. If drive is identified with what is measured, hours of food-deprivation for example, it is not quite clear why it is called a construct. If it is assumed to be an unknown quantity that increases with length of deprivation, its explanatory role is wholly redundant. For the construct only restates in a language of occult events what we observe when we see that an organism tries harder to get something it has been deprived of longer. If it is clear why adding to the time of deprivation increases or intensifies a certain behaviour, B, then it is clear by the same token why increasing drive or habit strength or motive affects B. If it is not clear why increased deprivation affects B, then neither is it clear why increased drive affects B. For all we know about drive is what is measured by time of deprivation. The alternative to redundancy is to suppose that by drive is meant more than what is measured. The 'more' in this case (which psychologists sometimes refer to as 'surplus meaning') is apparently the explanatory power which, in ordinary life, accompanies such commonplace observations as, men eat more voraciously when they are very hungry. If it is this surplus that explains, it is surely not something established by painstaking experimental procedures. It would be odd for the psychologist to claim that now, for the first time, we know that people eat because they are hungry, or pursue women because of sexual cravings, or a fast buck because of a desire to get rich quick. The horns of the dilemma of the drive construct are redundancy and platitude.

Concepts like effort, strength of feeling and desire, and thought are the currency of ordinary speech, explaining actions in piecemeal ways, without benefit of theory. They take us back to our initial observations, that most of our accounting of human action is *ad hoc*, but still sufficient. The grounds for their sufficiency needs still to be traced, but it is not the sort of thing that can be supported by the theories of psychologists. These theories are, by implication, causal chain theories. Deprivation produces a need, which gives rise to a drive, which pumps up the bodily machine to action. There are two ways of analysing the intervening terms. One way leads to an identification of need with deprivation, that is, we see as observers that the deprivation will have certain consequences unless something is done. The drive, in turn, is identified with the action, that is, with the propensity to behave in a given manner. If this
road is taken, it is up to the psychologist to show that the connexion of deprivation (or other numerically describable features of the situation) and action is (a) law-like and (b) extends our account of human behaviour. The other way leads to an identification of need and drive with physiological states. The first of these alternatives is behaviourism, in at least one sense of that term, the second reduces psychology to physiology.

Now behaviourism is in a sense the basis for a physiological science. It is a method for describing the movements that people and animals make without the usual ties of our descriptions to purposive explanation. Instead of talking about a person reaching out for a cup of coffee or a rat struggling towards its reward, behaviourism provides a way of talking about the movements of bodily parts through space together with the varying repetitions and speeds of such movements. These in turn can be connected with carefully measured and introduced (or withheld) items in the environment, like the amount of food, the time intervals of feeding, or the intensity, duration and frequency of shocks. This neutral description opens up a new view of human and animal behaviour; it sees it as preceding and following other events. This is of use if an older and a self-explanatory way of describing the behaviour muddles the issue. Talk about actions, purposes, desires, motives is no more amenable to the physiological account of behaviour than terms like hard, soft, fast and slow, green and brown are to a Galilean description of motion. The quantitative description of behaviour thus serves as a basis for rather more precisely worded inferences as to complicated neural processes than would be possible on an ordinary description of a man’s movements. So Hebb, noting that the lapse in time between a stimulus and a response is uniformly greater than that required for the actual transmission of nervous signals, surmises that something like a holding mechanism, familiar from telephone exchanges, keeps the message going, as it were, without immediately discharging it into the appropriate afferent nerves. The holding mechanism is a construct indeed, and a genuine one. It is of a logical piece with the disparate kinds of events it is supposed to connect and explain, and while it may not be observable in practice, it is not unobservable in principle. At least it is conceivable that some independent evidence of such neural structure and action might be forthcoming.

In this sense, then, behavioural psychology is a method for exploring the physiological structures that constitute the surmised links between stimuli and behaviour. It is not, in that sense, a body of laws or theory in its own right, but the procedure and the evidence that can become relevant to, and united in, physiological theory.

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Psychologists commonly eschew physiologizing, however; some because they hold that there are genuine psychological constructs like motive and drive, others because they feel that physiological knowledge and method of inquiry is insufficient to predict and explain human action adequately. These two views are often held together, but as we have already seen the danger of redundancy or platitude inherent in the supposition that there are constructs which avoid both common sense and physiology, we can confine our attention to the second of these claims. Sometimes these objections are empirical; physiological inquiry is in its infancy, and highly speculative, and it is not reasonable to expect that current knowledge and techniques can provide an adequate physiological explanation of S-R connexions. But in such circumstances it might be supposed that we ought, as psychologists, to busy ourselves with a physiological investigation and, until we improve our methods and enlarge our discoveries, refrain from talk about explanatory constructs. Many psychologists, however, reject physiology on logical grounds. Even admitting a more adequate conception of what is going on in the nervous system, it is argued that this information and the method of acquiring it will be much too complicated to be of service in accounting for specific human actions. Hebb, who is a chief exponent of the physiological tendency within psychology, provides these people with their best illustration. Interpreting or predicting gross and particular behaviour by means of neural processes would be like interpreting, describing and predicting a storm through a description of individual raindrops.

These reservations go back to the psychologist’s idiographic hopes. The psychologist talks a great deal about general laws which will enable him to predict, but he seems ultimately dissatisfied with any kind of prediction except that which will, in Allport’s way of talking, tell him what John will do next. He is also principally concerned with control, and here too he has in mind what he can do with a specific individual. A theory will satisfy him only if it will tell him exactly what to do in order to turn out a particular kind of person or promote a particular action. The odds do not favour this aim, except, of course, that in ordinary life we have a reasonable degree of success in such prediction. But this kind of prediction, as we have seen is essentially a business of assessment based on long acquaintance with a particular individual, and is not the short-circuited method provided by the application of general laws.

In any case, the psychologist’s hope is vitiated by the ambiguity of his use of the term behaviour. It is quite possible to isolate from the normal activities of human beings and animals types of muscular movement,
and to connect these with actual or hypothetical neural events, and these
in turn to classes of stimuli, for example, light frequencies or electric
shocks. This way of cutting up behaviour into units recommends itself
to psychologists because of its 'scientific' rigour. Unfortunately these
units do not add up in any way to the objects of the psychologist's interest.
This unit of interest is, in the psychologist's jargon, molar, that is, a division
of behaviour into practices, habits, and actions identified by the fact that
a given stretch of movements can be seen as purposive, meaningful or
intentional. Physiological description of human or animal behaviour, on
the other hand, is molecular, of which the elements are the spatio-temporal
properties of behaviour. Psychologists sometimes talk as if their choice
of a molar or molecular analysis is purely a matter of how finely you
want to cut the units. But there is no reason to suppose that a number of
molecular units add up to a molar unit or that a particular molar unit
subdivides neatly into so-and-so many molecular units. They are different
ways of cutting up behaviour into identifiable units of investigation;
they may very well overlap and so fail to contribute to each other's pur-
poses. Molecular subdivisions of behaviour provide precise descriptions,
for they are, by definition, measurable units of behaviour; but this does
not make them more precise units of the type formerly provided by
molar units. Part of the muddle here is to be found in the supposition
that when in ordinary life we talk about actions we are referring in a
vague way to what psychologists, with notions like speed of response or
muscular movement, are talking about in a precise way. As the rest of
this book will be concerned in one way or another with a reinterpretation
of the concept of action we may for the moment pass by this particular
confusion.

All of this leaves intact a kind of analysis of behaviour which, as it were,
is half-way between the molar concept of action and the molecular
concept of muscular movement, an analysis which is best seen in concepts
like speed of response, number of trials, and hours of deprivation. The
possibility of constructing a graph by computing, say, numbers of trials
against numbers of errors, or extinction responses against the number of
reinforcements, gives a mathematical means of forecasting learning
behaviour (provided it is properly interpreted) and so meets at least some
of the conditions of a scientific theory, mathematical formulation and
prediction. The mathematical description does not provide an explana-
tion of learning, though, if significant, the curves present some interesting
puzzles worth explaining. The curious fact about psychological research
is that so much argument goes on with respect to the proper shape of the
curves. There is, in other words, basic disagreement on the evidence,
and not merely on the proper way of explaining it. This is perhaps not
too surprising. Different experimenters use different subjects, and the
rates of learning and extinction might be thought to reflect differences in
the subjects not accounted for in the experimenter's variables. All the
various curves and the mathematical formulae designed to express them
may be correct for the particular range of subjects, but the total range of
response may be so wide that a generalized formula for these different
curves is totally useless. If, for example, some bodies in a gravitational
field were to move in circles around the centre of mass, others in ellipses,
others in ovoids and still others in negatively curved lines it would be
possible to construct mathematical formulae for each of the cases, and, per-
haps, a formula which would average out the different movements.
But the generalized formula would be of no use in describing or predict-
ing any particular case. It is surely not that sort of generalization that the
scientist hankers after. The parameters must provide limits which give
the formula a significant probability. Skinner's studies of individual cases
reflect the anxiety of one who supposes that his generalizations ought to
have pertinence to cases; thus his investigations allow the extrapolation
of curves which will predict the next trial, but always of a particular rat
or pigeon. To suppose that a science of psychology will be a compendi-
um of mathematical formulae for individual cases is to defeat the very
point of scientific organization. This I suppose is why so many psycho-
logists have found Skinner's results so anaemic, even if in his procedures,
Skinner has avoided the pitfalls of constructs or common sense explana-
tions. His procedure leads back to the difficulties of idiographic theory.

But with a difference. Skinner's plan seems to be to replace the normal
human and animal environment with laboratory conditions. He con-
ceives of psychology as a piece of human engineering. He wishes to
demonstrate that, with adequate controls, any desired kind of behaviour
can be produced in a subject. We all know this in a general way, and
Skinner is the first to admit that his views are built on quite ordinary
conceptions of the function of reinforcement in everyday life. In ordinary
life, however, we take these principles of teaching and, if you will, con-
ditioning, as techniques that might do the job desired, not as laws that
explain the manifold behaviour of human or animal populations. What
Skinner has added to these commonplaces, that pleasurable experiences
are likely to be repeated, that rewards promote learning, that punish-
ment (which of course he disavows as a method) may block the formation
of bad habits, is a technique for making more certain that a reward will

17 In the ensuing remarks, I am relying particularly on Cumulative Record (Appleton-Century
have the desired effect. The experimental environment makes it possible to control and induce behaviour in a way not paralleled in normal environments. The regularities of stimulus-response connections in the laboratory can not then be extrapolated as more accurate measurements of what must also hold for the world at large. They are artificially produced, and represent what human or animal nature can be like under such artificial circumstances. Both the conditioning psychologist and the bureaucratic sociologist are engaged in the practical endeavour, based on some of the potentialities of their human subjects, of constructing an environment which will produce the desired response, or the desired conformity. But if the experimental situation is taken as providing the key test for a general theory of behaviour, these techniques will be interpreted instead as laws of human nature and of society, as the only ways that human and animal subjects behave. Thus experiment seems to produce what we have never had before, explanatory laws of behaviour.

Psychology's successes are essentially successes in the mechanics of control, and not in the explanation of normal human action. Both in psychology and sociology successful methods for controlling particular kinds of behaviour are confused with general laws explaining human behaviour. The conditioned animal thus becomes, not the special case of what can happen in an artificially controlled environment, but the paradigm case for human behaviour generally, and the social conformist becomes, not the specialized product achieved by the cunning employment of specialized techniques of social control but the inevitable result of the interplay of social forces and individual motives.

As theory, then, psychology avoids the redundancy-platitude dilemma only by changing the human environment. It is an enormous step from the rules applicable to these artificial situations to laws of human nature. When these rules are extended to apply to the general condition of human existence they result in the typical tautologies or platitudes that we found also to be characteristic of sociological theories. Toward the end of his survey of learning theories, for example, Hilgard formulates a series of propositions which he feels psychologists might advance without fear of disagreement. Some of these are clearly tautological. For example, his first proposition tells us that 'brighter people can learn things less bright ones cannot learn'. It is nowhere made clear how we could identify the brighter person save in his ability to learn more than his duller brother. Or again, number two tells us that 'a motivated learner acquires what he learns more readily than one who is not motivated'. The tests of motivation are so intimately bound up with successful, avid, or persistent performance that it is difficult to understand this statement in any sense other than that a person who learns readily learns readily.

Others among Hilgard's statements are just as clearly platitudinous, in the sense that our use of certain kinds of concepts commits us to these propositions without further research. Thus number three: 'motivation that is too intense (especially pain, fear and anxiety) may be accompanied by distracting emotional states, so that excessive motivation may be less effective than moderate motivation for learning some kinds of tasks, especially those involving difficult discriminations'. A person bothered by great pain is not likely to attend to a task very well. So, he goes on to say, learning under control of reward is usually preferable to learning under control of punishment, a statement which is often taken to reflect a new break-through in learning theory. But a moment's reflection will lead us to see that it is rather difficult to think of pain, anxiety and punishment except as distracting. We know the distracting and obstructing effects of pain on learning or any activity, in knowing what we mean by pain.

A number of Hilgard's items relate to the old formula, practice makes perfect. Any concert pianist, athlete or student must know that (number 11) 'there is no substitute for repetitive practice in the overlearning of skills . . . or in the memorization of unrelated facts that have to be automatized', or (12) that 'information about the nature of a good performance, knowledge of his own mistakes, and knowledge of successful results, aid learning', or (13) 'transfer to new tasks will be better if, in learning, the learner can discover relationships for himself, and if he has experience during learning of applying the principles within a variety of tasks', or (14) that 'spaced or distributed recalls are advantageous in fixing material that is to be long retained'. We all do these things in trying to learn a subject-matter or acquire a skill. We must practice; we must know, approximately at least, what is aimed at if we are to know if we have achieved it. It helps to figure things out for oneself, or to be able to apply something learned already to a new subject-matter, or to go at a subject, especially involving long memorization, in spurts, going away from it and coming back. It would be difficult to reach any kind of goal in life without such procedures. It is difficult to imagine why refined techniques of experimental inquiry should labour to bring forth such mice. It is the same with the platitudes in which Hilgard points to the importance of realistic goal-setting, the advantage of balance of successes over failures, or active participation of the learner. Realistic goal-setting is, in the first place, part of what we mean by getting a balance of successes.

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18 Theories of Learning, pp. 486-7.
and I take it that if one consistently fails it is not long before one gives up a task. The importance of active participation has been debated in schools of education as if there were an issue to be decided. The learn-by-doing thesis is not a profoundly new discovery about human learning, but the observation that learning is, after all, a species of doing.

Finally, number eight, Hilgard's catch-all: 'The personal history of the individual, for example, his reaction to authority, may hamper or enhance his ability to learn from a given teacher.' This is less a generalization than a repository for an indefinitely large cluster of particular statements about the relationships between students and teachers. The generalization does not here support the particular assertions but merely summarizes those well known though *ad hoc* cases in which we find it difficult to attend to a certain manner of lecturing, or cannot be bothered to respond to a certain style of exhortation.

In short, something is wrong with a science that lists such propositions as items to its credit. These statements suggest instead how tautology is embraced in order to secure general truth, platitudes to avoid irrelevance. If the psychologist's statements have substance, it is as policies for achieving certain aims, which may or may not be undertaken, and hence may or may not form the way in which humans learn or acquire other habits. Explanation of human action still resides, for all the efforts of psychologists, in the *ad hoc* pronouncements which we make with confidence about the motives and intentions, desires and anxieties, the anticipated pleasures and the felt pains which account for this or that human action. It is possible to imagine the laborious analysis of behaviour into measurable units serving as the structure and the evidence for physiological hypotheses. But psychology as a science with its own explanatory laws, falls in a no-man's land between physiology and the *ad hoc* deliverances of everyday life.

**Chapter Three**

**INTERACTION**

In the last chapter I tried to show that redundancy and platitude are the consequences of typical attempts to apply wholesale the techniques of the natural sciences to explanation of human behaviour. This suggests that something might be radically wrong with the conception of explanation borrowed from physics and the philosophy of science, that is, the view that to explain an occurrence is to bring it under a law, to explain a law is to bring it under another law.¹ The law may be a generalization that embraces a great number of particular instances; thus the fragility of glass might be cited in explaining the shattering of a certain glass. Or it might be a mathematical formula, the values of whose variables arrived at by measurement provide descriptions of a great number of occurrences or predictions of a great number of future occurrences.

If this account is taken as orthodox, something must be done about vast numbers of explanations offered in ordinary life of human actions. For these apparently *ad hoc* appeals to this or that purpose, desire, or interest do not appear to be supported by anything like genuine laws. One way out of the difficulty is to take the view that psychology and the other sciences of human behaviour are late comers on the scene of human discovery. We simply don't know enough about behaviour to offer proper explanations. This view, like most philosophical revelations, has an air of paradox. Surely we do know perfectly well why someone is walking down the street when he tells us, or we discover, that he is going to the store to buy groceries. At the same time it is equally paradoxical to suppose that these quite confident assertions are really supported by law-like statements after all. For example, in *The Structure of Science* Nagel wishes to give an explanation of Cassius' plotting against Caesar in probabilistic terms, thus bringing it within the favoured conception of explanation. 'In ancient Rome (he says, pp. 22–23) the relative frequency (or probability) was high (e.g., greater than one-half) that an individual belonging to the upper strata of society and possessed by great hatred of

conclusions reached or actions undertaken rather than with a method of arriving at conclusions or deciding to act. Conclusions or actions are rational only if they are right. If they are wrong, it is supposed that the person acting or making a claim is succumbing to desires, interpreted as quasi-physical or Humean internal impressions, as events which could be linked causally to actions or speech. As a result, insufficient attention is paid to the possibility that rational action, and morality itself, might be defined or characterized in terms of procedures as against the consequences that match or fail to match particular moral standards. On the procedural view, a man whose actions are guided by his assessments, and his understanding of his own and others' actions by the grounds he finds for those actions in the situation of the actor, is looking at behaviour morally. So long as he describes his own and others' conduct as doing something well or poorly, effectively or clumsily, appropriately or mistakenly, he is a moral agent or observer. It may be that the grounds he discovers as the end products of his diagnoses shock or offend various moral sensibilities; but this is relatively unimportant. The point is, he thinks in terms of grounds. He acts or describes action, not by seeking temporal antecedents or functional dependencies, but by deciding that the situation entitles a man to act in the way he did or is likely to do.

The typical language we use to convey such a way of looking at behaviour is the language of motive. The phrase 'having a motive' does not imply that a person did, is now doing, will do, or has a disposition to do the act for which he has the motive. It implies rather that the situation in which the person finds himself would justify him acting in a certain way. The typical son of crime fiction, cut out of his father's will, has a motive for doing away with dear old dad; the girl a motive for getting even with her jilting lover; the dishonest bank clerk a motive for fixing the books.

A number of things can be said about our use of motive. First, when we ascribe motives to agents it is not at all like what we do when we speak of antecedent circumstances as the cause of events. Motives do not necessitate actions. Second, motive explanations are often post hoc; they follow upon the acts explained, without having to be known in advance of the acts to which they are applied. Third, ascriptions of motive are not inferences or hypotheses designed to account for observed tendencies in behaviour. There is an element of generality in our ascription of motives, but this generality is not of a statistical or nomothetic form justified by the instances accumulated in its support or the deductions made from it. The generality in this case is rather in the form of a moral principle or aim which is seen to bear on the situation. Rules and aims
apply to actions *prima facie*; they are neither moral absolutes in the manner
hankered after by Kant, nor deterministic in the way taken to be character-
istic of physical laws. But they can, when the action and the situation
match the principle or aim, be decisive. They are principles of justification,
of excuse, or entitlement, though I hope to use these terms without
the implication that I am endorsing the actions that come under them.
I understand the son’s rationale for killing the father who disinheritst him.
I can admit his provocation without benefit of studies of the actions of
his age, sex, or social group or an elaborate plumbing of his past, simply
because I understand that sort of situation to be provoking. At the same
time, I may deplore the measures he takes. When I ascribe to him
motives of anger, indignation, despair or monetary interest, I am offering
a moral explanation.

A view common to writers of ethics and philosophers of science stands
in the way of accepting this thesis. Describing and reporting, so goes
this view, is one thing; assessing, evaluating and judging quite another.
By psychologists and social scientists this view is expressed in the slogan,
science must be value-free; among writers of ethics in the slogan, value
cannot be deduced from fact.

Now it would be absurd to deny some of the arguments and examples
that fortify this thesis. We want to prune the historian’s account of his
Whig or Tory biases and discount as misleading the missionary’s colourful
descriptions of native licentiousness. Similarly, we want to block the
moves from the facts of evolution to the moral endorsement of later
stages of it, or from successful to right. If we begin with cases which
clearly exhibit the corruption of facts by moral attitudes or the irrelevance
of the facts to a moral thesis, it seems possible to generalize this No
Trespass into a permanent boundary between values and facts.

The gulf is widened if an attempt is made to define rigorously what
counts as fact, the sort of attempt upon which we have cast some doubts
in the previous chapter. For the essence of that attempt is to provide
criteria for a fact or datum entirely free of context. I have argued that it
is only context that nourishes our conception of facts, observations, or
data. It thus remains possible that what we call facts may in some cases
arise within a moral or appraising context, that moral conceptions provide
a way of looking at the world and so describing and explaining it. This
will be my thesis with regard to desire and need, pleasure and pain,
motive and reason. But let us first turn to cases that do not involve us in
the additional business of explanation; that is, cases in which an appraising
term only describes.

One application of this notion is to the theory of perception. Smells
and tastes are normally described by means of evaluative or commending
terms like sweet and sour, acrid and bitter, and the other senses too
occasionally admit of such descriptions, for example, soft and hard,
piercing and dulcet, bright and dim, and so on. It may be, though I
throw it in as an aside, that much of the temptation that arises in philos-
ophy to call what we sense subjective arises from the fact that we recog-
nize and identify sense-qualities by means of such appraising terms, which
are notoriously subject to context, so that we are unable to describe
dsensations with confidence for someone else, or even, perhaps, for our-
selves in other circumstances. Thus colour properties are eminently
objective, because they are freed of evaluative contexts at least much of
the time. There is a story here of interest to epistemologists, but our
theme takes us in another direction.

In a superficial sense, the view I am advocating here has been frequently
held by behavioural scientists. Our concerns with human agents, it is
argued, are moral concerns. In his *Textbook of Psychology*, for example,
Hebb claims social conflict and mental disorder as the chief objects of the
psychologist’s interests and the rationale of his investigations. Social
order and mental health are thus values directing psychological inquiry.
This is not quite what I have in mind. Medicine, too, is concerned with
values like survival and health. But its nomenclature is physical and
value-neutral. For the value concept of illness has been restricted to cases
that can be defined in terms of disease organisms, fever and toxic condi-
tions, and so identified and described without benefit of our attitudes
toward them. It may seem conceivable that Hebb’s concern with mental
illness and social conflict—from gang wars and criminal behaviour to
international conflict—could be translated into a concern with disease
entities or conditions, for example peculiarities in blood composition,
so that bio-psychologists could be poised with the appropriate hypo-
dermic to restore normal functioning.

The trouble is, the concept of mental illness does not accommodate
itself to this translation. Suppose, for example, that the blood theory
prohibited the application of the medical concept of illness to hysterics
or obsessives, or even to that particular kind of babbling insanity found
in backward patients. Would we say, nothing wrong with them after all?
The difficulty of accepting such a consequence reflects the intractably
moral use of the term ‘mental illness’. Our use of the term does not
indicate a vague attempt to get at an as yet poorly recognized physical
reality, which psychiatrists will supposedly some day apprehend, but a
sense of something having gone wrong with an individual’s performance
in one or many of life’s roles. Whatever the underlying physical basis, we need a term to denote these failures to achieve standard human performances. And there is no a priori reason to suppose that the physical correlates coincide with our social and, in this larger sense, moral needs.

The disturbances of mental illness, like their counterparts characterizing delinquent, criminal, business or political behaviour, are social problems. They do not match, necessarily, matters of physical disability. They are singled out for notice because they are unconventional, and depart in some way or another from a model of action that supposedly contributes to a properly conducted social order. Since the conception of social propriety is highly variable, what counts as mental illness, and all the rest, is also bound to vary. Consequently it is impossible to give sense to the hope that these variables will correlate with physical constants.

We need to recognize, then, that in observing and diagnosing human problems what is observed is productive and destructive, industrious and lazy, brave and cowardly behaviour. These are value judgments, to be sure, but they are also descriptive. Such labels are perhaps at the mercy of more capricious application than are those that seem to qualify as descriptions under positivistic restrictions. For they do serve as epithets, and the temptation to use them without careful attention to the standards of their use can easily lead one to suppose that they cannot, in any case, be applied with propriety.

Consider an interchange in which one colleague says to another, ‘You’re a coward, or you’d tell Jones that he doesn’t have the ability to work toward a degree’. And the man answers, ‘I’m not a coward, I’m only sensitive to the feelings of others’. Is one saying foul and the other fair, and that’s the end of it? Of course it is a case of foul and fair; but there is a matter of fact here which is very much bound up with calling foul and fair. A spectator might, without taking sides, see something in the actions of the man which would lead him to say, ‘Yes; coward’ or ‘No, it isn’t cowardice, it is a reluctance to hurt others, for look: here he was brave, and here and again here’. There are many possibilities of describing the action, all of which would involve using terms with such pejorative or laudatory possibilities, yet using them in ways which we would say illuminate or accurately describe a man or a situation. If we deprive ourselves of words because of their possible use to form attitudes or incite actions, we deprive ourselves also of what is often the relevant description. The man or situation is not seen and then appraised, or appraised and then seen in distortion; it is seen morally. Value and fact merge.

For reasons of precision, case of manipulation or success in forecast, we may wish to substitute physical, value-neutral terms of description for moral concepts. But we are then redefining our point of view, we are seeing the world differently, and we cannot suppose with complacency that the new precision will do what we were trying to do with the old and alarmingly vague vocabulary. It would be unreasonable to expect chemical categories of mental disorder to preserve the kinds of distinction currently enshrined under such labels as paranoid or schizophrenic, hysterical or compulsive. Paranoia depends on the notion of current or rational inferences which the paranoid transgresses, schizophrenia on standards of correctness in perception or propriety in mood. Even if there were a rough identity of reference, we should be misled in speaking of blood chemistry as the cause of errors, for it would be irrelevant to appeal to causes as grounds for correctness. To speak of paranoia and schizophrenia in the way that these terms contrast with proper ways of thinking, feeling, or acting is to use them in an appraising sense, and it then muddles the case to suppose that we are really all along talking about events in a physical setting. This must not be construed, as it is by some psychiatrists, as immunizing psychopathological behaviour from physiological or medical inquiry. It is only to suggest that if we look at this behaviour from a physiological or medical point of view, we must discard the psychiatrist’s terms along with the method of appraisal, for these are not independent of one another.

Perhaps these remarks are best illustrated by the psychologists’ use of the concept of intelligence. Since the development of intelligence tests, psychologists have wondered what they measure. Do the scores really measure intelligence, or do they simply reflect how well an individual can do on a certain test? The question is peculiar because it suggests that something lies within, not disclosed directly by performances of this or that kind but, it is to be hoped, validly inferable from these performances. Perhaps we could ask instead, do the test scores provide a reliable index to a range of performances? Do they indicate, for example, academic performance? But psychologists labour under the need to mechanize. And as they have mechanized motives and needs into drives, so they have somehow wished to treat tests as the signs of occurrences, enduring patterns or episodes which determine a man’s capabilities. We do, of course, use terms like intelligence in an explanatory way—we say a man is capable of thinking cogently about relativity theory and quantum mechanics because he is bright or intelligent—and this seems to afford a basis for the psychologist’s way of discussing the matter. And we do
find, with the psychologist, 'surplus' meaning in the concept of intelligence, that is, meaning over and above the actual behavior of the subject in answering questions or solving problems in a certain time. Such discomfort is quite proper, for intelligent behavior is not 'pure' behavior in the sense that pigeon-pressing-bar or rat-turning-right might be described as instances of unsullied observed reactions. But surplus meaning cannot be assigned to ghostly entities either. Intelligence is not something of which the behavior is symptomatic, something inferred from behavior, but is exhibited in the behavior, providing that one is looking at the given action, whether it be problem solving, similarity noting, inference making, or block building, from the point of view of paradigms of that kind of behavior. These are paradigms of what it is to solve a problem or to be especially imaginative, in short, what it is to do something well or properly. To speak of a man as intelligent is thus to describe much of his behavior as matching canons of correct procedure. It is at once to describe and to assess an agent's performance.

We shall see that statements ascribing desire, need, self-interest and anxiety to human agents, and role and status, function and habit to social forms and processes, arise in the context of moral appraisal. They have to do with the rules and conventions by which various kinds of human action are identified and assessed. It is the tendency among behavioral scientists to think of value as a subtle and dangerous obstacle to the business of objective description of human action. So these scientists feel that if they set their values to one side, articulate them, and isolate them in a preface all will be well.1 But values do not enter descriptions of human affairs as disruptive influences; rather, they allow us to describe human behavior in terms of action. Inasmuch as the units of examination of human behavior are actions, they cannot be observed, identified, or isolated except through categories of assessment and appraisal. There are not two stages, an identification of properties and qualities in nature and then an assessment of them, stages which then could become the business of different experts. There is only one stage, the delineation and description of occurrences in value terms. We describe people as more or less intelligent. The various refinements of tests and measures simply make more explicit (if they are successful) what we are doing in our ordinary and rather more vague appraisals. If there are disputes between the advocates of different tests, it is not because one test is a better index of some further property, the shadowy thing which really is intelligence. It may be an empirical dispute as to whether one or another test affords the most likely index to some further performance of interest to the parties to this dispute. Perhaps the further interest is classroom performance or ability in a particular profession. This presumably is to be settled by matching test scores against some rating of that further performance. But more frequently and more likely the dispute is over what work, what performances, what abilities are to be prized. Perhaps one party is thinking of speed in calculation, another individual initiative, another care and accuracy, another breadth of knowledge, and so on indefinitely. Intelligence, as a word functioning in appraisal contexts, must clearly reflect a variety of conditions, circumstances and criteria. And what counts as intelligent action must thus cover, as Wittgenstein would say, a family of cases, joined, not by some common characteristic to be seen in the action or the actor, but by the attitude taken by the observer towards this action, that is, seeing the action as meeting some criteria of excellence. But what will count as fulfilling the criteria, hence what will count as intelligent action, will vary from criterion to criterion and context to context. Consequently it would be quixotic to suppose that one could locate, test and describe action in such a way that a general theory of intelligent behavior and of rational conduct might emerge. For what counts as intelligent in one context might clearly be quite inappropriate in another.

There is a remarkable passage in John Collier's _His Monkey Wife_ in which the incredible chimpanzee is complaining about psychological tests of her intelligence which consist of the one-box-on-another-to-reach-a-banana sort typical of Kohler's laboratory. Of course, she says, they don't tell anything about her intelligence. In the first place, perhaps she doesn't want the banana, and besides, what does that sort of thing tell about the deep and inscrutable thoughts of a chimpanzee? So psychologists fixed on speed of response may insult a subject who thinks of intelligent action as a matter of care or originality. This subject in turn will find his supporters among other psychologists who sponsor a different view of intelligence. The debate is not a matter to be settled by research; indeed, in a sense it is often not a matter to be settled at all, except to recognize that different performances are prized by different observers. The point is, a question about intelligence could not arise unless some performances were prized; it thus becomes pointless to try to set aside our preferences in order to decide what intelligence really is. Appraisal

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1 Cf. e.g. Edward Tolman's preface to _Drives Toward War_ (Appleton-Century Crofts, 1942).
is the means for identifying the object of inquiry, so far as this is concerned, not with muscular movements and nervous impulses but with actions, that is, performances. Actions are movements that can be done well or ill, and appraised as achievements or failures. Lacking these terms of appraisal we are no longer in a position to identify performances.

What we say about intelligence is a matter of importance for the psychology of cognition, which is, after all, a study of intelligent performances. To seek ways of following the normal routes of scientific inquiry and explanation in order to understand why a person is engaged in a performance is gratuitous. Unless the performer is in a position to understand what he does, he cannot perform. It would be like explaining the moves of a mathematician by recourse to generalizations on what this and other mathematicians do in this or that circumstance. But the mathematician takes the next step because it is the next step in his proof, a matter to be justified by appeal to the rules of inference, the definitions and axioms with which he operates, and not by appeal to what he generally does. The same could be applied to many normal human tasks, at least those involving discrimination, inference and judgment, the kinds of performance of concern to the psychologist of cognition. Such a psychologist might claim, of course, that his concern is not with the strategy of cognition (though this is the claim of a standard volume on the subject)* but with the connection of successful performances to other factors, like manner of learning, emotional state, or environmental circumstances. This raises further questions and invokes new concepts, but the new questions and concepts are no longer precisely within the field of cognition and spill over into personality and learning theory.

Cognition theorists, on the other hand, claim at least as their distinct province the analysis of the kinds of behaviour that count as intelligent performances, and the development of simplified models of strategies for dealing with them. These are essentially philosophical tasks, for the simplified models are, like logical theories, ways of stating precisely the criteria for what counts as a proper or intelligent performance. The explanation of a part of a performance is given in the conception of what the performance is, for example, solving a mathematical problem, working one's way out of a maze, or choosing optimum advantage among alternative courses of action. To be able to perform is thus to explain one's action. A psychology of cognition does not, in this sense, contribute anything to our understanding of performances. It may clarify the nature of the performance, but this is a task which, as I have said, is philosophical and not empirical. If, in short, intelligence is defined in terms of certain sorts of behaviour, it clearly does not explain that behaviour. If it is identified with test scores, it does not explain further behaviour, for test scores serve only as indices to further performances. So whatever the status and contributions of a psychology of cognition, it does not constitute an explanatory theory of human action.

We do speak of intelligence as the reason for a performance, however. But we do so in particular contexts, in which the force of the appeal has much to do with what is being denied. We are saying, for example, that passing the exam was not due to accident, or knowing the questions beforehand, or the coaching he got. We are saying, in effect, that the performance is to his credit. This too is a description, for it enables us to make numbers of inferences about the conditions under which the performance was undertaken. But it is a favoured description, that is, one involving and provided by the sense in which intelligence is a word of appraisal.

This is the matter of importance in everything that follows. We do not apprehend a world of facts, sense-data, or physical objects, describable wholly in terms of spatio-temporal or sensed properties, and then, according to our values, call out 'good' and 'bad', 'fair' and 'foul' to every discrete item as it passes our ken. Sometimes we identify and describe items in our experience by means of terms of appraisal. The reports of occurrences that issue from the application of these concepts I shall call moral descriptions. The possibility of offering moral descriptions of action, that is, statements which can count as descriptively true or false though governed by appraising conceptions, is of the utmost importance to everything that follows. For I shall be arguing that much of what we offer by way of explaining human action is moral explanation. The objection might then be raised, to be sure, the concepts you analyse bear the stamp of moral explanation, but it is this that argues most forcefully for the view that some other way of treating human nature is imperative, if we are to advance accounts of behaviour that have factual content. It is thus necessary to show that moral explanations have factual content, that they describe features of human experience which will be intelligible to any user of a common tongue. In a term like intelligence, or others like courage and cowardice, the kind of logic at play not only hints at this thesis but requires it. For these terms, in virtue of their appraising function, allow us to describe what people do and are likely to do in ways which would be quite out of reach without them. Those who contend that moral explanations are vitiated because they lack empirical

content must then explain our use of terms like intelligence by denying either their descriptive or their evaluative powers. Such separation, I have urged, deprives these terms of either use; so a prima facie case is made at least for the possibility and propriety of moral explanations. It will turn out that the value-fact dichotomy, like so many of the philosopher’s most popular legacies, does not carry a degree of conviction equal to the tasks it is usually called on to perform.

Chapter Five

DESIRE

1) Explain a role.
2) Define a state.

There are a number of terms which appear to play a dual role with respect to human actions. Such terms as liking, preference, desire, pleasure and pain, love and hate, anger and fear are employed to explain actions. But they are also used in ways that suggest that they are names for states of the organism or of experience. It is tempting to combine the two senses and argue that, since the terms do explain and appear to name occurrences, the explanations we offer by their means are standard sorts of causal connexions. Desires and the rest thus become internal episodes which cause human and perhaps animal organisms to react in certain ways.

There are difficulties that stand in the way of merging these two senses of terms like desire. In the first place, as internal episodes, they are private and lack the guarantees for which psychologists hanker; that is, they cannot be operationally defined. If, to remedy this defect, desire concepts are defined operationally by ‘unpacking’ them (as Ryle would say) into descriptions of tendencies or patterns of behaviour, they lose their explanatory significance. That a man persists in a line of conduct may be explained by citing his strong desire, but not if desire means only the tendency or pattern of behaviour in which he persists.

In the second place, even if we allow that internal occurrences always accompany our use of concepts like desire, liking and pleasure, it is not clear that it is by virtue of such episodes that we identify or recognize instances of desire. For all we know, the feelings of desire may vary from person to person or episode to episode, whereas the terms themselves are applied with at least some degree of constancy.

Third, there are formidable arguments, advanced by Wittgenstein and others, against supposing that these terms refer to episodes at all. And finally, most of these terms, in their explanatory uses, depend upon future occurrences, the objects of desire or pleasure.

Theories about desire and its cognates often become hopelessly confused because it is assumed that desire concepts belong to a common logical family. And this may be the case, for certain purposes. They all contrast in some way with physical events in at least one favoured sense of that term. But this similarity is apt to blind us to differences of major importance for our purposes. I shall treat these concepts, then, in piecemeal fashion, even at the risk of repetition as I move from case to case.
Chapter Six

REASONS

Descartes and subsequent philosophers make a customary distinction between reason and the passions. They join ranks with contemporary psychologists in supposing that the latter afford typical instances of causal explanation. I have argued that in itself there is nothing wrong with this until the concept of cause is identified with a special technique of investigation, so that an explanation in terms of feelings, emotions, or desires seems to require the procedures and the evidence supplied by an empirical science of psychology. Members of this family of concepts might be said to be used in causal explanations, in the sense that, had there not been a feeling, or a desirable consequence, or a situation warranting certain kinds of action, the action would not have taken place. But we understand the relation of the action to feelings, consequences or situations in our identification of them as feelings of pleasure, desires for objects, and grounds for emotions. There is no further discovery or technique for forming hypotheses required in order to draw the connection expressed in such forms as: Had I not hit my thumb with the hammer, I would not have offended your feminine ears, or, Had I not thought how good a glass of beer would taste, I would have been home earlier.

In seeing this, however, it is possible to see as well that the distinction between the passions and reason is not an easy and perhaps not an intelligible one to draw. Acting on emotion or desire is not at all clearly marked off from acting from motives or on purpose, or from intending to act. Even the most rational of moves, that taken in a mathematical proof, say, is explainable by the same pattern that we have seen to be characteristic of explanations in terms of desires and emotions. The mathematician explains his move, or the chess player his, by appealing to rules of inference, the nature of the problem, the rules of the game or the disposition of the pieces that justify or perhaps strictly entail his moves. The use of desire is governed less strictly by rule and relevant circumstance than the use of motive or intention, which in turn is not governed as strictly as the moves in logic or a game of chess. And so our ascriptions of explanations of actions are apt to be made more tentatively in the first cases than the second, and in the second than in the last. Since they are not quite decisive, it looks as if further evidence ought to be forthcoming. But if the provision of warrants is the kind of strategy required to explain these

cases, the decisiveness and scope of our entitling formulae will vary with the degree of uniformity and explicit convention characteristic of the kind of action to be explained. Thus we must be satisfied, sometimes, with less precise and more tentative explanations, for we lack the kind of agreement as to what counts as desirable, pleasant, provoking or threatening that we have with respect to appropriate moves in a scientific laboratory, on a mathematician’s blackboard or a chess player’s chequered squares.

I do not, then, in marking off reasons from desires by beginning a fresh chapter, make a hard and fast distinction between them. It is possible, even, to speak of reasons as causes of behaviour, for we shall see that motives, intentions and purposes are, like desires and emotions, causal in a sense that does not require a special science to establish. I shall begin with motive, a term of special interest to the psychologist, and then continue with concepts like intention and purpose, which the psychologist rejects as old-fashioned and pre-scientific ways of talking, largely because they are less amenable to his techniques.

Motive

In a casual way (Chapter Four) we have seen that motives are ascribed to individuals in the context of situations entitling them to act. Often, people do not exercise their license, but when they do are we not entitled to say that the situation caused the action? The old man and his cantankerous ways and bloated bank account goaded his son into killing him. There is nothing wrong with putting the case this way unless it is implied that behind such a statement lies the authority of a generalization about cantankerous fathers and quick-tempered offspring. In short, we need to free the concept of cause of its theoretical entanglements.

This is perhaps the easier of the two tasks that face us. It is possible to say that, had the father not been so cantankerous as to cut the erring son out of his will, the son would not have shot him. And surely this is a typical formula of causal ascription: no B without A. But we need not and cannot support this assertion by showing that, if A is not present, B will not occur, or that whenever A occurs B will follow. The range of reasons for patricide are too wide to admit any sense to the first of these procedures, and the temperament of sons too various to suppose that cantankerous fathers who cut relatives out of their wills always or 90 or 50 per cent of the time come to a violent end. Being cut out of a will by an irascible parent is grounds for murder, though these grounds may not be exercised. But when the grounds are present, when, to clinch the case, the erring son is drowning in debt, we have no compunction in citing
the motive, or, if you like, finding the cause of his action in these circumstances. Whatever our choice of terms, the situation can become impressive enough to allow us to claim that it explains the conduct by providing grounds for it. The more complete and consistent the pattern, the more likely it is that we will speak of the situation as providing the motive for the action. Given a degree of complexity and inconsistency in the environment, we are more apt to say, a motive. And so on, down to the point at which we can conjecture motives, but not assign them. It is in part this frequent opacity of motive in ordinary examination of human doings that leads philosophers and psychologists to suppose that a new technique is needed to divine motives or a more rigorous criterion to limit the candidates to motive ascription. But the remarks offered with respect to desire explanations apply equally here. Motive is the kind of concept that is bound to have doubtful and indeterminate applications. Our conjecture as to motive is not in the absence of evidence we might possess but don’t, but is a review of a range of possibilities where behaviour just is ambiguous. The fact is, we act ourselves and observe others acting on mixed motives, tenuous motives, conflicting motives. We often find it difficult to assign a motive just because we find little in the situation which could justify our actions and face the scrutiny of objective appraisal. Suppose a thief said, 'Before I got caught I thought I knew why I stole.' There was a time when the situation in which he found himself seemed to afford him grounds: he was poor, or poorer than most, society was based on obvious inequities, he had his rights. Getting caught led him to construe the situation rather differently. Not that the same considerations no longer obtained; he now saw his act in a context including wider dimensions, new facts, which somehow disrupted the simple warrant, I am poor, therefore I steal.

It is cases like these, with the progressive opacity of the entitling relation that characterizes them, that add fuel to the fires of simplistic or scientific reformulations. An example of the first is Ryle’s well-known reduction of motives to dispositions to act. There are numerous objections to this view which seem to be overwhelming. Among them we need consider only two. The first is that it makes it impossible to suppose that a person ever acts on a motive only once. Yet this is a very common and successful case of motive-ascription, which is easily handled on the supposition that attributing a motive to someone (including oneself) is seeing a situation as providing grounds for an action. The second difficulty is that on Ryle’s account motive cannot, except redundantly, feature in an explanation of action. For if his account is correct, to say that a person commits

an act because of a certain motive is only to say that he generally commits the act, and to explain why he generally commits the act is simply to say that he generally commits the act. But it is quite normal to appeal to motives as a reason for doing a repeated act, as, say, when the unhappy husband continually gets drunk because of nagging at home. The nagging is his motive, and it is his motive, as much the first or any single time as it is for the whole collection of melancholy occasions.

The scientific reformulation is provided by psychologists, particularly personality theorists. They have given a curious twist to the term motive. First, of course, they wish to introduce motive into general law accounts. But in addition they want to internalize the motive, which in ordinary use is only the case per accidens, as when a man’s headache is taken as his motive for upbraiding his secretary. In a hypochondriacal society, pains and other internal disturbances may seem to provide excellent motives, but there is clearly no reason why motives must be internal. That the psychologist finds it necessary to suppose so is connected with the urge toward generalization of motive accounts.

A good case is found in a recent book by David McClelland who, beginning his study in the usual methodological fashion, says:

We need some more unique index of the presence of an aroused desire for achievement. Ideally, of course, we might favor something like a 'psychic X-ray' that would permit us to observe what was going on in a person’s head in the same way that we can observe stomach contractions or nerve discharges in a hungry organism.8

Lacking this device we must do the next best thing, by cunningly contrived tests and measures trap the traces of the achievement motive even where we cannot quite hang on to the thing itself. It turns out in McClelland’s study that what he has trapped are the reasons for a person choosing one line of action or persistent habit over another. But these reasons entitle the agent to act as he does, so that we see him acting for these reasons. They are not theoretical constructs connecting otherwise unintelligibly juxtaposed observables. In McClelland one sees a half-hearted physiologizing which remains theoretical in an older and pejorative sense, that is, it remains fanciful. Moreover, the explanatory value of his relation of the achievement motive to the behaviour he wishes to study is dangerously close to redundancy, a problem to which he seems on occasion to be sensitive. The upshot of his study of societies, which he hopes will replace the old-fashioned and hopelessly unscientific ways of historians and economists playing the role of amateur psychologists, is that societies that have achieved (according to his measures) did so because

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they wanted to succeed. He partially escapes obvious pleonasm by identifying wanting to succeed with the recorded 'fantasies', as he calls them, of the subjects under examination. When they are not available for his own tests, he examines their poems and essays, art works and other means they had of expressing themselves. Certain scores on these tests correlate with the success score in the politico-economic realm.

A narrow interpretation of the relation, however, is then open to a variety of interpretations, for example, that the high success scores in fantasy or in literature are rationalizing their economic concerns, or reflecting their basic interests. If the achievement motive is identified with an independent measure, it shows only that individuals who succeed, or spend a good deal of time in such effort, think or dream about success. This is not too surprising, and, if McClelland were to observe closely, it is an observation made over and over again by psychologically naïve historians. If it means only that people who succeed want to succeed, then it is risking tautology to stick the 'because' into the proposition. That he does so indicates that he is burdening the concept 'motive' with two incompatible meanings; first, the explanatory sense, according to which a situation has been disclosed which entitles a person to act as he does, and second, the psychological sense, which is descriptive, and identifies motive with tendencies in behavior.

To avoid these alternatives, McClelland, along with many psychologists, introduces a third use of motive. He rightly eschews the purely descriptive role of motive, but, wedded to mechanistic explanations, is led to think of motive as a force which is indirectly measured by his fantasy and behavior gauges. Since Freud, he says (p. 38), 'the notion that motives are rational or can be rationally inferred from action' has been for ever destroyed. This is owed to Freud because of his discovery that motives 'are not what they seem', that a man can have concealed motives, or be motivated by considerations exactly opposite of what is revealed in his actions. This, of course, is part of the general polemic of Freudian theory, with its efforts to set behaviour in a rather broader context. Freudian theory does not substitute causes for reasons so much as it stretches the notion of rational to include features of the situation which would not normally be thought of as providing grounds or reasons for acting. This is the point argued in the previous chapter.

But Freud himself, and most psychologists following him including McClelland, have tended to adopt a way of talking which suggests that the newly discovered motives are irrational, for they clearly are embarrassingly unlike standard reasons for acting. To speak of them as irrational, however, is to speak of them still as reasons, wrong or inadequate reasons, it may be, but still appealing to warrants for acting. Freud and all psychologists, however, have tended to treat 'irrational' as synonymous with 'non-rational', a move which they share with most moral theorists, as we have seen. Then it becomes possible for McClelland to suggest as the best means of apprehending motives in their hidden lairs the ideal of 'psychic X-ray', and the next-best thing, inferences to such entities from carefully measured fantasy performances.

The term motive, I suggest, will simply not stand this kind of abuse. If there really were such events as McClelland would like to X-ray, we are back where we started, that is, we should have to discover a method of noting their occurrence and whatever properties they might have, and then see through repeated experiment whether they have any connexion with other events manifested by an organism. But it is not necessary to postulate such events to allow the explanatory force of motive. McClelland's general case, why should x succeed where y fails, is an extraordinarily complicated one not always requiring a motive-type answer. To the question, why did x pursue a line of conduct with such perseverance, the answer might be: the example of his father, the morality of the time, or failure in love. Any of these might provide motives, if suitable evidence is forthcoming as to the relation of son to father, the exhortations of priests and politicians, teachers and parents, of the details of private life. For each of these would explain why someone devotes himself wholly, and thus with some likelihood of success, to a particular activity. But McClelland wants nothing of piecemeal or rational accounts. Hence the internalization of motive; it is the only way he can purchase his precious generality, and justify his research as a means of explaining human conduct.

So much for motives and causes. Philosophical delicacy in this matter seems to result from accepting the view that a cause is an event invariably connected to its effect, so that causal ascriptions must always be backed by generalizations. This view, in turn, demands a hypothetical status for motives, unless we admit that in most cases motives are not assigned on general grounds. So motives become internal and dubious events, where they had been clear, but piecemeal, circumstances surrounding particular actions. If a philosopher comes to see that the use of motive depends neither on generalization nor on internal occurrences, he is apt to deny motives causal status. If neither of these conditions is necessary to speak of motives as causes, however, causal talk about the motives for action becomes innocent of Humean implications. Moreover, it is often pertinent to speak this way since the situation singled out as the motive is taken to
be necessary to the occurrence of the action. The use of cause in motive-contexts should not tempt us, then, to think of invariant relations. Assigning a motive is showing that circumstances warrant actions. The investigation of circumstances is often a complicated business, calling for the routine work and the sharp eye of detective and scholar, and turning up various results from case to case. The matter of warrant is general, in a sense, for it involves reference to rules, principles, maxims, like those governing our use of emotion words. But there is nothing absolute or hierarchical about such warrants, or any among them, that would allow us to single out the circumstances that always motivate action. This is a typical gambit of moral theories.

The doctrine of egoism is perhaps the most obvious attempt to offer a general theory of human motivation, finding self-advantage at the root of every action. Its possible triviality is skirted by claiming self-advantage to be a universal maxim. This is moral universality, but it is disguised, as in Hobbes’s physical universality, as if something forces us to act on our own advantage. No evidence is offered for the physical doctrine, for self-advantage is never identified in any coherent way with a class or range of events which can be shown to determine behaviour in every case. Its persuasiveness is borrowed from the inclination people have to regard self-advantage as the best possible reason for doing something. Thus a moral doctrine, which of itself might seem dubious, is dressed up in a mechanical suit. Stripped of this deterministic clothing, it stands among many competitors as possible grounds for acting.

Moreover, like pleasure, self-advantage is a vacuous aim. It is the form of an aim rather than a specific objective. Normally, when we say a person is pursuing his own advantage, we mean that he is pursuing objects incompatible with the advantage of others, like the case in which an employer seeks quick returns at the expense of the company and the employees. It is not, in other words, that such motives or aims are intrinsically selfish, though some aims have a more decided tendency to conflict with the interest of others. It is surely not Hobbes’s or any egoist’s intention to maintain that one ought to pursue that which conflicts with the aims of others, yet it is not clear otherwise what specific actions the egoistic hypothesis entails.

Egoism attempts to combine a number of logically incompatible aims: (1) to explain what men do; (2) to do so generally, and therefore (3) to give the explanation a mechanical appearance. The result is a theory which is either vacuously true, empirically false, or morally vicious. This, of course, in no way denies the more rough-hewn generalization on which Hobbes built his political theory, that men do tend to pursue aims that all too frequently are socially disruptive. This might be called a fact about man, but equally it might be called a fact about the circumstances and environment of human life. That profit-seeking is selfish is as much a fact about the limitation of resources as it is a fact about profit-seekers.

Egoism, then, is a typical consequence of the attempt to generalize and internalize motives, wresting them in the process from the moral context in which they explain. There are undoubtedly many sources of the temptation to convert and modernize motives, the preference for mechanical models to which Wittgenstein calls attention in the Blue Book being one. Our general view of a subject (like explanation) is usually governed by those cases which are tidiest, surest and most successful. Motive accounts seem defective because of their rough edges, their tentativeness. Often there is no basis for deciding among competing motives, and this leads philosophers of science and psychologists to suppose that in ordinary language we simply do not have concepts or techniques precise enough to get at the real motive. But this misconstrues the case entirely. If our assigning of motives is often tentative and obscure, it is because the canons governing the use of motive terms are themselves not entirely clear. For one thing, they depend too much on the nuances of particular cases to afford the grounds for precise application dear to the hearts of scientific-minded philosophers. We can admit this vagueness or tentativeness without adopting the corollary so often taken to accompany it, that we require in place of ordinary talk more precise definitions and methods of investigation to sharpen our apprehension of what is at present obscure. There simply are cases in which the ascription of motives is tentative and vague, not because our tools are inadequate, but because human action is often fundamentally ambiguous, often aimless, frequently equivocal. To suppose that there really is a motive which we have failed to read by ordinary means is to deny what we observe and practise in acting as we do. Only the quixotic quest for certainty could have sustained the long effort to convert motives into something else, which they could not possibly be.

The pursuit of self-knowledge is another instance of the attempt to convert moral reasoning into factual discovery. Very commonly, we
REASONS

engage in ruminations of considerable length which appear to have self-understanding as their goal. So it seems that such a process parallels research in science; it is the introspective counterpart of discovering and experimentally manipulating data. A few examples will show, I think, how such a conception has gone off its logical rails.

Recall the thief who, until he was caught, understood why he stole. It is clear as it need be here how self-knowledge, self-explanation, is tied to patterns of justification of behaviour. Stealing was consistent, perhaps, with his conception of himself as a moral agent, but being caught was not. It led him to see the act of theft in a less romantic, less convention-protesting way, and more as something embarrassing and humiliating. It became for him less a sin than a gaiterie. But, in any case, his reappraisal of himself was a moral reappraisal, not a discovery of hidden mechanisms, or psychic paramechanisms.

Consider another case. A man thinks of himself as a romantic hero, a conception not shared by his wife, and so he acquires a mistress. Partly it is an excused act, for doesn’t his wife fail him? But more than that, it matches a conception of himself—would it be misleading to say?—as a moral agent. Many moral philosophers would be horrified to consider this a moral conception, but that is where professional moralists have typically gone wrong. Morality, considered philosophically, needs to be thought of, not as that set of prescriptions for action or models for human agency that one affirms, but any pattern of argument, any model for behaviour that is of the persuasive or ideal-appealing form. To forget this is to misdescribe as caused or compelled by desires the behaviour of which we disapprove.

To continue the story. Pressures begin to mount. The mistress demands too much of his time, she is costing him money. He, poor fellow, is past the point where he can extricate himself without exposure and disaster. The action, originally so clear, so patent, becomes shrouded in mystery. Why does he do what he does? he mourns. Perhaps he goes to an analyst, or engages in gloomy self-accusation which he misconceives as self-research, and reads Dostoievski and Kierkegaard. But his troubles cannot be properly described as the dredging up of memories, hidden motives and other gems at the bottom of the psychic mine. Memories may, of course, feature in arguments through which he readsjusts the image of himself, and new principles of action may emerge from his reflections. It is always easier for him to accept new patterns of behaviour on the supposition that what he is doing is the result of the inevitable welling up of actions from psychic depths. So it is with the mythology of the unconscious, and so with the man of letters disguising his moral point

with the cloth of character, as if novelists had perceptions which we ordinary men lack. This heightened awareness is less oracular or divinatory than it is Hortatory.

The poets, says Thornton Wilder in The Ides of March, do not give us deeper insights but express more urgent longings, an aphorism which can serve us as a text. Character portrayal in fiction and epic is not designed to report inner truths, but to project a pattern of actions and sentiments, self-consistent and capable of withstanding the buffets of objections and circumstance. We come close to understanding this when we appraise the tragic hero as 'larger than life'. But our moral hyper-sensitivity leads us to forget that comic heroes, buffoons, villains are created on the same principles, Tom Jones as much as Ivan Karamazov, Panurge as well as Faust, Falstaff as well as Hamlet. If we think of the latter half of these pairings as deeper creations than the former, it is not because of deeper or more general truths about human kind contained in them, but because the noble, the distressed and the distraught seems to have greater moral appeal than the pleasure-seeking, riotous and sensual. But Tom Jones, Panurge and Falstaff are equally models of conduct, equally forceful and consistent and detailed creations which provide patterns of moral argument for specific actions and entire ways of life. The so-called psychological critics have mis-stated their case in supposing that they are assisting in the novelists' endeavours to find the hidden facts, like so many biologists uncovering the hidden secrets of life or physicists disclosing the elusive structure of the atom. All criticism is moral criticism if it is germane to the work of art, not in the sense that it praises and condemns from the standpoint of a moral code (for example, Wagner’s music is ‘bad’ because it is sensual), but in the sense that it helps to reveal the nature of the moral point of view embedded in the work. If the work of art is bad, it is because it fails to work out its moral theme consistently, not because it disagrees with our own.

Self-knowledge, then, is not a likely candidate for the study of some fixed thing called human nature. The physiologist might with some justice claim to talk about human nature, for he can with some legitimacy claim to have discovered patterns, functions and facts that account for the observed behaviour of the organism. Psychoanalysts and psychologists, writers, critics and philosophers, cannot in the same sense be said to provide the lineaments of human nature, conceived as permanent and enduring features of man’s life. They deal instead in the changeable patterns by which behaviour is justified. What counts as grounds for the moralist varies from time to time, person to person, and circumstance to
circumstance. Criteria of this nature cannot be supposed to have the permanence or fixity of the physiologist’s account.

The psychologist attempts to set the boundaries of a territory that lies between ethics and psychology. His typical concepts reflect, however, the questionable extent or existence of this territory. Drive, motive, purpose and intention represent decreasing degrees of physiologizing in the attempt to set these boundaries. The claim that these terms denote items to be discovered, directly or by inference, and that through their discovery we shall understand human nature, suggests physiological investigation. But the strategies of psychological investigations suggest that it is not physical but psychological occurrences which constitute the object of the psychologist’s investigations. The question is, are there such occurrences? There are candidates for this office, the chief of which is bodily sensation, which goes half-way toward the physical in having location, and half-way toward the psychological in its privacy. But we bring bodily sensations into an account of human action by seeing them as pleasant and unpleasant, desirable and undesirable, that is, from the moral point of view. So far as they enter into explanations of behaviour, they do so through justifying licenses, not through causal inferences.

**INTENTION**

So much for the generalizers and internalizers. Numbers of philosophers have found fault with psychological uses of motive and Cartesian internalizations of reasons for action. But in their alternatives, they have generally ignored the role of moral categories in describing and explaining human action. The relation usually envisaged between explicans and explicans of this type is logical. For these philosophers, rules, conventions, procedures and strategies entail, as opposed to entitle, the action to be explained. And this provides additional fuel for anti-causal fires.

This view is defended in a number of ways. A popular defence in recent years depends on the analogy of human actions to games. We shall turn to that analogy in Chapter IX. First, let us examine a concept which also serves to focus arguments against the causal interpretation of human action, namely, the concept of intention.

We may begin by noting that a man may intend to do something and not carry it out. He may have intended to catch the 8.15 to the city but missed it because his watch was slow. Such a case might tempt us to claim a similarity between intentions and motives. Motives, too, are easy to detach from those events which are alleged to be consequences of them. But intentions are attributed to individuals rather more intimately than motives. We may ascribe a motive to a man, as we have seen, without special attention to his words, actions or disclaimers. The circumstances, informed by some conception of appropriate grounds, allow the assigning of motives. But we would not attribute intentions to a man simply because he had grounds for acting in that way. Each of us, no doubt, has grounds for murder, larceny or other crimes, but we should be in a sorry way if we were accused on this account of intending to commit these crimes.

The fact that connexions of intention to action are frequently broken may seem to argue also against the supposition that intentions are causes. We are not disposed to ascribe causal efficacy to events that have no regular consequences. But the irregularity of connexion between intention and act might be thought to parallel irregularities that do also occur in nature. A housewife mixes all the ingredients for bread, but it turns out that the oven is not sealed or the yeast was stale. And so, in place of bread, a stone. It looks as if ‘he intended to catch the 8.15, only his watch was slow’ is like this. The reasoning is, if a normal causal sequence is broken, look for the intersection of the normal chain of events with some other chain that brings about different consequences. Ascriptions of intention could then be regarded as causal, differing from some other kinds of causal ascription only in the greater frequency of irregularities.

A major obstacle stands in the way of this temptation, however. If by cause is meant an event which, save for interferring conditions, has a given event as consequence, intentions fail to fit the pattern, quite apart from any decision as to whether intentions constitute a class of events. The man intends to catch the 8.15 today. It may be true as well that he intended to catch it yesterday, and so through all his drab commuter history. But the evidence we require today need have nothing to do with yesterday or tomorrow. We see him running from his car toward the platform, looking frantically at his watch, and cursing it and his luck, as the train pulls away yards ahead of him. We need not be utterly confounded by the failure of the Humean criterion of cause to apply, any more than we need be by its failure or lack of pertinence in cases like hitting and striking. In any case, we don’t observe the interaction of intentions and action.

Perhaps intentions are logically tied to actions. We should certainly be hard put to it to give any other than the Ryleian answer to the demand, Describe your intention. It is: to mix a martini, catch the 8.15, go to the opera, and so on. Descriptions of intentions are descriptions of actions. But, it might be asked, what about intentions not carried out? There is no special difficulty here, to describe the intention is still to describe the action, only in this case, an envisaged action. Envisaged actions clearly depend on performed actions for their meaning, just as unspoken thoughts depend upon first acquiring the power of utterance. Special means for
divining intentions seem out of place. An intention may be hard to read in action because of the complexities of conduct in which it is embedded. But if it is to be discovered, it is a discernable action.

If this is so, however, intention has no explanatory significance. Intentions are ways of describing actions, not explaining them. Ordinary circumstances bear this out. If I am asked, why are you going to the movies? it is no answer to say, because I intend to. This the questioner understands in being able to describe my actions as 'going to the movies'. Sometimes avowals of intention enter into an explanation. If I am raising a loan, I might explain it by saying I intend to buy a car. But this because the sequence of actions which lead to the envisaged goal is complex, and protracted in time, so that one sometimes needs to be informed that the observed actions are designed to lead to certain further actions or occurrences. Still, by giving the intention, the whole sequence is described. To talk of intentions is thus to answer the question what, not why.

We might speak here, also, as Anscombe does in Intention, of mental causes, by which she means causes known without observation. I answer the doorbell, brush a fly off my nose, or recoil from a snake coiled in the grass. The actions might be described as intentional, though the latter two cases merge into involuntary reflexes. (A reflex action might fulfill an intention; but we might just as well say, fulfills a purpose, and postpone discussion until we come to that term.) But the intention in these cases would not be part of a causal chain, for example, doorbell ringing—intention—answering the door. The implication in Anscombe's account is that because the actions are intentional they are causes known without observation. Yet she admits that the patellar reflex can be known without observation (for example, to a person with his eyes shut). The contrast intended is not clear. It seems she might mean 'known without observation', known without repeated observation, that is, known without research. This would create difficulties only if one were already committed to the Humean view of causation, while admitting to embarrassment in the face of cases like answering doorbells and awareness of patellar reflexes. The fact is, we observe many causes without research, and many of these have nothing to do with human behaviour, but solely with physical interaction.

There is, then, no special reason to encumber ourselves with a term like mental cause. Actions may be intentional, and may follow upon physical cues or causes. It would be pretentious to insist that a person had formed an intention in order to account for the link between doorbell ringing and rising to answer it, even more so, to speak of the intention as the link between the fly and the act of brushing it off. Notice here that the causal link answers the question Why, the intention the question what. Someone might ask 'Why are you waving your hand like that?', and get the answer, 'There's a fly on my nose.' To the question, What are you doing, the answer would be 'I am swatting at a fly'. The explanation is not the intention, but a description of the action as intentional already assumes a kind of explanation. The explanation is part of the description.

If appeal to intentions explains what is being done, it does not explain causally, but by making an action intelligible. We say, 'I did x because of y' where y and x must be different events. (Otherwise, are we not involved in circularity?) The point of the causal location is to connect one event with another. There is no particular reason why the events connected should be of the same type, as if, when our effect is an action—answering the door—the cause must somehow be mental. In the first place, both the cause and effect are physical. Both are observable; both occur somewhere at some time. In the second place, it is never quite clear what a philosopher is trying to contrast when he contrasts movements and actions. 'Doorbell's ringing' is a way of talking which clearly depends upon conventions, meanings, and the like. But it is none the less an observable occurrence. The blow on the knee with the rubber hammer also depends upon conventions, meanings. We shall see in a moment that the distinction of action and movement enters in another way. But it is clear that the general distinction between action and movement rests upon some conception of pristine observables, a notion already dismissed as incoherent.

Still, we say something different about events when we speak of them as intended, and when we speak of them as caused. It has only been shown so far that a description of intention does not exclude a causal account. But we need to explicate the sense in which an intentional description makes movements intelligible, and thus explains them. The clear sense is the one in which an action is puzzling. In a very funny scene from a recent film a bicycle-riding postman is plagued by a bee. He is watched from a neighbouring hillside by a puzzled farmer, who only sees the postman's frantic gestures. In a minute, the farmer discovers for himself why the postman indulges in his antics, for the bee comes up the hillside. It is as if we might say, where we observe the occasion of the action it is no longer puzzling. Seeing the bee entitles us to interpret the arm-flailing as 'trying to brush off the bee'. Hearing the doorbell ring entitles us to call the action 'answering the doorbell'. The relation of bee and doorbell to action is in such cases not causal; that is, our understanding the movements as intended is not merely another way of
saying that the bee or doorbell caused the actions. (Though we could say this.) Rather, the bee or doorbell provide the appropriate circumstances to such actions. We say, that is the sort of thing to do in such circumstances. The bee and doorbell, looked at this way, are logical cues to the action, for we think of bees as the sort of companions to be rid of, and ringing doorbells as the sort of signals for answering doors. Notice here how much of psychology is a matter of what we could say, by converting the way we do talk to the form proper to causal talk, but what we do not need to say, given our ordinary techniques and information.

The doorbell case leads directly to a convention. That is what bells are put there for. It might appear, on the other hand, that the buzzing bee does not mean flailing arms, but rather that the bee's sting creates a desire to be rid of the pesky insect. But even here, though we have taken the issue back to such rudimentary human experiences as pain and fear, the force of our account is the force of justification. The relation of pain and fear is not causal, for it does not involve research into regularities, but depends upon identifying pain as that to be avoided if possible. For all practical purposes—except with untutored children, psychologists and sociologists—we might as well have stopped with the bee as the thing to be avoided. We learn by experience to segregate the stinging and non-stinging bees, poisonous and non-poisonous reptiles. We might, for philosophical reasons, wish to unpack 'That's a bee' into an argument of the form, 'Bees sting, stings hurt, therefore avoid bees.' The connexion between the hurting and the avoidance is not like that between the sting observed in the abdomen and the pain and swelling its plunging into the skin causes. 'Pain' involves 'avoidance'.

A number of philosophers have recently advanced the claim for practical, as against contemplative, knowledge. Anscombe (Intention, §§ 32–36) in particular notes the peculiarities of practical syllogisms, which have directives to act as their consequences. To render such arguments formally valid, a major premise is required which in fact no one who takes appropriate action (and therefore understands the point of the persuasive argument) would accept. Any other rendering of practical reasoning is so hedged round with qualifications that the arguments seem hardly necessary. This is because the terms that are used in most practical reasoning are learned in the context of acting in certain ways. So 'it's pleasant' includes the notion of worthy-of-being-pursued, 'I want it' includes actions taken toward achieving (or trying), (cf. Anscombe § 36), and 'That's a bee', the notion of getting rid of it.

All of this is a matter of appropriate contexts. And this is what has bothered so many philosophers. For they have wished to offer a criterion for the use of a term on the supposition that, if one could not be offered, our successful use of the word would be paradoxical. The difficulty has been a failure to realize that we learn terms in context, and our application of a term is by an analogy to the paradigms which inform our further applications of it.

We might then explain a person's action by saying 'the doorbell's ringing', or 'A bee is buzzing round his head', without research or the unpacking of argument. Here it is pretty clear that the relation between the 'cause' and the action is one of _entitlement_. The action is appropriate to the circumstances in accordance with either standard cases or criteria. Actions are movements seen and identified as warranted or not by circumstances. Movements, in a sense opposed to actions, are events seen as instances of interaction—push-pull, contact, collision. If I ask 'What happened?' upon hearing a loud crash in the kitchen, and my wife says 'The wind knocked over a vase on the sill', the account is of movements because it appeals to interactions with other things, processes or events that have spatial and temporal location. By extension, the same might be said of the more tenuous uses of cause described by Hume, that is, where regularity of connexion is substituted for observed interaction. If she says, 'I'm emptying ice cubes', the account is of actions because of its ties with warrants for an occurrence.

There are many borderline cases. Someone laughs hysterically and when asked why says, 'My cat is rubbing against the soles of my bare feet'. We might wish to speak here of cause and effect; equally we might be inclined to think of laughing as 'natural,' that is, as a consequence warranted by the circumstances. We call it helpless laughter; yet we do not mean helpless in the way that a man falling from a second floor window is helpless, quite unable to alter the speed of his fall. It makes sense to speak of controlling one's laughter. Might we say laughter is expressive behaviour? It is designed to convey information. The same holds for a cry of pain, which can be suppressed if circumstances warrant. Sometimes, it is true, we say that a cry of pain, a laugh, a gesture, is involuntary. But this only shows that the voluntary/involuntary distinction does not have the force of the caused/free distinction. The distinction between voluntary and involuntary is generated by the concept of free action. For consider the involuntary cry of pain. It is not like the jerk of the leg when tapped; it is certainly not like the body falling from an upper window. It might be described better as a conflict of intentions. One wants to let anyone nearby know that one is hurt; one also has special
reasons for not letting the particular person nearby know. Suppose that in the course of a much desired massage, the masseur actually pinches a muscle. The cry of pain is involuntary only if it would have been suppressed in order to insure that the massage did not end. That is, it can be understood as involuntary only against a background of the action which would have accomplished the desired result or preserved the desired circumstance.

Anscombe and Melden both consider interesting cases of the involuntary which could not be accounted for this way, for example, the peristaltic motions of the gut, the involuntary recoil of the hand from a hot stove, and the familiar patellar reflex. Perhaps these are all cases to which we would ordinarily apply the concept involuntary. But it must not be supposed that they are therefore alike, or contain a common ingredient, except that they are contrasted with the clear case of voluntary, that is, intended action. The cry of pain or scream of terror are different sorts of departures from this case than the patellar reflex or the action that has unforeseen consequences. The clearest case is when I say, 'Stop, you're hurting me'. If I groan, howl, scream or sob, the case is perhaps not quite as clear. But standards of expressive performance still apply. If I say, 'Ouch!', we'd be inclined to say 'voluntary', if we had to say one or the other. If I moaned or screamed we might or might not. Perhaps the scream sounds put on, perhaps not. If the former, we will probably call it voluntary, if not, involuntary. But even if involuntary, what is ascribed to me can be interpreted in various ways. The cry might cause the masseur to stop his ministrations, and I might say: 'I didn't mean that you should stop.' So the cry in this case is called involuntary. This is a case of an act not having its expected consequences. If it did, if the masseur simply eased up a little with those strong hands of his, the cry would have accomplished its object and would be called voluntary. Thus voluntary and involuntary occasionally get mixed up with 'meaning to say'. A verbal slip is involuntary because the speaker meant to say something else. But this slip might receive further interpretation, as either intended or not. There are cases in which verbal slips might be accounted for phonetically, as shifts in sounds or syllables; others where the slip betrays a different intention, as amply attested to by Freud. The involuntary is not the absence of intention, but that which is contrary to some intention, and which of itself may be regarded as an intentional act, or an expression of intention. On the other hand, the patellar, or any, reflex, the sudden withdrawal of a hand from a hot surface and, on occasion, a cry, might (as Anscombe suggests) be called non-voluntary. The question of intention does not arise in such cases. The man whose leg jerks at the tap of the hammer neither intends to do so nor intends to do something incompatible or inconsistent with this. Only if he does intend something different is the knee-jerk involuntary.

Hume's account of caused and free acts is thus correct in application but incorrect in formulation. He wishes to show that free (intended) action is a sub-class of caused (determined) action, so that they can be shown to be compatible. They are compatible, since ascribing intentions has nothing particularly to do with causes. An action, in the clear case, is intended if it squares with what one wants or is trying to do. And this is a matter that is settled (when it is settled) quite independently of those sequences of events which might be thought to provide the causes of action. The pain is (perhaps) the cause of the cry. But the cry may or may not achieve its intent. If it does, we call the cry intended, subject to certain modifications, for example, how the cry is uttered. If it does not, we are apt to say it is involuntary, that one gives way to the cry knowing that it will lead to the cessation of what one wants continued.

The upshot is that we need not be concerned with voluntary and involuntary, or free and constrained, in deciding how it is that we apply the adjective 'intentional' to actions. For it is in the context of intention that we come to talk about voluntary and involuntary, free and constrained.

Still, the problem of assigning intentions is not resolved by removing these subsidiary considerations. The phrase, 'where the action matches what one wants or is trying to do', suggests that the paradigm is connected in a crucial way with expressions of intention. Thus, I say, 'I'm going to the store for cigarettes,' and do so. It might appear that the decision depends upon the performed action, a view certainly supported by the fact that even had I not expressed my intention, my action would have betrayed it. The same would hold for animal intentions. Anscombe talks about a cat crouching, with a mouse or bird in view, as a case of intentional action, and intending in acting, but not an expression of intention.* This is obvious in that a cat does not tell us what he is doing. Furthermore, denials of intention are seldom sufficient if actions reveal I say, 'I'm going down to the corner for cigarettes,' and my wife observes, 'Taking the cat?' and when I come back three hours later, says, 'Three hours to buy cigarettes?' My expression of intention is lightweight compared to these aspects of my actions. Compare Wittgenstein's remark: 'If you had said the words, would you necessarily have meant them quite


* Anscombe, p. 13.

* Cf. Anscombe's well-poisoning case, §§ 23–35.
some have greater logical force than others. The importance of any one criterion depends upon the context. A man whose car goes out of control does not intend to run down his victim. But the line between accident and intention is not decidable in advance by a rubric defining intention in terms of degrees of ludicrousness or implausibility of the accompanying actions. Quite similar sequences of events might, in one case, be judged intentional, in another accidental, depending upon such a variety of contexts that it would be quite hopeless to attempt to state them. Any attempt to theorize about intentions is a quixotic procedure, defeated from the start.

This might very well be the answer if one thinks of ascribing intentions to others. But further troubles arise with self-ascriptions. Ascribing intentions to oneself clearly does not depend on witnessing or noting anything. Ascribing intentions to others, however, involves a great deal of observation and shrewd calculation, though it is by its nature quite distinct from the way of going about an investigation characterized by scientific research. One way of handling first person ascriptions is given by Austin, who says that 'I intend', 'I am going to . . .' and so forth are performances, and so denies that the ordinary queries with regard to truth and falsity and manner of acquaintance apply here. If it is knowledge at all to say I intend, it is a queer sort of knowledge, queer at least after one has spent all one's time examining descriptive discourse. For it is not 'knowledge about' at all. To be able to say 'I intend' correctly marks the learning of a procedure, a performance appropriate to specified circumstances.

It might be asked why should a person perform this way or that way? Will it do here to say that any number of things may occasion the performance? I open the door of the refrigerator and, finding no beer, announce my intention of going to the store for more. I am nagged by my wife day after day, and finally say, 'Tomorrow I'll wash the car'. Finding no beer, being nagged, might be thought of as the occasions for announcing intentions. A Martian observer, looking at things from the outside, might even formulate regular connexions between special circumstances and special announcements of intention. But if we ask, what do we recognize, observe, or take note of that makes for intending or expressing intentions, we should have to appeal to a desire for something plus a knowledge as to what will achieve that end. So announcements of intention assimilate to the justifying procedures characteristic of our explanatory use of desire. We assess situations in the light of the indefinite range of desirable objects and activities, and are thus led to declarations of intention and descriptions of actions.

7 *Investigations*, § 641.
When my wife acidly comments the following day: 'I see you didn't wash the car,' can she also say I lied? Not exactly. She might say that my intentions aren't worth much. (Compare, your confederate money isn't worth the paper it's written on.) What she is saying is that my expressions of intention turn out to be very poor indices of future performance. And this is, indeed, much like Austin's standard case of promising. The accusatory 'You said you would' is not 'You lied to me' but 'You're not reliable.' We can lie only in cases in which what we say refers to an occurrence which anyone, in principle, could witness and describe. But the close similarity of the two—both are cases of unreliability, after all—leads us to think that something, the intention, is or is not present.

We can now give an answer to the question, how does a person go about intending, if not by special knowledge of ingredients of his soul, by pointing to the rules for the employment of intentional expressions. And we can forestall the question, how does a person know whether he's seriously intending or not, by taking note of sufficient examples which show when such a question naturally arises. It does not, for the speaker or hearer, arise in the simple case of announced and immediately performed action, but only in the case of announced (and usually repeatedly announced) but unperformed actions. 'Do you really mean to do it this time?' suggests that there is something or other to attend to or grab hold of (like the muscular contractions James talks about in his chapter on Will, for instance), which he could then tell the truth (or lie) about. But to ask a person whether he's serious is really to put pressure on him to get the job done this time. Consequently, there is no particular question that a person can address to himself, 'Do I really intend?' and hope to find an answer. Seriously intending is a move in the intending game which does not arise unless the game is complicated by failures at performance or by doubts that the performance will be carried out.

Of course, a person might say of himself, 'I did intend to, after all' and thus seemingly describe himself in some peculiar way. So the wife whose husband continually brings home a lady friend for coffee manages to spill coffee all over the lady's elegant dress. 'I didn't mean to,' she cries in alarm, excusing her conduct. But later, especially after it happens for the third time, honest attention to circumstance might lead her to say, 'I suppose I meant to after all.' Here she uses intend as we generally do of others, considering actions in the light of circumstances and motives.

This is fortified by the fact that an ascription of intention to others and the denial of intention to ourselves is usually a business of praise and blame, a matter in which the crucial question is the justification of an action. Consider Anscombe's case of St. Peter's three denials (pp. 91-93), in which she wishes to allow that Peter did not intend to deny Jesus. Surely he did say, 'Nay, Lord, I will not,' and surely he did say an hour later, 'I do not know the man.' If Judas, now, had professed good faith at the Last Supper, would we say he really didn't intend to go back on his word, but failed to foresee what the penalty was to be, or forgot for the moment the 30 pieces of silver jingling in his pocket? Here, though he regretted his action, we should be less inclined to say he meant well, poor man, but circumstances. . . . In both cases we see a pattern of actions mitigating against earlier expressions of intention. If we distinguish the two cases, it might be because in one case a man is only saving his own skin, in the other his action leads to betrayal and execution. The seriousness of the breach is one way of deciding whether the expression of intention is to be taken as an excuse or not. Incessant repetition of the act which the person promises to avoid is another. Repeated failure to perform an action intended is still another. All these criteria suggest that what is primary in ascribing intention to others is the business of justifying or excusing. The person who says, 'But I meant to' is excusing a failure to perform. If we refuse the excuse, we wish his expressions of intention to be taken seriously by attaching penalties, by holding him to them.

Still, this fails to bring out one aspect of the judgment of intention. Judas's act was premeditated, St. Peter's was not. Note how with each new formulation it looks as if we have hit upon the characterization of intention, but each in turn points to a particular feature, and sometimes only a particular oddity in the diffuse use of this term. Premeditation fails also. It is, to be sure, a way of distinguishing between actions for which we would be prepared to hold a person responsible and those we would excuse. The act for which elaborate preparation has been made counts as intentional no matter what. But we would not say the unprepared, the spontaneous act, is therefore unintentional.

Intention is thus one of those concepts calculated to rouse the psychologist to a fury of experimental investigation, and rigorous definition of variables. For one might infer from the breakdown of all criteria that ordinary speech and observation fail to get at the thing in question. Did a man intend or not looks as if it should have a straightforward answer. And when we discover that, on one occasion, we say a man intended because 'he said he would', on another because we can point to all the preparations he has made, on still another because we can note that he does the same thing every time, and still wonder, with any and all of these criteria, whether he really intended, it looks as if the ordinary man.
needs professional assistance in the quest for intentions. Often an ascription of intention on one of these grounds runs foul of an ascription on another, which makes it, from the psychologist's viewpoint, all the more hopeless. The point is, however, that that is the sort of game being played, when we intend or announce intentions and perform actions. There are built-in ambiguities in this game. Furthermore, when the various rules of application conflict, the procedure is one of ad hoc judgment, guided perhaps by like cases, as the law is guided by precedent, but not determinable by special kinds of discovery. The ambiguities and the indecisiveness in many cases make it appear that something ought to be discoverable which would resolve the doubts that arise from our imperfect and primitive modes of pre-scientific observation of the world. But in the case of intention, as in so many of the concepts of ordinary life, ambiguity and vagueness and indecisiveness are part of our usage. No general theory would help us out of our difficulties.

Elaborate planning, perhaps, takes precedence most frequently over other grounds in assigning intentions. The man who has a basement chock-full of cunningly-contrived explosive devices is not making toys or engaging in a hobby, whatever his avowals or the subsequent failure to perform may indicate. But occasionally what seems to be elaborate planning may turn out to be something else. It could be a hobby, especially after all opportunities to blow up the White House or the U.N. are passed up.

To sum up. We ascribe intentions to others and sometimes to ourselves (1) by seeing an action in the light of a situation (and thus seeing it in the light, also, of some vaguely defined collection of human practices, institutions and rules), (2) by noting the tendency of actions in a situation, (3) by listening to avowals of intention, and assessing the manner of utterance as well as the character of the avowee, (4) by noticing the intensity or elaborateness of planning which appears to have an action of a certain sort as consequence. In a further sense, like Austin, we can regard expressions of intention as performatives, made felicitous (in Austin's term) by the situation and the rules that bear upon it, but not in any sense to be regarded as reports.

We might be tentatively inclined to say, then, that ascribing or uttering intentions is thus very much of a piece with other kinds of human speech depending for their use upon the procedures for justifying or excusing actions. It might even be said at this point that the very concept of action, singled out by Wittgenstein, and more clearly by Melden, as what most clearly distinguishes the mental or the human act from the inanimate movement, is a consequence of our tendency to judge and appraise the motives of human and animal agents.* This would seem to underlie Wittgenstein's comment, already quoted, that 'an intention is imbedded in its situation, in human customs and institutions' (§ 137). It is also suggested by many of Melden's comments on the difference between action and movement. "The movement (e.g., arm raising) just is an action", Melden says. It suggests that something distinguishes rising arms from arm raisings. But when we look closely at his argument it looks rather as if the two are alternate descriptions of the same events. For, this is Melden's central puzzle, it is the same set of physical events that on some occasions will be called movements and on others actions. Melden has some interesting things to say at this point about Camus's *The Stranger*. 'The writing of this book treats actions as if they were movements' (pp. 192–3), and thus makes its point about a character to whom the very terms of human existence are alien. The point of describing human actions as movements, as in the novels of Robbe-Grillet, whose metticulous physical descriptions have much of the same alien and depressing effects, is to bring out the oddity, the normality of such a life. It makes its case against a background of normal descriptions of actions, as intended, meant and lived. At least this is Melden's point.

But the Camus case also shows us that it is (always?) possible to redescribe actions as movements. It does not generally suit our purposes to do so, and this is a fact of utmost importance frequently forgotten by psychologists. They have been rigorous enough in rejecting common sense methods, but quite naïve in retaining common or garden purposes in asking questions about intelligent action. In any case we see certain physical movements as movements or as actions. What leads us to make this choice of ways of describing? A man falling from a height can be described quite adequately as 'the body falling'. But a man jumping from a window ledge cannot be described as a body projected from the ledge. To say he intended or wanted to is to reflect the distinction, but since our manner of assigning intentions and desires is by way of actions, it is an appeal to intentions and desires as grounds for the distinction would be circular.

We might speak of a sequence of movements as an action when one or more of a variety of conditions is met. (1) It matches an expression of intention. (2) It results in what might be supposed to be a desired consequence. (3) It is a convenient substitute for a mechanical description, when difficulties stand in the way of such a description. (4) It occurs as

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* Cf. also Hamlyn's article, 'Behaviour' (Philosophy, XXVIII (1953), pp. 32–45), which points to the importance of Aristotle's distinction between *energeia* and *kinisis* in this matter.

** Free Action, p. 187.
part of a game, as a rule-abiding procedure, entered into by numbers of players.

(1) is rather unimportant. We are quite prepared to admit intentions without expressions, or to deny them in contradiction to expressions. But we should, especially in certain vague and unusual cases, be gratified to hear an expression of intention as a way of deciding what in the world a person wanted to do.

(2) and its corollary, the repeated production of consequences, has been discussed. It amounts to this. Certain things happen in the physical world as a result of our actions, and we see them as desirable. When this occurs, especially in conjunction with extensive planning or constant recurrence, we think of the movements involved as actions. But notice especially here the borderline cases, like brushing a fly from one's nose (Melden, p. 206), which we might think of as involuntary, yet they count as actions. They seem to be instances of causal connection; certainly they can be described that way. Yet they can be looked at also as achieving a purpose, and in this way such a gesture becomes a typical purposive act. The injection of purpose into this account would make it appear as if an assemblage of movements counts as an action because it is explained this way. But of this more in the next section.

(3) is a rather different kettle of fish. It suggests that action accounts are second-best stand-ins for mechanical accounts. The latter are, after all, difficult to come by sometimes. They require a sophistication of technique which is at times all out of proportion to the importance of the material, and so we content ourselves with speaking in the rougher way. But a mechanical account of movements simply does not give us the kind of description or explanation of behaviour for which we are usually looking. The possibility of giving a mechanical account of muscular movements does not subvert the effectiveness of an action account. Melden's discussion of this point would thus seem essentially sound. He wishes to emphasize the possibility of a causal-chain description of the arm rising, while noting that in the context of typical questions about this movement, the physiological detail would be irrelevant. We want to know why he raised his arm, not why or how it rises.

But put the question another way. Suppose we ask, why do we find certain questions relevant? Two answers seem to have been given to this question. The gist of one of these answers is to be found most forcibly expressed in Austin's lectures, where he reminds us of the extent to which many of the terms we use are learned ostensively. 11 Thus, to transfer

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belong to the point of view of the actor rather than the point of view of the spectator.

This way of putting the contrast has its disadvantages. For it makes it seem as if the language of the actor is only appropriate to the actor, to the person deliberating, deciding, aiming or intending. This focuses our attention on expressions of intention and purpose as opposed to reports of them, and leaves the field of reporting to the spectator. The ideal spectator, in turn, is the scientist. Without considerable care, then, the dichotomy of actor and spectator could lead us full circle to the view against which I have been arguing all along, namely, that knowledge, discovery, truth and understanding are the exclusive properties of science.

The dichotomy is instructive in another way, however. The spectator's point of view finds its most ready expression in the language of observation, event and occurrence. The most natural way to characterize the spectator's point of view is to imagine a succession of objects wholly describable in terms of their physical properties and relations to other objects. This is the Humean picture. It is always an occurrence that needs explaining, and this seems to imply that the form of any explanation will consist in relating an occurrence to the occurrences that precede it. In contrast, for the actor's point of view we must imagine something operating intelligently, according to rule or plan, or in such a way as to reach a particular goal. When we superimpose one of these pictures on the other, we get the blur and distortion characteristic of the psychologist's attempts to talk in purposive terms, yet from the point of view of the spectator.

The action of a man, a rat, or a pigeon is thought of implicitly as goal-seeking (that is, drive-reducing), but the form of description is that of discrete events linked by ties of resemblance and conjunction. To say that a man does something in pursuit of an aim is clearly explanatory, but the psychologist's way of representing this explanation is the event language of the spectator. Thus purposes come to be thought of as quasi-physical occurrences causing behaviour, and talk about purpose is open to all the criticisms to which positivist philosophers subject it. For there is nothing the spectator can find that will count as purpose, nor is there any idea what sort of event is being looked for.

To note that purpose talk belongs to the point of view of the actor reminds us that we need not look for an event justifying the introduction of an explanation. Purposes are not events any more than motives or intentions. Recognizing or comprehending purposes is not a matter of observing particular events or successions of them. It is rather a matter of setting what a man or animal is doing in a context that makes those movements intelligible. It is seeing that a bodily movement is consistent with, or more strongly, logically entails, a consequence, which, in its turn, can be seen as something worthy of being attained. The connexion with motive and need talk is obvious. Just as some particular circumstances can be viewed as standard occasions justifying or excusing actions, and others as standard examples of things worth resolving or achieving, so particular circumstances or the lack of them can be thought of as the most natural circumstances worth trying to bring about.

In one sense, then, there is no special story to be told about purpose. It is a general title encompassing motives, intentions, needs and desires, and refers to the type of understanding that consists in justifying an action by the contribution it makes to achieving or blocking some state of affairs. This is the point of view of the actor, since it is necessary, in making such judgments, to appraise a situation for what it is needed, required, desired, justified or excused in it, whether we are about to act ourselves or are merely observing the actions of others.

To psychologists these are empty words. There is, for them, only one adequate or intelligible manner of describing states of affairs, the event language with its attending explanatory pattern according to which later events are to be traced to earlier. Thus if teleological explanation is possible, there must be some antecedent events that in some mysterious way embody the purpose of the organism. 'In its extreme form', Hull says, 'teleology is the name of the belief that the terminal stage of certain environmental-organismic interaction cycles somehow is at the same time one of the antecedents determining conditions which bring the behaviour cycle about.' So interpreted, teleological explanations can easily be shown to be useless; first, because the antecedent conditions cannot be found, and second, because, even if they could be, their discovery would have to wait upon the completion of the cycle. In effect, 'this means that the task of deduction [explanation?] cannot begin until after it is completed! Naturally, this leaves the theorist completely helpless.' In short, purposive accounts won't do because they are not causal accounts.

Moreover, they deny the conditions of investigation of behaviour on which Hull insists. 'An ideally adequate theory', he says, 'even of so-called purposive behavior ought therefore to begin with colorless movement and mere receptor impulses as such, and from these build up step by step both adaptive and maladaptive behavior.' His criticism of

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Ibid., p. 25. Charles Taylor, *The Explanation of Behaviour* (Routledge & Kegan Paul, 1964), pp. 16 ff. and again 114 ff., quotes these passages and criticizes the views contained in them in a way similar to that undertaken here.
purposive explanation amounts to saying that purposes won’t do because they are causes, and a purposive or actor’s point of view won’t do because it is not the point of view of events spread out in space and succeeding one another in time. It is taken as self-evident that an explanation or description in actor language is inadequate, just because it assumes that form.

The proper retort to such an argument is to point to the case with which we offer actor’s accounts of others’ actions as well as our own. But this in turn requires comment. The facility is no doubt due to the fact that in ordinary life the range of likely goals, justifying circumstances, needs and desires is pretty standard. The same feature of justifying explanations, of course, is also their danger. For we too readily ascribe to others the detail of our own desires, motives and needs, only to find that people are not as similar to one another as the point of view of the actor might seem to require. Disappointments like this lead to modifications of our accounts of the behaviour of others, but these modifications are in the first place dictated within the context of justifying or purposive explanation and, in the second place, provide us, by the failure of our explanations of particular cases, with a richer store of examples of justifying occasions for action. And this enlarges the basis for our own decisions and judgment as well as the understanding of the actions of others.

Sometimes teleological explanation is regarded as extraordinarily, indeed suspiciously, easy because of failing to distinguish having a purpose and fulfilling a purpose. It is difficult to state criteria for having a purpose, for it suggests to most philosophers the mystery of internal entities or episodes which remain out of reach of scientific investigation and so can be introduced at will without fear of refutation. Obviously, if this were the case, teleological explanations would be quite generally defective. But, as I hope to show presently, the notion of having a purpose can be given meaning without recourse to such mysteries. The other half of this distinction, fulfilling a purpose, is sometimes resorted to when the difficulties of interpreting having a purpose are taken to be insuperable. The result of this is that a purposive explanation becomes a backward description, utilizing a later term in a sequence of events as the point of departure for describing earlier events. In this sense the choice of the language of purpose is little more than one of descriptive style. Anything could in that sense be purposive, for we might even give teleological form to our description of something as incorrigibly mechanical as Boyle’s law. This is the sense of purpose that Nagel appears to adopt, with the result that the term can be easily removed from the range of acceptable explanatory strategies. On the view that purposive accounts are no more than backward descriptions it is easy enough to show that they are inadequate devices by which to explore nature and so, by implication, human nature. When we consider almost any case of the emergence of physical or biological theory from its primitive beginnings we can see how the language of purpose is replaced by that of mechanics. Thus it was once supposed that antibody formation was ‘instructive’, that the body learned from the environment what to do with an invading organism and ‘chose’ a protein to combat it. Now it is supposed that antibody formation is ‘elective’: the response to an invading organism is elicited in a purely mechanical—in this case chemical—way, determined by the genetic code. Is it simply a methodologically better account? Or are we inclined to say that some events simply are not or cannot be construed as purposive without being misleading? We might say of some cases, like those described by Nagel as ‘isoperimetric’ or ‘variational’ forms of law (pp. 407–8), that the teleological formulation is simply a convenient way of calling attention to a phenomenon which is more accurately and non-commitally described by the mathematical formula. An example would be Snell’s law, that the angle of incidence of a beam of light equals the angle of reflection, which can be stated in teleological form by saying that light rays follow minimum paths. We might say the same of metabolism or any self-regulating mechanism, including, for instance, the pressure-temperature balance in a star. It is simpler to express what happens by talking about what is maintained than to write out complicated descriptions of causal sequences. But in other cases we might be more inclined to say that purposive vocabulary is used more seriously, as a description, and not as a mere facility of expression, which is what Nagel makes of it.

It is thus easy for Nagel to dispose of purposive explanation, since he admits only the fulfilling sense of purpose. If, on the other hand, we admit the distinction between fulfilling and having a purpose, we must ask what limits the application of purpose concepts in the latter sense. It may help to begin by considering why we are disposed to think of some goals as ‘natural’.

Part of the answer to this question is surely due to the fact of human similarity. We start with paradigm aims—a baby that is wet or hungry wants to be changed or fed, a man that is alone wants company, a man working hard at unpleasant tasks wants success. There will be occasions in our experience in which paradigms no longer apply, and these we find

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14 The Structure of Science, pp. 421–2.
15 The example is borrowed from Peter Medawar, The Future of Man (Mentor Books, 1961), pp. 88–89.
bizarre. If, none the less, the behaviour of a man exhibits a marked persistence, pattern, or direction, we are apt to come to think of the terminal point of his behaviour as an aim, and so realize that the range of human wants is larger than we had supposed. And note that we are able to speak of 'terminal points' because we bring a purposive point of view to bear on the behaviour sequence.

This procedure has the appearance of circularity. For it is a purposive way of looking at behaviour that makes it natural to describe it purposively. To talk purposively it must be possible to speak of actions as terminating, yet our justification for talking this way seems to depend on having already prepared ourselves to view the behaviour as fulfilling a purpose. Chimpanzees in the wild, for example, post a sentinel who screams angrily at the approach of something dangerous, at which the band flees. There is an immediate and well-nigh irresistible disposition to say that the sentinel warns the band of danger and that he is posted for that purpose. The reason is that we have, willy-nilly, described the chimp behaviour in purposive language. 'Posting', 'watching', 'running away', 'screaming', 'danger' are all terms from the purposive vocabulary.

Now it is also the case that the sentinel chimp does not stop screaming when the danger is gone, and this suggests that the action fails to qualify as purposive on at least one ground. Hebb, for example, considers four criteria for the appropriate application of purpose to behaviour. The fourth of these, that activity is not purposive unless it ceases when the objective is achieved, is not met in the case of the chimpanzee. But this might lead us to say that purposive activity is a function of intelligence, that the fullest possible set of conditions is perhaps never met, though approximated by creatures like men, of higher orders of intellectual capacity. As we have noted already, ascription of intelligent performance is an appraisal of actions relative to specific goals, and so purpose, as a member of the same family of concepts, that is, performance concepts, also depends upon appraisal.

Hebb's other criteria are also instructive. The first, the capacity for language use, he regards as too narrow, since it excludes animal behaviour. The second, capacity for problem solving, and the third, the long-range anticipation of future difficulties may, with the fourth, come close to singling out the features that would allow us to correctly apply purposive language to behaviour. But one difficulty with all these criteria is their actual or potential application to machines. The most natural way of talking about and making clear the workings of a machine is not, curiously enough, by means of a 'mechanical' account, that is, a description of its interlocking parts, but purposively by showing its design. We can also, of course, think of the machine in terms of the programming of it, that is, as an axiomatic system translated into a set of physical equivalents. Leibniz, in fact, might be interpreted as one who attempted to substitute a logical for both a purposive and a mechanical account. The possibility of giving different, but equivalent, accounts suggests that there are no teleologically ordered events, nor mechanical events, but only teleological or mechanical descriptions. Our preference for one or the other depends on our interests or felicity of expression. But it has no deeper significance. Anything, on this view, would count as either purposive or as mechanical, depending on what one wanted to do or say. Interest in accuracy and predictive power might lead to mechanical description, felicity of expression to purposive.

None of this, however, gets at the fact that we do think it more natural to ascribe purposes to some cases, the behaviour of organisms, for example, than to others. We are also, I believe, rather inclined to suppose that, if a purposive explanation is appropriate, a mechanical explanation will not do. Part of this may be due to the fact that descriptions which make purposive talk seem natural are themselves purposive descriptions. Some of the events surrounding the signalling of the chimpanzee, for example, are described as dangerous, that is as circumstances to be avoided. When the actions bring about this result we are apt to regard the actions as purposive in a stronger sense than that characteristic of Nagel's backward descriptions. If the behaviour is perfectly attuned to the occasion by beginning at the moment of awareness of the offending or enticing circumstance and ending with avoidance or achievement, the act is purposive. If there is a response to the occasion not quite as ideally suited to achievement or avoidance we might speak instead of emotion (fear or rage, for example) rather than purpose. If this is so, note how the language of purpose is built on the basis of emotion. For in emotions, situations conceived as dangerous, enticing or satisfying are recognized and responded to, but the reaction is not always perfectly calculated to do whatever is most appropriate to achieve or avoid the situation. Purpose, then, is restricted to those cases in which a rational plan is exhibited in addition to a signal of some goal-like character.

17 Cf. Methodology, § 17.
18 The preference for mechanical explanation on verificationist grounds is tied in part to the fact that theories of verification have been developed by using the science of mechanics as a model. This makes it appear as if nothing could count as verified and no explanation regarded as adequate unless it has mechanical form. But this confusion of the strategy of verifying with a special blue-ribbon case of it should not lead us to overlook the fact that there are better and worse, hence both more and less verified, purposive explanations.

18 Hebb, Textbook, pp. 206 ff.
REASONS

If we speak this way, the limitations on the use of purpose language are evident. However perfectly the movements of physical objects or processes (a river, say) fit some prefigured end (reaching sea-level) it is not as clear how we could ascribe emotion or need to the motions of inanimate objects. One could think, I suppose, of the churning of the stream as evidence of its striving to fulfil its desires. But the thing that makes all extensions of purpose-talk to inanimate things strange is the lack of any intelligible way of claiming that the water is aware of the object of danger, or conceives that an object could be the source of danger, or feels the pain that engenders the thought of danger. The stressed terms in the preceding sentence are intelligible only when accompanied by a method of awareness, eyes for seeing, skin for touching and feeling pain, ears for hearing, and so on. It is worth noting in passing that part of the difficulty with a sense-datum theory of perception is that it dissolves the logical bonds that join eyes to things seen. Being aware is for a special kind of thing to grasp, perceive, discover or view some feature of the world by means of some feature in its own make-up. In short, to speak of being aware it is necessary that one speak of an organism that is aware, that has the proper equipment for being aware, and to speak of purpose or emotion or need in a strict, as opposed to metaphorical, way is to speak of being aware of something in the world (including, of course, the organism) that contributes to or relieves the organism of fears and loves, needs and goals. Thus, whatever else, the language of purpose is a language that applies only to organisms.

It may be instructive to ask how it was that Aristotle and other philosophers of antiquity came to think of purpose as universally explanatory. In part it is probably due to the fact that Aristotle shares with modern philosophers a preference for a logician’s approach to the issues of science and metaphysics. He wishes to lay down general criteria for explanation, applicable to all cases. In practice he does not follow his rule, putting the peculiar features of the case at hand before the general claim. But he does introduce a proviso for a complete explanation which will include reference to the reason for which, as well as the what, how, and type. Thus, if Aristotle had become convinced that natural events have no purposes or reasons, he would have been forced to conclude that explanations of nature are logically incomplete. Similarly, modern philosophers are apt to treat reasons as inadequate explanations, because they do not rest on general truths, and hence cannot predict new cases.

But there is perhaps a more fundamental reason why explanation-in-general might have been thought to require a purposive component. The idea of explanation in itself is something that only slowly emerges from the expanding experience with and grasp of a world that men acquire individually in growing up, and collectively in social lore and science. In children the question why is intimately connected with actions and desires. They first ask why, I venture to say, because someone tells them what they must not do. Consequently the answer to the question why is first exemplified in giving a rule. Rules in turn belong to the matrix of human aims, for formulating them answers to and facilitates the desires and ends of action that men in society conceive. Quoting a rule is the initial answer to puzzlement. It is natural to suppose, then, that men would seek rules governing new cases in which puzzles arise, or view the world in such a way that puzzles would take this form. Purposive answers would thus be required to questions about behaviour of inanimate objects as well as organisms. One might speculate, for the sake of clarity if not for the sake of information, that purpose is the original and univocal answer to the question why, and serves as an initial paradigm which all satisfactory explanations must meet. Dealing with nature requires other and equally primitive models, such as the model of interaction with which we began. It would at least be foolish to suppose that mechanical ideas belong to a peculiarly sophisticated and scientific point of view. But purpose is none the less a difficult concept to shake off and leads us to clothe our most mechanical ideas in the purposive language of ends.

That we do so reminds us that it is only a part, though an important part, of explaining, to devise or assert something that bears in some way on the facts, whether this is a matter of consistency with, prediction of, or verification by the facts. Equally important to our use of the concept of explanations make something clear or plain (ex-planare) and this ingredient points to the persons to whom explanations are offered. To make plain may be to show how a perplexing action is purposive, it may be to show how discrete elements in a physical system are really connected by interlocking parts, or it may be something else still. But the intelligible form into which the perplexing facts are fitted must be part and parcel of our conceptual framework if we are to regard this case as cleared up by the given form. Our conceptual scheme, like that of the Greeks, is in many ways shot through with purposive notions. It leads us, even in the most remote by-ways of physical science, to describe movements of bodies as actions, to think, for example, of geodesies of relativity theory as minimum paths and the particle exchange of elementary particle physics as account keeping.

It is thus not surprising that we should wonder whether there is any limit to the application of the concept of purpose. Surely, we say, there is nothing unintelligible about the idea of the hand of God hurling the
Mechanical and purposive descriptions, then, exclude one another and the explanatory strategies that arise from them. This is probably not very surprising. The question of importance is why we think it proper to confine mechanical descriptions exclusively to inanimate objects, and purposive descriptions to organisms. One reason has been stated above. Having an aim, if it is to mean more than reaching a certain conclusion with relevant consistency, is tied to awareness of pains and pleasures, of objects desirable and fearful. And awareness depends on the special feature of animal organisms, their capacities for sensation and perception. Thus it is only in the context of organisms that the phrase '... as if...' can be deleted from the ascription of aims and the notion of having an aim introduced.

Beyond this, our talk about physical movement, and Aristotle's, is always talk about classes of objects, never about individual things save by way of illustration. Normal purpose talk, on the other hand, is always in connection with individuals, or some collection of them which, because of common action, can be treated as an individual. A man may have a long-term purpose, of course, and he may share it with many others. But though many of his actions may be animated by a single and distant goal, a given item of his behaviour may serve as an element furthering quite different goals. He may lift an object to put it on a shelf, develop his muscles, or show off his strength. In contrast we look at falling objects, for example, as indifferent members of a class. The question is about the explanation of this general circumstance, that bodies in general fall, not about the way in which, on a particular occasion, a falling body fits into some special and limited design. There is no way in which a purposive account can be successfully applied to what happens in general, for the likeness of movement from case to case does not reflect necessarily (or actually) a similarity of moves as instruments in a design. We must know a good deal about the particular circumstances in which a man or animal acts in order to give a purposive account of his action quae action. Knowing merely the physical movements will not help at all. Thus the strategies of purposive explanation are alien to accounts of kinds of things. An argument from design presupposes a conception of the universe as an individual, and not as a collection of similar processes.

We might conclude then that purposive explanations are restricted to the particular behaviour of particular organisms. Generality is achieved only by the accumulation of cases of similar organisms living in similar circumstances. We do not know better why organism A acts as he does because we discover that organism B acts in the same way. We only know that organism B and the whole class of B-like organisms have the
same aims and methods of achieving them as A. The original ascription of an aim to A is made possible only by seeing A's behaviour as contributing to a consequence that the observer recognizes as a legitimate goal, that is, a circumstance that he can appreciate as a goal. In this sense to ascribe purpose is to see an act as felicitous, as right or proper, by reason of fitting logically to a certain design.

It might, last of all, be asked why we could not conceive of a discrete physical movement as a particular move in a strategy directed at achieving or avoiding something. The short way with this question is to point out that the possibility of doing so is disbarred on the grounds that we are not dealing with an organism. But it might also be noted how different the concepts are that make possible the description of movements, on the one hand, and actions (moves, elements in a strategy) on the other. The concepts applied to movement are generalizations, or abstractions, of physical resemblances—down-up, fast-slow, right-left and their refinements. The concepts of action are abstractions of strategies—pushing-pulling, lifting-lowering and, of course, much more rule-governed cases like buying-selling, arguing-persuading, informing-lying, and so on.

A quite general frame of reference (space-time) serves to generate movement concepts. In describing an occurrence in those terms we are committed to a description that is indifferent to the individual cases. We have chosen a method that abstracts a common feature from all possible contexts in which special occurrences of the relevant type take place. In talking about acts or moves, on the other hand, we preserve in our concepts a relation to, and dependence upon, the particular contexts of which the moves form the elements. As the conceptual framework requires its own kind of description of the phenomena, so it determines also the kind of explanatory strategy that is relevant.

MEN AND MACHINES

Sometimes attention is called to the extraordinary capacities of negative feedback machines to engage in human or quasi-human performances, in order to show that purposive accounts can be reduced to mechanical accounts without remainder. We know that robots are machines. We also find ourselves describing their behaviour in purposive ways; the machines do tasks, calculate, they take cognizance of and analyse their surroundings. The machine thus presents us with an analogy to organisms. Purposive descriptions seem equally possible in both cases. We are apt to think, then, that if we only knew about the inner workings of a man the way we know about the workings of a machine, we would see that in the organism case the distinction between mechanical and purposive explanation is only a function of our ignorance.

Philosophers have attempted in various ways to block the reduction of purposive to mechanical explanation. Technical capacity, for example, is often appealed to. Machines are rather more efficient than men at their appointed tasks; why on earth would we employ machines otherwise? But their performances are limited and inflexible. The human mind is capable of reasoning backward from conclusions, of seeing what arguments would support a given conclusion, or what tactics would reach a given end. A machine, in contrast, follows an inflexible pattern of inference that is built into it. It cannot foreshadow the course of argument or policy but only proceed step by step. This is often suggested by those who would say that a machine can't imagine or that it lacks insight. Unfortunately these arguments too are arguments from ignorance. A machine has not been produced that will engage in certain kinds of human performance, but it is possible that machines might be devised that would.

The criterion that I have appealed to as basic to the use of purposive language, namely, the capacity for awareness, might also be considered applicable to machines. For we build machines with receptors that take in data and, by analysing it, act appropriately. The question is whether awareness leads or could lead in the case of the machine to emotional reactions. It might be argued, of course, that we simply have not bothered to build a machine to feel pain or fear or love. Once again the argument depends upon our ignorance of what it would be like to install pain, fear, or love circuits. Part of the difficulty has to do with a feature of being aware that is obscured in the case of vision. We do not feel the strain on the concepts in claiming that a robot sees, in the way we would if we were to claim that the robot felt. For in the case of vision it is an outer world that is to be apprehended by eyes or eye-like mechanisms. It is the common world, and the functionally similar mechanisms, that allow us to extend a vision vocabulary to machines. But in the case of feelings we lack a coherent way of talking about the mechanism, and the common world is a world of shared attitudes, not observable things. If, then, we imagine Searle's complete android, the machine that has all the human properties—solving problems, devising theories, becoming angry, responding to electric shocks, with all too human shrieks, and is even manufactured out of protein—we might still balk at including it.

[Notes:


21 Ibid., p. 136.]
among men, because that inclusion depends upon the machine entering into the human community as a member.

Suppose we did all this, calling the machine by name, treating him with respect and trying not to hurt him (unless we wanted to). Is there any point now to the question, is he man or machine? If he really is a machine there must be a point which at the show can be given away. But how? Perhaps someone comes along and says: 'That a man? Why, I made him myself.' And we might shudder or feel queasy at the thought of the nature of our contact with him. Still, manner of manufacture might diminish in importance. Imagine a strikingly beautiful female or an eminently athletic male android. They do all the things that constitute normal human performance, only perhaps a bit better. A man might finally come to say, 'Origin's a mere prejudice, I love her.' Or the manager, whose team has been languishing at the bottom of the league and suddenly comes alive, with an android guiding it to victory, likewise might shrug off the manner of creation. Notice how the athletic world especially has encountered and overcome other prejudices, against negroes, for example. They were different, so the story went, yet this prejudice began to vanish when the noted differences were seen to have nothing to do with the kinds of activities in which 'we' and 'they' work and interact.

Thus, the difference between men and machines is first of all a difference of attitude. A machine, defined as that which has been devised out of simpler materials by a human artificer, could very well overcome the attitudes that govern our manner of interacting with it, if it showed itself human in performance. We might have qualms about its manner of manufacture and repair, its extraordinary capacities for endurance and survival, but the significance of these qualms will come to depend upon what is central to our human interests. The machine which cannot make a mistake, suffer pain, appreciate a joke, succumb to emotional displays is not going to raise problems as to whether we have really created a man. On the other hand, the lack of one or more of these characteristics is not going to eliminate a creature from some more intimate grouping with men than we might now be able to envisage. If a creature is both capable of and limited to typical human performances, we are well on the way to applying the term 'man' to him, even though his manner of origin is not quite traditional. But, of course, our conventions in calling this creature a man are not likely to be absolute, any more than calling a man an anthropoid lifts him bodily and wholly out of one Aristotelian niche and drops him into another. Matters relating to origin and repair are important, to be sure; perhaps they are important enough to retain a looseness in the application of human predicates to the android. He is, let us say, man-like, something we might also want to say of naturally born rational creatures from other worlds. Too often, the defenders of the machine want to press likenesses until they are total. Suppose a machine differs from us only in being manufactured, and then a child is born to it in the normal way. Is the new creature a machine? Compare: was Adam or Abel the first man? In short, the questions, are men machines, and could there be machines admissible to the company of men, bother us in part because we are so often committed to a genus-differentia rigidity, so that to say an x is a y is to deny altogether that x could also be a z, or in some other way enter into a rather different classificatory arrangement. We might come to say, there are machines like men; we might even come to be fooled by a machine. Such a mistake presupposes a criterion for deciding the matter, however. But if it were a genuine philosophical problem, then machines must have been devised that put a strain on our normal conceptual apparatus. Ordinarily, in such impasses, we have recourse to a new concept. So monkeys are not men, nor men monkeys, but both are anthropoids. Undoubtedly, such a procedure would be forthcoming in the machine case as well.